METEOROLOGICAL ESSAYS:

BY

FRANÇOIS ARAGO,

MEMBER OF THE INSTITUTE.

WITH AN INTRODUCTION

BY

BARON ALEXANDER VON HUMBOLDT.

TRANSLATED UNDER THE SUPERINTENDENCE OF

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ADVERTISEMENT.

THIS translation of Arago's Meteorological Essays forms the first volume of a translation of all the principal works of this distinguished Author. The Introduction by Baron von Humboldt, which is included in this volume, fully explains the origin and plan of the collected edition of Arago's works; and it therefore only remains to state that Arago's Popular Astronomy is in the hands of Admiral Smyth and Mr. Robert Grant, and that the first volume of that work, comprising the first and second volumes of the French original, will appear early in November. Other portions of Arago's works will be translated by the Rev. Baden Powell.

In conformity with the classification adopted by M. Arago, Terrestrial Magnetism formed a part of the Meteorological Essays, and is here retained in the same position. This Essay appears to have been written for the most part anterior to the considerable advances which have recently been made in that branch of terrestrial physics. It contains, however, in addition to much interesting information in regard to the early views of its illustrious author, a *résumé* of the results of M.

Arago's own magnetical observations, which occupied a large portion of his time and thoughts between the years 1820 and 1835. The deductions from these observations are now for the first time made public, and are due to MM. Barral and Thoman, who, at M. Arago's request, undertook the labour of their calculation.

The failure of M. Arago's health and sight having prevented him from revising the Essay on Terrestrial Magnetism as fully as he appears to have done some of the others, the Editor of this volume has added a few notes to particular passages which appeared to him to require some additional notice. An index to the Editor's notes will be found in page 504., at the close of the general index.

CONTENTS.

CHAP. XLIV.

CHAP. XLV.

CHAP. XLVI.

CHAP. XLVII.

CHAP. XLVIII.

CHAP. XLIX.

CHAP. L.

CHAP. LI.

CHAP. LII.

AURORA BOREALIS.

CHAP. XII.

CHAP. XIII.

CHAP. XIV.

CHAP. XV.

CHAP. XVI.

CHAP. XVII.

INTRODUCTION.

BY

BARON ALEXANDER VON HUMBOLDT.

In these few pages I fulfil, too confidently perhaps, and without duly considering the measure of my ability, a mournful duty. Invited by the kind regard of a family who are dear to me, to prefix a short introduction to the collected works of the illustrious man whose friendship, during nearly half a century, has contributed to the happiness of my life, some apology may seem to be required for having acceded to this request; I offer none, however, because I have felt that neither modesty, nor the claims of other literary occupations, can have any place when asked to lay on this recently closed tomb the homage of my admiration and lively gratitude.

M. Arago and I were fellow members of the Academy of Sciences of the French Institute; the intimate relations maintained between us throughout a long series of years, his pleasing and constant habit of conversing with me and writing to me concerning his labours and his scientific projects, afforded me the most favourable opportunities of observing, I will not say the development of the faculties of his powerful mind, but their progressive application to the great discoveries which we owe to him. Without intending to write either a Eulogium or a Biographical Notice, I propose, therefore, to avail myself of the acquaintance which I thus possess with all the materials which have been brought together in the collection of M. Arago's works. I would recall the vast extent embraced by the labours of a single man in the different branches of human knowledge, and how, amidst this variety of objects, his mind tended ever towards the same leading aims, *i.e.* generalisation of views, the connection of phenomena which had long appeared to stand alone, and a nearer approach to the less accessible regions of natural philosophy. The action of forces manifested in light, heat, magnetism, and electricity, as well as in the combinations and decompositions of chemistry, belong to the series of mysterious effects on which the brilliant discoveries of the 19th century have shed an unexpected illumination. In the field of these glorious conquests, M. Arago has taken his place among the great physicists of our age. At once ardent in discovery and circumspect in regard to conclusions which might be beyond the legitimate reach of partial results, he especially delighted in indicating new paths by which the desired goal might be more and more nearly approached, and in recognising identity of cause in phenomena apparently most diverse. When we rise from the consideration of the method followed by M. Arago to that of the powerful faculties which he exercised, we cannot measure their extent without astonishment. While to scientific men he moved back the limiting boundaries of the study of Nature, he had, at the same time, a marvellous aptitude for the diffusion of knowledge already acquired. Thus no kind of influence was wanting to him, and the authority of his name equalled its popularity.

It was five years after my return from Mexico, during which interval I had had the inestimable advantage of working with M. Gay-Lussac, with whom I had travelled in Italy, Switzerland, and Germany, that I became acquainted with M. Arago on his arrival from Algiers in July, 1809. He had travelled along the coast of Africa in the month of August, 1808, after having been long a prisoner in a

Spanish citadel at the conclusion of important trigonometrical operations for the connection of the Balearic Islands with the continent.

My attention had long been drawn to M. Arago, not only from his having been distinguished by the choice which, on the strong representations of Laplace, the Bureau des Longitudes had made of him to proceed to Spain for the purpose of terminating, in conjunction with M. Biot, the French arc of the meridian; but also, and more especially, by the opinion of the most illustrious of geometricians, — the author of the "Mecanique Analytique," — Lagrange, with whom I had then the honour of being on intimate terms, and who, with the sagacity which characterised all his judgments, had recognised the great and early developed powers of the young philosopher. He had been struck in him from the first with that faculty of penetration which in complex problems discerns, and rapidly and clearly lays hold of, the decisive point, and had often spoken of him to me as a young man of extraordinary promise. This divination of Lagrange, who was generally so sparing of praise, has remained in my memory as a title of honour worthy of record.

When M. Arago's arrival on the coast of France was made known at Arcueil, where I was then living in the enjoyment of the friendship of Berthollet and Laplace, I wrote to congratulate him before he quitted the Lazaretto at Marseilles; and this was the first letter which he received in Europe after having been exposed to so many dangers and sufferings in preserving the fruits of his observations. I recall a fact of very little moment, — but I do so because M. Arago, feeling the charm which friendship gives to life, preserved a long-continued and lively recollection of it, and dated from thence the origin of our subsequent intimacy.

In September, 1809, at the age of twenty-three, M. Arago was elected a member of the Academy of Sciences by 47 votes out of 52. He succeeded Lalande, whose rare merit, attacked with too much levity during his long scientific career, has since his death been universally recognised. It was not alone M. Arago's arduous astronomical and geodesical labours which the Institute desired to recompense by his election: the attention of scientific men had also been drawn to several important optical and physical investigations. In concert with M. Biot, M. Arago had determined the weight of atmospheric air relatively to that of mercury, and had measured the deflection undergone by a ray of light in the different gases. Thenceforward the prism and the repeating circle were capable of furnishing data respecting the proportions between the constituent parts of the atmosphere, and even of showing how little those proportions vary. Such is the admirable mutual connection which links together different natural phenomena, that it has long been possible for the geometrician to prove to the chemist, solely by the measurement of an angle of refraction, that atmospheric air contains less than twenty-seven or twenty-eight per cent. of oxygen.

The velocity of light had been to M. Arago the subject of a no less ingenious investigation in physical astronomy. By the application of a prism to the object-glass of a telescope, he had proved not only that the same tables of refraction may serve for the light which proceeds from the sun and that which comes from the fixed stars; but also a fact which was already sufficient to cast great doubts on the emission theory, viz. that the rays of light coming from the fixed stars towards which the earth is moving, and the rays of light coming from the fixed stars from which she is receding, are refracted by exactly the same quantity. In order to reconcile this fact (obtained from very delicate observations) with the Newtonian hypothesis, it would have been necessary to admit that luminous bodies emit rays of all velocities, and that it is only rays of a certain velocity which are visible, *i.e.* that they alone produce in the eye the sensation of light.

In considering the kind of researches to which M. Arago had addicted himself even before he quitted France, we may remark in the first instance an extreme predilection for everything relating to refraction, *i. e.*, to the path of luminous rays, and the causes which alter their velocity. This predilection, as Arago himself has often told me, took its rise from the assiduous perusal of the works on Optics of Bouguer, Lambert, and Smith, which had very early fallen into his hands. Can I fail to remark how great an influence must have been exercised on M. Arago's mind, during the three years employed by him in geodesical operations, by the aspect of Nature, fertile in the plains, wild and often grand on the summits of the mountains; by the colour of the agitated waters of the ocean; by the varying height of the clouds; by the mirage over the arid shores, and in the strata of the atmosphere in which the night signals appeared to be multiplied, and to be alternately elevated and depressed; and generally by the life passed in the open air, beneficial in so many respects; in enlarging his sphere of thought, and stimulating his imagination, and exciting his curiosity, amidst the continual perturbations occurring in the, nevertheless, so regular, succession of phenomena? A traveller whose life is devoted to science, if susceptible to the impressions of grandeur in the scenes of Nature, brings back from distant and adventurous rambles not only a treasure of recollections, but what is still far more precious, — a disposition to enlarge the horizon of his contemplations, and to consider a great number of objects at once, and in their mutual relations. M. Arago had a marked preference for all the phenomena of meteorological optics; he was especially fond of investigating the laws which regulate the perpetual variations of the colour of the sea, the intensity of reflected light on the surface of clouds, and the play of atmospheric refractions.

If I might here permit myself to enter into some details, I would recall how greatly the young astronomer had been struck by the facility with which, when seated on a mountain cliff descending abruptly to the water, his eye could penetrate to the bottom of the sea and discern its rugged inequalities. This simple observation led him at a subsequent period into highly interesting discussions concerning the ratio of the light reflected at an acute angle from the surface of the water to that which comes from the bottom of the water; it also led him to the ingenious idea of suggesting, for the discovery of shoals or rocks under water, the use of a plate of tourmaline, cut parallel to the axis of double refraction, and placed before the pupil of the eye in such a position as to eliminate the rays reflected from the surface of the water under an angle of 37°, and consequently, completely polarised. As said by himself in the instructions drawn up for the voyage of circumnavigation of the Bonite, this was an attempt to introduce polarisation into the art of navigation.

The number and variety of M. Arago's investigations, directed equally to terrestrial and celestial physics, will render an account of his life, when it shall be attempted, a very difficult task. In all these we find the same penetrating spirit, the same ardour in scientific advance, and at the same time the same temperate reserve in the formation of conjectures. It has been said elsewhere, and justly, that M. Arago had derived from his thorough study of mathematics, that strictness of method, and security of view, which he brought both to his own experimental researches and to the appreciation of those made by his contemporaries. The public believe that generally speaking they have a right to feel a little mistrustful as to the solidity of researches by the same author on a considerable variety of subjects; the presumptuous expression of "universal knowledge," is highly dangerous, besides being always misapplied. Bacon, Newton, Leibnitz, and Cuvier, possessed very varied, but assuredly not universal, knowledge. By the extent and variety of his knowledge, M. Arago takes a place by the side of the most eminent men of whom science can boast.

In order to place in a true light the merit of men who have left behind them a bright track to mark their passage amongst us, we should fix our attention first on the most salient points in what has been effected by them. The great discoveries of M. Arago belong to the years 1811, 1820, and 1821. They relate to optics, to phenomena in physical astronomy, to electricity in motion, and to the development of magnetism by rotation. To indicate them more specifically, they are,— 1st, the discovery of *coloured* or *chromatic polarisation*; 2ndly, the precise observation of the *displacement of the fringes*, caused by the encounter of two luminous rays, one of which passes through a transparent plate, as glass, for example,—a phenomenon which indicates a diminution of velocity, or a retardation while in progress, and is in direct opposition to the emission theory; 3rdly, the first observation of the property of attracting iron filings possessed by the wire which conducts electricity in Oersted's experiment, otherwise called the reophore of the pile; the happy idea of causing the electric current to pass in a *helix* round a needle, and to *magnetise* it as well by the discharge of a Leyden jar as by a voltaic pile; and, 4thly, *rotation magnetism*.

The discovery of chromatic polarisation led M. Arago to invent the polariscope, a photometer, the cyanometer, and apparatus of various kinds used in studying different optical phenomena. It was by experiments on chromatic polarisation that previously to 1826 M. Arago showed by physical evidence that the light of the sun proceeds, not from an incandescent solid or liquid mass, but from a gaseous envelope. Having found the means of distinguishing direct from reflected light, it was possible to ascertain that in the light which proceeds from the tails of comets a portion is polarised, and that that light must therefore necessarily be supposed to be, in part at least, borrowed. Chromatic polarisation also furnished M. Arago with a means of recognising that the diffused light of the atmosphere is in part polarised by reflection, and that by successively examining the atmospheric strata at different heights and in different azimuths there is discovered a *neutral point* in regard to polarisation, situated in the sun's vertical plane, and about 30° above the point opposite to that luminary. This point, which is called neutral, because there is no sensible polarisation at it, differs from the two other "*neutral points*" of Babinet and Brewster, which were discovered later.

In this fine series of optical investigations I have stil to speak of two subjects which have been in a very great degree elucidated by Arago and his constant friend Fresnel, himself a master and discoverer in several departments of optics; they are subjects whose importance cannot be denied, since they touch on the great phenomena of *interference* and *diffraction*. The first of these is the *scintillation of stars*, a phenomenon which the illustrious Thomas Young, to whom we owe the fundamental laws of the interferences of light, had thought inexplicable. The scintillation is always accompanied by a change of colour, and of the intensity of the light. The luminous rays from the stars, after having traversed an atmosphere in which there are always strata differing in temperature, density, and humidity, and, consequently, in refracting power, re-unite to form an image, vibrate in accord or disaccord, and either reinforce or destroy each other by interference. I take pride in recalling that extracts from this fine theory of scintillation were published, for the first time, in 1814, in the fourth book of my "Voyage aux Régions Equinoxiales du Nouveau Continent." The memoir itself, which is full of curious historical researches, is one of the principal ornaments of the collection of the works of my illustrious friend. Other extracts relating to the same subject, but taken from more recent manuscripts, belonging to the year 1847, have been inserted in the astronomical part of my work entitled "Cosmos."

The *Interference* of light, respecting which, Grimaldi (of Bologna) had already obtained some vague glimpses about the second half of the 17th century, has given occasion to the enunciation of a fundamental truth, which has already been often proclaimed, viz., "That under certain conditions light added to light produces darkness." No doubt there is inscribed in these few words the victory of the undulatory over the emission theory; but this victory could not be regarded as assured and complete until supported by simple experiments which could not be rejected. As I have already said, M. Arago had discovered, in 1818, the remarkable effect produced in the phenomena of interferences by a very thin plate, placed in the path of one of the two interfering rays. There then occurs a *displacement of the fringes*, and a retardation in the light, which moves more slowly through a denser substance. "The property of two rays of light to destroy each other by interference being once established," says M. Arago, in alluding to other experiments made by him conjointly with Fresnel, "is it not yet far more extraordinary that we should be able at pleasure to take from them that property, and that one ray should lose it for the moment only, while another, on the contrary, should be deprived of it for ever after?"

When Mr. Wheatstone, in his fine experiments on the duration of the electric spark (1835), had succeeded in using with great success his ingenious rotatory apparatus, M. Arago immediately perceived the possibility, by applying the same principle of rotation, of measuring, by angular deviations, the difference of the velocity of light in a liquid and in air. He gave an account of the experiment which he proposed to make to the Institute towards the end of the year 1838. Aided by an experienced and skilful artist, (the younger Breguet,) he succeeded, after many changes in the apparatus, in realising his project. In the course of the trials for this purpose, M. Breguet had succeeded in causing an axis to turn eight thousand times in a second, when relieved of the weight of the mirror which it carried. At last, in 1850, all was ready, and the apparatus, with its various improvements, could be made to work; but the great and sad alteration which

M. Arago's sight had, almost suddenly, undergone, took from him the hope of himself engaging in the observations. In a note presented to the Institute, on the 29th of April, 1850, he announced this, saying, with a noble simplicity, "My pretensions must be limited to having propounded the problem, and publicly proposed certain means for its resolution. In the present state of my eyes, I can only accompany with my best wishes the experimentalists who shall be willing to follow up my ideas, and add a fresh proof in favour of the undulatory theory to those which I have deduced from a phenomenon of interference, too well known to physicists for it to be needful that I should recall it here." M. Arago had the satisfaction of seeing his wishes fulfilled. Two experimenters, equally distinguished by their talent and by the delicacy of their observation, M. Foucault, to whom we owe the physical demonstration, by means of the pendulum, of the earth's rotation, and M. Fizeau, who has determined by an ingenious method the velocity of light in the atmosphere, succeeded, after adding some further improvements to the means proposed by M. Arago, in resolving the question, in the sense which is subversive of the emission theory. MM. Foucault and Fizeau presented the results of their labours to the Academy of Sciences, the former in May, 1850, and the latter in September, 1851.

If I have dwelt at some length on the principal researches of M. Arago on light, it has been because it was the subject to which he devoted himself with greater perseverance than to any other, for an interval of more than forty years. His discoveries in electricity and magnetism, however important they may be in themselves, only occupied him, comparatively speaking, transiently. The attraction exercised on iron filings by the reophore wire joining the two poles of the Voltaic pile, and the magnetisation effected by the employment of a wire coiled as a helix, either continuously or with interruptions and changes of direction, had been observed by M. Arago previous to the admirable labours of Ampere, and these observations had already given a lively impetus to electro-magnetic researches.

Rotation-Magnetism was discovered by M. Arago during a visit which he made to England, together with myself, for the purpose of comparing the length of the pendulum conjointly with M. Biot. The results of our observations were not so satisfactory as we could have desired; but M. Arago, while engaged with me on the slope of Greenwich Hill, in determining the magnetic intensity, by the number of vibrations performed in a given time by a dipping needle, made the important remark, which was *exclusively his own*, that a magnetic needle which has been set in motion, comes to rest sooner when placed in proximity to other substances, metallic or non-metallic, than when at a distance from them. From this first remark, rendered fruitful by ingenious combinations, he was led in 1825 to explain the phenomena produced by the rotation of disks acting on needles at rest, as well as the influence which water, ice, and glass exercise on magnetic needles. The excitement of magnetism by motion became a subject of warm discussions between Nobili, Antinori, Seebeck, Barlow, Sir John Herschel, Babbage, and Baumgartner, which lasted for six years, or until 1831, when the brilliant discovery of Faraday linked all the phenomena of magnetism by rotation to the fruitful principles of *induced currents*. Such is the character of the onward progress of the sciences at those periods, unhappily too short, in which they advance with a rapid step, in which ideas tend towards increased generalisation, and the minds of the students of nature are gradually rising towards a higher order of conceptions.

In tracing the present sketch of the more important of M. Arago's labours, and of the influence which they have exercised, I have availed myself, in addition to my own recollections, of those of two men devoted to his memory: M. Auguste de La Rive, the celebrated Genevese professor, and M. Barral, a chemist and physicist of rare merit, holding an official station in the instruction given at the Ecole Polytechnique, of which establishment I preserve a personally grateful recollection from having long worked there under the direction of M. Gay-Lussac.

After the general view above given, I have still to enter into some details respecting the distribution of the materials of which M. Arago's collected works will consist. But I must first premise that it will be difficult to follow any very determinate order, so close are the links which unite the different sciences, and which new discoveries are multiplying daily, and so uncertain are the limits which divide them.

I also find myself constrained to increased indefiniteness, from the circumstances of my being at a distance from France (which was long to me, as it were, a second fatherland), and of not having M. Arago's manuscripts before me. I divide the labours of my illustrious friend generally into six groups.

1st. *Literary and Biographical Part.*

I believe I am a just exponent of the public voice, amidst all the dissimilarities of opinion, when I extol in M. Arago's "Eloges Académiques," the critical care given by him to inquiry into the facts, the impartiality of his judgments, the lucid clearness of his scientific expositions, and a fervour which increases in proportion to the elevation of the subject. The same qualities distinguish the different discourses which were pronounced by him in the political assemblies, where from the nobleness and purity of his convictions, he occupied so eminent a place, and the reports which were drawn up by him for the purpose of causing due honour to be paid to science in the persons of some celebrated inventors.

In order that the merits of the men whose life and labours he proposes to trace and to characterise may be justly appreciated, M. Arago generally begins with a sketch of the state of knowledge at the period when their career commenced. He brought to this work no less patience than ardour; and in consequence his éloges are of high importance for the history of science, and particularly for the history of great discoveries. Profound convictions, acquired by long and arduous researches, have sometimes rendered his judgments severe, and have caused him to be exposed to unjust criticisms. The discovery of the decomposition of water, for instance, and the invention of the high-pressure steam engine which has so powerfully seconded the dominion of man over nature, are facts in regard to which, as in regard to many others also, sentiments of nationality are not the only cause of the divergence of opinion existing among scientific men.

A zealous defender of the claims of intellect, M. Arago, in his éloges, often leads us to feel how much elevation of moral character dignifies and ennobles mental labours. In his expositions of the principles of science, on which he knew well how to shed an admirable and persuasive clearness, the style of the orator becomes the more expressive, the greater the simplicity and precision which it offers; in such passages he attains what Buffon denominated *la vérité du style*.

2nd. *Part relating to Astronomy and Celestial Physics.*

Operations for the measurement of the French arc of the meridian in its most southern part, executed conjointly with M. Biot.—Figure of the earth.—Investigations relating to the precise determination of the diameters of the planets.—New ocular micrometer, and new prismatic telescope different from that of Rochon.—Summer and winter solstices; vernal and autumnal equinoxes; declinations of southern and circumpolar stars; absolute position of the pole star in 1813; latitude of Paris; parallax of 61 Cygni (conjointly with M. Mathieu).—Geodesical operations made on the coasts of France and of England, conjointly with M. Mathieu and with English men of science, for determining the difference of longitude between Greenwich and Paris.—Researches on the declination of some stars of the first and second magnitudes, made conjointly with MM. Mathieu and Humboldt.—Photometric researches on the comparative intensity of the light of different heavenly bodies, and of the light proceeding from the margin and from the centre of the sun's disc.—Intensity of the light in different parts of the moon.—Variability of the ashy-coloured light of the moon's disc.—Polar regions of the planet Mars.—Bands or "belts" of Jupiter and Saturn.—Light of Jupiter's satellites compared to that of the central planet.—Physical constitution of the sun and of its different envelopes.—Light emanating from the gaseous parts of the sun.—Singular phenomena presented by total eclipses of the sun.—Reddish protuberances showing themselves on the contour of the moon during a total eclipse of the sun.—Rays of polarised light in the light of comets.—Cause of the scintillation of stars.—Tables of refraction.—Irradiation.—Effect of telescopes on the visibility of stars during daylight.—Considerations respecting the diffused light of the atmosphere.—Velocity of light proceeding from the stars towards which the earth is moving, and from the stars from which she is receding.—Velocity of transmission of rays of different colours.—Means furnished by the phases of Algol for measuring the velocity of transmission of the rays of light.

The "Popular Astronomy," which contains the exposition of the different courses of public lectures delivered by M. Arago, from 1812 to 1845, in the magnificent theatre of the Observatory, and which were attended by all classes of society with the most lively interest, will be the principal ornament of this second part of his works. In reading the treatise entitled "Popular Astronomy," many pleasing and, at the same time melancholy recollections will be awakened in the minds of those who had the good fortune of being present at the lectures given by M. Arago, and of admiring the manner of their delivery — so simple, so persuasive, so engaging, and awakening so much regard for the speaker.

III. *Optical Part.*

Difference in the nature of the light emanating from incandescent bodies, solid or gaseous.—Means of distinguishing by the polariscope polarised from ordinary light.—Constant relation between the proportion of polarised light existing in the transmitted or refracted pencil and that which exists in the reflected pencil.—M. Arago also found, in conjunction with M. Fresnel, that polarised rays do not exercise any mutual influence when their planes of polarisation are perpendicular to each other, and that consequently they cannot in such case produce any fringes, although all the conditions required for the appearance of that phenomenon, in other cases, shall have been scrupulously fulfilled.—Treatise on photometry, founded on the theory of undulations, (an investigation at once experimental and theoretical, great part of which was contained in seven memoirs presented to the Academy of Sciences in 1850). — Refraction of luminous rays in different gases and under different angles. — Memoir on the possibility of determining the refracting powers of bodies from their chemical composition. —Investigations on the affinity of bodies for light, made conjointly with M. Biot.—Chromatic polarisation; its varied applicability in celestial and terrestrial physics.—Circular (rotatory) polarisation, or phenomena of colorisation, discovered as early as 1811 by M. Arago, in plates of quartz, cut perpendicularly to the axis of the crystal; (the white ray which traverses presents the most vivid colours when looked at through a doubly refracting prism).—Reflected and transmitted coloured rings.—Application of double refraction to photometry.—Formation of photometric tables, showing the quantities of light reflected and transmitted by a plate of glass, for inclinations comprised between 4° and 26°, and continued up to perpendicular incidence by a particular process.—Estimation of the loss of light which takes place by reflection at the surface of metals, and demonstration of the important fact that there is no loss of light in total or entire reflection.—The law of Malus, called the *law of the cosine*, on the division of polarised light, "which was at first only an empirical mode of representing appearances," was demonstrated experimentally by M. Arago, for the case in which the polarised pencil traverses either a doubly refracting prism or a tourmaline cut parallel to its axis. (The polarimeter of M. Arago, employed in these kinds of experiment, was so delicate that it indicated unequivocally the presence of an eightieth part of polarised light in a pencil of rays. In all these experiments relating to photometry, the experiments and calculations were made by MM. Laugier and Petit under the direction of M. Arago.)—Demonstration of the possibility of constructing an interferential barometer, thermometer, and refractor.—Views respecting the measurement of mountains by the polariscope, and of the height of clouds by the aid of a graduated polarimeter.

IV. *Electro-magnetical Part.*

Discovery of the property of attracting iron filings possessed by the reophore, or current-bearing wire joining the poles of the pile.—Magnetisation of a needle by means of the passage of the electric current in a helix; consecutive points resulting therefrom.—Rotation-magnetism, by which it was made certain that all bodies are susceptible of acquiring magnetism, a fact already divined by William Gilbert, and rendered probable by the ingenious experiments of Coulomb.—Observations of the horary variation of the magnetic declination at Paris from 1818; secular change of the same phenomenon.—Discussion respecting the movement from east to west, of the nodes or points of intersection of the magnetic with the geographic equator.—Perturbations, occasioned by the influence of polar auroras, in the march of the horary variations of the magnetic declination at places where the polar aurora is not visible.—Simultaneity of perturbations of the declination (magnetic storms), proved by corresponding observations at Paris and Kasan, at Paris and Berlin, at Paris, Berlin, and the mines of Freiberg in Saxony.—Observation of the deflection by the approach of a magnet, of the jet of light which unites the two charcoal points of the conductor in a closed electric current; analogies which this experiment presents to the phenomena of the aurora borealis.—Discovery in 1827 of the horary variation of the magnetic inclination and force.

V. *Part relating to Meteorology and the General Principles of Atmospheric Physics.*

Determination of the specific weight of atmospheric air, made conjointly with M. Biot.—Experiments made with M. Dulong, for the purpose of establishing that Mariotte's law undergoes no essential variation up to the pressure of twenty-seven atmospheres, and much beyond.—Dangerous experiments made with the same physicist (M. Dulong), on the elastic force of steam at very high temperatures.—Table of the elastic force of steam, and corresponding temperatures.—Formation of halos, and polarised light which halos reflect.—Cyanometer.—Optical researches on the causes of the colour of the water of the sea and of rivers.—Cold produced by evaporation.—Researches on the quantity of rain falling at different heights and at different places.—Explanation of the injurious effects which have been attributed to the moon under the name of the "*lune rousse.*"—A very extensive memoir on thunder, and different kinds of lightning, enlarged by numerous additions dictated by M. Arago, during his last illness, to a learned and devoted secretary, M. Goujon, a young astronomer attached to the Paris Observatory, who in the same manner wrote the "Astronomie Populaire" to the dictation of his illustrious master. — Experiments on the velocity of sound, made in 1822 between Montlhéry and Villejuif, conjointly with Messrs. Gay-Lussac, Bouvard, Prony, Mathieu, and Humboldt, assisted by the artillery of the Garde Royale.

VI. *Part relating to Physical Geography.*

Level of seas. — Thermometric state of the globe. — Temperature of the surface of the sea in different latitudes, and of the sea water in successive strata down to the greatest depths. — Currents of warm and cold water. — The waters of the ocean compared to the atmosphere above them in respect to temperature. — Colour of the sky and of clouds at different heights above the horizon. — Neutral point of polarisation in the atmosphere. — Use of a plate of tourmaline cut parallel to the faces of the prism, for seeing shoals and the bottom of the sea. — Temperature of the air at the North Pole.— Mean temperature of the interior of the earth, at depths accessible to man; (observations made on the temperature of wells, bored to different depths, have conducted to the law of the increment of heat in descending below the surface of the earth).

Such is the outline, — very incomplete, notwithstanding the vast treasures which it contains, — of the labours of M. Arago. They have raised him to the rank of one among the most eminent men of the nineteenth century. His name will be honoured wherever respect for services rendered to science, a just sentiment of the dignity of man and the independence of thought, and the love of public liberty are preserved. But it is not alone the authority of a mighty intellect which has given to M. Arago the popularity which he enjoyed: the conscientious zeal which failed not when death approached, the desperate efforts to fulfil up to the last moment the most minute duties, have contributed to the honour in which his name is held. Nor should the charm of his diction, the amenity of his habits and manners, and the kindliness of his character be forgotten. Capable of the most tender devotion, the vivacity of his ardent mind and disposition always tempered by its natural sweetness and kindness, M. Arago enjoyed, in the midst of an intelligent and affectionate family, the peaceful happiness of domestic life. In the surviving circle of those most dear to him M. Arago found all that a touching assiduity, the exercise of intelligent foresight, and the most tender and inventive zeal could offer to soothe and alleviate during the slow exhaustion of his strength. He died surrounded by his sons,—a sister, Madame Mathieu, worthy of the tender affection of such a brother, —and a niece, Madame Laugier, who gave to him the most unremitting and devoted care, and who, at the last moment, showed no less fortitude in grief than she had done nobleness in her entire and touching self-devotion.

At a distance from M. Arago's bed of suffering, I could only testify from afar my deep affliction. Even the certainty of the near approach of this loss could not lessen its bitterness. As a last homage to the memory of him who has been so recently taken from amongst us, I subjoin here a few lines which have been already published elsewhere:— "That which characterised this man of unique mould was not alone the powerful genius which originates new ideas, and renders fruitful those already attained, or that rare lucidity which can describe recently discerned and complicated departments of discovery as clearly as if they had been the long acquired possessions of human intelligence; it was also the engaging combination of the force and elevation of an impassioned character with the affectionate sweetness of a more gentle one. I am proud of thinking that, by my tender devotion, and by the constant admiration to which I have given expression in all my works, I have belonged to him for forty-four years, and that my name will sometimes be pronounced by the side of his great name."

ALEXANDER VON HUMBOLDT.

Potsdam, November, 1853.

METEOROLOGICAL ESSAYS.

THUNDER AND LIGHTNING.

I HAVE often been consulted on the subject of lightning conductors, by architects charged with the care of public buildings; by officers of the corps to which rightfully appertains the construction of powder magazines; by the commanders of ships both of war and of commerce; and by a great number of citizens of all classes of society. I may therefore be permitted to affirm that, generally speaking, physicists by profession are the only persons who have a true and exact idea of the preserving properties of this kind of apparatus. If lightning conductors are asked for and erected, it is simply out of deference to the decisions of academies. Every one desires, by this means, to shelter his own responsibility under the Ægis of Science; but as to an entire conviction of the efficaciousness of the method, this, I think, will be rarely found. Some do not go beyond doubts, and wait before pronouncing an opinion until real demonstrations shall be offered to them, instead of mere analogies. Others, comparing the apparent insignificance of the preservative with the vastness of the possible damage which it is designed to avert, declare that it is repugnant to their reason to admit that a slight metallic rod can suffice to shelter a great edifice, or a majestic ship, from the dreaded thunder-stroke. In their view, these rods, which shoot up into the air with such high-sounding names and lofty pretensions, are really quite inefficacious either for good or for evil. Others give themselves up to an opposite set of ideas, and attribute to these metallic rods a powerful but injurious action. They say that it is deliberately calling down the thunderbolt on the buildings on which such rods are elevated, creating a peril which would not otherwise have existed, and endangering, at the same time, adjacent houses, by inviting the descent of the storm-cloud which might else have passed on, and discharged its contents harmlessly at a distance.

Frederick the Great of Prussia tacitly ranged himself among those hostile to Franklin's invention when, whilst yielding to public opinion and to that of the Berlin Academy by permitting lightning conductors to be erected on his barracks, arsenals, and powder magazines, he expressly forbid their being placed on the palace of Sans Souci.

The doubts and difficulties alluded to have struck deep root in many minds. In reflecting on the best means of eradicating them and augmenting the number of enlightened partisans of the use of conductors, it occurred to me that it would be advisable to separate observation altogether from theory; and that the safest and most reasonable proceeding would be to analyse the well-authenticated effects of lightning, and to try to deduce from them general inferences, without borrowing anything analogically from the electric experiments of physical philosophers. I thought it, in short, advisable to become, in the first instance, the exact historian of even minute details relating to the meteor itself, being ready afterwards, if it appeared desirable, to seek amongst phenomena which on a far smaller scale either surround us in ordinary life, or which we have learnt to produce in our laboratories, for points of contact or comparison which might prove more or less fruitful in inferences. This was the plan of operation which I had formed for myself when I first announced the publication of a notice or memoir on the phenomena of thunder. I imagined that all the requisite elements would have been found in modern treatises on physics, and that I was only engaging in a short work, and incurring merely the obligation, first, of collecting facts, constant in their occurrence, definite in their features, and having well-marked distinctive characters; and next, of co-ordinating them according to a particular arrangement and method suited to the object which I had in view. So far from this proving to be the case, I have been obliged to have recourse to the original authorities for my facts, and to look over many hundred volumes of our Academy of Sciences, of the Journal de Physique, of the Philosophical Transactions of London, those of Berlin, &c. &c.; and to do the same by a multitude of other works, memoirs, narratives of voyages and travels ancient and modern, &c., written for the most part without method, clearness, or definite object; in short, to read everything that might offer the hope, often disappointed, of discovering among a thousand useless details one fact or remark which might be available for science.

Some persons, I am aware, have been of opinion that I was committing a grievous error in selecting such a subject for one of my "Notices;" because they considered that it had been utterly exhausted by Franklin and the numerous physicists who had followed up or emulated his researches; and more particularly by justly celebrated Academic Commissioners who had been from time to time appointed both in Paris and London to report officially, for the information of the authorities, respecting the use of lightning conductors. Far from having been led to concur in this opinion, the effect of my own laborious inquiries has been to make me dissent from it more and more. So far from the question having been previously exhausted, I consider that after all the pains which I have myself taken, the most to which I can pretend, is to have supplied for the future history of thunder and lightning a sort of "canvas," to be gradually filled up, by the arrangement in their appropriate places of facts with which meteorology has still to be enriched. Notwithstanding the many observations, forgotten or overlooked, which I have been able to bring again to light, and to group in systematic order, it is by the vacancies still requiring to be filled up, and which so far from disguising, I have desired to point out, that I deem my memoir may prove more especially serviceable. May it be the means of inducing travellers and meteorologists to regard this formidable meteor as a rich subject of study! The fulfilment of this wish would amply repay the time and care which I have bestowed on the subject.

CHAPTER I.

DEFINITIONS.

In conformity with usage I should wish to begin this notice with fixing the signification of the words "foudre" and "tonnerre" (thunderbolt or lightning, and thunder). But good definitions are not always attainable either by all persons or on all subjects. I will, therefore, take legal definitions, or those which the French Academy has recorded in its new dictionary.

"*Foudre.* Le feu du ciel, la matière électrique lorsqu'elle s'échappe de la nue en produisant une vive lumière et une violente détonation."

Lightning. Fire from the sky, electric matter escaping from a cloud and producing a vivid light and violent explosion.

"*Tonnerre.* Bruit éclatant causé par l'explosion des nuées électriques."

Thunder. The loud noise caused by the explosion of electric clouds.

It may be very true that persons difficult to satisfy, or scrupulous in such matters, might still find something to blame in these few lines. They would be entitled to ask whether the learned, technical, and modern word electricity is suitable in the definition of a phenomenon as old as the world, and which had been known as the cause of so many fatal accidents before physical philosophy had caught the first glimpse of any of the rudiments of electrical science. Fault might also be found with what is problematical or theoretical in both the definitions,—for instance, with the words "explosion des nuées," which can in no way be attached to any of the eight or ten hypotheses which have been proposed to explain the rolling of thunder. But what result could be drawn from such reflections? possibly that the honourable authors of the dictionary had in the case before us been less happy than usual. Well, then, it would still have to be shown whether anything better could have been done. Suppose we should say that the "foudre," "thunderbolt," or "thunder and lightning," is "a meteor or meteorological phenomenon, which, when the sky is covered by a particular kind of cloud, *manifests itself by a sudden dart or flash of light,* followed, after a greater or less interval, *by a noise more or less prolonged.*" Such a definition would escape most of the criticisms noticed above, inasmuch as it contains nothing hypothetical, nothing borrowed from the modern experiments of physicists; nothing which is not the result of simple and direct observation; but perhaps on reflection other objections might be found. We have, however, to remark in particular that the word "tonnerre"—"thunder," the direct signification of which is, noise, an explosive or rolling noise, is so often taken for the equivalent of the word "foudre," (as in the expressions; "le tonnerre est tombé," "frappé de tonnerre," "feu du tonnerre," &c.,) —that the two expressions have come to be employed indiscriminately, even in cases where some confusion, or, at least, want of clearness of style, may be occasioned thereby. Our best authors do not commit this fault; as may be shown by the often-quoted phrase of one of our greatest prose writers: "Le ciel a plus de tonnerres pour épouvanter qu'il n'á de foudres pour punir." (Heaven has more thunders to alarm than thunderbolts to punish.)

CHAP. II.

EXTERNAL CHARACTERISTICS OF STORM OR THUNDER-CLOUDS.

In ordinary discourse clouds are regarded as a symbol of variability and vagueness of form. "Changing," or "changeful as the clouds," is a proverbial expression. Yet we are about to examine, with those who make meteorology an object of study, whether the clouds in the midst of which the thunderbolt is born and nursed, from whence issue dazzling jets of light and detonations louder than those of artillery, are not distinguished from ordinary clouds by some peculiar features, constantly attaching to them, and easily recognised.

Among these distinctive features I will mention in the first instance a kind of "fermentation" to which storm clouds appear to be exclusively subject. An English physicist, Forster, compared this fermentation to the movement observed on the surface of a "piece of cheese full of mites, which seems agitated in every point, without ever materially changing its place."

When in calm weather we see that there begin to rise somewhat rapidly, at some point of the horizon, very dense clouds, resembling heaped-up masses of cotton, terminated by a great number of well-defined rounded contours, almost as sharply marked as would be the summits of dome-shaped mountains covered with snow; when these clouds appear as it were to expand or swell out, diminishing in number as they increase in size; when, notwithstanding all these changes of form, they remain constantly attached to their first base; and finally, when these contours, which at first were so numerous and so distinct, have gradually melted into each other so completely, that the whole presents the aspect of only one single cloud, then, according to Beccaria, we may announce with certainty the approach of a thunderstorm.

To these preliminary phenomena there succeeds, still on the horizon, the apparition of a very dark cloud which seems to touch the earth and connect it with the clouds which have just been described. The dark tint spreads gradually to the higher clouds; and it is worthy of remark that it is at this stage that their general surface, or at least that which is seen from the plain, becomes more and more uniform. From the highest parts of this single and compact mass, there spring long branch-like clouds, which, without detaching themselves from it, gradually overspread the sky.

At the moment when these branches begin to be formed, there are usually seen numerous scattered, hovering, small white clouds, very distinct and with very well defined edges, to which the celebrated physicist of Turin gives the name of *ascitizi*, or additional, or subordinate, clouds. Their movements are sudden, uncertain, and irregular. They appear to be under the attracting influence of the great mass of cloud, and gradually, one after another, float towards it and join themselves to it. These "ascitizi" had already been remarked by Virgil, who compared them to tufts of wool. They are the white patches which are seen to interrupt here and there the uniform dark surface of a great storm cloud.

When the great dark cloud has increased so as to pass the zenith, and overspread the greater part of the sky, the observer sees beneath it many small ascitizi, without being able to discern where they come from or how they have formed. These ascitizi appear torn or rent, or as it were ragged fragments of cloud. They throw out here and there long arms. Their march is rapid, irregular and uncertain, except that it is always horizontal. When in their opposite movements two of these clouds happen to approach each other, they appear to extend towards each other their irregular arms; after having almost touched, an evident repulsion takes the place of the previous apparent attraction, and the same arms which had been outstretched to meet, now turn away from each other.

The above remarks are the substance of what has been said on the subject by an author (Beccaria), who lived in a country (the plain around Turin) almost entirely surrounded by high mountains. In order to judge how far they apply generally and how far only locally, we ought to be able to compare with them the description of the commencement, progress, and full development of a storm in a country without mountains.*

No one will doubt that there is something local in some of the circumstances attendant on the formation and development of storm clouds, in reading the following description, given by M. Antoine d'Abbadie, of thunder-clouds of frequent occurrence in Abyssinia.

"Storm-clouds in Ethiopia have," he says, "always a plain or uniform surface on their inner side, and a checkered surface on their outer side, and are, generally speaking, far from being very dense; sometimes clouds in which strong manifestations of electricity take place are so thin that stars can be seen through them." M. d'Abbadie remarks further that these clouds have a tendency to cluster round lofty peaks, by which they appear to be attracted.

Let us add to these different remarks that storm-clouds are often diverted from the direction in which the wind would carry them to follow the course of rivers. Mr. Sturgeon mentions having often observed this to take place at the junction of the Thames and Medway.

In all that Beccaria has said of the gradual disappearance of the many and great undulations in storm-clouds as they rise from the horizon to the zenith, he spoke only of their *under surface,* as alone visible from his observatory at Turin. I should be unable to speak of their upper surface, if I had not thought of consulting on the subject officers of the État Major, formerly pupils of the École Polytechnique, who having been recently engaged in covering the Pyrenees with their admirable net-work of triangulation, were likely to have had frequent opportunities of seeing thunderstorms beneath them.†

* St. Lambert, in his poem "Les Saisons," begins the description of a thunderstorm by two lines, in which he speaks of clouds rising from two opposite points of the horizon:—

> "On voit à l'horizon de deux points opposés
> Des nuages monter dans les airs embrasés."

Is he describing a local phenomenon?

† I have to address my especial thanks to two officers of high merit,

From them I learned, that even when a stratum of clouds appears perfectly smooth and uniform on its under surface, its upper surface consists wholly of high protuberances and deep cavities.

Captain Hossard pointed out to me one sign precursive of a thunderstorm which has not, I think, been mentioned by any previous meteorologist. He had remarked that during great heats, there take place suddenly at several points of the lowest stratum of clouds upward rushings, extending vertically like rockets, which may bring distant parts of the atmosphere into direct communication.*

Franklin went further than Beccaria in one respect: he considered that a single large cloud cannot become a thundercloud; that when this appears to be the case, an observer placed in the horizontal prolongation of the large cloud from whence lightning and thunder issue, would always see beneath it a series of very small clouds one above another; the lower ones sometimes approaching very near the earth.

Thus, according to Franklin, two conditions are requisite for a cloud to produce thunder and lightning: it must itself be large, and there must be moreover small clouds interposed between its under-surface and the earth. But is it quite certain that lightnings never dart from a single small cloud? I propound the question simply as one of fact, not at all as a question of theoretical possibility. Well then, to the question of fact, most meteorologists have replied with the American philosopher, in the negative. I may cite, for instance, the great

Captains Peytier and Hossard, from whom I have received notes equally remarkable for exactness of observation and for the knowledge in physical science implied in them.

* Captain Peytier has noticed that in certain localities the thunderstorms which burst on the mountains seem to have for their germ, if I may use the expression, fragments of clouds formed over the low country, or detached from a wide-spread canopy of clouds which had previously overhung the surrounding plains. He remarks, that an observer placed on one of the peaks overlooking Roussillon or Gascony — on the Canigou or the Pic du Midi de Bigorre, for instance, — would see in the forenoon, some hours after sunrise, clouds form over the plains, often rising rapidly and clustering sometimes round one and sometimes round another of the mountain summits, where they usually give birth to a thunderstorm. On mornings when the plain was already overspread, fragments of the pre-existing clouds would become detached here and there, some earlier and others later, and when a considerable number of such fragments had gathered round one of the summits of the chain of mountains, the storm would burst.

name of Saussure. I find on this subject the following passage in the narrative of his celebrated *Voyage au Col du Géant:*—

"I have never seen thunderstorms take place in these mountains except by the meeting or conflict of two or more clouds. During our stay on the Col du Géant, so long as we saw in the air or on the summit of Mont Blanc only a single cloud, however dense or however dark it might appear, no thunder ever came from it; but if two strata were formed one above another, or if clouds rising from the plains or the valleys approached the clouds which hang on the summits of the mountains, their meeting announced itself by violent gusts of wind, thunder and lightning, hail and rain."

There are physicists,—and among them Saussure assuredly occupies one of the foremost ranks,—whose observations are to be admitted almost without examination or discussion in regard to *positive* facts; but in regard to *negative* facts such implicit faith would be a great fault. For we ought to comprehend the possibility that the rare and fortuitous circumstances, under which alone some particular phenomena can be developed, may never have presented themselves to an observer, however eminent. I have, therefore, without being discouraged by Saussure's assertion, sought in old meteorological journals, which are certainly far from deserving the contempt with which it is too much the fashion to speak of them, whether *small isolated clouds* have never been observed to send forth thunder and lightning. The pains I have taken have not been fruitless.

I read in a Memoir of the Academician Marcorelle of Toulouse, that on the 12th of September, 1747, the sky being perfectly serene and clear, with the exception of a small perfectly round cloud about a foot and a half in diameter, thunder was suddenly heard, and a woman of the name of Bordenave was killed by a thunderbolt, by which she was burnt on the bosom without her clothes having been injured.

I find in the Botanico-Meteorological Observations made at Denainvilliers, near Pithiviers, a note by M. Duhamel du Monceau, dated 30th of July, 1764, which is also conclusive, and which I will transcribe:—

"At half-past five in the morning, in bright sunshine, there passed a small *solitary cloud.* There issued from this cloud a thunderbolt, which struck an elm-tree very near the Chateau de Denainvilliers; tore off a strip of bark from two to four inches wide from a height of rather more than twenty feet above the ground to the root of the tree; and ploughed in the wood a furrow the size of a finger in breadth and depth, at the bottom of which there was a black, thread-like line where the wood appeared cracked or split further in. At the same moment, a sulphurous smell, which created great alarm, was perceived at a neighbouring farm-house."

Bergman saw himself "the lightning dart to a church-steeple from a very small cloud, the sky being otherwise perfectly clear."

I hope small clouds may be definitively reinstated in their rights when I shall have added to the above a fourth observation, which I owe to Captain Hossard.

In 1834, that officer, in descending by the path which passes over the Col de la Faucille in the Jura, saw a small cap of cloud form round a neighbouring summit, called the Colombier de Gex, of which the height above the sea is 1,600 metres, or 5,250 English feet. The cloud had scarcely existed a few seconds when a strong thunder-clap was heard from it.

Although the above discussion is certainly not calculated to increase our faith in negative facts, yet I may add that, according to Beccaria, thunder and lightning never issue from *smoky* clouds; that is to say, from those strata of clouds which are characterised by their apparent uniformity of composition and regularity of surface.

We will here close this chapter. At a future, and, perhaps, not distant, day, there will doubtless be data on the subject of which it treats more definite, precise, and full than those at my disposal. It is a subject which is certainly well deserving of the attention of meteorologists, and will afford to those who will not hesitate to bestow assiduous observation on things so variable, changing, and inconstant as the clouds, a valuable harvest of facts useful to science.

CHAP. III.

THUNDER AND LIGHTNING OF VOLCANIC CLOUDS.

Lightning is sometimes elaborated and manifested in clouds whose nature is apparently quite different from that of our ordinary atmospheric clouds.

The younger Pliny wrote to Tacitus two well known letters on the subject of the Eruption of Mount Vesuvius, which in the year 79 of our era occasioned the death of his uncle, Pliny the Naturalist. In the second of these letters, the younger Pliny speaks of black and dreadful clouds (they were clouds of volcanic ashes), rent by serpentine fires (this would be a just description of some flashes of lightning in ordinary storms); of "clouds which *opened* and emitted long furrows of flames, resembling lightnings."

Many quotations to the same effect might be made from the writings of Padre della Torre. To take a single instance, in the description of the eruption of Vesuvius in 1182, we find that "a very dense (densissimo) smoke lasted from the 12th to the 22nd of August, and that lightnings (saette), were often seen darting in the midst of the smoke."

Bracini, who was an eye-witness of the eruption of Vesuvius in 1631, said that the column of smoke which rose from the crater extended in the atmosphere to a distance of nearly 90 geographical miles, and that frequent lightnings issued from this kind of cloud and killed several persons and several animals.

During the eruption of Vesuvius in 1707, Giovanni Valetta wrote from Naples to Richard Waller, "On the third and fourth days the crater of the volcano sent forth lightnings, similar to some kinds of sky lightning. They were tortuous, serpentine, and followed by thunder-claps. The frequency and intensity of the thunder and lightning caused it at first to be believed that heavy rain was at hand, but it was afterwards perceived that they originated in a dark cloud composed not of ordinary vapour but entirely of ashes."

The peasants who dwelt at the foot of Vesuvius told Sir William Hamilton, after the eruption of 1767, that they had been far more terrified by the incessant flashes and frequent strokes of lightning, than by the burning lavas and other menacing phenomena which are the constant attendants of a volcanic eruption.

During the terrible eruption of 1779, there issued from the crater of Vesuvius, together with the fiery lava, frequent bursts of smoke, "as black as can possibly be imagined," which smoke Sir William Hamilton said seemed to be crossed by zigzag flashes of lightning at the very moment of its escape from the crater.

The eruption of Vesuvius in 1794, so well described by the same observer, afforded indications no less positive. On the 16th of June, nothing visibly ignited appeared above the crater; black smoke and ashes were all that issued from it, and these formed over Vesuvius a gigantic cloud, from which darted those zig-zag lightnings so well known to meteorologists, and which the inhabitants of the neighbourhood of Vesuvius call *ferilli.*

The volcanic lightnings seen by Hamilton, in 1799, were not accompanied by any audible discharge or thunder. In 1794, on the contrary, they were always followed by discharges resembling the most violent thunder-claps; the storm, formed solely by the influence of the volcano, was in all respects similar to ordinary thunderstorms. The accidents caused were the same, and in particular the examination of the house of the Marchese de Berio, at San-Jorio, struck by the volcanic lightning, showed the perfect similarity between the effects of volcanic and ordinary atmospheric thunderstorms. The volcanic cloud was in very great part composed of ashes as fine as Havannah snuff: it was carried by the wind as far as the town of Tarentum (nearly 200 geographical miles from Vesuvius), where a flash from it struck and partially destroyed a house.

I have hitherto spoken exclusively of the eruptions of a single volcano, and though I have little fear that any of my readers would be inclined to suppose that the clouds of smoke and ashes from Vesuvius have the exclusive privilege of forming lightning, yet I will proceed to cite other instances.

My first quotation shall be from Seneca. In the Nat. Quæst. lib. ii. § 30. I read that during a great eruption of Etna the rolling of thunder was heard, and lightnings were seen in the midst of the clouds of burning sand emitted from the volcano.

My second quotation is from the Descrizione dell' Etna del Abate Francesco Ferrara.

In the beginning of the year 1755 there rose from the crater of Etna an immense and very black column of smoke, which was frequently traversed by tortuous lightnings (tortuose balenazioni).

When the short-lived small island called Sabrina was upheaved near St. Michael, one of the Azores, in 1811, Captain Tillard observed that the most dark and opaque portions of the exceedingly black columns of dust and ashes which rose from the ocean, were furrowed by lightning of extraordinary intensity.

Even the little volcano which appeared in July, 1831, between Sicily and Pantellaria, furnishes an instance to the point or which we are speaking. We learn from John Davy that on the 5th of August there rose from time to time, to a height of 3000 to 4000 English feet, columns of perfectly black dust, from which flashes of lightning, followed by thunder, darted in different directions.

It may be thought that I have given far too much importance to the thunder and lightning produced in volcanic clouds. I know that it may be said that immense columns of steam or aqueous vapour often rise from the craters of volcanoes; that it is this vapour which constitutes the principal part of the volcanic cloud, and that the ashes and black impalpable dust mixed therewith only have the effect of altering its whiteness, semi-transparency, &c. &c.

My answer is a simple one. Supposing it to be true that the excessively black cloud which, first rising from the crater to a prodigious height, and then spreading out laterally in every direction from the ascending column, gives to its falling substance of mingled vapour and dust or ashes the appearance of the drooping branches of a gigantic pine tree, so well described by the younger Pliny and by modern observers — supposing it, I say, to be true, that this cloud consists, in very great measure, of the vapour of water,—we should still have to inquire why this aqueous vapour or steam, when it rises from a crater in a nearly pure state never, or scarcely ever, if I am rightly informed, gives birth to thunderstorms, and why when mixed with ashes or with volcanic dust it always does so. Nothing, moreover, has, I believe, determined the reality of the above supposition, taken in a general point of view. There is nothing to prove, for instance, that the thick black cloud which, in 1794, extended from Vesuvius to Tarentum, did not consist on arriving at that town exclusively of impalpable dust. According to the account of Captain Tillard, black columns of smoke rose from the ocean near the Azores before the small island of Sabrina had begun to appear above the surface of the sea. In this case must not the steam formed at the submarine volcanic hearth have been in great measure condensed during its ascent through the sea as, in Watt's admirable machine, the steam is condensed by the contact of cold water? I will not press these considerations farther at this moment, but I shall presently cite a fact which will give them great weight, since it will prove that volcanic dust, after separation from the volcanic cloud, and on reaching the ground in a state of extreme dryness, is sometimes so strongly impregnated with the lightning-matter as to give rise to remarkable phenomena of phosphorescence.

CHAP. IV.

HEIGHT OF STORM-CLOUDS.

I SHALL have occasion to explain in the sequel, that in striking certain kinds of rock, lightning produces local phenomena of fusion and vitrification well known to observers. These superficial vitrifications of small and well defined extent have been remarked by my illustrious friend M. de Humboldt on the culminating part of the principal summit of the mountain of Toluca (west of Mexico), at the height of 4,620 metres, or 15,158 English feet, above the level of the sea; by Saussure, on the summit of Mont Blanc, at 4,810 metres, or 15,780 English feet elevation*; by Ramond, on Mont Perdu, 3,410 metres, or 11,188 English feet; and on the Pic du Midi at 2,935 metres, or 9,630 English feet. Would not these facts be thought by most persons to warrant the inference, that storm-clouds rise sometimes, at least in mountainous countries, to elevations exceeding —

In Mexico	-	4,620	metres, or	15,158	Engl. ft.
In Switzerland	-	4,810	„	15,780	„
In the Pyrenees	-	3,410	„	11,188	„

The inference would be true, as we shall see presently, but by no means strictly warranted by the premises: the supposed demonstration is inexact, for it implies, as an essential con-

* To speak more accurately, I ought to say that the superficial vitrifications which are the certain marks of lightning were perceived, not on the summit of Mont Blanc itself, but on that part of the mountain, but little inferior in height, which is called the Dome de Gouté. The traces on the summit of Mont Blanc which Saussure considered to be the indication of a recent thunder-stroke, consisted of fragments of rock lying in all directions on new fallen snow several feet from their original place.

dition, the truth of the common opinion, adopted without reflection, that lightning always darts downwards from the clouds. I am about to cite a fact which proves the actual occurrence of an opposite motion. We shall see that different objects have been struck and injured by lightning from clouds much below them.

We can therefore only look for the most part for certain determinations of the greatest elevations attained by storm-clouds to the narratives of travellers among the summits of the principal chains of mountains of either continent, and I have accordingly drawn from this mine the following instances.

Bouguer, in his work on the figure of the earth, speaks of a thunderstorm in which he and La Condamine were overtaken on the Pichincha, one of the summits of the Cordillera of Peru; elevation, 4,868 metres, or 15,970 English feet.

On the 5th of July, 1788, the day after their arrival on the Col du Géant, the two Saussures, father and son, were assailed there by a violent storm, during which the thunder and lightning was almost incessant. The height of the storm-clouds above the mountains was neither determined nor recorded by estimation. All therefore that can be said in this respect is, that they were decidedly above the height of the rock on which the tents had been fixed: 3,471 metres, or 11,388 English feet above the level of the sea.

One paragraph of the celebrated account given by these two great observers, in which they speak of storms produced on the summit of Mont Blanc whenever there were formed there two distinct strata of clouds, might authorise our adding a thousand metres to the above amount, or to affirm that on the Alps Messrs. de Saussure *saw and heard thunderstorms* whose seat was 4,500 metres, or 14,760 English feet, in vertical elevation above the ocean level.

Thanks to Captains Peytier and Hossard, we may include the Pyrenees in the present chapter.

In August, 1826, at the trigonometrical station of the Pic de Troumouse (height, 3,086 metres, 10,125 English feet), thunderstorms were formed in a stratum of clouds, of which the surface nearest to the earth was about 3,000 metres, or 9,840 English feet, above the level of the sea.

In the same month of the same year, at the Pic de Baletous, the lower surface of the storm-clouds was at 3200 metres, or 10,500 English feet.

In August, 1827, at the station of the Tuc de Maupas (height 3,110 metres, or 10,200 English feet), MM. Peytier and Hossard heard thunder-claps in clouds having their lower surface at 3,300 metres, or about 10,830 English feet.

It is thus assured that in the Andes, the Alps, and the Pyrenees, real and frequent thunderstorms take place at immense heights above the ocean. Are the thunderstorms which break over the plains ever equally high? This is a question of more than pure curiosity. If we suppose it answered in the affirmative, the density of the atmosphere would appear to be the sole determining condition. Take the contrary hypothesis, and there would be manifested an action of the earth, which, whatever might be the nature of the influence exercised, would be characterised by the remarkable fact, that the rising surface of a country raises the region of thunderstorms in correspondence with itself; so that it would appear as if a high table-land or mountain communicated to adjacent atmospheric strata, of a certain density, properties which they would not possess if the earth did not thus rise towards them. These reflections will suffice to show that the aim I had proposed to myself is not yet reached; before it can be attained I have still to seek the height of thunderstorms in level countries, or countries only a little raised above the surface of the sea!

When we are near a chain of mountains we may estimate the height of clouds by referring them to the summits or other determinate points which they may be seen to touch or to cover, and of which the vertical elevations are known by barometric or trigonometric operations. In the plains we may have recourse to an equally satisfactory method founded on the comparison of the time of the apparition of the flash of lightning, and that at which the sound of the thunder reaches the place occupied by the observer. I will point out presently the principles of this method; at present I am rather concerned with the results which it has afforded.*

* If these results are not more numerous we must throw the blame on the unfortunate habit which most authors of Treatises on Physics have had of presenting all problems as already solved, all questions as completely exhausted. Definitive assertions, of which the tone indicates that nothing more remains to be said where doubt ought to accompany every word, are

I find in a collection of Memoirs, by De l'Isle, Member of the Academy of Sciences, four observations made at Paris, on the 6th of June, 1712, at intervals of six minutes, which, being properly calculated, give for the *vertical height* of the clouds in which the thunder and lightning are produced the enormous elevation of 8,080 metres or 26,510 English feet! De l'Isle's Memoir contains seventy-seven observations, but after the 6th of June, 1712, none that can be calculated. By an inconceivable forgetfulness the angular height of the region in which the lightnings were seen is only given once.

The same omission is remarked in the observations collected by the Abbé Chappe, at Bitche, in Lorraine, in the course of the year 1757. Those made by the same observer at Tobolsk, in Siberia, in 1761, are more complete. I find from them the vertical height of the storm-clouds,—

July 2nd - - 3,340 metres, or 10,960 Engl. ft.

(the thermometer was at 21° Centigrade or 69°·8 Fahrenheit).

July 13th - - 3,470 metres, or 11,385 Engl. ft.

Two observations, made at Berlin, by the celebrated Lambert, on the 25th May and the 17th June, 1773, give for the height of the storm-clouds on those days:—

First observation -	1,900 metres,	or 6,234	Engl. ft.
Second -	1,600 „	5,250	„

The above determinations are too few to allow us to venture to derive from them any general conclusions. It is however a remarkable circumstance, that the greatest elevation of a storm-cloud of which we can find an account is from a country of plains, and, if De l'Isle was not mistaken, almost double the greatest height observed in the Alps. Observations of this

essentially injurious to the progress of science. To point out gaps which require to be filled up is even more useful than to record discoveries. It has been in trying to dispose of certain difficulties in the Newtonian theory of emission that several exact physical philosophers have been led to give an entirely new face to the science of optics. It has been by not yielding to the authority of those who insisted that there was "nothing more to be learnt respecting electricity and magnetism excepting that which it belongs directly to calculation to discover," that these two sciences have been enriched by an innumerable series of astonishing phenomena of which not the slightest idea existed a few years ago.

kind are however extremely easy, and the opportunities of making them sufficiently frequent; we may, therefore, confidently hope that, being once duly called upon, astronomers and meteorologists will hasten to supply the desideratum which I have thus pointed out.

Thus far I have been intent on noting the greatest elevations at which thunderstorms are formed. Unfortunately I find almost as great a paucity of documents if I seek to elucidate the question of ordinary elevations.

De l'Isle's observations, as I have said in a previous page, never being accompanied by any appreciation of the angular altitude of the flashes of lightning, are incapable of yielding more than limitary values. I subjoin those of least amount:—

	Metres.	E. feet.
In May a thunderstorm at Paris had a vertical elevation of *less* than - - - -	2,400	7,874
In June another storm had a vertical elevation of *less* than - - - - -	1,000	3,281
July 2nd, a third storm, vertical elevation *less* than	1,400	4,593
July 21st, a fourth storm, vertical elevation *less* than - - - - -	1,400	4,593

I do not see that it is possible to deduce from De l'Isle's observations any limits lower than the above.

Le Gentil, who staid some time in the Isle of France, at Pondichéry, and at Manila, stated confidently, from his own observations, that at those three tropical stations the lower stratum of clouds, in which *ordinary thunderstorms* are formed, is scarcely ever more than 900 metres (2,953 English feet) above the low grounds. Exceptionally, however, on the 28th of October, 1769, a storm at Pondichéry was at an elevation of more than 3,300 metres (10,827 Eng. feet).

Observations at Tobolsk give:—

	Metres	E. feet.
A case in which the storm-cloud may have had a vertical elevation of only -	214	702
A case in which the height was -	292	958
Six cases corresponding to elevations between - - - -	400 & 600	1,312 & 1,968
Three cases of clouds between - -	600 & 800	1,968 & 2,625
Five cases corresponding to elevations greater than - - -	800	2,625

Mr. Haindiger, a learned Austrian physicist, has recently published accounts of two cases, from which it follows that the clouds in which lightning is produced are sometimes much lower than would be supposed from reading the determinations given above.

On the 26th of August, 1827, a thunderstorm burst over the convent of Admont, in Austria, during vespers, and killed two young priests in the church choir. The cloud from which the lightning struck was only 8 metres thick, and its vertical height above the ground was not more than 28 metres, or 92 English feet.

The convent is in the valley; a chateau situated on the slope of the hill, 117 metres, or 384 English feet higher, was inhabited only by the care-taker and his wife. During the whole time that the storm lasted these two persons saw the cross on the belfry of the convent, which is 36 metres (118 English feet) high, rise above the upper surface of the bed of clouds, while the under surface touched one of the windows of the belfry, placed about 28 metres (92 English feet) above the ground.

Besides the bed or stratum of cloud just mentioned, and which covered the whole valley, there was another higher stratum, of which the exact elevation, according to points of comparison to which it could be referred and to which it corresponded, was about 732 metres (2,402 English feet). The interval between the two strata of cloud was therefore about 696 metres (2,284 English feet); the discharges took place from the one to the other, and almost always appeared to be from the lower to the higher cloud.

A remarkable thunderstorm occurred at Gratz, on the 15th of June, 1826. Within an hour, at the utmost, the lightning struck nine times, and five times set objects on fire.

The town of Gratz is built, as is well known, on the side of the Schlossberg; and the citadel stands on the top of the hill, which is 490 metres (1,608 feet) high. During the entire interval for which the storm lasted the citadel remained unobscured, and the sky above it always perfectly serene and blue, while the clock-tower of the Johanneun, 123 metres (404 English feet) below the citadel, was almost entirely immersed in cloud. By combining these various measurements, it was found that the vertical height of the upper surface of cloud was about 106 metres (348 English feet), and that of the lower surface about 70 metres (230 English feet); the thickness of the stratum of cloud being therefore 36 metres (118 English feet).

Being furnished with good chronometers and excellent means of determining angular altitudes, M. d'Abbadie did not fail to endeavour to determine the ordinary height of storm-clouds in Abyssinia, where his zeal for science had conducted him. The following were his principal determinations on this subject:—

Dates.			Height of Cloud above the Spot at which Mr. D'Abbadie was observing.	
15 February,	1844	-	2,036 metres	6,680 Engl. ft.
12 Feb.	1844	-	1,896 „	6,220 „
26 Oct.	1843	-	1,087 „	3,566 „
20 Oct.	1845	-	212 „	696 „

I have not collected all these numbers from idle curiosity: in the sequel they will be seen to occupy a place in the discussion of certain leading questions, much controverted among physicists, and will aid us in the inquiry whether lightnings always descend from the clouds to the earth, and whether, on the contrary, they do not sometimes remount from the earth to the clouds.

CHAP. V.

ON DIFFERENT KINDS OF LIGHTNING.

THE luminous phenomena of storms which we call lightnings are sufficiently dissimilar in form, and sufficiently varied in their properties, to induce me to divide them into several classes.

§ 1. *Zigzag Lightnings, or Lightnings of the First Class.*

The *first class* of lightnings comprises those which every one must be supposed to have seen, and which appear to consist of a vividly luminous line or furrow, very narrow, thin, and sharply defined.

These lightnings sometimes vary in colour. Meteorologists assert that they have seen them crimson, violet, or bluish.*

* Those who might be inclined at first sight to regard these remarks as entering into great minutiæ, will, I hope, form a different opinion when we shall have shown that these various hues are connected with the atmospheric

Notwithstanding their incredible velocity their course is not a direct one; most often, on the contrary, the lines described by them in space form sharply marked zigzags.*

I have read somewhere, but cannot at this moment find the passage, that after several zigzags the flashes of lightning having as it were doubled back upon themselves, returned towards the region from which they had originally darted.† This, which is only a very rare exception in the case of ordinary storms, is, on the contrary, a very frequent occurrence in volcanic clouds. I quote the words of Sorrentino on the eruption of Vesuvius in 1707.

"The inhabitants in the midst of the otherwise most profound obscurity, saw flashes of lightning (saette) darting on every side. The flashes which issued from the fiery mountain did not extend beyond Cape Pausilippo, where the cloud of ashes also ceased. From thence they turned back by the same path which they had first traversed, darting again towards and striking the furnace from whence they had issued."

Sir William Hamilton speaks on the same point with no less distinctness:—"These volcanic lightnings (those of the eruption of Vesuvius in 1799) very rarely quitted the black cloud of ashes which was advancing towards the town of Naples, and appearing to threaten its entire destruction: they *returned* towards the crater of the volcano and regained the ascending fiery column from which they had been seen originally to issue.

region in which the lightnings are produced, and when it shall be made evident that a simple appreciation of colour may in some cases be equivalent to several kinds of meteorological determinations made in the region of clouds.

* Howard had seen lightnings which after having completed their downward course returned, and in this retrograde, upward movement, retraversed a third, or even a half, of the interval between the clouds and the ground, and then again darted downwards and struck. I have not inserted the quotation in the text, because the learned English meteorologist speaks of the slowness with which these different movements are performed, and extreme rapidity is the characteristic feature of the lightnings of the first class.

† Might it not be affirmed that the ancients must also have remarked these strange retrograde movements? We find in the Natural History of Pliny this passage (speaking of omens):—"Nothing is more important than to observe from what region the lightnings proceed, and towards what region *they return*. *Their return* to the eastern quarter is a happy augury. When they both come from the east, the prime quarter of the heavens, and *return thither*, it is the presage of a sovereign felicity."

Once or twice only these lightnings (or *ferilli*, as the Neapolitans call them) fell on the Somma, and set fire to dry grass and bushes."

The singular retrograde movement of lightnings of the first class is shown with remarkable clearness in the following fact. M. l'Abbadie reports that he had seen, in Ethiopia, lightnings of the first class dart from an upper horizontal cloud to a second less elevated and similar to the first, and return again upwards, having described a track resembling a V.

It is not a rare circumstance for the lightnings of which we are now speaking to dart from one group of clouds to another; their more ordinary course, however, is from the clouds to the earth.

In the latter case persons have thought that they saw the lower extremity of the flash assume the particular form of the head of a dart. A far less doubtful point is, that these lightnings occasionally fork, sometimes into two, and sometimes even into three branches; thus a single luminous line shoots from the cloud, and after a certain length of course there are two or three perfectly distinct lines. The angular divergence of these is considerable; they strike points on the ground at some distance apart.

The Abbé Richard (author of the "Histoire Naturelle de l'Air et des Météores") supplies me with a strong case in point. He himself *saw* a luminous furrow leave the cloud singly, divide into two at some distance from the ground, and these two halves diverge, and separately strike two different objects.

When we have occasion to pronounce on the forms of phenomena of fortuitous occurrence, and of such transient duration as lightnings of the first class or kind, it is fortunate to be able to quote observers of so much merit as Nicholson. I hasten, therefore, to draw from a note by this celebrated physicist, found in the corner of a journal, and without any author's name, a few precious words, which I hailed with the more pleasure as the title of the note did not prepare me to expect them.

"On the 19th of June, 1781, a violent thunderstorm passed over the west end of London. I was at Battersea, and I remarked that the lightnings (which were attended by very well marked and distinct explosions) were in many cases *forked* at their lower, but never at their upper, extremities."

As cases of bifurcation are not common, we may naturally imagine how much more rare the separation of one flash of lightning into three distinct branches is likely to be. I thought, however, that I might affirm that this trisection did sometimes actually occur, from what I had found stated in the account of a thunderstorm published by William Borlase. The passage on which I thus depended is perhaps wanting in precision; but on the other hand it has the advantage of coming from an observer who had no system to support, and who even appears not to appreciate the importance of his own remark. I was, however, desirous to find a second example of the division of a flash of lightning into three, not admitting of any possible objection being made to it. Is it not well worthy of remark, that I have been obliged to have recourse, for this purpose, to volcanic clouds? I learn from the work of the Abbé Ferrara that on the 18th of June, 1763, there were formed on the southern slope of Etna, and at some distance from the summit, openings from whence issued vast globular volumes of a black smoke, mingled with ashes and burning dust. Well, these clouds were incessantly traversed by forked lightnings having three points ("da tricuspidali balenazioni").

One of my friends, who I had asked to oblige me by searching in Kaemtz's German Meteorology for some quotation which might be usefully added to the two preceding, tells me that he finds that that excellent observer once, but only once in his life, *saw* a flash of lightning divide into three.

Since this notice was first printed in 1837, I have received from M. Jean de Charpentier, the following account, which also relates to a flash of lightning of the first class, which, before reaching the ground, divided into three.

This triple or tricuspidate flash was observed at Freiberg on the 25th of June, 1794. The middle point struck a house situated on the open space near the cathedral; the branch on the south side set fire to a house in the suburb near a mill called the Stockmühle; and the third, or northern branch, passed above the town in a north-west direction and set fire to a cottage near the village of Klein Watersdorff. The house which was burnt near the Stockmühle was 1,195 metres, or 3,921 English feet to the south of the house which was struck near the cathedral; and from the latter to the cottage which was burnt, the distance was 2,600 metres, or 8,531 English feet.

In the letter which he did me the honour to write to me, M. de Charpentier cites the case of five trees standing some distance apart being struck by lightning while only a single clap of thunder was heard. From a combination of all the circumstances, he argues, with a good deal of probability, that to explain the fact we must admit the action of a five-branched flash of lightning. As, however, it is not impossible to account for the various effects in a different manner, I think it right to insist only on the preceding case, in which the learned naturalist and his father actually *saw* one flash of lightning branch into three.

I have omitted all the passages in which the ancient poets speak of triple pointed lightnings, and have only cited cases of bisections and trisections of single flashes on the authority of physicists who have been *eye-witnesses* of their actual occurrence. I could easily have gone much further and have found cases of branching into four, five, ten, &c. &c., if I sought for the indications of such cases in the effects produced by lightnings on reaching the ground. I should in such case cite for instance the careful investigation by Mr. Griffith, of the circumstances of the thunderstorm which on the 3rd of June, 1765, did much damage at Pembroke College, Oxford, whence it would seem to result that the lightning had entered the College at the same instant *at four different points* at considerable distances apart; and I should particularly insist on the circumstances of a thunderstorm which in April, 1718, ravaged the neighbourhood of Landerneau, and of St. Pol-de-Leon, when *twenty-four churches* were struck by lightning, although only *three distinct thunder-claps* were heard. But as I have said, I prefer at present to set aside all considerations more or less conjectural, and more or less liable to difficulties or objections; and to keep exclusively to phenomena which have manifested themselves by an evident and visible separation of a single line of light into two or more distinct lines.

The lightnings of our *first class* are characterised in Italy by a particular designation, "*saette.*" According to an opinion which prevails very extensively amongst ourselves, both with men of science and with the public generally, this kind of lightning (the saette of the Italians), *i. e.* narrow and zigzag lightning, is that which principally, if not exclusively, causes destruction or conflagration; in a word, these lightnings are exclusively entitled to the appellation of "foudre," or "thunderbolts."*

After the cases of bifurcation which I have cited, I ought to mention that M. Gamot (a former pupil of the École Polytechnique) has written to me an account of having himself seen, in October, 1838, lightnings proceeding from two very different points of a storm-cloud, unite, and descend to the earth united in one. He thought he could feel confident that it was not an ascending flash bifurcating before reaching the cloud.

§ 2. *Lightnings of the Second Class.*

We come now to lightnings of the second class (Sheet Lightning). The light of these, instead of being narrowed to bright sinuous lines or traces, almost without apparent breadth, spreads on the contrary over immense surfaces. It is neither so white nor so vivid as that of the fulminating lightning. It has (not unfrequently) an intense red tinge, and sometimes blue or violet predominate. When a sheet of lightning of this class is furrowed by a zig-zag lightning of the first class, the difference of colour becomes evident to the most unpractised eye.

The lightnings of the second class sometimes seem merely to illuminate the margins of the cloud from which they emanate; sometimes their bright light embraces or pervades the whole surface of those clouds, and indeed seems to issue from their interior: the clouds seem to open and disclose their bright interior; these are popular modes of expression, and I should seek in vain for any which should better depict the phenomenon.

Descriptions can be but very imperfectly successful in characterising meteorological phenomena; but in regard to the lightnings of which we now speak, readers who may find the above account insufficient, may be aided by being told that they are by far the most common of all. Many persons have never seen, or at least have never remarked, any others. During an ordinary thunderstorm, there may be as many thousands of them as there are single flashes of the narrow sinuous lightning of the first class.

* Seneca cut short the distinction his cotemporaries made between lightnings and thunderbolts; saying, lightnings are thunderbolts which do not reach the ground, and thunderbolts are lightnings which do reach it. (Quæst. Nat. liv. ii. § 21.)

§ 3. *Lightnings of the Third Class.*

If it is admitted that the name of lightning is to be given to every atmospheric light whose apparition coincides with the effects of thunderbolts, we are inevitably led to range some of these phenomena in a third class, exceedingly distinct from the two classes with which we have been hitherto occupied.

The lightnings of the third class differ from those of the two former by their time of duration, their degree of rapidity, and their forms. Every one has remarked that the sharply defined linear zig-zag lightning, and the broader superficial lightning with only vaguely defined contours, last but an instant. Observations, which we shall presently analyse, will show us how short is this instant: the resulting fractions of a second will appear astonishingly small. The lightnings of the third class, on the contrary, are visible for one, two, or even ten or more seconds of time. They move from the clouds to the earth with sufficient slowness for the eye to watch their march and estimate its rate. The spaces they occupy are well defined and distinctly bounded; their form must be nearly spherical, for seen from a distance the appearance is that of circular disks of light.

The spherical form which I here attribute to certain lightnings, or if the reader prefers, to certain luminous masses, which during thunderstorms are sometimes seen to traverse in different directions, and with different velocities, the space between the clouds and the earth, is so rare, that I deem citations in full from the accounts given by eye-witnesses indispensable. I have the less scruple in introducing the different examples which I have collected, because these fire-balls are at present a stumbling-block to estimable theoretical meteorologists, and because I think they may afford an explanation of those cases, of indeed very rare occurrence, in which good lightning conductors have proved inefficacious.

Before proceeding further I wish to meet an objection which would not fail to be urged by those persons (and they are many), who make their admission of a fact subordinate to the possibility of including it in known theories. This objection is the following:

Have these globes of fire or fireballs which you enumerate really existed? Was not the spherical form attributed to them an optical illusion? Would not a flash of lightning of the first class, supposing it to be cylindrical, in case of its direction being exactly towards an observer, appear to him to be circular or globular?

This objection would have weight if the spheroidal form had only been seen by those who being *exactly* in the path of the lightning would probably have been struck by it. But an observer placed out of the lightning's path, viewing it transversely, and seeing it strike a house or other object near or at a distance, cannot be led to attribute to it the form of a globe, unless it be really globular. The circumstances here supposed were almost always those of the subjoined examples, and the objection need not therefore engage our attention further.

CHAP. VI.

FORMER EXAMPLES OF LIGHTNINGS OF THE THIRD CLASS OR GLOBES OF FIRE.

§ 1.

M. Deslandes collected with great care for transmission to the Academy all that had been observed in Brittany during the celebrated thunderstorm of the night 14th—15th April, 1718. At Couesnon, near Brest, among the ruins of the church which had been entirely destroyed, different witnesses agreed in attributing the catastrophe to "three fiery globes, each three or four feet in diameter, which united, and then proceeded with a very rapid course in the direction of the church."

§ 2.

In March, 1720, near Horn, during a most violent thunderstorm, a fireball was seen to fall, and after touching the earth, *rebound*, strike the dome of the tower, and set it on fire.

§ 3.

On the 3rd of July, 1725, near Aynho, in Northamptonshire, a shepherd and five sheep were struck by lightning and killed. During the greatest violence of the storm, the Rev. Joseph

Wasse *saw a fireball* almost as large as the moon, and *heard* the hissing sound of its passage through the air above his garden. During the same storm, a tradesman of the town *saw a fireball* as large as a man's head burst into four pieces near the church.

§ 4.

A thunder-stroke did much damage to a house at Dorking, in Surrey, on the 16th of June, 1750. All those who witnessed the event declared that they had seen in the air, *large balls of fire* round the house which was struck. These balls on reaching the ground, or the roofs of houses, split into a prodigious number of pieces, which were scattered in all directions.

§ 5.

In the account given by Mr. Borlase of a thunderstorm in 1752, which did much damage near Ludgvan, in Cornwall, he says that perfectly distinct *balls of fire* were repeatedly seen falling from the clouds towards the earth.

§ 6.

In January, 1770, a thunderbolt fell on the tower of Schemnitz, in Hungary. "Its form was that *of a globe*, and its size as large as a cask."

§ 7.

In the Isle of France, in 1770, the clouds were seen one evening to descend as low as 400 metres, or 1,312 English feet, as could be judged by the mountains near the port. Very heavy rain fell. "There was much lightning, but," said the Academician Le Gentil, "instead of being of the ordinary form, it consisted entirely of *very large globes of fire*, which appeared suddenly, and disappeared in the same manner, without any explosion."

§ 8.

On the 20th of June, 1772, while a thunderstorm rolled over the parish of Steeple Ashton, in Wiltshire, *a globe of fire was seen to hover* in the air above the village for a considerable time, and afterwards to fall perpendicularly on the houses, where it did much damage.

§ 9.

It would be difficult to have any better evidence than that of Mr. Nicholson for a phenomenon observed on the 1st of March, 1774, near Wakefield, which appears to me to belong to the class of those with which we are now occupied. After a violent thunderstorm, and when there only remained in the sky two clouds which were but little above the horizon, Mr. Nicholson saw *meteors*, similar to those called shooting stars, descend every moment from the upper to the lower cloud.

§ 10.

In September, 1780, previous to the thunderbolt by which he was thrown to the ground, and two of his servants were killed, Mr. James Adair, of East Bourn, in Sussex, saw *several balls of fire* fall from a large black cloud into the sea.

§ 11.

The thunderbolt which on the 18th of August, 1792, fell on the house of M. Haller, at Villars-la-Garenne, had traversed the village under the form of a globe of fire.

§ 12.

On the 14th of February, 1809, the ship-of-the-line "Warren Hastings," which had only been launched a few days before at Portsmouth, was struck *three times* in a very short space of time. On each occasion the lightning approached the masts under the form of a ball of fire.

§ 13.

I read in Howard's "Climate of London," that at Cheltenham, in April, 1814, a *ball of fire* darted from storm-clouds to a haystack, which it pierced through and through.

§ 14.

Luminous globes or balls are still more frequent in volcanic than in ordinary storms. During the eruptions of Vesuvius of 1779 and 1794, Hamilton and other observers repeatedly saw such globes, of very considerable magnitude, issue from the thick cloud of ashes, and burst in the air like the shells which in fireworks are filled with what are called "serpents." The flames which darted in every direction from these globes at the moment of their explosion always took a zigzag course.

§ 15.

After these notices of globular luminous masses with perfectly defined circumferences, I proceed to cite cases of others, which leaving fiery particles along their track, bear some resemblance to the rockets of our fireworks.

Thus Schübler, whose name is so well known to meteorologists, mentions lightnings observed by himself, which presented the appearance of a current of fire of the size of a man's arm, terminated by a *ball*, larger and more brilliant.

I am assured that Kaemtz has repeatedly seen the same phenomenon.*

§ 16.

The above quotations all relate to phenomena observed in the open air. They might have been made much more numerous if I had followed the thunderbolts into the interior of buildings, where they oftenest assume a globular form. I will only cite a few such facts, of, as it appears to me, undoubted anthenticity.

A short time after Philip V. had made his entry into Madrid, the palace was struck by lightning. The persons assembled at the moment in the royal chapel saw two *balls of fire* enter it. One of these balls divided into several smaller ones, which before disappearing, *bounded* repeatedly like an elastic ball.

On the 7th of October, 1711, during a thunderstorm, a large globe of fire fell in the midst of some of the inhabitants of Samford Courtney, in Devonshire, who were standing in the church porch. At the same instant four similar globes, but only the size of a man's fist, exploded withinside the church, and filled it with flame and sulphurous smoke. One of the pinnacles of the tower was grazed by the stroke.

§ 17.

The same day, in 1772, on which as mentioned in § 8, a

* Professor Muncke has reported that a vertically descending lightning, which appeared to be sixty or seventy yards long, was transformed under his eyes into a great number of small balls.

globe of fire was seen to hover over Steeple Aston, the reverend Messrs. Wainhousé and Pitcairn, who were in a room in the parsonage, suddenly saw appear, at the height of their faces, and at about a foot from them, a *globe of fire* of the size of a fist. It was surrounded by black smoke. In exploding it made a noise which might be compared to the discharge of several pieces of ordnance at once. Immediately afterwards a strongly sulphurous vapour spread throughout the house. Mr. Pitcairn was dangerously wounded; his body, clothes, shoes, and watch presented all the same appearances as those attendant on a stroke of lightning of the more usual kind. Different coloured lights filled the apartment, and were violently agitated to and fro.

(It should be said, although the circumstance is but little connected with the object of the present chapter, that Mr. Pitcairn thought he had seen the globe of fire in the room one or two seconds *after* feeling himself struck.)

§ 18.

The engraver Solokoff declared that the lightning which killed the physicist Richmann, in 1752, had a globular form.

§ 19.

In 1809, lightning entered the house of Mr. David Sutton, at Newcastle-on-Tyne, through the chimney. After the explosion, several persons saw on the floor, at the door of the drawing-room in which they were assembled, a globe of fire which remained stationary; it afterwards advanced into the midst of the room and broke into several fragments, which exploded in their turn, like the stars of a rocket.

§ 20.

In seeking in the sequel for the explanation of the spheroidal form which the matter of lightning affects under particular circumstances, we shall probably have to inquire whether this form is ever presented at sea. I reply beforehand to such a question, that on the 13th of July, 1798, the East India Company's ship, the Good Hope, being in 35° 40′ S. latitude and 44° 20′ E. longitude from Greenwich, was struck by lightning of globular form, which produced a most violent detonation, killed a sailor instantaneously, and seriously wounded another.

CHAP. VII.

GLOBULAR LIGHTNINGS.

When in preparing a notice on lightning for the Annuaire of the Bureau des Longitudes, in 1837, I was led to notice, as a distinct class, lightnings or thunderbolts of a globular form, remarkable, besides their peculiar form, for the slowness of their movements, I could only cite in support of the distinction thus drawn, a very small number of well-authenticated facts, the greater part of which are given in the preceding chapter. Since attention has thus been called to this remarkable form of the meteor, so great a change has taken place, that I am now only embarrassed by the difficulty of selection among the numerous circumstantial accounts which I have received.

I will first, however, notice three facts which were cited in 1837, and which on reflection have appeared to me to belong to the class of phenomena which it is the design of the present chapter to consider.

§ 1.

In a letter to Vallisnieri, dated 10th of September, 1713, Maffei gives an account of which the substance is, that having a short time before stopped at the Castle Fosdinovo, in the territory of Massa-Carara, to take refuge from a thunderstorm accompanied by a deluge of rain, he was received by the lady of the castle in a room or hall on the ground floor; and that while there, he and the Marquis of Malaspina saw suddenly appear, at the *surface of the pavement*, a bright flame or fire (fuoco), partly white, partly blue, — that it appeared greatly agitated, but without any progressive movement, and that it vanished as it had appeared, that is to say, quite suddenly, but after having acquired considerable size.

As it vanished, Maffei felt a peculiar kind of tickling sensation running down from the back of his shoulder; pieces of plaster detached from the vaulted ceiling of the hall fell on his head; and, lastly, he heard a crashing sound, different, however, from the usual rolling of thunder.

Must we not class the luminous meteor and explosion which thus took place at Fosdinovo with the phenomena of thunder and lightning? The same writer, Maffei, relates in a letter to Apostolo Zeno, that the thunder-stroke which on the 26th of July, 1731, struck the principal tower of Casaleone, and tore off the shield on which the arms of the town were carved, as well as several parts of the stone mouldings, and of which the noise was like that of a cannonade, &c. &c., had been *preceded* by the apparition of a great fire (*gran fuoco*), at a very small distance above the ground in the open space or *place* near the tower. This fact had not been witnessed by any known man of science, but rested on the evidence of the persons who lived on the "Place;" Maffei is therefore careful to mention that the Abbé Girolamo Lioni da Ceneda had stated that he had himself seen, near Venice, a very vivid flame appear two cubits above the ground and disappear, and that a dreadful noise was heard immediately afterwards.

I pass to an observation by the author of the "Histoire de l'Air et des Météores," which is fully as circumstantial as that of Maffei:—

"On the 2nd of July, 1750, being at three in the afternoon," says the Abbé Richard, "in the church of St. Michel at Dijon during a thunderstorm, I suddenly saw between the two first pillars of the great nave a flame of rather intense redness appear suspended in the air about three feet above the pavement of the church. This flame then rose to a height of from twelve to sixteen feet, at the same time increasing in size; it then continued to ascend, but in a diagonal direction, moving at the same time to a horizontal distance of several fathoms, until at about the height of the organ-loft it dilated and disappeared, with a noise as if a cannon had been discharged in the interior of the church." (t. viii. p. 291.)

§ 2.

M. Cusarens writes to me that during a violent thunderstorm in the month of September, 1823, he saw a globular thunderbolt strike a tree and produce all the ordinary phenomena of lightning, not omitting the odour which usually accompanies its explosions.

§ 3.

Mr. Steinmein communicates to me in a letter the following account of an observation of a globular thunderbolt in 1826 at Altona. He writes:—

"It was, I think, in 1826 that a thunderbolt burst on the house of one of my friends and colleagues at Altona, where I was then practising medicine; the house is situated about 100 or 130 feet above the Elbe. My friend, Dr. Van der Smissen, was walking in his drawing-room, when he at the same instant heard a thunderclap and saw an ignited mass of about the shape and size of a hen's egg appear near the wall and run along a plank of the floor, which was covered with a coat of varnish, as is usual in our town. The ball ran towards the door about as quickly as a mouse would have run the same distance; then, with an explosion, it leapt to the banisters of the stairs descending to the ground-floor, and there disappeared as it had come, without causing any damage or leaving any traces.

§ 4.

I received the following description of globular lightning from M. Hapouèle, a well-informed proprietor of the Département de la Moselle:—

"About 2 o'clock on a summer-day in 1837 (I cannot remember the precise date) I was standing in front of my stables, under the shelter of the projecting roof. Opposite to me, at some little distance, was the open door of a dwelling-house, and between it and the place where I was standing there was a large space covered with farm-yard manure. Suddenly I heard a violent clap of thunder, and saw a ball of fire of the size of a large orange descend a little obliquely, about forty feet from me, towards the middle of the heap of manure, which it seemed about to enter, but on arriving within three or four feet of the heap its downward course was exchanged for a horizontal one parallel to the ground, and directed towards the door which my wife had that instant shut. When arrived within forty or fifty feet of the house the electric ball resumed the oblique movement with which it had descended, and reascended slanting upwards towards the clouds, and passing within half a yard of the edge of the roof, to a height of fifty or sixty yards, when I lost sight of it.

§ 5.

Mr. Butti, marine painter to the Empress of Austria, addressed to me from Trieste the following communication:—

"In the year 1841, and if my memory does not deceive me in the month of June, I was staying at Milan at the Hotel del Agnello, in a room on the second floor looking over the Corsia dei Servi. It was near six in the afternoon, rain was falling in torrents, and the flashes of lightning lighted up the darkest recesses of the room better than gas does with us. The peals of thunder were exceedingly loud. The windows of most of the houses were closed and the street was deserted, for, as I have said, the rain fell so heavily that the road was changed into the bed of a torrent. I was sitting quietly smoking my cigar and looking at a distance through the open window at the rain, which being from time to time lighted up by a sunbeam formed threads of gold, when I heard in the street the sound of running feet, and several voices of men and boys calling out *Guarda! Guarda!* (Look! Look!) After half an hour's cessation of human voices or sounds, my attention was roused by this noise; I ran to the window, and turning my head to the right, from whence it proceeded, the first thing which met my view was a globe of fire at the level of my window moving in the middle of the street, not horizontally, but sensibly slanting upwards. Eight or ten persons, still calling out *Guarda! Guarda!* with their eyes fixed on the meteor, kept up with it by following at the pace which soldiers call accelerated. The ball of fire passed quietly in front of my window, so that I was obliged to turn my head to the left to look after it. The next moment, fearing that I should lose sight of it behind some houses which were not in the same line with the one in which I was, I hastened down stairs and into the street, which I reached in time still to see the meteor, and to join with the rest of the curious spectators who were following it. It moved still with the same slowness, but in its oblique upward march had already risen considerably, and in three minutes more it struck the cross of the steeple of the church Dei Servi and disappeared. Its disappearance was accompanied by a sound like that of the discharge of a 36-pounder gun, heard at a distance of thirteen or fourteen miles with a favourable wind. I can only give an idea of the dimensions and colour of this fiery ball by comparing it to the moon as one sees it rise over the Alps in clear winter nights, as I remember having sometimes seen it at Insprück in the Tyrol, of a reddish yellow, with some parts more red than the rest. The difference was, that I could not see the precise outline of the meteor as one does that of the moon; it seemed enveloped in an atmosphere of light of which one could not define the limits."

§ 6.

M. Babinet communicated to the Academy of Sciences on the 5th of July, 1852, the following Note:—

"The object of the present notice is to bring before the Academy a case of globular lightning which the Academy had charged me a few years ago (June, 1843,) with the care of investigating and authenticating, and in which the ball of lightning had struck a house (Rue St. Jacques in the neighbourhood of the Val de Grace) as it withdrew. The following is a brief summary of the account given by the workman into whose room the globular thunderbolt descended and then remounted.

"After a rather loud thunderclap, but not immediately after it, the workman, a tailor by trade, being seated by his table finishing his meal, suddenly saw the chimney-board fall down as if overset by a slight gust of wind, and a globe of fire the size of a child's head come out quietly from the chimney and move slowly about the room at a small height above the tiles of the floor. The tailor said it looked like a good sized kitten rolled up in a ball, and moving without showing its paws. It was bright and shining rather than hot and burning; the man said he felt no sensation of heat. The globe came near his feet, like a young cat that wants to play and rub itself against its master's legs; but by moving his feet aside, and making various precautionary manœuvres,—all done by his own account very gently,—he avoided the contact. It appears to have played for several seconds about the feet of the workman, who remained seated, his body bent over it, and examining it attentively. After having tried some excursions in different directions, but without leaving the middle of the room, it rose vertically to the height of the man's head; to avoid its touching his face he raised his body and threw himself back in his chair, still keeping the meteor in view. When it had risen three or four feet above the tiled floor the globe became a little elongated, and rising obliquely directed itself towards a hole pierced in the chimney three and a half feet above the mantleshelf.

"The hole had been made to allow a stove-pipe, which the workmen used in winter, to pass through; but, according to his own expression, "the thunder could not see the hole, for it was covered by paper which had been pasted over it." The globe of fire, however, went straight to the aperture, unpasted the paper without hurting it, and made its way into the chimney; then, said the witness, when it had just had time at the pace it was going, that is to say, pretty slowly, to get to the top of the chimney (at least 20 metres, or 66 feet from the ground of the courtyard), it made a dreadful explosion, which destroyed the upper part of the chimney and threw the fragments into the yard on the roofs of smaller buildings, which they broke through: happily no one was hurt. The tailor's lodging was on the third story, which was not half way from the bottom to the top of the house; the lower stories were not visited at all by the thunderbolt. The movements of the luminous globe were always slow and not by jerks. Though bright it was not dazzling, and no sensible heat came from it. It does not appear to have had a tendency to follow conducting bodies, or to have been impelled by currents of air."

§ 7.

Madame Espert wrote to me in July, 1852, the following letter:—"The remarks on the effects of globular thunderbolts, written by M. Meunier in a recent number of 'La Presse,' induce me to transmit to you the account of a meteorological phenomenon of this kind which I have witnessed. I live in the cité Odiot, No. 1., on the second floor, from which I overlook the Beaujon gardens. It was at half-past six in the evening, in the month of June, 1849, on the 16th, I believe, and a Friday, the same day on which the cholera was most severe in Paris. The temperature was suffocating, the sky appeared calm at the moment, but heat lightnings were seen on all sides As I passed by my window, which is very low, I was astonished to see a large balloon or ball, very like the moon when she appears augmented in size, and coloured by vapours. It descended slowly and perpendicularly from the sky towards a tree in the Beaujon gardens. My first idea was that it was an ascent of M. Grimm; but the colour of the balloon, and the hour, made me think I was mistaken, and while I was seeking to guess what it could be, I saw the lower part of the globe, when seven or eight yards above the tree, flame out. It was like paper burning gently, with sparks and small flames, and when an opening two or three times as large as a hand had thus been formed, a terrible explosion burst asunder the envelope and there darted from it ten or twelve zigzag lightnings, which shot forth in all directions; one of these struck a neighbouring house, No. 4., where it made a hole in the wall as a cannon ball might have done: this hole still exists; and, lastly, a remainder of electric matter began to burn with a white vivid and brilliant flame, and to revolve like an artificial sun in fireworks.

"The phenomenon lasted above a minute, and presented such a beautiful spectacle that I had not even an idea of danger or fear, and could only cry out, 'How fine! How beautiful!'

"Nevertheless, the explosion had been so violent, that three men in the street had been thrown down by it, and, as will readily be believed, a great sensation had been caused in the district. My cook was almost asphyxiated by a flash which passed across her window. The porter's wife let fall a dish which she held in her hand, but could not say afterwards whether it was simply from fright or whether she had been shaken by the commotion caused by another lightning which ran down the great staircase, on the landing of which she was. Another ray of lightning entered the boarding school of Madame Loiseau, Rue Neuve de Berry, where it wounded one of the governesses; the inhabitants of No. 4. rushed out into the court, terrified but unhurt.

"Paris resounded with this terrible thunderstroke; but perhaps I am the only person who happened actually to see the display of the phenomenon, and I would not for a great deal have lost being an eye-witness of so admirable and marvellous a spectacle!"

§ 8.

At the Beuzeville station, on the railroad from Paris to Havre, during a thunderstorm which took place on the 17th of May, 1852, at five in the afternoon, there were observed some very curious facts, which I insert from a letter of M. Lalande, written from the account given by M. Maillot, the station master.

"I had left my wife to replace me at my post at the telegraph, and had gone to the goods' shed on the other side of the inclined plane, for the purpose of hastening the loading of a waggon to be attached to the mixed train which was to ascend the plane at 6 h. 18 m. At this moment I saw in the south-east, advancing towards the place where I was, a luminous globe, resembling the mimic bomb shells used in the representation of battles. I called out to one of the factors that he might enjoy the sight, and thus he as well as myself saw this luminous ball, which we expected to pass over our heads, suddenly stop and disappear, just as it was above the telegraph wires, about sixty feet from us. At the same time the lightning fell, as we afterwards learnt, in the churchyard at Beuzeville, which would lead me to believe that the kind of zigzag which appeared to drive the luminous globe towards us was itself the ordinary striking lightning or thunderbolt. The storm afterwards passed on with increased violence to Criquetot-lez-Neval, where the hail did much damage."

§ 9.

I have still to record a double case of globular lightning observed by M. Al. Meunier, Chef de Bureau at the Ministère de l'Intérieur, and related in a letter addressed to M. Jamin, which that physicist has kindly communicated to me.

"In the month of June, 1852, I was walking along the Rue Montholon, between eleven and half-past eleven at night, when thunder and lightning burst forth with a degree of violence unusual at Paris. At first I paid little attention, and continued my walk, but about the middle of the street a lightning of extraordinary brilliancy blazed forth, and was instantly followed by a peal of thunder like a discharge of artillery. The lightning appeared to me like a very large shell thrown with great violence into the middle of the carriage way, where it burst with a tremendous explosion. The impression at the moment of the appearance of the globe was, that it was as if the moon were detaching itself from the sky. It was about the same size, and I should almost say the same colour. The sight did not cause me to stop or slacken my pace, for I remembered the saying that when one has seen the lightning the danger is over. I merely settled more firmly on my head my hat, which the wind, or the commotion produced by the electric discharge, had displaced, and continued my way unhurt to beyond the Place Cadet. Just as I stept on to the foot-pavement, I saw approach, a little obliquely, a fresh globe of fire similar to the first, but which had in addition, at its upper part, a kind of red flame, which might be compared to the fuze of a shell, although it was rather larger. This globe, which had not been preceded by a flash of lightning, at least not that I saw, descended with dreadful velocity, and burst in the street with a noise such that I have never heard the like. It gave me a violent shock on the right side, so violent indeed, that I was thrown by it against the wall. No doubt the noise of the explosion seemed to me so great only because I was in a position to hear it perfectly; but what appeared to me most especially remarkable was the globular form of the thunderbolt. My recollections on this point are most precise. As to the accident in itself, no seriously bad consequences followed from it; it only caused me to be unable to digest for a fortnight. I will add, in conclusion, that this thunderstroke formed the termination of the storm, and that on the following day the newspapers announced that the lightning had struck in the neighbourhood, I think in the Rue Lamartine."

CHAP. VIII.

LIGHTNINGS SOMETIMES ESCAPE FROM CLOUDS AT THEIR UPPER SURFACE, AND ARE THUS PROPAGATED UPWARDS IN THE ATMOSPHERE.

There is in Styria a high mountain called Mount St. Ursula, on the summit of which a church has been built. Jean Baptiste Werloschnigg, Doctor of Medicine, visited this church on the 1st of May, 1700, and saw very dense and very black clouds form *halfway up the mountain* and become the seat of a violent thunderstorm. On the summit of the mountain the sky continued quite serene, and the sun shone with great brilliancy. Persons might, therefore, deem that they were in perfect safety in the church, and yet *lightnings from the cloud below* killed seven persons in it by Dr. Werloschnigg's side.

CHAP. IX.

WHAT IS THE DURATION OF A FLASH OF LIGHTNING OF THE FIRST OR OF THE SECOND CLASS.

THIS question is one of more importance than may appear at first sight. Its solution, which is very recent, depends on considerations of some delicacy. They are taken in part from a child's amusement or experiment which every one must have made or seen made, and which consists in rapidly moving a piece of burning stick, or glowing charcoal or ember, so as to produce the appearance of a continuous riband of light.

Let us suppose the stick to describe a complete circle, and to take *a tenth of a second* only in doing so. In such case *experiment proves* that we see a luminous circumference in which the most attentive eye discovers no interruption to perfect continuity. The sense of vision reports that the glowing end of the stick occupies all points in the circumference of the circle simultaneously; yet in reality it only attains each of these points successively, or one after another, and a *tenth* of a second elapses between its quitting any one of them and returning to it again.

One important inference follows from this experiment. It will become evident if we fix our attention for a moment on some single point, say, for example, the uppermost point of the circle traced by the stick. When the glowing charcoal occupies this point, the rays of light which proceed from it form its image in the eye of the observer, on a particular part of the retina. When the charcoal moves, this image should also move, and indeed it does so, since we always see the charcoal where it actually is. But it would seem that as we see this second image the first image ought to have disappeared, since the cause which produced it, if it has not disappeared, has at least changed its place; so far from this being the case, there is time for the glowing charcoal to make a complete round, to return to its first place, and reproduce on the retina of the spectator the image of the uppermost point of the circle, before the sensation resulting from its first passage through that point has ceased or been effaced.

It follows, then, that the impressions received by us through our sense of vision have a certain duration, or last a certain time. The human eye, at least, is so constituted that *the sensation of light does not cease until a tenth of a second after the complete disappearance of the cause which produced it.*

We have just recognised that a radiant point which takes only a tenth of a second to make a complete circuit, has to our eyes the effect of a circumference of a circle completely luminous throughout. It is evident that if two, three, ten, or a hundred radiant points are placed successively between the supposed point and the centre of rotation, (or centre of the circle which it describes), and are made to turn simultaneously and with the same rapidity, they will produce, two, three, ten, to a hundred circumferences of concentric luminous circles. Every one, therefore, will readily comprehend that if these different moving radiant points are contiguous, if they touch each other, and are sufficiently numerous to form when in a state of repose a line of continuous light between the first point and the centre, the circumferences which they describe when in motion will also touch, and thus instead of the two, three, ten, or a hundred *separate* circumferences of the preceding experiment, we shall have a *circular surface entirely illuminated.*

Here, in complete analogy with what happened in the previous experiment with single points, a luminous *line* turning round one of its extremities produces *a circular surface of light,* if it returns to each successive position through which it has passed before each of the images which it has produced on the retina during a first revolution has been effaced; that is to say, if the line describes the entire circle within *a tenth of a second.*

Now in place of a single moveable luminous line, let us suppose four illuminated lines, all similar and equal in intensity of light, placed at right angles to each other, so as to divide the circumference into four equal parts. It will now no longer be needful that the apparatus should make a complete revolution in a tenth of a second; a velocity *four times less,* or a rotation performed in *four tenths* of a second, will suffice to produce a circular surface, which shall in the same manner appear completely luminous.

For what is it that is required for this continuity of brightness? It is that no point of the circle shall be deprived of *real* *light* for more than a tenth of a second. Well, then, let us suppose the moment when one of the four luminous lines is vertical; the next following line will in its turn become vertical in a fourth part of the time required for an entire revolution, *i. e.* in a fourth part of four-tenths, or in one-tenth of a second. The third rotating line will in the same manner, in the course of another tenth of a second, succeed the second line in the occupation of the vertical position, and so on. Thus, when the vertical *image* of the first line is about to fade, the second of the four rectangular luminous lines of the apparatus comes to renew it; when the vertical image of the second line reaches the term of its duration the third line takes its place; the fourth line in its turn comes to the vertical just as the image of the third begins to be effaced; and lastly, the first line returns to the position in which we first supposed it placed exactly in time to occupy by its light the vertical direction which the disappearance of the image of the fourth line was about to leave dark.

I have thus shown in detail, in too great detail perhaps, how four luminous lines placed rectangularly to each other, and describing a circle round their point of intersection in four-tenths of a second, illuminate, with an apparently continuous light, the vertical radius of the circle. Every one will see the obvious remark that the same reasoning would have applied to a horizontal radius, or to a diagonal, or other oblique radius; and thus the mode by which luminous surfaces are produced by the rotation of simple lines, is sufficiently explained.

To resume, a *luminous* line produces the appearance of a circular surface of light, *when it turns* round one of its own extremities with sufficient rapidity to describe the entire circumference of a circle in a tenth of a second of time.

This point is a simple matter of fact connected with the conformation and sensibility of the human eyes. It is thus that things are, but they might have been otherwise: it is only by experiment that we learn the truth in this respect.

The experimental truth being once established that "*a tenth of a second* for the rotation *of a single line* is the least rapidity required to produce a circular area of continuous light, it follows necessarily and mathematically that the least rapidities or velocities of rotation required for ten, a hundred, or two hundred lines, placed at equal angular intervals, to produce the same effect in turning round their common point of intersection, will be ten, a hundred, or two hundred times less than in the case of a single line, that is to say, they will correspond to one, ten, or twenty seconds for an entire rotation.

Nothing in the above reasoning implies that the rotating lines need to shine by their own light. We may therefore expect to observe precisely the same phenomena whether the lines which are made to rotate are self luminous, or whether they only shine by reflected light; only it is requisite in the latter case that the line should be of such a nature and form, and disposed in such a manner relatively to the source of illumination, that the eye shall perceive them *equally* in all the positions which they assume in turning. Such, for instance, would be the *flat and non-polished spokes*, or radii, of a wheel in frosted silver; the flat and non-polished spokes of a wheel of any kind of substance if covered with a coat of white lead, &c., and whatever might be the kind of lamp placed in front of them to light them up, or even if a simple wax candle were used for that purpose. The radii not being polished would never act as mirrors in any of the positions assumed by them. They would be seen only by the kind of light which all bodies receiving light assimilate to themselves, to give back to us *in all directions*, or in the state of diffused light; vermilion with a decided red hue, brass with an evident yellow tinge, dead silver and white lead with perfect whiteness, &c. A spoke of dead or frosted silver turning round one of its extremities in a tenth of a second will give rise to the appearance of a circular white surface; four, ten, or a hundred spokes of the same material at equal intervals will produce the same effect if they turn respectively in 0·4 of a second, in 1 second, or in 10 seconds.

Let us keep for a moment to the latter case — to that of a hundred thin metallic spokes or radii, fixed so as to form with each other equal angles, and made to rotate so as to produce to the eye the effect of a circular disk of light. This desired effect will begin to be produced when the velocity of rotation is that of a turn in ten seconds. A less velocity will not suffice, but any greater velocity, however considerable, would conduct, still better, if possible, to the same result.

Among the infinite number of velocities greater than that which is strictly necessary for the turning radii to appear to form a continuous surface, let us, in order to fix our ideas, make choice of one. Let us suppose our hundred spokes to make a complete turn in a tenth of a second, a velocity very easily obtainable; then each spoke will employ a hundredth part of that time, or a thousandth part of a second, to pass from any one of its positions to that occupied at the same moment by the preceding spoke.

Let us recollect carefully this quantity (a thousandth of a second), and let us introduce into our experiment one supposition more; viz. that the light which illuminates the hundred spokes of the revolving wheel, and without the presence of which they would not be seen at all, as they are not self-luminous, does not shine uninterruptedly. Let the supposition be that the wheel turns always uniformly at the rate agreed upon, *i.e.* one revolution in a tenth of a second, but that it does so *in the dark*, excepting when illuminated by a light which appears only for an instant. It will then be the duration of this instant which will determine whether the wheel, when thus illuminated, shall be seen as a wheel having from the centre to the circumference alternate spaces, void and occupied, dark and bright; or as a continuous surface, equally illuminated throughout.

Let us first assume that the light strikes the revolving wheel only for an instant, *infinitely brief*. It will then strike upon and illuminate the different radii only *in one of their positions*. Each radius being seen only in this *single* and special position, will then produce on the retina an image of which the duration experimentally found is a *tenth of a second*. The revolving wheel will then be seen for a tenth of a second in its true form, and as if it were motionless.

Let us now pass to another supposition, which I will call *extreme* (an expression which will soon be seen to be justified). Let us assume that the light *lasts a thousandth part of a second.*

A thousandth of a second is, in the hypothesis, the time required for each spoke to pass from one of its positions to that occupied *at the same moment* by the preceding spoke. In this short interval of time, therefore, there will not be in the interior of the rotating wheel even the space of a mathematical, or ideal, radius or line which will not be occupied in its turn by one of the actual or material radii or spokes; there will not be one of these thousand or million positions in which the spokes do not receive illumination, and do not form an image on the retina. These images, be it well remembered, last a tenth of a second, that is to say, 100 times longer than is necessary for all the *geometrical*, or infinitely narrow, radii of the wheel to send back a luminous line to the eye of the observer. Therefore, at a given moment, all these luminous lines will be seen simultaneously, and the wheel, though actually consisting of occupied and unoccupied spaces, will appear a uniform continuous surface, illuminated at all points.

If now we were to endeavour to apply the same considerations to the case in which the duration of the light should be less than that which each spoke of the revolving wheel requires for passing from one of its positions to the place occupied at the same moment by the preceding spoke, every one may see, without difficulty, how different would be the results of the experiment. Let us, for example, assume the time for which the apparition of the light continues to be only half that before supposed, or only *half a thousandth part of a second.* In *half* a thousandth of a second each material radius, or spoke, passes through only half the angular interval comprised between one of its positions and the position held at the same instant by the preceding spoke. At the mathematical instant at which the light appears, each spoke is caught and illuminated in one of its positions; when the light disappears each spoke has traversed only half the distance which it had to pass through in order to reach the position of the preceding spoke. At the instant of the first apparition of the light certain spaces were included between every two spokes. Well then precisely one half of each of these spaces has remained unvisited by any of the spokes of the revolving wheel during the brief interval which we have just assigned as the duration of the light. These spaces being thus void, could not therefore reflect to the observer any ray of the illuminating light; consequently the wheel must have appeared to consist of a series of alternately dark and light sections.

Those persons who were aware that the sensation produced on the eye by any kind of light lasts for a little while after the light has really disappeared, might not unreasonably have concluded, that even if it were only on account of that circumstance they could not confidently expect an exact solution of the question proposed in the title of this long chapter; yet the actual result has been, that this apparent obstacle has itself supplied means of investigation whereby we can operate upon thousandth parts of seconds better than could have been done by the usual methods with entire seconds. Let the details of the experiment be duly considered, and it will be seen that this is not an exaggerated statement.

I desire to know the duration of each of those flashes of lightning which furrow the face of the sky on a dark night. I set up, facing the region where the thunderstorm is taking place, a metallic wheel having a hundred thin spokes. A clock movement gives to the wheel a regulated and uniform velocity of rotation of ten turns in a second of time, or one turn in a tenth of a second. The wheel is put in motion, and I place myself between it and the storm-clouds, not, however, so as to prevent the flashes of lightning from illuminating the wheel. As in the hypothesis all is in darkness except when the lightnings appear, I do not usually see the wheel at all. A flash occurs, and at that moment the wheel is lighted up; I ought, therefore, to see it, and I do see it in effect; but the conditions under which it appears depend on the duration of the flash. If this duration has been *infinitely short* the wheel will have been seen, for a tenth of a second, as consisting of a *hundred luminous spokes*,—motionless,—and of the apparent breadth of the true spokes.

If the flash has lasted *a thousandth part of a second* the wheel will have appeared to be *a circular disk full of light from the centre to the circumference.*

To durations of *half*, a *third*, a *fourth*, or a *fifth* of a *thousandth part of a second* will correspond the appearance of circular disks having respectively *half*, *two-thirds*, *three-quarters*, and *four-fifths of their whole surface* completely deprived of light.

By enlarging the dimensions of the wheel we may increase our scale of measurement as much as we desire, and thus facilitate the estimation of the relative spaces; moreover, by varying the velocity of rotation we may even avoid altogether the necessity of estimating by the eye the proportion of the illuminated to the dark portion; we may reduce the whole question to the determination of the velocity at which the entire circle appears illuminated. If a velocity of rotation of one turn in the tenth part of a second does not produce a continuous circle of light, we gradually accelerate the movement until this continuity is obtained. Supposing this effect not to begin to be realised until the wheel turns once in a *half*, or *a third of a tenth of a second*, this will prove that the flash of lightning did not last more than *half* or *a third of a thousandth of a second*, and so on for whatever other numbers might be found.

Having now come to the end of this long and minute explanation, I may state that, after having multiplied the spokes of the wheel as much as possible, and after having had recourse to the greatest velocities of rotation compatible with safety and uniformity in the clock-work, the revolving wheel when presented in thunderstorms to lightnings either of the first or second class never appeared a continuous surface; its spokes were always seen as distinctly and sharply defined as if the wheel were at rest; their apparent breadth was not even in the least degree increased. We shall be keeping far within the conclusion which this experiment would authorise when we content ourselves with saying that the most brilliant and extensive flashes of lightning of the first and second class, even those which appear to embrace the whole extent of the visible horizon, have not a duration *equal to the thousandth part of a second of time.**

CHAP. X.

ARE STORM-CLOUDS EVER CONTINUOUSLY LUMINOUS?

THE obligation which, when I commenced writing the history of thunder and lightning, I imposed on myself, viz., that of consulting all memoirs, however obscure, or however little esteemed, in which I should have any reason to suppose that notices relative to the subject might possibly be found, has led to the disinterment of a fact, the importance of which is such that it is really surprising that it should not have been better appreciated.

* Mr. Wheatstone, to whom we owe the ingenious experiments of which I have just given an account, has succeeded, by means of a very important modification in his fine apparatus, in demonstrating that the electric spark of our machines does not last the millionth part of a second. It is greatly to be desired, that these new means of investigation should be perseveringly applied to the study of lightning. Great discoveries will probably be thus arrived at.

This fact, as the title of the chapter indicates, is the *emission*,—not an intermitting, *but a* CONTINUOUS *emission of light from the surface of certain clouds.* I find it recorded in the most clear and distinct manner in a memoir of Rozier as having occurred on the 15th of August, 1781, and in a memoir of Nicholson as having occurred on the 30th of July, 1797.

On the 15th of August, 1781, the sky at Beziers became after sunset covered with clouds; at a quarter before eight thunder began to be heard; at five minutes after eight it was quite dark, and the storm had acquired great intensity. "At that moment," says Rozier, "as I was examining the direction and effect of the lightnings, I saw a luminous point appear from behind the slope of the hill which bounds on one side the view from my house. This luminous point gradually increased in size and extent, and presently formed a zone, or phosphoric band, appearing to my eyes three feet wide. When complete, its horizontal length subtended an angle of 60 degrees.

"Above this first luminous zone there was formed another of the same height, but of only 30° extent, or half that of the lower band. Between the two there remained a vacant interval, of which the height equalled that of either of the bands taken separately. . . . .

"In both zones I remarked irregularities nearly resembling those in the edges of the great white clouds which are the precursors of thunderstorms. The edges were not all equally luminous, but in the middle of the zones the brightness was uniform. While the zones were advancing towards the east, lightning darted three different times from the extremity of the lower zone, but without being followed by any audible detonation.

The luminous zones were not joined to the general mass of storm-clouds; they were much nearer to the earth. "The phenomenon" (that of the bright bands) "was seen from five minutes after eight to seventeen minutes after eight (or for nearly a quarter of an hour)." At 8 h. 17 m. a south wind sprung up and drove the storm away from Beziers."

Now let us hear Nicholson:—"On the 30th of July, 1797, I rose at five in the morning; the sky, excepting towards the south, was covered with very thick clouds, which were travelling very rapidly west-south-west. Lightnings appeared frequently in the north-west and south-west; they were followed, after eleven or twelve seconds, by violent thunderclaps. The lower, most waved and jagged portions of the clouds, had a constant red tinge, and I learned that this tinge had been much stronger and more marked before I had the opportunity of observing it. I was also told that at a quarter after four the houses opposite to mine appeared as if viewed through a dark blue glass; when I looked at the sky I saw the clouds of a very intense leaden blue."

These two observations, and especially that of Rozier, in which there is nothing which can be deemed equivocal, seem to me to have some kind of analogy with a remark of Beccaria's, which I wish therefore to commend to the attention of observers, if only in the light of a conjecture and a subject of inquiry.

The Physicist of Turin says:—"It has very often happened to me, on nights entirely dark, and particularly in winter, to see scattered clouds draw together, and by their agglomeration form a general uniform cloud, with a smooth surface, and of apparently inconsiderable density. Clouds of this kind send forth in all directions a reddish light, without definite limits, but of strength sufficient to have enabled me to read print of ordinary size (*mediocre carattere*). This nocturnal light from clouds has been principally observed by me in winter nights, between heavy falls of snow. I am myself inclined to attribute it to the 'matter of lightning' (electric fire), to which are to be universally ascribed general clouds without apparent undulations. This matter, if circulating through the vapours in quantities ever so little above what they are capable of transmitting, must manifest luminosity, as many cabinet experiments have shown. If very slender and extremely frequent darts of light occur at all the points where the vapours present slight variations of density, it is evident that the result will be a faint general luminosity, without definite boundaries." (*Dell' Elettricismo Terrestre Atmosferico*, p. 288.)

In relation to the phosphorescence of clouds, I subjoin the following observation, for the knowledge of which I am indebted to the celebrated Director of the Armagh Observatory, Dr. Robinson:—

"In determining the lines of magnetic force in Scotland, Major Sabine remained some time at anchor in Loch Scavig, in the Isle of Sky. The loch is surrounded by high mountains of bare rock, one of which was remarked to be almost always enveloped in a cloud resulting from the precipitation of vapours brought from the Atlantic by nearly constant south-westerly winds. At night this cloud appeared luminous in itself, and in a permanent manner. Major Sabine also saw repeatedly ascend from it streamers similar to those of the aurora borealis. He decidedly repels the explanation of these being distant auroras existing near the horizon, the direct view of which was intercepted by the mountains, allowing only the upper part to be seen. He was fully persuaded that the luminous phenomena, both the persistent and the intermittent ones, whatever might be their nature, were connected with the cloud itself."

Dr. Robinson also mentioned that he had himself made various observations on the phosphorescent properties of ordinary fogs in Ireland. It is much to be desired that this distinguished astronomer should communicate them to the public without delay.

The admixture of certain foreign substances with our atmosphere, which sometimes takes place, communicates to it the phosphorescent faculty in a high degree. For example, a Memoir of M. Verdeil, physician at Lausanne, informs us that the celebrated dry fog of 1783 "diffused at night a light which permitted one to see objects at a certain distance, and which extended equally to all parts of the horizon. The light was similar to that of the full moon with a sky generally covered, or when concealed behind a thick but more partial cloud."

The dry fog of 1783 was the seat, and perhaps the cause, of frequent thunderstorms. A work of Deluc, which has but few readers, entitled "Idées sur la Météorologie," seems to show that clouds may become luminous without our being altogether warranted in seeking an explanation of the fact in small, incessantly renewed, lightnings. The following is the passage:—

"As I was returning home in London, about eleven o'clock on a winter's evening, the air being very serene, without being very cold, there being no moonlight, I saw a luminously dappled part of the sky forming a zone of several degrees of breadth, stretching nearly from east to west, passing thirty or forty degrees to the south of the zenith, and nearly touching the horizon on either side. My lodgings were very near the country, which made it easy for me to observe the phenomenon in its whole extent, and I did so from the moment I perceived it until its close. This species of cloud, which was as bright throughout its entire length as is a thin cloud veiling the moon, at first quite concealed the stars. Gradually the dappling became more pronounced, and stars appeared in the intervals of the mottlings. I afterwards perceived them through the mottlings themselves, which now only resembled a gauze, and in about ten minutes entirely disappeared. There had been, I imagine, some phosphoric decomposition; for else from whence would the light, which came from the whole cloud, have proceeded? There was not the least sign of electricity; all was in repose, excepting a little general movement in the zone of which we have been speaking."

When we reflect on the immense effect of clouds in weakening the dazzling light of the sun on particular days in winter, we have much reason to be surprised at the fact that after sunset, when it is quite night, and even at midnight, the sky being similarly covered with clouds, and when there is no moonlight, there should be still light enough in the open country for people to move about without striking against obstacles lying in their way. It is scarcely possible to admit that this faint diffused light, which, on a cloudy night, is so great an advantage to us, comes from the stars. But if we exclude stellar origin, the only resource remaining to us for the explanation of the fact is, the supposition that all clouds have a luminosity of their own, the difference between them in this respect being one of degree. At the one extremity of the scale would be the bright clouds observed by Rosier; lower down, and at a considerable interval, those of Nicholson; lower still the snowy clouds of Beccaria; lastly, at the lowest end of the scale, would be those thick dense clouds which cover the sky in the darkest nights of winter, and which yet are the cause that even at midnight the darkness in the open air is never quite equal to that of a subterranean space, or of a room having no windows.*

* My purpose had been to touch only on one point of a simple meteorological phenomenon, yet such are the necessary connections of the different sciences, that without thinking of, or intending it, we have, I believe, penetrated a little way into one of the greatest problems of natural philosophy. I mean the question of how it is that our sun has shone for so many ages without losing any of its brightness. Ordinary combustions are irreconcilable with such endurance. At some period in the course of time, the combustible itself and the matter which supports the combustion must, we should suppose, have been exhausted.

Let us regard phosphorescence as a necessary consequence of a gaseous condition and state of cloud; and let us further suppose the sun to be

CHAP. XI.

ON THUNDER PROPERLY SO-CALLED, OR ON THE NOISE HEARD WHEN THE LIGHTNING ESCAPES FROM THE CLOUDS.

THE apparition of lightning is usually followed, after longer or shorter intervals, by sounds which every one has heard, but probably without sufficiently remarking the different characters by which they are distinguished under different circumstances.

Lucretius gave, I think, a very exact idea of a particular kind of thunderclap, by comparing it to the peculiar grating sound made by paper when it is torn (Book VI.).

I would not venture to assert that the comparison has been rendered much more exact by the substitution of the sudden tearing of a strong silk stuff for that of paper or parchment.

Sometimes the sound of thunder seems clear and sharp like a single pistol-shot.

More generally it is full and very grave; some observers even affirm that it becomes more and more grave as the resonance is more prolonged. Practised musicians can alone decide this question.

In the sound of thunder two circumstances appear deserving of particular attention. On the one hand, its long duration; and on the other, the successively lessening and increasing intensity so often repeatedly renewed during the resonance of one and the same explosion. It has not been by chance that the expression "*rolling* of the thunder" has been generally adopted, nor is it without reason that this rolling has been compared to the noise caused by a heavy cart moving rapidly down a very rocky or rugged road.* We shall soon have occasion to

surrounded by a continuous stratum of clouds; and the difficulty will disappear; for phosphorescent emissions do not necessarily imply loss of substance. It might possibly be sufficient to extend the state observed by Rozier of certain parts of the storm-clouds at Beziers, to a whole atmosphere, in order to arrive at something resembling the brightness of the sun. If my conjectures should be well-founded, Nicholson would have caught sight, at a few minutes' interval, of the two atmospheric constitutions giving rise to red and to blue stars.

* No one, I hope, will be astonished at my noticing the method successfully adopted in some theatres for imitating by very simple means not only

examine whether echoes are principally, or only secondarily, concerned in these effects. In the meantime I will notice such well-assured facts as I have been able to collect respecting the greatest observed duration, in a plain country, of the rolling of thunder *corresponding to a single flash of lightning.* Attention is required to the words printed in italics, for even in our climates thunder is sometimes heard continuously for hours together, but at such times flashes of lightning also follow each other almost uninterruptedly.

In the register of observations made at Paris, by de L'Isle, I find at the date of

June 17. 1712, a peal of thunder which lasted 45 seconds.

On the same day the next most considerable results were—

41, 36, and 34 seconds.

In succeeding observations on the 3rd, 8th, and 28th of July, de L'Isle found the duration of the longest peals of thunder respectively;

39, 38, 36, and 35 seconds.

Those who have not studied thunderstorms with the attention of a meteorologist or physicist, may not perhaps be aware that the strength of the sound of each explosion is not always greatest at the commencement. Thunder often begins by a hollow rolling which is succeeded by loud outbursts, and these again are followed by a rolling which dies away rapidly but gradually. On certain points of theory excellent tests will be supplied by numerical estimations of the intervals between the faint beginnings, in particular cases of thunder, and the period of intense resounding noise. At present we have unfortunately very few data of this kind. Those which I am about to cite are also furnished by de L'Isle; it is surprising that his observations should never have been referred to.

the kind of almost uniform muttering which is the sound of distant thunder, but also the sudden crashing, jerking bursts of near thunder. The operator employs a thin rectangular plate of hammered iron about a yard long, and half a yard broad. He takes it by one of its corners between his fore finger and thumb, and, by a particular turn of the hand, makes the corner of the metal which he holds bend sometimes in the one, and sometimes in the opposite direction. By varying the rapidity of these alternations the various modifications of the noise of thunder are imitated with great success.

June 17. 1712, a thunderstorm over Paris:—

At	0 seconds	a flash of lightning.
At	3	„	the thunder begins to be faintly heard.
At	12	„	beginning of the full force of the thunderclap.
At	19	„	thunder dies away.

Thus no less than 9 seconds elapsed between the commencement of the thunder, and that of its full force:—

July 21. 1712, Paris:—

At	0 seconds	a flash of lightning.
At	16	„	thunder commences faintly.
At	26	„	becomes loud.
At	32	„	dies away.

The following notices have over the preceding ones the advantage of giving the duration of the loud portion of the thunderpeal:—

July 8. 1712.

At	0 seconds	a flash of lightning.
At	11	„	thunder begins gently.
At	12	„	loud crashing sound or thunderclap begins.
At	32	„	„ „ „ „ „ terminates.
At	50	„	thunder dies away.

The reader will here remark that the full sound of the thunderclap lasted 21 seconds.

Same Day.

At	0 seconds	a flash of lightning.
At	11	„	thunder begins gently.
At	12	„	the loudness of the thunderclap begins.
At	38	„	„ „ „ „ „ terminates.
At	47	„	thunder dies away.

The duration of the full thunderclap has here risen to nearly half a minute.

I will cite one more case, because it presents the novel circumstance of a reduplication of intensity during the course of the thunderclap.

At	0 seconds	a flash of lightning.
At	10	„	thunder commences very faintly.
At	13	„	thunderclap.
At	20	„	redoubled intensity.
At	35	„	thunderclaps cease.
At	39	„	thunder dies away.

The intensity of thunder, meaning thereby the strength of the sound at the loudest part of the burst or thunderclap, presents astonishing variations.

The Reverend William Paxton, in writing to the Dean of Exeter an account of the thunderstroke which, on the 2nd of March, 1769, threw down one of the pinnacles of the tower of Buckland Brewer, said that the noise of the explosion was at least equal to that of *a hundred* pieces of cannon discharged at once.

On the other hand I find in the notes for which I am indebted to Captains Peytier and Hossard, that in the Pyrenees, thunderclaps taking place close by them, in the middle of the very clouds in which they were enveloped, produced hollow sounds similar to those of powder not compressed fired in the open air.

Fulminating balls or globes, one of the forms of lightning, sometimes produce most violent explosions. When the "Montague" was struck at sea by one of these on the 4th of Nov. 1749, the sound heard was, according to the account of the master, Mr. Chalmers, equal to that of several hundred guns going off at once, but it did not last more than half a second. Thunder is only heard at a greater or less interval *after* the appearance of the flash of lightning. Every one must have remarked this fact—and the tables just given from de L'Isle's observations have also brought it to the reader's notice. The cause of the phenomenon is simple, and we shall soon have to discuss it in detail; the inferences to be drawn from such discussions will be the more valuable and useful, if we have worked with either high or low values for the elapsed interval. Let us, therefore, seek for maxima and minima of observed intervals between the flash of lightning and the corresponding thunderclap.

In regard to the *maximum*, the celebrated geometrician Lambert did not think that as much as 40 seconds ever elapsed between the flash and sound; but at the very time when he expressed this opinion, he might have found in de L'Isle's Memoirs, published at St. Petersburg, results considerably above the limit which he thus adopted. Observations of the 2nd May, 1712, at Paris gave:—

42, 48, and 48 seconds.

Those of the 6th of June following gave:—

47, 48, 48, and 49 seconds.

From an observation of the 30th of April there would result the enormous interval of

72 seconds.

In the observations of Chappe, made at Tobolsk in 1761, I remark on the 2nd July:

42, 45, and 47 seconds.

On the 10th of the same month I find,—

46 seconds.

On the other hand, the *smallest* intervals between the flash of lightning and the thunder which I find among the very small number of observations of De L'Isle are,—

3, 4, and 5 seconds.

Chappe's observations give several cases of

2 seconds.

These last results will be of little use to us. On the other hand, very interesting and theoretically important inferences might be deduced from intervals not exceeding small fractions of a second. Unfortunately fractions of seconds are rather difficult to estimate, and ordinary observers do not even think of taking account of them. If the noise has followed the flash within less than a second, they declare without any further consideration the two phenomena simultaneous, whereas these are the very occasions in which precision is more than ever important. In consulting my own recollections I feel perfectly confident that I am keeping within the limits of the truth, and I even flatter myself that I do not expose myself to being contradicted by any well-practised observer, if I say that the interval between the flash and the sound is often less than half a second.

CHAP. XII.

ARE THERE LIGHTNINGS WITHOUT THUNDER, AND WITH A PERFECTLY CLEAR SKY?

THE phenomenon of lightnings without thunder, with a perfectly clear sky, is too well known, and rests on too general experience for it to be necessary for me to cite on this point the testimony of any particular meteorologist. Who, indeed, in our climates has not seen and remarked "heat-lightnings?" Bergman tells us that in Sweden, where they are also very common, the peasants call them "barley lightnings" (kornbleck), because they are usually seen in August, when the barley is beginning to ripen.

It is by mistake that it has been asserted that heat-lightnings always remain near the horizon. Their light sometimes overspreads the whole visible extent of the heavens. This remark will not be without its use when we come to inquire whether heat-lightnings have an independent existence, or are only the reflection of other lightnings.

CHAP. XIII.

DOES THUNDER EVER OCCUR WITHOUT LIGHTNING?

SENECA asserted that thunder sometimes takes place without lightning. (*Quæst. Nat.* lib. ii. § 18.)

I am ashamed to have to confess that as respects Europe I am almost reduced to Seneca's statement. Thunders without lightning, notwithstanding the points of theory on which they might afford solutions, have but little excited the attention of observers. The citations which I am enabled to make from other parts of the world leave, I think, little room to doubt the reality and generality of the phenomenon.

Thibault de Chanvalon wrote in October, 1751, in his register of meteorological observations at Martinique:—"Of eight days in this month on which it thundered there were two days *without lightning.*" In November, I read:—"Thunder only on one day; three rather strong thunderclaps, but *without lightning.*"

On the 19th of March, 1768, near Cosseir in the Red Sea, a violent thunderclap spread consternation among the crew of the small bark on which the traveller James Bruce was proceeding. This thunderclap *had not been preceded by any lightning.*

CHAP. XIV.

DO LIGHTNINGS EVER TAKE PLACE WITHOUT THUNDER IN A CLOUDY SKY?

THIS question ought to be answered affirmatively. I might refer to a very ancient testimony, that of Lucretius. In the Sixth Book of his celebrated poem on the "Nature of Things" (l. 216, 217.) he says, that harmless lightnings escape *in silence* from certain clouds, and cause neither disturbance nor terror.

Lightnings without thunder *in cloudy weather* appear to be common in the West Indies. Thibault de Chanvalon mentions them in his meteorological observations at Martinique. I find in his Tables, under the head of July, 1751, "thunder six days, *lightning without thunder two days*." To which I must add, that during these two days the sky appears to have been covered with clouds.

The observations made at Rio Janeiro by Dorta, and recorded in the Memoirs of the Academy of Lisbon, are no less positive; they give me —

In 1783	- - -	24 days of lightning without thunder.
1784	- - -	48 " "
1785	- - -	47 " "
1787	- - -	51 " "

The Meteorological Journal kept in 1826 at Patna in India (latitude 25° 37′ N.), by Mr. Lind, leads to a still larger result; I find from it

73 days of lightning without thunder.

If we had under our eyes the very detailed observations made in Brazil and India, perhaps we should find these numbers somewhat reduced; perhaps we should find that the above enumerations of "days of lightning without thunder" include some serene days. As, however, thunder and lightning are rare except in the rainy season, the reductions could not be very important.

I cannot close this chapter without citing from observers in Europe some instances of lightning without thunder.

Although I attach much less value to a general assertion than to a particular observation accompanied by minute details (and among these I even go so far as to include the date and hour of observation), I will mention that in the Dissertation on Thunder which in 1726 gained for its author a prize from the Academy of Bordeaux the father of Lozeran de Fesc speaks of *very vivid* lightnings which during certain storms dart from the clouds in all directions, and almost uninterruptedly, without giving rise to any appreciable noise.

The following observation is by the younger Deluc:—

On the 1st of August, 1791, after sunset, the sky as seen from Geneva appeared covered with clouds to the westward, over the Jura. These clouds were traversed by brilliant lightnings, but no thunder was heard. It may be objected that a distance of 12 to 20 kilometres (from 6½ to 11 geographical miles) was sufficient to entirely deaden the sound, but we are able to advance a step further.

The clouds over the Jura gradually spread until they reached the zenith at Geneva. "Then," said Deluc, "there still issued from them such lightnings as might have been expected to be followed by thunderpeals of overwhelming loudness, instead of which scarcely any thunder was heard." One of these flashes of lightning, however (Deluc does not say that it surpassed the others in brilliancy), was attended, on the contrary, by a terrible detonation. This was followed by a short but heavy fall of rain. "Afterwards lightnings continued; but," adds Deluc, "*I heard no more thunder.*"

The following passage is taken from the Meteorological Observations and Essays of John Dalton:—

"Kendal, August 15. 1791, between eight and nine in the evening. I do not remember that I ever saw at Kendal so much lightning in so short a time. *Some thunder* was heard, but distant."

CHAP. XV.

IS THUNDER EVER HEARD IN PERFECTLY CLEAR WEATHER?

SENECA affirms that "thunder sometimes mutters when the sky is without clouds." (*Quæst. Nat.* lib. i. § 1.)

Anaximander must also have believed in the reality of this phenomenon as he had sought to discover its cause. (*Quæst. Nat.* lib. ii. § 18.)

Lucretius, on the contrary, said unhesitatingly, "Where the sky is serene the noise is not heard" (lib. vi. l. 98.); and further on, "Thunder and lightning are only engendered amidst thick clouds piled one above another to immense heights, not under a sky perfectly serene or merely thinly veiled."

Suetonius relates, that towards the end of the reign of the emperor Titus a clap of thunder was heard with a perfectly clear sky.

In Eginhard's "Life of Charlemagne" a luminous meteor is said to have struck and overthrown the horse on which the Emperor was riding, the sky being serene.

Senebier speaks of thunder on serene days as of a recognised fact: unfortunately he does not say whether his conviction rests on theoretical considerations or on direct observation. (*Journal de Phys.*, tome xxx. p. 245.)

Volney is more explicit. On the 13th of July, 1788, at six in the morning, *the sky being without clouds*, at Pontchartrain, 8¾ geographical miles from New Orleans, he heard four or five thunderclaps. *It was not until* a quarter after seven that a cloud appeared in the south-west. In a few minutes the entire sky was covered with clouds, and soon hail fell as large as a man's fist. (*Du Climat des États-Unis.*)

We shall be in danger of falling into error, if we were to seek for instances of serene days accompanied by thunder in countries subject to great earthquakes. The latter phenomena are often preceded by long subterranean noises, partly resembling thunder, which by an acoustic illusion which has not yet been satisfactorily explained, seem as if they had their seat in the atmosphere. This is why I have not cited the dreadful thundering sounds heard in the finest weather, a century ago, at Santa Fé de Bogota, and in commemoration of which an annual mass, called "*la missa del ruido,*" is celebrated in the cathedral of that city.

CHAP. XVI.

THUNDERBOLTS DEVELOP BY THEIR ACTION, IN THE PLACES WHERE THE EXPLOSION TAKES PLACE, OFTEN SMOKE AND ALMOST ALWAYS A STRONG ODOUR, WHICH HAS BEEN COMPARED TO THAT OF BURNING SULPHUR.

If I were to attempt to cite all the cases in which the sulphureous odour has manifested itself, I should have to make almost a complete catalogue of the thunderbolts of which the effects in closed apartments have been traced a short time after the explosion; I shall therefore confine myself to a few examples, and will cite, in the first instance, cases in which the odour developed has been so powerful as to be perceived in the open air.

Wafer, Dampier's surgeon, relates that in crossing the isthmus of Darien, the falls of rain which he experienced were accompanied by furious thunder and lightnings, and that at such times the air was infected with a sulphureous smell, strong enough to take men's breath away, especially in the midst of the woods.

I read in another part of Wafer's account:—"After sunset" (the voyagers were about to pass the night in the open air on a small hill) "it began to rain as if heaven and earth were coming together. We heard every instant terrible claps of thunder. The lightnings had such an intensely strong smell of sulphur that we were almost suffocated."

In his Memoirs *for a General History of the Air,* Boyle relates that at the time when he was living on the banks of the Lake of Geneva, violent and frequent thunder and lightning impregnated the air with a very powerful sulphureous smell, which nearly suffocated a sentry posted on the very edge of the lake.

In February, 1771, in the Isle of France, the French academician, Le Gentil, saw the lightning strike a spot in the country at a very little distance from the balcony where he was standing, at the house of the Comte de Rostaing. Four hours after the explosion, and although much rain had fallen, Le

Gentil and M. de Rostaing, happening to pass near the point which had been struck, found a very decided sulphureous odour.

Every one may perceive why I have brought together, in the first place, manifestations of sulphureous odours in the open air, and they will therefore readily comprehend that the same reason applies even more strongly to the interest which would attach to similar effects at sea.

When the British ship "Montague" was struck on the 4th of December, 1749, by a globe of fire, accompanied by a detonation which the Master, Chalmers, compared to the noise of the simultaneous discharge of several hundred pieces of cannon, the smell of sulphur was so strong that "the ship seemed to be nothing but sulphur." At this time the "Montague" was in 42° 48′ north latitude, and 9° west longitude, or about 55 geographical miles from the nearest land.

On the 31st of December, 1778, at three in the afternoon, the East India Company's ship "Atlas" was struck by lightning in the Thames. A sailor was killed in the cross-trees. The ship looked for an instant quite on fire, but it did not really receive any discoverable damage. Only a strongly sulphureous smell was produced, which lasted through the rest of the day and the following night.

The "New York," a packet, of 520 tons, was twice struck by lightning in the course of the 19th of April, 1827, in about 38° north latitude and 61° west longitude, or when its least distance from land was about 328 geographical miles.

At the first stroke, the ship being then unprovided with a proper lightning conductor, much damage was done; however, as the lightning found on its path many pieces of metal, these acted in a measure as conductors, and it passed off into the sea without setting anything on fire. The cabins were nevertheless filled with thick clouds of sulphureous smoke.

When the second explosion came the lightning conductor was properly mounted. For a moment the ship blazed with light as the first time, but no sensible damage was done. Nevertheless different parts, and especially the ladies' cabin, were suddenly filled with sulphureous vapours, so thick that nothing could be seen through them.

I now subjoin some curious instances of sulphureous vapours in houses or other buildings struck by lightning.

On the 18th of July, 1767, lightning entered the flues of six chimneys in a house in the Rue Plumet, at Paris, and left everywhere a suffocating smell, which powerfully affected the throat.

On the 18th of February, 1770, and for a long time after the thunderstroke which had struck down senseless many persons assembled at service in the church of St Kevern, in Cornwall, the church was filled with an almost suffocating sulphureous smell.

After the thunder-stroke which produced many serious or fatal accidents on the 11th of July, 1819, at Chateauneuf-les-Moustiers, (Basses Alpes), the church was so filled by a black and thick smoke, that the people had to feel their way across it.

The sulphureous odour is sometimes developed even where no luminous phenomenon has been manifested. I think that I may derive this inference from the following passage extracted from the account given to me by M. Rihouet, of the thunder-stroke experienced by the ship-of-the-line, the "Golymin," in 1812.

"In going round the ship after the accident," said M. Rihouet, "I was accompanied by an officer and the master gunner. On arriving at the great powder magazine, in the after part of the ship, I found it untouched, but when I had the adjoining bread-room opened, there issued from it a thick and black smoke and sulphureous smell, which nearly suffocated us, although the master gunner had opened the door only a very little way, and instantly reclosed it. We directly afterwards entered the place, and found to our great astonishment no trace of fire, but a complete overturning of its contents; more than twenty thousand biscuits had been tossed about without our being able to find any traces of the path which the fulminating matter must have followed to arrive at the spot."

CHAP. XVII.

ON THE CHEMICAL MODIFICATIONS IN ATMOSPHERIC AIR OCCASIONED BY THUNDER AND LIGHTNING.

AFTER the grand and celebrated experiment in which Cavendish succeeded, by the aid of the electric spark, in reuniting in liquid nitric acid the two gaseous elements of which the air which we breathe is composed, it could scarcely be doubted that lightning does not dart its fiery furrows across vast extents of atmospheric air without affecting it: many years, however, elapsed before a German chemist, Liebig, submitted this very natural idea to the test of effective experiments.

In 1827, the Giessen Professor published the analysis of the residual quantities obtained by the distillation of 77 samples of rain water collected in porcelain vases at 77 different times. Of these 77 samples, 17 were of rain which had fallen during thunderstorms; and these 17 all contained nitric acid in greater or less quantities combined with lime or ammonia. In the other 60 samples, collected when the rain was not accompanied by thunder and lightning, Liebig found only two in which there existed traces, (and in these, merely traces,) of nitric acid.*

We here see realised in meteorological phenomena one of the most brilliant results of modern chemistry. We see the sudden combination of azote and oxygen, effected in the experiments of the illustrious English chemist in closed vases, determined by the agency of lightning in the upper regions of the atmosphere. There is here a vast and important field for the experiments of chemists and physicists. It should be examined whether, other circumstances remaining the same, the quantities of nitric acid produced during thunderstorms vary with the seasons, and with the elevation, and consequently also with the temperature, of the clouds from which the lightning darts. It should be asked whether in the intertropical regions where, for months together, thunder and lightning prevail daily with so much force, the nitric acid, formed by their agency at the expense of the two gaseous elements of the atmosphere, may not suffice for the supply of those natural deposits of nitre, the existence of which, in certain localities where no animal matter was to be found, has been so difficult to explain. Possibly the course of such investigations might also lead to the discovery of the still unknown origin of some other substances, as lime, ammonia, &c., which Liebig has found in rain water which had fallen during thunderstorms. If even the only success obtained should

* The above was first printed in 1837, prior to the experiments of M. Barral. The important observations of this physicist lead to modifications in the conclusions of Liebig, to which we shall revert in the sequel.

consist in throwing light on the natural production of nitre, it would be a considerable gain. There would be moreover something particularly curious in showing that lightning or thunderbolts prepare and elaborate in the upper regions of the atmosphere the principal element of that other thunderbolt (gunpowder), of which men make such prodigious use for mutual destruction.

CHAP. XVIII.

LIGHTNING OFTEN FUSES PIECES OF METAL WHICH ARE STRUCK BY IT.

§ 1.

This chapter might consist of very few lines if all that was required was the establishment of the fact, that lightning fuses instantaneously thin metallic plates, or slender metallic threads, which it meets with in its course. But it is extremely important to become acquainted with the extent of this faculty; to examine what are the greatest thicknesses of such and such metals which have ever been melted by lightning; in short, to assign to this curious phenomenon, not its possible, but its observed limits; and that by extending the investigation to all times and countries.

Aristotle, in his Meteorology, lib. iii. chap. 1., after having enumerated the different kinds of lightning or thunderbolts distinguished by the ancients, says, in speaking of the effects of one kind, "the copper (or literally the coppering) of a shield has been seen to melt without the wood which it covered having been injured."

This property possessed by lightning of melting metals is also mentioned by Lucretius, Seneca, and Pliny. They especially cite iron, gold, silver, bronze, and copper. The singular comparative immunity remarked by Aristotle as enjoyed by wood, seems also to have presented itself to the Roman philosophers, in analogous circumstances. "Silver money," says Seneca, "is melted without the purse which contained it being injured .... the sword is liquefied in the scabbard, which remains unhurt; the iron of the javelin flows down the wood, and the wood does not catch fire." Pliny states, that "gold, copper, and silver, contained in a bag, may be melted by lightning without the bag being burnt, and without the wax of the seal upon it being softened." Lucretius speaks of the liquefaction of brass.

Unless we could suppose the power of lightning to have been prodigiously weakened in the course of several centuries, we should have great abatements to make from the above results.

"The sword is liquefied in the scabbard!" If by this is meant that a thunderstroke liquefied the entire metallic mass of a Roman broad-sword, modern observations present nothing similar. If the word liquefaction does not necessarily convey the idea of a general fusion,—if for its employment it sufficed that the blade should have presented here and there, or even throughout its entire extent, traces of a fusion limited principally to the surface,—then Seneca's fact of the fusion of the sword, even with the singular circumstance of the scabbard remaining unhurt, may be confirmed by instances taken from the meteorological annals of our own time.

In 1781, M. d'Aussac, and the horse on which he was riding, were killed by lightning, in the neighbourhood of Castres. M. Garipuy, of the Toulouse Academy, having after the catastrophe carefully examined the silver-hilted sword which M. d'Aussac had worn, perceived "two small places in the shell ornament of the hilt melted; one in the upper part and the other in the lower.

"Evident but superficial marks of fusion at the point of the sword blade, extending over a length of rather more than half an inch.

"Fusion of the surface of the piece of iron which formed the end of the scabbard; this piece was also pierced by an oblong hole, through which M. Garipuy could pass the flat broad blade of his penknife.

"Fusion of the upper edge of the sword blade at a distance of 13 inches from the hilt; the fusion being less than 3 tenths of an inch by 12 hundredths of an inch, with the additional circumstance that opposite to the melted portion the scabbard was not burnt, but only perforated, the hole being about a tenth of an inch in diameter."

M. de Gautran, who was by M. d'Aussac's side at the moment of the explosion, and whose horse was also killed, had a large hunting-knife (couteau de chasse), on examining which, M. Garipuy remarked "that the small silver chain which hung from the hilt to the guard had been melted near the guard and detached from it.

"That the silver which covered the hilt had been melted over a square surface of nearly 3 tenths of an inch, and throughout its whole thickness, which indeed was very inconsiderable. That the extremity of the blade, as well as the silver end of the scabbard, had been melted opposite to each other, and that in the interval between these two melted portions so close to each other, the intervening leather of the scabbard had been perforated and not burnt."

The reader will no doubt have remarked that M. d'Aussac's sword was fused not only at its two extremities, that is to say, at the points at which the lightning entered and went out, but also at the part where, according to all appearance, it divided itself between the horse and the rider.

We have here in a single event, well authenticated and well observed, fusion of silver, and fusion of two sword blades, without the scabbards having been burnt. But the fusion of the blades only took place over a superficial layer of small extent, and of which, moreover, we have reason to believe the thickness to have been exceedingly small. These two circumstances, especially the latter, being once admitted, we have, according to the true principles of the propagation of heat, a perfectly simple explanation of the scabbards not having been much injured, and of their not having taken fire. A comparison may serve to render any explanation unnecessary.

Any one who has brought a thin metallic thread or fine wire to a red or white heat, by holding it in the outer part of the flame of a wax candle or an Argand lamp, must have remarked the incredible rapidity with which the wire cools down when it is taken out of the flame. Not a second elapses between the moment at which the metal emits a brilliant light, and that in which it is completely dark. It has scarcely quitted the flame before it can be handled with impunity. This cooling, rapid as it is, would be even more so if the incandescent wire, instead of being suspended in the air, rested on a massive metallic blade of the ordinary temperature, which would draw away its heat by conduction. Now why may we not regard this wire as part of the greatly heated (or it may be even melted) superficial layer of small extent, which, as the result of the thunderstroke, formed into a coating at the surface of the metallic mass? As this layer cools with excessive rapidity, it is not wonderful that it should not ignite the leather or other analogous substance of which the scabbards were formed, in the case of M. d'Aussac's sword, and in that of the old Roman swords referred to by Pliny and Seneca.

§ 2.

On the 12th of June, 1825, at Cordova, the Marquesa de Paralez was thrown to the ground by a stroke of lightning, her shawl burnt, and a gold chain which she wore round her neck broken. Fragments of the chain were given me by M. Jose Mariano Vallejo who had himself been a witness of the event, and also a sufferer by it. I cannot perceive any distinct traces of fusion on the links. By what kind of action was the chain broken? I cannot tell.*

§ 3.

The expressions used by Pliny and Seneca as to the fusion of sword blades and pieces of money were long taken according to the fullest sense which the words could bear. The entire sword blade was supposed to have been melted, and thick copper, gold, or silver coins were supposed to have been completely and instantaneously liquefied. When this was once admitted, how could it be conceived that a wooden scabbard could have remained full of a heavy mass of incandescent iron without taking fire; or that the texture of a purse could have suffered without injury the prolonged contact of copper, silver, or gold, in a state of fusion? This difficulty, which appeared insurmountable, led Franklin to form a supposition, very strange certainly, but inevitable if the premises were admitted, *i.e.*, that lightning might have the property of effecting *cold* fusions; that by its instantaneous action the molecules of metals might without any development of heat be brought to that state of mobility which is implied by the word liquefaction. Subsequently,

* Silk threads gilt, when exposed to a very intense current of artificial electricity, present effects very suitable for the elucidation of the phenomena under consideration. The gold which covers the silk is volatilised without the threads being broken by the heat.

authentic observations, totally exempt from ambiguity, led him to recognise that his theory had been established on a false foundation as to fact; so true is it that the old story of the tooth of gold contains a lesson by which the most eminent men and of the clearest understanding may still in some degree profit.

I proceed to cite one or two observations affording moreover *direct* proof that fusions effected by lightning are not cold.

On the 16th of June, 1759, a house was struck by lightning in Southwark. Mr. William Mountain went immediately to visit it; he was shown the place where a bell-wire had been melted; he looked on the floor for its remains, and found them principally along the line vertically beneath that which the wire had occupied on the ceiling. These remains consisted of very small globules of iron contained in cavities of the boarded floor evidently burnt.

Although the above is sufficient to show that the fusion of the bell-wire took place by heating, I subjoin a few additional details and remarks. Among the globules extracted from the burnt depressions in the floor there were different sizes. The smallest pieces, having been completely fused, were perfectly spherical in shape, and the larger pieces deviated more and more from sphericity as their diameters were greater. The fall of all these burning particles explains, in a very natural manner, what was said by the servants who were in the apartment at the time, "We saw it rain fire in the room!"

After the explosion which struck the packet-ship New York (Chapter xvi. p. 63), the deck was seen to have globules of iron scattered over it, which burnt the wood in fifty different places, although torrents of rain were falling at the time, and hail was heaped up to a thickness of two or three inches.

§ 4.

Two facts have sufficed to show that lightning, like ordinary fire, fuses metals by heating them. We have now, as I before announced, to seek for the greatest effects of this kind which have ever been produced. Citations ought to abound on this subject, but unfortunately the little precision usually employed in accounts and descriptions of damage by lightning, reduces us, where we might have hoped for a rich harvest, to a mere gleaning.

I find in the Philosophical Transactions that, according to the account of Captain Dibden, in a chapel in Martinique struck by lightning in 1759, an iron bar of 25 millimetres, or nearly an inch, square, which was built into the wall, was reduced from its original size to that of a very thin wire.

If this diminution of the diameter of the bar was really caused by fusion, which is by no means certain, this fact would perhaps occupy the foremost rank among all those of the same kind which modern meteorology has been able to collect.

§ 5.

When the packet-ship New York, which has been already spoken of, received its second thunderstroke (19th April, 1827), there was at the mainmast head an iron rod, 1·2 metre (nearly 4 feet) long, 11 millimetres (or 0·43 of an inch) thick at its base, and terminating at its opposite extremity in a very sharp point.

The upper portion of this rod melted by the thunderstroke formed a cone of 11·8 inches long, and 0·24 inch diameter at the base. To the base of the rod was attached a flexible iron chain, like a Gunter's or measuring chain, consisting of links united by hooks at either extremity, and formed of iron 0·23 of an inch in diameter; the links being each about a foot and a half in length.

This chain was carried obliquely from the mast-head into the sea. Its length was certainly upwards of 130 feet. After the thunderstroke not more than three feet of it in all remained. About three inches were still attached to the base of the upper metallic rod, and there were found on the deck only two hooks with their intermediate ring quite puffed out and distorted, and a small fragment of chain. By referring to a preceding page in § 3. of this chapter, it will be seen why we are authorised in assuming that by far the greater part of the chain had been actually melted, and not merely broken and driven into the sea.

We conclude, therefore, that a thunderstroke may fuse, completely and throughout its extent, an iron chain of 130 feet in length, communicating with the sea by one of its extremities; the diameter of the iron forming the links being 0·23 of an inch.

§ 6.

Franklin, in 1787, found that a stroke of lightning had melted, at his own house in Philadelphia, a conical copper rod $9\frac{1}{2}$ inches long, and rather more than 3-tenths of an inch thick at its base.

This rod surmounted a thick iron bar, which was carried from the roof into the damp earth.

In 1754, Franklin had himself the opportunity of examining the effects of the violent thunderstroke which broke and dispersed in every direction the pyramid of timber, 69 feet high, which formed the steeple of the belfry tower, also built of timber, of the town of Newbury in the United States. After producing this extensive damage, the lightning, on arriving at the upper part of the square tower, followed an iron wire which connected the clapper of the bell with the wheel-work of the belfry situated much lower down.

The wire, which was of the thickness of a knitting needle, and nearly twenty feet long, had disappeared, with the exception of a piece under two inches in length, which after the accident still hung from the hammer of the bell, and another piece of the same length which was found attached to the clock-work. The place where the wire had passed along the plastered walls and the ceilings of two stories in the tower, was marked by a black furrow similar to that left by a train of powder after it has exploded. This kind of black pigment was no doubt composed of the substance of the wire reduced to impalpable molecules.

§ 7.

The first stroke sustained by the New York on the 19th of April, 1827, during its passage from America to Liverpool, melted a leaden pipe of rather more than three inches diameter and half an inch thickness, which communicated from the quarter gallery to the sea, through the sides of the ship.

§ 8.

Nature rarely proceeds per saltum: by the side of each effect there is always another of the same kind but a little less in degree, so that the transition between the least and the greatest takes place without breach of continuity. Imagine the thunderstroke which melted a particular metallic bar to be weakened, and the bar will no longer be melted, but will only be made to pass to that stage of incandescence and softness which would permit a blacksmith to weld it to another bar similarly prepared. Let the thunderstroke be weakened a degree further, and the bar will only undergo a certain amount of heating. One or two citations may serve to show that this is not an idle speculation.

On the 20th of April, 1807, lightning struck the windmill of Great Marton in Lancashire. A large iron chain used for drawing up the corn must have been, if not melted, at least considerably softened. The links being drawn downwards by the weight below them, were cemented to each other, and so firmly united that after the thunderstroke the chain was found to have become a solid bar.

The phenomenon observed at Great Marton was again presented, in June, 1829, at the Windmill of Toothill, in Essex, where in the same manner, after a violent thunderstroke, the links of the chain used for drawing up the corn-sacks were found cemented together.

§ 9.

On the 5th of April, 1807, lightning fell on the house of the forester of the wood of Vezinet, between Paris and St. Germain. After the event, a key was found to be cemented by its ring or handle to the nail on which it had been hung a short time before.

§ 10.

In March, 1772, lightning struck one of the four iron rods which rise above the highest point of the dome of St. Paul's in London. It was intended that by means of different intermediate pieces of metal, these rods should always be in immediate connection with large metallic rain-pipes descending to the earth. One of these communications was, however, slightly interrupted, and immediately adjacent to this want of perfect continuity of conduction, Messrs. Wilson and Delaval remarked effects which warranted them in believing that an iron bar four inches broad and half an inch thick had been rendered red hot by the action of the lightning.

§ 11.

For the object which I have in view it is not sufficient to have assigned the thicknesses of different pieces of metal which have been fused by lightning; the determination of thicknesses which have resisted its action will be no less useful.

There was in the town of Cremona a high tower surmounted by a weathercock which was struck by lightning in August, 1777. The rod of the weathercock passed through a marble pedestal; and the marble round the rod was shivered to pieces, and the fragments scattered in all directions. The heavy mass of the weathercock itself was thrown to a distance of twenty feet from the tower; it was also perforated. From these various circumstances we are entitled to consider the stroke to have been among the most violent which occur in our climates. Well, then we have to remark, that the rod of the weathercock, which was 0·47 of an inch thick, was broken, but showed no trace whatsoever of fusion.

§ 12.

On the 12th of July, 1770, a thunderbolt fell on the house of Mr. Joseph Moulde, at Philadelphia. Captain Falconer, who was in the house, says that the explosion was of prodigious intensity. Even if we had not had this statement, the great intensity of the stroke might have been inferred from six inches of a copper rod which rose from the roof (diameter unknown) having been fused. From this copper rod the lightning passed into a round iron rod of half an inch diameter, which was brought down the building and entered to the depth of nearly six feet into the earth. The iron rod was neither melted nor in any way injured.

§ 13.

The violent thunderstroke, cited in § 6. of the present chapter, as having overthrown and scattered the timber pyramid surmounting the tower at Newbury, was propagated along the iron rod of the clock pendulum without melting it, this rod not being thicker than a good sized goosequill.

The inference which may be derived from this observation, in regard to the faculty which rather thin metallic rods possess of transmitting very violent discharges, would be somewhat equivocal and open to discussion, if we were not able to show that the thunderbolt, the power of which on its striking the steeple at Newbury was abundantly evidenced by the extensive damage which it occasioned, still retained considerable force on its arrival at the pendulum rod: proofs of this are not wanting. On quitting the pendulum rod, the lightning in its descending march damaged the tower in several places. Stones were even torn from the foundation and projected to a height of six or seven and twenty feet.

§ 14.

While Captain Cook was in the harbour of Batavia, his ship was struck by lightning with such force that the shock was compared to that of an earthquake. No appreciable damage was done, however, in any part of the ship; only a copper wire two-tenths of an inch in diameter, which extended from the summit of the main-mast to the sea, appeared for a moment to be on fire.

CHAP. XIX.

LIGHTNING CONTRACTS OR SHORTENS METALLIC WIRES THROUGH WHICH IT PASSES WHEN ITS POWER IS NOT SUFFICIENT TO FUSE THEM.

It is probable that this singular contraction is produced whenever the lightning is not sufficiently powerful to fuse the metallic wire through which it passes. I am, however, only aware of one perfectly authenticated fact of this kind. It is due to the celebrated English Instrument Maker, Nairne.

On the 18th of June, 1782, lightning entered the house of Mr. Parker, at Stoke Newington. Different indications manifested that it had first passed down the vertical pipe on the outside of the house through which the rain-water from the roof was conveyed, and that it afterwards entered a bed-room, where it ran along the wire communicating between the bed and the door, for the purpose of enabling a person in bed to raise or lower at pleasure a drop-bolt affixed to the door of the room.

This wire was uninjured, and the positions occupied before and after the event by a ring at one of its two extremities, showed that in passing along about 16½ feet of wire the lightning had had the effect of shortening or contracting it two or three inches.

This contraction being proved, even in a single case, affords a simple explanation of how it happens that metallic wires stretched between fixed or nearly fixed points, are often broken by lightning strokes.

CHAP. XX.

LIGHTNING SOMETIMES FUSES AND INSTANTLY VITRIFIES CERTAIN EARTHY SUBSTANCES.

I HAVE already, in Chapter IV. p. 14, said a few words on the vitreous bubbles and coatings which geologists have observed on the most elevated rocks of Mont Blanc, the Pyrençes, and Toluca. I now subjoin some more precise details.*

In 1787, Saussure found on the summit of the Dôme du Goûté schistose masses of hornblende, covered with blackish bubbles and drops about the size of a grain of hempseed, and evidently vitreous. He was the more disposed to regard these bubbles as effects of lightning, because he had remarked similar ones on bricks which had been struck by lightning.

M. Ramond, who saw the same phenomena on several summits of the Pyrenees, did me the kindness of setting down at my request the following interesting notice:—

"The Pic du Midi is a mountain which stands in a great measure isolated, and rises high above surrounding points. Its summit is of very small extent; it consists of a mica schist of extreme hardness dividing into rather thick tablets, which adhere strongly to each other; it does not break into laminæ, but into parallelipipedons with oblique angles, like trap. Its colour is a grey black, rendered a little silvery by the presence of mica. The lightning acts upon it superficially only, causing a coating

* "Thunder-stones," said the Emperor Kanghi, "are metals, stones, or pebbles, which the fire of lightning has transformed by suddenly fusing different substances, and cementing them together, so that they cannot be separated. In some of these stones we can clearly perceive a kind of vitrification." (*Mém. des Missionaires*, tome iv.)

of yellowish enamel, or glaze, on which there are blisters or bubbles; these latter are sometimes spherical, sometimes, from the convexity having burst, concave; they are usually opaque, but sometimes semi-transparent. There are rocks whose entire face is varnished or glazed by this kind of enamel, and covered with bubbles often of the size of a pea; but the interior of the rock remains perfectly unchanged, the fused part not being more than four hundredths of an inch thick.

"The summit of the Mont Perdu, which I reached twenty years ago, presented to me the same phenomenon. It is almost entirely covered with snow, and does not, like the Pic du Midi, offer continuous surfaces of rocks, but only small fragments heaped together without order. These consist of a calcareous stone, bituminous and fetid, but containing a large intermixed portion of exceedingly fine quartzose sand. Many of these fragments bear evident marks of lightning. Their surface is loaded with bubbles of yellow enamel; and, as on the Pic du Midi, the fusion is only superficial, and does not penetrate to the interior of the stones notwithstanding their small size; and, what is not less remarkable, the heat, which has been sufficient to vitrify the surface, has not taken from the stone that cadaverous odour, from which we can so easily free it either by dissolving it in an acid or by heating it a little strongly.

"Lastly, at the Roche Sanadoire, a mountain in the Departément du Puy-de-Dôme, composed of clinkstone porphyry, and which I believe to be of volcanic origin, I saw, twelve years ago, the surface of the rocks vitrified and covered with bubbles from the action of lightning. Here also the fusion is superficial, and shows itself by bubbles and blisters in a glaze of very small thickness."

Messrs. de Humboldt and de Bonpland having ascended the highest summit of Toluca (to the west of the city of Mexico), found the surface of the rock called "*el Frayle*" vitrified. The rock is a red trachytic porphyry containing large crystals of lamellar feldspar, and a little hornblende. The vitrified part was about 6 feet square. The olive-green coating was only about ·004 of an inch thick, and resembled that of some aerolites. There were cavities in several parts of the rock, and the inside of these holes presented the same vitreous crust. The place where the illustrious travellers discovered these masses, is a kind of rocky tower, only nine or ten feet wide, which rises perpendicularly above the ancient crater of the volcano of Toluca, now full of water.

§ 2.

Saussure, Ramond, and Humboldt have all considered the bubbles and vitreous coatings of the Alps, the Pyrenees, and the Cordilleras to be without doubt the effects of lightning; but this opinion is not the result of direct observation; they arrived at it by the method of exclusion, and adopted it because no other explanation appeared to satisfy the circumstances of the phenomena. I pass, therefore, to facts in which there can be nothing equivocal.

On the 3rd of July, 1725, at Mixbury, in Northamptonshire, in the open country, the lightning fell on a flock of sheep, and killed the shepherd and five sheep. Near the shepherd's feet there were remarked two holes in the ground of three or four inches diameter, and about three feet deep. The Rev. Dr. Joseph Wasse examined these holes, by careful digging on every side of them, and found that they were cylindrical for half their depth. They then became narrower, and still lower down each of them forked into two branches. In the direction of one of these branches there was found a very hard stone about a quarter of a yard long, five or six inches wide, and four inches thick. This stone was divided in two by a recent crack, and its surface was vitrified.

§ 3.

In 1750, the tower of the Asinelli, at Bologna, was struck and injured by lightning. On carefully examining a brick which had more particularly received the stroke, Beccaria remarked that the very thin layer of mortar (sand and lime) which adhered to one of the faces of the brick had been completely vitrified over a space of three inches long, and an average breadth of seven-tenths of an inch. The vitreous layer was greenish, and very transparent.

§ 4.

On the 3d of September, 1789, lightning struck an oak in Lord Aylesford's park, and killed a man who had sought shelter under its branches. The stick which the poor man carried in his hand, and on which he appeared to have been leaning, seems to have been the channel or path principally followed by the lightning, for the ground at the spot on which the end of the stick rested was pierced by a perforation five inches deep, and two and a half inches in diameter. This perforation, which was examined only a few minutes after its formation by Dr. Withering, contained only some burnt roots of grass. This would probably have been all that would have been observed, if Lord Aylesford had not determined to have a little pyramid built on the spot where the fatal event had occurred, with an inscription upon it, warning passers by not to seek shelter under a tree during a thunderstorm. In digging for the foundation, it was found that the soil had been blackened in the direction of the perforation to a depth of about ten inches; two inches lower down the quartzose soil presented evident traces of fusion. The specimens sent to the Royal Society of London, with Dr. Withering's memoir, consisted of:

1st. A quartzose stone, one of the corners of which had been completely fused.

2nd. A block of sand agglutinated by the action of heat, for there was no calcareous matter among the grains. There was in this mass a hollow part where the fusion had been so perfect that the quartzose matter, after having flowed along the cavity, presented at the bottom of it a globular appearance.

3rd. Smaller pieces, all having " some hollow part."

CHAP. XXI.

LIGHTNING TUBES, OR FULGURITES.

My readers must now have become so familiar with the idea of fusions and vitrifications instantaneously effected by lightning, as to be quite prepared for the curious question, which has formed the subject of much animated debate, respecting lightning tubes, or fulgurites.

Lightning tubes had been discovered more than a century ago (1711) by the Pastor Herman, at Massel, in Silesia, as is proved by specimens preserved in the Mineralogical Collection of Dresden. It is to Dr. Hentzen that belongs the honour of having rediscovered them in 1805, in the Lande, or sand plain of Paderborn, commonly called the "Senne," and of having been the first to point out their origin. Since then they have been found in great numbers at Pillau, near Königsberg; at Nietleben, near Halle, on the Saale; at Drigg in Cumberland; in the sandy country at the foot of the Regenstein, near Blankenburg; and in the sands near Bahia, in Brazil.

At Drigg the tubes have been found amidst mounds of moving sand more than forty feet high, very near the sea. In the Senne they have been most often found on the sides of sand hills, which are about thirty feet high, and sometimes also in cavities which look as if they had been sunk artificially, and are about 200 or 230 feet in circumference, and 13 to 16 feet deep. At Nietleben, the tube dug out by Mr. Kaserstein was about half way down the south-east side of a sand hill.

Fulgurites are almost always hollow. At Drigg their total or external diameter was two inches. Those of the Senne have at the surface of the ground openings varying from two hundredths to six-tenths of an inch. They contract in descending, and often terminate in a point. The thickness of the sides varies from two-hundredths of an inch to an inch.

They usually descend vertically into the sand, but they have often been found in oblique directions inclined at an angle of 40 degrees with the horizon.

Their total length sometimes exceeds thirty-three feet. Numerous transverse fissures divide them into fragments varying in length from four-tenths of an inch to five inches. The sand surrounding the tubes dries, and perhaps rolls away in course of time, and these fragments are then found on the surface of the ground, and are driven about by the wind.

Most often on digging in the sand only a single tube is found, but sometimes, after reaching a certain depth, the principal tube divides into two or three branches, each of which sends out smaller lateral branches from an inch to nearly a foot long. These last-mentioned small branches are conical, and are terminated by points which incline gradually downward.

The inside part of lightning tubes is perfect glass, smooth, and very bright, like vitreous opal or hyalite. It scratches glass and strikes fire as a flint.

All these tubes, whatever may be their forms, are surrounded by a crust composed of agglutinated grains of quartz. This external crust is sometimes smoothly rounded, but it more often presents a series of asperities not unlike in appearance the rugosities on the small branches of the Dutch elm, or the crevassed bark on the lower part of the trunks of old birches. The irregularities of the internal vitreous channel correspond with those of the external surface; it is as if during the fusion the whole tube had been crumpled in different directions.

When examined with a magnifying glass, the black and white grains which compose the outer crust of the fulgurites appear rounded, as if they had experienced a commencement of fusion. At a certain distance from the centre the white grains acquire a reddish tinge.

The colour of the internal mass, and especially that of the external parts, depends on the nature of the layers of sand traversed by the tubes. In the upper layers, containing a little vegetable soil, the outside of the tubes is often blackish; lower down they are of a yellowish grey; lower still, of a greyish white; and lastly, where the sand is pure and white, the tubes are also almost perfectly white.

What is the origin of these tubes? Are they incrustations formed around roots which have disappeared? Are they stalactites, or other productions of the mineral kingdom? Are they cells or dwellings belonging to ancient inhabitants of the sea, of the class of worms? Or lastly, are they products of lightning?

These four different suppositions have been put forward. The three first may be disposed of by a single remark.

At Drigg, where the mounds of sand are continually liable to displacement by the action of the winds, the tubes must have been of recent date, for unless supported on all sides they break with the slightest shock.

As regards the fourth supposition, let us see whether the indications of fusion which the tubes present throughout their whole extent, preserve merely the vague character of indications, or whether they acquire that of decisive proofs when submitted to the test of minute experiment.

At Drigg the sand in which the tubes were discovered consists of white or reddish grains of quartz mixed with some grains of hornstone porphyry. The latter, when presented alone to the blowpipe, are easily fused, but they are not in sufficient quantity in the sand to act as a flux.

A quantity of the sand taken generally, and treated in the same way, first becomes red, then passes into an opaque white, and ends by being slightly agglutinated; it then resembles, both in tint and cohesion, the outside layer of the fulgurites, or lightning tubes.

The same sand being exposed to the flame of a spirit-lamp, heightened by a current of oxygen gas according to Dr. Marcet's method, and the action kept up for some time, gave an enamel analogous to that which clothes the inside of the tubes. The fusion was however imperfect, and yet we know that Dr. Marcet's lamp fuses thick platinum wire with a vivid scintillation. Analogous experiments made with the sand of the Senne gave the same results.

At a certain distance from the centre of the fulgurites the sand of the crust became, as was said above, of a reddish tinge. Thrown into hydrochloric acid, the red sand parted with its colour, and became like the sand taken from those layers where it is whitest and most pure. The liquor having been poured off and submitted to alkaline re-agents, traces of iron were manifested.

The ordinary sand of the Senne, after having been exposed for some moments to strong heat in a platinum crucible, became reddish, and then much resembled that which surrounds the tubes, with only the difference of being a little redder. When the crucible had attained red heat the resemblance became perfect.

The sand thus reddened in the platinum crucible being subjected to the action of hydrochloric acid, parted with its colour, just as does the reddish sand of a fulgurite tube: the liquor poured off presented the same traces of iron, and, after the iron had been completely precipitated, traces of lime.

What then remains yet wanting for a full recognition that fulgurites originate in the action of lightning? One thing more only is wanting, *i. e.*, the discovery of one of these tubes at the very spot in the sandy region towards which the lightning should have been seen to direct itself. Even this proof can be supplied.

Dr. Fiedler, who has published in Germany Memoirs containing detailed investigations concerning fulgurites (*Blitzrohre*, lightning tubes), reports (though indeed simply on hearsay) the two following facts: —

A druggist of the colony of Frederichsdorf, on visiting the place where two men had just been struck by lightning, discovered in the ground two tubes quite similar to those of the Senne.

On the borders of Holland, in a district consisting entirely of sand, a shepherd having seen lightning strike a particular sand-hill, found that at the very spot towards which the flash had appeared to him to be directed, the sand had been melted and had run into the form of a tube.

Lastly, we have a fact which disposes of every difficulty.

On the 17th of July, 1823, lightning struck a birch tree near the village of Rauschen (in the province of Samland, near the Baltic), and at the same time set fire to a juniper bush. Several persons, who hastened to the spot from the village, saw near the tree two narrow and deep holes, one of which, notwithstanding the rain, felt warm to the touch. Professor Hagen of Könisberg caused the holes to be carefully dug round. The first, which was the one which had been found to be warm, presented nothing particular, nor did the second offer anything remarkable for upwards of a foot in depth; but a little below this a vitrified tube commenced. The fragility of the tube, the necessary result of the extreme thinness of its walls, rendered it impossible to extract it otherwise than in small fragments, from about an inch and a half to two inches long. The interior vitreous surface was very shining, of a pearl grey colour, and speckled throughout its whole extent with black points.

After this instance, in which, as Mr. Hagen has said, "Nature was taken in the fact," no one can doubt that lightning can make a way for itself through sand, can bring that substance in an instant to the state of fusion, and give to it, throughout the great length of thirty or forty feet, the form of a hollow tube vitrified on the inside.*

* I know not if I am right in the view, but it seems to me that a fact recorded by Bayle in his works is even more extraordinary than all the phenomena of fusion and instantaneous vitrification which I have been speaking of. The following is the story: —

"Two large drinking glasses, exactly alike, stood side by side on a table. Lightning entered the apartment and appeared to dart so directly to the glasses that it seemed as if it must have passed between them. Neither of them, however, was broken. In one, Bayle noticed a very slight alteration of form, but the other had been so much *bent* (which necessarily implies softening), that it could hardly stand upright on its base."

CHAP. XXII.

LIGHTNING SOMETIMES PIERCES WITH SEVERAL HOLES BODIES WHICH ARE STRUCK BY IT.

In the autumn of 1778 lightning fell on the house of the Engineer Caselli, at Alexandria. The only appreciable damage which it caused was in the glass of a window, the panes of which were pierced by one, two, or three holes of nearly two-tenths of an inch diameter. The glass was starred round the holes, but none of the panes were cracked from side to side.

In August 1777, lightning struck the Parish Church of the Holy Sepulchre at Cremona, broke the iron cross on its summit, and threw to a distance the weathercock (of tinned copper, and covered with a coat of oil paint), which had been placed immediately below the cross.

The weathercock was pierced by eighteen holes; the edges of nine of these holes stood out prominently on one of the faces of the weathercock; and the edges of the other nine holes were equally prominent on the opposite side.

There were no indications which led the inhabitants of Cremona to suppose that the weathercock had received several strokes of lightning. If, however, we were to have recourse to the supposition of repeated strokes, in order to explain the multiplicity of holes, the opposite prominences of their edges would oblige us to admit nine strokes in one direction, and exactly the same number in the opposite direction. The particular manner in which the holes are grouped would require that by a singular chance the strokes coming from opposite directions should have struck, in pairs, nearly contiguous parts of the metal; and, lastly, the nearly identical inclination of all the projecting edges would no less imperatively require that the eighteen strokes should have been parallel.

Unless I am much mistaken, the improbability of the concurrence of all these conditions would lead all persons to agree with the opinion adopted by the physicist to whom the first description of the phenomenon is due, viz. the belief that the eighteen holes found in the Cremona weathercock were the result of a single stroke of lightning.

On the 3rd of July, 1821, lightning struck a house situated near the Protestant Church of St. Gervais at Geneva. On a minute examination of the effects produced by it, the Editors of the *Bibliothèque Universelle* perceived several holes, bearing evident marks of recent fusion, in the sheets of tin laid over the great beam from which the roof sloped. The most remarkable effect of the kind was that which had been produced on a new tin sheet, placed round the lower part of a chimney where it issued from the roof, and again bent so as to cover part of the slope of the same roof. This sheet was perforated in two places, the holes being almost circular, with diameters of about an inch (1·18), their centres being five inches (5·1) distant from each other; the edges of the tin, round these holes, were turned in one direction in one, and in the opposite direction in the other, both in a very decided manner.

On the subject of holes with opposite edges produced by lightning, I find in the *Giornale* of Pietro Confiliachi and Gaspare Brugnatelli (1827, p. 335.) an observation of Doctor Fusinieri, which is, I think, especially remarkable on account of the circumstance that the holes with turned back edges appear not to have been formed at the point where the lightning first struck. The following is the translation of the words of the Italian physicist: —

"On the 25th of June, 1827, about eight in the evening, lightning struck the house No. 1349 at Vicenza. The tin horizontal gutter running along under the eaves to receive the rain, was first struck, and suffered a rent of four or five inches. A vertical tube running from it to carry off the rain was pierced with three holes. The upper one, an inch in diameter, had no turning back of the edges, either inwards or outwards. Six and a half inches lower down, there was a nearly circular hole with edges turned inwards, and, three inches lower still, another hole of the same size as the last, and having the edges turning back on the outside."

CHAP. XXIII.

TRANSPORT OF BODIES OCCASIONED BY LIGHTNING.

The phenomena in which masses sometimes of considerable weight are removed to notable distances, are well deserving of being studied. I will cite some instances.

In the night between the 14th and 15th of April 1718, a stroke of lightning blew off the roof, and overturned the walls of the church of Gouesnon, near Brest, as a mine of gunpowder would have done. Stones were thrown in all directions; some as far as 167 feet.

The stroke of lightning which at some former time struck the Castle of Clermont, in Beauvoisis, made a hole $25\frac{1}{2}$ inches wide, and $23\frac{1}{2}$ inches deep, in a wall, the building of which is referred by general tradition to the time of Cæsar, and which, at any rate, was so hard that a pickaxe could scarcely make any impression on it. The fragments struck from the hole were dispersed in various directions to distances of more than fifty feet ($52\frac{1}{2}$).

During the night between the 21st and 22nd June 1723, lightning struck and shattered a tree in the forest of Nemours. The two broken off pieces of the trunk were, one above sixteen feet long, and the other twenty-three feet. Four men could not have lifted the smaller one, yet the thunderstroke sent it to a distance of forty-nine or fifty feet. The second, and larger, was found above sixteen feet off in a direction opposite to that of the first; its weight exceeded the power of eight men to move it.

In January, 1762, lightning struck the steeple of the church of Breâg, in Cornwall. The south-western pinnacle of masonry was shattered into a hundred pieces, and totally demolished. One stone, weighing three cwt. was thrown over the roof of the church to the southward to a distance of sixty yards. Another stone was found 400 yards from the steeple, towards the north, and a third in a south-west direction.

"At Funzie, in Fetlar (in Scotland), about the middle of the last century, a rock of mica-schist, one hundred and five feet long, ten feet broad, and, in some places, four feet thick, was in an instant torn by a flash of lightning from its bed, and broken into three large, and several lesser, fragments. One of these, twenty-six feet long, ten feet broad, and four feet thick, was simply turned over. The second, which was twenty-eight feet long, seventeen broad, and five feet in thickness, was hurled across a high point to the distance of fifty yards. Another broken mass, about forty feet long, was thrown still farther, but in the same direction, quite into the sea. There were also many lesser fragments scattered up and down." (Dr. Hibbert, from MSS. of Rev. George Low, of Fetlar, cited by Sir Charles Lyell in vol. i. p. 260. of his Principles of Geology.)

On the 6th of August, 1809, at Swinton, about five miles from Manchester, lightning produced on a part of Mr. Chadwick's house, remarkable mechanical effects, of which I subjoin a description, postponing for the present the consideration of their explanation.

A small brick building for holding a store of coals, with its upper part forming a cistern, was placed with its back against one of the walls of the house; its own walls were thirty-five inches thick, ten and a half feet high, and their foundations went down nearly a foot below the surface of the ground.

On the 6th of August, at half-past two in the afternoon, after repeated discharges of distant thunder, which seemed to be approaching, a terrible explosion was heard. It was immediately followed by torrents of rain. Sulphurous vapour surrounded the house for a few minutes.

The external wall of the little building, forming the coal-cellar and cistern, was torn from its foundations and lifted bodily. The explosion transported it upright, and without overturning it, to some distance from its original place. One of its extremities had moved nine feet and the other four feet.

The wall thus lifted and removed consisted, without counting the mortar, of 7000 bricks, and might weigh about twenty-five tons.

At the time there was about a ton of coals in the cellar and some water in the cistern. (*Manchester Memoirs*, vol. ii. 2nd series.)

M. Liais relates that during the storm which burst at Cherbourg in the night of the 11–12th July, 1852, lightning struck the mizen mast of the "Patriote," which was in the port. The mast was split for a length of eighty feet, and several fragments were thrown to a great distance. The force of projection was such that a piece 6½ feet long, nearly 8 inches square at the thickest end, and the other extremity pointed, staved an oak timber planking more than an inch thick, situated 262½ feet distant. It entered the planking by the thick end, nearly half its length went through, and it was stopped by a knot in the timber.

CHAP. XXIV.

MAGNETIC ACTION OF LIGHTNING.

Lightning in passing near the needle of a compass affects, and sometimes altogether destroys, its magnetism, and sometimes inverts its magnetic poles. Under similar circumstances it may convert into magnets of more or less strength bars of steel which previously showed no traces of magnetism.

These properties of lightning are undoubtedly very curious. My readers will not, I think, be sorry to learn how they were discovered; and they will also wish to know whether the reversal of the poles of compass needles is a very rare phenomenon. The following citations will convey the desired information on both these points.

About the year 1675, two English vessels were sailing in company from London to Barbadoes. Not far from the Bermudas a thunderstroke shattered the mast and rent the sails of one of the ships while the other sustained no damage. The captain of the latter, seeing that his consort had altered her course, as if making for England, asked the cause of this sudden change of purpose, and found, much to his astonishment, that her captain and crew believed themselves to be still following the same course as before. An attentive examination of the compasses of the vessel which had been struck by lightning showed that the characteristic mark on the compass cards, which, before the stroke, pointed, as is usual, towards the north, now pointed on the contrary to the south, showing that the poles had been completely reversed by the lightning. This state of the compasses continued throughout the remainder of the voyage.

Boyle relates that in the month of July, 1681, the ship "Albemarle," being then about a hundred leagues from Cape Cod, was struck by lightning. Much damage was done to the masts, sails, &c. When night came, and the stars appeared, every one perceived by them that of the three compasses on board, two, instead of pointing north as before, pointed south, and that in the third compass the mark formerly indicating north now pointed to the west.

A lightning stroke took place on the English ship "Dover," Captain Waddel, on the 9th of January, 1748, in 47° 30′ N. latitude, and 22° 15′ W. longitude from Greenwich. The mainmast, deck, cabins, and some parts of the planking, suffered more or less. The poles of the needles of the four compasses on board were reversed; the north end had become the south, and *vice versâ*.

Some years ago a stroke of lightning destroyed the magnetism of four compasses on board the brig "Medusa" during its passage from La Guayra to Liverpool. Of these four instruments two were on deck and two in the captain's cabin. (*Silliman*, vol. xii. 1827.)

The thunderstroke which has been already repeatedly mentioned as having fallen on the packet ship "New York," in 1827, had the effect of either greatly lessening, or actually destroying, the magnetism of the needles of the four compasses on board.

The reversal of the poles of compass needles by the influence of lightning must be an occurrence more frequent than physicists imagine. In the short interval, 1808 to 1809, I was myself almost an eye-witness of two events of this kind. The first occurred on board the French corvette "La Baleine," which I saw enter the harbour of Palma, at Majorca; the second case was that of a Genoese ship which was wrecked on the coast at some distance from Algiers, having come on shore while the captain, deceived by the anomalies which lightning had caused in the compasses, thought himself sailing towards the north.

In the instance of the "Albemarle," Boyle's account, of which I have availed myself, mentions a compass-needle, which after a thunder-stroke pointed to the west. Nautical journals cite cases in which, by the influence of lightning, the needles have been permanently turned towards the N.N.W., N.W., or S.W.,

&c. &c. To say the same thing in other words: lightning would seem to have not only the property of changing the poles of needles from north to south, and *vice versâ;* and of effecting a similar change from north or south, to east or west, but also of making all intermediate alterations; so that the change would not necessarily be of 180° or of 90°, but might have any value, from 0° to 180°.

It has been, it appears to me, without any valid reason, that these facts have been regarded as impossible. Compass-needles are usually very elongated lozenges of steel; the poles occupying the two extremities of the greater diagonal. But with a little care, and suitable manipulation of the natural or artificial loadstones or magnets employed for magnetising the needles, the poles could be brought to coincide with the extremities of the lesser diagonal, which would then place itself approximately in the meridian, and the longer diagonal would thus mark the east and west points.

That which may thus be done by the use of magnetising bars or loadstones may also sometimes be done by lightning. The action of this meteor may transfer the poles from the acute to the obtuse angles of the lozenge, or to any other point intermediate between these two extreme positions. After such a change the point of the fleur-de-lis, which the artist had carefully adapted to the north pole would correspond to some other point; and what wonder, therefore, that its new direction should be, according to the amount of displacement, north-west, north-east, west, or east, or any intermediate point?

I have certainly taken the most unfavourable conditions of which the case could admit, by assuming the needles of sea compasses to have been always made of compact masses of steel of a certain breadth. Compass needles were formerly composed of two distinct steel wires, a little bent at the middle. Being brought together at their ends, these wires formed the outline of what was in those days an open or empty, instead of, as is now the case, a filled-up lozenge. One of the wires formed the two right sides, and the other wire the two left sides, of the lozenge. At the two extremities of the greater diagonal (*i.e.* at the two acute angles of the lozenge), there was between the two wires only a simple contact, or simple juxtaposition. In such a system there is room for the most complicated distribution of magnetism, by the formation of consecutive points, and thus for all those varied anomalies which have been unduly ascribed to the mistakes of seamen.

CHAP. XXV.

IMPARTATION OF MAGNETISM BY THE ACTION OF LIGHTNING.

We will proceed from cases in which lightning has modified the state of bodies previously magnetised to cases in which it has been itself the magnetising agent.

In June, 1731, a tradesman had placed in the corner of his room at Wakefield a large case containing an assortment of knives, forks, and other steel or iron articles to be sent to the colonies. Lightning entered the house precisely at this angle, broke the box, and scattered its contents. The knives and forks, whether showing marks of fusion, or appearing perfectly uninjured, had all become strongly magnetic.

After the "Dover" had been struck by lightning in January, 1748, Captain Waddel perceived that many pieces of steel and of iron, situated near the binnacle, had become strongly magnetised.

I have read, somewhere, a story of a stroke of lightning in a shoemaker's shop in Swabia, which had the effect of so magnetising all the tools, that the poor artisan could no longer make use of them. He had to be constantly freeing his hammer, pinchers, and knife from the nails, needles, and awls, which were constantly getting caught by them as they lay together on the bench.

When the "New York" arrived at Liverpool, in May, 1827, after having been twice struck by lightning, Dr. Scoresby found that the nails of the partitions and pannels which had been broken, the iron fastenings of the masts which had fallen on the deck, the knives and forks which at the instant of the discharge were in the biscuit room, and lastly, that the steel points of the mathematical instruments, had become very decidedly magnetic.

The effects of lightning on the needles of sea-compasses have often led to very serious consequences. We have already stated a case, in which, after a stroke of lightning, the crew, deceived by the false indications of their compasses, wrecked their ship on the very dangers from which they thought they were receding as fast as their sails could carry them. Lightning, by instantaneously magnetising the multitude of pieces of hard iron or steel throughout a ship may moreover create powerful centres of attraction; and hence, even without the compasses being themselves affected, local deviations may result, the more hurtful because on the high seas the navigator has but few means at his command for ascertaining their existence, and especially for determining their value. Nor are these two kinds of perturbations the only ones to be guarded against. When lightning magnetises the different pieces of steel which enter into the composition of a chronometer, and especially its balance wheel, a new force, that of the earth's magnetism, becomes superadded to those which originally regulated the march of these admirable but very delicate pieces of mechanism. This new force sometimes causes sensible accelerations or retardations; and from these result, after a certain number of days' navigation, very dangerous errors in the supposed geographical longitude. For example, the chronometers of the "New York" were on their arrival at Liverpool 33^m 58^s in advance of what they would have given if the ship had not been struck by lightning.

When M. Rihouet was hurt by the lightning which struck the "Golymin" in the night of the 21st-22nd February, 1812, all the steel parts of a repeating watch, which hung near his head, were magnetised. Twenty-seven years afterwards the magnetism so imparted still continued.

The danger which may arise to navigators from the effects of lightning on the march of their chronometers has only been remarked of late years.

CHAP. XXVI.

LIGHTNING IN ITS RAPID MARCH IS INFLUENCED BY ACTIONS DEPENDENT ON THE TERRESTRIAL BODIES NEAR WHICH IT EXPLODES.

NOTHING appears to me more suited to show that in its prodigiously rapid course lightning is still governed by forces dependent on the nature and position of terrestrial bodies near which it explodes, than the account addressed to Nollet by the Count de Latour-Landry, in July, 1764, relative to the thunderstroke which struck the church of Antrasme, near Laval.

On the 29th of June, 1763, in the midst of a violent thunderstorm, lightning struck the steeple, and entered the church of Antrasme, where it either fused or blackened the gilding of picture frames, and of decorations round certain niches; it left blackened and half burnt the small pewter-flasks intended to hold the wine for the celebration of the mass, which stood at the top of a small cupboard; and, lastly, it opened two deep holes, as regular as if they had been drilled with an augur, in the credence table painted to resemble marble, in a niche of soft stone.

All these injuries were repaired, the places where the gilding was damaged were regilt; the holes were plugged, and the painting restored. In the following year, on the 20th of June, 1764, lightning again struck the same steeple, and entered the church, where it blackened the same gildings as in 1763, and no more, melted exactly where it had melted before, blackened and burnt the two same pewter flasks as the year before, and, lastly, the two holes which had been stopped and painted, had the plugs driven out of them.

Those who will take the trouble of reflecting upon the thousands of combinations which might have caused the paths of the lightning to have been different in the two years, will, I imagine, have no hesitation in viewing, with me, the perfect identity of effect as demonstrating the truth of the proposition placed at the head of this chapter.

Lightning struck, at Peronne, on the 10th of September, 1841, a room where twenty-five years before the poet Béranger had narrowly escaped being killed by a stroke of lightning.

CHAP. XXVII.

WHEN THE ATMOSPHERE IS TEMPESTUOUS THERE ARE SIMULTANEOUSLY GREAT PERTURBATIONS IN THE INTERIOR OF THE EARTH AND AT THE SURFACE OR BELOW THE SURFACE OF WATERS.

DAVINI wrote to Vallisneri that he had observed, near Modena, a spring whose waters were limpid in clear weather, and became troubled when the sky was covered. I do not know whether this remark has been since confirmed; but, at any rate, Vallisneri did not throw any doubt on the subject. He added, as the result of his own observations, that the *salses* of Zibio, Querzola, Cassola, &c., in the same Duchy of Modena, and the Solfaterras, announce a thunderstorm before it bursts, and even before it is formed, by a certain kind of ebullition, by sounds similar to those of thunder, and sometimes even by true fulminating strokes.

Toaldo cites two similar phenomena of which he had personal knowledge, and which I will report.

In the Vicentine Hills, at a little distance from the parish church of Molvena, there is a fountain or spring which the inhabitants call Bifoccio, because it really does include two springs. When a thunderstorm is preparing, this fountain, even after a long drought, and even when it appears quite dried up, suddenly overflows its basin and fills a wide channel with muddy water which spreads into the neighbouring valleys.

Before proceeding to Toaldo's second instance, I will notice a fact, the analogy of which with the two last mentioned will be readily seen, and which leads to the same inferences. Not far from Perpignan (in the Département des Pyrénées Orientales) an Artesian well had been bored which at first furnished an abundant supply of gushing water. The quantity, however, rapidly diminished, and this was attributed by the inhabitants to a choking up of the lower part of the hole, I should almost say to the formation of an earthy piston. One day when the sky was covered with heavy storm-clouds, there was heard a subterranean bubbling sound, soon followed by an explosion, after which the Artesian well again yielded the same quantity of water as at first.

About three miles from the spring of Bifoccio, near the parish church of Villaraspa, in the court-yard of Signor Pigati of Vicenza, there is, as described by Toaldo, a deep well, which at the approach of a thunderstorm seems in a state of ebullition, and sounds issue from it so as to spread alarm among the neighbouring inhabitants.*

I think I may venture to affirm that the study of the two cases of which I have been speaking offers a degree of interest far exceeding that which attaches to many objects which are sought in distant parts of the earth.

We learn from Brugnatelli's journal that on the 19th of July, 1824, after a thunderstorm, the waters of the Lake of Massaciuccoli, in the territory of Lucca, became white as if a great quantity of soap had been dissolved in them. This state still continued on the 20th. The following day a number of dead fish, both large and small, were found on the bank.

Do not these circumstances present a double indication of some subterranean emanation making its way during the thunderstorm of the 19th through the muddy bottom of the lake?

Historians and meteorologists have mentioned local inundations, of which the effects have appeared to be much beyond what would have been apprehended from the quantity of rain which had fallen from the clouds within a given radius. In such cases it has most often happened that immense masses of water have been seen to rise for a greater or less time from underground by apertures previously unknown; and also a violent thunderstorm has been the precursor, and probably the primary cause, of the phenomenon. Such were in all respects, for example, the circumstances of the inundation, which in June, 1686, destroyed almost the whole of the two villages of Kettlewell and Starbotton

* This is perhaps the proper place for saying a few words on the subject of the rolling subterranean noises heard during thunderstorms, by persons placing themselves near several of the natural apertures by which the celebrated Lake of Zirknitz periodically fills and empties itself. Valvasor tells us that two of these openings are called in the language of the country (Carniola), "Vella," and "Mala-Bobnaza," signifying the "lesser" and the "greater drum." This is certainly quite enough to prove a subterranean noise; but in this case there is a doubt (which, as we have seen, does not exist at Villaraspa, since at that place the phenomenon manifests itself before the storm bursts), viz.: Is the noise a simple acoustic phenomenon, a succession of echoes; or does it result from a sort of subterranean storm subordinate to the atmospheric storm? We have not sufficient data for determining between these different hypotheses.

in Yorkshire. During the thunderstorm an immense crevasse was formed in the neighbouring mountain, and according to the accounts of eye-witnesses, the mass of water which rushed forth from it contributed at least as much as the rain which fell, to the lamentable misfortunes which followed.

I might analyse a great number of cases similar to the above, but as from their nature they must always leave on the mind some uncertainty, and must be in some degree equivocal in their bearings, I will content myself with one more citation, having the high authority of Beccaria.

In October, 1755, a sudden inundation caused extensive devastation in most of the Piedmontese valleys. The Po overflowed. This disaster was preceded by dreadful thunders ("orrendi tuoni"). According to the general, and indeed unanimous belief, its principal cause was the immense volume of subterranean water, which during the thunderstorm issued from the interior of the mountains by new openings.

These local ruptures of the solid crust of the globe would not be extraordinary, if it should be proved that when thunderstorms are gathering, water has a tendency to rejoin the clouds, manifesting itself by decided phenomena of intumescence. This is precisely what appears to be evidenced by observations made on board the packet-ship "New York," in April, 1827.

While the thunder rolled over and around the ship, the sea boiled in a manner which might have been ascribed by the beholders to the action of submarine volcanos. More especially remarkable were three columns of water which shot up into the air, fell back foaming, and rose again, again to fall back in the same manner.

There is at the Mont d'Or, in Auvergne, a very ancient building, in the middle of which there is a stone basin hollowed out of a single block, and called in the country *Cæsar's basin.* It is three feet three inches wide, and nearly four feet deep. It has at the bottom two openings, through which two columns of water, issuing from the ground, rush forth bubbling or boiling, that is to say, occasioning a noise, or kind of eructation, the intensity of which, according to the often repeated observations of Doctor Bertrand, is considerably augmented when thunderstorms are gathering.

The inhabitants of the valley had themselves made the same remark, saying that the noise of the gushing spring of Cæsar's basin gave them notice of the approach of thunderstorms, and that it was a sign which never deceived them.

Such a phenomenon is assuredly deserving of careful and continued observation and examination. It would be also very useful to science to examine whether, as Berzelius thought he had remarked, well corked flasks containing water charged with carbonic acid really burst much more frequently during thunderstorms than at other times, especially if it could be also proved that vibrations communicated to the glass by the detonations of the thunder, had no share in the effect observed by the illustrious Swedish chemist.

The celebrated Duhamel du Monceau says that lightning, unaccompanied by thunder, wind, or rain, has the property of breaking oat-stalks. The farmers are acquainted with this effect, and say that the lightning beats down the oats.

On the 3rd of September, 1771, Duhamel himself witnessed this phenomenon at the Chateau of Denainvilliers, near Pithiviers. In the course of the preceding night, or rather morning, there had been much lightning. When daylight came it was found that all the ripe ears, with fine bunches of grain, had been broken off at the first joint of the stalk. The green ears only were still upright. The farmers determined to mow the whole.

Duhamel also reports as positively assured that lightning, when buckwheat is in flower, prevents the proper setting of the grain, and causes it instead to drop off.

In reference to the action exercised by the atmosphere on vegetation when thunder is in the air, the following fact is vouched for by the Editors of the "Bibliothèque Britannique de Genève," of which one of them had himself been witness. I transcribe their own expressions.

In the month of May of last year (1795), the bark was being stripped from the trees of an oak wood situated on a height two leagues from Geneva. This can only be done at the season when the sap, being in flow between the wood and the bark, destroys their mutual adhesion sufficiently to permit the easy separation of the bark; and the workmen generally remark, moreover, that the state of the atmosphere has a marked influence on the facility and success of the operation. One day, the wind being from the north, and the sky clear, the barking was only effected with great difficulty. After twelve o'clock the sky became clouded in the west, muttering thunder was heard, and that moment the bark came off, as it were, of its own accord, to the great surprise of the labourers, who were all much struck by the phenomenon, and who agree in ascribing it to the state of the atmosphere with the less hesitation, as the effect ceased when the disposition to thunder had passed off." (*Bibliothèque Britannique*, vol. ii. p. 221.)

I pass over many prevalent opinions respecting the effects of thunder in turning milk sour, spoiling wine or beer, and hastening the corruption of meat, &c. I am not aware of any precise experiments establishing the exact truth in these points. The unanimous assertion of cooks, wine or beer sellers, butchers, &c., may justly call attention to the subject, but cannot afford a substitute for proofs.

CHAP. XXVIII.

THE EXCEPTIONAL STATE IN WHICH ATMOSPHERIC STORMS PLACE THE SOLID PART OF THE GLOBE SOMETIMES MANIFESTS ITSELF BY FULMINATING EXPLOSIONS, WHICH, WITHOUT ANY LUMINOUS APPEARANCE, PRODUCE THE SAME EFFECTS AS THUNDER AND LIGHTNING PROPERLY SO CALLED.

I AM only acquainted with one direct observation justifying the above; but it is so clear, direct, and to the point, and Mr. Brydone had collected all the circumstances with such intelligent and enlightened care, that it scarcely seems admissible even to entertain a doubt respecting the conclusions which follow from it.

On the 19th of July, 1785, between noon and one o'clock, a storm burst in the neighbourhood of Coldstream. While it lasted, there took place in the surrounding country several remarkable accidents, which I propose to analyse.

A woman who was cutting grass near the banks of the Tweed fell on the ground. She immediately cried out, and told her companions that she had just received from beneath her feet, she could not tell in what manner, a most violent stroke. At that moment there had not been, within the horizon, lightning or thunder seen or heard.

The shepherd of the farm of Lennel-hill saw, only a few feet off, a sheep which had appeared to be in perfect health a few moments before, suddenly fall. He ran to raise it up, but found it was quite dead. At that time the thunderstorm seemed to be at a great distance.

Two tumbril carts laden with coal, each driven by a lad sitting on a small front seat, had crossed the Tweed, and had just climbed to the top of a steep ascent near the river bank, when there was heard all around a strong detonation, resembling the noise made by the nearly simultaneous discharge of several fowling pieces or muskets, but without any rolling sound. At the same instant, the driver of the hindmost cart saw the cart before him, the two horses, and his companion fall to the ground. The driver and the horses were quite dead! Let us scrupulously examine the details of this event.

The wood of the cart was found to be much damaged, especially where there were nails and other iron fastenings.

Many lumps of coal were scattered to a distance on every side of the cart. The appearance of several among them was as if they had been for some time on the fire.

The ground was pierced by two circular holes at the very spot on which the wheels rested at the moment of the explosion. Half-an-hour after the event these holes emitted an odour which Brydone compared to that of ether.

The circular iron bands round the tires of the two wheels showed evident marks of fusion at the parts which had rested on the ground at the moment of the explosion, but nowhere else.

The hair of the horses had been burnt, particularly that of the legs, and under the belly. In examining the print made in the dust which covered the road, it was evident that at the moment of their fall the animals were quite dead; that they fell as inert masses, without experiencing any convulsive movements.

The body of the unfortunate driver presented here and there marks of burning. His clothes, shirt, and especially his hat, were reduced to tatters. They gave out a strong smell.

We have here undeniably the principal effects of an ordinary stroke of lightning: yet the detonation was not preceded by any flash of lightning or any sort of luminous phenomenon. This remarkable fact is vouched for by the driver of the second cart, who at the moment of the accident was chatting with his companion, about twenty yards ahead, and saw him fall without having perceived any light. We may also refer to the evidence of a shepherd belonging to the neighbouring farm of Saint Cuthbert, who declared to Mr. Brydone that he was watching the two carts when the detonation took place, and that the fall of the cart, horses, and driver was accompanied by the formation of a whirling cloud of dust, but that he saw no lightning nor any appearance of fire. Lastly, we have to add, that at the time of the accident Mr. Brydone had stationed himself before an open window with some of his friends for the purpose of showing them by a seconds watch, which he had in his hand, how to deduce the distance of the storm-clouds from the interval of time elapsing between the flash and the noise; and that he heard the loud detonation of which we are speaking without its having been preceded by any flash of lightning.

Great drought had prevailed in the country for a considerable time when the accident which has been described took place.

CHAP. XXIX.

THE PARTICULAR STATE WHICH AN ATMOSPHERIC STORM COMMUNICATES TO THE SOLID AND LIQUID PART OF THE GLOBE BY ITS INFLUENCE, IS SOMETIMES MANIFESTED BY BROAD AND BRILLIANT PHENOMENA OF LIGHT, OF WHICH THE EARTH IS AT FIRST THE SEAT, AND WHICH, AFTER AN EXPLOSION HAS TAKEN PLACE, DISAPPEAR, EITHER BY VANISHING ON THE SPOT WHERE THEY WERE FIRST SEEN, OR BY A MORE OR LESS EXTENSIVE, AND MORE OR LESS RAPID CHANGE OF PLACE.

THE fact which I am about to relate proves that by the influence of a thunderstorm flames may be developed under water and shoot upwards from its surface.

In the night of the 4th—5th September, 1767, during a violent thunderstorm, the farmer of a pond, near Parthenai, in Poitou, saw the surface of the pond covered throughout its extent by a flame so thick as to hide the surface of the water. The next day all the fish in the pond were floating dead on its surface.

Lastly, it appears that great luminous meteors, of a nature analogous to lightning, are sometimes formed at the surface of the earth, even when no thunderstorm appears in the sky. I find the proof of this in an event at sea, which has been already cited summarily in a preceding chapter (chapter x. p. 51.), for a different object.

On the 4th of November, 1749, in 42° 48′ north latitude and 9° west longitude from Greenwich, a few minutes before noon, and in clear and serene weather, a ball of bluish fire of the apparent size of a large millstone, advanced rapidly towards the English ship "Montague," rolling on the surface of the sea. When within a small distance of the ship, it rose almost perpendicularly, not above forty or fifty yards from the main chains, and exploded with a noise equal to that of several hundred cannons fired at once. The maintopmast was shattered into above a hundred pieces, and the mainmast was rent quite down to the keel. Five sailors were thrown to the ~~ground~~ senseless, and one of them was severely burnt.

The fulminating nature of the phenomenon appears to me to be shown by the sulphurous smell which filled the whole ship, and more particularly by the circumstance that some of the spikes that nailed the fish of the mainmast were drawn with such force out of the mast, and sunk so fast in the main deck, that the carpenter was obliged to take an iron crow to get them out.

The cause of these luminous phenomena, to avail myself of the fine expression of Pliny, "still remains hidden in the majesty of nature."

Independently of the problematical fires of which we have spoken, which during thunderstorms are visibly produced at the surface of the ground, remaining stationary there for a time, and only quitting it to explode at a small height above it, as in the case of the fires of Posdinovo and of Dijon, — Maffei, Chappe, and others, deem that lightning or thunderbolts are almost always elaborated on the ground; that it is from the ground that they suddenly dart; that instead of descending from the clouds to the earth, their course is, on the contrary, upwards, from the earth to the clouds.

Those who are partisans of this opinion, say that they have distinctly seen lightning rise like rockets. Admitting as a fact the exceedingly rapid rate of movement which results from Mr. Wheatstone's experiments, it is difficult to conceive the possibility of distinguishing by the eye whether a flash of lightning between the clouds and the earth rises or descends. I know not, however, how to charge so many practised observers with error. May it be that like those globular lightnings of which we have spoken so much at length in chapter vii., lightnings ascending from the earth may move more slowly than those which are formed in higher parts of the atmosphere? This subject calls for fresh researches. Any person who shall have seen—distinctly seen—a flash of lightning attached to the earth at one of its extremities, and not reaching the surface of the clouds by its other extremity, will have carried the question a decided step in advance.

CHAP. XXX.

FIRES OF ST. ELMO.

DURING THUNDERSTORMS, VIVID LIGHTS, ACCOMPANIED BY A SLIGHT HISSING SOUND, ARE OFTEN SEEN ON THE MOST PROJECTING PARTS OF TERRESTRIAL BODIES.

In thunderstorms, the projecting parts of bodies, and especially the metallic parts, sometimes shine with a rather vivid light, which the ancients designated by the names of Castor and Pollux. These fires are now most generally known as the Fires of St. Elmo. In some parts of the Mediterranean they are called St. Nicholas, St. Clare, and St. Helena.

Cæsar's Commentaries contain one of the most ancient accounts of these phenomena which have been preserved to us. In the book on the war in Africa we read: "This same night (a night on which there was a thunderstorm and a heavy fall of hail) the iron heads of the javelins of the Fifth Legion appeared on fire."

Seneca relates that near Syracuse a star placed itself on the iron of the lance of Gylippus, and rested there.

We read in Livy that the javelin with which Lucius Atreus had just armed his son, newly enrolled as a soldier, threw out flames for upwards of two hours without being consumed.

Pliny had himself seen similar lights at the points of the pikes of soldiers placed by night on guard on the ramparts.

Plutarch speaks of similar observations made in Sardinia and in Sicily.

Procopius tells us, that in the war against the Vandals, Belisarius was favoured by heaven with a similar prodigy.

The above appears to me to be a sufficient collection of instances of flames appearing on land, at the points of lances, javelins, &c. The same authors would furnish me with a much greater number of citations relative to analogous apparitions taking place during thunderstorms in different parts of ships.

Plutarch, for instance, relates, that at the moment when Lysander's fleet was coming out of the Port of Lampsacus to attack the Athenian fleet, the two flames, called the stars of Castor and Pollux, appeared on either side of the galley of the Lacedemonian Admiral.

In ancient times, the apparition of flames on the masts, yards, or rigging of ships was regarded as an omen. They were, therefore, observed with great care, and the accounts scrupulously collected by historians. A single flame was called Helena, and was regarded as a menacing sign. Two flames, Castor and Pollux, announced, on the contrary, fair weather and a successful voyage.

In case my readers should be curious to know in what point of view navigators in the time of Columbus regarded these phenomena, I borrow from the "Historia del Almirante," written by his son, the following passage, which bears strongly the impress of the ideas of the 15th century.

"On the night of Saturday," (in October 1493, during Columbus's second voyage), "there was very heavy thunder and rain. St. Elmo appeared at the mast-head with seven lighted tapers; that is to say, we saw the fires which sailors believe to be the body of the saint. Litanies, prayers, and thanksgivings were then heard all over the ship, for seamen believe that as soon as St. Elmo appears the dangers of the tempest are certainly past. Whatever may be thought of this opinion, and whatever may be the fact respecting it," &c. &c.

We learn from Herrera that Magellan's sailors entertained the same superstition. He says: "During the great tempests St. Elmo appeared at the mast-head, sometimes with one and sometimes with two lighted tapers. These apparitions were hailed by acclamations and tears of joy."

Perhaps a close examination might show that the prestige which surrounded the fires of St. Elmo in ancient times has been preserved to a much later period than is usually supposed. The singular comparison, or rather assimilation, of these flames to lighted church tapers is not met with in the accounts of voyages in or after the middle or end of the 17th century. Perhaps, however, it may have been the original source of another also rather strange opinion, which caused the fires of St. Elmo to be looked upon as material objects which might be laid hold of at the mast-head and brought down to the deck. I borrow from the "Mémoires de Forbin" a passage which will present these ideas in all their *naïve* simplicity, while at the same time it will serve to show the enormous dimensions to which the fires of St. Elmo sometimes attain.

"During the night" (it was in 1696, approaching the Balearic Islands), "the weather suddenly became exceedingly dark, accompanied with dreadful thunder and lightning. Fearing the violence of an impending tempestuous wind, I had all the sails furled. We saw about the ship more than thirty fires of St. Elmo. One in particular, at the mainmast-head, was more than a foot and a half high. I sent a sailor to fetch it down. When the man was aloft he called out that the flame made a noise like gunpowder fired after it has been wetted. I bade him take off the vane and come down; but he had hardly detached it from its place, when the flame left it and placed itself on the end of the mast from whence it could not be got off. It staid there for some time, until it had gradually burnt out."

If I were to close my citations here, my readers might, perhaps, have reason to suppose that the fires of St. Elmo were more active formerly than in modern times. I will therefore relate a few facts of more recent date, showing, as formerly, luminous appearances presenting themselves during thunderstorms, at the extremities of all kinds of bodies, even at a very low height.

In the Itinerary of Fynes Moryson, Secretary to Lord Mountjoy, we read, at the date of 23rd December, 1601, that at the siege of Kinsale, while the sky was traversed by lightnings (unaccompanied by thunder), the horsemen who were patrolling saw "*lamps burn*" at the points of their lances and swords.

On the 25th of January, 1822, during a heavy fall of snow, M.

de Thielaw, who was travelling towards Freyberg, remarked that the ends of the branches of all the trees by the roadside appeared luminous. The light appeared slightly bluish.

On the 14th of January, 1824, after a thunderstorm, Mr. Maxadorf, in looking at a waggon laden with straw which was immediately beneath a thick black cloud in a field near Cothen, observed that the straws on the top stood on end and appeared on fire. At the same time the driver's whip threw out a bright light. The phenomenon disappeared as soon as the wind had carried away the black cloud; it had lasted ten minutes.

On the 8th of May, 1831, after sunset, some artillery and engineer officers were walking, during a thunderstorm, on the terrace of the fort of Bab-Azoun, at Algiers. They were bareheaded, and each, on looking at his companions, saw with astonishment their hair standing on end and tipped with light. When they raised their hands, similar luminous points formed at the ends of their fingers. (*Voyage de M. Rozet.*)

During the thunderstorm of the 8th of January, 1839, when lightning struck the church tower at Hasselt, some labourers who were on the dyke between Zwolle and Hasselt, near the last-named town, observed a singular phenomenon. A few moments before the explosion of the thunderstroke which fell on the church, they remarked that their clothes were covered with fire. As they were making vain efforts to rid themselves of these flames, they looked around and saw with increased terror the trees and masts sparkling with the same flame; the thunderstroke resounded, and immediately the flames disappeared. (*Journal de la Haye.*)

It will perhaps be said, is it not surprising that phenomena which develop themselves with such intensity near the ground, and on the projecting parts of ships, should be so rarely remarked on the points of church steeples, or on those of the weathercocks with which the roofs of so many houses are surmounted? I have but a word of reply: I believe the fires of St. Elmo are not seen at the summits of high edifices only because they are not looked for there. Wherever there have been attentive observers, summits of every kind have recovered their rights in this respect.

Watson recorded an account which he had received from France, in which it was stated that M. Binon, Curé of Plauzet, had remarked, throughout a period of twenty-seven consecutive years, that during great thunderstorms the three points of the cross on the church-steeple appeared surrounded by flames.

In Germany the summit of the tower of Naumburg, had been cited as a singular instance of the apparition of flames, but in the month of August, 1768, Lichtenberg saw similar flames on the tower of St. James at Göttingen.

On the 22nd of January, 1778, during a violent thunderstorm accompanied by rain and hail, M. Monger saw several of the highest pinnacles in the city of Rouen surmounted with luminous points.

In 1783, M. Sauvan published, that on the 22nd of July, there being a thunderstorm that night, he had seen for three-quarters of an hour a crown of light surrounding the ball of the steeple of the church of the Augustines at Avignon.

Before closing this chapter it may not be without use to remark that in atmospheric circumstances, apparently at least, quite similar, and during thunderstorms of equal intensity, the sort of fires which we have been considering have nevertheless, I do not say merely different degrees of intensity, but are also very dissimilar in shape, most often appearing as a plume; but occasionally the light is concentrated into a small globe, without any trace of diverging jets or points of flame.

CHAP. XXXI.

DURING GREAT THUNDERSTORMS, DROPS OF RAIN, SNOW-FLAKES, AND HAIL-STONES, PRODUCE LIGHT ON REACHING THE GROUND, OR EVEN ON ENCOUNTERING AND STRIKING AGAINST EACH OTHER.

MANY physicists having denied the reality of this phenomenon, I have thought it right to collect with particular care all the instances in which it has been observed. My readers will thus be enabled to form their own judgment on the subject.

Hallai, the Prior of the Benedictines, at Lessay, near Coutances, wrote to Mairam: "On the evening of the 3rd of June, 1731, the thunder was extraordinarily loud, and there fell at the same time rain resembling drops of molten metal."

In 1761, Bergman wrote to the Royal Society of London: "I have twice observed, towards evening, without thunder, rain fall of such a character, that everything sparkles at its contact, and the ground seems to be covered with waves of fire."

It might seem as if northern countries were more suited than others for the production of luminous rain, as in the very small number of citations which I am enabled to make on this subject, there is another which it will be seen belongs to Sweden.

During the morning of the 22nd of September, 1773, in the district of Skara, East Gothland, thunder and lightning were accompanied by a very abundant rain; afterwards the heat was exceedingly oppressive; at six the rain recommenced, and then, all the accounts agree in stating that every drop darted fire as it reached the earth.

On the 3rd of May, 1768, near La Canche, two leagues from Arnay-le-Duc, Mr. Pasumot was overtaken by a heavy thunderstorm in the open country. As he stooped to allow the rain which had accumulated on the brim of his hat to run off, the water from his hat, in falling from a height of about two feet, met that which fell directly from the clouds, and sparks were struck out by the collision.

On the 28th of October, 1772, on the road from Brignai to Lyons, the Abbé Bertholon was overtaken by a thunderstorm about five in the morning. Rain and hail fell in great abundance. The rain-drops and hail-stones, which in falling struck the metallic parts of the saddle of the horse which the Abbé rode, flashed light.

An acquaintance of the celebrated meteorologist, Howard, related to him, that being at night on the road between London and Bow, during the violent thunderstorm of the 19th of May, 1809, she distinctly saw the rain become luminous as it reached the ground.*

The above is all that I have been able to collect on the subject of luminous rain. Hail and snow will afford me only one or two citations.

Bergman, in the letter written in 1761, which has been already noticed, after speaking of rain which became luminous

* During a thunderstorm some travellers remarked that in spitting, the drops were luminous almost on issuing from their mouths. As the fright experienced by them at this event might be renewed in the case of others, it may be well thus to notice the circumstance, which, moreover, is not without some importance in respect of theory.

on touching the ground, says that he had sometimes observed the same phenomenon in heavy falls of snow.

On the 25th of January, 1822, some miners at Freyberg told Lampadius that the small hail which fell during a thunderstorm was luminous when it reached the ground.

To save those who may seek for an explanation of these phenomena from going astray, by looking for its cause in any properties peculiar to water in either a liquid or a frozen state, I have to remark that showers of dust have also been observed to be luminous.

Thus the dust as fine as Havannah snuff, which fell on the town of Naples and its neighbourhood during the eruption of Vesuvius in the year 1794, emitted a phosphoric light, pale indeed, but distinctly visible at night. An Englishman, Mr. James, who was at the time in a boat near Torre del Greco, remarked that his own hat, and those of his boatmen, and particularly the parts of the sails on which the dust had principally collected, gave out a sensible degree of luminosity.

CHAP. XXXII.

GEOGRAPHY OF STORMS.

ARE THERE PLACES WHERE IT NEVER THUNDERS? IN WHAT PLACES IS THUNDER MOST FREQUENT? IS THUNDER AS FREQUENT IN MODERN AS IN ANCIENT TIMES? DO LOCAL CIRCUMSTANCES INFLUENCE THE FREQUENCY OF THIS PHENOMENON? IS THUNDER QUITE AS FREQUENT ON THE HIGH SEAS AS IN THE MIDDLE OF CONTINENTS? WHAT AS TO FREQUENCY IS THE GEOGRAPHICAL DISTRIBUTION OF THUNDERSTORMS IN THE PRESENT DAY?

Botany, zoology, entomology, &c. &c., have all given occasion to curious and important geographical classifications, and it may therefore naturally be expected that I should say something about the geography of thunderstorms. In the absence of a satisfactory solution of the questions above announced, I will at least attempt to show the course to be followed when sufficient documents shall have been collected.

First question. Are there places where it never thunders?

Pliny (Hist. Nat. lib. ii. § 52.) says that it does not thunder in Egypt. At the present day thunder is frequent at Alexandria, and occurs three or four times a year at Cairo.

In Plutarch's treatise on Superstition, we read, "He who never goes to sea does not fear the waves, nor he who does not follow arms, war. He who stirs not from home does not fear highway robbers; nor does the dweller in Ethiopia fear thunder."

I am by no means disposed to believe that in the time of Plutarch thunder was never heard to the south of Egypt, as is implied by this passage. Since thunder occurs at Cairo, and is very frequent in Abyssinia (at Gondar, for instance), I venture to affirm (although I am not in possession of direct observations), that thunder is general throughout the extent of ancient Ethiopia.

If, however, I am unable to name any place within the warm or temperate regions of the old continent where thunder is never heard, it is quite otherwise in America.

Those among the inhabitants of Lima in Peru (12° S. lat. and 77° 10′ W. long.), who have never travelled, can form from their own experience no idea of thunder. We may add, that they are equally unacquainted with lightning, for even noiseless and sheet lightnings never appear in the atmosphere of Lower Peru, often misty, but never showing true clouds.

I now pass from the tropical to the frigid zone.

In 1773, from the end of June to the end of August, the "Racehorse," commanded by Captain Phipps, was constantly navigating the Spitzbergen seas. During the course of these two summer months, thunder was not once heard, nor was a single flash of lightning seen.

My friend, the Rev. Dr. Scoresby, formerly so celebrated a whaling captain, and who has given so interesting a description of the polar seas, says that in his numerous voyages he only twice saw lightning beyond the parallel of 65°.

In Captain Parry's attempt to reach the North Pole, his party travelled over the ice with their sledge boats from the 25th of June to the 10th of August 1827, between 81° 15′ and 82° 44′ latitude. In this interval, they never saw lightning or heard thunder.

The "Hecla" remained at anchor from the 20th of June to the 28th of August. At Hecla Cove, on the coast of Spitzbergen, in 79° 55′ north latitude, none of the observers on board or on shore ever heard thunder or saw lightning.

Lastly, the "Hecla" navigated the icy seas between 71° 28′ and 79° 59′ lat. from the 1st of May to the 19th of June, and between the 28th of August and the 16th of September crossed the zone comprised between the 80th and the 62nd parallel. During these periods also no indications of thunderstorms were perceived.

From all these documents it may be affirmed that, beyond the 75th parallel of latitude, thunder and lightning are unknown in the open sea and among islands.

The observations of Captain Ross's Expedition corroborate this result. In 1818, the ships commanded by that officer were from the beginning of June to the end of September in Davis Straits and Baffin's Bay, between 64° and 76½° north latitude. The meteorological tables corresponding to this season do not mention a single flash of lightning or sound of thunder.

Captain Parry's Expeditions enable us to extend to regions much surrounded by land the rule which we have so far only been entitled to apply to extensive seas and to islands.

The meteorological tables of the first voyage of this intrepid navigator to Baffin's Bay, Barrow's Strait, and Melville Island commence in June 1819, and extend to September 1820 inclusive. This makes two summer seasons (or seasons of thunderstorms); and during the whole of these two seasons passed between 70° and 75° N. lat., thunder and lightning were never once heard or seen.

Placing ourselves a very little way on this side of the 70th parallel of latitude, we find thunder very rare, perhaps scarcely heard once a year, but we can no longer say that we are absolutely beyond the region of thunderstorms.

The meteorological tables of Captain Parry's second voyage to Baffin's Bay embrace the interval comprised between the 1st of June 1821, and the 30th of September 1823, or twenty-eight months, including three complete summers or seasons of thunderstorms. During this long interval, and in latitudes all a little below 70°, I find the following entry, but that one only:— "6th of August, 1821. Several very vivid flashes of lightning were seen to the westward, and succeeded by hard rain for some hours."

The latitude on the 7th of August must have been about 65° 30′.

At Fort Franklin, latitude 67½°, and longitude 120° 12′ W., from the beginning of September, 1825, to the end of August, 1826, or during an entire year, Captain Franklin and his companions only heard thunder once, May 29. 1826.

The meteorological tables at the same station, for the interval comprised between the beginning of September, 1826, and the middle of May, 1827, only mention one day of thunder, the 11th of September, 1826.

During his arduous expedition in the northern parts of America, Captain Back experienced in the beginning of August, 1834, a violent storm of thunder and lightning at Point Ogle, in 60° 20′ N. latitude, and 95° W. longitude.

Iceland has been often cited as a country where thunder is never known. The word *never* must be changed. Mr. Thortensen, Doctor of Medicine in that island, has favoured me with his valuable meteorological observations made at Reikiavik (latitude 65°), from the 20th of September, 1833, to the 30th of August, 1835. In this space of nearly two years, I find one day, the 30th of November, 1833, in which thunder was heard.

Second question. What are the places at which there is most thunder?

Although we have been able to cite one country, situated in the equinoctial zone (Lower Peru), where there is no thunder, yet, generally speaking, it is in tropical countries that there is most thunder. The numerical table, which will form the most suitable conclusion of this chapter, will show that in France, England, and Germany, the mean annual number of days of thunder and lightning rarely amounts to twenty, whereas, at Rio Janeiro, and at places in India, it is above fifty. M. Boussingault, who has had much opportunity of forming an opinion in connection with his interesting explorations of the Cordilleras, thinks that in the neighbourhood of the equator on all days of the year, and probably at all hours of the day, electric discharges are continually taking place in the atmosphere, so that an observer, if gifted with sufficiently sensitive organs, would hear continually the sound of thunder.

Third question. Is thunder as frequent now as in former times?

Meteorologists who desire to compare the ancient and modern states of the globe in respect to temperature, rain, atmospheric pressure, magnetism, &c., find themselves without any point of departure because the ancients possessed neither thermometers, rain gauges, barometers, or compasses of any kind. The question placed at the head of the present paragraph is a more simple one, for which instruments would not seem to have been necessary. If instead of discussing at great length, and very uselessly, the physical cause of the meteor, Pliny, Seneca, &c., had condescended to tell us how many days in each year it thundered at Rome, Naples, &c., the comparison of their numbers with those which are found in meteorological tables of our own time would lead to curious results. It is manifestly impossible to find any substitute for such data: I have, however, thought that I might permit myself to seek in an assemblage of the records of thunderstrokes preserved by historians, certainly not a real solution of the question proposed, but a mere glimpse, or simple indication, which might in our uncertainty make the balance incline on one side rather than on the other.

Herodotus says, "Xerxes entered the country of Ilium, having Mount Ida on the left hand. But as they passed the night at the foot of that mountain, many of their men were destroyed by thunder and lightning."

It will be seen presently that according to the information which I have been able to collect, thunder is not now more frequent in Asia Minor than in European climates; and I certainly much doubt whether in reports to the Ministers at War, thunder and lightning have ever figured amongst the causes accounting for the diminution of armies, and I have no idea that any of our generals have ever had occasion to speak, as does Herodotus, of having lost many men in such a manner.

Pausanias relates that when a Lacedæmonian army was encamping under the walls of Argos, many soldiers were struck by lightning.

I consider that I have obtained sufficient evidence to prove that at the present time Attica and the Peloponessus are not remarkable either for the number or the intensity of their thunderstorms. The tendency, therefore, of inferences drawn from the account of Pausanias (as well as from that of Herodotus) would be to lead us to believe that there has been a notable diminution of thunder and lightning in Greece since ancient times. But I must point out a circumstance which materially lessens the importance of Pausanias's evidence, as bearing on the point of the ordinary mean annual amount of this atmospheric phenomenon. It is that the lightning and thunderbolts from which the Lacedæmonian army suffered so much, coincided with a dreadful earthquake.

Pliny the Naturalist furnishes me with the following passage:—

"In Italy, during the war, the construction of towers between Terracina and the Temple of Feronia was abandoned because all those which had been built had been overthrown by lightning."

"Many towers overthrown by lightning," is an effect probably much beyond what now takes place in the territory of Terracina in a considerable number of years.

I might, perhaps, avail myself of the very just remark, that if the history of ancient nations is full of fables, on the other hand their fabulous history abounds in true historical events. Whereas modern history does not afford a single example of any personage of note struck by lightning, we find in the Greek Poets the names of Salmoneus, Capaneus, Semele, Enceladus, Typhon, Ajax (the son of Oileus), Esculapius, Adimantus Prince of Phlios, Lycaon, &c. &c. If the poets should appear too uncertain authorities to be adduced in a question of physics, I would cite the death of Tullus Hostilius (according to Livy), and of Dionysius of Halicarnassus; the death of the Emperor Carus, struck by lightning in his tent about the year 283, according to Flavius Vopiscus: the death of the Emperor Anastasius the First: and the account of lightning darting across the litter in which Octavius Augustus was travelling when among the Cantabrians, and killing the slave who preceded it as torch-bearer.

Ctesias states that in his presence Artaxerxes caused an experiment to be made at his own risk and peril, which consisted in averting a thunderstorm by the aid of a sword planted in the ground. The risk and peril of such an experiment, even during the most violent thunderstorm, would in the present day be thought too slight to mention.

Those persons, then, who are persuaded, very erroneously I believe, that ancient authors always measured their words and statements according to a severely just and true standard of fact and reason, may, if they please, find in this passage of Ctesias a proof that thunderstorms had formerly a degree of intensity unknown in modern days.

Such a conclusion has, indeed, been drawn, not only from the cases of persons struck by lightning, but also from the supposed very considerable number of monuments in and around Rome which had been so struck. Let us proceed, as far as we can, to enumerate the cases appealed to.

Ancient authors had spoken of a thunderbolt which struck the walls of Velletri as being an indication of the high destinies of a citizen of that town: it was the birthplace of Augustus.

Suetonius mentions that after the death of Cæsar the monument of his daughter Julia was struck by lightning; he also speaks of a thunderbolt having struck a portion of the palace of Augustus on the Palatine Hill.

The same historian remarks that some time before the death of Augustus a stroke of lightning destroyed the first letter of that emperor's name in the inscription on the pedestal of his statue.

Under the reign of Caligula lightning struck the Capitol of Capua and the temple of Apollo Palatine at Rome.

Lightning struck the monument of Drusus, the father of the Emperor Claudius, a few days before the death of the latter.

While recognising that, as respects the point in question, each of these historical facts taken separately would be of little importance, it may be admitted that, taken together, they perhaps give a very slight degree of probability to the idea that thunder-storms have diminished in intensity since ancient times.

The number of instances of thunderbolts recorded by ancient writers, taken in connection with the periods over which their dates extend, is, however, far from being so considerable as the arguments on the subject might seem to imply. In comparing them with corresponding phenomena in modern times, the effect of lightning conductors in lessening the number of such instances, ought not to be forgotten.

Without attempting to give precise elements for such a comparison, I may mention, that the church of St. Geneviève was partly destroyed by lightning in 1483; that previous to the establishment of a lightning conductor on the spire of Strasbourg cathedral, that magnificent edifice was almost annually visited and injured by lightning; that a few years ago the dome of the Invalides was struck; that a recent stroke of lightning rendered it necessary to take down entirely one of the towers of St. Denis; and that within a very limited extent of territory, on the coast of Brittany, M. la Pezlaie has been able to register the following cases of buildings struck by lightning:—

Spire at Brasport, in 1817.
Steeple of the church at Crozac, in 1822.
Steeple of the parish church at Auray, in 1828.
Spire at Pluvigner, in 1831.
Steeple of Locmaria-Plabennec, in 1833.
Church of St. Michel at Quimperlé, in 1833.
Steeple of the church of Plougean, near Morlaix, in 1843.
Steeple of the church of Bercran, near Landerneau, date unknown.

Fourth Question. –Do local circumstances influence the frequency of the phenomenon?

The answer to this question cannot be doubtful, since we have already remarked that one country, Lower Peru, where it never thunders, corresponds in geographical position to the regions where, generally speaking, there is most thunder. As, however, the absence of thunderstorms in Lower Peru is accompanied by the absence of clouds, properly so called, which are there replaced by a singular, opaque, permanent vapour or fog, known in the country by the name of *garua*, other citations are desirable.

I am disposed to place in the foremost rank, one which I take from a work published at Glasgow, in 1835, by Mr. Graham Hutchison, entitled "On Meteorology, Marsh Fevers," &c.

In Jamaica, from early in November to the middle of April, the summits of the Port Royal Mountains begin, between eleven o'clock and noon, to be covered with clouds. At one o'clock these clouds have attained their greatest degree of density; torrents of rain fall from them, lightnings dart across them in every direction, and the rolling of the distant thunder is heard as far as Kingston. Towards half-past two the sky recovers its serenity.

Mr. Hutchison states that these phenomena are of daily occurrence during five consecutive months. If we take this as an exact observation, it gives for Kingston 150 days in the year when thunder is heard (apart from the summer months), while in the neighbouring islands, and in places on the continent similarly situated in climatological respects, the number of days of thunder is under fifty; thus manifesting the great influence exercised by the Port Royal Mountains in the production of thunderstorms.

This permanence of thunderstorms in Jamaica, in regard to which it is very desirable that meteorology should be furnished with more circumstantial and precise data, is said also to be met with at some points of the neighbouring continent. M. Boussingault writes to me that during a certain season of the year it thunders almost every day at Popayan; and that he had himself counted in one month (the month of May), more than twenty days of thunderstorm. The fact had indeed been remarked before, for no one in those countries disputes with the inhabitants of Popayan the honour of "having the most mighty thunder in the republic."

The equinoctial regions would furnish me, if needed, with other examples. I might cite in the neighbourhood of Quito the Valley of Chillo, in which, according to the general belief of its inhabitants, there is much more thunder than in neighbouring countries; but I wish rather to hasten to the consideration of our temperate climates.

In looking at the Table which closes this chapter, it will be seen that in Europe the mean annual number of days on which thunder occurs varies, speaking generally, so slowly with a change of latitude, that we should expect for Paris and the neighbourhood of Orleans results almost identical, or differing at the utmost by two or three units. The fact, however, is very different.

At Paris, the mean number of days of thunder in a year is fourteen, while at Denainvilliers, between Pithiviers and Orleans, it is half as much again, or nearly twenty-one.

There is thus manifested a local influence, of which the cause must be sought elsewhere than in the form of the ground, for it would be difficult to cite any country less varied in this respect than that which surrounds both Paris and Orleans.

Are we to seek for such a cause in the presence of the Loire, the great forest of Orleans, or the Sologne? This is a question on which I do not wish at present to enter. Some meteorologists consider that the nature of the soil may be influential on the greater or less number of thunderstorms. The following remarks on this subject were addressed to Mr. Luke Howard, in 1803, by Mr. Lewis Weston Dillwyn:—

East of Devonshire, storms frequent, (few metallic mines).
Devonshire generally, storms fewer, (more mines).
Cornwall, storms still fewer, (country of mines).
Swansea, thunderstorms very rare, (great abundance of iron mines).
South Devon, thunderstorms rather frequent, (no mines).
North Devon, storms notably less frequent than in the south, (many iron, copper, and tin mines worked).

Mr. Dillwyn also maintained that thunderstorms are more frequent and severe in limestone countries than in others.

I have no means of verifying the facts on which Mr. Dillwyn based his opinion; which I report, therefore, not because I regard it as established, but because it may become an interesting subject of inquiry.

If it were possible to obtain satisfactory evidence of a close and distinctly marked connection between the geological character of the ground and the number or the intensity of thunderstorms, it would be a great discovery in the Physique du Globe; I should, therefore, almost feel that I had neglected a duty, if I omitted to cite other places where such a connection had been surmised as well as in the west of England. I find in the *Statistique Minéralogique et Géologique du Département de la Mayenne*, by M. Blavier, Engineer of Mines, the following passage:—

"In the departement de la Mayenne, there exist masses of granular or compact diorite (*grunstein*), which contain a notable proportion of iron, and act on the magnet. We have been assured that the inhabitants of certain Communes, that of Niort, for instance, always see the most threatening thunderstorms either disperse as they approach, or else turn off in certain directions. We think that the explanation of this fact should be looked for in the conducting action of several considerable masses of diorite which show themselves in this country."

I owe to my friend, M. Vicat, the following note which lends some support to these views:—

"In 1807, I was travelling as a student engineer of bridges and highways, and having been directed to trace a route across that part of the chain of the Apennines which separates Piacenza from the shores of the Mediterranean, I had occasion in the course of my duties to reside for some days in a hamlet called Grondone. A few hundred paces from this hamlet, there is a rich iron mine in the form of a pointed eminence, apparently piercing the soil, and rising, to the best of my recollection, to a height of about a hundred feet. This mine, which is situated in a serpentine rock, is worked, and is said to produce 70 per cent. of metal, and furnishes the ore to the furnaces of a small town called Ferruira. Its elevation above the Mediterranean must be nearly that of the ridge of the Apennines' chain, as it is not far from the separation of the waters which flow into that sea and those which flow to the Adriatic.

"The point which I wish to describe, which is generally well-known in the country, and which I have often been able myself to verify, is the following. It rarely happens that one of the hot days of July or August passes without an electric cloud being formed over the country about Grondone. This cloud gradually increases in size, remains suspended for some hours over the iron mine of which I have spoken, and then bursts there into a thunderstorm of short duration. The miners have learnt by experience when it is time to leave the place, retiring to a short distance from the mine, and returning to their work when the explosion is passed and the cloud dispersed. I have often watched the cloud form about noon, remain quietly until four or five in the evening; and then, after a few thunderclaps, give birth to a short storm.

"There are probably other points in the Apennines where particular causes produce small storms within very circumscribed limits. I infer this from having often remarked that while the sun is shining brightly, and not a trace of cloud appearing in any part of the sky visible to an observer who may be travelling along the valley, perhaps the bed of one of those torrents, which being usually dry, form in some parts of the Apennines only practicable routes, he may suddenly hear the rush of a mass of miry water rolling with it large stones, and advancing with a speed which scarcely allows the muleteers and travellers time to escape."

Colonel Jackson has pointed out to me the environs of Bialystock in Lithuania as being in summer the theatre of constantly recurring thunderstorms with frequent strokes of lightning. These storms last only two or three hours; at other times the sky is remarkably serene.

The possible influence which the nature of the soil may exercise on the occurrence of thunderstorms ought not perhaps to appear so surprising, since it had before been thought not to be without effect on the superficial extent of heavy showers of rain. In July, 1808, Mr. Howard, in travelling rapidly from London to St. Alban's, remarked the ground to be alternately dry or wetted by the rain according to the calcareous or sandy nature of the soil. The alternations of wet and dry were too frequently repeated to be attributed with probability to the effects of chance.

Fifth Question.—Is thunder less frequent in the open sea than in the middle of continents?

I have thought it right to examine this question, which has been answered affirmatively without any proofs of the justice of the statement being offered. So far as my researches have gone, however, they confirm it. In marking off on a map of the world, according to their latitudes and longitudes, all the places where ships have been assailed by storms accompanied by thunder, the mere inspection of the map appears to show that the number of these points diminishes with increasing distance from the continents. I have even seen some reason to believe that beyond a certain distance from land, thunder never occurs. I present this result, however, with the greatest possible reserve, for I might at any moment find in the account of a voyage, proof that I had generalised too hastily. Meanwhile I have had recourse to the kindness and nautical experience of Captain Duperrey. I subjoin in full his letter written after the publication of the first edition of this notice, in which I appealed to his profound knowledge in meteorology. I might have placed some of the facts pointed out by this distinguished navigator in some of my earlier chapters, but after reflection I preferred not to separate the different pieces of interesting information which his letter contains.

"Paris, Sept. 21. 1838.

"I wish I could tell you all the pleasure which I have felt in reading the last 400 pages of the Annuaire du Bureau des Longitudes, which has just appeared; but your time is precious, and if I venture to occupy it for a few moments, it must at least be on subjects which will appear to you to justify my doing so.

"Your interesting notice on thunder has recalled to my recollection several facts of rare occurrence, which I have had the good fortune to witness, and which I regret I did not add to those which I have already had the honour to communicate to you.

"You say, that, not being discouraged by Saussure's assertion, you have sought in old meteorological collections to discover whether small isolated clouds never produce lightning and thunder. I find an entry on this subject in a journal which I kept on board the 'Uranie,' and of which I gave a copy to M. de Freycinet when we returned to France.

"'Being in the Strait of Ombay, in the month of November, 1818, we saw one evening a little white cloud, from which lightning darted in every direction. It rose slowly notwithstanding the strength of the wind, and was at a great distance from all the other clouds which appeared fixed near the horizon.'

"From recollection I can add to the above the following particulars. The cloud in question was of a rounded form, and might occupy a surface equal to the apparent size of the sun's disk. From every part of this cloud there escaped zigzag lightnings, and a great number of successive discharges like the independent fire of musketry from a battalion of men. This phenomenon, which I only saw once in my life, lasted fully half a minute; with the last discharges the cloud completely disappeared. I do not know why M. de Freycinet has not mentioned this circumstance.

"I ought to add that while in these Straits we saw several luminous balls traversing the atmosphere in all directions; thunder was frequent, as indeed is usual in the Indian Archipelago; but we also felt the effects of a whirlwind, the violence and extraordinary noise of which induced us to clue up all the sails. The whirlwind lasted but a very short time, and appeared in weather otherwise magnificent and with an extremely clear sky.

"I will next mention a fact bearing on the question of the effects of lightning on the rate of chronometers, which you have treated of in your notice. It is noted in p. 19. of the hydrographic part of the account of the *Voyage de la Coquille*, but I subjoin it here in more detail.

"As may be seen in the work referred to, our chronometers had been regulated at Amboyna, and their daily rate fixed, on the 27th of October, 1823, as follows:—

		s.
"No. 118.	by Louis Berthaud	— 5·3.
No. 160.	the same	—26·2.
No. 26.	Motet	+10·1.

"On quitting Amboyna for Port Jackson, I shaped our course so as to obtain some knowledge of Timor and the Savu Islands. During this first part of our passage, and more especially while in sight of Timor, we were assailed by frequent thunderstorms, with violent discharges of lightning in near proximity to the ship, and attended by exceedingly loud thunder. In consequence of these storms when we arrived at the Savu Islands, of which the difference of longitude from Amboyna had been very exactly determined in 1792 in d'Eutrecasteaux's voyage, none of our chronometers was found to assign their position correctly. The rates observed at Amboyna were no longer applicable, and these watches which we were accustomed to depend upon to within less than 5 minutes of a degree, were now in error from 15 to 40 minutes; when we arrived at Port Jackson their indications would have placed us more than 40 leagues in the interior of New Holland.

"On having their march determined afresh at Port Jackson, their new daily rates, January 19. 1824, were found as follows:—

	s.
"No. 118. - - - - - -	+ 7·0.
No. 160. - - - - - -	—18·7.
No. 26. - - - - - -	+27·6.

"All, therefore, had had their march accelerated, and as the new rates gave the difference of longitude between the Savu Islands and Port Jackson correctly, we can only attribute the change from the march observed at Amboyna to the violent, loud, and often repeated thunderstorms which we experienced in the neighbourhood of Timor.*

"I have never myself witnessed the effects of lightning on compass-needles, bnt I do not the less advise navigators to provide themselves with a dipping needle and circle, and to experiment with it immediately after any shock from lightning on board their vessel. It is well known that by turning the instrument in azimuth until the inclination of the needle is a minimum, the end which dips below the horizon points in the direction of the earth's magnetic pole, northern or southern, according as the place of observation is in north or south magnetic latitude. This operation, to which I have often had recourse during the voyage of the 'Coquille,' is, I think, indispensable in cases where after the thunderstorm the sky continues for some time clouded, and particularly if the ship is near shore, or among islands or other dangers.

"I find in the 'Tableaux des routes de la corvette la Coquille, &c.,' published by me in 1829, an instance of the extraordinary distances to which the light of lightnings may be perceived.

* From the 16th to the 17th of August, 1824, M. de Bougainville, being then in the Straits of Malacca, experienced a violent thunderstorm, in which the lightning exploded so close to the frigate that the binnacle compass-cards made an entire revolution.

"On the evening of the 6th of March, 1823, being between the parallels of Lima and Truxillo, and about thirty leagues from the shore, we saw very brilliant lightning in the east and north-west, quite on the edge of the horizon. The wind was S.S.E.; the weather was magnificent, and the sky remarkably clear and pure. We heard no thunder. It has, indeed, been long well known that thunder never occurs on the shores of Lower Peru; but it is also known, that, according to Antonio de Ulloa, the case is different thirty leagues inland. We may then assume that the lightnings of which the light was visible within the horizon of our ship had been produced in storm-clouds sixty leagues distant.

"I will next notice an event caused by lightning, which I did not myself witness, but of which I can guarantee the authenticity. The 'Coquille,' of which I took the command in 1821, had previously been employed as a transport, and the authorities had not thought fit to furnish her with a chain conductor. Being at anchor in the Bay of Naples, she had one day been struck by lightning in such manner, that without any part of the masts being touched, the lightning penetrated the hold, and escaped through the planks of the portion of the hull which was under water. The leak thus caused was such that the ship would have foundered if at her signal of distress the other ships in port, and the various fishing boats in the neighbourhood, had not come to her aid with remarkable celerity, and towed her to the shore just in time to save her from sinking.

"You have more particularly requested me to write to you on two subjects, in respect to which I feel more honoured by the invitation than satisfied with the replies which it is in my power to give.

"If I examine generally the data which we possess on the subject, I am, like yourself, inclined to admit that thunder-storms are less frequent at sea than on land, and that it may be inferred from thence that there may be, at a great distance from continents and islands, places where they never take place. But I also see that there are anomalies which modify all my anticipations, and require that I should be on my guard. A navigator coming from the Moluccas, or Sonda Islands, where thunder occurs almost every day in the year, must find himself, on gaining the high seas, greatly relieved, and much impressed with the absence of the din to which he has been accustomed; but a totally opposite impression would have been made on an inhabitant of Lima who should have accompanied us on our passage to the Society Islands; for he would, for the first time in his life, have heard the crashing of thunder on three successive days, when we were 600 leagues from Peru, 600 leagues east of Tahiti, and nearly 230 leagues north-west of the little island called Easter Island.

"Your numerous researches lead to the inference that thunder does not take place in the icy regions of the northern hemisphere. I think the same is true in the opposite hemisphere. We know, indeed, that some of Lord Anson's sailors, in the 'Centurion,' were seriously hurt by lightning, at some distance westward of Magellan's Straits; but I think we may infer from the voyages of Cook and Bellingshausen, and that of the 'Uranie,' that in the parallel of Cape Horn, in the middle of the great southern ocean, at the greatest distance from land, or at a point about 560 leagues from Oparo, Antipodes, Easter, Peter the First, and Alexander Islands, there is scarcely any thunder.

"I am almost certain that thunder is very rare on the route which leads in a straight line from the Cape of Good Hope to the Islands of St. Helena and Ascension; and at St. Helena itself we may feel assured that the ashes of Napoleon will never be disturbed by thunderbolts. It is otherwise, however, with some other portions of the Atlantic, Pacific, and Indian Oceans.

"Thunder occurs at 240 leagues east of Brazil and Patagonia, and on the equator between Africa and America; also at the point in the Northern Atlantic most distant from land, which is 380 leagues from the West India Islands, Guiana, the Cape Verds, the Azores, and the Bermudas. Thunder and lightning are also met with 200 and 240 leagues south of the Cape of Good Hope, New Holland, New Zealand, and Easter Island; and if we consult the narratives of the voyages of La Peyrouse, Dixon, Mears, and Freycinet, we find these phenomena no less brilliant than elsewhere, not only at 250 leagues north-east of the Marianas, and more than 300 leagues north of the Sandwich Islands, but also in 40° N. latitude and 180° longitude (from Paris), precisely in the central part of the North Pacific, as far as possible, or at an extreme distance, from Japan, the Aleutian Islands, and the north-west of America. I say 'an extreme distance,' because there is nowhere on the globe, even including the icy regions, any spot on the surface of the sea which is more than 600 leagues from any land, and that the places of which I have been speaking, and at which different navigators seem as it were to have agreed together to see flashes of lightning, are at this distance from all the surrounding great lands.

"It should also be remarked, that probably the number of recorded cases may appear to be less than it really is, from the uncertainty in which we are often left by the expressions of navigators respecting the 'violent storms' and 'stormy weather' which they have such frequent occasion to mention. It is not easy to say what sailors mean by 'storm.' I think the following passage of Dixon may, however, throw some light on this point.

"On leaving Nootka he says:—

"'The 26th of September, 1786, towards three in the morning, we had a great storm and heavy rain; the claps of thunder were terrible, and the flashes of lightning so frequent and vivid that those of our people who were on deck were blinded by it for some time; every flash of lightning left behind it a very disagreeable smell of sulphur. Towards six in the morning the storm abated.'

"It is evident, that if the thunder and lightning had been more moderate, Dixon would have said nothing about them, and we should still have been in doubt as to what he meant by 'storm.'

"The following passages from Captain Mears's voyages do leave us thus in doubt. Being in command of 'La Felice,' proceeding in 1788 from Samboingan to the north-west coast of America, he experienced violent storms; he says:—

"'The weather continued stormy until the 17th of April, when the wind veered suddenly to E. S. E., and blew with increased violence.' Further on he adds: 'On the morning of the 24th the wind veered to south and east, a sure sign of approaching stormy weather: at noon it blew so hard that we had to furl all our sails; and, until three in the afternoon, suffered the most violent hurricane which we can ever remember having experienced. The birds had left us from the beginning of the storm.'

"Among all the narratives of voyages, I really can only find those of Dampier, Cook, La Peyrouse, Dixon, Vancouver, the 'Uranie,' and perhaps, also, the 'Coquille,' in which the occurrences of the phenomenon which we are now considering have been noticed with regularity.

"I remark that Captain Lütke, commanding the Russian corvette 'Siniavine,' having taken his scientific instructions in London in 1826, prior to his voyage of circumnavigation, has made the same omission as that with which you have justly taxed those who drew up the meteorological tables of the Royal Society of London,—having taken the trouble to indicate by particular signs the various meteorological phenomena, with the one unfortunate exception of thunder and lightning.*

"In conclusion, I believe that there are, both at sea and on land, parts of the earth where thunder never occurs; but it must also be added, that there are parts of the ocean at the

* The only occasion, within the Editor's remembrance, in which the Royal Society, as a body, have published what might perhaps be called "scientific instructions" for the use of travellers or residents in foreign countries, was the report drawn up on the occasion of the equipment of Sir James Clark Ross's Antarctic Expedition, and of the British Magnetical and Meteorological Observatories, entitled "Report of the Royal Society on the Objects of Scientific Inquiry in Physics and Meteorology." Pages 46, 47. and 76. of this report contain both general and specific instructions for observing and recording the phenomena of thunderstorms; and if the detailed instructions in the pages referred to on the subject of thunder and lightning may be deemed by some less full than might have been otherwise desirable, it may doubtless be ascribed to the circumstance of the then recent publication of M. Arago's first edition of the "Notice sur la Tonnerre," the study of which was expressly recommended, copies being procured for that purpose and supplied to the expedition and to the colonial observatories. The meteorological journals kept at the latter establishments, published in extenso by the British Government, afford the best testimony that the recommendations of the Royal Society, in this respect, were not disregarded. If these journals have not received from M. Arago the same diligent and impartial examination of which the present essay affords so many proofs, it is doubtless because the volumes reached him when his eyesight had begun to fail.

In respect to the late Admiral Lütke, Admiral Duperrey is mistaken in supposing that he either sought or received meteorological instructions in London, when the "Siniavine" put into Portsmouth, in 1826, on her passage from Russia to the South Seas. At Admiral Lütke's request, the editor of this volume had prepared an *invariable pendulum* and some *magnetical apparatus* for him; Admiral Lütke came to London for the purpose of receiving, and of making his base observations with these, and in that work the Editor assisted him; but he can state on his own knowledge that Admiral Lütke neither made application for nor received instructions from the Royal Society regarding meteorology.—Editor.

utmost distance from land where, nevertheless, thunder and lightning do occur.

"In regard to the question, whether in the temperate regions, as is the case in the torrid zone, thunderstorms are, generally speaking, less frequent at a greater distance from land, I think it is difficult to offer a reply; not only because the voyages which we can consult are too few, but also because the chances are against each navigator being at any given point of the ocean on one of the twenty days which constitute the mean annual number of days of thunder observed on land in our latitudes.

"Excuse the length and want of method of which I am conscious in this letter, in which I perceive that the principal question on which you desired a solution, must still remain to be determined by more numerous observations in future, and by a profound examination.

"Receive, &c. &c.,

"L. J. Duperrey."

Without discussing what are the parts of the ocean where thunder never occurs, I consider that I may affirm positively the general diminution of thunderstorms at sea as compared with land. I find a decided proof of this diminution in the interesting account published by Captain Bougainville of his voyage.

The Thetis, commanded by him, left the harbour of Touran, in Cochin China, about the middle of February, 1825, and sailed to Sourabaya, at the north-east extremity of Java. During two months' passage they had scarcely a single thunderstorm. While remaining in harbour, from the 19th to the 30th of April, thunder occurred without exception every afternoon. On the 1st of May, the Thetis sailed for Port Jackson. For several days she continued to be almost exactly on the parallel of Sourabaya; nevertheless, no sooner had she lost sight of the land of Java than thunder ceased to be heard. The test appears to me as complete as could be looked for, and the inference from it is extensively confirmed by observations collected in various parts of the world. We may therefore view the atmosphere of the ocean as far less suited to engender thunderstorms than that of continents and islands.

Sixth Question.—What is, at the present time, the geographical distribution of thunderstorms as respects frequency?

The reply to this question ought to consist of an extract from tables formed by meteorologists in all parts of the world. If such tables were more numerous, more complete, and more precise, I should only have to make a mere compilation; unfortunately, my task has been less simple. Mere extracts from meteorological tables might lead to the most serious errors. One or two examples will show my meaning.

The Meteorological Tables of the Royal Society of London have long been cited as models. They contain, besides the daily observations of the thermometer and barometer, the quantity of rain measured, and the direction of the wind, a minute indication of clear and cloudy days and of fogs and mists.

Thunder is never, or scarcely ever mentioned. Seeing the great importance of this meteor as compared to other atmospheric phenomena which are scrupulously registered, one might be warranted in believing that thunder never occurs in London, whereas it is almost as frequent there as in Paris. It is only not mentioned in the tables, because the phenomenon did not receive the attention it deserved from those who drew up the tables, which have, therefore, always been in this respect incomplete.

Similar omissions are found in the collections of the Academies of the United States of America; where they are the more inexcusable, because the number and intensity of the thunderstorms in those countries much exceed what is observed in corresponding latitudes in Europe. The worst part of these omissions or negligences (I will not use any harsher term), is, that when they are made without notice being given, science is exposed to be led astray.

In the following table I have sought, as far as possible, to report only observations on the exactness of which I could depend. I have classified the different towns according to the number of their days of thunder, and not according to their latitudes, which would have given a very different result. Where I had the requisite data, I have indicated the distribution of thunderstorms in the different months of the year by numbers, integers and fractions.* It is right that I should

* If it should be asked how fractions come to be employed in a case which at first sight appears to admit only of whole numbers, the answer is a very simple one: 0·3 placed opposite to February, means that in that month thunder takes place 3 times in 10 years; 0·1 opposite to November indicates

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Only one year's observations by Mr. Richard Brooke:— (Annual Number of Days of Thunder.)

	Days.		Days.		Days.	
Jan.	0	May	10	Sept.	0	
Feb.	0	June	8	Oct.	1	=41·0
March	5	July	11	Nov.	0	
April	1	Aug.	5	Dec.	0	

§ 5.

Island of Martinique (lat. 14° 30′; long. 61° W.) =39·0

It never thunders in January, February, March, and December. Thunder most frequent in September.

§ 6.

Abyssinia (lat. 13° N.; long. 37° E.)

A single year's observations by Bruce.

	Days.		Days.		Days.	
Jan.	0	May	6	Sept.	4	
Feb.	0	June	7	Oct.	4	=38·0
March	4	July	3	Nov.	0	
April	4	Aug.	6	Dec.	0	

§ 7.

Island of Guadaloupe (lat. 16° 20′; long. 61° 40′ W.) =37·0

No thunder in January, February, March, and December. Thunder most frequent in September.

§ 8.

Viviers (Dep^t. de l'Ardèche), (lat. 47° 30′; long. 4° 40′ E.) Ten years, from 1807 to 1816.

Extremes: 14 in 1814; 35 in 1811.

	Days.		Days.		Days.	
Jan.	0·0	May	4·0	Sept.	3·1	
Feb.	0·1	June	3·4	Oct.	2·2	=24·7
March	0·6	July	5·1	Nov.	0·6	
April	2·2	Aug.	3·4	Dec.	0·0	

§ 9.

Quebec (Canada), lat. 46° 45′; long. 71° W.

	Days.		Days.		Days.	
Jan.	0·0	May	2·5	Sept.	1·0	
Feb.	0·0	June	5·5	Oct.	0·5	=23·3
March	0·0	July	8·0	Nov.	0·1	
April	0·6	Aug.	5·0	Dec.	0·1	

§ 10.

Buenos Ayres (lat. 34° 30′ S.; long. 58° 20′ W.) Seven years' observations by M. Mossotti.

	Days		Days		Days	Annual Number of Days of Thunder.
Jan.	1·9	May	1·7	Sept.	2·9	=22·5
Feb.	2·6	June	1·1	Oct.	2·3	
March	2·1	July	1·3	Nov.	1·8	
April	1·8	Aug.	1·0	Dec.	2·0	

§ 11.

Denainvilliers, near Pithiviers (Loiret) (lat. 48° N.; long. 2° 20′ E.). Twenty-four years' observations by Duhamel, between 1755 and 1780.

Extremes: 15 days of thunder in 1765; 32 in 1769.

	Days.		Days.		Days.	
Jan.	0·1	May	3·6	Sept.	1·5	=20·6
Feb.	0·1	June	4·5	Oct.	0·5	
March	0·5	July	4·4	Nov.	0·3	
April	1·6	Aug.	3·5	Dec.	0·0	

§ 12.

Smyrna (lat. 38° 30′ N.; long. 27° E.). Only one year's observations by M. de Nerciat.

	Days.		Days.		Days.	
Jan.	2·0	May	1·0	Sept.	3·0	=19·0
Feb.	4·0	June	0·0	Oct.	0·0	
March	4·0	July	0·0	Nov.	1·0	
April	1·0	Aug.	0·0	Dec.	3·0	

§ 13.

Berlin (lat. 52° 30′ N.; long. 13° 20′ E.). Fifteen years' observations by Béguelin, from 1770 to 1785.

Extremes: 11 days of thunder in 1780; 30 in 1783.

	Days.		Days.		Days.	
Jan.	0·0	May	2·6	Sept.	1·3	=18·3
Feb.	0·0	June	3·9	Oct.	0·1	
March	0·1	July	4·2	Nov.	0·1	
April	0·6	Aug.	5·3	Dec.	0·1	

§ 14.

Padua (lat. 45° 20′ N.; long. 12° E.). Four years' observations, from 1780 to 1783.

	Days.		Days.		Days.	Annual Number of Days of Thunder.
Jan.	- 0·0	May	- 1·2	Sept.	- 0·7	
Feb.	- 0·0	June	- 3·5	Oct.	- 1·0	=17·3
March	- 1·2	July	- 3·5	Nov.	- 1·5	
April	- 2·2	Aug.	- 2·5	Dec.	- 0·0	

§ 15.

Strasbourg (lat. 48° 30′ N.; long. 7° 50′ E.) Twenty years' observations by M. Herrenschneider =17·0

Extremes: 6 days of thunder in 1818; 21 in 1831.

(I have no account of the distribution in the different months of the year.)

§ 16.

Maestricht (lat. 51° N.; long. 5° 40′ E.). Eleven years' observations by Mr. Crahay.

Extremes: 8 days of thunder in 1823, and 27 in 1826.

	Days.		Days.		Days.	
Jan.	- 0·0	May	- 2·5	Sept.	- 1·4	
Feb.	- 0·1	June	- 2·9	Oct.	- 0·5	=16·5
March	- 0·4	July	- 3·7	Nov.	- 0·1	
April	- 1·5	Aug.	- 3·3	Dec.	- 0·1	

§ 17.

La Chapelle, near Dieppe (lat. 50° N.; long. 3° 35′ E.). Eighteen years' observations, under the inspection of M. Nell de Breauté, by M. Racine.

Extremes: 6 days of thunder in 1820, and 23 in 1828.

	Days.		Days.		Days.	
Jan.	- 0·2	May	- 2·6	Sept.	- 1·3	
Feb.	- 0·2	June	- 3·2	Oct.	- 0·7	=15·7
March	- 0·5	July	- 2·3	Nov.	- 0·8	
April	- 1·1	Aug.	- 1·8	Dec.	- 1·0	

§ 18.

Toulouse (lat. 43° 30′ N.; long. 1° 20′ E.). Seven years' observations, from 1784 to 1790 - =15·4

Extremes: 4 days of thunder in 1784, and 24 in 1788.

§ 19.

Annual Number of Days of Thunder.

Utrecht (lat. 52° N.; long. 5° E.). Many years' observations, cited by Muschenbroek - - =15·0

Extremes: 5 days of thunder in 1740, and 23 in 1737.

§ 20.

Tubingen (lat. 48° 30′ N.; long. 9° E.). Nine years' observations by Kraafft - - - =14·6

§ 21.

Paris (lat. 48° 50′; long. 2° 20′ E.). Nineteen years, from 1785 to 1803.

Extremes: 7 days of thunder in 1796, and 22 in 1794.

	Days.		Days.		Days.	
Jan.	0·1	May	1·8	Sept.	0·7	=12·2
Feb.	0·1	June	3·0	Oct.	0·6	
March	0·2	July	2·5	Nov.	0·1	
April	0·8	Aug.	2·2	Dec.	0·1	

Ten years' observations, from 1806 to 1815.

Extremes: 8 days in 1815, and 25 in 1811.

	Days.		Days.		Days.	
Jan.	0·0	May	3·2	Sept.	1·5	=14·9
Feb.	0·3	June	3·1	Oct.	0·7	
March	0·1	July	2·7	Nov.	0·1	
April	0·5	Aug.	2·4	Dec.	0·3	

Ten years, from 1816 to 1825.

Extremes: 6 days in 1823, and 22 in 1822.

	Days.		Days.		Days.	
Jan.	0·1	May	3·0	Sept.	1·6	=13·2
Feb.	0·0	June	2·8	Oct.	0·3	
March	0·5	July	2·1	Nov.	0·2	
April	1·0	Aug.	1·5	Dec.	0·1	

Twelve years, 1826—1837.

Extremes: 8 days in 1831, and 20 in 1827.

	Days.		Days.		Days.	
Jan.	0·0	May	3·1	Sept.	1·2	=14·6
Feb.	0·1	June	2·9	Oct.	0·6	
March	0·3	July	3·2	Nov.	0·0	
April	0·9	Aug.	2·2	Dec.	0·1	

Annual Number of Days of Thunder.

Mean of the four series.

Forty-one years from 1785 to 1803; and from 1806 to 1837.

	Days.		Days.		Days.	
Jan.	0·1	May	2·7	Sept.	1·3	=13·6
Feb.	0·1	June	2·9	Oct.	0·5	
March	0·3	July	2·6	Nov.	0·1	
April	0·8	Aug.	2·1	Dec.	0·1	

§ 22.

Leyden (lat. 52° N.; long. 4° 20′ E.). Twenty-nine years' observations by Muschenbroek.

Extremes: 5 days of thunder in a year not specified, and 17 in 1748.

	Days.		Days.		Days.	
Jan.	0·1	May	2·1	Sept.	1·0	=13·5
Feb.	0·4	June	2·7	Oct.	0·3	
March	0·2	July	2·9	Nov.	0·3	
April	0·3	Aug.	2·9	Dec.	0·2	

§ 23.

Athens (lat. 38° N.; long. 23° 40′ E.). Three years' observations from 1833 to 1835 - - =11·0

Extremes: 7 days of thunder in 1835 and 18 in 1834.

§ 24.

Polpero (coast of Cornwall), lat. 50° 20′ N., long. 4° 10′ W.). Twenty-three years' observations by Jonathan Couch - - - - =10·0

§ 25.

Petersbourg (lat. 60° N., long. 30° 20′ E.). Eleven years' observations (from 1726 to 1736), by Kraafft.

	Days.		Days.		Days.	
Jan.	0·0	May	2·7	Sept.	0·1	=9·1
Feb.	0·0	June	2·1	Oct.	0·0	
March	0·0	July	2·5	Nov.	0·1	
April	0·7	Aug.	0·9	Dec.	0·0	

§ 26.

London. Thirteen years' observations by Mr. Howard, made at Plaistow, Clapton, and Tottenham near London (from 1807 to 1822).

Extremes: 5 days of thunder in 1819, and 13 in 1809.

	Days.		Days.		Days.	Annual Number of Days of Thunder.
Jan. -	- 0·0	May -	- 1·8	Sept. -	- 0·4	
Feb. -	- 0·2	June -	- 1·4	Oct. -	- 0·1	
March	- 0·4	July -	- 2·0	Nov. -	- 0·2	=8·3
April	- 0·4	Aug. -	- 1·3	Dec. -	- 0·1	

§ 27.

Pekin (lat. 40° N., long. 116° 20′ E.). Six years' observations by the missionaries (from 1757 to 1762).

Extremes: 3 days of thunder in 1757, and 14 in 1762.

	Days.		Days.		Days.	
Jan. -	- 0·0	May -	- 0·5	Sept. -	- 0·3	
Feb. -	- 0·0	June -	- 2·0	Oct. -	- 0·1	
March	- 0·0	July -	- 1·7	Nov. -	- 0·0	=5·8
April	- 0·2	Aug. -	- 1·0	Dec. -	- 0·0	

§ 28.

Cairo (lat. 30° N., long. 31° 20′ E.). Two years' observations by Dr. Destouches (1835 and 1836).

Extremes: 3 days of thunder in 1836, and 4 in 1835).

	Days.		Days.		Days.	
Jan. -	- 1·0	May -	- 0·0	Sept. -	- 0·0	
Feb. -	- 0·0	June -	- 0·0	Oct. -	- 0·0	
March	- 0·5	July -	- 0·0	Nov. -	- 0·5	=3·5
April	- 1·0	Aug. -	- 0·0	Dec. -	- 0·5	

CHAP. XXXIII.

IN OUR CLIMATES HOW MANY PERSONS ARE STRUCK BY LIGHTNING IN EACH YEAR?

In a statistical report drawn up by the desire of Government, and published in 1852, we find that sixty-nine persons are annually killed in France by lightning. We have reason to suppose this number to be below the truth, as there may be many such accidents of which the authorities are not informed; and sometimes individuals, who have taken shelter under trees during a storm, are killed by lightning without there being any indications which suggest the cause of the catastrophe.

I had for several years noted down all the cases of persons struck by lightning which came to my knowledge by the newspapers. In looking over the following list, of which the imperfection will be immediately obvious to every one, since it speaks only of accidents over a small portion of France, a better idea will be formed of the errors by which the result published by official authority is liable to be affected.

1841.

May 6th. A man, at Lons-le-Saulnier.
" 8th. A man, at Paris, on the bank of the Seine.
" " A girl, at Lille.
June 11th. A boy, near Tours.
" " A man, at Montrevel (Ain).
" " A man, at Neulise.
" 23rd. A man, near Hazebrouck.
Sept. 25th. A young girl, at Valensole (Drôme).
" " A young girl, at Pierrelatte.
" " Two men, at Buygny-Saint-Macloux.
October. A man, near Nantes.

1842.

May. A man, near Rodez.
June. Four persons in a boat, port of Marseilles.
" A man, near Bayonne.
" Three persons, who had taken shelter under a tree near Rouen.
Aug. 24th. Two persons, at Ille.
" " A man, at Lusignan-le-Petit.
" 26th. A man, at St. Jean-de-Crieulon, near Vigan.
" 28th. A man, at Gonédic, near Saint-Brieuc.
Sept. A man in bed, in the village of Vertaure (Haute Loire).

1843.

April. Two children, who had taken shelter under a tree, at Bougenais, near Nantes.
July 8th. Two children, at Braffe, near Tournay.
" " Two persons, who had taken shelter under a tree, at Génis (Perigord).
" 19th. A man, in the village of Roche Jean.
Aug. 16th. Three persons, sheltered under a corn rick, at Riom.
" " A man, at Arcachon.
" " A man, who had taken shelter under a tree, near Lille.
" 26th. A man, who had taken shelter under a tree, near Lille.
Sept. 1st. A young girl, in the Commune of Aubarède (Hautes Pyrénées).
" 9th. A man, who had taken shelter under a tree, at Camblannes.
" " A man, at Metz.
" 10th. A man, who had taken shelter under a tree, near Senlis.

1844.

March. A man, near Douai.
April 26th. A man, in the Commune of Masparraute.
June. A man, at Moulins.
„ 27th. A man, engaged in ringing a bell, at Sarliac.
July. A man, sheltered under a tree, at Saussines (Gard).
August. A young girl, at Hadel (Vosges).
„ A man, near Mâcon.
Sept. A man, engaged in ringing a bell, at St. Robert (Corrèze).
Oct. 5th. Three men, at Franceuil (Indre et Loire).
„ 15th. A child, near Niort.
„ „ A child, near Rochefort.
„ 22nd. Eight men, at Sauve (Gard).

1845.

May 28th. A man, sheltered under a tree, near Montmarault (Allier).
June. A man, near Soissons.
„ A child, bourg du Péage (Drôme).
July. A man, near Honfleur.
„ A man, at Saint Loubès.
„ A man, sheltered under a tree, near Reims.
„ 23rd. A child, near Toulouse.
Aug. 19th. A man, at Saint-Désert.
Sept. 5th. A man, ringing bells, near Toulouse.
„ 7th. A man, near Orthez.
October. A child, sheltered under a tree, at Doué (Maine et Loire).

1846.

May 7th. A man, ringing a bell, at Cornille.
June 4th. A man, sheltered under a tree, at Orignolles.
„ 10th. A woman, sheltered under a tree, at Pau.
„ 15th. Five men, at Donjon (Allier).
„ 18th. A man, sheltered under a tree, at la Teste.
„ „ A young girl, at Foissiat (Ain).
July 6th. A man, at Vinça.
August. Four men, at Levreux (Indre).
Sept. A man at Marsais (Charente Inférieure).
„ 10th. A man at Arthès (Hautes Pyrénées).
„ 29th. A man, at Arles.

1848.

July 19th. A man, at St. Germain-des-Bois.
„ 20th. A woman, at Montreuil.
Aug. 10th. Two persons, at Montbard.

1849.

March 1st. Two men, sheltered under a tree, at Bazelat (Creuse).
„ 30th. A young girl, near Foix.
April 10th. Two persons, at Puyloubier (Bouches du Rhone).
„ 20th. A young girl, at Laprade.
May. A man, at Lyons.
„ A man, at Casseul (Gironde).

CHAP. XXXIV.

AT WHAT SEASON ARE STROKES BY LIGHTNING MOST FREQUENT?

WHILE I am far from regarding proverbs and popular sayings in general, as constituting codes of national wisdom, I am, at the same time, disposed to think that physicists have been wrong in regarding with contempt those sayings or proverbs which refer to natural phenomena. It would, no doubt, be a great mistake to receive them blindly; but it is no less so to reject them without examination. Guided by these principles I have sometimes found important truths where others had seen merely groundless and obstinate prejudices. I therefore thought it right to submit to a test from which no one has a right to appeal,—*i. e.*, that of observation,—a country aphorism much opposed to more generally received opinions, to the effect "that lightning is never more dangerous than in the cold season."

The method I have followed has been a very simple one. I took note of all the cases of strokes of lightning which I met with recorded by navigators, with their dates assigned, and have classed them according to the months in which they occurred. It is obviously necessary that such a table should be restricted to one hemisphere, as, on opposite sides of the equator, the same months correspond to opposite seasons. It was also desirable not to include in the field of inquiry the intertropical regions, where the different months differ very little from each other in respect to temperature. I avoid all these difficulties by restricting myself to the space comprised between the coasts of England and the shores of the Mediterranean inclusively.

The following are the results:—

JANUARY.

1749. The "Dover," an English merchantman, on the 9th; in lat. 47° 30′ N., long. 20° W.
1762. "The Bellona," an English 74; day, lat., and long. wanting.
1784. "The Thisbe," English man-of-war (Irish coast) on the 3rd.
1814. "The Mitford," English ship-of-the-line (in the port of Plymouth); day not specified.
1830. "The Etna," "Madagascar," and "Mosquito," English ships-of-war (near Corfu); day not specified.

February.

1799. "The Cambrian," English man-of-war, on the 22nd (near Plymouth).
„ "The Terrible," English ship-of-the-line, on the 23rd (near the English coast).
1809. "The Warren Hastings," English ship-of-the-line, on the 14th (at Portsmouth).
1812. Three (French) ships-of-the-line, on the 23rd (at Lorient).

March.

1824. "The Lydia," of Liverpool, on the 23rd (in her passage from Liverpool to Miramichi).

April.

1811. "The Indefatigable," "Warley," "Perseverance," and "Warren Hastings," English vessels proceeding in company, on the 20th; lat. 46° N., long. 9° W.
1824. "The Hannibal," of Boston, on the 22nd; lat. 46° N., long. 38° W.
„ "The Hopewell," English merchantman, on the 22nd; lat. 40° 30′ N., long. ?.
„ "The Penelope," of Liverpool, on the 22nd; lat. 46° N., long. 37° W.
1827. "The New York," packet of 500 tons, on the 19th; lat. 38° 9′ N., long. 59° W. (in a passage from New York to Liverpool).

May.

June.

July.

1681. "The Albemarle," an English vessel, day not specified, near Cape Cod; lat. 42° N.
1830. "The Gloucester" and "The Melville," English ships-of-the-line (exact date not specified, but in the summer), near Malta.

August.

1808. "The Sultan," English ship-of-the-line, on the 12th, Port Mahon.

September.

1813. *Five* out of thirteen ships-of-the-line, under Admiral Lord Exmouth, on the 2nd, at the mouth of the Rhone.
1822. "The Amphion," of New York, on the 21st, not far from New York.

October.

1795. "The Russell," English ship-of-the-line, on the 5th, near Belle Ile.
1813. "The Barfleur," English ship of 98 guns, at the end of the month, in the Mediterranean.

NOVEMBER.

1696. "The Trumbull," English galley, on the 26th, harbour of Smyrna.
1723. "The Leipsic," Austrian frigate, on the 12th, near Cephalonia.
1811. "The Belle Ile," Liverpool brig, day not specified, at Bideford, in Devonshire.
1832. "The Southampton," English ship-of-the-line, on the 5th, in the Downs.

DECEMBER.

1778. "The Atlas," East India Company's ship, on the 31st, at anchor in the Thames.
1820. "Le Coquin," French vessel, on the 25th, in the Bay of Naples.
1828. "The Roebuck," English cutter, day not specified, at Portsmouth.
1832. "The Logan," of New York, on the 19th, in the passage from Savannah to Liverpool.

On running the eye down this list, and recollecting at the same time how much more frequently thunder-storms occur in summer than in winter, it seems difficult not to admit that, at sea at least, the thunder and lightning of the warm months are much less dangerous than those of the cold and temperate portions of the year. To myself this result appears to be actually established by the evidence we possess; nevertheless, I should have wished to support it by more complete statistics, but the documents are wanting. I may add that it has not depended on me that so small a number of French ships have appeared in the list. For the English vessels I have had the advantage of being able to cite the instances collected in Mr. Snow Harris's excellent "Memoirs on Lightning Conductors."

CHAP. XXXV.

LIGHTNING CHIEFLY STRIKES ELEVATED POINTS.

ALL other circumstances being equal, the highest points are those which lightning strikes by preference. Certainly it would not be difficult to cite, in opposition to this general rule, particular instances whose causes have remained concealed in the masonry of buildings, or in the interior of the earth; but, nevertheless, no one who has taken note, in any given locality, of the occasions in which the spires of neighbouring villages on the one hand, and on the other the lower surrounding habitations, have been struck by lightning, will be otherwise than disposed to recognise the truth of the statement placed at the head of this short chapter.

CHAP. XXXVI.

LIGHTNING SEEKS OUT BY PREFERENCE METALLIC SUBSTANCES, WHETHER EXTERNAL OR CONCEALED, WHICH ARE EITHER AT OR NEAR THE POINT TOWARDS WHICH IT FALLS, OR NEAR ITS SUBSEQUENT SERPENTINE COURSE.

It is only as it enters metallic bodies, or at the moment of quitting them, that lightning produces much damage.

Of all the properties of lightning these are undeniably the most important to us in their results. It will not therefore be thought surprising that I have sought to establish them on the foundation of numerous observations which, by the variety of the attendant circumstances, can leave no room for doubt.

§ 1.

There can scarcely be a more instructive example of the property which metals possess of attracting to themselves the whole, or almost the whole, of the fulminating matter with which they may be suddenly surrounded, than is afforded by the stroke of lightning, cited in a previous chapter, which, in 1754, caused so much damage to the immense timber spire or tower of Newbury, in the United States.

Lightning fell on the upper part of the tower: the stroke must have been a violent one, since it shattered and threw to a distance a timber pyramid seventy feet high. After it had overthrown this heavy mass, the lightning found on its path a metallic wire which connected the clapper of the bell with the wheelwork of the clock, twenty feet lower down, and threw itself entirely, or almost entirely, upon this wire, which it melted in some parts. I justify the words entirely, or *almost entirely*, by the statement, that for this length of twenty feet the surrounding timber of the tower suffered absolutely no injury, although the lightning was far from having exhausted its powers on the upper pyramid, as is clearly shown by the damage which it did in continuing its descending course beyond the place where the wire stopped.

As soon as it had reached the lower end of the wire, the lightning threw itself afresh on the timber of the tower, and injured it considerably. On reaching the ground, its force was still such, that it tore up several stones from the foundations of the building, and projected them to some distance.

§ 2.

During the night of the 17th of July, 1767, lightning fell at Paris on a house in the Rue Plumet and visited the different parts of it. In one room there were several picture frames; the lightning only attacked the one which was gilt. A tin lantern and two bottles of very thin glass stood on the same table; the lantern was demolished and actually melted, while the two bottles were untouched. In another room an iron stove was broken to pieces, while no other damage was perceived in the room. In another part of the house was a wooden box containing many iron articles; the lightning broke the box, and caused the articles to present evident marks of fusion, but did not cause to ignite half a pound of gunpowder contained in an open powder-flask placed among the metallic masses.

§ 3.

On the 15th of March, 1773, lightning fell at Naples on the house of Lord Tylney. There was held there on that day a grand reception: the apartments contained not less than five hundred persons, not one of whom sustained any actual injury.

On the following day, Saussure and Hamilton, who had both been present at the event, examined the rooms, and found that almost all the gilt parts had been affected. The gilt mouldings and cornices of the ceilings, metallic rods placed so as to protect the tapestries from the contact of furniture, the gilt portions of sofas and arm-chairs in contact with those rods, the gildings of the door-posts, and, lastly, the bell wires, had all suffered more or less by fusion, discoloration, or scaling off of the surface. As usual, the maximum of effect had taken place where the lightning in its course had met with interruptions in metallic continuity.

A stroke of lightning capable of melting a bell wire is strong enough to kill a man. Yet here, as we have said, no one was hurt. We have thus a sufficient proof that the fulminating matter or lightning, in passing through the nine rooms which formed the suite of apartments, directed itself by preference, or almost in totality, to the metallic substances found in the different rooms.

§ 4.

The above precise and characteristic facts justify my now passing to the consideration of instances which will show us the lightning evidently turning aside from its original path, in order to strike metallic masses situated behind, or even inside, large blocks of masonry.

Lightning having struck a rather thick iron rod erected on Mr. Raven's house, in Carolina, U. S., afterwards ran along a wire carried down the outside of the house to connect the rod on the roof with an iron bar stuck in the ground. The lightning in its descent melted all the part of the wire extending from the roof to the ground-story, without injuring in the least the wall down which the wire was carried. But at a point intermediate between the ceiling and the floor of the lower story things were changed; from thence to the ground the wire was not melted, and at the spot where the fusion ceased the lightning altered its course altogether, and striking off at right angles made a rather large hole in the wall and entered the kitchen.

The cause of this singular divergence was readily perceived, when it was remarked that the hole in the wall was precisely on a level with the upper part of the barrel of a gun which had been left standing on the floor leaning against the wall. The gun barrel was uninjured, but the trigger was broken, and a little further on some damage was done in the fire-place.

The circumstances of the fact above related lead to two principal conclusions. One is, that the action, whatever may be its nature, whereby metals draw to themselves the matter of lightning, may take place through walls; the other is, that the amount of mass of metal is not without influence, and that, under some circumstances, lightning may quit a thin wire to seek out a massive rod, even at some distance.

§ 5.

In 1759, in the Island of Martinique, the detachment who were conducting the English Captain Dibden as a prisoner of war from Fort Royal to St. Pierre stopped for shelter from the rain under the wall of a small chapel, which had no tower or steeple. In this position two of the soldiers were struck dead by lightning, which at the same time tore an opening, about five feet wide and above three feet high, in the part of the wall against which they were leaning. On examination it was found that the demolished part of the wall corresponded exactly to some massive iron bars inside the chapel, intended to support a tomb. Those of the party who had not had the misfortune of fortuitously placing themselves opposite the pieces of metal sustained no injury.

§ 6.

A very violent stroke of lightning, on the 10th of June, 1764, struck and greatly damaged the fine steeple of St. Bride's, in London; its effects were immediately afterwards examined and described by William Watson and Edward Delaval. The most remarkable particulars were the following:—

The lightning first struck the weathercock, and from thence descended along the iron bar or spindle, almost entirely imbedded in the massive cut stones of which the steeple was formed. The spindle was 2 inches diameter and 20 feet long, and its lower end rested in a cavity 5 inches deep, hollowed out in the centre of the lowest of the cut stones. The bar was joined as solidly as possible to the stone by leaden soldering.

Now what was the effect of the lightning on the spire forming the upper part of the steeple of St. Bride's?

It was to remove or slightly blacken a little of the gilding of the highest point of the copper cross on the summit, and to fuse here and there small portions of soldering. In its course of twenty feet along the iron bar it left no appreciable traces, either on the iron or on any part of the surrounding masonry; but as soon as the continuity of metal failed, the real damage began. The large block of cut stone, into the middle of which the lower end of the bar was soldered, showed, in the shivers which had been split off from it, and in the cracks formed in it, and running in every direction, manifest marks of a violent commotion. At the height of this stone there was formed in the side of the spire a very wide opening from within outwards, several large pieces of Portland stone being thrown off. From thence the descent of the lightning seemed to have taken place by jumps from each iron bar or holdfast to the next. But in this kind of progress it must be remarked that the lightning does not restrict itself to those metallic masses which are visible. The cramps or holdfasts, used in the thickness of the masonry to fix the stones to each other, were equally sought out by the lightning. In short, at or near the extremities of the various pieces of iron employed in the construction of the building, stones were cracked, split, pulverised, displaced, and even projected to a distance, while everywhere else there was either no damage, or none of any importance. The effects produced are as if the lightning could only escape from the extremities of the metallic pieces of which it had taken possession by a violent effort, destructive of everything around.

§ 7.

This property of fulminating matter or lightning, whereby it seeks out metals and throws itself on them in great strength, even through thick masses of stone with which they may be covered, and which on quitting the metal it rends away, is too important and interesting, more especially on account of the applications of which it is susceptible, for me to scruple adding another fact to the preceding.

In 1767, lightning entered a house in the Rue Plumet, at Paris, by a stack of chimneys. Its action inside the house has been already described. On the outside the whole damage was concentrated on a single part, which yet was not the highest or the most exposed; the entablature was completely demolished, and thrown to a considerable distance. When the pieces of iron which the entablature had previously concealed, being thus laid bare, were seen, it was perceived that they had been the principal cause of an effect which must otherwise have appeared quite inexplicable, as regards the place where it occurred and the degree of intensity.

§ 8.

We have seen lightning, which was completely harmless while running down a continuous iron rod, manifest itself on issuing from the end of the rod by the fracture, pulverisation, and projection to a distance of the solid materials which surrounded the extremity or point of issue. These fractured, pulverised and overthrown, or demolished materials, were, generally speaking, blocks of cut stone or other masonry. Would exactly the same effects have been observed with different substances? Are there bodies into which lightning, on issuing from a metal, may pass without breaking or destroying anything? Is common earth one of such bodies?

When an iron rod struck by lightning has its lower extremity sunk in the earth, there are two different cases to be considered. If the earth is dry the lightning on issuing from the rod only penetrates into it by a kind of explosion, producing effects analogous to those occurring in masses of masonry and blocks of cut stone. But if, on the contrary, the earth is very moist, the lightning passes into it quietly, silently, and harmlessly, without any appreciable mechanical effects. Moist earth (and of course, therefore, for the same reason, water without earth) allows the fulminating matter, escaping from the metallic bars with which they are in contact, to pass through it nearly in the same manner as it would have done through a prolongation of the same bars or rods, or through other metallic masses in contact with them. I will cite a few facts in support of these assertions.

On the 28th of August, 1760, lightning struck an iron bar erected (as part of a lightning conductor) on the house of Mr. Maine, in the United States, and partially fused it. The bar went down to the ground and entered the earth, but not sufficiently deeply, and into earth not sufficiently humid. In consequence, the lightning in quitting the bar did not do so without an explosion, causing holes and other displacements in the dry earth, and throwing itself in part on the foundations of the house, where it did some slight injury.

On the 5th of September, 1779, at Manheim, lightning struck an iron bar rising vertically above the roof of the house of the Saxon ambassador and continuing without interruption along the roof and down one of the walls to the ground. The earth, however, into which the lower part of the conductor was sunk, was not very moist, and the lightning in quitting it caused a whirl of sand, which was seen at the moment by several persons, and of which evident traces were afterwards found.

Mechanical effects are not the only means of proving that ground but little humid possesses only in a very imperfect degree the property of carrying off from metallic bars the fulminating matter with which they may be charged. Phenomena of light often lead to the same result.

An iron bar, an inch and a quarter or an inch and a half square, whatever may be its length, conducts the most violent stroke of lightning, transmits it quietly to the earth, and disperses it there — providing the earth is moist — without any luminous appearance anywhere. Supposing the earth, on the contrary, to be dry, the bar at the moment of the explosion will be seen radiant with light. Moisten only the surface of the ground, and that surface will appear on fire. Thus lightning struck the iron bar or conductor, of which the upper extremity rose above Mr. West's house in Philadelphia, and the lower end of which went down to a depth of five feet into imperfectly moist earth. Violent rain, which was falling at the time, had wetted the pavement, and at the moment of the explosion vivid furrows of flame flashed across the wet pavement for a distance of many yards.

CHAP. XXXVII.

EXPLANATIONS, REMARKS, AND VIEWS SUGGESTED BY THE COLLATION OF THE PRECEDING OBSERVATIONS.

Before we proceed to discuss the different methods which have been proposed for the most efficient protection against lightning, let us look back on the long course which we have been passing over; not, assuredly, with the view of deriving from what has been said a theory wherein all the phenomena described shall fall at once into due place, but with the far more modest hope of arriving, by means of inter-comparison, and the light which the phenomena may be made to throw on each other, at the discovery of some truths, which the examination of each fact taken singly has not yet disclosed to us.

From the most ancient times it has been known that sound is not a material substance. Aristotle, for example, had perfectly recognised that sound results from simple undulations of the common air. At the present day the same may be unhesitatingly said of light with a single modification. Light is also a consequence of undulatory movement,—not of the air, but of a certain extremely rare and highly elastic medium, filling the whole universe, and which it has been agreed to call ether.

Is the phenomenon of which we have been treating (that of Thunder and Lightning), which manifests itself almost always simultaneously by light and by sound, to be classed in the same category? Although a declared partisan of the theory of luminous undulations, I own that on the above question I remain in a state of complete indecision.

When I view Mr. Wheatstone's experiments as perfectly assured, and when my attention rests on the incomparable rapidity with which lightning traverses the aërial regions, and the solid bodies through which it is propagated at the surface of the earth, I feel little inclined to imagine it composed of material molecules, or of a mass of very small projectiles: undulations seem far more accordant with such velocities. But then again my mind reverts to those great mechanical effects—to the actual removal of material masses of considerable weight—effected by strokes of lightning. If I then recal to my recollection that in experiments in which by the methods of proceeding the test applied has been of exceeding delicacy, without the slightest deviation being obtained,—when operating, for example, with levers suspended in a vacuum to spiders' threads, and with light concentrated in the focus of the largest mirrors or the largest lenses,—all my doubts are renewed, and the idea of fulminating undulations presents itself to me encumbered with ten thousand difficulties.

Let us, however, now proceed to a rapid examination of the principal phenomena which have been described.

§ 1. *Lightning.*

The Etruscans, whose knowledge of lightning, as connected with the supposed science of augury, was celebrated by all antiquity, distinguished three kinds: one was harmless lightning, directing attention to celestial indications; the second produced a certain amount of damage; the third was a destructive fire, inflicting death on individuals, ravaging kingdoms, and leaving nothing which it encountered in its original condition.

Jupiter sent forth the first kind at his pleasure; he might not dart the second without the assent of a council composed of twelve gods; and the third imperatively required a decree from the higher divinities.

It is difficult to conceive how nations, among whom such ideas as these prevailed, should have thought there was any necessity for inquiring how thunder and lightning were engendered in clouds, how the light was formed, and how the noise was produced. Yet these questions occupy a large space in the treatises of Aristotle, the poem of Lucretius, the writings of Pliny, and the "Naturæ Quæstiones" of Seneca. The last-named philosopher has summed up in a few words the opinions, more or less dissimilar in form, but very analogous in substance, of the physicists of antiquity concerning the origin of lightning.

"Fire is engendered by the percussion of steel on a suitable stone, or by the mutual friction of two pieces of wood. It may then be, that clouds, being borne along by the wind, inflame in the same manner by means of percussion or friction." (*Quæst. Nat.* lib. ii. § 22.)

I would invite those who might be disposed to regard with too much disdain the above certainly very forced analogy or assimilation, to remark how many blanks the centuries which have since elapsed have still left to be supplied in the desired explanation of the phenomenon which the celebrated author had in view.

Fulminating matter (so to call it for the present), notwithstanding what the rapidity of its propagation might have led us to think, does not move with indefinite freedom through solid bodies. The fractures and transportations which it occasions are an evident proof of this. It might be said, What, then, could be more natural than to suppose that in traversing atmospheric air, this fulminating matter drives the molecules of which the air is composed before it, and that hence there result successive compressions throughout the entire course? Compressions of some little strength, as the pneumatic-light apparatus proves, are always accompanied by a disengagement of light; the course of the fulminating matter ought therefore to be marked, as it is, by a line of light.

The reasoning seems well connected; it is, however, open to more than one objection.

If, at each point of the track passed over by the lightning, it is necessary for the disengagement of a little light that certain volumes of dense air should be very sensibly compressed, it is difficult to conceive how all these displacements of molecules can be reconciled with the exceeding velocity of propagation of lightning given by Mr. Wheatstone's experiments.

The analogy borrowed from the pneumatic-light apparatus is defective in its very foundation. Atmospheric air is not alone concerned in the case with which the comparison is made. M. Thénard's experiments prove that if we operate with a perfectly clean barrel of an air pump, and with a piston of felt, moistened not with any oily or fatty substance, but simply with pure water, the compression is not accompanied by light. The light appealed to was therefore probably only produced in the syringe by such substances taking fire, in consequence of the disengagement of heat always occasioned by any strong gaseous compression.

The zigzag course of lightning has always appeared so surprising that persons have been disposed to regard these apparent zigzags as mere illusions, as the result of irregular refractions of the rays of light occasioned by the clouds, or atmospheric vapours. (*Logan, Phil. Trans.*, vol. xxxix.)

Astronomers, who have such frequent opportunities of observing the heavenly bodies through vapours or clouds without seeing their position altered from what it would have appeared if the air had been clear, can hardly consider it needful to give a serious refutation of so strange an idea.

A flash of lightning with zigzags forming very acute angles, or a bicuspidate or tricuspidate flash, are so strongly contrasted with the regular curves followed by bodies under the influence of accelerating forces, that one feels at first unwilling to admit that such a flash marks out in the air places occupied in succession by one and the same substance. Let lightning, on the other hand, be supposed to be not a body but an undulation; and when so viewed, the double, triple, or multiple refractions which the luminous undulations undergo in certain crystals, present striking analogies with which it is possible for the mind to feel satisfied; we have only to remember that the atmosphere contains a great variety of exhalations, and especially aqueous vapour, irregularly disseminated, which may oppose unequal resistance in different directions.

The globular lightnings of which I have cited so many examples, and which are so remarkable, first for the slowness and uncertainty of their movements, and next for the extent of the damage which they occasion in exploding, appear to me to be at present one of the most inexplicable problems within the range of physics.

These balls, or globes, of fire seem to be agglomerations of ponderable substances, strongly impregnated with the matter of lightning. How are such agglomerations formed? In what regions are they produced? Whence are derived the substances of which they are composed? What is their nature? Why do they sometimes pause for some time in their course, and are afterwards precipitated with great rapidity? &c. &c. To all these questions science, as yet, returns no answer.

Lightning in traversing the atmosphere determines here and there a combination of its two gaseous elements, transforming them into nitric acid. May it not then be possible that the same action may sometimes effect instantaneously a kind of semi-union of substances of any sort which may exist in a certain volume of air? Should this conjecture, which, be it well understood, I present only as such, appear inadmissible, I would, however, remark that M. Fusinieri declares that he has always found metallic iron, iron in different degrees of oxidation, and sulphur, in the pulverulent deposits surrounding the fissures through which lightning has forced a passage. Without certainly wishing to resuscitate superannuated ideas respecting "stone thunderbolts,"* I would remark that it is not proved that we ought to reject as false all the accounts which speak of strokes of lightning accompanied by the fall of material substances. What is there to justify the following account, taken from the works of Boyle, being treated as fictitious?

"In July, 1681, near Cape Cod, the English ship 'Albemarle' was much damaged by lightning. The stroke of lightning was followed by a bituminous substance, which burnt with a smell

* The so-called "thunderstones," or stone "thunderbolts," revered by certain nations, were generally in shape like a wedge, a hatchet, or like an arrow or lance-head. There is no reason to doubt thal they were really fashioned by men's hands, since we find perfectly similar objects among the tools and weapons of the natives of America, and know how they manufacture them. Our own continent was also formerly inhabited by savage nations. The same wants, and the same scarcity of iron, would naturally lead to similar articles being made; and when the art of working in metals improved, and supplied instruments far better adapted for use, as being far more sharp and convenient, the stone ones were superseded and abandoned; and it is not surprising that many of them should have been preserved almost uninjured in the ground.

Such stones have also been found imbedded in trunks of trees, and it used to be said that they had been introduced there by violent thunderstrokes, their presence being otherwise inexplicable. In this way of reasoning the same might have been said of toads, and of ancient coins, which have been found by woodcutters in similar situations.

resembling that of gunpowder, falling into the stern boat. It burnt itself out where it fell, after vain attempts had been made either to extinguish it with water, or to throw it overboard with wooden poles."

Let us next inquire into the nature of "heat,"- or "summer"-lightnings, which take place in clear nights.

Seneca says, "In the calmest night, and even while the stars are shining, lightning is seen to flash; but," he adds, "in such case you may be sure that at the place from whence the lightning comes, there are clouds which the spherical form of the earth hides from our view. The flash darting upwards appears in the clear and serene part of the sky, although formed in a dark cloud." (*Quæst. Nat.* lib. ii. § 20.)

Lozeran de Fesc also, in his dissertation on thunder, which gained a prize from the Academy of Bordeaux, in 1726, did not regard heat-lightnings as primordial, or as an original phenomenon, but as the reverberation or reflection from atmospheric strata, of more or less elevation, of ordinary lightnings originating in a thunderstorm, of which the direct view was intercepted by the earth's spherical form.

This explanation is a very simple one, and has been adopted by most physicists. What, indeed, can be more natural than to attribute to the atmosphere a certain degree of reflecting power? Is it not from this that twilight is reflected long before the sun has risen and long after he has set?

This reasoning might be open to some doubts, derived from considerations of quantity. Might it not be said that the atmosphere, although a sufficiently good reflector to send back to us the crepuscular light proceeding from the sun, cannot return to us any sensible amount of light from the comparatively very feeble strength of lightning? The following remarks may afford an answer to this doubt.

In 1739, in the course of experiments on the velocity of sound, Cassini and Lacaille saw in the air the light of the flash of the gun fired at the foot of the lighthouse of Cette, although at the stations where they had taken up their posts, the town and the lighthouse were entirely concealed from them by intervening objects, such as the mountain of St. Bauzeli, &c. In 1803, M. de Zach was having powder signals made on the summit of the Brocken, in the Hartz, for the determination of differences of longitude. Observers, stationed on the Kenlenberg, more than sixty leagues distant, saw the flash of from six to eight ounces of powder, fired in the open air, for each signal, although the Brocken itself, by reason of the earth's figure, cannot be seen from the Kenlenberg. Lastly, I will add that at Paris, when the gun in the low battery of the Invalides is fired, an observer in the Jardin du Luxembourg, in the walks near the Rue de l'Enfer, from which one cannot see any of the different stories of the building, nor even the lofty summit of its dome, perceives in the air, at each discharge, a light, which extends to the zenith and beyond.

If the faint light resulting from the ignition of such small quantities of powder is thus distinctly reflected, may we not expect that this will be much more the case with the infinitely more vivid light of some lightning?

We have certainly said enough to establish the possible, and some may think the probable, truth of the explanation referred to. There is, however, something more to be done; we must try to give to this explanation of the phenomenon of "sheet lightning" the character which belongs in most cases to modern scientific theories. We have still to pass from "a conjecture" to a real demonstration. The two cases which are subjoined offer, as it appears to me, a combination of all the conditions to be desired. I find one in De Saussure's Travels, and the other in Luke Howard's two volumes of Meteorology.

On the night between the 10th and 11th of July, 1783, the illustrious historian of the Alps was at the Hospice of the Grimsel: the weather was calm, and the sky serene and clear. However, in looking in the direction of Geneva, he saw on the horizon some bands of clouds, from which issued flashes of lightning which appeared to be absolutely unattended by any sound. That same night, and at the same moment, the town of Geneva was experiencing the most dreadful thunderstorm which the inhabitants had ever witnessed.

On the 31st of July, 1813, Mr. Howard, saw from Tottenham, near London, faint heat-lightnings on the south-east horizon. The stars were shining in a perfectly cloudless sky. Mr. Howard heard soon afterwards from his brother, who was on the coast, that on the same night, and simultaneously with the silent lightnings at Tottenham, there was seen from Hastings a violent storm on the French side of the Channel, over the space extending from Dunkirk to Calais. Thus the lightning of which the faint light was seen in the atmosphere of London had been produced in clouds nearly fifty leagues distant.

But the having proved that heat-lightnings are sometimes reflected lightnings, does not prove that this is always their origin. Those who believe that a sky perfectly serene is often traversed by direct lightning spontaneously produced in an unclouded atmosphere, may support their opinion by the remark that at Paris, for example, heat-lightnings are often seen during entire nights, and from all quarters of the horizon, while yet clouds have not supervened; so long a continuance of a sort of oasis of serenity amidst surrounding disturbances, seems, it must be admitted, improbable.

When there shall be as many meteorological observers dispersed over the surface of a country as science requires, it will be easily discovered from the comparison of their journals, whether the heat-lightnings seen in a given place were or were not the reflection or reverberation of lightning belonging to a distant thunderstorm. Meanwhile, I think it may not be impossible to determine the question by observations at a single place, by a single observer, aud at the moment of the occurrence of the phenomenon.

The instrument which I would propose is not a complicated one. It consists of a tube twelve or fifteen inches long, having at the end to be turned towards the lightning a cork perforated with a circular aperture of a few millimetres. This aperture is to be covered with a plate of rock crystal, with parallel faces of about five or six millimetres, and cut perpendicularly to the sides of the hexaedral prism of the natural crystal. At the opposite, or eye, extremity of the tube, there should be an achromatic prism of carbonate of lime, or quartz, or any other crystal of double refraction. This prism is achromatised.

If without the prism we were to direct the tube towards an object either self-radiant or merely illuminated, we should see a single circular disk more or less luminous. Through the doubly refracting prism we see two such disks.

When the light of the observed object is direct white light, the two disks appear white. If, on the contrary, the original illuminating light only reaches the tube after have been reflected at some angle notably different from 90°, the two disks are differently coloured. If, for instance, one of the disks is red, the other will be green. The two tints are changed as the tube is made to turn round its axis, but they are always complementary to each other, *i.e.* their union produces white.

The light reflected by atmospheric air has in the instrument just the same properties as if reflected from glass, or water, &c.; direct the tube towards the clear sky and the two disks will appear in bright and vivid colours. It is only in a very narrow zone near the sun, and a still more circumscribed space opposite, that the colouring is insensible.

It is hardly necessary to add a few lines of explanation of the manner in which the simple tube which has been described will lead to the desired solution of the question proposed.

Let us suppose it to be night, the air serene, and occasional so-called heat-lightnings illuminating the sky from time to time. The tube is turned to the part of the sky where the lightnings are most frequent. The observer looks through it as through an ordinary telescope, and when a flash appears he immediately sees two bright disks. If these disks are white, or rather if they are both of the same tint as the lightning, it may be inferred with certainty that the observer has seen not reflected but direct light, not the reflection of a distant lightning from below the horizon, but the lightning itself, produced in the part of the atmosphere above the horizon. If, on the contrary, the disks are coloured, it is a proof that the light of which the crystals in the tube have made a kind of analysis, is reflected light, proceeding from lightnings produced below the visible horizon. By measuring the intensity of the colour of the disks it might be possible to decide what atmospheric region these last-named lightnings occupied; but I must not at present permit myself to enter into minute details. It is sufficient to have shown the manner in which, by the aid of a very simple observation, the doubts which have been suggested on the subject of heat-lightnings may be dispelled.

If the supposition of silent lightnings produced in clouds is now little in vogue, it is because in the only tolerably plausible explanation of the phenomena of thunder and lightning which has been propounded, the sound would result at least as inevitably as the light from the action of the physical forces to which the explanation has recourse. The storm clouds are therefore supposed to be exceedingly distant, when it is requisite to account for the entire absence of any kind of explosive sound after the apparition of dazzlingly bright lightning.

There is, however, no proof of these immense distances, and at all events, they would not afford an adequate explanation of the observation of Delue (cited in Chap. XIV. p. 60.), in which of lightnings of the same intensity, and produced among the same clouds, some were followed by deafening thunder-peals, and others by absolute silence. If a proof be desired that in the atmosphere sound does not necessarily attend every production of light, I may cite the following: —

Waterspouts are sometimes accompanied by very brilliant lightning. On the 4th of June, 1814, in the Illinois territory, Mr. Griswold was at a little distance (1300 feet) from one of these meteors. Almost incessant, and incomparably brilliant, lightning descended from the clouds to the earth very near to the external surface of the waterspout, or even perhaps along that surface; yet absolutely no explosion was heard.*

The cases of thunder without lightning, to which the attention of my readers was called in Chapter XIV. admit of a very simple explanation.

Let us suppose two distinct strata of clouds superimposed. Let us further suppose the upper stratum to become the seat of a great thunderstorm, that it is traversed by brilliant flashes of lightning, and that resounding thunder-peals issue from it. If the clouds of the lower stratum are very opaque or very thick, the light of the lightnings, however vivid, will not pierce them; it will be totally absorbed in them, so that no perceivable light shall reach the earth; and yet, as bodies not permeable to light are easily traversed by sound, the observer who does not see the lightning will hear the thunder perfectly well.

The two suppositions, first, of two strata of clouds existing simultaneously one above the other, at different heights in the atmosphere; and secondly, of a thunderstorm manifesting itself in the upper stratum only, might, if necessary, be supported by the narratives of so many travellers of unquestionable

* All the observers of this waterspout regarded the absence of noise in the midst of such dazzling irradiations as an unexampled phenomenon. Mr. Griswold believes that there really was as much noise as in an ordinary thunderstorm. He supposes that the rapid giratory movement of the air hindered the sonorous vibrations from issuing from the circumference of the waterspout, and from being propagated into the nearly tranquil surrounding air of the atmosphere. I doubt whether this explanation, ingenious as it is, will persuade many. The supposition of light produced without noise will be probably preferred.

veracity, that there can be no doubt that I have here indicated one of the causes of thunder without lightning. I say only "one of the causes," for I have cited (p. 98. *et seq.*) cases of thunderbolts apparently not having their seat in the clouds, and in which violent detonations were not preceded by any luminous phenomenon.

§ 2. — *Of ordinary thunder; of the interval which divides it from the flash; of its rolling sound; thunder claps and peals; the greatest distances to which thunder is heard; thunder on serene days; duration of lightning.*

Sometimes the thunder is not heard until a rather long interval of time after the flash is seen. This requires explanation; for no one doubts (although the fact is far from being a demonstrated one) that the light and the noise were produced at the same moment. The phenomenon is indeed so simple that the ancients, whose knowledge of physics, generally speaking, was far from considerable, had already recognised its true cause. We read, for example, in the 6th book of Lucretius' poem, remarks intended to establish that light in general travels much more rapidly than sound. Some verses later it is stated as following inevitably from the premises, that in thunderbolts the light of the flash must reach the earth much sooner than the crash of the thunder, although both light and sound were produced at the same instant and by the same shock.

This explanation is perfectly correct. The only advantage which we possess on this point over the philosophers of antiquity, is that for any given distance we can assign in seconds and fractions of seconds the interval at which the sound must follow the light.

Two astronomical phenomena (the occultations of Jupiter's satellites and aberration) have proved that light uniformly traverses space with a velocity of about eighty thousand leagues in a second of time; whence it would follow that it takes only an eight-thousandth part of a second in travelling ten leagues. Now beyond all doubt ten leagues is a distance exceeding the height at which thunder and lightning are produced in our atmosphere. Unless, therefore, we choose to attempt to take into account an altogether inappreciable fraction of a second, we may suppose in all our researches on the subject of thunder and lightning, that we see the latter at the very instant of its development.

In regard to sound, we may affirm from the most recent experiments that at the temperature of +10° Cent. or +50° Fahr., its velocity is 337 metres or 1105 feet 8 inches in a second of time. If then the cloud in which the thunder and lightning originate is 337 metres distant in a straight line, an entire second will elapse between the apparition of the light and the arrival of the sound.

	Metres.		Ft.	In.
2 seconds interval would correspond to a distance of -	674	or	2,211	4
3 " " " -	1,011	or	3,317	0
10 " " " -	3,370	or	11,056	8

And so on always in proportion.

Thus an observer by determining with a chronometer the number of seconds and parts of a second elapsing between the flash and the commencement of the thunder, can deduce with the greatest ease the distance between himself and the meteor, by simply multiplying the number expressing the time by 337, which will give it him in metres, or by 1105·7 which will give it him in English feet.

We must carefully remember, however, that the distance so obtained is the distance of the cloud in a straight line, which, generally speaking, is measured on a line inclined to the horizon; it is the hypothenuse of a right angle triangle whose two other sides are a horizontal line from the place of observation and the vertical height of the cloud above the horizontal line.

In order to deduce from the length of the hypothenuse, thus obtained, the vertical elevation of the cloud, it is necessary to know the angular height or altitude of the extremity of the flash nearest to the place of observation; to know whether it is 10°, 20°, 45°, &c. This element, the altitude, can be measured with sufficient precision by the aid of a theodolite, or a reflecting instrument, such as a sextant or circle, by taking as a point of reference or mark, as near as may be to the part of the sky in which the flash appeared, some of those fortuitous accidents or features of form or light which are never wanting in storm clouds. This, once known, the calculation is quickly performed.

It is in this way that the absolute heights of clouds reported in Chapter IV. were determined. This kind of observation has hitherto been too much neglected: Meteorology has a great interest in its wider extension. Both the greatest and the smallest intervals occurring between the flash and the sound of thunder deserve the especial attention of physicists: the greatest intervals, because they serve to determine the greatest height of storm clouds; and the smallest intervals, on account of their possible connection with a much controverted question on which I will now say a few words.

When a second of time elapses between the flash and the report, we know that the clouds are *at the utmost* at a vertical height of 337 metres or 1106 feet; if the interval has been $\frac{1}{2}$ a second, the height cannot be more than 553 feet; and intervals of 4 tenths, 3 tenths, 2 tenths, and 1 tenth would correspond respectively to heights less than 135, 101, 67 and 34 metres, or less than 442, 332, 221, and 111 feet.

The highest point of the Invalides is 105 metres ($344\frac{1}{2}$ feet) above the ground. Let us suppose a person standing near the building during a thunderstorm to see one of those flashes of lightning which do not appear to quit the clouds; and let him, moreover, have assured himself that the thunder followed the flash after the short interval of 3 tenths of a second. It would follow, according to what has been above said, that the clouds supposed to be the seat of the lightning could not be more than 101 metres (or 332 feet) high, and that they should therefore have enveloped the summit of the dome.

If, then, this were not the case, if the summit were seen throughout freely, and the clouds above it, it would be proved that the detonation did not take place in their bosom, and the theory of ascending lightning would have obtained an almost irresistible argument in its favour.

At Strasbourg, where the spire of the cathedral is 142 metres high, this kind of observation would extend to cases in which the interval between the flash and the sound should be as much as 4 tenths of a second. In the neighbourhood of mountains, by obtaining beforehand a tolerable number of well-defined and convenient marks, it would be easy to take cases of whole seconds. Lastly, whole seconds of interval would nowhere prevent the employment of the method if a captive balloon were made use of; and thus either the exact height of the clouds, or, at least, its lowest limit, could be determined.

I know not if I am mistaken, but observations of this kind appear to me to deserve in the highest degree the attention of physicists. Would it not be interesting thus to decide, by a simple comparison of figures, the interminable question of ascending lightnings, *i. e.* of lightnings which have been supposed to rise upwards from the earth? Those who think that, in the production of all these phenomena, there is invariably a concurrence of two effluxes, one ascending and the other descending, would perhaps find in such experiments as I have been speaking of, made simultaneously at two different places, the means of recognising where the detonation has been produced.

Would not they have given to their system a high degree of probability, if, for example, the seat of these detonations should appear to be intermediate between the clouds and the earth?

Proceeding from the numerical data above reported, let us now seek to determine, in the next place, the *greatest* distances at which thunder has ever been heard.

I have said in p. 56., that De l'Isle once counted 72 seconds between the lightning and the thunder: this number, the largest mentioned in the annals of Meteorology, multiplied by 337, gives, as the distance of the cloud in which the lightning appeared, 24,264 metres, or about 6 leagues of 4000 metres each (about 13 geog. miles).

Next to this exceptional result of 72 seconds, the highest which I have anywhere discovered is 49 seconds; this number, similarly treated, gives rather more than 4 leagues, or about 9 geog. miles.

The greatest distance at which thunder has ever been known to have been heard appears therefore to have been 6 leagues, or 13 geog. miles; and the greatest ordinary distances scarcely more than 4 leagues, or about 9 geog. miles.*

* My readers may be glad to find here a statement of limits of distance directly ascertained. On the 25th of January, 1757, the steeple of the church of Lostwithiel, in Cornwall, was struck by lightning, and almost entirely destroyed: the noise of the thunder is stated to have been tremendous. The celebrated Smeaton was "*thirty miles*" from the spot; he saw the lightning, but heard absolutely no sound. Muschenbroek reports that thunder is sometimes very loud at the Hague without being heard at Leyden, about 9, or at Rotterdam, about 12 geog. miles distant. There are also cases of very violent thunderstorms bursting over Amsterdam and not heard at Leyden, which is nine leagues, or about twenty geog. miles, distant.

The smallness of these distances will appear particularly striking on remembering the much greater distances at which the noise of cannon is heard. I find, for instance,

That a cannon fired at Florence is sometimes heard at the old castle of Monte Rotondo, near Leghorn, at a distance in a straight line of $20\frac{1}{2}$ leagues.

That a cannon fired at Leghorn is sometimes heard at Porto Ferrajo at nearly as great a distance.

During the siege of Genoa by the French, the noise of their artillery was heard as far as Leghorn, 37 leagues distant.

The smallness of the distance which is sufficient to completely extinguish the noise of the loudest thunder, has been matter of comment and wonder in all countries. Thus I find in the *Mémoires des Missionnaires de la Chine*, vol. iv., that the Emperor Kang-hi, who had taken a scientific interest in the phenomena of thunder and lightning, estimated 10 leagues as the greatest distance at which thunder was heard, while he asserted that he had heard the noise of artillery 30 leagues off, or three times as far. Researches ought now to be directed towards discovering whether the great degree of loss of sound which we have been considering may not depend merely on the partial reflections which it undergoes on encountering obliquely the separating surfaces of atmospheric strata of different density.*

* We know in general very little of the different causes which may influence the intensity of sound, and their mode of operation. Derham said, that sounds are heard much further and more distinctly in winter, and especially in frosty weather, than in summer. This opinion has been confirmed by Captain Parry. I read in the account of his first voyage (p. 143.): "The distance at which sounds were heard in the open air during the continuance of the intense cold was so great as constantly to afford matter of surprise to us, notwithstanding the frequency with which we had occasion to remark it. We have, for instance, often heard people distinctly conversing, in a common tone of voice, at the distance of a mile; and to day I heard a man singing to himself as he walked along the beach at even a greater distance than this." Derham thought he had remarked that new fallen snow weakens the effect of sound more than old snow whose surface has formed into a smooth crust. He also says, that fogs have the effect of considerably deadening the waves of sound. Probably this is the case with extensive and uniformly distributed fogs; but under other conditions fogs have the contrary effect. Thus, in November 1812, there being to a small height in the atmosphere a thick and continuous fog, M. Howard heard distinctly the noise of carriages on the London pavement, although he was distant about five miles from the middle of the town.

M. de Humboldt's observations, made on the banks of the Orinoco,

M. de Saint Cricq assured me that the cannon of Waterloo was heard at the town of Creil, at a distance of 50 leagues. According to M. Elie de Beaumont, the cannonade of the 30th of March, 1814, was heard very distinctly in the commune of Casson, situated between Lisieux and Caen, at a distance of about 44 leagues from Paris in a straight line.

The results obtained as to the greatest distances at which the noise of thunder is heard will enable us to dispose of an important question: we shall be able to decide whether thunder heard on serene days is merely the resonance of ordinary thunder, elaborated in clouds below the horizon, or whether, on the other hand, it is really produced, and originates, in a pure and serene atmosphere.

A man of small stature, whose eye is 5·25 feet above the ground, can see an object placed on the ground at the distance of 2·2 geog. miles.

If the object be at the height of 82 feet, it will be seen at a distance of about 12 geog. miles.

If it is 1640 feet high, it will be discerned at a distance of about 46 geog. miles.

Lastly, suppose it to be 3280 feet, and it will still be seen at a distance of more than 63 geog. miles.

Let us now return to an observation related in an earlier part of this volume. Volney, whose exactness is well known, happening to be at Pontchartrain, heard very distinctly four or five thunder-claps. Looking round he saw no cloud, either in the sky or near the earth. If these five thunder-claps did not proceed from the transparent atmosphere, within the visible horizon,—if their origin were to be looked for in clouds situated beyond the horizon, it will be necessary that those clouds should

have fully established the fact, that sounds are propagated to a much greater distance in the night than in the day. Is it equally certain that, according to the suggestion of my illustrious friend, the difference depends on the currents of heated air which, during the daytime, rise from the ground towards the upper regions of the atmosphere?

It is a generally received opinion, that wind blowing in a contrary direction to that in which the noise is propagated, diminishes it considerably; and on this point facts confirm the general impression. It is otherwise, however, with the also widely prevalent opinion, that wind blowing in the same direction keeps up the force of the sound, and carries it further. The observations of F. Delaroche would seem to establish that, whilst the propagation of sound is impeded (or the sound lessened in intensity) by some winds, there are none which are favourable to it.

vacuum so occasioned, as in an apparatus known generally in our *cabinets de physique* under the name of "*crève-vessie*."

No doubt the sudden return of air into a space where a vacuum had been produced must occasion noise. If lightning in traversing the atmosphere produces a vacuum thunder will result: but through what physical agency does lightning produce a vacuum? This no one has discovered: the explanation of thunder is therefore still to be looked for; hitherto all that has been done is to suggest the replacement of one difficulty by another still greater.

But, whatever may be the physical cause of thunder, we must not postpone inquiry into the origin of those long rollings which every one has remarked, and of those sudden and so often repeated changes of intensity which French meteorologists term "*éclats*."

People were long agreed in regarding the rolling of thunder as a simple effect of repeated echoes. This explanation was afterwards abandoned, as it had been adopted,—I mean, without mature consideration. I would propose to examine what place may at present be assigned to it as the result of serious discussion.

All who have witnessed a thunderstorm in some valley surrounded by high mountains know how greatly local circumstances may increase the resonance, intensity, and duration of thunderclaps or peals. We need not then examine whether echoes sometimes take part in the production of these phenomena. The question is, to determine whether echoes are always the cause of the rolling observed.

I have cited, in a previous page, cases in which the rolling sound lasted 36, 41, and even 45 seconds. Has it been proved that echoes could occasion such long rollings? In regard to echoes strictly so-called, the most remarkable case which presents itself to my mind at the present moment, is one mentioned to me by my friend the Rev. Dr. Scoresby, near the lakes of Killarney, where, at a station pointed out by the guides, he continued to hear the report of a pistol-shot for half a minute. For our purpose, we should want at least three-quarters of a minute; but we may assume that if the resonance of the discharge of a cannon had been substituted for that of a pistol, the 30 seconds would have become 45 seconds, or even more. I am the more disposed to think that the intensity of the sound should here be taken into account, because in a locality in the environs of Paris which has never, I believe, been cited as particularly remarkable in respect to echoes, at the foot of the tower of Montlhéry, during experiments on the velocity of sound made in the month of June 1822, Messrs. de Humboldt, Bouvard, Gay Lussac, and Emile de Laplace, heard, during a space of time from 20 to 25 seconds, the rolling report of the cannon which was fired close by them. There is, therefore, little hope of thus arriving at anything decisive as to the exact part which echoes take in the production of the rolling sound of thunder.

Sailors assure us that thunder on the high seas has a prolonged rolling as on land, although there are no walls, rocks, woods, hills, or mountains to reflect the sound. Muschenbroek says, however, that in the same locality where a cannon-shot is only heard as a single report when the sky is clear, the noise is repeated several times when the sky is overcast. Perhaps this observation of the Dutch physicist may be thought too little circumstantial to be admitted. If this should be the case, the following remarks, extracted from the notice published by me in 1822, on the experiments on the velocity of sound of which I have already spoken, may be acceptable.

"At Villejuif, it four times happened to us to hear the cannon-shot at Montlhéry as two distinct reports, at two seconds' interval. On two other occasions the sound of the Montlhéry gun was accompanied by prolonged rolling. These phenomena never occurred except at times when there were clouds. When the sky was completely serene the noise was single, lasting only an instant."

In conclusion, I will add a remark which might be adduced to show that the rolling of thunder does not result always and solely from reflected sound.

The sky being uniformly overspread with clouds, a flash of lightning appears in the zenith; a few seconds later thunder bursts, and its rolling is prolonged; some time afterwards another flash darts through the cloud in the same part of the sky near the zenith; thunder follows, but this time the clap, though very strong, is single; it does not last. How can such great dissimilarities be explained if we make the rolling of the thunder a mere phenomenon of echoes?

One of the most fertile and ingenious authors of whom

England can boast, Dr. Robert Hooke, was I believe the first who introduced into the explanation of the rolling of thunder an important circumstance which has been unduly neglected by the greater part of modern physicists. I allude to the essential distinction, drawn in p. 424. of his Posthumous Works, printed in 1705, between simple lightnings and composite or multiple lightnings. Each simple flash of lightning is regarded by him as occupying only a single point in space, and producing a short, instantaneous sound; on the other hand, *i. e.* the noise produced in multiple lightnings, is a prolonged rolling, because the different parts or points of the long lines occupied by those lightnings being generally at different distances, the sounds engendered at them, whether successively or at the same physical instant, must require successive moments to arrive at the ear of the observer.

This ingenious view of Dr. Hooke's was reproduced fifty years ago in the *Encyclopædia Britannica,* by Mr. Robison. As such a recommendation should commend it to meteorologists, I subjoin a quotation from the celebrated Edinburgh Professor.

"I saw, parallel to the horizon, a flash of lightning which might be about three miles long. It appeared to me coexistent; no one could have said at which end it began. The thunder consisted of a very strong clap at the outset, and afterwards of an irregular rolling which lasted about fifteen seconds. I imagine that the discharges occurred simultaneously over the vast extent of the flashes of lightning, but that they were not everywhere of the same intensity.

"By the aid of the sonorous undulations of the air, different portions of the sonorous agitation reached the ear successively, thus producing the effect of a prolonged sound. Such would be also the apparent effects to a person placed at the extremity of a long file of soldiers who should all discharge their muskets at the same instant. A person so situated would also hear an irregular rolling, if we suppose the muskets to be unequally loaded in different parts of the line."

Let us pursue somewhat further this comparison of a file of soldiers all discharging their pieces at the same instant, and we shall thus see how flashes of lightning of the same apparent length may occasion such dissimilar noises and rollings. In order to fix our ideas, let us suppose, first, that the file of soldiers occupies a straight line, and let there be 1 metre (3 feet 3 inches and 4 tenths) between each soldier and his next neighbour. Let us moreover suppose the observer placed at one end of the file, and at one metre from the first soldier; the report of the musket of the first, second, third, and hundredth soldier will reach him respectively $\frac{1}{337}$th, $\frac{2}{337}$ths, $\frac{3}{337}$ths, and $\frac{100}{337}$ths of a second after the actual firing. If there are 337 soldiers in the file, the noise of the firing will last an entire second, although all the pieces were actually discharged at the same instant. With a file of twice the length and twice the number of soldiers (or 674), the sound would last 2 seconds; with ten times as many, or 3370 soldiers, 10 seconds; and so on, always in the same proportion.

Now, keeping our file of soldiers still in a straight line, let us draw a perpendicular through the middle, and let us station the observer at some given point on this perpendicular. When all the soldiers fire, as before, simultaneously, the first sound which will reach his ear will be the report of the musket of the soldier who is in the middle of the file, or directly at the end of the perpendicular on which he is himself placed. He will next hear successively, but in couples, the reports of the muskets of each pair of soldiers placed symmetrically, or at equal distances from the middle; the last sound, terminating the rolling, being from the two men placed at the two extremities of the whole line.

Now let us substitute for the rectilinear file of soldiers a circular one; and let us place the observer in the centre. In this position, all the soldiers being at the same distance from him when they discharge their muskets as before at the same instant, he will hear, not a rolling, as before, but the noise of all the muskets united as a single report.

Is it necessary to say more, in order to render easily comprehensible the strict connection which exists between the successive stages in a peal of thunder, and the zigzags of flashes of lightning? When a flash of lightning, which we will suppose to have been darting in the direction of a line of which the observer is at one of the extremities, suddenly turns, so as to present itself to him for some instants no longer "end on," but with its length facing him, it is evident that there must be an augmentation of noise. It is no less clear that this augmentation will be followed in turn by a sudden weakening, if by a second inflection the movement is again nearly in the direction of the visual line; and so on. It seems to me, however, that observations designed to place this intimate connection between the zigzags of lightning, and the abrupt changes of intensity in prolonged peals of thunder, in the rank of demonstrated truths, would be interesting, and deserve to be recommended to the attention of physicists.

§ 3. *Length of flashes of lightning.*

Those who have reflected a little on the modes of proceeding of the human mind, attach little importance to theories, excepting on account of the experiments or connections which they may suggest, and which would not have been thought of if it had not been for their guidance. This kind of merit is not wanting in the theory which we have just presented of the rolling of thunder. It will give us, if not the lengths, at least estimations which we may feel sure are within the truth; and even this is in itself something.

Let us imagine a flash of lightning wholly on one side of the zenith. Let us draw two visual rays to its two extremities. These two rays, and the lightning which we suppose rectilinear, form a triangle, of which the eye of the observer occupies the inferior angle.

In every triangle of this kind, each side is smaller than the sum of the other two sides. We may then lay down the following inequality: the visual ray, or line from the eye of the observer to the most distant extremity of the flash, is less than the sum formed by adding to the length of the line from the eye to the nearest extremity of the flash the length of the flash itself. But if two quantities are unequal, they continue unequal after both have sustained an equal diminution. From each of the two lengths above stated to be unequal, let us subtract the shorter visual ray, or that drawn from the eye to the nearer extremity of the lightning, we shall have remaining on the one side the difference between the longer and shorter visual rays; and on the other, the shorter visual ray, plus the length of the flash of lightning, minus the shorter visual ray; or, finally, the length of the flash of lightning.* If, therefore, we are able to

* Any calculation, however simple, is always difficult to express in words. The thing to be conveyed is merely the principle in geometry,—that, in

estimate this difference in metres or feet, we shall have a limit, of which the length of the flash of lightning cannot fall short. Let us now see whether such an estimation of the difference between the two lines of sight is possible.

Why is the flash of lightning followed by rolling thunder?— Because different parts of the flash are, generally speaking, at unequal distances from the observer. What is the duration of the rolling sound?—The duration is, as we have explained, the time which the sound requires for traversing an interval of space equal to the difference in length of the two lines drawn from the observer to the two extremities of the flash. If we multiply by 337 the number of seconds for which the rolling of the thunder has lasted, we shall therefore obtain, expressed in metres, the difference between the two rays, drawn to the two extremities of the flash, just as if it had been possible to measure that difference in space. The result of the multiplication will be the "lowest limit" which we were seeking. Let us now cite some numerical results.

I said in an earlier part of this volume (p. 54.) that de l'Isle observed at Paris, in 1712, thunder-peals in which the rolling lasted 39, 41, and 45 seconds. By multiplying these three numbers by 337, we obtain results in metres corresponding to 3 leagues and 3 tenths; 3·4, and 3·8 leagues (of 4000 metres each), or about 7·9; 8·2, and 9·1 geographical miles. Who would have expected such enormous results?

For greater simplicity, I began by supposing the flash of lightning to be situated entirely on one side the zenith. Any other hypothesis would not alter the results at which we have arrived. Only the calculated limits (for, for want of an angle, we could only find limits), would be still more below the real length of the flash of lightning.

In Abyssinia, M. d'Abbadie found trigonometrically that the length of certain flashes of lightning exceeded 6700 metres, or 3·7 geographical miles.

M. Petit saw, at Toulouse, lightnings as much as 17,000 metres, or 9·6 geographical miles in length.

Mr. Weissenborn, the German translator of the first edition

every rectilinear triangle, one side is greater than the difference between the two other sides,—a principle which itself follows from that other principle, of which no one can be ignorant; *i. e.*, that any one side of a triangle is smaller than the sum of the other two sides.

of this notice, determined trigonometrically the length of a flash of lightning which he observed near Weimar, on the 2nd of May, 1839. He found it 8680 metres, or about 6·2 geographical miles. It is to be remarked that these results are in no way influenced by echoes.

§ 4. *Odours developed by strokes of lightning.*

Some physicists have thought it unnecessary to have recourse to any particular causes for the purpose of explaining the penetrating odour always present when lightning strikes terrestrial objects. They say, may not the fulminating matter as it passes in greater or less abundance across the nervous papillæ of our organs, itself excite in them a movement analogous to that occasioned by the action of such or such an odour?

This might be to a certain degree an admissible supposition if we had only to explain an instantaneous odour. But wherever a lightning explosion takes place, it developes, even in the open air, odours which last a long time (*See* p. 63.). When lightning makes its way into a closed space, its passage is followed by the formation of sulphurous vapours, which are sometimes so thick that nothing can be seen through them (*See* p. 64.). There is, therefore, evidently the presence of disseminated matter in the air. Are we to suppose it to consist of substances brought by the lightning, similar to those which compose the deposits studied by M. Fusinieri, and which have served to afford us a partial or incipient explanation of globular lightnings (*See* p. 150.)? or do they proceed from the sudden vaporisation, or conversion into vapour, of substances contained in green or dry wood, wood varnished or not varnished, or in the walls, stones, or earth, &c., in the interior of which the lightning has circulated? This is a question which cannot be decided at present. Whichever of the above explanations may prevail, too much stress should not be laid on a supposed constancy in the nature of the odour developed. It would not be difficult to show, if needed, that if this odour has usually been compared to sulphur, others, again, have compared it to phosphorus, and lastly, others to nitrogen. This last would be the odour admitting of the easiest explanation, as may have been seen in Chapter XVII. p. 65.

§ 5. *Lightning effects fusions and instantaneous vitrifications, shortens metallic wires along which it is transmitted, and pierces many holes in the bodies which it finds on its passage.*

I cannot add anything to the facts already related respecting these singular effects of lightning. We are completely ignorant of the manner in which lightning developes instantaneously so much heat. With the view of explaining the many holes which are sometimes the result of the passage of lightning through metallic plates, there have been imagined modes of agglomeration and propagation of the fulminating matter, in which the least defect is that they do not account for the opposite directions of the lips or returns round the holes. These opposite directions give the idea of two opposite currents meeting at the surface of bodies struck by lightning. The contraction or shortening of wires seems as if it might be the result of the efforts made by the fulminating matter to escape transversely, and which manifest themselves by phenomena of light; but I will not insist further on these vague glimpses. Fresh experiments and observations can alone assign them a legitimate place in science.

§ 6. *Substances transported to a distance by lightning.*

Bodies in motion produce mechanical effects which depend at once on their mass and on their velocity. However slight, therefore, might be the mass of the fulminating matter, yet if endowed with sufficient velocity (and in this direction we have now an indefinite margin) we should, as far as intensity goes, arrive easily at an adequate explanation of all the facts collected in Chapter XXIII.; but besides intensity of removing action by strokes of lightning, we had occasion to remark that the fragments of bodies shattered by lightning are sometimes, or rather are usually, projected to a distance in every variety of direction. This circumstance would be difficult to connect with an explanation of the mechanical effects of lightning which should rest solely on the theory of the shock of bodies, but it can be derived with great simplicity from the hypothesis, that lightning by its action developes in the substances which it traverses an eminently elastic fluid, the tension of which must inevitably act in every direction. Would it be a very hazardous supposition if we were to assume the elastic fluid in question to be no other than the vapour of water, *i. e.*, that the tension of which we have spoken is the force of steam? Lightning, as we have seen, fuses thin metallic wires, or at least renders them suddenly incandescent; may we not conclude that it will also render incandescent the minute aqueous particles which it may find on its passage?

On consulting the table given by M. Dulong and myself of the elasticity of steam corresponding to different degrees of the thermometer, it will be found to have already reached 45 atmospheres when the water attains 500° Fahrenheit.

What force would it not have at the much higher temperature of red hot iron? Such a force would evidently be adequate in point of intensity to all that we know of the mechanical action of lightning. Those who prefer an actual exemplification to a theoretical deduction, need only, in order to arrive at the same conclusion, to consult persons who have been engaged in casting metals, as to the terrible effects which result from the presence of even a single drop of water in a mould at the moment when the incandescent metal flows into it. Suppose humidity in the fissures or small cavities of a block of free-stone; and if struck by lightning, the steam suddenly developed will shatter the stone to pieces, and the fragments will be projected to a distance in every direction (*See* p. 86. and 87.). Under the same circumstances the abrupt transformation into highly elastic vapour, or steam, of the water interspersed in the earthy stratum on which the foundations of a house rest, will suffice to raise the house bodily and remove it to some distance (*See* p. 87.). When Watt saw for the first time the hollow enamelled tubes produced by lightning in a mass of sand, he immediately exclaimed, "Here is an effect of the elastic force of the steam produced by lightning in traversing the sand." Nothing, however, appears to me more clearly and directly indicative of the action of steam than the singular and minute division effected in wood by the passage of lightning.

Lightning rends wood in the direction of its length, splitting it into a multitude of thin laths or still more slender fibres.

Lightning struck the Abbey of St. Médard at Soissons, in 1676. The following is the account by an eye-witness of the state of the pieces of timber of the roof. "Some among them, three or four feet in height, were split from top to bottom into rather thin laths; others into long pieces of the thickness of matches, and others again into thin threads or fibres in the order of the natural fibres of the wood, so slender that they looked not unlike half-worn brooms."

The above relates to dead wood: let us next take the case of green or live wood, and we shall find analogous effects.

On the 27th of June, 1756, at the Abbaye du Val, near l'Ile-Adam, lightning struck an oak tree growing singly, about 52 feet high and 4 feet 3 inches diameter at its base.

The trunk was entirely stripped of its bark.

The bark so torn off was found scattered in small pieces, thirty or forty paces off, on all sides of the tree.

The trunk, to within six and a half feet from the ground, was split longitudinally into pieces almost as thin as laths.

The branches continued still attached to the trunk, but they also had not retained a morsel of bark, and showed very remarkable longitudinal jagged indentations.

Neither the trunk, the branches, the leaves, nor the bark presented any traces of combustion; they only appeared to have been completely dried up.

In the same year, 1756, on the 20th of July, lightning fell on a large oak tree in the forest of Rambouillet.

This time the branches were completely severed from the trunk, and were dispersed around it with a certain regularity. They presented no jaggedness, and the bark was entire. The trunk had not been stripped of its bark, but, like the oak tree of l'Ile-Adam, it had become an assemblage of laths; the separation into laths being prolonged down to the ground in this case, instead of only to a certain height above it.

I cannot resist the desire of citing a third case, related by Professor Muncke, in Poggendorf's "Annalen."

The trunk of the tree, which was 3 feet 3 inches thick at the ground, had been entirely destroyed; that is to say, the lightning had torn it into filaments several yards long and less than two-tenths of an inch thick, such as might have been scraped off by an instrument.

Three branches, from twenty inches to nearly two feet thick, had fallen to the ground, as if cut clean off by a single stroke of an axe. They retained their leaves and their bark. There were no traces of ignition or carbonisation to be seen anywhere.

The entire absence of carbonisation, the division of the trunk of a tree into such numerous and slender filaments, and the dispersion of these filaments in a thousand different directions, all look, I repeat, like necessary consequences of the action of an elastic force developed among the woody fibres.

Let the lightning convert suddenly into vapour the hygrometric water contained in the old timbers of a roof, or the sap filling the longitudinal capillary tubes in growing wood, and you will have in all respects the phenomena of the roof timbers of the Abbaye of St. Médard de Soissons, of the oak trees of l'Ile-Adam, and of the forest of Compiégne, &c. &c.*

The detailed discussion into which we have entered respecting the transport of heavy bodies by the effects of lightning, shows that these curious phenomena may be explained without having recourse to supposed new principles in physics. It also follows from what has been said, that no inference respecting the actual direction of the meteor itself can be derived from the direction in which masses have been moved by its effects, and that in researches made by those who have occupied themselves with ascending lightnings, such as rest on a basis of this kind, have no very solid foundation. The question is sufficiently important to justify our pursuing it somewhat further.

Some physicists, as we have already explained, make lightning consist in a very subtle matter which darts with the greatest velocity from the body which sends forth the shock or stroke towards the body which receives it; other physicists see in lightning only phenomena of vibration. Whichever of these two hypotheses might be adopted, it would, in either case, have been supposed hitherto that the direction in which lightning is propagated, — in other words, the direction in which either the subtle matter or the vibration is propagated, — should coincide with the direction of the mechanical effects produced. Lightning, which throws a body downwards, it was naturally said,

* Trees are often struck dead by lightning even when the external damage sustained by them appears extremely slight. Mr. Tull, author of "The Philosophy of Agriculture," thinks that this effect is the consequence of the rupture of the small vessels through which the lightning has passed. According to our view, lightning acts here mechanically, as frost does when it rends the capillary tubes composing the succulent stalks of certain plants. Only, as watery juices dilate much more in passing from a liquid state to a state of vapour than they do in freezing, lightning should produce much more numerous, and therefore more fatal, rents than frost. By looking at cases from this point of view, physiologists may perhaps arrive at last at recognising the particular mode of action by which death by lightning most often takes place.

should be called descending; and, conversely, the name of ascending lightning should be given to lightning which should project from below upwards substances which it met with in its course. There might, in the same way, be oblique and lateral lightnings from different quarters of the horizon. There are not wanting facts which could be used in support of these distinctions. We will cite some.

On the 24th of February, 1774, lightning struck the steeple of the church in the village of Rouvroi, to the northwest of the town of Arras. One of its effects was to lift the pavement, consisting of great blue stones, of a porch situated vertically beneath the steeple.

In the summer of 1787, lightning struck two persons who had taken shelter under a tree near the village of Tacon, in the Beaujolais. Portions of their hair were thrown to the top of the tree. An iron circle belonging to a sabot of one of the poor victims was also found after the event caught in a branch very high up.

On the 29th of August, 1808, a round thatched pavilion, belonging to a tavern, situated behind the Hôpital de la Salpêtrière, at Paris, was struck by lightning, and a workman, who was sitting under it, was killed. Portions of his hat were found incrusted in the ceiling.

If we regard all these phenomena of uplifting as the direct effects of the lightning, it will be difficult not to admit, with those who have discussed them, that at Rouvroi, Tacon, and the Salpêtrière it was ascending; that instead of descending from the clouds to the ground, it darted from the ground towards the clouds. Admit, on the contrary, the possibility of *indirect* effects; and take the vapour of water as an intermediate agent, and the raising of the pavement at Rouvroi, and the throwing up of the iron part of the sabot at Tacon, and of the fragments of the hat at the Salpêtrière will no longer be held to furnish any indications of the direction of the movement of the lightning.

Sometimes lightning only partially tears the bark off trees struck by it. In such cases, it is not rare to find long strips of bark, and of the layer of soft wood immediately next to the bark, completely detached at the bottom, but adhering to the trunk towards the upper part of the tree. The old collections of the Academie des Sciences would furnish me, if needful, with several instances of effects of this kind; I could also find such by looking over the "Journal de Physique," and, in particular, a memoir by M. Mourgues, on storms observed at Marsillargues, near Montpellier, in June, 1778, and a memoir by M. Marchais, on several trees of the Champs Elysées at Paris, &c., struck by lightning; but all these instances of bark stripped from below upwards will cease to have the meaning which those who discussed them were pleased to attribute to them, when the vapour of water comes to be regarded as the possible agent in the operation.

The same may be said of another phenomenon, which attracted equal attention, and has been described with the same minute care by observers. The leaves of trees struck by lightning, as those of the trees belonging to M. Mourgues, at Marsillargues, the leaves of the trees in the Champs Elysées examined by M. Marchais, &c. &c., were found to be yellow, crisped, burnt up, and convex underneath, while their upper surfaces remained green and uninjured, save that having been previously flat, or slightly convex, they had become concave; just as is the case with parchments on the side which has not faced the fire. "See," it was exclaimed, "in these circumstances so many proofs that the fiery current of the lightning moved from below upwards." The movement from below upwards seems, indeed, pretty well established; but who could now affirm that the ascending current did not consist of vapour at a high temperature, resulting from the vaporisation of water by the action of descending lightning on the humidity of the ground?

We might lastly have recourse to the same agent (aqueous vapour) if we wished to explain how it happens that the turf at the foot of trees struck by lightning is often turned up, and sometimes folded back, like the leaves of a book, at the two sides of the place where the ground is torn.

I have gone thus minutely into this discussion, because I thought it important to show that the facts from which many physicists thought they had established the existence of ascending lightnings do not really demonstrate such a result. I would add, however, that the question of ascending lightnings appears to me to be completely resolved by the combination of circumstances in the event related in Chapter XXVIII. I therefore admit, without reservation, the existence of ascending thunderbolts. I know that physicists of the first rank do not believe in it, and that they would even disdain to enter into any discussion on the subject; but facts must be held superior to the most imposing authorities. When, a century ago, Maffei, adducing the local phenomenon which he had observed at the Castle of Fosdinovo, put forth in a systematic manner his ideas on ascending thunderbolts, he took the precaution, (more prudent in this than Galileo,) of showing that they were quite reconcilable with the passage in the Scriptures respecting fire from heaven on Sodom and Gomorrah. Celebrated scientific theories, although some persons regard them with a kind of religious veneration, do not demand so much reserve. It is perfectly open to any one to examine, dispute, and criticise them, stopping only where observation and experiment begin to fail.

I will conclude this Chapter with an observation by M. de la Pilaie, of which it is difficult to connect the explanation with the action of aqueous vapour.

An oak tree having been struck by lightning at Commeraye, near Lamballe, at the end of May, 1843, M. de la Pilaie remarked that the bark of the trunk showed a slit or crack from the base of the trunk to the separation of the upper branches, the crack narrowing from below upwards, and the bark at its edges being ragged like lint. But whereas these woody fibres and other torn parts should have been torn from above downwards, they were, on the contrary, torn from below upwards, as if the lightning had remounted from the base to the summit. It had, moreover, traced a furrow, or superficial crack, on the wood itself, which furrow disappeared entirely at the upper part, where the slit in the bark had become very narrow and was confined only to its outside surface.

CHAP. XXXVIII.

ON THE DANGERS INCURRED FROM LIGHTNING. — HOW FAR ARE THESE DANGERS SUCH AS TO DESERVE CONSIDERATION? — BUILDINGS AND VESSELS STRUCK BY LIGHTNING.

§ 1.

How far is the danger of being struck by lightning sufficiently great to give importance to means of escaping it? This question has several sides: it may be looked at as respects individual persons, as respects houses, and as respects vessels.

In the interior of the great towns of Europe, men appear to be very little exposed to danger from lightning. Lichtenberg said that he had assured himself that in the course of half a century only five men were seriously hurt by lightning within the town of Göttingen. Of these five three died.

It is said, that at Halle one man only was killed by lightning between 1609 and 1825, *i.e.* in more than two centuries.

At Paris, where civil registers are kept with so much regularity, the chief statistical officer in the Prefecture assures me that for many years past not a single death has been reported from lightning. Yet we know that within this interval there have been persons struck by lightning in the département de la Seine; even if it were only the workman spoken of in p. 175.,—an agricultural labourer killed in the open fields, in the commune de Champigny, on the 26th of June, 1807; and, on the 3rd of August, 1811, a man, who had been cutting grass at Romainville, as he was fleeing from the storm, with an iron pronged fork in his hand. It follows, therefore, that these deaths, caused by lightning, must have been enrolled in a general manner amongst "accidental deaths;" and similar negligences, or omissions, may be supposed to have taken place elsewhere. We should not, therefore, be justified in accepting strictly and literally the statement of the number of deaths from lightning at Göttingen and at Halle. It would further be highly unsafe to generalise from such results, by applying to all countries what may have been observed in one, or in deducing from what has happened in a village what may be expected in a large town. While Göttingen, Halle, Paris, &c. count scarcely one death from lightning in a century, on cursorily opening a few volumes I find the following:—

In the night between the 26th and the 27th July, 1759, lightning fell on the theatre of the town of Feltre, killed many of the spectators, and more or less hurt all the rest.*

On the 18th of February, 1770, a single stroke of lightning threw to the ground senseless all of the inhabitants of Kevern, in Cornwall, who had assembled for Sunday service in the church.

* Lightning often occasions conflagrations. On this occasion the contrary happened: it extinguished all the lights.

In 1808, lightning twice struck the village inn of Capelle, in Brisgau, killed four persons in it and hurt many others.

On the 20th of March, 1784, lightning entered the theatre at Mantua; out of four hundred persons present it killed two and hurt ten.*

On the 11th of July, 1819, lightning struck the church of Chateauneuf-les-Moutiers, in the arrondissement de Digne, département des Basses Alpes, during divine service, killed nine persons on the spot, and more or less injured eighty-two; the same stroke killed, in a stable adjoining the church, five sheep and a mare.

No one, however, will, I think, be disposed to contradict me, if I venture to affirm, in spite of what is related above, that for every inhabitant of Paris the danger of death from lightning is less than the danger of being killed in passing along the street by the fall of a chimney or flower-pot, or of a workman engaged on a roof; this latter danger being, I imagine, one which occasions very little uneasiness. In respect to the fear excited by thunder and lightning, it ought, however, to be said, that the vivid and sudden light and resounding peals produce involuntary nervous effects, which even the most strongly organised tem peraments do not always altogether escape. It should also be said, that if the number of occasions when lightning actually strikes is small, the whole number of flashes of lightning and peals of thunder seen and heard in the course of the year is very great; that the harmless and the dangerous are not distinguished apart, and that the danger, however small in reality, must seem the greater on account of the considerable number of times when it is apparently renewed. This last consideration will be placed in a clearer light by recurring to the comparison above made, and supposing that the fall of the chimney, or the flower-pot, or the man, is announced by a loud detonation heard over the whole extent of the capital, so that each individual might suppose himself to be in the street where the accident is to happen, and thus, it will be easily conceived, might be more apprehensive than is now the case, although not really in any greater danger.

I have been speaking of accidents in large towns: according

* The lightning, moreover, melted earrings and watch-keys; it split or "cleft" diamonds,—and all this without in the least hurting the wearers.

to an opinion widely prevailing, persons are much more exposed in villages and in the open country. Theoretical considerations, which the plan I have formed for myself forbids my adducing at this moment, would tend to confirm this opinion. I cannot attempt to decide the question by an appeal to facts, for they have not been collected with sufficient completeness. It must be added, that we have no exact knowledge of the differences between one country and another, or one more circumscribed locality and another, as to the frequency and intensity of the actual strokes taking place during thunderstorms.

In the republic of New Granada, El Sitio de Tumba Barreto, near the gold mine of the Vega de Supia, is avoided as much as possible on account of the frequency of lightning strokes. It is remembered that many miners have been killed there by lightning. While M. Boussingault was crossing El Sitio, during a thunderstorm, his negro guide was struck to the ground by lightning. The Loma de Pitago, in the neighbourhood of Popayan, is similarly celebrated. A young Swedish botanist, M. Plancheman, persisted, in spite of the advice of the inhabitants, in crossing the Loma when the sky was covered with storm-clouds, and was killed there by lightning. But, without speaking of particular spots, and looking only to large countries, it sometimes happens with us that entire years may pass without hearing of any fatal accident, while elsewhere, during particular seasons, such may happen almost daily. Thus, I find that in the summer of 1797, from the month of June to the 28th of August, Volney counted up in the newspapers of the United States eighty-four serious accidents, and seventeen deaths. In France, the newspapers of 1805, if I am rightly informed, did not notice the death of a single person from lightning; those of 1806 only recorded the deaths of two children, killed by the same stroke, on their mother's knees, at Aubagne (in the département des Bouches du Rhone); in 1807, of two young labourers, of the commune of Saint-Geniez, struck by lightning while getting in their crops; and in 1808, the same papers only spoke of one person, a boatman, killed by the waterside at Angers. However, even in the same country, different years give very different results as to the number of fatal strokes. In 1819, there were killed, on the 28th of June, three horses near Vitry le Français; on the 11th of July, as I have already said, nine persons in the church of Chateauneuf;

on the 26th of July, a man in the open country, at Maxey-sur-Vaize (Meurthe); on the 27th of July, a labourer, his wife and his son, who had taken refuge under the porch of a chapel, near Chatillon-sur-Seine; the 1st of August, forty-four sheep, near Beaumont-le-Roger (Eure); the 2nd of August, a workman, who had taken shelter under a tree at Bordeaux; and, on the same day (2nd of August), an agriculturist of Vigneux, near Savenay, who was struck dead in his room, and two little girls, of ten and twelve years of age, in the house of the Abbé Coyrier, in the département du Cantal; and, lastly, on the 27th of September, at five in the morning, a servant-maid, who was in her bed at Confolens (Charente).

I have, however, given in a former chapter a kind of statistical table of all the cases of persons struck by lightning, which I could learn of on sufficient authority, in the course of some years: it shows that if on the one hand the number of sufferers is sufficiently limited for any one to regard the probability of death by thunder as very small, on the other hand, the number of deaths from that cause is sufficient to make it reasonable not to neglect the methods which science has pointed out for avoiding such accidents.

§ 2. *Destruction of buildings and ships.*

If there are few persons who perish from the effects of lightning in our towns, the number of houses and other buildings struck and seriously damaged by it is, on the contrary, considerable.

In the course of a single night, between the 14th and 15th of April, 1718, twenty-four steeples were struck by lightning within the space extending along the coast of Brittany, between Landerneau and St. Pol de Leon.

During the night between the 25th and 26th of April, 1760, lightning fell three times within the short interval of twenty minutes, on the church and buildings of the Abbaye de Notre-Dame, at Ham.

In the morning of the 17th September, 1772, four different buildings were struck by lightning at Padua.

By a memoir of Henley, dated December, 1773, I learn that on the same day, and if I am not mistaken, nearly at the same instant, lightning struck in London, St. Michael's Church, the

Obelisk in St. George's Fields, the New Bridewell, a house Lambeth, a house near Vauxhall, and many other places very distinct from each other, without counting a Dutch ship at anchor in the Thames near the Tower.

A German savant writing in 1783, computed that in the course of 33 years lightning had struck 386 steeples, and killed thereby 121 ringers.* The number of persons hurt but not killed was much greater still.

In December, 1806, during a single thunderstorm, lightning demolished, wholly or in part, the steeples of St. Martin of Vitré, of Erbré, of Croisilles, and of Etrelles.

On the 11th of July, 1807, the Church of St. Martin at Vitré was again struck. Five days before, lightning had fallen at la Guerche, and on ten churches or other buildings within a league radius round that town.

At Paris, on the night between the 7th and the 8th of August, 1807, lightning struck the sign over a shop in the Rue de Thionville, a house near the Halle, a lamp in the Rue de Perpignan, and also in the Rue aux Fèves, at Vaugirard, and at Passy.

On the 14th of May, 1806, we find lightning injuring a joiner's shop in the Rue Caumartin; on the 26th of June, 1807, ravaging nine rooms in a house at Aubervilliers; on the 29th of August, 1808, lightning struck a "guingette" near the Barrière des Gobelins, where it killed or injured several persons, and near the Barrière Montmartre, a tavern full of people, several of whom were thrown to the ground senseless; on the 14th of February, 1809, lightning demolished a windmill situated on the road to St. Denis; on the 29th of June, 1810, it broke and threw to a distance whatever it found on its passage, in a house in the Rue Popelinière; and on the 3rd of August, 1811, it fell on a house near the Barrière de Pantin, and hurt several persons in it.

On the 11th of January, 1815, during a storm which extended over the space comprised between the German Ocean and the Rhenish Provinces, lightning struck twelve steeples

* The number of ringers who suffered will appear less surprising if I mention that, when lightning struck the steeple of the village of Aubigny, on the 17th of June, 1755, it killed, by the same stroke, three men who were ringing the bells, and four children who had taken shelter under the tower of the steeple.

scattered over this great extent of country, setting some of them on fire, and considerably damaging the rest.

I think I may leave these enumerations without its being necessary to add that I believe them to be very incomplete; every one will have understood without my saying so, that all that they can pretend to is to offer a "lower limit."

The desirability of protecting buildings from lightning must be measured by the number which are annually struck, and also by the extent and importance of the damage so occasioned. Three or four citations will serve to show the force of this latter consideration.

In 1417, lightning set fire to and consumed the wooden pyramid which terminated the campanile of St. Mark at Venice. The pyramid was reconstructed, but it was again reduced to ashes by lightning on the 12th of August, 1489.

On the 20th of May, 1711, a single stroke of lightning not only did very great damage to the interior and exterior of the principal tower of the town of Berne, but also extended its ravages to nine neighbouring houses.

The pyramid of St. Mark at Venice, which this time was built of stone, was struck by lightning on the 23rd of April, 1745. The repairs cost more than 8,000 ducats.

On the 27th of July, 1759, lightning set fire to and consumed the whole of the timber of the roof of the Cathedral of Strasbourg.

In the month of October following, lightning struck the upper part of the magnificent tower, and injured one of the pillars which supported the lantern so greatly, that it was thought it would be necessary to take down the latter. The repairs of the damage cost upwards of 12,000*l.*

The three strokes of lightning which, in the night of the 25th to the 26th of April, 1760, fell on the church of Notre-Dame, of Ham, set on fire and completely ruined this large and fine edifice.

I ought not here to omit mentioning the extensive mischief sometimes produced by lightning when it strikes a powder magazine.

On the morning of the 18th of August, 1769, lightning struck the tower of St. Nazaire, at Brescia. This tower stood over a subterranean magazine containing between 113 and 114 tons of gunpowder belonging to the republic of Venice. This immense mass of powder was fired at once. The sixth part of the large and fine town of Brescia was overthrown, the remainder was much shaken and threatened to fall in ruins. Three thousand persons perished. The tower of St. Nazaire was thrown up into the air whole, and fell back in a rain of fragments, some of which were found at enormous distances. The mere money value of the damage amounted to 16 millions of francs (or 640,000*l.*).

On the 18th of August lightning set fire to the powder then remaining in the magazine at Malaga. The building was overthrown. The whole town would certainly have shared the same fate, if it had not some time before asked and obtained that the greater part of the powder should be removed to magazines at a distance.

On the 4th of May, 1785, lightning set fire to the powder magazine at Tangiers. The magazine and the greater part of the surrounding houses were overthrown.

On the 26th of June, 1807, at half-past eleven in the morning, a powder magazine at Luxembourg, very solidly built on the rock long previously by the Spaniards, and containing at the time of the explosion more than twelve tons of powder, was fired by lightning and blown up; thirty persons were killed, and more than two hundred were mutilated or severely wounded. The lower town (the Grund) was made a heap of ruins. Very large stones, which had been part of the magazine, were afterwards found a league distant, having been carried there by the explosion.

On the 9th of September, 1808, lightning struck a magazine in the fort of St. Andrea del Lido, at Venice, and blew it up. The explosion completely destroyed a barrack, an adjacent chapel, and a wall belonging to the demi-lune, and did considerable damage to another barrack inhabited by the gunners.

I have cited so many cases of explosions of powder magazines because, by successive generalisations, it has been actually asserted that lightning, when it makes its way into such buildings, never sets fire to the powder which they contain. Having thus shown how little such an opinion can be maintained, I must next confess that in certain cases the circumstances and results have been such as might seem to authorise the strangest hypotheses.

Thus on the 5th of November, 1755, lightning fell near Rouen on the powder magazine of Maromme, split one of the rafters of the roof, and broke into small pieces two casks filled with gunpowder, yet without causing any ignition; (the magazine contained 800 such casks).

In 1775, on the 11th of June, at daybreak, lightning fell on the tower of St. Secundus, at Venice, entered the magazine, tore off shelves, and overthrew cases filled with powder, yet (which was then deemed miraculous) without causing any ignition or explosion.

After the list of vessels struck by lightning which I have given in pp. 137—139., it might seem superfluous for me to insist further on the desirability of doing what we can to preserve ships from the effects of this meteor; that list, however, having been made out for a particular object, contains much fewer instances than it would have done if I could have dispensed with the date and the geographical position of the occurrence. Thus, from the very limited circle of observations at my command, I might have added to the forty-two citations in pages 137--139. the following:—

The (name unknown), English merchantman, struck by lightning in 1675, near the Bermudas.

The (name unknown), merchantman, struck by lightning at Bencoolen in 1741.

The (name unknown), Dutch ship, set on fire by lightning in 1746, in the harbour of Batavia. When the fire reached the gunpowder, the ship blew up.

The (name unknown), Dutch ship, struck by lightning and much damaged in 1750, near Malacca.

The "Harriot," English packet, bound to New York, in 1762. The three masts were struck and shattered.

The "Modeste," French frigate, set on fire by a stroke of lightning in 1766, and completely consumed.

Captain Cook's ship, and a Dutch vessel struck by lightning in the harbour of Batavia.

The "Zephyr," French frigate, struck by lightning at Port au Prince (St. Domingo), on the 23d of September, 1772. The mainmast injured.

"Le Meilleur Ami," of Bordeaux, struck by lightning at Port au Prince, on the 25th May, 1785. The main and mizen masts, and the maintopmast were broken into a thousand pieces.

The "Prevost de Langristin," of La Rochelle, struck by lightning at Port au Prince, on the 29th July, 1785. It was necessary to replace the main and maintop mast.

The (name unknown), a French schooner had its mainmast shivered by lightning on the same day (July 29th, 1785), and in the same port.

The "Duke," English ship of the line, of 90 guns, struck by lightning in 1793 off Martinique. One of the masts was entirely split.

The "Gibraltar," English ship of the line, struck by lightning in 1801, and much damaged, exactly over the powder-magazine.

The "Perseus," English vessel, struck by lightning at Port Jackson, in October, 1802. The ship narrowly escaped being wrecked in consequence.

The "Désiree," English frigate, struck by lightning at Jamaica in 1803. Shivers from one of its masts were found on shore.

The "Theseus," English ship, struck by lightning near St. Domingo in 1804.

The "Mignonne," English corvette, in the month of June, 1804, at Jamaica. Three sailors were killed and nine hurt: the principal mast was much damaged.

The "Désiree," near Jamaica, 20th of August, 1804; several parts of the frigate were set on fire by lightning.

The "Repulse," English ship, in the Bay of Rosas, in 1809.

The "Dedalus," English frigate, at Jamaica, in 1809. Part of the crew were thrown down senseless. Lightning set fire to the very small quantity of powder then in one of the magazines.

The "Hebe," English frigate, at Jamaica, in 1809, struck by lightning and lost one of her masts.

The (name unknown), English schooner, at Jamaica, in 1809. This ship foundered in consequence of the same stroke which damaged the "Dedalus" and the "Hebe."

The "Glory," English ship of the line, had all her masts split in 1811, near Cape Finistère.

The "Norge" (?) English man-of-war, and a merchantman, were struck by lightning in June 1814, at Jamaica. The man-of-war was dismasted.

The "Palma," English frigate, lost one of her masts by lightning in 1814, in the Port of Carthagena, in South America.

The "Medusa," English brig, in the voyage from La Guayro to Liverpool.

The "Amphion," American ship, considerably damaged by lightning on the 21st of September, 1822, in her passage from New York to Rio Janeiro. All the compasses were spoiled.

The "Jessie," of London, about the middle of November, 1833, was so much damaged, that the crew abandoned her in 45° N. lat. and 14° W. long.

The "Carron," English steamer, struck by lightning, in 1834, on her passage from Greece to Malta.

In looking attentively over the above, and the preceding list, it may be remarked, (and I think the remark likely to make an impression,) that in fifteen months of the years 1829—1830, five ships of the English Royal Navy were struck by lightning in the Mediterranean, viz., the "Mosquito," 10 guns, the "Madagascar," 50, and the "Ocean," "Melville," and "Gloucester," ships of the line; all these vessels suffered considerably in their masts. To those persons who say that damage by lightning is of very little importance in a pecuniary point of view, I would add that the mainmast of a frigate costs 200*l.*, and that of a ship of the line as much as 400*l.*

To so many authentic instances of the effects of lightning, I might add that the English ship "Resistance," of 44 guns, and the "Lynx," after some strokes of lightning, disappeared altogether from among a convoy to which they belonged; that the "York," 64, of which no tidings were heard after she had entered the Mediterranean, probably either blew up or foundered from the effects of lightning; that the cases of vessels set on fire, mentioned in the above list, are not the only ones which might be cited; that, for example, the "Logan," of New York, (spoken of earlier in the volume) of 420 tons, and 20,000*l.* value, was entirely consumed; that the "Hannibal," of Boston, had the same fate in 1824; that the crews suffer as well as the masts, rigging, and hulls; that two men were killed, and twenty-two hurt by the lightning which struck the "Cambrian" at Plymouth, in 1799; that by a stroke of lightning the "Sultan," at Port Mahon, in 1808, lost five men killed on the spot, and two who were thrown overboard and drowned, besides three severely burnt; that nine seamen were killed by the stroke of lightning received by the "Repulse" in the Bay of Rosas (1809); that three seamen were killed, and five wounded on board the Austrian frigate "Leipsic," when she was struck by lightning in 1833, off Cephalonia, &c. &c.

But what I have already related should be sufficient. I have cited facts without either exaggeration or reserve. Every one may appreciate for himself the importance attaching to the modes of preservation from lightning which have been devised or imagined; and we may now proceed to a serious examination of them.

CHAP. XXXIX.

MEANS OF PRESERVATION FROM LIGHTNING.

I SHALL, I hope, be excused for briefly recalling certain pretended means of preservation which with our present knowledge may appear absurd. But I would at any rate observe that the study of the aberrations of the human mind should not be separated from that of true discoveries, and that besides it is possible that very gross errors may still retain many partisans.

§ 1. *On the means which men have thought suitable for their personal preservation from lightning.*

Greek literature has completely initiated us into the ideas of the ancient philosophers respecting the cause of thunder; but we find in it only very slight and imperfect allusions to two or three supposed means of preservation.

Herodotus (book iv. chap. 94.) relates, that when it thunders and lightens the Thracians are in the habit of shooting arrows against the sky, to threaten it.

Remark that he distinctly says "to threaten it." There is no question at all of the power which the arrow, being metallic and pointed, would have of taking from the clouds some little portion of their fulminating matter. This is so clear, that even that fanatically-ardent admirer of antiquity, Dutens, drew back from the idea of assimilating the Thracian arrows to the lightning-conductors of modern times, and from making the invention of Franklin's apparatus go back to the time of Herodotus.

Pliny relates, that the Etruscans knew how to call down lightning from the sky, and to direct it at their pleasure. That they thus made it strike a monster called Volta, who was ravaging the environs of Volsinia; that Numa possessed the same secret; and that Tullus Hostilius, from want of exactness in the accomplishment of the ceremonies which he had borrowed from his predecessor, drew the lightning upon himself. As to the means of thus evoking and directing the meteor, Pliny speaks only of sacrifices, prayers, &c. We may, therefore, pass to another subject.*

The ancients believed (*vide* Pliny, lib. ii. § 56.) that thunder never penetrates beyond six feet below the surface of the ground. They, therefore, regarded the greater part of caves or caverns as perfectly safe asylums; and Suetonius said that as soon as a thunderstorm could be foreseen, Augustus used to retire into a low vaulted place.

The vitreous tubes formed by the action of lightning, discussed

* Is it true that there existed a Roman medal inscribed with the legend "Jupiter Elicius," and representing that god hovering on a cloud, while an Etruscan sends up a kite?

Duchoul made an engraving of a medal of Augustus, having on it a temple of Juno, goddess of the air, the roof of which is armed by several pointed rods. Is this medal authentic? (Laboissière, Acad. du Gard.)

at so much length by us in Chapter XXI., and which sometimes go down to more than thirty feet below the surface of the ground, show how much the ancients were mistaken. No one knows, no one even at the present time could say, at what depth there would be complete security from descending lightnings; still less could such be assigned for ascending lightnings.

For the sake of adding to the protection afforded by the thickness of the masonry, stone, or earth covering over either an artificial or natural cave or cavern, the emperors of Japan, according to Kæmpfer, have a cistern holding water fixed above the grotto into which they retire during thunderstorms. The water is designed to extinguish the fire of the lightning.

Under certain conditions, to be presently described, a sheet of water affords an almost certain protection for whatever is below it; but it must not be thence inferred that fish may not be killed by lightning in very extensive masses of water.

We are told by Weichard Valvasor (Phil. Trans. vol. xvi.) that about the year 1670 lightning having fallen on the lake of Zirknïtz, such a quantity of fish were seen soon afterwards floating on the surface of the water that the inhabitants of the neighbourhood filled twenty-eight small carts with them.

On the 14th of September, 1772, immediately after a stroke of lightning at Besançon, the surface of the water was seen covered with fish, stunned and floating with the current.

It was generally believed by the ancients, that persons lying in bed were safe from lightning. This opinion, extraordinary as it may be, has long continued to have some partisans. I even see that Mr. Howard took a particular pleasure in recording the two following anecdotes.

On the 3rd of July, 1828, lightning fell on a cottage at Birdham, near Chichester. A wooden bedstead was broken to pieces by it, the bedclothes and mattress were rolled off on the ground, together with the person who was sleeping in the bed, who, nevertheless, escaped quite uninjured.

On the 9th of the same month, at Great Houghton, near Doncaster, by a stroke of lightning, the coverlid was torn off the bed in which Mrs. Brook lay, without that lady suffering in the least, otherwise than by the fright.

But these facts may have placed opposite to them others no less authentic.

The sixty-third volume of the *Philosophical Transactions* contains a memoir, in which the Rev. Dr. Kirkshaw gives an account of all the circumstances attendant on the stroke of lightning by which, on the 29th of September, 1772, Mr. Thomas Heartly was killed instantaneously, while sleeping in his bed. His wife, who was sleeping by his side, was not even awakened. The only effect of the lightning in her case was a numbness and pain in the right arm, which lasted a few days.

On the 27th of September, 1819, at 5 o'clock, A.M., at Confolens (Charente), lightning fell on a house, where it killed the servant-maid, who was in her bed. The body was marked by a furrow from the neck to the right leg.

The idea that a mattress formed a sufficient protection against lightning, was formerly very general; and, in consequence, some persons used to take refuge under the mattresses of their beds during thunderstorms. The stroke of lightning in the barrack of St. Maurice, at Lille, on the 5th of September, 1838, showed that it would be a mistake to place much dependence on such a preservative. Dr. Poggiale observed that the mattresses of two beds, in each of which a soldier was sleeping, were pierced through and through by the lightning.

Among the Romans, sealskins were regarded as efficient preservatives against lightning. They were used, on this account, for covering tents, under which timid persons sought refuge during thunderstorms. Suetonius relates, that the Emperor Augustus, who had a dread of thunder, always wore one of these skins.

In the Cévennes, where Roman colonies existed so long, the shepherds collect with care the cast-off skins of snakes; and even at the present day twist them round their hats, and believe that they thereby secure themselves against lightning. (Laboissière, Acad. du Gard.) It would seem as if these snakeskins had been formerly regarded as having the same property as that attributed to the more rare and costly sealskins.

We know of no reason, practical or theoretical, to justify the value attached to sealskins by Augustus. The general idea that during thunderstorms there may be some preference due to one kind of clothing over another as regards lightning, is not, however, in any way opposed to modern knowledge. I might even cite many cases in which persons appear to have escaped, or to have suffered from lightning, according to the material of which their clothes were made.

Rubruquis, in his *Relation d'un Voyage en Tartarie*, undertaken by the orders of Louis IX., says that the inhabitants of that country are extremely afraid of thunder and lightning; that when they hear thunder they expel all strangers from their dwellings, wrap themselves up in felts, or thick black cloths, and remain thus without moving until the storm ceases.

On the day of the catastrophe at Chateauneuf-les-Moutiers, related in p. 179., two out of three priests who were round the altar were severely injured, the third was quite unhurt; he was the only one who was dressed in vestments ornamented with silk.*

The following facts are still more remarkable; for they show that an animal may be more or less hurt on different parts of his body, according to the colour of the hair over those parts.

Early in September, 1774, at Swanborough in Sussex, lightning struck an ox of a reddish colour spotted with white: it was afterwards remarked with surprise that not a hair was left on the white spots, while the red part showed no signs of injury. The owner of the animal told Mr. James Lambert that two years before, another ox, spotted with white, which had also been struck by lightning, presented exactly the same phenomenon.

Lastly, on the 20th of September, 1775, a dappled horse having been struck by lightning at Glynd, its owner remarked that on all the white spots the hair came off on the slightest touch, while over the other parts it had its usual adherence.

Suetonius said that "when the sky was stormy, Tiberius was careful to wear a laurel crown, from the idea that the foliage of the laurel is never scathed by lightning."

The Chinese consider the mulberry and the peach tree as good preservatives against lightning. (Edouard Biot.)

The opinion that certain kinds of trees are never struck by lightning still prevails extensively.

Mr. Hugh Maxwell wrote, in 1787, to the American Academy,

* According to indirect experiments, all physicists recognise oil-cloth, silk, and wool as less permeable to lightning than linen or other tissues of vegetable substances. They are less unanimous on the preference due to wet or dry clothing. Nollet objects to wet garments, that water is one of the bodies towards which lightning turns by preference. Franklin, on the contrary, deems wet garments advantageous, from the idea that they may form safe conductors, transmitting the fulminating matter, if it should strike them, directly to the ground.

that from his own experience, and from the information which he had collected from a great number of persons, he thought he might assert that the elm, the chestnut, the oak, and the pine are often struck by lightning; the ash, rarely; and the beech, birch, and maple, never.

Captain Dibden did not admit such marked distinctions. In a letter to Wilson, dated in 1764, he contented himself with saying, that in the forest in Virginia, which he had been visiting in the preceding year, the pines, although much loftier than the oaks, were nevertheless much less often struck. He added, "I do not remember having ever seen any oaks growing among the pines in places where a few of the latter had been struck." I subjoin a few facts which will dispel many doubts.

The ancients believed that the laurel is never struck by lightning. The expression *never* could not now be justified; for I find in the notes of Poinsinet de Sivry, one of the translators of Pliny, that Sennert, Vicomercatus, and Sachs record several instances of laurels struck by lightning.

Maxwell classes the beech among the trees which the lightning respects. I find in a pamphlet by M. Héricart de Thury, distributed amongst the members of the Academy of Sciences, that an old beech tree, which had been preserved in 1835, when the surrounding tall timber trees had been felled in the middle of the forest at Villers-Cotteret, was struck by lightning and nearly destroyed in the month of July in the same year.

Theoretical considerations had tended to induce a belief that resinous trees would be spared by lightning; yet we see that Maxwell named pines among the kinds of trees most often struck. In the pamphlet by M. de Thury, which I have just cited, I find, among the instances of trees struck by lightning:—

A pine at St. Martin-de-Thury, on the 2nd of August, 1821.

A fir-tree at St. Jean-de-Day (Manche), in June, 1836.

A wild cherry-tree at Anthilly, in August, 1834.

An acacia at St. Jean-le-pauvre-de-Thury, in September, 1814.

An elm at Moiselles, in June, 1823.

Oaks and poplars.

Men are often struck by lightning in the middle of open plains. Many facts show that the danger is still greater under trees; from this double remark, Dr. Winthrop inferred that when surprised by a thunderstorm in the open country, the best thing to be done to avoid lightning is to place oneself at a little distance from some large tree; by "a little distance," he meant anything from 16 to 40 feet. A still more favourable station would be one intermediate between two trees, at the prescribed distance from both. Franklin approved these precepts. Henley, who also thought them confirmed both by theory and experience, recommended in the case of a single tree, five or six yards beyond the extremity of the longest branches.

From analogy physicists have admitted that lightning respects glass; it was but a step from thence to suppose that a cage constructed wholly of glass would be an asylum perfectly secure, and such cages have not only been proposed for persons afraid of lightning, but have actually been made!

I am quite ready to grant that during a thunderstorm a covering of glass a little diminishes the danger which may exist, but I cannot admit that it does away with it altogether. My reasons are the following:—

When the Minuzzi Palace, in the territory of Ceneda, was violently struck by lightning on the 15th of June, 1776, more than eight hundred panes of glass were either pierced or broken by the lightning.

When in September, 1780, in his house at Eastbourne, Mr. James Adair was thrown to the ground by the stroke of lightning which killed two of his servants, he was standing behind a glass window. The framework of the window was uninjured, but the panes of glass had completely disappeared; the lightning had reduced them to powder.

In this case it is, indeed, possible to suppose that the glass was broken by the concussion of the air caused by the detonation. Let us take facts which are less doubtful.

On the 17th of September, 1772, lightning struck at Padua a house situated at Prato della Valle; it pierced a pane of glass in a window on the ground-floor, making a hole as round and clean as if drilled with an auger.

The engineer Caselli of Alessandria, in 1678, observed in the glass of his windows, immediately after a stroke of lightning, round holes, with scarcely any adjoining cracks. (*See* p. 84.)

In September, 1824, the house of Mr. William Bremmer, at Milton Comage, being struck by lightning, one of the panes of glass in a window was found to have been perforated by a circular hole the size of a bullet; the whole of the rest of the pane was without crack or fracture of any sort.

A perfectly circular hole, without any crack or starring, could not be the result of the concussion of the air caused by a detonation; it is a fact which might also be cited in support, if need were, of the extreme rapidity of the motion of fulminating matter. The three observations which have been cited (*i. e.* the one in Mr. Bremmer's house, and those of Padua and Alessandria,) may serve to undeceive those who imagine that panes of glass oppose an impassable barrier to the passage of lightning.

Very numerous examples have proved that persons are never struck by lightning without its attacking more particularly portions of metal worn by them. It may, then, be admitted, that the danger of being struck is sensibly increased by metals attached to the person. Every one will be ready to admit this, where the metallic masses are at all considerable; I may mention that on the 21st of July 1819 lightning fell on the prison of Biberach, in Swabia; and that, in the great hall, amidst twenty prisoners, the one struck was the condemned chief of a band of robbers, who was chained by the waist.

The supposition may be more difficult to justify, as applied to the small portions of metal which form part of our ordinary apparel. The curious observation made by M. de Saussure and his travelling companions, on the Breven, in 1767, may, however, perhaps be regarded as evidence on this point.

The weather was thundery. On raising the hand, and stretching out a finger, a kind of pricking was felt at the finger-ends. M. Jalabert, who had gold-lace round his hat, heard, moreover, a buzzing round his head. Sparks were drawn both from the gold button of M. Jalabert's hat, and from the iron ferrule of a large stick which they carried.*

* I had long known, that according to the reports of different observers, the atmosphere, when strongly impregnated with fulminating matter during a heavy fall of snow, becomes sonorous to such an astonishing degree that it is sufficient to move one's fingers with some rapidity to produce musical sounds. However, when speaking of luminous plumes during thunder-storms in Chap. XXX., I did not venture to mention the singular acoustic properties which are said to follow from the atmospheric condition of which we are speaking. A note, which I have just met with in Brewster's Encyclopedia, though it does not remove my doubts, has so far shaken them as to induce me to revert to the subject.

The celebrated Edinburgh physicist says that Messrs. Tupper and Lanfiar, having ascended Mount Etna in July, 1814, were overtaken in their

Let the intensity of the storm be a little increased, and under circumstances similar to those which existed on the Breven, the slight gold-lace and little button will become causes of explosion, and M. Jalabert will be struck by lightning rather than his companions, whose hats were not adorned with gold-lace or buttons.

The following fact, reported by Constantini in 1749, is still more directly to the purpose.

A lady was putting out her hand to close a window during a thunderstorm, the lightning darted, and the gold bracelet which she wore disappeared altogether, so that no vestige of it was found. She herself received only some very slight hurts.

Without these preliminary remarks, my readers might have been surprised at my introducing here the explanation given by the celebrated traveller Brydone, of an accident which happened to a lady of his acquaintance, Mrs. Douglas.

She was looking out of her window during a thunderstorm, when a flash of lightning reduced her bonnet to ashes, without doing any other injury whatsoever. Mr. Brydone considered that the lightning had been attracted by the thin metallic wire supporting the front of the bonnet, and accordingly recommended the discontinuance of the use of such wires, as well as of hair pins and bodkins*, and other metallic ornaments for the hair. He added, that if, as he naturally feared would be the case, his counsels on these points should be ineffectual, he should recommend a thin chain or wire, which might be hooked

descent, when not far from the Casa degli Inglesi, by a heavy fall of snow, accompanied by violent thunder. The travellers and their guide not only heard, as Saussure and Jalabert had done, a simple hissing sound, when they stretched out a hand with one finger open and the rest closed,—but when they proceeded to move the said finger in different directions and with different degrees of rapidity through the snowy atmosphere, they found they could at pleasure produce a great variety of musical sounds sufficiently intense to be heard at a distance of more than forty feet.

I know very well what difficulty there is in forming a conception of how discharges proceeding from snow-flakes could have such regularity of intervals of space as is needed for the production of musical sounds; but where should we be if we were to deny every fact which we cannot explain?

* Kundmann relates that a brass bodkin worn by a young girl to fasten her hair, was fused by a stroke of lightning; the wearer escaped, and even her hair was not burnt.

on to the front of the bonnet during a storm, and falling down to the ground would act as a conductor.

Strictly speaking, no doubt it is better not to have pieces of metal about the person during a thunderstorm; but is it worth while to regard the amount of increased danger occasioned by a watch, a buckle, a chain, pieces of money, wires, pins, or other pieces of metal employed in men or women's apparel? It is a question which does not admit of being answered generally, since it will be viewed differently by different persons, according as they are more or less influenced by the degree of apprehension which they entertain.

§ 2. *When lightning falls on men or animals ranged either in a straight line or in an open curve, it is at the two extremities of the line that its effects are, generally speaking, most intense and disastrous.*

This theorem, if I may permit myself to call it so, appears to follow from the facts which I have collected, and which I proceed to analyse. Every one, I hope, will understand that I am here considering a purely scientific question; and that in indicating the place least exposed to danger, I am very far from advising any one to take it; since if they thereby lessened their own risk, it would only be by increasing the risk of others.

On the 2nd of August 1785, at Rambouillet, lightning fell on a stable where thirty-two horses were ranged in a row. Thirty were thrown down; only two were killed, one falling dead on the instant, the other dying subsequently of the severe injuries he received: they were the two which occupied the two extremities of the file.

On the 22nd of August, 1808, lightning fell on a house in the village of Knonau, in Switzerland. Five children were sitting reading on a bench in a room on the ground-floor. The first and the last were killed on the spot; the three others suffered only a violent concussion.

At Flavigny (Côte d'Or), five horses were in a stable where lightning entered. The two first and two last were killed, the middle one was quite unhurt.*

* I cite this fact in support of the proposition at the head of this chapter, although at the time and at the place where it happened it was explained by the remark, that the horse which escaped was blind, and the four others were not.

One of my friends informs me that a few years ago, in a town in Franche-Comté he was told, only a few days after the occurrence, of lightning having fallen on a string of five horses in the open field, and having killed the first and the last, and left the three middle ones apparently quite unhurt.*

We know and understand that when lightning encounters a metallic bar, it is only on entering and on quitting it, that it does much damage. It is easy to conceive that the same thing may happen with any other kind of body; but I think this would scarcely have been imagined to extend to cases where the continuity is largely interrupted; as for example,—that thirty-two horses, arranged with the ordinary intervals in a stable, should have to be considered, so far as the effects of lightning are concerned, as a single mass having a beginning and an end. Yet to what other assimilation can we have recourse to explain the curious phenomenon to which this paragraph is devoted?

§ 3. *Precautions which may be adopted by persons afraid of lightning.*

Franklin has given some precepts for the use of such persons

* In the year nine of the republic, lightning fell at Praville, near Chartres, on a windmill, set it on fire and everything was burnt. The miller was at the time on the road walking between a horse and a mule both laden with corn. The two animals were struck dead; the man was violently stunned, but otherwise escaped with only the loss of his hat and some locks of his hair, which were burnt.

I have not placed this case in the text because I think it is less direct to the point, inasmuch as it is not self-evident that lightning kills all kinds of animals with equal facility, and it even appears to me to be shown by several facts that men resist its effects better than horses and dogs.

On the 12th of April, 1781, Messrs. d'Aussac, de Gautran, and de Laval-longue, were struck by lightning near Castres. The three horses on which those gentlemen rode were killed on the spot. Only one of the riders, M. d'Aussac, perished.

In June 1826, near Worcester, a mare led by a boy was killed by lightning; the boy was unhurt.

In June 1810, Mr. Cowens was in a room with his dog by his side, when lightning entered the room; the dog only was killed, Mr. Cowens barely felt the shock.

On the 11th of July 1819, as already related, lightning killed nine persons out of the congregation assembled at divine service in the church at Chateau-Neuf-les-Moutiers; but it was not added that several dogs which were in the church were all killed without exception. These animals were all found dead in the attitudes in which they were before the stroke.

who, during thunderstorms, are in houses not provided with lightning conductors.

He recommends them to avoid the neighbourhood of fire-places. Lightning does indeed often enter by the chimney, on account of the internal coating of soot, which is one of the bodies for which, as for metals, lightning evinces a preference.

For the same reason avoid, as much as possible, metals, gildings, and mirrors on account of their quicksilver.

The best place is in the middle of the room; unless, indeed, there should be a lamp or chandelier hanging from the ceiling.

The less the contact with the walls or the floor, the less the danger. A hammock suspended by silken cords in the middle of a large room, would be the safest place.

In the absence of means of suspension, the next best place is on substances which are bad conductors, such as glass, pitch, or several mattresses.

These precautions must be supposed to diminish the danger, but they do not altogether remove it. There have been instances of glass, pitch, and several thicknesses of mattresses being traversed by lightning. It should also be understood, that if the lightning does not find round the room a continuity of metal which it may follow, it may dart from one point to another diametrically opposite, and thus encounter persons in the middle of the room, even if they were suspended in hammocks.

Some meteorologists—Balitoro, among others,—assert that lightning never strikes the northern face of buildings; they say the south-east is the aspect most exposed to danger.

This opinion is said to be sufficiently prevalent in Italy to induce many persons to move, during thunderstorms, into those rooms which look towards the north. If all this be correct, perhaps it may only be from the direction in which, in our climates, the wind almost always blows during thunderstorms.

Clouds coming from the south, strongly charged with lightning, may be expected to discharge it on the first, or southern, face of the buildings over which they pass. But now that we know that the streamers of the Aurora range themselves parallel to the magnetic dipping-needle, what right have we to deny the possibility of a community of direction in fulminating darts?

According to Nollet, with similar elevations, and all other circumstances equal, spires and steeples covered with slates, are more often and more violently struck by lightning, than those which are built of stone.

I do not think that the cause of such a difference should be sought in some specific difference between the substance of the slate and that of the stone. It would be more likely to be found in the humidity which, during heavy rain, so easily penetrates between the slates to the timber and laths on which they rest, and to the number of iron nails employed in fixing them.

The greater the mass and the volume of the conducting matter brought together in one place, the greater the chance of being struck by lightning in its neighbourhood. This being once admitted, since living man is a sufficiently good conductor of the fulminating matter, ought we utterly to reject the opinion of some able physicists (of Nollet, for example), who think that the danger of being struck by lightning in a church increases with the number of persons assembled there?

There is another cause which may contribute to render numerous assemblages of men or animals dangerous during thunderstorms. Their perspiration cannot fail to occasion an ascending column of vapour; and, as it is well known that moist air is a better conductor of lightning than dry air, such a column has a natural tendency to attract the lightning by preference to the place from which it proceeds. Need we therefore be surprised that flocks of sheep are so often struck by lightning, and that a single stroke should sometimes cause the death of thirty, forty, and even fifty of those animals?

In America it is a generally received opinion, that barns full of grain or of forage are more often struck by lightning than other buildings.

This circumstance would also seem to be attributable to an ascending current of moist air, of which the origin may be easily accounted for, since crops are usually housed before they have arrived at a state of great dryness.

It sometimes happens that one person in the midst of a numerous group is struck by lightning, without the determining causes of this kind of selection being at all discerned,—without the sufferer wearing more metal about his person than the rest of the party, and without his position relatively to surrounding objects presenting any apparent peculiarity.

I say any *apparent* peculiarity, because a cause may be active without being apparent; for a mass of iron concealed in the thickness of a wall, produces just as much effect as if it was uncovered, &c. It can very rarely happen that it can be safely affirmed, that all circumstances were identically the same in the situations occupied by the persons struck and those who were spared. The latter may have been further from some mass of metal, water, &c. &c., existing, concealed and unsuspected, under the floor, behind a wainscot, in the earth, &c. &c.

It would seem difficult to arrive, in this manner, at discovering whether they are any specific differences between different individuals, in their degree of liability to being struck. This doubt could only be elucidated by the aid of indirect experiments, which will be examined in a future chapter. In this place I content myself with saying that there are specific differences; and that, during a thunderstorm, in situations perfectly similar, one man may, by the nature of his constitution, be in greater danger than another.*

§ 4. *Is the danger of being struck by lightning increased by running, or by rapid motion during a storm?*

It is said that it is dangerous during storms either to run or to ride fast; it is even added, that persons should not walk or ride at all against the wind, or in the direction opposite to that

* On further consideration, I am disposed to give in this place, in a few words, a general idea of the experiments alluded to.

The matter which darts in sparks from the conductor of an electric machine after the plate has been turned for some time, is fulminating matter. Like fulminating matter, or lightning, it is transmitted almost without loss of power through great extents of metal, water, &c. It also traverses pretty freely a number of men joined hand in hand, and forming a chain. It is, however, found that there are some persons who abruptly arrest, or form an obstacle to, the communication, and who do not feel the shock, even if placed next to the first person in the file. Such exceptional persons must be classed exceptionally among the non-conducting or imperfectly-conducting bodies which lightning respects, or at least rarely strikes.

Such strongly marked differences imply intermediate shades. Now every degree of conductibility corresponds during a thunderstorm to a certain degree of danger. A man, who should be as good a conductor as a metal, would be as liable to be struck as a mass of metal; a man who would interrupt the transmission of electricity when forming one of a chain, would be almost as little likely to be struck as if his body were of the nature of glass or of resin. Between two such extreme cases there would be persons whose liability would be similar to that of wood, of stones, &c. &c. Thus it would seem that the place occupied by each person is not the only influential circumstance: the physical constitution of the individual may also have some share in the result.

in which the clouds are moving. These two recommendations, when examined, are seen to signify that one should avoid being in a current of air.

Does a current of air really tend to attract lightning, or to facilitate its fall? For want of decisive means of answering this question, the advisers refer to the general habit which prevails of shutting doors and windows when a storm commences, as if it were the result of actual experiment or experience; saying that nations living remote from each other would not have generally agreed on the same practice if it possessed no real advantage. Need I remark that there is no popular prejudice which might not be justified by such reasoning?

Moreover, during a thunderstorm, wind and rain prevail, and would alone be sufficient to render the practice alluded to a very natural one. It is true, that in some countries the custom is also based on superstitious ideas; for in Esthonia they say that they close even the smallest apertures of their houses against the entrance of the Evil Spirit, who is at such times fleeing from the Divine wrath, manifested by the voice of the thunder (Salverte, *Des Sciences Occultes*). Is it not remarkable that in some countries the Jews have been led by religious ideas to a precisely opposite practice? The Abbé Dechman says that as soon as lightning flashes across the clouds, the Jews of those countries throw open their doors and windows, in order that the Messiah, whose coming is to be heralded by thunder, may enter the house chosen by him.

To revert to the examination of the question of the influence of currents of air, so far as the present state of our knowledge permits:—

The atmosphere opposes a certain resistance to the passage of the matter of lightning. It is probable that this resistance diminishes as the temperature and humidity increase, and as the barometric pressure diminishes. If so, whatever diminishes the density of the air tends more or less to attract the lightning. A man running, in calm weather, leaves behind him a space in which, mathematically speaking, the air is comparatively rarified. All other circumstances being equal, lightning will be more likely to strike such a space.

The circumstances attending on the following fact were communicated to me by my illustrious *confrère*, Admiral Roussin; it may be regarded as in some slight degree favourable to the above conjectures.

The frigate *La Junon*, sailing to India, was assailed on the 18th of April, 1830, not far from the Canaries, by a violent thunderstorm, during which, notwithstanding the lightning-conductors, lightning fell on board the ship.

The fact of the fall of the lightning does not appear doubtful. Immediately after the explosion, a strong sulphurous smell was perceived all over the ship. Persons on the after part of the deck saw a flame detach itself from the chain of the conductor. This flame showed itself at a point situated half-way between the maintop and the quarter netting, and went to the larboard side, where it disappeared in the waves, whereas the extremity of the chain communicated with the sea on the opposite, or starboard side of the ship. Lastly, I have to add that, simultaneously with the thunderclap, one of the crew was so completely asphyxiated that he was thought to be dead.

After the accident, it was ascertained that the chain composed of brass wires twisted like cordage, and forming a cylinder of about 4-tenths of an inch diameter, had not been anywhere broken. The point of the metallic rod screwed to the mainmast head, and with which the conducting chain communicated, was alone burnt.

We have thus the fact of a lateral discharge of lightning coming from the conductor actually known to us in all its details. It would remain to find the explanation. The first explanation which suggests itself, is to suppose that the metallic chain was of much too small diameter. Might it not be supposed, in addition, that at the moment of the discharge the extremity of the chain did not enter the water? This extremity is attached to a brass or copper plate, nailed usually to the two or three first streaks below the water line. The plate was on the starboard side, and the wind was on the starboard quarter; and in the narrative the wind is spoken of as very strong at the time. Everything, therefore, tends to lead us to believe that the rolling of the ship momentarily lifted the lower extremity of the conductor; unfortunately we cannot say how much; and this circumstance must be admitted to materially lessen the weight of the conjecture which I have ventured to put forward.

On board the frigate every one was convinced that the lightning had quitted the conductor from the effect of the very violent wind which was then blowing. I am certainly very far from regarding this explanation as sufficient. On the other hand, however, I would not venture to declare it unworthy of examination. Under the lee of the conducting metallic chain, as under that of the rigging, masts, &c., there would have been, as the result of a phenomenon well known to those conversant with hydraulics under the name of lateral communication of motion, a sort of vacuum; that is to say, a small space in which the atmospheric pressure was considerably weakened. Now it would not be according to a true philosophical spirit unreservedly to deny any influence from this abrupt diminution of pressure, especially in the presence of many observations in physics, which I shall have to speak of in the sequel, when viewing the phenomena of artificial electricity in connection with those of lightning.

I have thus noticed the principal considerations on which the recommendation not to move rapidly during thunder may have been grounded. It is permitted, however, to every one to ask himself whether what is gained by standing still or moving slowly, in lessening the risk of lightning, is an adequate compensation for the unpleasantness of being wetted by a heavy shower.

§ 5. *Is, as some physicists suppose, the constitution of the clouds from which lightning is incessantly darting, such, as to cause imminent danger of death to those who should traverse them?*

The intimate constitution of clouds is too imperfectly known for us to be able to estimate from theoretical considerations the amount of danger which may be incurred from approaching too nearly to the focus of a thunderstorm. On this point, general opinion appears to me to be much more a matter of sentiment than the result of any thorough discussion. Black clouds sometimes dart afar destruction, conflagration, and death! What, then, ought not to be expected from their close proximity? Such is the vague impression; and perhaps Volta himself had no better guide when, in his memoir on the formation of hail, he treated the project of traversing a storm cloud as a piece of unheard-of daring. The question has, however, appeared to me to deserve examination. I thought it important to know whether meteorologists might preserve the hope of being able, sooner or later, to study the phenomena of lightning in the very region where it is elaborated. It seemed also useful to appreciate at its just value the danger incurred on certain mountains, when storms are formed too rapidly to allow travellers time to escape them. My task has been simply to examine whether persons had ever been in the midst of clouds which were the seat of decided thunderstorm without perishing there; but I knew it was indispensable that I should only admit clear and precise observations exempt from ambiguity. I find all these characters in a narrative by the Abbé Richard, author of the *Histoire de l'Air et des Météores.*

At the end of August, 1750, M. Richard was driving in a carriage up the little mountain of Boyer, a short distance from Senecey, between Chalon-sur-Saone and Tournus. Three quarters of the way up the mountain there was a stationary cloud, in which thunder growled from time to time. M. Richard soon reached this cloud. From the moment of doing so the thunder no longer manifested itself by sudden claps, alternating with intervals of silence: it now made a continual rumbling, resembling that of a heap of walnuts rolled about on the floor. When the observer had attained the summit of the mountain, he found himself above the cloud, which had not ceased to be a thunder-cloud, for it was traversed by brilliant lightnings, and loud detonations issued from it.

The next example which I shall cite is not vouched for by the testimony of a physicist. This may, perhaps, be rather an advantage, since the few and very simple circumstances of the phenomenon were collected by a person who had no system to support. I wrote down the following account from my sister's dictation:—

"Some years ago I set off one morning, with two friends, from the village of Estagel, for Limoux. Our carriage had already climbed a good portion of the steep and winding road of the Col St. Louis, when the whole valley became suddenly covered with storm-clouds. One could not doubt their being such, since bright flashes and loud thunderclaps proceeded from them. The ladies who were with me, and I myself, wished to turn back, but our driver was of an opposite opinion; he accordingly went on to meet the storm. As we were much frightened we shut our eyes, that we might not see the lightning, and stopped our ears that we might not hear the thunder. We had been for about a quarter of an hour in this state, when to our great joy the driver informed us that all danger was passed. The cloud was now below us; it was still the seat of lightning and thunder, but our uneasiness ceased, for we enjoyed a clear sky and the brightest sunshine."

Captains Peytier and Hossard, of whom I have already made honourable mention, were on the Pyrenees in the midst of clouds forming the focus of a manifest thunderstorm, on the following occasions.

On the summit of the Peak of Anie, at a height of 7,215 feet, on the 15th of June, 1825, and on the 20th, 24th, and 25th of July, 1827.

The storm of the 15th of June lasted six hours; the hair of the observers and the tassels of their caps stood on end; a hissing sound was heard round every prominent point of their persons.

On the summit of the Peak Lestibète, at a height of 6,073 feet, on the 4th, 5th, 6th, and 13th of July, 1826.

During the storm of the 13th, there fell star-shaped hailstones nearly an inch and two-tenths in diameter.

On the mountain of Troumouse, at a height of 10,125 feet, on the 9th and 13th of August, 1826.

The storm of the 9th lasted twenty-four hours; it hailed and rained, and the thunder-peals were very frequent. The tent, although made of three folds of very tight and strong jean, sometimes looked as if it were all on fire. Captain Hossard's loaded fowling-piece, left outside the tent for the sake of precaution, showed on the morrow several evident traces of fusion at the extremity of the barrel. Seen from the valley this storm appeared so violent that the inhabitants of Héas were alarmed for the safety of the two officers and their guides.

On the Peak of Baletous, at a height of 10,320 feet, on the 25th, 30th, and 31st of August, 1826.

Rain, hail, snow, exceedingly vivid lightnings instantly followed by thunder. On the 31st lightning struck a ptarmigan which the guides had suspended by a string to a pole. The end of the wooden pole was charred; the feathers had been torn from the bird in a stripe from the head to the tail. Seen from the village of Arrens the storm had appeared so violent that the inhabitants did not expect the observers to return from the Pic de Baletous.

§ 6. *Are persons struck before they see lightning?*

I doubt whether a few years ago any physicist would have ventured publicly to propose the above question. It was then supposed that nothing could be more rapid than light. A well ascertained velocity of 80,000 leagues per second seemed sufficiently astonishing for the imagination not to seek for anything beyond it. But Mr. Wheatstone's experiments might reasonably change these dispositions. They have, I will not say demonstrated, but at least indicated, the possibility of velocities much more considerable than that of light, and this in that electric matter, the identity of which with the matter of lightning so many comparisons tend to establish. The question enounced in this heading is therefore deserving, in a theoretical point of view, of an examination, in which meteorology could but gain. I also believe that the problem has some points of contact with physiology; and, lastly, I have thought that many timid persons might be in part relieved from the distressing state of mind which takes possession of them during thunderstorms, by its being proved to them that the danger they fear is already past when the lightning appears.

A farmer, in Cornwall, Thomas Oliver, who was thrown to the ground senseless by a terrible stroke of thunder on the 20th of December, 1752, had been so far from either hearing the thunder or seeing the lightning, that when he came to himself, at the end of a quarter of an hour, his first thought was to ask who had knocked him down.

A man was struck by a thunderbolt near Bitche, on the 11th of June, 1757. After his recovery from a long swoon, the Abbé Chappe asked him what he had perceived; he answered, "I heard nothing and I saw nothing."

The Rev. Anthony Williams, rector of St. Kevern, Cornwall, was struck by the thunderbolt which, on the 18th of February, 1770, damaged his church, as before described; on returning to himself, after a long swoon, he declared he had neither seen lightning nor heard thunder.

Mr. Howard questioned the survivor of two gardeners, who had been thrown senseless to the ground by a thunderbolt, in 1807, at a country house, near Manchester. The man (George

Bradbury) declared positively that he had neither heard the thunder nor seen the flash of lightning at the moment of the accident.

On the 11th of July, 1819, a thunderbolt fell, as before stated, on the church of Chateau-Neuf-les-Moutiers, in the département des Basses Alpes. Nine persons were killed and eighty-two hurt. The curate of Moutiers was among the latter. He was taken up completely asphyxiated; his surplice was on fire; when he recovered consciousness, two hours after the accident, he declared he had "known nothing as to what had happened."

Mr. Rockwell, who was struck by lightning in August, 1821, had neither seen the lightning nor heard the thunder.

A man of the name of Reeves was at work on repairs to Salisbury spire in June, 1829; he fell senseless from the effects of a violent thunderstroke. When recovered from a long swoon, he declared that when he fell he had not seen the flash of lightning.

CHAP. XL.

ON DANGERS CAUSED BY WIRES OF ELECTRIC TELEGRAPHS.

It sometimes happens that posts supporting the wires of an electric telegraph are struck by lightning and the wood torn in the usual manner, the wires remaining intact and supporting the upper portions of the posts. Sometimes several adjacent posts are struck at the same time; in other cases the posts struck have intermediately between them others which remain uninjured. These facts, which have been perfectly well authenticated, have caused it to be supposed that the wires of electric telegraphs increase the danger of being struck by lightning to persons passing at a short distance from them.

Mr. Henry, of the United States, has tried to connect these phenomena with the known laws of electricity. Even in fine weather the parts of the wire which are at different heights, being in different states, currents will be caused from the higher to the lower part of the wire.

An analogous current will be produced when a precipitation of moisture takes place more at one extremity of the wire than at the other, and it will easily be originated by thunder showers or snow.

Induction may be supposed to be a still more frequent and usual cause of electric currents, proceeding from a cloud which is moving in the air nearly parallel to the line of wire; it must even be admitted that this cause (*i.e.* induction) produces currents along the iron rails of the railroad itself. Mr. Henry, indeed, relates that under favourable circumstances he had himself seen electric sparks show themselves in the interstices between two contiguous rails.

When during thunderstorms it may be deemed desirable to take precautions against the apparatus for signals being interfered with by induced currents, or to guard the operators against strong sparks proceeding from a thick wire, a very thin wire is substituted for the thick one.

During thunderstorms very small birds have been seen suspended by their claws to the wires of the electric telegraph on which they had alighted. As to the larger birds which are sometimes seen strewed on the ground along the wire, their death is not to be attributed to electricity, but rather to their having struck against the wire from not having perceived it.

It may be prudent during thunderstorms to avoid any very near proximity to the wire of the electric telegraph, as the only sure means of escaping the shock of sparks which may depend, as we have just said, on phenomena of induction.

CHAP. XLI.

ON VARIOUS MEANS WHICH HAVE BEEN FORMERLY ADOPTED AS SUPPOSED PRESERVATIVES OF EDIFICES FROM THUNDER.

§ 1. *Ancient supposed means of preservation of buildings from thunderbolts.*

COLUMELLA relates that Tarchon considered that he had completely protected his dwelling from thunderbolts by surrounding it with white grape vines.

Nearly two thousand years of subsequent experience have taught us nothing which could justify this supposition.*

In the 15th century, a naked sword was placed at the mast-head of ships to turn aside lightning from them. St. Bernardine of Sienna, who has been the means of transmitting the knowledge of this custom, speaks of it as a prejudice. (Laboissière, *Academie du Gard*, 1822.)

We shall show presently what is necessary to be added to the sword in order that it may produce any good effect.

Other circumstances being equal, lightning strikes the most elevated points by preference. From this incontestable fact, it has been inferred that any object is always protected by a higher object in its neighbourhood; thus, for example, that a house near church towers or steeples runs no risk from lightning; but in drawing such an inference, it is forgotten that specific circumstances, whether visible or concealed, may compensate, and more than compensate, the influence of superior elevation. Facts support this objection.

On the 15th of March, 1773, lightning fell at Naples on the house inhabited by Lord Tilney, although the house was looked down upon from every direction, at four or five hundred paces distance, by the cupolas and towers of numerous churches. It should be added, that these cupolas and towers were at the time wet from heavy rain.

Many cases might be cited of men killed by a thunderbolt when standing close to hay-ricks and stacks of wheat-sheaves two or three times as tall as themselves, which yet were not touched.†

§ 2. *Is it true that trees rising above a house, at a small distance from it, protect it completely from being struck by lightning, as many physicists affirm?*

According to the evidence of those who purchase the growing

* In the south of Europe, and especially in Italy, when cultivators see a vine branch on which the leaves and the fruit are completely withered, they do not fail to regard it as the effect of lightning.

† "Thunderstones" were formerly regarded as a preservative from the destructive effects of lightning. It was said to be sufficient at the beginning of a storm to strike such a stone three times against each of the walls of a house to render it quite secure from injury by lightning. We should not have to go very far to find this absurd practice still in credit with some persons. Any prejudice which allies itself with fear rarely fails to have a long term of existence.

timber on extensive tracts of forest, in order to fell it and employ it in wheelwrights' and joiners' work, trees are much oftener struck by lightning than is usually imagined. In sawing and working up the wood, numerous splits and fissures are found, which had evidently been caused originally by a stroke of lightning.

This observation falls in with a remark which M. de Tristan deduced from the observation of sixty-four distinct thunderstorms accompanied by hail, which in the space of twenty-six years (from the 1st of January, 1811, to the 1st of January, 1827) occasioned great damage in different parts of the département du Loiret, adjacent to the Forest of Orleans. M. de Tristan's remark was to the effect that a thunderstorm has its intensity very sensibly diminished in passing over an extensive forest.

According to these observations, it would appear certain that trees draw from storm-clouds a considerable part of the fulminating matter with which they are charged. Trees may, therefore, be regarded as being a means of attenuating the severity of thunder-strokes, but it is going beyond the limits of observation to invest them with an absolute preserving power. I subjoin a few facts to show how well-founded are my doubts on this point.

On the 2nd of September, 1816, lightning fell, at Conway, in Massachusetts, on the house of Mr. John Williams, and damaged it very greatly. Yet there were in its vicinity Lombardy poplars, of from sixty to eighty feet in height, the tops of which were thirty or forty feet above the roof of the building. One of these poplars was only six feet from the place where the lightning penetrated the masonry. None of these trees were struck.

If another proof of the inefficacy of trees as protectors from lightning be desired, I would cite the circumstances attendant on the lightning which, on the 17th of August, 1789, struck the house of Mr. Thomas Leiper, near Chester, in the United States. I take them from a notice published in 1790 by the celebrated David Rittenhouse.

Mr. Leiper's house is placed at the foot of a rather steep rise, so that to the west the ground is higher than the top of the house at little more than sixty feet from it. Moreover, there is on this rising ground an avenue of fine oak trees. The thunderstorm was from the west, and must, therefore, before it reached the house, have passed over trees much higher than any part of the roof or the chimney-tops. Yet the trees were untouched, and the house was struck.*

CHAP. XLII.

ON MEANS BY WHICH IT WAS IMAGINED OR PRETENDED THAT ENTIRE TOWNS, AND EVEN EXTENSIVE DISTRICTS, COULD BE PRESERVED FROM STROKES OF LIGHTNING.

§ 1. *In early Times.*

CTESIAS of Cnidos, one of the companions of Xenophon, relates, in a passage which has been preserved to us by Photius, that he had received two swords, one from the hands of Parisatis, mother of Artaxerxes, the other from those of the king himself; he adds:—"If these swords are planted in the ground with the point uppermost, they drive away clouds, hail, and thunderstorms. The king," he goes on to say, "made the experiment in my presence, at his risk and peril."

This is, no doubt, a very curious passage; but does it really deserve all the importance which has been attached to it? It is now perfectly established, that, I do not say a short sword merely, but even that a long, pointed metallic rod, erected on the top of a building, does not drive away *clouds*. It cannot be doubted, therefore, that the opinion expressed by the Persians on that point was an erroneous one,—at least that it must evidently have been destitute of proof; this being once admitted, ought we not to suppose that the physician of Artaxerxes re-echoed another rash and unfounded conjecture, when he attributed to his sword the additional property of driving away *thunderstorms?* At any rate,—and it would not be the first time that even a true statement has suffered from a bad neighbourhood,—need we be surprised that the

* This apparent anomaly admits of a satisfactory explanation from theory; for the tree-covered hill is an arid rock with very few inches of soil, while the house was almost surrounded by water, was armed with two lightning conductors and their accessories, and had several metal rain-pipes running from the roof to the foundations.

supposed experiment of the two sword blades should have been long overlooked, when we find in the same chapter in which it is recorded, that Ctesias also mentioned, with the same degree of positiveness, a fountain sixteen cubits in circumference, and of great depth, which every year became filled with liquid gold? adding, that a hundred pitchers were every year filled with gold from it; earthen pitchers being used, in order that when the gold had become solid, the pitcher might be broken, to take it out?

In the time of Charlemagne, it was usual to put up long poles in the fields to drive away hail and thunderstorms; but lest fanatical admirers of old times should see in this practice an anticipation of Franklin's lightning conductors, I hasten to add that the poles were not to be efficacious unless they were surmounted by pieces of paper. Such papers or parchments were no doubt covered with magic characters, since Charlemagne in proscribing this practice, in a capitulary of 789, described it as superstitious.

§ 2. *Effect of large fires kindled in the open air.*

Certain physical experiments led to the idea being entertained that large fires would take away from the clouds the greater part of the fulminating matter with which they were charged. It was thought (and it was Volta's opinion) that such fires would thus become a means of preventing thunderstorms, or of rendering them less formidable. Let us see how far observation has supported these conjectures.

I set aside altogether the strange idea that the sacrifices in the open air made by the ancients, with their flaming altars and black columns of smoke rising from the bodies of the victims,—in short, all those ceremonies, supposed by the vulgar to be designed to propitiate and disarm the thunder-bearing Jove,—were simple physical experiments, of which the priests alone possessed the secret, and which had for their real object the gradual diminution, or even the complete subdual of thunderstorms, by purely physical means. I pass to less fabulous matters. I am indebted to Mr. Matteucci's kindness for the following account.

There is near Cesena, in Romagna, a parish, throughout the extent of which, for seven miles round, the peasants, by the Curé's advice, place at about every fifty feet heaps of straw and brushwood, which they set on fire when a storm is seen approaching. This practice has been in force for three years, during which time the parish has not suffered either from thunderstorms or from hail, although it formerly suffered much every year from hail, and the neighbouring parishes have done so during the last three years.

Three years are not a sufficiently long period of time to allow of any definitive conclusion as to the preserving influence of large fires. The experiment is being continued, and the public will not fail to be informed of the results.

When, in my Eloge on Volta, I recalled the ideas of that illustrious physicist on the possible advantage which might be found in large fires during thunderstorms, I imagined that some encouraging inferences might be obtained, by comparing the meteorological observations in those counties of England where so many tall chimneys and furnaces send forth torrents of flame day and night, with similar observations in the adjoining agricultural counties.

As I have mentioned in p.116, *et seq.*, such a comparison has been made between agricultural and mining districts, and the latter show a sensibly smaller number of thunderstorms. I do not, however, think that this decides the question. Tall chimneys abound in England wherever there are mines; and thus the greater rarity of storms in such localities may as well be attributed to the mineralogical character of the ground, as to the presence of the great fires required for the treatment of the ores. In 1831, when I drew up the Eloge of Volta, I had omitted to consider one of the aspects of the difficulty.

In the cases of which I have been speaking, the question regarded the simultaneous effect of a great many fires. As to a single fire, however considerable, we are, I think, able to show that it does not deprive the nearest clouds, those which are directly above it, of their fulminating matter.

I would recall, the 1st of July, 1810, the end of the Rue du Mont-Blanc, the Hotel Montesson, then occupied by the Prince of Schwartzenberg, and the fête given by the Austrian Embassy to Napoleon and Marie Louise. In the middle of the night the vast ball-room became on fire. The immense columns of flame, which the firemen could not subdue, did not prevent a dreadful thunderstorm taking place at the end of the night. The flashes of lightning followed each other with terrible rapidity; the whole sky appeared on fire; thunder was unintermitting; at last torrents of rain fell and quenched the remains of the conflagration.

§ 3. *On the discharge of cannon as a means of dissipating thunderstorms.*

Nautical men seem to entertain rather generally a belief that the noise of artillery disperses thunderclouds, and, indeed, clouds of all kinds; but they cite few authentic facts in support of their opinion. The most distinct statement which I have been able to find on a subject so well deserving of being studied, is contained in the *Mémoires du Comte de Forbin,* first published in 1729, but the date referred to is 1680.

This intrepid sailor writes, "During the stay which we made on these coasts (near Carthagena in South America), there formed daily, about four in the afternoon, storms with lightning and dreadful thunder, and which always did some damage in the town on which they burst. The Comte d'Estrées, to whom these coasts were not unknown, and who, in his different voyages to America had been more than once exposed to these sorts of hurricanes, had discovered the secret of dispersing them by firing guns. He made use of his ordinary remedy against the storms I now speak of; and the Spaniards having perceived this, and having remarked that after the second or third discharge the storm was entirely dissipated, were struck by the prodigy, and not knowing to what to attribute it, showed surprise mingled with fear," &c. &c.

In the present day agriculturalists in some districts, being encouraged by the opinions of military men, have recourse to the firing of cannon when they think themselves threatened with thunderstorms, and especially hailstorms. At what epoch did this practice originate? I cannot tell exactly, but everything inclines me to suppose it not very ancient. In the first "Encyclopedia," the publication of which goes back to 1760, I find in the article *Orage,* by M. de Jaucourt, "We have heard more than once from our military men that the noise of cannon dissipates storms, and that hail is unknown in besieged places. This effect does not seem out of all probability, and after all, what would be risked by a trial? Only the cost of some hundred weight of gunpowder, and of the removal of some pieces of ordnance, which would not have their value diminished by having been so employed. Perhaps by means of the kind of undulation excited in the atmosphere by the discharge of several cannon fired in succession, the clouds beginning to form and ferment might be shaken and dissipated."

This passage evidently shows that the use of artillery for dispersing storms had not then become a practice, and that it was recommended as an important subject for experiment; but in 1769, a further step in advance had been made. I find in vol. viii. of the *Histoire de l'Air et des Méteores,* that in May, 1769, the county of Chamb, in Bavaria, had suffered from violent storms, and that much damage had been done "excepting where the inhabitants had introduced the practice of making frequent discharges of small cannon on hearing the first claps of thunder."

It was about the same period (1769), that the Marquis de Chevriers, a retired naval officer, living on his estate of Vaurenard, in the Maconnais, thought he would combat the scourge of hailstones by the method which he believed he had seen successful against storm-clouds at sea, *i. e.*, by firing ordnance. He consumed annually in this way two or three hundred weight of mining powder.

The Marquis de Chevriers died at the beginning of the Revolution; but the inhabitants of his Commune were so convinced of the good effects of his method of proceeding that they continued to follow it. I find in a memoir drawn up at the different places themselves, by M. Leschevin, Commissaire en Chef des poudres et salpêtres, that in 1806 mortars or cannons were used in the communes of Vaurenard, Iger, Azé, Romanèche, Julnat, Torrins, Pouilly, Fleury, Saint Sorlin, Viviers, des Bouteaux, &c. The commune of Fleury used a mortar which was charged with upwards of a pound weight of powder at once; others employed smaller mortars of various sizes; the discharges were usually made on heights. The consumption of mining powder for this sole purpose amounted annually to eight or ten hundred weight.

Nor was the proceeding confined to the Maconnais. Not long since a mayor, not far from Blois, told me that in his commune, mortars were fired when storms were seen to be approaching, and he wished to know whether science justified the practice; which question, it may be said, by the bye, did not seem to indicate that experience had fully demonstrated its efficacy.

Thus, the only actual observation in favour of this method is the one above mentioned on the coasts near Carthagena; but in matters of meteorology the experience of a few days does not appear a sufficient ground for any general conclusions. While examining my recollections to discover among them any fact which might come in aid of that related by Forbin, I found one which has precisely the opposite tendency; it curiously happens that it also comes from an admiral of the time of Louis Quartorze, and from the eastern coasts of South America.

Let us revert in imagination to the month of September, 1711, and the squadron under Duguay-Trouin in sight of Rio-Janeiro. This squadron (consisting of the ships of the line le Lys, le Magnanime, le Brillant, l'Achille, le Glorieux, and le Mars, the frigates Argonaute, Amazone, Bellone, Aigle, and several vessels of smaller dimensions), employed the whole day of the 12th in forcing the entrance of the harbour, defended by the formidable artillery of a great number of forts, and by that of four ships of the line and three frigates. From the 12th to the 29th a continued combat of musquetry and artillery was kept up day and night; shells were thrown, several mines were exploded by the Portuguese; they blew up several of their own ships; magazines were set fire to, &c. &c. Lastly, on the 20th, the day when the place was taken, two of Duguay-Trouin's ships "le Brillant," and "le Mars," the battery of the Isle des Chévres, consisting of five mortars and eighteen twenty-four pounders, kept up an incessant fire which destroyed part of the intrenchments of the town; at night the signal given by the commander was obeyed by a general fire from the batteries and the ships; and yet all this did not prevent the bursting of a storm, accompanied by incessant peals of tremendous thunder.

The above constitutes what may be termed an experiment combining all the conditions desirable for its success, and yet these many thousand discharges, far more powerful than all the small artillery employed in the Maconnais, did not prevent the storm from forming, or disperse it after it had formed.

If, on the one hand, a single fact (that which I borrowed from Forbin), seems insufficient to show that discharges of artillery may dissipate thunderstorms, so also the single fact which I have taken from the memoirs of Duguay-Trouin, may be deemed insufficient to prove the opposite thesis. Any one having at hand detailed annals of the last wars, would find in them a multitude of documents suited to elucidate the question we are discussing. I will mention two which occur to me, hoping they may induce others to make analogous citations.

The 25th of August, 1806, was the day chosen for the attack of the island and fortress of Dannholm, near Stralsund; General Fririon, in order to occupy and fatigue the Swedish garrison, had the fortress cannonaded throughout the day. Notwithstanding this continued and active discharge of artillery, a violent thunderstorm burst about nine in the evening.

By a singular coincidence, "the Duke," an English ninety gun ship, was struck by lightning in 1793, while exchanging cannon shots with a battery on the island of Martinique.

Finally, on this subject, I subjoin the result of a little examination which I have made, and which in the absence of more direct experiments may not appear entirely devoid of interest.

There is in the Bois de Vincennes, about 3,280 yards from the Observatory at Paris, a polygon where artillery practice is carried on during certain months of the year. It is armed with eight siege guns for direct fire, four siege guns for ricochet fire, and six mortars; and lastly, a moveable battery of six guns. Practice takes place on certain days in the week, from seven to ten in the morning. The firing each day is about 150 discharges. As the sound of the firing is still very considerable at the Observatory, it seemed to me that if its influence on the atmosphere is what so many persons have supposed, the days of practice ought to be less clouded than the other days of the week. I have submitted this idea to minute examination.

General Duchan, Commandant of the School of Vincennes, kindly furnished me at my request with a return of the days on which artillery practice had taken place, from 1816 to 1835. The entire number of days amounted to 662.

The meteorological registers of the Observatory gave me for each of these days the state of the sky at nine in the morning. Out of the 662 days there were 158 on which the sky at nine o'clock was completely clouded. If the firing had not taken place would this latter number have been more considerable?

It seemed to me that the most unexceptionable proceeding would be to examine similarly the state of the sky on the days immediately preceding and succeeding the days of practice, and to regard the mean of the two numbers so obtained as the normal meteorological state corresponding to the days of practice. I mean by normal in this case, their state entirely disengaged from any possible influence of the firing. The results I obtained were: —

Out of 662 days preceding the days of practice, 128 were clouded.

Out of 662 days of practice, 158 days were clouded.

Out of 662 days following the days of practice, 146 were clouded.

The mean of 128 and 146, or 137, is so much *lower* than 158, that we might be inclined to infer that, instead of dissipating and driving away the clouds, the noise of the firing condensed and retained them; but I am well aware that the numbers on which I have operated are not sufficiently considerable to justify my going so far as to draw such an inference. I will confine myself to saying that the discharge of heavy artillery does not appear to have any influence in lessening the formation of ordinary clouds.

We have here, then, another problem, which will require further inquiry. I take the liberty of recommending such inquiries to the care of general officers commanding our schools of artillery. Observations on the state of the sky made on the spot while the firing is going on will be particularly valuable; those made one or two leagues off will not satisfy every one, since it may be apprehended that possibly at a little distance the sky may have become exceptionally overcast, by the driving back of the clouds, which without the firing would have remained in the zenith of the place where the guns were stationed. It will be always indispensable to add to the observations made on the days of practice similar ones on the preceding and following days, made very exactly at the same hours. By merely noting the variations taking place during the time the practice lasts, there would evidently be a risk of attributing to the influence of the firing that change in the state of the sky which shows itself almost every morning as the sun rises higher in the heavens.*

* Of the 662 days of artillery practice at Vincennes, and the preceding and following days, the number of perfectly clear days was —

Days before those of practice	83
Days of practice	84
Days after those of practice	80

CHAP. XLIII.

IS IT USEFUL, OR IS IT DANGEROUS, TO RING CHURCH BELLS DURING THUNDERSTORMS?

I SHALL examine this important question without allowing myself to be swayed by the positive decisions of different scientific, administrative, or judicial bodies*; but also without any disposition to think that opinions generally entertained may not be well founded.

It is but a step from the opinion we have just been discussing, according to which the noise of artillery should break up and dispel clouds, rapidly transforming the most clouded sky into one of clear azure,—to the supposition that similar effects may result from the prolonged resonance of large bells. But has this been, in fact, the succession of ideas which has led to bells being tolled with the hope of dispersing thunderstorms? I would the less venture to affirm this, because some learned person may perhaps discover that the practice of bell-ringing during thunder was anterior to the invention of gunpowder. I believe it will be more consistent with truth to look for the origin of the practice in a different connection.

Church bells are blessed with great pomp before being suspended in their places; I subjoin some extracts from the prayers with which, according to the Paris ritual, our churches resound during those ceremonies:—

"Bless . . . ., and whenever it rings may it drive far off the malign influences of evil spirits, whirlwinds, thunderbolts, and the devastations which they cause, the calamities of hurricanes and tempests." "May there thus be driven afar the snares of the enemy, the violence of hail, tempests, whirlwinds,

* In 1747 the Academy of Sciences itself considered it dangerous "to ring large bells, or to excite any other violent agitation in the atmosphere when a thunderstorm is overhead."—*Histoire de l'Academie*, 1747, p. 52.

A legislative decree, dated 21st of May, 1784, sanctioned an "ordonnance," made in the bailliage of Langres, which expressly forbade the ringing of church bells during thunder. Two years before, the same had been forbidden in the Palatinate by the Elector Charles Theodore. I might also cite several orders forbidding the practice throughout the extent of several dioceses.

and hurricanes; may disastrous thunderstorms lose their fury." "May the sound of this bell put to flight the fiery darts of the enemy of man; the ravages of thunder and lightning, the rapid fall of stones, the disasters of tempests," &c.

But, besides this, may there not be a second cause, scarcely less powerful, in the desire which men feel when frightened to divert their fears by noise? Look at a poltroon in the dark, he sings. Look at a town a prey to the horrors of civil war, the tocsin is rung much longer than would have been necessary as a signal or alarm. Savage nations in all parts of the earth send forth deafening clamours to terminate the eclipse of the sun, or moon, which terrifies them.*

In regard to the disadvantages of bell-ringing, the strongest apparent case derived from fact is probably the following, which I take from an old volume of the *Mémoires de l'Academie des Sciences.* During the night from the 14th to the 15th of April, 1718, in the space comprised between Landerneau and St. Pol de Leon, in Brittany, twenty-four churches were struck by lightning, and, said Fontenelle, precisely those churches in which the bells were rung to drive away the thunder and lightning. M. Deslandes, who transmitted the details to the Academy, added, "Neighbouring churches in which the bells were not rung were spared."

This observation, however, is too laconically given. Thunderstorms sometimes ravage long and very narrow strips of

* In thus treating noise as a sort of general panacea, a singular discovery has been arrived at, which, notwithstanding its little connection with my subject, I permit myself to mention here without scruple, regarding the possible utility of the notice as a sufficient apology for its introduction.

Thomas Gage relates, in his Travels, that in some parts of America loud noises were had recourse to as a means of driving away a scourge, less formidable in appearance than thunder and lightning, but in reality much more destructive.

Towards the middle of the last century Gage was at Mexico, in the district of Guatimala, when a thick cloud of locusts descended on the country, threatening it with complete devastation. Instead of employing against these insects the complicated and not very efficient means sometimes resorted to in the south of France, the magistrates desired all the inhabitants to take up drums, horns, trumpets, &c.: they did so, and advanced towards the invaders, making the loudest possible noise with all their various instruments, with such success, that the locusts took flight, and were driven into the Pacific, where they perished!

This method is also employed in Wallachia, Moldavia, and Transylvania. (Phil. Trans. 1749.) I have an account of a comparatively recent instance of the practice in Bessarabia.

country; may not this have been the case in Brittany? Were not the churches which were spared out of the line of direction followed by the storm-clouds? Moreover, in those churches where the bells were rung, the death or serious injuries suffered by the ringers attested in the most unequivocal manner the fact of the fall of lightning. Elsewhere, might not slight injuries to walls, or falls of plaster, very naturally not have been remarked? Further, what were the comparative heights of the steeples which were struck and those which escaped? &c. &c.

Where all these uncertainties exist it must be admitted that M. Deslandes' observation is wanting in the characters requisite for a real demonstration, and the inference drawn from it can only be accepted and registered by science simply as a probability.*

In August, 1769, the custom of beginning to ring the bells when thunder is heard was strongly argued against from the fact of the steeple of Passy, where ringing had been continued incessantly, having been struck by lightning; but on more complete inquiry it appeared, that during the long time for which the storm lasted, the bells had been rung equally perseveringly at Auteuil and Chaillot, and yet that these two steeples, which are on either side of that of Passy, sustained no damage.†

* The numerous and serious disasters on the occasion of the 15th of April, 1718, by no means injured the reputation of bell-ringing in the minds of the people of Lower Brittany; the 15th of April that year was Good Friday, and as those who rung the bells on that day infringed by so doing one of the precepts of the Church, it was not thought surprising that misfortunes followed.

† In 1781, the Abbé Needham, of Brussels, considered that he had proved, by cabinet experiments, that bell-ringing has absolutely no effect, good or bad.

He had a wooden model of a steeple made 1 metre high; he placed in it a bell of 15 centimetres diameter, with an arrangement for putting it in motion when desired. At the top of the steeple there was a metallic ball having a properly established communication with the ground. This ball was placed opposite to the perfectly similar ball of the conductor of an electric battery charged to saturation. When the bell did not ring, the striking distance, or distance at which the spark darted from the ball of the conductor to the ball of the steeple, was 7 millimetres, or 0·2756 of an English inch. The balls were then placed at double the distance apart, and, although the bell was then made to ring strongly and quickly, no spark or escape of electric matter was perceptible between the two balls.

Mr. Needham's remark in conclusion is, that he "regards this experiment

To sum up the results of what has been said.

In the present state of our knowledge, it is not proved that the sound of bells tends to call down thunderbolts, *i.e.* that it has ever caused lightning to strike buildings which would otherwise have escaped.

But it is, nevertheless, earnestly to be recommended, for the sake of the ringers, not to ring the bells. They incur danger proportionate to that of those who imprudently take shelter under tall trees during thunderstorms. Lightning strikes elevated objects, and, in particular, the summits of spires and steeples; the hempen cord attached to the bell, and ordinarily saturated with moisture, conducts the discharge down to the hand of the ringer, and hence so many deplorable accidents have arisen.* Let us remark, that if the cord, whether wet or dry, does not go down the whole way to the ground, as is ordinarily the case, it might very well happen, that if no one were near, the fulminating matter, after reaching the ring at its lower extremity, might return back on its path, remount to the summit of the steeple, and be dissipated in space. According to this view, the absence of damage in the interior of a tower or belfry would not authorise the conclusion, that if ringers had been present they would not have suffered.

as decisive." Let us see, however, whether it does not still leave room for some doubts.

Having operated successively with the two balls at 7 and 14 millimetres apart, Mr. Needham had a perfect right to infer from his results, that the sound of the bell did not augment to any considerable degree the facility of electric discharges: that it did not *double* the "striking distance;" but I think, that to have entitled him to affirm that the noise had absolutely no effect, it would have been needful that he should have increased the distance from 7 to 14 millimetres, not, as he did, at once, but gradually.

The small electrified masses which Mr. Needham placed in presence of each other were both solid bodies. In the atmosphere we see, on the contrary, floating clouds which might have their forms so modified by vibrations of the atmosphere, as to cause a sensible change in the electric tension of the face looking towards the earth. Mr. Needham's experiment, in its possible application to the effects of bell-ringing during thunderstorms, would have been very valuable if it had given a positive result. The negative reply afforded by it has, I think, little or no meteorological value.

* In the view of adding to the warnings against the dangers with which this unwise practice is fraught, I subjoin the notice of another accident similar to those cited in Chap. xxxviii.

On the 31st of March, 1768, lightning having struck the steeple of Chabeuil, near Valence, in Dauphiné, killed two of the young men who had assembled there to ring the bells, and severely wounded nine others.

My readers will have remarked the reserve, as regards the general effects, with which I have treated the question of the real or imaginary utility of the practice of ringing the bells during thunderstorms; and they may be surprised at the assurance with which some of the administrative authorities venture to express themselves on the subject. In an order, given by the Préfet de la Dordogne, 1st of July, 1844, I read: "that the opinion, according to which the sound of large bells would have the property of averting strokes of lightning, or of paralysing their effects, is founded solely on superstition, and that such a practice must, instead, *infallibly occasion strokes of lightning*. . . ." We see by this passage that false science is as dangerous as entire ignorance, and that it *infallibly* conducts to inferences which have nothing to justify them.

CHAP. XLIV.

ON MODERN LIGHTNING CONDUCTORS.

Having passed in review a long list of means by which men have successively hoped to protect themselves from the danger of lightning, we will now turn our attention to the lightning conductors of our own time, those "paratonnerres" of which Franklin conceived the idea, and of the efficiency of which, whatever may have been said, there appears no reason to doubt. We propose to show this efficiency, both by reasoning and by the evidence of facts, without, for the present at least, borrowing anything from modern theories of electricity.

All other circumstances being equal, lightning directs itself, generally speaking, by preference to the most elevated portions of edifices. It is there, consequently, that the preservative means, whatever they may be, should be applied.

All other circumstances being equal, lightning directs itself by preference to metals. When, therefore, a metallic mass occupies the most elevated point of a house, we may feel pretty nearly certain that lightning, if it falls, will strike that point.

Lightning which has entered a metallic mass only does mischief to surrounding masses at the moment when it quits the metal, and in the vicinity of the point or points at which it issues from it. A house may, therefore, be rendered safe, from its highest point to its foundations, if the metallic parts of the roof are prolonged without interruption to the ground.

Damp earth offers to the fulminating matter which is passing along a metallic bar a channel by which it escapes easily and without effort, without detonation, and without producing any kind of damage, providing the bar plunges a little deeply into the earth. By carrying the continuous bar which has already preserved from injury the exterior of a building, down some way into ground which is always damp, the foundation, and, speaking generally, the whole subterranean portion of the building, will be similarly preserved.

When there are upon the roof or summit of an edifice, several distinct metallic masses completely separated from each other, it is difficult, and even impossible, to say which of them will be struck by preference, for the point from which the storm-clouds come, the direction and rapidity of their advance, are probably far from being without influence. The only safe practice is to unite all these various metallic masses by rods of iron or copper, or bands of lead, zinc, &c., so that there may not be any one of them which shall not be, if I may use the expression, in metallic communication with the bar which is destined to transmit the fulminating matter or lightning to the damp earth, and which runs down one of the upright walls of the building.

Thus we arrive by observation alone, without borrowing anything from theory, at a simple, uniform, and rational means of protecting buildings, great or small, from the effects of lightning. Every one must now comprehend the mode of action in this arrangement, or the office filled by the bar which goes down to the ground, and sinks, more or less, deeply into it; every one will understand why this bar has received the name of *conductor*.

Without quitting this subject, we will go back a little; but only to examine questions of quantity and of shape.

At what distance apart, or rather how near to each other, ought the metal plates distributed over the roof of a building to be, in order to make it certain that no intermediate point shall be struck by lightning? The question cannot be solved in an absolute manner. It is evident that the more extensive and massive the metal, the more extensive and intense will be its action. Only it may be affirmed, that if the desired connections are established between the sheets of lead, zinc, &c., which in carefully constructed buildings are almost always laid over the main beams, between the cramps and other metallic fastenings prepared for the slates or tiles, and between the gutters and rain-pipes, so that the whole of these pieces are duly connected with each other and with a proper conductor, all will have been done for protection against lightning which the most timid prudence could require.

By a "proper conductor," I mean, on the one hand, that it should go down into the ground until it reaches earth which is always damp; and, on the other, that it should be sufficiently massive to transmit the strongest lightning without being fused by it.

The opponents of lightning conductors have argued against them from our present ignorance (an ignorance which we must expect will long continue) as to the *maximum* effect which lightning may produce; and, therefore, as to the maximum of dimension which *may* be required for conductors. The difficulty, although founded on truth, really has in it nothing which need stop our proceedings. If we take the dimension to be given to conductors from experience, and if that which we adopt has been found to resist the strongest lightning recorded during three or four centuries, what can reasonably be asked more? When the engineer decides on the height and width of the arches of a bridge, the vault of an aqueduct, the section of a drain, &c., what does he concern himself with? He examines records as extensively as he can, and he keeps somewhat beyond the dimensions dictated by the greatest floods and heaviest rains which have ever been observed; he thus goes back as far as evidence will enable him to do, but without troubling himself with physical revolutions or cataclysms anterior to history, and of which geologists only have been able to find the traces and estimate the magnitude. Greater precaution or foresight than this is not demanded from the constructor of lightning conductors.

But the apparatus which we actually employ does not consist merely of conductors placed in immediate communication with metallic masses, which would at any rate have formed integral portions of the buildings as being required in their construction; the preserving metallic masses in which the conductor terminates are slender rods placed for this express purpose on the summits of buildings; and, moreover, usually terminating in very sharp points incapable of oxydation. Great advantages, which we will now proceed to render evident, result from these arrangements and particular forms.

Let us suppose one of the long, slender, pointed rods of which we have been speaking to be broken across, and that we are able to increase or contract at pleasure the interval between the two broken ends of the metal. During a thunderstorm this gap, or interruption of metallic continuity, becomes the seat of very curious phenomena.

Let the interval be only a few hundredths (less than a tenth) of an inch, and during the whole time that the thunder is rolling over head you will see the interval occupied by a slightly hissing flame. When the interval is increased to some tenths of an inch, or to upwards of an inch, the light will only pass from the upper to the lower end intermittingly; the continuous flame will be replaced by momentary jets; and instead of the slight hissing sound there will be heard discharges as loud as a pistol-shot.*

Of what does the matter which thus darts from the upper extremity of the gap in the conductor to its lower extremity consist?

Fulminating matter, or the matter of lightning, sometimes streams off without detonation, but producing continuous light (Castor and Pollux), whose apparition is only accompanied by a slight hissing sound; exactly the same thing takes place with the matter which streams across the gap, or hiatus, in the conductor.

Suppose a sudden emission of light, and there will be in the break of the conductor a detonation; just as when a thunderbolt bursts in the midst of clouds.

* If experiments purposely made had not long since attested the reality of these phenomena, they would have been discovered accidentally. Captain Winn, commanding an English frigate, remarked at the beginning of a thunderstorm, that the continuity of his lightning conductor was accidentally interrupted by a gap or interval of about one inch; as long as the storm lasted, or for about two hours and a half, this interval was marked with vivid and almost continual sparks.

Former meteorological treatises have mentioned the case of an English ship, in which the conductor being also interrupted, the crew saw with affright, for three consecutive hours, the space in which the metal was deficient occupied by a jet of flame.

Lightning fuses metals; the matter which passes along the conductor liquifies in the same manner the thin wires which it meets with on its passage.

The spark which emanates from the conductor transforms a mixture of oxygen and nitrogen into nitric acid; we have seen that lightning also in darting across the atmosphere produces this acid.

A stroke of lightning imparts magnetic poles to bars of steel, and strengthens, or destroys, or often inverts, the poles with which such bars had previously been endowed by being magnetised by some of the ordinary modes of proceeding; all this can be executed at pleasure by the use of the intermitting sparks of the conductor; the difference in the effects (augmenting the force of the poles, or reversing them) depending exclusively on the situation of the needle in reference to the spark.

Strokes of lightning kill men and animals; when the two ends of an interrupted conductor are far apart, when the spark has to be very long, and when it deviates in its course, then the danger is great to whoever is exposed to being struck by it; and when the lower part of the conductor is suppressed, it is especially great to whoever may be so placed as to run the risk, from his position, of becoming a substitute for the missing portion of the conductor.*

* It will not be inappropriate to give in this place a succinct description of the interrupted conductor by the side of which the celebrated physicist, Rickmann, was killed at St. Petersburgh on the 6th of August, 1753.

Imagine an ordinary glass bottle having the bottom perforated, with an iron rod passing through the aperture, and kept in its place by pieces of cork.

Insert the bottle upright in a hole made in the roof of a house, so arranged that the upper end of the rod may rise five feet above the surface of the roof, and that its opposite or lower extremity may be, as it were, suspended in the middle of the apartment directly under the roof.

To this lower extremity a metallic chain is attached.

This chain is prolonged down to the story in which the physicist's cabinet is situated: it is not carried down in a straight line, but is several times inflected as required by the conditions of the locality; without, however, being allowed to touch any part of the building, being, where necessary, kept from touching the walls by the interposition of plates of glass or thick layers of sealing wax.

Inside the cabinet the chain hangs vertically from the middle of the ceiling through an opening with glass sides.

All these arrangements, and particularly the use of insulating substances, are intended to produce, and did produce, the result of concentrating the

So many points of resemblance scarcely permit us to doubt that the luminous, hissing, detonating matter perceived in the interval between the two parts of the interrupted conductor,—that matter, I say, which is found capable of effecting fusions, producing chemical combinations, magnetising and unmagnetising steel needles, and killing men and animals,—is in reality no other than, or in no way different from, fulminating matter taken from storm-clouds by the intervention of the apparatus. Thus the lightning conductors which we now place on buildings as protections to those buildings against damage by lightning, have, in addition to their value in this respect, the property of gradually depriving the storm-clouds of the fulminating matter with which they are charged, and which the conductors carry off and convey silently and harmlessly into the ground.

If we suppose the fulminating matter accumulated in the clouds not to be susceptible of sudden reproduction, it will follow that lightning conductors must have an effect in diminishing the intensity of thunderstorms, and the number, violence, and severity of strokes of lightning.

I anticipate a difficulty which may possibly present itself to the minds of some persons who have not adequate notions on modern physics. They may remark that we have been speaking latterly of conductors in which the continuity was interrupted by gaps at certain points, and they may ask whether it is demonstrated that continuous conductors have also the privilege of charging themselves with the fulminating matter and transmitting it to the ground.

That such a question should be answered affirmatively there can be no doubt, but we cannot offer the same audible and visible proofs, because here all takes place silently, and without development of light. If, however, the supposed inquirer asks to be shown that during thunderstorms something of some kind really is passing along the continuous conductor, let him bring

fulminating matter in the apparatus, and of preventing it from escaping in any other way than by the conductor which Richmann employed, and which he brought from time to time near the extremity of the pendant chain from which he thus drew sparks.

On the 6th of August, 1753, while he was arranging his apparatus, a tongue of blue flame detached itself from the end of the chain, produced a discharge similar to the report of a pistol-shot, and darted straight to the face of the Professor at a distance which could not at the utmost exceed a foot. Richmann fell dead on the spot. The engraver, Sokolow, who was by his side, also fell to the ground, but, after a few moments of insensibility, revived.

near to it a small steel bar, or needle, held transversely, and he will find that it becomes magnetised, just as it would have done under the action of the sparks which occupy the gap in an interrupted conductor. Or if we diminish sufficiently the mass, or thickness, of the uninterrupted or continuous conductor, without, however, breaking it at any point, it will sometimes be surrounded throughout its entire length by hissing light. If the thunderstorm is very violent, this light may appear even without the ordinary thickness of the conductor having been diminished.

The English frigate the "Dryad"—being fitted with the new apparatus of Mr. Snow Harris, in which there is substituted for the ordinary conductor employed in ships an equal weight of thin copper cylinders, fitted to the the masts so as to become one piece with them,—was several times exposed on the African coast to those violent storms which navigators term tornadoes. The fulminating matter descended along these continuous copper tubes in such quantities as to produce a kind of luminous atmosphere, and a noise like that of water boiling very hard.

We have now arrived at a point at which we may study the influence of the height, shape, and position relatively to surrounding objects, of the iron rod forming the upper part of the lightning conductor. The measure of this influence will be found in the number of sparks traversing a given gap in the conductor, in given atmospheric circumstances, and in a given time.

The number of these sparks increases rapidly with the increase of the height of the rod; and, on the other hand, diminishes very rapidly if, while the height of the rod itself remains the same, it is equalled, or still more if it is exceeded, by that of any of the near surrounding objects; there cannot, therefore, be the least doubt as to the propriety of using very tall rods, and of placing them on culminating points of edifices, by which means their faculty of attenuating the intensity of thunderstorms will be most developed.

The influence of form appeared to be more difficult to determine. Some wished the rod to terminate in a ball, while others followed Franklin in extolling very acute points. The question will be elucidated by an experiment, which, by the by, I have never seen quoted.

In 1753, Beccaria erected, on the roof of San Giovanni di Dio, at Turin, an iron rod supported near its base by flying buttresses formed of substances which do not readily transmit lightning. At a small distance from the lower extremity of the iron rod, the part of the apparatus more especially called conductor commenced. The upper part of the rod carried a moveable metallic point, which was suceptible, by merely pulling a silken string, of being directed either towards the sky or towards the earth. With the point turned downwards, the apparatus did not give sparks: the point was then suddenly directed upwards to the sky, and in a few moments sparks appeared. The point was then again turned towards the earth, and no more sparks were seen.

Under some atmospheric circumstances there were sparks, whatever might be the position of the point; but even then it was easy to see that the sparks were stronger and more frequent when the point was turned upwards than when it was turned downwards.

This experiment, which it would be very useful to repeat, shows unequivocally that a pointed rod is much more effective than a blunt one in gradually drawing away from thunder-clouds the fulminating matter with which they are charged. It would seem that it ought to terminate decisively in favour of pointed conductors the debate which made so much noise about the middle of the last century, and in which the king of England took an active part, from aversion to Franklin.

There is here, also, a question as to quantity. Is the quantity of fulminating matter drawn from the clouds by pointed conductors considerable? May there result from this action a sensible mitigation of thunder-storms? Where there are many conductors, are strokes of lightning less formidable? I think that Beccaria's experiments have furnished me with the elements required for the elucidation of these doubts.

This skilful physicist had erected at Turin, on two points of the Valentino Palace very distant from each other, two thick and rigid metallic wires, which were kept in their place by the help of bodies of the class called by physicists insulating bodies. Each of these wires was at a very small distance from another metallic wire, which, instead of being insulated, was carried down the wall of the building to the ground, into which it sunk deeply. The first-named wires, therefore, formed the upper portions of "lightning conductors;" *i. e.* the portions which attracted the lightning from the clouds, and the wires which descended along the wall were the lower portions, or those destined to transmit it innocuously to the ground. These being the circumstances, during thunderstorms, vivid sparks—I might say lightnings of the first class—were darting incessantly between the upper insulated and the lower non-insulated wires. The eye and the ear were hardly able to perceive the intermittence: the eye did not detect any interruption in the light, and the ear heard a sound which was almost unbroken.

No physicist will contradict me when I say, that each spark, taken singly, would have given a shock attended with pain; that ten sparks united would have numbed a man's arm; and that a hundred would perhaps have given a fatal stroke. A hundred sparks passed in less than ten seconds: thus, every ten seconds there passed from the one wire to the other a quantity of fulminating matter sufficient to kill a man; in one minute, six times as much; and in an hour, therefore, 360 times as much. We are therefore entitled to say, that each of the lightning conductors on the Valentino Palace took from the clouds each hour a quantity of fulminating matter capable of killing 360 men. We said there were two such conductors: the above number should therefore be doubled; and we have thus sufficient lightning to kill 720 persons. But the palace was made up of seven pyramidal roofs, covered with sheets of metal communicating with metallic pipes which went down into the ground. The summits of these pyramids were pointed, and rose higher into the air than the extremities of the two wires with which Beccaria operated. We have every right to suppose that each pyramid drew from the clouds as much matter, at the very least, as the wires; and multiplying seven by 360, we have 2520, to which we must add the previous 720; giving a total of 3240. Taking, therefore, everything at the lowest; supposing the palace to act only by its points, and the whole rest of the building to remain absolutely inoperative, we still find that this one edifice takes from the clouds, in the short space of an hour, as much lightning matter as would have been sufficient to kill upwards of 3000 persons.

Some physicists, while they admit that lightning conductors are useful, and that they receive and transmit innocuously to the ground the strokes which must otherwise have done so much damage to the buildings, still deny any great benefit from their gradual and silent action. I think the figures which I have found ought to undeceive them. The point is, however, too important for us not to consider it under other points of view.

I have related the manner in which Richmann was killed. If, at the moment of the disaster, a stroke of lightning had darted from the storm clouds to the metallic rod on the roof, the event, as to the physical inferences to be derived from it, would belong to the very numerous class of cases in which men have been killed by lightning when placed near interrupted metallic bars,—*i. e.*, bars which were not in direct communication with the earth: but, in the case before us, everything shows that there was no external thunderbolt.* What took place was, that the bar, which rose only to a height of six feet above the roof of the house, the chain, and the lower rod, had silently become charged with fulminating matter, which they had drawn from the clouds, not suddenly, but by little and little; and the quantity thus drawn was sufficient to kill one man and cause another to fall senseless to the ground; to fuse some length of an iron rod, and to do notable damage in several rooms of the celebrated physicist's apartments.

I own that, in presence of these facts, I attach little value to the theoretical considerations, according to which some persons have treated as mere insignificant atoms the quantities of fulminating matter which our lightning-conductors are able to draw from the clouds. These atoms, if that name is to be given to them, would, at all events, be strong enough to force open doors, break or displace furniture, cause considerable damage to walls, and kill men.

If, say the dissentients, lightning conductors have the faculty of taking away from the clouds the fulminating matter with which they are charged, how is it that thunderstorms still burst over towns where these apparatuses abound?

The answer is easy: Lightning conductors do indeed draw to themselves part of the fulminating matter of the clouds, but

* In an account published by M. Lomonosow, some time after Richmann's death, mention is made of several of the neighbours having seen fiery darts proceeding from the clouds to the roof at the moment of the catastrophe. The reality of these observations might be contested; at all events, no one pretends to have seen and heard a true stroke of lightning with thunder.

no one pretends that they deprive the clouds of the whole. Such a belief would be the less justifiable, because storm-clouds appear to have a sort of intercommunity; so that usually the fulminating state of one of them (if I may be allowed to use the expression) cannot be changed without other clouds, even to great distances, being also affected. This important fact is shown in the following manner: —

Let us return to the lightning apparatus before described, with an interrupted conductor, and let us consider it attentively during a thunderstorm. Sparks of a certain degree of vividness are seen from time to time in the interrupted space or interval. Well, then, almost every occurrence of thunder and lightning, considerable or inconsiderable, near or distant, occasions a sudden alteration* in the number and vividness of the sparks. The moment of this alteration coincides almost exactly with the apparition of the flash of lightning; thus, if the thunder-cloud from which the lightning proceeded is very distant, the abatement of the sparks may precede by half, three quarters, or even a whole minute, or more, the moment when the sound of the thunder reaches the ear of the observer.

Toaldo speaks of a thunderstorm of the 28th of September, 1773, which extended simultaneously over the entire space comprised between Padua, Treviso, and Venice, and much beyond, which lasted more than six hours, and with such violence, that during the whole time the heavens, wherever it prevailed, appeared on fire. Let us suppose the different regions of this immense sheet of clouds to have been in a certain degree of mutual interdependence, — that is to say, that the fulminating state of each part was connected with the mean fulminating state of the whole; and this being the case, no one will be able to suppose that a few lightning conductors erected in the town of Padua, for example, could have exercised a sufficiently powerful action to preclude everywhere the possibility of thunderbolts.. When, on the contrary, the storm-clouds only extend over a much more limited space, and also under certain special cases of the distribution of the fulminating matter over their surface, the tranquillising influence of even a very small number of lightning conductors may be prompt and efficacious.

* When this alteration is studied with the help of an electrometer, the changes are indicated with remarkable instantaneity, and they can, moreover, be measured.

Several physicists, Toaldo being one of the number, have stated that they twice saw at Nymphenburg, in Germany, storm-clouds, from which flashes of very vivid lightning were incessantly darting, advance towards the castle, and after passing over the lightning conductors erected upon it, become merely dark clouds, not showing any flashes of light—mere dead coals or extinguished embers, to use Toaldo's expression.

In 1785, M. Cosson, the curé of Rochefort, wrote to the Abbé Bertholon, that on the 4th of December a cloud, "which sent out many flashes of lightning, and in which the thunder growled, directly after it had been carried by the west wind over the lightning conductor on the church, became quiet, and only showed occasionally some very faint apparitions of light." Bright plumes of light, or little flames, seen shining on the point of the rod of the lightning conductor on the church, showed evidently that it was exerting a powerful action; but yet if we had not had the curé's statement we could not have ventured to affirm that a single conductor could have been sufficient to almost completely deprive the storm-cloud of its thunder and lightning.

The property, to the consideration of which we have devoted so many pages, is developed more largely in proportion as the rods of the lightning conductors are more elevated. Nothing can better prove this than the numerous experiments made with kites; and in these, none have given nearly such striking results as those made by our countryman M. de Romas, at Nérac.

This intrepid physicist sent up into the air, to heights of four and five hundred feet, a kite of which the string had wound round it a metallic thread or wire, like the thick base strings of a violin. During a very moderate storm, accompanied by only a few instances of very slight thunder, Romas drew from the lower extremity of his string, not mere sparks, but flames nine or ten feet long and an inch broad. They were accompanied by reports as loud as those of a pistol-shot. In less than an hour, Romas drew thirty such, without counting a thousand more of seven feet and under.

The Nérac physicist remarked several times that thunder and lightning totally ceased while his experiments were going on. Dr. Lining of Charleston, and Mr. Charles, though their operations were on a smaller scale, also transformed thunder-clouds into ordinary clouds without thunder and lightning.

CHAP. XLV.

"PARAGRÊLES," OR APPARATUS WHICH MAY SERVE TO AVERT DAMAGE BY HAIL; AS "PARATONNERRES," OR OUR ORDINARY LIGHTNING CONDUCTORS, ARE CONSIDERED TO AVERT DAMAGE BY LIGHTNING.

THE observations and experiments spoken of in the preceding Chapter opened a wide and brilliant field of inquiry, into which it is much to be regretted that other observers have not entered. The formation of hail appears to be indisputably connected with the presence of an abundant quantity of fulminating matter in the clouds. Withdraw that matter, and the hail will either not be formed, or will remain in its rudimentary state, so that all that falls will be a harmless, extremely minute hail or sleet. Does any one doubt the great benefit which, in some countries, agriculture would receive from the disappearance of hailstorms? I reply, that in 1764 an enlightened inhabitant of the south of France wrote in the "Encyclopedia," "There is hardly ever a year in which hail does not ravage the half, and sometimes three-quarters, of the dioceses of Rieux, Comminges, Conserans, Auch, and Lombez." The single storm of the 13th of July, 1788, smote in France one thousand and thirty-nine communes. An official inquiry made the damage amount to twenty-five millions of franks (one million sterling).

I know very well that the employment of kites is not exempt from danger; that a thunderstorm forms, develops itself, and strengthens, while the air is generally calm; that the wind, by the help of which the apparatus must be sent up, does not begin to blow until the rain and hail are already falling, &c. Accordingly, kites are not what I think ought to be employed. What I would wish is, that captive balloons should be resorted to for this great and important experiment. I should wish them to be sent up to a much greater height than that attained by Romas's kites. If by rising three or four hundred feet above the elevations ordinarily reached by the upper extremities of the rods of lightning conductors, there were obtained, instead of little plumes or tips of flame, flames ten or twelve feet long, what may we not expect would happen when the whole apparatus, having risen to a height three, four, or ten times as great, according to circumstances, would almost graze the under surface of the clouds? and when, moreover (and this is an important particular), the metallic point which is to attract the fulminating matter, and which would be in communication with the long semi-metallic cord which officiates as conductor, being fixed on the upper part of the balloon, is presented to the clouds nearly vertically, or in the position of the rod of an ordinary lightning conductor? It does not appear to me that there would be anything too bold in supposing that it might be possible to dissipate the most violent thunder or hail storms by such means. At all events, an experiment so highly and directly interesting both to science and to the agricultural wealth of countries is well deserving of trial. If balloons of moderate dimensions were employed, the expense would certainly be less than the vine-growing districts now expend uselessly in firing so many guns and mortars.

The ravages occasioned in the vine-districts of Burgundy by hailstorms are especially great; it has been calculated that in 1847 the two small communes of Vaux and Arbuissonas lost by hail, crops exceeding in value a million and a half of francs (60,000*l.*). Soon after the publication of the "Annuaire" of 1838, several proprietors in the departements of Saone et Loire, and of the Côte d'Or, expressed their wish of combining together to bring into practice the method which I had proposed. M. Berthelier de Chaussailles consulted me on the means of overcoming the obstacles which might oppose the realisation of the project. The doubts which have since arisen on the electric origin of hail, and the difficulties which have been put forward against the theory of Volta, have proved to me that a full examination of the meteorological question must first be made. But in the part of the country in which I live I have not had the opportunity of making this examination in a thoroughly satisfactory manner. When science shall have contributed all that can be asked from her on the subject, it may, perhaps, be found that the idea suggested may be reverted to; and that by the aid of balloons surmounted by metallic points, storm-clouds may be converted into harmless clouds, to the great benefit of agriculture.

CHAP. XLVI.

ON THE SPHERE OF ACTION OF FIXED LIGHTNING CONDUCTORS.

To what distance does a well-constructed "paratonnerre," or lightning conductor erected on the roof of a building, exert efficaciously its preserving action? At what distance from the rod, measured horizontally, may there be almost a certainty of not being struck by lightning?

This question, of which the importance cannot be denied, has not, I think, been studied with all the care which it deserves.

Guided by vague analogies, J. B. Leroy, who was so much concerned with the construction of lightning conductors, said, in 1788, that a rod four or five metres high, fixed on the highest part of a building, protected all around within a circle of sixteen metres radius. In other words, this is equivalent to saying that the preserving action would extend horizontally in every direction to more than three times the height of the rod of the lightning conductor above the building on which it is placed.

The Section of Physics of the Academy restricted this limit. Being consulted in 1823 by the Minister of War, it appeared to adopt Mr. Charles' opinion, and admitted, though without saying on what grounds, that the rod of a lightning conductor protects a circular space which has a radius equal to twice its own height.

So imposing an authority naturally drew general assent; and accordingly the authors of the most recent treatises on physics and meteorology assign generally to the circular space, completely protected by a lightning conductor, a radius equal to twice the height of the rod.

Supposing this to be exactly true for a rod erected on an ordinary building of stone or brick, or an ordinary sloping roof of timber covered with tiles or slates, will the same hold good if considerable masses of metal form part of the building or of the roof? Certainly no one could venture to affirm this.

The protection afforded to a roof or a terrace is said only to extend to twice the height of the rod above the roof or terrace: is the sphere of action of the protecting rod equally restricted when brought down to a different and lower level; for instance, when measured on the ground? or instead of this being the case, does the rod of a conductor erected on a steeple, protect *on the ground* a circle of which the radius should be double the joint height of the steeple and the rod? These important questions seem to have been scarcely even propounded. I subjoin some numerical data, which, without altogether resolving them, may serve to afford some useful guidance to architects or constructors.

On the 15th of May, 1777, the powder magazine at Purfleet was struck by lightning, notwithstanding the conductor, which Franklin, Cavendish, Watson, &c. had caused to be placed on it.

The lightning fell on an iron cramp, which by means of lead soldering held together two of the slabs of the cornice surrounding the building at the base of the roof. From thence it darted to a pipe for discharging the rain-water, which it followed down to the water in a well, without doing any damage excepting the fracture of a stone which intervened between the cramp and the pipe.

I find on examining the drawings of the buildings (which are drawn to scale) that the point of the rod of the lightning conductor was twenty-six feet above the level of the slabs of the cornice, and that the horizontal distance between the vertical prolongation of the rod, and the iron cramp which was struck, was only twenty-four feet.

Thus the rod of the conductor, so far from having protected, at the spring or base of the roof, a circular space of a radius equal to twice its own height above that level, had not even extended its protecting influence to once that distance.

The end of the rod was eleven feet above the point of the roof on which it had been erected. Double this quantity, or twenty-two feet, would leave the cramp two feet beyond the circle of action, if that circle is estimated according to the view of those who say its radius is equal to twice the height of the end of the rod above the actual point on the building on which it is fixed. Thus the most restricted of the modes of estimation alluded to is not invalidated by the occurrence at Purfleet; but the larger estimate is directly opposed by it; in saying this, however, it is important to recollect that the point of the rod was not a very sharp one, and that the amplitude of the sphere of action has had to be measured relatively to a strong course of masonry of cut stone interspersed with metallic cramps.

On the 17th of June, 1774, lightning fell at Tenterden, in

Kent, on one of the four chimneys of Mr. Haffenden's house, although one of the number was surmounted by the rod of a lightning conductor. The chimney which was demolished was surrounded at a little distance by leaden pipes, and was fifty feet distant from the rod, the point of which did not rise more than five feet above the level of the four chimneys; thus the distance was ten times as great as the height of the rod above the place struck. It is evident, therefore, that this often-cited case is in no respect contradictory of received ideas. It should also be added, that the form and construction of the conductor were not perfectly irreproachable.

A violent stroke of lightning fell on the large poorhouse of Heckingham (in Norfolk), on the 17th of June, 1781, notwithstanding the eight lightning conductors erected upon it. The point first struck was at one of the lower corners of the slope of the roof; it was covered by a broad sheet of lead.

From this point to the nearest lightning conductor the horizontal distance was 58 feet, and as the upper extremity of the rod only rose 22 feet above the level of the spot struck,—less, therefore, than half the horizontal distance between that spot and the prolongation of the vertical rod,—it follows that the point struck was not situated within the circle which the above quoted received opinions have named as that which ought to be perfectly protected. Moreover, there was here also ground for the remark, that the conductors did not terminate in earth sufficiently humid.

Dr. Winthrop, of New Cambridge, relates that a tree was struck by lightning, and scored throughout its entire length, although it was only at a distance of fifty-two feet in a horizontal line from the lightning conductor on the church steeple.

If, as seems natural to suppose, the height of the steeple was not less than six-and-twenty feet above the top of the tree, the fact related by Dr. Winthrop would be directly at variance with the idea, that the radius of the action of a lightning conductor is to be measured by double the absolute vertical height of the extremity of the rod above the objects to be protected.

A stable, belonging to William Littleton, Governor of South Carolina, was struck and much damaged by lightning, although situated only sixty feet from a house furnished with a good lightning conductor.

This account does not state either the elevation of the point struck, or that of the lightning conductor; we cannot, therefore, deduce from it anything respecting the radius of action of the apparatus.

I notice another fact which is also not sufficiently circumstantially related; as the objects still exist, there is nothing to prevent the deficiencies being supplied.

The tower of the church of St. Michael, Cornhill, London, is surmounted by an excellent lightning conductor; but this did not prevent lightning from striking the leaden covering of the summit of the steeple of St. Peter's, though situated much lower than St. Michael's, and not more than 200 feet distant from it.

There is wanting in this account the statement of the vertical height of the lightning conductor on St. Michael's above the leaden covering of the steeple of St. Peter's. If, as seems probable, this difference does not amount to a hundred feet, there is nothing in the event to invalidate the rule which makes the radius of action equal to twice the relative height.

Taken conjointly, the above facts authorise the assumption, that the preserving action of lightning conductors erected on the tops of buildings may be safely estimated to extend to a horizontal distance, equal to twice the height of the rods of the conductors measured from the points on which they are fixed. Even the event at Purfleet confirms this determination.

In order to protect an extensive building, it will therefore be necessary to arm it with several lightning conductors; and the less the height of the rods the more they must be multiplied. Their number will be sufficient, when there shall be no point of roof, or terrace, or other portion of the building, of which the horizontal distance from the nearest rod shall be greater than twice the height of that rod above its base.

This rule being a logical deduction from facts, it is difficult to understand how, in considering the construction of lightning conductors, Franklin should have attached so little importance as he appears to have done to questions of elevation. All that he required was, that the points of the rods should rise a little above the tops of the chimneys. I also see, in a note signed by Cavendish, Priestley, Lord Mahon, Nairne, Watson, &c., the height of the rods fixed at 10 feet. In France, our builders go up to 10 metres (nearly 33 feet), and even only stop there on account of considerations connected with solidity. Of these two classes of dimensions, there can be no doubt which ought to be preferred.

CHAP. XLVII.

ARE LIGHTNING CONDUCTORS HAVING THEIR RODS INSERTED HORIZONTALLY, OR IN VERY INCLINED DIRECTIONS, IN THE ENTABLATURE OF A BUILDING, USEFUL?

All other circumstances being equal, lightning must be expected to strike, and actually does strike, the most elevated portions of buildings; but where are we to find perfect equality of circumstances? In how many ways may not such equality be disturbed? Are not a metal cramp, the handle for opening and shutting a casement window, the pipe of a stove, &c. &c., sufficient to do so? Besides, if clouds charged with fulminating matter were not generally terminated by nearly horizontal surfaces, the most elevated portions of buildings would not indisputably possess the unenviable privilege which we have just attributed to them. Now I would bring to the reader's recollection those ragged fragments, or shreds of cloud (descending in times of thunder almost to the ground), which the general mass draws along with itself wherever it is carried by the wind; certainly nothing can well be less suited than a vertical rod for gradually and silently drawing away the fulminating matter with which these pendant clouds are charged. On the other hand, a horizontal, or obliquely placed, rod would be extremely well fitted for the purpose. This is not, however, the only office which such rods might perform; they would also serve to receive strokes of lightning which would otherwise have struck the lateral faces of the buildings. Is it supposed, as is thought by some physicists, that these faces cannot be exposed to the same danger as the culminating parts of edifices? My answer is ready, and will consist in the relation of some facts which I have collected, and which appear to me to leave no room for the slightest doubt.

Mr. Alexander Small wrote to Franklin from London, in 1764, that he had seen in front of his windows a very vivid and slender fulminating dart, moving low down, without apparent zigzags, in a nearly horizontal direction, and had seen it strike a steeple far below its summit.

In September, 1780, a violent stroke of lightning killed two men on the ground-floor of the house of Mr. James Adair, at East Bourne; it also entered the first story by a window and did much damage, whilst the upper story and the roof were entirely untouched.

These effects might have been guessed, before they were ascertained to have taken place, from the observations of different persons who, at the time of the occurrence, were walking on the sea shore. The line traced by the meteor appeared to lead straight to the very middle of the front of the house. It was only there that it broke, and divided into several branches.

On the 12th of August, 1783, lightning damaged the steeple of the cathedral at Lausanne. It fell first on a horizontal iron bar, serving to unite two little columns situated at two-thirds of the height of the edifice. There is no reason to doubt that the fulminating flash really had this comparatively unusual direction: a trustworthy eye-witness distinctly saw it dart on the bar; and Dr. Verdeil, to whom the observation was immediately communicated, made in consequence the most scrupulously careful examination, in the course of which he could not discover any trace whatsoever of the action of lightning higher than the bar in question.

This lateral stroke, directed to a point so distant from the summit of the steeple, is the more remarkable because the building was accidentally well provided with a kind of lightning conductor.

Dr. Verdeil says in effect: "At the summit of the steeple there is a kind of shaft with eight longitudinal faces, surmounted by a long iron rod, on which the weathercock turns, and terminating in an iron point formed like the head of a pike. This octagonal shaft is entirely covered with copper sheathing. Eight bands of the same metal descend from the shaft along the angles of the spire, which is covered with enamelled tiles; these bands terminate in a horizontal rain-gutter, which entirely surrounds the base of the spire, and empties itself through two tubes of very thick metal into two large copper reservoirs, which are always full of water. Two long copper tubes run down from the bottom of these reservoirs into a common cistern, from whence they are conveyed to a fire-pump, which they fill whenever there is rain. This pump communicates through metallic spouts with the spout which pours out the rain-water on the pavement."

Let us admit there to be rain (and we know that much rain had fallen for half an hour previous to the fulminating stroke of the 12th of August, 1783), and we have a combination of metallic bars, plates, and tubes, forming, as we have before said, an almost unimpeachable lightning conductor.

We next take the case of lightning striking one of the sails of a windmill (the mill at Toothill, in Essex,) in repose, and inclined to the horizon at an angle of 45 degrees. This occurred in 1829. Who would not imagine that the point struck would be at the upper part of the sail? It happened quite otherwise: on the middle of the sail there was an iron knob, and it was entirely upon this that the lightning fell, leaving the upper parts of the sail untouched; so that the advantages of greater height were much more than counterbalanced by the presence in the lower portion of a few pounds weight of metal.

If I required to prove that inclined lightning conductors ought always to be affixed to buildings, the facts I have cited would be too few; but it will be remembered that I only wished to show that in certain cases oblique rods may do good service.

CHAP. XLVIII.

ON THE BEST FORM AND ARRANGEMENT TO BE GIVEN TO THE SEVERAL PARTS OF WHICH A LIGHTNING CONDUCTOR IS COMPOSED.

§ 1. *On the Point.*

WE have proved that if it be desired, as it is reasonable it should be, not to lose the advantage of the property whereby lightning conductors may serve to withdraw gradually and silently from storm-clouds the fulminating matter with which they are charged, we must make their rods terminate in a very sharp point. If this point is made of iron, the rust occasioned by the action of the air and water will soon destroy it; it will be blunted, and its withdrawing faculty will be diminished day by day.

This inconvenience was at first counteracted by gilding the point of the iron rod for a certain way down: gilding on iron being very little durable, it was afterwards found better to screw on points of copper gilt to the extremities of the rods; and, lastly, points of iron or of copper have been generally replaced by platinum points, since improvements in metallurgy have enabled the supply of the latter at very moderate prices.

Platinum points are to be preferred to copper ones, not only on account of their inalterability under the action of water and air, but also on account of their nonfusibility. A stroke of lightning which would fuse or blunt a copper point, would, on the contrary, leave to the platinum one that sharpness on which the great intensity of its action depends. If it is remembered that the rod of the lightning conductor may be struck at the beginning of a thunderstorm, and that, moreover, in many cases, the replacement of points requires costly scaffoldings, the advantage of the nonfusibility of platinum points, both as regards safety and economy, will be readily appreciated. These advantages are such, that in 1790, at a period when the manner of working platinum was scarcely known, the Philosophical Society of Philadelphia received with great applause the proposal of Mr. Robert Patterson to make the points of lightning conductors of another substance very little liable to fusion, viz., plumbago (carburet of iron).

In some countries—*i.e.* in Germany and in England — some constructors of lightning conductors adapt to the extremity of their rods, not a single point, as is done in France, but a vertical point, around which are arranged several others very divergent, and inclined to the horizon at different angles.

One reason alleged in favour of this practice was this; if we suppose that one point becomes oxydised by the air and blunted, and loses thereby part of its power, several blunted and rusted points would act in their combination as powerfully as a single uninjured point. This reason in favour of many points has no very great weight at the present time, inasmuch as a single platinum point perfectly answers the same purpose; but it was not the only reason given; it was hoped that by the employment of points both differently inclined, and directed towards different parts of the horizon, there would always be some one amongst them which would present itself perpendicularly to the storm-cloud, whatever might be the form and position of the latter. All this may well appear a little overstrained, but yet, until very careful repetition of Beccaria's experiment, on which we dwelt in a preceding page (p. 230.), shall have established that a vertical point withdraws from every kind of cloud more fulminating matter than an inclined one; or better still, until by following up the method of the celebrated Turin physicist, it shall have been proved that a single point always acts more powerfully than several divergent or radiating points, it will not be proper to treat the employment of conductors with multiple points as a device to be regarded only with contempt. I admit, nevertheless, that for the present, and until these experiments shall have been made, it will be wise, and quite sufficient, to keep to the form originally recommended by Franklin.*

§ 2.—*On the Conductor.*

It is on the good construction and good arrangement of the conductor that the preserving action of Franklin's apparatus principally depends.

Both the conductor and the upper rod ought to be sufficiently thick and massive to be exempt from the danger of being fused by a stroke of lightning. According to all the facts collected in Chapter XVIII., it follows that this condition will be abundantly satisfied by the employment of square or cylindrical bars of iron or copper 8-tenths of an inch diameter. If a greater thickness than this is given, and particularly at the base of the rod, it is only to resist better the force of the wind.

In order to preserve the rods and the conductors from rust, they are usually covered with a coat of paint. In America such minute precautions have been taken, that the paint chosen is lamp-black, on account of the property which that substance possesses of rendering compositions, into which it enters in considerable proportion, pretty good conductors of fulminating matter.

As the conductor can only fulfil its office properly by possessing the condition of parting with this matter at the same

* I do not speak here of the method which some constructors of lightning conductors had adopted, which consisted in making the points of *magnetised* iron or steel, which must be regarded as quite useless.

rate that it receives it, it is absolutely necessary to supply what may be wanting in the degree of conductibility of the ground, by the multiplication of the points by which the fulminating matter may flow off.* If the conductor goes down into earth only moderately humid, and therefore only moderately permeable to fulminating effluxes, it will be necessary for the contact between them to extend through a considerable length. This length may be less if the ground remains strongly saturated with moisture throughout the year, and less still if the conductor goes down to a natural sheet of water.

The highly necessary multiplication of the number of points by which the fluid may flow off into the ground, might be obtained by, as it were, spreading out the metal, bringing the lower part of the conducting bar, by the action of proper machinery, into the form of a wide plate, and thus extending as much as possible the surface brought down into the ground. I even believe, that if this expansion, or extent of surface, were sufficiently great, it would not be necessary to sink it below ground; a mere superficial contact would be sufficient. For instance, I believe this would be quite sufficient in the case of buildings surrounded at their base by a border of lead or of tin, bent at right angles in such a manner that one side of the angle is laid against the upright wall, and the other rests upon the ground. Let the conductor be in good contact with this border, and the fulminating matter which, during the most stormy weather, it receives from the rod, will be able to pass off through so great a number of points, that there will no longer remain any reason to apprehend either luminous jets or detonations. It is for this reason, if I am not mistaken, that such a monument as the column of the Place Vendôme, resting on a wide metallic pedestal, which itself rests with its whole under surface in contact with the ground or pavement, can dispense with a conductor.

Usually, it is by making the conductor divide into several branches, and not by spreading it out, that constructors of lightning conductors augment the underground surface by which the fulminating fluid is to pass off into the earth.

When the conducting bar enters the ground there is an

* Mr. Hare, Professor of Chemistry in the University of Pennsylvania, proposes, when possible, to make the underground portions of lightning conductors communicate with the cast-iron pipes used to distribute water through the different parts of towns.

alternative between two opposite difficulties. If the soil is humid the fulminating matter passes off easily, but the metal rusts and is soon destroyed. If the soil is dry the bar lasts a long time, but it fulfils its functions very badly. It was therefore very desirable to discover some substance which should be a good conductor, but should not injure iron. Charcoal, when it has been passed through a red heat, is of this description. Accordingly, as Robert Patterson proposed in 1790, those constructors of lightning conductors who are well acquainted with all the resources which science offers, take care to make the conducting bar pass through a kind of well filled with *baker's braise* or *embers.* I put this in italics, that there may be no mistake; it is indispensable that the charcoal should have been heated to redness; common charcoal cannot answer as well.

When the conductor goes down into a natural sheet of water, experience has proved that it is sufficient that the conductor should be sunk about three feet into the water.

I have spoken of a natural sheet of water, in opposition to artificial reservoirs or cisterns of rain water. It is a mistake to suppose that cisterns of which the bottom and sides have been rendered water-tight, whether by cement, slabs, bricks, or otherwise, can be regarded as equivalent to wells properly so called. The bricks or slabs, or hydraulic cement, being dry in the middle of their thickness, offer only a very difficult passage to the fulminating matter, which cannot, therefore, as in the case of a well, pass rapidly away and disperse itself to a distance through a multitude of fissures and small openings full of water, or, at least, of moisture. Instead of this, in the water-tight cistern, the fulminating matter having for a moment taken possession of the water, and not being able to pass on further and escape, turns back, re-ascends the conducting bar by which it had descended, and throws itself, with a fulminating stroke or detonation, on some object in the vicinity.

I may fairly be asked to adduce some proofs in support of this theory, and I hasten to do so.

On the 19th of June, 1819, lightning struck the principal pinnacle of the Cathedral of Milan, which pinnacle was surmounted by a lightning-rod, of which the conductor plunged into what was supposed to be a large well,—the whole apparatus being in good order. Nevertheless near the conductor, which had itself sustained no injury, there were found, at different elevations, marbles broken or detached, arabesques defaced, &c. On a thorough examination of all the circumstances, by Professor Configliachi, it proved that the supposed well was in reality a tiled cistern!

On the 4th of January lightning struck the protecting apparatus placed on the summit of the lighthouse of the port of Genoa. The rod and the conductor were broken in several places, though all had appeared to be in good order, and although the conductor went down into water; but this water was contained in a water-tight cistern of small capacity, hollowed out by the hand of man in the rock on which the lighthouse stands.

However slight may be the resistance which is opposed by a metallic bar to the passage of the fulminating matter, it is well not to neglect it; and, as this resistance must increase with the length of the bar, it will be proper, if no serious objection exist, to bring the conductor by the shortest possible path from the foot of the vertical rod, which attracts the lightning, to the moist earth into which it is to discharge itself.

I have just spoken of the thickness proper to be given to the conductor with a view to what may be called simple, or single, strokes of lightning, in which the rods had received only the fulminating matter by which they had been immediately struck. These dimensions might very well be insufficient if, at a given instant, one conductor had to receive and to transmit to the ground all the fulminating matter which had simultaneously struck several lightning rods. This remark shows the obvious necessity of having as many conductors as rods. It does not, however, prevent its being useful to establish an intimate connection between the base of all the rods by means of the iron bars which run along the ridge beams of roofs, and which need not be as thick as the conductors expressly provided as such. It will also always be an advantage to extend the same kind of communication to large metallic pieces forming parts of the roofs or parapets of buildings, and especially to the iron beams which are now coming so much into use.

Rigid metallic bars only adapt themselves to the various inflections of the roofs, cornices, and architectural ornaments, by means of many breaks and junctions, wherein, in course of time, water and consequent rust occasion undesirable interruptions. These disadvantages are avoided at the present time, by the substitution of flexible metallic cords in lieu of the bars which were formerly exclusively employed. These cords have, and ought to have, the same dimensions as the bars previously used. The strands of which they are formed may be pitched separately; but this must not prevent the entire cord from being afterwards itself pitched with the greatest care. Of course it must always be distinctly understood that the tar is to cover only those parts of the cord which are to be preserved from contact with air and moisture. It is indispensable that those parts which are intended to be sunk in water, damp earth, or embers, should, on the contrary, have their metallic surfaces as perfectly bare as possible.

Some constructors have thought it necessary to interpose between the rods and conductors and the roofs and walls of the buildings, non-conducting substances, such as glass, pitch, &c., to prevent any appreciable quantity of fulminating matter from deviating laterally and darting from the conducting bar to the objects which it was designed to protect. But this practice of insulating the conductors has been nearly given up, having at last been recognised as an unnecessary (and very costly) precaution; inasmuch as fulminating matter, once engaged in a metallic bar sufficiently thick, and terminating in liquid of indefinite extent, does not quit such a bar to throw itself on the materials of which buildings ordinarily consist, unless in quantities so small as to be incapable of producing injury, or even any sensible effects.

It would seem as if the same arguments should lead to the decision of a question which has been much debated among physicists, *i.e.* whether it is a matter of indifference whether we place lightning conductors inside or outside our buildings. I own that on this latter point I am far less ready to answer affirmatively. Voltaire says, "There are some great lords whom one should only approach with extreme precaution; lightning is such a one." I feel inclined to admit that the illustrious author may be right, when I remember the case previously cited by me (p. 142), in which the lightning, quitting the conductor on the outside of Mr. Raven's house, went off horizontally through the wall in order to strike a fowling-piece standing upright in the kitchen. How much injury might not have resulted from this lateral movement, if the lightning had not had to traverse a thick wall?

It will be said, the conductor was not thick enough; no doubt that is true: but I will now cite a case in which every thing appeared to be in good order, and the apparatus acting as well as could be wished; and yet the fulminating matter deviated from its path; and there is every reason to believe disastrous consequences would have resulted if a thick wall had not in the same way intervened between the conductor and a number of labourers inside the building.

On the 31st of July, 1829, in the prison at Charleston, simultaneously with a loud clap of thunder, three hundred persons sustained a violent shock, the general effect of which was a great weakening of muscular strength for several seconds: no one suffered any permanent injury.

The prison was provided with three lightning rods and conductors, all in good condition, and placed eighteen feet apart; and, indeed, the building was not apparently touched in any way. How, then, did it happen that this preserving effect of the conductors did not extend as usual to the inhabitants as well?

This question has been deemed to be satisfactorily answered by the large quantity of iron in the interior of the prison. Mr. Bryant, the governor, estimated it at 100 tons, to which it must be added, that much the greater number of persons had either hammers, files, guns, or pikes in their hands.

Hitherto, physicists do not appear to have attached any importance to the form of the inflections which it is necessary to make in the conductor in bringing it from the sloping roof to the vertical wall. In passing the eaves, the cornices, &c., the conducting bar or chain is bent into a right angle, and sometimes even into an acute angle. Nor is it very uncommon to find equally abrupt changes of direction in other parts of the conductor, even near the ground. Let us suppose a violent stroke of lightning to occur, and we shall see that such inflections may become dangerous, at least if I may judge from several events of which I have read accounts, and which seem to authorise the belief that, in calculating the march of fulminating matter, we should not altogether put aside acquired velocity. On this subject, the "Description of St. Domingo," by Moreau de St. Méry, vol. i. p. 393., gives an account of lightning at first following the conductor with regularity, then abandoning it at the point where the bar was bent so that its two parts formed an acute angle, and, passing on through the air, striking objects situated in the prolongation of the first side of the angle.

The Mémoires of the Academie de Lausanne, tome i., also show us lightning which, having arrived in a very oblique direction at the middle of a horizontal bar, was only propagated through it in the continuation of the same direction, although every thing in the bar was quite symmetrical. The question being now proposed, it may be expected that cabinet experiments will soon show if these considerations are unfounded; in the meantime, it can but be advantageous to avoid acute angles in the form of the conductor, and to take care to make the necessary changes of direction gradual by gentle curves.

On the 16th of December, 1852, lightning struck and fused the rod erected on the tower of the seminary of St. Anne d'Auray; the conductor was broken at the part where, after having followed the contour of the cornice, it bent again to descend vertically to the ground.

This is a fresh proof of the necessity of not making the conductor bend into too acute angles. (Relation de l'Abbé Pinel, in the journal *le Cosmos*, 12th of January, 1853.)

The minute dust of gunpowder, which is carried about by the slightest current of air, and which is deposited on all internal and external projections or ledges of powder magazines, becomes a source of real danger in such establishments. Suppose this minute dust to be ignited by a spark resulting from an imperceptible interruption to the continuity of the conductor, it may be the means of setting fire to the barrels of powder. The consideration of this possibility has given rise to suggestions for placing the lightning conductors intended for the protection of powder magazines, not on the buildings themselves, but at the extremity of long upright masts, to be fixed some feet from the walls. This idea was put forward as early as in 1776 by Toaldo. At a later date (in 1823) it received the high approval of the Academy of Sciences; unfortunately its application is subject to a very serious difficulty arising from a question which we have already had under consideration. We know very well that the points must rise higher than the highest part of the building; but what is the radius of their circle of action? If we might suppose it equal to twice the absolute height of the point of each lightning rod above the ground, a small number of such apparatuses would be sufficient to protect the largest magazine. But if, on the other hand, we admit that the radius is only to be estimated at twice the height of the points above the most elevated parts of the magazines, we shall find that there are buildings of this kind which, unless by an immense outlay, it would be impossible to protect by means of lightning conductors on detached masts.

Although I have already insisted at much length on the rules which ought to be adhered to in the establishment of lightning conductors, yet I will add here an account of the stroke of lightning which so seriously threatened the powder magazine of Bayonne on the 23rd of February, 1829. Faults which have been actually committed, and which have narrowly escaped causing great calamities, always leave in the mind more durable recollections than do simple precepts or rules. It may moreover be useful to show how an arrangement of apparatus of the highest pretension may become most objectionable, merely by the neglect of what might appear slight circumstances.

The powder magazine at Bayonne is a building 57 feet long by 37 feet wide. The roof slopes both ways, and both the ridge and the gable ends are covered by large sheets of lead, connected together. The rod of the lightning conductor is 22 feet high, and a piece of lead which is soldered to one of the sheets is wrapped round its base. By this arrangement all the metallic parts of the roof are in communication with each other.

The conducting part of the apparatus is an inch and two-tenths in diameter: instead of being sunk into the earth in the usual manner at the foot of the building, it is supported in a horizontal position, nearly 32 inches above the ground, by five wooden posts. It is only at a distance of 33 feet from the outer wall of the magazine that it is sunk vertically in a square pit about 7 feet each way, with masonry sides, and filled with charcoal to a depth of about three feet and a half from the bottom. In order to multiply the points of contact between the charcoal and the natural soil, the four walls of the pit are terminated below by open arches. The pointed end of the conductor rests on a peg stuck into the bottom of the pit. Metallic roots diverging from the principal stalk, and branching out into further subdivisions, extend through all parts of the mass of charcoal, above which there is a layer of loose earth, covered over with a pavement of stone slabs.

On the 23rd of February, 1829, at four in the afternoon, a few minutes after a heavy driving shower of rain and hail with a strong west wind, lightning struck the point of the rod and fused it for a length of about half an inch. So far there was nothing extraordinary. But there were found evident indications of discharge of fulminating matter at many other points; therefore the metallic rod had not protected the building completely.

At the south-west corner the sheet of lead covering the gable showed a rent of $7\frac{1}{2}$ inches in one direction, and $8\frac{1}{2}$ in another, precisely above an iron link used to fasten together two of the stones of the cornice.

The lightning had also left traces of explosion on the five wooden posts, of which we have already spoken as having been used to support the conductor horizontally above the ground.

The piece of lead capping the post nearest to the building had been raised, and the two nails which fastened it pulled out; on the cap of the second post there were two nearly circular holes, and a small rent; on that of the third there were three holes, one of which was $2\frac{1}{2}$ inches long by 4-10ths of an inch wide. The lead of the fourth and fifth posts had only one hole pierced in each. In all these openings or rendings the lead was turned back from below upwards.

Such are the principal facts put on record in a letter to the Minister at War by the Colonel Director of Artillery at Bayonne, and in the Report drawn up by a Commission named by that officer to ascertain the mischief done.

The section of Physics of the Academy of Sciences being called upon at the time to give its opinion on the event, and to explain the inefficiency of an apparatus which might appear at first sight to have been established with great care, recorded the fruits of its examination in a report drawn up by Gay Lussac; and I cannot do better than analyse the principal conclusions therein contained.

The conductor did not offer to the fulminating matter a sufficient channel for flowing off; it therefore opened for itself fresh outlets by the south-west corner of the building and by the five wooden supports.

The cause of the insufficiency of the lightning apparatus at Bayonne is to be looked for in those really inexplicable arrangements adopted by the constructors, which we have described above. The metallic conductor ought either to have been immersed in a well of water, or at least to have been in extensive contact with moist earth. But on the contrary, as if it had been feared to offer too great a number of outlets to the fluid, all the portion of this bar which ran horizontally was supported at a height of 32 inches above the ground by wooden posts, in other words, by very imperfect conductors*: subsequently the bar only went 6 or 7 feet into the ground. It is true that the extremity of the bar was surrounded by charcoal,—but it was only charcoal, not embers which had been red hot,—and ordinary charcoal does not possess any remarkable amount of conducting power.†

Under these circumstances is it very surprising that the lightning branched off? that for want of sufficient outlet by its intended path, a not inconsiderable portion of it should have followed the direction of the five posts in order to reach the ground? and that, moreover, at the south-west corner of the building, a part should have darted from a lead plate in communication with the conductor to the iron link joining the two stones which the lead covered? The preference thus given to the south-west corner may also be explained by the circumstance,

* This arrangement had probably been suggested by a very just, but in this case very ill interpreted, precept of Franklin's. The great American physicist desired that the lower extremity of a conductor should not remain too near the walls of the building. He feared, that in the absence of sufficient conductibility in the soil, the explosion of which this end must inevitably be the seat, might go off laterally towards the foundations of the building, which might be shaken thereby, if the proximity were too great. He therefore advised that the conducting bar, after being sunk in the ground, should be inflected, and carried to some distance from the walls; but he would never have sanctioned this distance being obtained at the cost of diminishing the number of points of contact between the bar and the soil. He doubtless would have approved the 33 feet of lateral deviation in the conductor at Bayonne, but would have done so on the express condition that, instead of being carried in the air on posts, these 33 feet of metallic bar should have been sunk in the ground.

† I must repeat that it has been established by many experiments that ordinary charcoal, *i.e.* charcoal but slightly calcined is, when dry, scarcely at all a conductor of fulminating matter. When saturated with water it has decided conducting properties, but even then much less so than embers which have been in a strong fire. In the absence of this latter kind of charcoal, pulverised coke may be used.

that the wall at this corner, having just before the explosion had the thunder shower, driven by the west wind, beating against it, had become a semi-conductor.

CHAP. XLIX.

ON THE ORGANS WHICH ARE MOST USUALLY AFFECTED IN DEATHS OR INJURIES OCCASIONED BY STROKES OF LIGHTNING.

THE solution of the question which forms the heading of this chapter is in the highest degree interesting to those who are concerned in legal medical investigations. It must, however, be admitted, that it has not yet been treated with the attention and strictness which it requires.

John Hunter said, that lightning in traversing the body produces an entire and instantaneous destruction of the vital principles. I must be pardoned for saying that this is but repeating known facts in obscure terms. According to Brodie, death follows from the action of the lightning matter on the head.

Edwards considered death caused by lightning to be the result of a disorganisation of the nervous system. Others confine the action to the cerebro-spinal system, but without however citing any decisive experiments in support of their opinions.

Lightning produces on animated bodies struck by it considerable mechanical effects, which for the most part have some manifest relation to the portions of metal disseminated in the clothing of the sufferer. Sometimes the marks left by the lightning are merely superficial, being entirely confined to the skin; under other circumstances the very bones are broken. A case has even been reported in which the skull of a man struck by lightning appeared as if it had been crushed by a heavy bruising instrument. It is not uncommon for the clothes of the person struck to take fire.

It has been said on the authority of Hunter, but without sufficient proof, that the blood of a man or of an animal struck by lightning does not coagulate in the body, and that the muscles never acquire the rigidity of an ordinary corpse; but this assertion is shown to be incorrect by authentic anatomical examinations and dissections made by Schultès of Landshut. It has also been said that in this kind of death putrefaction manifests itself sooner than usual.

When it happens that the person struck to death by lightning had about him a knife, needles, or anything made of steel, the powerful magnetism which is at the same time imparted to those objects is perhaps the most evident proof which a surgeon or magistrate could receive of the nature of the fatal accident.

Instances have been cited in which strokes of lightning, not sufficiently strong to cause death, had occasioned deafness, or had produced amaurosis, with dilatation of the pupil and loss of its power of contraction. In some cases the deafness or the amaurosis very soon disappear; in other instances they have lasted for several days, or for several weeks.

The most frequent result of strokes of lightning of moderate intensity is partial paralysis, more or less persistent, of the legs or arms.

Mr. Edouard Robin attributes death occasioned by lightning to a kind of asphyxiation, or a sort of sudden disappearance of atmospheric oxygen. He thinks he finds a confirmation of his theory in observations made by an Italian physician, from which it would follow that in the body of a person whose death has been occasioned by lightning, putrefaction is, comparatively speaking, but little active.

CHAP. L.

PERSONS STRUCK BY LIGHTNING HAVE FREQUENTLY THE HAIR ON THE DIFFERENT PARTS OF THEIR BODIES BURNT OFF.

Instances of this kind of effect are both numerous and well authenticated. I will only notice here a few, which have been characterised by exceptional circumstances. I subjoin an extract from an account communicated to me by M. Rihouet, captain of a frigate, who was performing the duties of second in command on board the "Golymin," ship of the line, when that vessel was struck by lightning, on the night of the 21st of February, 1812, when going out of the harbour at L'Orient.

M. Rihouet received several hurts in the head. "The next day," he said, "when I wanted to shave myself, I found that the effect of the razor upon the beard was to pull it out by the roots, instead of cutting it; and since that time I have had no beard. The hair on my head, my eyebrows and eyelashes, and generally all the hair on my body, successively came out in the same way by the roots, and did not grow again. During the year 1813, the nails on my fingers scaled away, but those on my feet underwent no visible change."

I find in the *Cartas eruditas* of Pére Feyjoó, that after the fall of a thunderbolt in the town of Santiago, a young man, named Juan Francisco Menendez Miranda, near whom the meteor passed but without hurting him at all, began to lose the hair from his head and all parts of his body, so that in the course of a few days none remained.

CHAP. LI.

VERY INTENSE STROKES OF LIGHTNING KILL MEN, ANIMALS, AND VEGETABLES; STROKES OF LIGHTNING OF A MODERATE DEGREE OF INTENSITY HAVE FREQUENTLY APPEARED TO RID MEN AND ANIMALS OF MALADIES FROM WHICH THEY WERE BEFORE SUFFERING, AND HAVE EVEN MARKEDLY ACCELERATED THE GROWTH OF VEGETABLES.

M. Quatrefages, in 1838, has reported in detail two perfectly well authenticated cases of such effects.

On the 20th of June, 1831, a person employed about the telegraph at Strasburg, was struck by lightning in his sentry-box, and fell senseless on the floor. His neck and arms were rigid and paralysed, as were also his lower limbs. The paralysis of the left side lasted until the following morning.

His health had been pretty good previous to the accident; but as soon as his hurts were healed, he often repeated that he had never felt so well in his life before. He became in a remarkable degree fatter and stouter, and he himself always attributed to the stroke of lightning, the sensible improvement which took place in his health from that time forward.

On the 10th of June, 1835, at Martinique, M. Roaldès, having been struck by lightning, fell to the ground paralysed in his lower limbs and in his right arm; but this paralysis lasted but a short time, it yielded to repeated rubbing, and three hours after the accident no trace of it remained. This powerful shock was followed by the recovery of M. Roaldès's health, which had been previously very indifferent.

M. Cartheuser records a case of amaurosis cured by lightning.

At Plancy (Département de l'Aube), lightning fell on the 20th of July, 1843, in a workroom where several men were engaged with hosiery looms. One amongst them, who suffered from rheumatic pains, was entirely cured.

A valuable sick horse, belonging to the lieutenant-colonel of the 7th regiment of chasseurs, formed part of the column struck by lightning at Tarbes, on the 13th of June, 1842; the animal had several setons, and had been condemned by the veterinary surgeons. Nevertheless, the day after the accident, its health showed rapid improvement, and in the course of twelve days it was quite out of danger. (*L'Echo du Monde savant*, August 7. 1842.)

The following fact is completely at variance with the prevailing ideas respecting the influence of thunderstorms on the development of certain insects, more particularly silkworms:—

On the 11th of June, 1842, lightning fell on a farm-house at St. Jean-du-Pin, near Alais, and severely hurt three persons who happened to be in the building belonging to the establishment appropriated to silkworms. Neither the brilliant light, nor the noise, nor the sulphurous vapours, nor the smoke, nor the fulminating matter, did the least harm to the silkworms; on the contrary, they appeared electrified in the conversational sense of the word, and went on working with redoubled activity.

I might subjoin several instances in which vegetable life was promoted by the effects of lightning. I will give only one, of which I had myself the opportunity of ascertaining the reality.

There existed a few years ago, between Tours and Rochemort, a château, called Comacre, with an avenue of fifteen hundred poplars. One of these was struck by lightning, which left evident marks of its action, both on the trunk of the tree and on the ground adjacent to it. From thenceforth the growth of this tree became something quite peculiar; the dimensions of its trunk soon surpassed those of any other tree in the avenue, to such a degree that the difference attracted the notice of the least attentive, and of those who were entirely ignorant of the event which had occasioned it.

CHAP. LII.

IS IT PROVED, AS MATTER OF FACT, THAT THE APPARATUSES CALLED, LIGHTNING-CONDUCTORS HAVE PRESERVED THE BUILDINGS ON WHICH THEY HAVE BEEN ESTABLISHED FROM DAMAGE BY LIGHTNING?

FROM the manner in which the above question is framed, every one will at once have perceived that the intention in the present Chapter is to resolve it exclusively by mere facts, without having in any way recourse to the deductions, highly simple, direct, and legitimate as they are, by which in recent pages the mode of action of the apparatuses in question has been made apparent. The facts we are about to offer will be taken from all countries, and will be numerous, because it is by their number that they acquire value and importance. We do not learn, either from the Bible or from Josephus, that the Temple at Jerusalem was ever struck by lightning during an interval of more than a thousand years—from the time of Solomon to the year 70,—although from its situation it was completely exposed to the very frequent and violent thunderstorms of Palestine. Remembering the care with which ancient nations recorded strokes of lightning by which any degree of injury was done,—how often, for example, the Roman Annals mention the Capitol and other public buildings being struck by lightning,—it appears natural to infer from this silence, with the Orientalist Michaelis, that the Temple did not receive any severe stroke of lightning in the course of ten centuries. The probability of the justness of this inference is much strengthened by the circumstance that the Temple, being overlaid internally and externally with wood, would certainly have caught fire if struck by a violent thunderbolt.

Supposing the fact to be thus well established, we have next, with Michaelis and Lichtenberg, to seek a cause, and we find a very simple one.

By a fortuitous circumstance the Temple was armed with lightning-conductors quite similar to those which we now employ, and which we owe to Franklin's discovery.

The roof, constructed in what we now call the Italian manner, and covered with boards of cedar having a thick coating of gold, was garnished from end to end with long, pointed, and gilt iron or steel lances, which Josephus said were intended to prevent birds from resting on the roof and soiling it. The walls also were overlaid throughout their extent with wood thickly gilt. Lastly, there were in the courts of the Temple cisterns into which the rain from the roof was conducted by metallic pipes. We have here both the lightning-rods and a supply of means of conduction so abundant, that Lichtenberg is quite right in saying that many of our present apparatuses are far from offering in their construction so satisfactory a combination of circumstances.

The conclusion at which I arrive is, that the long immunity enjoyed by the Temple of Jerusalem presents the most manifest proof of the efficacy of lightning-conductors.

In Carinthia, at the castle belonging to the Counts Orsini, the church, which is situated on a hill, was so often struck by lightning, and so many deplorable accidents had happened in consequence, that at last it was determined not to use it for the celebration of divine service during the summer months. In the course of the year 1730, one single stroke of lightning entirely demolished the steeple. It was rebuilt, and subsequently continued to be struck by lightning four or five times a year on the average, without counting some cases of extraordinarily violent thunderstorms, in which the steeple received five and even ten strokes of lightning in the course of a single day. After one such storm, about the middle of the year 1778, the building was again found so near falling that it was taken down and rebuilt; and this time it was furnished with a pointed lightning-rod and a good conductor. Five years afterwards, *i.e.* in 1783, being the date of the note written by Lichtenberg from which I take these details, the steeple, instead of having been struck twenty or twenty-five times, had only been struck once, and even on that one occasion the metallic point had received the stroke, and no harm followed.

In the spring of 1750, lightning fell on the Dutch clock-tower at New York. It continued its course through several ceilings, running down the wire which connected the wheel-work with the hammer which struck the hours. As far as the wire extended, the lightning did no mischief to the building; it did not even enlarge the apertures by which the wire passed through the ceilings, though they were only about half an inch wide. For the greater part of the distance, the wire was only diminished to two-thirds of its previous thickness, but its lower portion was completely fused; and from thence the lightning darted to the hinges of a neighbouring door, broke the door, and then dispersed itself.

In 1753, lightning fell on the same steeple, and produced precisely similar effects, although the wire communicating between the tongue of the bell and the wheel-work of the clock, had been replaced by a small copper chain.

In 1755, a fresh explosion took place; but now the rod of the weathercock communicated with an uninterrupted external iron conductor, which went down into the moist earth; and this time both the door and the wire of the clock were quite untouched; the building also suffered no injury.

From the time when it was first built, the church of Saint Michael at Charlestown was visited and injured by lightning every second or third year; at last, a conductor was placed upon it, and in 1774 Mr. Henley was informed from America, that during the course of the fourteen years which had elapsed since the erection of the apparatus, the church had not been once struck.

In 1772, Toaldo stated, that the royal residence at Turin, the Valentino, had ceased to be struck by lightning since Beccaria had armed its principal pavilions with slender metallic rods, having attached to their lower extremities wires which went down into the ground. The castle had often sustained injury previously.

The Campanile of St. Mark's at Venice, of which the construction goes back to a very distant date, is not less than 340 feet high. The pyramidal elevation which rises from it, is 90 feet high. The whole is surmounted by the figure of an angel made of wood covered with copper, and 10 feet high.

The great elevation of this building, its insulated position, and, above all, the multitude of pieces of iron in its construction, rendered it in a high degree obnoxious to danger from lightning; and, in fact, it has been frequently struck. Unfortunately the town registers do not mention all such strokes; but, generally speaking, only those which entailed large expenses for repairs. The following is the list taken from the documents:—

1388. 7th of June (no details given).
1417. Pyramid burnt.
1489. 12th of August, Pyramid again reduced to ashes.
1548. June (no details given).
1565. (No details given.)
1653. (No details given.)
1745. 23rd of April, great damage done. Thirty-seven cracks threatened the fall of the tower. The repairs cost upwards of sixteen thousand pounds.
1761. 23rd of April, damage not considerable.
1762. 23rd of June, considerable damage.

In the beginning of the year 1776, a lightning-conductor was placed on the Campanile; and I do not learn that, since that period, the edifice has ever been injured by lightning.

The fine tower of Sienna was frequently struck, and on every occasion much damage was done. In 1777, it was provided with a conductor; and this had but just been effected, when on the 18th of April it received a fresh discharge. But this time no damage was done.

I read in a memoir by Sir William Snow Harris, that six churches in Devonshire having tall steeples, were all struck by lightning within the short time of a few years; but that one only among them escaped injury, being the only one which was furnished with a lightning-conductor.

Geneva is much exposed to thunderstorms, nevertheless the towers of its cathedral—although the highest edifice in the town, and rising higher than any other object for a great distance in the country around—have for two centuries and a half enjoyed the privilege of not being struck by lightning, while the much lower steeple of St. Gervais has been repeatedly injured.

Saussure, as early as 1771, sought to discover the cause of this singular anomaly; he found it in the accidental conductors with which the towers of the cathedral are furnished. The middle tower is nearly three centuries old; and said Saussure, "as it is constructed entirely of wood, it must always, as is still the case, have been covered from top to bottom with tin; now, it is easy to conceive that so considerable a mass of metal must always have formed an excellent conductor, and that its widely extended base might easily meet somewhere with some substance to finish the communication." Let us add, in order to complete the explanation of the illustrious physicist, that the communication with the ground was effected, in different degrees indeed, through the intermediation of all the materials at all parts of the edifice, and that the number supplied what might be wanting in intensity. Lastly, let us remark, that for more than a century, the pipes of lead or tin used for conducting the rain-water below ground, have constituted a communication more perfect, perhaps, than that of ordinary rods.

The column called "the Monument," in the city of London, was erected in 1677, by Sir Christopher Wren, in commemoration of the Great Fire of London. Its height is more than 200 feet above the pavement in Fish Street. Its upper portion is terminated by a large metallic basin filled with a number of metallic pieces, more or less contorted and spreading in different directions, and, as they are intended to represent flames, all terminate in very sharp points. Four upright iron bars descend from the basin to the gallery, and support the iron staircase which terminates at the basin. One of these bars, which at its base is fully 5 inches broad and 1 inch thick, communicates with the grappling-irons of the stairs which go down into the ground. Every one will see that we have here the multiple points which have been spoken of previously as being used in some lightning apparatuses, combined with good conductors. I am not aware that in the interval which has elapsed since 1677, the Monument has ever been injured by lightning.

The damage done to Strasburg cathedral by lightning, was formerly almost every year such as to occasion considerable expense. Since the rather recent period at which it has been provided with a lightning-conductor, no damage has been sustained, and this item of expenditure has disappeared from the municipal budget.

On the 12th of July, 1770, lightning fell at Philadelphia, at one and the same time, on a sloop not provided with a lightning-conductor, on two houses equally undefended, and on a third house which was furnished with such an apparatus. At all these four points the detonation appeared tremendous. The two first-named houses and the sloop were seriously injured; the house which had a lightning-conductor remained perfectly unhurt; only it was remarked that the point of the rod was melted for some way down.

In 1813, in the month of June, at Port Royal in Jamaica, the ship "Norge," and a merchantman, neither being provided with conductors, were struck by lightning and much injured. The other vessels in the harbour, which were very numerous, and amidst which these two vessels were, sustained no injury: they were all provided with lightning-conductors.

In January, 1814, lightning fell in Plymouth harbour. Of the numerous vessels stationed there, one only, the "Milford," was struck and injured. it was the only one which at the moment was unprovided with a conductor.

In January, 1830, in the channel of Corfu, three terrible strokes of lightning fell on the conductor of the English ship the "Etna." The vessel suffered no injury. Two other vessels, the "Madagascar" and the "Mosquito," which were not far from the "Etna," were also struck. They had no lightning-conductors, and they sustained considerable damage.

CHAP. LIII.

DO LIGHTNING APPARATUSES HAVING SLENDER POINTED RODS ATTRACT THUNDERBOLTS?

I HAVE just proved that lightning does not damage buildings on which it falls, when they are provided with proper conductors. These apparatuses, if sufficiently multiplied, are almost certain preservatives. I know of no case in which their efficacy has failed, in which there have not been immediately discovered palpable errors of construction; I do not, however, wish to affirm the occurrence of very rare exceptions to be absolutely impossible. Although there cannot remain any serious doubts or difficulties as to the existence of a powerful action exercised by metallic rods, and especially by pointed rods, on the fulminating matter, either when contained in clouds or when it has already escaped from the cloud in the form of a zigzag flash of lightning, the case is different in regard to fulminating matter which may have assumed a globular form, and apparently have assimilated to itself ponderable substances. These exceptional cases, however, are so rare, that they do not deserve consideration in the point of view now under notice. Indeed, it is not on this account that any scruples are entertained respecting the use of lightning-conductors; but while not denying that they exercise influence, it is said that by their mode of action they attract the lightning and occasion it to strike a house provided with them oftener than it would have done if they had been absent.

This opinion was maintained by Nollet in 1764; Wilson was also a warm supporter of it; and as the protection from injury offered by the conductor was not deemed perfectly infallible, these two physicists considered that the multiplication of strokes supposed to result from the action of the point more than counterbalanced the good effects of the apparatus, which they inferred, therefore, to be more dangerous than useful.

Some surprise will probably be felt at my affirming that there are in the writings even of the most declared partisans of Franklin's invention, pretty evident indications of an opinion that lightning-conductors with pointed rods augment the number of strokes of lightning. But if this were not so, what would be the meaning to be attached to the following precept of Toaldo?—"With respect to powder magazines we should keep on the defensive; we should not place any points on the building, but content ourselves with bringing all the pieces of metal into communication with the conductor." This prejudice prevents many persons from having recourse to lightning-conductors, by a sentiment analogous to that which would induce them to keep at a distance from a thick parapet of earth, against which the fire of a battery should be incessantly directed, although the balls might always fall powerless against it. It is a prejudice however, which must be utterly done away with, if the facts reported in the preceding Chapter are examined with a little attention.

We see in the case of the church in Carinthia there spoken of, an average of four or five strokes of lightning annually, until a conductor is established; and afterwards, one stroke in five years.

In the church in Charlestown, the diminution is such, that in the course of fourteen years there is not a single stroke of lightning recorded; whereas, judging by what took place before the establishment of a conductor, six or seven strokes should have been observed.

At the Valentino palace, the lightning-conductors established by Beccaria caused the entire disappearance of strokes of lightning, which were previously of such frequent occurrence.

The Monument in London, although only accidentally provided with a virtual conductor, appears to have been exempt for nearly 180 years.

In 1814, at Plymouth, among a great number of vessels in the port, one only is visited by a stroke of lightning, and it is the one which is unprovided with a conductor.

Lastly, we have to describe a case, in which, according to Fontenelle's expression, Nature is taken in the fact:—

On the 21st of May, 1831, the "Caledonia" was sailing in Plymouth Sound, when a very violent thunderstorm took place. From the town the lightning was seen to strike the water at very moderate distances from the ship; it also fell on the shore and occasioned several accidents; but amidst all this the "Caledonia," armed with its conductors, was never touched, and sailed with the same security as under a serene sky.

The next fact is of rather a different kind. A country-house belonging to the family of the celebrated physicist Melloni, is situated near the village of Vallera, about two miles from the town of Parma; the belvedere of this house is overlooked, at a distance of less than two hundred feet, by the tops of oaks, elms, and ash trees, and also by the tower of the village church. The inhabitants of the neighbourhood do not remember either the house, the trees, or the church ever being struck by lightning prior to 1830, in which year a lightning-conductor was erected on the roof of the belvedere. In the summer of 1831, however, lightning struck the conductor with such force, that the copper-gilt point, which was a pretty thick one, was entirely fused, and the conductor much shaken.

Assuming the correctness of the belief entertained as to the previous absence of strokes of lightning, it may be said that the above account indicates that the metallic rod was the determining cause of the stroke; but, at any rate, it also shows that the apparatus, in which the conductor connected with the rods went down into a well always containing a certain quantity of water, answered its purpose perfectly, for the house received no kind of injury.

I have cited many cases, because in such questions numbers are essential. One or two facts, favourable or adverse, would have been of no importance. The cause of the curious influence which, I think I have shown, is generally exercised by lightning-conductors in actually diminishing the number of strokes, (in addition to rendering those which fell harmless), will be easily perceived on referring to Beccaria's experiments on the prodigious number of sparks which, during thunderstorms, the pointed rods on the Valentino silently withdrew from the clouds. However, whether clear or obscure in a theoretical point of view, the fact itself appears sufficiently established.

CHAP. LIV.

MEANS OF PROTECTING FROM STROKES OF LIGHTNING SUCH MONUMENTS AS THE COLUMN OF THE PLACE VENDÔME AND THE OBELISK OF LUXOR.

THIS question was warmly debated at the time when the obelisk of Luxor was covered in its upper portion with a small pyramid formed of a composition of artificial stone, intended to replace that which had been mutilated, either by the sudden action of lightning, or by the slower action of other meteorological agencies.

I will give a very brief view of the arguments brought forward on each side.

First, in regard to the column in the Place Vendôme. This column is covered for its whole height with a thick metallic casing; it may, therefore, be itself compared to a lightning-conductor of colossal dimensions. Suppose lightning to strike any part of the statue on the top, the fulminating matter will be instantly dispersed over the whole of the metal of the monument, and the intensity will thus be diminished in an immense proportion. On reaching the base of the column, the current, now scarcely sensible, will find an amply sufficient outlet in the damp stones of the pedestal and the pavement of the square. In this particular case, the erection of an intentional lightning-conductor would be useless.

We will now speak of the obelisk of Luxor. Suppose there be substituted for the small pyramidal mass with which the obelisk has been surmounted, a similar one of metal; and that to each of its angles, corresponding to those of the obelisk itself, there should be attached a metallic cord descending to the ground; an arrangement which would in no degree impair or affect the monumental appearance of the monolith, and would not conceal any part of the hieroglyphical inscriptions on its sides. In order to reply beforehand to all objections, suppose further that the four cords spoken of are prolonged through the masonry of the pedestal into the damp earth; in this way, all the conditions of a good lightning-conductor will be satisfied; and it may be affirmed that the obelisk will be fully secured from injury by thunderstorms, however violent.

The adequacy of such an arrangement was not contested; only it was said that the monolith might by its mass dispense with any artificial protection; not reflecting that, even if the damage it might suffer were limited to the shivering off of some small fragment from this ancient monument, such an occurrence might be highly unfortunate both for art and for future archeological studies. To reason from the mass of the obelisk that there is no danger, is to forget facts which science has recorded (as in the case of the rock, upwards of a hundred feet long and ten feet broad, torn up by a stroke of lightning in Scotland, about the middle of the last century); and to set aside altogether the popular belief which, according to M. Merimee, attributes to a stroke of lightning the fall and fracture of the fragments of the great Menhir of Locmariaker. It should be remarked that the joint weight of these two fragments is above 246 tons.

CHAP. LV.

PHENOMENA PRODUCED BY ARTIFICIAL ELECTRICITY, AND THEIR RESEMBLANCE TO PHENOMENA PRODUCED BY FULMINATING MATTER.

YELLOW amber, when rubbed strongly, attracts light bodies, such as down or light feathers, straws, and saw-dust. Theophrastus among the Greeks, and Pliny among the Romans, had remarked this property, but without appearing to attach to it more importance than they would have done to any mere accident of form or colour. They had no idea that they had thus actually touched the first link in a long chain of discoveries. They failed to recognise the importance of an observation from which the moderns have drawn a whole world of facts, as curious by their singular character, as they are important in the results which have been deduced from them. They have been called electric phenomena, from the word *electron*, by which the Greeks designated amber.

Terrestrial substances or bodies may be divided into two classes, in respect to the possibility of developing electricity in them by friction. Thus glass, resin or sealing-wax, amber, &c. easily become electric by friction. Dufay, member of the Academy of Sciences at Paris, recognised that there are essential differences in electricity developed at the surface of glass, and that developed under analogous circumstances at the surface of resins. The first of these electricities is called vitreous or positive electricity; and the second, resinous or negative electricity.

Suppose a wand in which resinous electricity is developed, to be placed near another in which vitreous electricity has been excited, and you will instantly see fire dart from the one wand to the other, with the remarkable peculiarity that the flash in question, instead of being in a straight line, affects a decided zigzag form. The same phenomena will present themselves, but with rather less intensity, when, the distances remaining the same, a non-electrified wand is brought in presence of one either positively or negatively electrified.

If one of these wands terminates in a point, and it is this point which is presented to the electrified wand, the latter loses electricity; but in this case the luminous manifestations are much less distinctly marked. We find all these same circumstances, point by point, in the phenomena presented by natural fulminating matter, or lightning; as for example, in the experiment of Beccaria, related in p. 230., in which the very peculiar power of points is evidenced.

By the aid of artificial electricity we are able to fuse metallic wires, of greater or less length and thickness according to the strength of the machine employed.

These phenomena, viewed in combination and in their details, resemble perfectly the phenomena of fusion produced by lightning, described with numerous details in the preceding Chapters (XVIII. XX. and XXI.).

A flash produced in a mass of air acquires exactly the same properties, whether it originates spontaneously in the fluid, or whether it arises from particular combinations; thus in both cases the same odour will be developed, the fusion operated will be the same, and the metallic plate struck by the flash will be equally perforated by one or by two apertures, &c. &c. There is only one circumstance in which the physicist cannot imitate Nature;—he cannot produce globular lightning—he cannot form those spherical agglomerations which move slowly and yet do not lose the property of fulminating. There is in this point an hiatus in science, which it would be very important to supply. Meanwhile, however, and whatever may be the result of investigations to be made on this subject, there is at the present time one fact which is perfectly well established, *i. e.* that ordinary electricity and artificial electricity are generally one and the same thing.

M. Muncke relates, that an unusually strong man having accidentally received through his arm and chest, the charge of a battery of only six feet square of surface, fell senseless, and remained in a state of complete insensibility for an hour.

Franklin found that artificial electricity, accumulated in two jars of the capacity of about six gallons, is sufficient to kill a turkey.

These two instances, regarded in connection with the numerous cases in which we have seen men and animals killed by the matter or substance of lightning, show the analogy, or rather, the perfect identity of the two substances.

CHAP. LVI.

OF THE PART ASSIGNED TO THUNDER AND LIGHTNING IN THE ECONOMY OF NATURE.

When we spoke in Chapter XVII. (p. 65.) of the chemical modifications which lightning impresses on atmospheric air, we said that the experiments of Cavendish, executed on a small scale in the laboratory, on the production of nitric acid from the nitrogen and oxygen of the air combining under the influence of electricity, give reason to suppose that lightning, in traversing vast extents of atmosphere, may produce the same acid. We added, that some analyses of rain which had fallen during thunderstorms, made by Liebig, had shown the truth of this conclusion. More recently, M. Barral having analysed, month by month, the water collected from all the rains which had fallen during two years at the Observatory at Paris, has given a higher idea of the important part performed by the matter of lightning in its passage through the regions of the atmosphere. M. Barral constantly found nitrate of ammonia in the rain-water furnished by each separate month of the two years, including some months in which there was no thunder at Paris. This result is no way opposed to the electric origin of nitrate of ammonia *; for it follows from the results collected in an earlier part of the present volume, that there is probably not a single day in the year on which there have not been thunder and lightning in some part of the globe. Now, the clouds which dissolve in rain at Paris, have passed over regions of which we have no right to limit the extent. When we consider the important office which ammoniacal salts perform in vegetation, we may not be far from thinking that the explanation of the benefit of fallows is connected with the passage of the matter of lightning through the atmosphere, whether it is disengaged slowly without giving out light or visible sparks, or whether it is accompanied by thunder and lightning.

We have seen that there are localities where thunder is much more frequent than at other places not far distant from them. It is also known that natural deposits of nitre, and of saltpetre in caves, are only met with in particular soils. It would be interesting to examine whether places where saltpetre forms, in soils which contain the alkaline earths necessary to its composition, are not under some special conditions relatively to the disengagement of atmospheric electricity; so that, for example, thunder and lightning may take place there with exceptional frequency or violence.

* Ammonia, which, as is well known, is formed of hydrogen and nitrogen, may itself proceed from the same electric cause, which in decomposing atmospheric air, would yield hydrogen in the state which chemists call *nascent*, and which is especially well suited for its combination with the nitrogen of the air.

CHAP. LVII.

ON THE THEORY OF LIGHTNING.

We think we have been able to show the identity of the common electricity of our laboratories with atmospheric electricity. But there remains to be explained from whence is derived the immense quantity of fulminating matter which circulates so abundantly during thunderstorms through all bodies, and which accumulates in certain clouds to explode from them with such various effects. This subject is worthy of the attention of all friends of science, and we have never failed to place it among the objects to be more especially regarded in instructions given to voyagers and travellers, and to meteorologists. In the course of the present notice, I have been careful to call attention to numerous points of the globe at which a greater number of observations are much needed. I will only add here an indication of some particulars which we deemed it proper to recommend both to the officers of the "Bonite," in her voyage of circumnavigation performed in 1836 and 1837, and to the scientific expeditions to the North Cape and Iceland, and to Algeria.

§ 1. *Places where it never thunders.*

I have said that there are probably parts of the ocean where it never thunders. In Norway it is asserted, that thunderstorms become more rare in receding from the sea-coast. Some travellers even consider that there are notable differences, in this respect, between the entrance and the bottom of the great fiords by which the country is intersected. The subject is well deserving of the attention of meteorologists.

§ 2. *Electricity near Cascades.*

In 1786, Tralles found near the Staubach, that the exceedingly minute rain which detached itself from the Fall, gave evident signs of negative electricity. The Reichenbach Fall presented the same phenomena. Volta, a short time afterwards, verified the exactness of Tralles's observation at the Pissevache, and also at many minor cascades, wherever the fall of water, however insignificant, gave occasion to the dispersion of minute drops of spray carried away by the wind. In all these cases Volta, as Tralles had done, found the electricity, as it appeared to him, negative.

At first the Berne physicist attributed the electricity of the fine spray or aqueous dust which surrounds all large waterfalls to the friction of the minute droplets upon the air. Soon afterwards he recognised with Volta the evaporation, which these droplets undergo in falling, as being the true cause of the electricity. This explanation has been combated by Professor Belli. Without denying that the evaporation may have some effect on the phenomenon, Mr. Belli attributes the principal share in its production to the action which the electricity of the atmosphere must be supposed to exercise on running water. He says the water will be, by influence or by induction, in a negative state whenever the atmosphere is charged with positive electricity, as is ordinarily the case. At the instant when this water separates into thousands of minute drops, it cannot fail to carry to the objects with which it may come in contact, the electricity which the induction from the atmosphere has given to it.

Professor Belli's theory admits of a test which will at once *demonstrate* its correctness, or the contrary. If it is correct, the electricity of the cloud with which waterfalls are surrounded will not always have the same sign; it will be negative if that of the atmosphere is positive, and, on the other hand, will be found to be positive when the clouds may be negative. It must, therefore, be by means of observations made during stormy, and not during serene, weather, that we shall be enabled to choose between the theory of Volta and that of Belli.

§ 3. *Explanation of the removal of bodies occasioned by lightning.*

We have had occasion to cite in a previous Chapter some experiments by M. Fusinieri, who has studied the effects of thunder and lightning under an entirely novel point of view.

According to this physicist, the electric sparks which proceed from ordinary machines, and which we see traverse the air, contain brass in a state of fusion and incandescent molecules of zinc when they emanate from a brass conductor, and in like manner they contain impalpable particles of silver when they proceed from a silver ball. In the same way a sphere of gold produces sparks, which, in passing through the atmosphere, contain gold in a state of fusion, &c. &c.

In the centre of all these sparks there are molecules which are only fused, but on the external contour the metallic particles undergo a more or less powerful combustion from their contact with the oxygen of the atmosphere.

When a spark proceeding from a gold ball traverses a plate of silver, even though it may be a pretty thick plate, there are perceived on both surfaces of the latter, at the points where the electric jet enters and issues, a circular layer of gold, which must no doubt be of very small thickness, since, after some time, the natural volatilisation suffices to cause it to disappear. According to Fusinieri, these two metallic stains or layers are formed out of the gold in a state of fusion contained in the electric spark. There would be nothing extraordinary in this being the case with the stain on the first surface where the jet enters; but if we adopt this explanation for the stain formed on the further side where the jet issues forth again, we shall be obliged to admit that the gold disseminated in the primitive spark has, at least in part, traversed, in association with the spark, the whole thickness of the silver plate.

It is scarcely necessary to add that a spark coming from a copper ball gives occasion to analogous phenomena.

Not only does a spark which emanates from a particular metal, in traversing another metal, part with a portion of the molecules with which it was first charged, but it moreover takes up from the second metal fresh molecules. Fusinieri even affirms that at every passage of the spark there are effected mutual exchanges between the two metals which are in presence; that if, for example, the spark goes from silver to copper, there is not only a transfer of the silver on to the copper, but also a transfer of the copper on to the silver. I will not insist further on these phenomena, which, indeed, I have only cited in this place with the view of showing that the sparks of our ordinary electrical machines contain ponderable substances.

Fusinieri believes that similar substances exist in lightning, and that they exist there also in a state of great subdivision of ignition and of combustion. According to this physicist, these transported substances are the true cause of the transient odours left by lightning wherever it explodes, as well as of the pulverulent deposits which are found around the fractures through which the electric matter has forced a passage. In these deposits, which have been too little regarded by observers, Fusinieri has found metallic iron, iron in different degrees of oxydation, and sulphur. The ferruginous spots or stains left on the walls of houses might, perhaps, be supposed to be derived from the iron which the lightning might have taken up from the portions of that metal always found in buildings? but what can be said of the sulphurous stains on the same walls, and still more, of the ferruginous stains on trees which have been struck by lightning in the open country? M. Fusinieri therefore thinks himself justified in inferring from his experiments, that at all elevations, or at least up to the height of storm-clouds, the atmosphere contains iron, sulphur, and other matters of which chemical analysis has not yet determined the character, and that these substances are taken up by the electric spark and brought by it to the surface of the earth, where they form extremely thin and minute deposits around the points which are struck by the lightning.

Assuredly this new mode of regarding electric phenomena is well deserving of being followed up with all the exactness of which the present state of science is susceptible. All persons, therefore, who may see lightning strike an accessible point will do a very useful thing by carefully collecting the matter, black or coloured, which the electric fluid appears to have deposited in those parts of its course where abrupt changes of velocity must have taken place. A scrupulous chemical analysis of these deposits may lead to unexpected and highly important discoveries.

ELECTRO-MAGNETISM.

CHAPTER I.

ON THE RESEARCHES MADE IN FRANCE WITH THE VOLTAIC PILE.

THE first few words written by me on electricity, were suggested by a passage in the "Bibliothèque Universelle de Genève," on the occasion of the galvanic experiments of Mr. Children; and were inserted in the "Annales de Chimie et de Physique" for 1816. I repeat them here, for the sake of showing how unfounded was the reproach addressed to a great nation such as France, of having neglected to derive from new discoveries the advantages they were capable of affording for the continual advance of science.

"The editors of the 'Bibliothèque Universelle,' in their February number for 1816, have prefixed to the account they have given of the galvanic experiments which Mr. Children has published in the last volume of the 'Philosophical Transactions,' a paragraph to the following effect:—

"'A rather considerable sum was granted in France, a few 'years ago, for the construction of a voltaic apparatus, to be 'placed at the disposal of the most skilful chemists. Much 'might have been hoped for from such an endowment, but its 'effect seems to have been limited to the kind of moral galvanism, 'or action on public opinion, so much aimed at by the then head 'of the French Government,' &c. 'We have not *heard* that 'science has reaped any more advantage from it, than industrial 'art has done from the million promised to the inventor of the 'best spinning machine for hemp or flax. It is not from without, '—from any such external stimulus,—that one can look for the 'vivifying principle by which genius acts, and discoveries are 'made; it is in the mind, the personal character of the individual, that this sacred fire,' &c. &c. 'Thus, a private individual 'in London, Children,' &c. &c.

"In this passage, the experiments of Gay Lussac and Thenard, to whom the great pile of the Ecole Polytechnique was confided, are totally ignored. The new title given to the journal ('Bibliothèque *Universelle*,' previously 'Bibliothèque Brittanique'), might have led to the hope of greater impartiality and more general knowledge. Since, however, its editors have never *heard* that science had derived any advantage from the apparatus in question, I have the pleasure of calling their attention to a work in two volumes, published in 1818, by Messrs. Gay Lussac and Thènard, entitled 'Recherches physico-chimiques faites sur la Pile,' and of pointing out to them a very long chapter which it contains on the causes which occasion the energy of a galvanic battery to vary; the measure of its effects; the influence exercised by different liquids employed in the troughs, or in the receiver; the variation of intensity dependent on the number and surface of the plates, &c. It is not for me to assign the degree of merit which appertains to these researches, but I may say that I have no doubt that the editors of the journal would have better satisfied the wishes of their readers in all countries, if they had substituted for the paragraph which has called for these remarks some details, on the effects to be expected from piles of large dimensions; on the circumstances of construction which render them suitable to produce such or such phenomena; on the length of time during which their effects last; the expenses of maintaining them in action, which are considerable; the small quantities of re-agents which they furnish, &c. &c.; and if they had recalled the fact, that, under some circumstances, the well-directed action of ordinary chemical agents produces effects which cannot be obtained by galvanism; and here it would have been natural to have introduced a notice of the unsuccessful attempts made by the celebrated Davy to decompose boracic acid by means of the pile, and of the purely chemical proceedings which conducted Gay Lussac and Thènard to success in that important discovery, &c. Since it was considered that Mr. Children's interesting memoir could not dispense with a preamble, it would, I think, have been an act of simple justice to have noticed, that the influence of large surfaces in the elements of the pile had been already pointed out and appreciated in France, more than ten years before, in a memoir by Messrs. Thènard and Hachette, of which an extract was printed in the eleventh cahier of the

'Journal de l'Ecole Polytechnique,' &c. The examination of all these questions would, it is true, have required some laborious research; but it is but right that those, who assume a sort of magisterial office on subjects of science, should take the pains of studying those subjects, and should not distribute praise and blame merely according to what they happen to learn by 'hearsay.' I am also somewhat tempted to ask the editors where they heard that the study of physical science had fallen into great disrepute in France (vide No. 2. p. 85.); but at present I will only assure them that they are ill-informed in supposing that no useful result has been gained by the labours of those of our mechanicians who have occupied themselves with the spinning of flax. M. Molard, whose authority I am sure they would admit, would have given them more correct information on this subject."

The success of the efforts of Philippe de Girard for this object is at the present time well known. In regard to the employment of the voltaic pile, is it not in France that almost all the phenomena which enable us to reduce magnetical laws to those of electricity have been recognised? May not Ampère be said to have created the science of electro-dynamics?

CHAP. II.

MAGNETISATION OF IRON AND STEEL BY THE ACTION OF THE VOLTAIC CURRENT.

In the reports of the meetings of the Bureau des Longitudes, there is the following notice, dated 20th September, 1820: "M. Arago spoke of a new experiment, from which it follows that the Voltaic pile imparts magnetism to soft iron."

On the 25th of September, 1820, I gave an account of my experiments to the Academy; this being several months before Sir Humphry Davy read a memoir on the subject to the Royal Society of London. The "Moniteur" spoke of the discovery which I had made in the following terms: —

"M. Arago announces having remarked that the connecting wire which establishes the communication between the two poles of Volta's pile takes up, and becomes charged with, iron filings, as a magnet would do. It follows that this wire does not act solely on needles already magnetised, but that it also developes magnetism in iron not previously magnetised. Thus compass needles not yet magnetised are deflected, or drawn aside, by the action of the connecting wire."

My experiments had for their point of departure the brilliant discovery of Oersted, which was communicated to me in 1810, at Geneva, by Pictet. This discovery, however singular its results might appear, could not leave any doubts in the minds of philosophers; nevertheless, I was very happy to profit by the kind offer of M. de la Rive (who has himself discovered some curious phenomena with the powerful Voltaic piles in his possession), and to assist at the verification which he made in his laboratory at Geneva of Oersted's experiments, in presence of Messrs. Prévost, Pictet, de Saussure, Marcet, de Candolle, &c. I was thus enabled to convince myself of the exactness of the principal results obtained by the Danish philosopher, viz., 1st, that a metallic wire in communication with the two poles of the pile acts on a magnetised needle. 2ndly, that the nature of this action depends, if not on the position of the pile, at least on the direction in which the positive and negative fluids move in the conducting wire relatively to the poles of the needle. 3rdly, *that if the conducting wire is placed below the needle, it produces a deviation in the opposite direction to that which it produces when it is above the needle.* In performing the experiments M. de la Rive sometimes kept the needle alone beneath the receiver of an air-pump, and sometimes placed both the needle and the conducting-wire under the receiver: the results were always the same.

Oersted had only discovered the action exercised by the Voltaic current on a needle which had been *previously magnetised.* In repeating the experiments of the Danish physicist, I recognised that the Voltaic current also occasions a strong development of *magnetic virtue* in iron or steel which was previously quite destitute of it. I explained my discovery in 1820, in the "Annales de Chimie et de Physique" (vol. xv. p. 94. *et seq.*) in the following manner: —

I said:—" I will report the experiments which establish this result nearly in the order in which they were made.

" Having adapted a rather fine cylindrical copper wire to one of the poles of the Voltaic pile, I remarked that the instant the wire was in communication with the opposite pole it attracted soft iron filings as a magnet would have done.

"The wire being plunged into the filings, drew them to itself equally all round until by their addition it had become almost the size of an ordinary quill.

"The moment the wire ceased to be in communication with both the poles of the pile *at the same time*, the filings detached themselves from it and fell off.

"These effects did not depend on any previous magnetisation of the filings, since soft iron or steel (not in communication with the poles of the pile) did not attract the slightest portion of them.

"As little could the effects be explained by attributing them to any ordinary electric action; for on repeating the experiments with filings of copper or of brass, or with saw-dust, these substances are in no case found to attach themselves to the connecting wire in any sensible manner.

"The attraction which the connecting wire exercises on iron filings diminishes very rapidly as the action of the pile becomes weaker. Possibly it may hereafter be found that the weight of the quantity of filings lifted by a given length of wire may afford a measure of the energy of the pile at the different epochs of the same experiment.

"The action of the connecting wire on iron is such as to be exercised at a distance, for it is easy to see that the filings are lifted much before the wire is in contact with them.

"I have thus far spoken only of a wire of copper or brass, but wires of silver, platinum, &c., give analogous results. It would still remain to be examined whether with the same form, mass, and diameter, wires of different metals would act with exactly the same degree of intensity.

"The connecting wire only imparts a momentary magnetism to soft iron; if small particles of steel are used they sometimes become permanently magnetised. I have even succeeded in this manner in rendering a sewing needle permanently magnetic."

The connecting wire is, as we see, endowed with a very intense magnetic virtue so long as it is in communication with both the poles of the pile. It has more than once happened to me to find in it some traces of this property for some moments after the communication between the poles has been completely broken; but this is a very fugitive phenomenon, and I have not been able to reproduce it at will. M. Boisgiraud, who has occupied himself with the same question, has not been more fortunate, although in one particular case the platinum wire which he was employing preserved sufficient force, after having been quite disjoined from the pile, to support a small sewing needle.

CHAP. III.

MAGNETISATION OF A NEEDLE BY MEANS OF THE PASSAGE OF THE ELECTRIC CURRENT IN A HELIX.

Ampère, to whom I showed the experiments related in the last Chapter, had just made the important discovery that two rectilinear and parallel wires, along which two electric currents are passing, attract each other when the currents are moving in the same direction, and repel each other when the currents are moving in opposite directions. He had, moreover, deduced therefrom, by analogy, the inference that the attracting and repelling properties of magnets depend on electric currents, which circulate around the molecules of iron or of steel in a direction perpendicular to the line joining the two poles of the magnet. Ampère further supposed that in a horizontal needle pointing towards the north, the current in the upper part moves from west to east. These theoretical views instantly suggested to him the thought that a stronger degree of power would be obtained by substituting for the straight connecting wire which I had employed, a wire coiled as a helix in the central cavity of which the steel needle should be placed; he hoped, moreover, that there would thus be obtained a constant position of the poles, which would not be the case in my method. We both, Ampère and myself, submitted these conjectures to the test of experiment, in the following manner:—

A coil of copper wire forming a helix, was terminated at the two ends by straight portions, which could be adapted at pleasure to the two opposite poles of a powerful horizontal voltaic pile. A steel needle, wrapped in paper, was introduced into the cavity of the helix, but only after the communication between the two poles had been established, in order that it might not be possible to attribute the effect which was expected, to the electric discharge, which manifests itself at the instant when the connecting wire is brought into contact with the two poles. During the course of the experiment, the portion of the wire within which the steel needle was enclosed, was kept always perpendicular to the magnetic meridian; so that there was nothing to be apprehended from the earth's action.

The result was, that, after remaining within the helix for some minutes, the steel needle had become pretty strongly magnetic; and, at the same time, the position of the north and south poles was perfectly conformable to the result which Ampère had deduced beforehand, from the arrangement of the helix, and from the hypothesis that the electric current passes along the connecting wire by proceeding from the zinc to the copper extremity of the pile.

It would seem, then, to be proved by these experiments, that if a steel wire is magnetised by a galvanic current, which passes along it longitudinally, the position of the poles is not determined solely by the direction of the current; and that slight, and almost inappreciable, circumstances—such, for instance, as a feeble commencement of magnetism, or a slight irregularity of form or texture, may quite change the results; but that if the galvanic current circulates round the steel along the spires of a helix, it will always be possible to say, beforehand, where the north and south poles will be found to have placed themselves.

On reflecting, however, on the singular discordances which experiments of magnetisation by *electric discharges* have presented to physicists who have occupied themselves with these researches, it appeared to me to be necessary to submit the phenomena of currents in helices to more decisive tests. The reader will judge whether we attained this object.

I first thought of forming two symmetrical* helices of copper

* These symmetrical helices are like those which botanists denominate *dextrorsum*, or right-handed, and *sinistrorsum*, or left-handed. Their diameters are equal, and the spirals of which they consist are inclined at the same angles, but they never can be superposed in whatever manner they may be presented to each other; so that no reversal can occasion them to change their kind. The right-handed helix is that which we see in nature in a great number of climbing plants: it is also almost the only one employed in the arts.

A cylinder of steel which is enclosed in a right-handed helix acquires an austral pole (meaning thereby the pole which points towards the north), on wire, separated by a rectilinear portion of the same wire. The coils or spirals of one of these helices turned in one direction, and those of the other in the contrary direction, but the inclinations were the same in each helix, and the diameters were equal, about two English inches. A thin steel cylinder or wire, enclosed in a small glass tube, was placed inside the first helix. I afterwards placed another cylinder or wire, perfectly similar to the first, and similarly protected from any electric discharge by a glass casing, in the adjacent helix; a small end of the copper wire established a constant communication between this latter helix and the positive pole of the pile. In order to commence the experiment, therefore, it was only necessary to attach to the negative pole the wire which proceeded from the extremity of the second helix. The moment this communication was formed, the electricity accumulated at the positive pole of the instrument, flowed off by the straight portion of the connecting wire, reached the first helix, gradually followed all its coils, arrived at the second helix by the straight piece of wire between it and the first helix, and having passed along the coils of the second helix, went to the negative pole. Therefore the two steel cylinders were each subjected, during the experiment, to the action of a galvanic current of the same degree of strength; this current was moving bodily in one direction only, but whereas it circulated from left to right round the first cylinder, the same movement was executed from right to left round the second cylinder. Now, in all the experiments of this kind which we made in Ampère's cabinet, with a pretty strong pile which he possessed, this simple change in the direction in which the current circulated around the steel cylinders, was sufficient to occasion a complete inversion of the poles; so that the two cylinders enclosed in the two symmetrical helices were, at the same instant, magnetised in opposite directions.

the negative, or copper, side of the connecting wire; whereas the same pole will be formed on the positive, or zinc, side, if a left-handed helix is employed. These results are in conformity with Ampère's theory.

CHAP. IV.

CONSECUTIVE POINTS PRODUCED IN MAGNETISING STEEL CYLINDERS BY CURRENTS CIRCULATING IN A HELIX.

I COILED copper wire for a length of two inches from right to left into a helix; then an equal length of wire in the same manner from left to right; and, lastly, a similar quantity again from right to left. These three helices were separated from each other by rectilinear portions of the same wire.

One and the same steel cylinder of a suitable length, and of rather more than ·04 of an inch diameter, and enclosed in a glass tube, was inserted in the three helices at once. The galvanic current, in passing along the coils of these different helices, magnetised the corresponding portions of the steel cylinder as if they had been detached and separate from each other: for I remarked that at one of the extremities there was a north pole; at two inches distance a south pole; farther on a second south pole, followed by a north pole; lastly, a third north pole, and, two inches farther on, or at the other extremity of the cylinder, a south pole. Thus, by this method, the number of these intermediate poles, which physicists have denominated consecutive points, could be multiplied at pleasure.

I ought to remark, however, that in general in these experiments the influence of the helices is exercised, not only on the portions of the steel wire or cylinder enclosed in them, but also on the adjacent portions; so that, for example, if the intervals comprised between the consecutive helices are small, the parts of the steel wire, corresponding to those intervals, will themselves be magnetised, as if the movement of rotation impressed on the magnetic fluid, according to Ampère's idea, by the influence of a helix, was continued beyond the extreme spires of the coil.

Having sought to discover what were the circumstances which caused the position of the poles to vary when steel wires or cylinders were passed over longitudinally by a galvanic current, I found, invariably, even with a very active pile, that if the connecting wire is perfectly straight, a steel cylinder placed over it received no magnetism from it. The sewing needle which I had employed in my first experiments had, it is true, acquired poles; but at that time the dependence of the effects on the form of the connecting wire was not known; and, for the purpose of keeping the needle in its place more easily, I had rolled the wire a little round its extremities, thus forming a coil.

CHAP. V.

ON THE PRINCIPLE OF ELECTRIC TELEGRAPHS.

It has been seen above that, in 1820, immediately after the publication in France of Oersted's Memoir, I proved that the connecting wire develops magnetism in cylinders or wires of iron or steel without being in contact with them.

For this to be effected the wire must be placed transversely to the current.

I may add here that Ampère and I assured ourselves that the magnetism developed by the connecting wire is very strong where it is made to circulate round a spiral coiling at a distance, and several times, round the wire which it is desired to magnetise.

Although the experiments which established the correctness of this result were made conjointly by my friend and myself, yet I must declare that it was Ampère who, guided by his theoretical ideas, conceived the possibility of this augmentation of force.

The momentary development of magnetism in a mass of soft iron, by the action of the voltaic current, is the principle on which the mode of action in the greater number of electric telegraphs rests.

CHAP. VI.

PROJECTED EXPERIMENT ON THE MAGNETISM OF ELECTRIC LIGHT.

At the conclusion of the preceding memoir on the magnetisation of iron and steel by the action, at a distance, of the voltaic current, I made the following remarks:—

"There is at the Royal Institution of London a voltaic pile of 2000 pairs of plates of four inches square. By the use of this powerful apparatus Sir Humphry Davy has found that an electric discharge is produced between two charcoal points fitted to the extremities of the positive and negative conductors, even when these points are distant from each other two or three-hundredths of an inch. The first effect of the discharge is to bring the charcoal to a red heat; as soon as the incandescent state is established, the points may be gradually removed to a distance of four inches apart without the intermediate light being interrupted. This light is extremely vivid, and broader in the middle than at its extremities: it has the form of an arc of a circle.

"The experiment succeeds better in proportion to the rarefaction of the air. Under a pressure of a quarter of an inch the discharge from one charcoal point to the other commenced at a distance of half an inch; afterwards, by gradually moving the points further apart, Sir Humphry Davy obtained a continuous purple flame seven inches in length.

"It is no doubt very natural to suppose that an electric current alone will act on the magnetic needle quite as if it was moving along a connecting metallic wire. Nevertheless, it seems to me, that an actual experiment on the point deserves to be recommended to physicists who have very powerful voltaic piles at their command, particularly on account of the views it may suggest relatively to auroras. Moreover, independently of all immediate application, would not the production, in a vacuum, or in very rarefied air, of a flame, which acting on the magnetic needle should be alternately attracted or repelled by the poles of a magnet, be a phenomenon well worthy of attention?"

The experiment which I thus commended to the attention of the scientific world was made some time after the publication of the above note by Davy, and was subsequently repeated, with particular care, by M. de La Rive, of Geneva, who thinks that the magnet, when placed in presence of the luminous arc, does not repel or attract the electric current itself, but only the particles of carbon which are transported from one pole to the other, and are traversed by the current.

CHAP. VII.

MAGNETISATION BY THE ACTION OF ORDINARY ELECTRICITY.

The Report, or "procès verbal," of the meeting of the Academy of Sciences of the 6th of November, 1820, records, "that I announced verbally that I had produced, by the employment of ordinary electricity, all the phenomena of magnetisation, which I had previously observed with voltaic electricity." The "Moniteur" of the 10th of November mentions my experiments in the following terms: —

"M. Arago announced that he had magnetised steel wires, by placing them, enclosed in glass tubes, within helices of wire along which electric sparks were made to pass; thus presenting a fresh analogy between the modes of action of ordinary and of voltaic electricity. In this experiment, the north and south poles were formed at the one or the other extremity of the wires, according to the direction of the current, and that of the coils of the helix. M. Arago produced as many consecutive points as he made changes in the direction of the helix relatively to the length of the wire, just as he had before done when using the voltaic pile. He, moreover, remarked, that the helix ceased to exercise any action on the steel wire when the latter was placed outside the helix, even when in contact with it."

It will be seen that these phenomena, in which magnetisation is obtained by action at a distance, are to be carefully distinguished from the experiments, which did not yield accordant results, which were formerly made by Wilke*, Franklin†, Alibard‡, Beccaria§, Van Swinden‖, and Van Marum¶, on the magnetisation of steel needles, through which they caused the spark to pass.

* Memoires de l'Academie de Swède, tome xxviii.

† Franklin "On Electricity," &c. p. 91. edit. of 1769.

‡ "Expériences sur l'Electricité," traduites par d'Alibard, tome ii. p. 135. 144, 145.

§ Beccaria, "Dell' Electricismo Artificiale," §§ 731, 732, 733.

‖ Van Swinden, "Analogie de l'Electricité et du Magnetisme," tome i. p. 492. *et seq.*

¶ Van Marum, "Description d'une très grande Machine Electrique," &c., tome i. p. 168, *et seq.*

Franklin speaks of the magnetisation produced by the electric discharge, in a letter dated 27th of July, 1750. He used, in these experiments, sewing needles, *through which* he passed the discharge proceeding from four large glass jars. His results were the following:—

With a given discharge, most magnetism is obtained when the needle is placed north and south, and the minimum when it is east and west.

If the needle is placed east and west at the moment of the discharge, the end by which the electric fire enters will turn to the north when the needle is suspended.

If the needle is placed north and south at the moment of the discharge, its north end will still be to the north when it is suspended, whether the spark entered at that end or at the opposite extremity.

If this last result were correct, electricity could never change the poles of a compass needle when it is in its natural position. We know, however, that lightning does produce that effect.

In the notice on Thunder and Lightning (chap. xxv. p. 91.), I spoke of magnetisation by lightning. I will add here two facts which were not mentioned there.

Lightning fell on a clockmaker's shop at St. André, in Dauphiné, in August, 1739, and broke a file into two pieces four inches from the end, so that there still remained seven inches of it in the handle. The part detached had become sufficiently magnetic to lift up keys, and to communicate magnetism to a knife.

The fragment of $4\frac{1}{2}$ inches in length was afterwards broken into two, when one of the two portions was found to attract iron at either extremity; the other (the one in which the point of the file was), only at the broken end. (*Phil. Trans.* vol. xli. pp. 614, 615.)

Many physicists have remarked that a piece of steel becomes magnetic by being broken; therefore it could not be concluded from the present fact, if it stood alone, that lightning is in itself capable of magnetising steel.

Franklin, in a letter dated 27th of July, 1750, speaks of an account written by Captain Waddel, of the effects produced on board his ship by a stroke of lightning; some of his compass needles *had lost all their magnetism;* in others the poles were reversed, the north end turning to the south.

CHAP. VIII.

ON ROTATION MAGNETISM.

THE first publication which I made of this discovery is thus mentioned in the *procès verbal* of the sitting of the Academy of Sciences, on the 22nd of November, 1824:—

"M. Arago communicated verbally the results of some experiments which he had made on the influence which metals, and many other substances, exercise on the magnetic needle, the effect of which is to produce a rapid diminution of the arc of vibration of a needle without sensibly affecting its time of vibration."

While engaged with my friend Alexander von Humboldt, in 1822, on the slope of Greenwich Hill, in a determination of the intensity of the magnetic force, I had recognised that the horizontal needle, after being put in motion, came to rest much sooner when placed in its box than when suspended at a distance from all foreign bodies. It had appeared to me that this remark ought to conduct to important inferences on the generality of phenomena which had till then remained circumscribed and as it were isolated in the midst of science. I never ceased turning over this class of ideas in my mind; and even now, when I no longer see, and when I am no longer able to observe, it seems to me that there are many researches still remaining to be essayed in the path which I have opened, notwithstanding the apparently satisfactory explanation which Faraday has given of a part of the phenomenon which I have discovered.

On the 7th of March, 1825, I made to the Academy of Sciences a fresh communication on this subject, which is reported in the following manner in the "Annales de Chimie et de Physique" (t. xxviii. p. 325.):—

"M. Arago placed before the Academy an apparatus which exhibits under a new form the mutual action which magnetic and non-magnetic bodies exercise on each other.

"In his earlier experiments M. Arago had shown that a plate of copper, or any other substance, solid or liquid, placed under a magnetic needle, exercises on that needle an action of which the immediate effect is to lessen the arc of vibration of the needle without sensibly altering the time of vibration. The phenomenon which he has now brought before the notice of the Academy is, so to speak, the converse of the preceding one. Since a needle in motion is brought to rest by a plate which is itself at rest, M. Arago thought that it should follow that the movement of a plate in motion would draw after it a needle previously at rest. He found, that if a plate of copper, for example, is made to turn, with a determinate velocity, beneath a magnetic needle enclosed in a case surrounding it on all sides, the needle no longer places itself in its ordinary position; it rests in a position different from the magnetic meridian; and so much the more distant from the plane of that meridian, as the movement of rotation is more rapid. If this movement of rotation of the plate is sufficiently rapid, the needle, though quite at a distance from the plate, also turns continuously round on its point or centre of suspension."

After the publication of my discovery, and of my experiments which I repeated before a great number of persons, several English, Swiss, and Italian physicists studied the same phenomena. Their researches in general confirmed my results. Yet in the Number of the "Bibliothèque Universelle" for January, 1826, there is a memoir by Messrs. Leopold Nobili and Bacelli of Modena, containing several experiments in direct opposition to some of mine, and which would tend to cause it to be admitted that it is not true that all bodies in Nature exercise a particular and very intense action on a magnetic needle in motion. The acknowledged merit of those gentlemen imposed on me the duty of not leaving their statements unanswered. I have refuted their experiments in the Thirty-second volume of the "Annales de Chimie et de Physique" (p. 213. 1826); and I will presently repeat my explanations on this subject. Meanwhile I have to repel certain objections which at the time I believed to proceed from Dr. Brewster. What I then wrote still remains; but I no longer address the language then used to that illustrious physicist, in whom I have since recognised a true love of science, and who has become a foreign associate of the Academy, and my friend.

Those who discover a new fact in the sciences of observation must expect, first, to have its correctness denied,—next its importance and utility contested,— and afterwards will come the chapter of priority,— then, passages, obscure, insignificant, and previously unnoticed, will be brought forward in crowds as affording evident proofs of the discovery not being new. I had certainly flattered myself that I had escaped the latter difficulty, and that much less on account of the care with which I was conscious that I had sought in the writings of physicists for observations which might connect themselves with my experiments, than by reason of the flattering distinction which the Royal Society of London had deigned to bestow on them. On reading the Seventh Number of the "Edinburgh Journal" I perceived that I had been mistaken, for I found in it the following passage:—"Few branches of modern science should excite a more lively interest than that which treats of the influences of rotation on magnetic phenomena. We are proud of thinking that this remarkable discovery has been first made in our own country; and that, with the exception of a small number of important experiments made in France, it has been followed exclusively by the Fellows of the Royal Society."

The decision, it will be seen, is clear, positive, and decided. This kind of merit is often found in the "Edinburgh Journal"; —the qualities of correctness and truth, perhaps, not quite so often. Their absence was, perhaps, never more remarkable than in the passage in question: a few dates will prove its absolute inaccuracy.

On the 22nd of November, 1824, I communicated to the Academy of Sciences experiments on the influence which a metallic body, or any other kind of body in repose, exercises on magnetic needles which are vibrating at a short distance from its surface. This experiment was published in the greater part of the Paris Journals on the 23rd and 24th of November. It was even reported, by a letter from Paris, in the Number of the "Edinburgh Journal" itself, which appeared on the 1st of January, 1825.

The experiment, in which the needle previously at rest is drawn round by a metallic plate in motion, was communicated to the Academy, as has been already said, on Monday, the 7th of March, 1825. It was performed by causing a metallic plate to rotate with different degrees of velocity beneath a needle placed in a glass case, and separated from the moving plate by a membrane, to protect it from any agitation of the air. The part of the apparatus which communicated the motion was entirely of copper. The apparatus itself is now to be found in most *cabinets de physique.* But it must be remarked, that the experiment last mentioned is only that of the 22nd of November repeated in another shape, being, in fact, deducible from the former one, by that generally received principle in mechanics, that action and re-action are equal. The rotation experiment is useful for studying the phenomena whenever very great velocities are required, and vibrations are employed by preference when liquids or substances in a state of powder are to be operated on. The inferences are the same in both cases. Let us now pass to the consideration of the dates of the English memoirs.

Mr. Barlow presented his memoir, "On the Temporary Magnetic Effect induced in Iron Bodies by Rotation," to the Royal Society on the 14th of April, 1825, and it was not read until the 5th of May, 1825.

Mr. Christie's memoir, "On the Magnetism of Iron arising from its Rotation," was read on the 12th of May, 1825.

The memoir of Messrs. Herschel and Babbage, of which the writer in the "Edinburgh Journal" was no doubt not disposed to speak, because its authors were so good as to entitle it, "Repetition of M. Arago's Experiments," has for its date of reading, the 16th of June, 1825.

Since, then, the 22nd of November, 1824, and the 7th of March, 1825, are anterior to the 5th and 12th of May, 1825, I leave the reader to judge what grounds the Scotch writer could possibly have had for so gratuitously assigning the priority to his countrymen.

Mr. Barlow announces that he had begun his experiments on the effect of the rotation of a sphere of iron in the month of December, 1824. December comes after November: I have therefore personally no interest in contesting this date; but I think it right to maintain as a general principle that *publication*, in some manner or other, is the only title which ought to be admitted in the history of science; although in so doing I deprive myself of the advantage of proving that the results treated of in this notice had been communicated to a great number of scientific men, French and English, nearly two years before I presented them to the Academy. However, the month of December, always indicated by Mr. Barlow himself as that in which he began his experiments, did not suit the writer in the "Edinburgh Journal," since my first publication is dated in

November, and so we find in the Eighth Number of that journal, published in April, 1826, the following passage: —

"*About* the month of November, 1824, Mr. Barlow's experiment in which he produced a certain deviation of the magnetic needle, by the influence of the rotation of an iron sphere, became the subject of conversation at the Royal Society," &c.

Mr. Barlow had said that he had only begun to occupy himself with the phenomena produced by the rotation of iron in *December;* this was certainly vexatious, since *November* was the date of my first publication! How was this difficulty to be got over? The problem seemed an embarrassing one, but it will be seen that the Scotch writer resolved it with much ingenuity; it was sufficient for the purpose to forget that the last month of the year had a name of its own: December is certainly a word which he will never write in future; why, indeed, should he? May not the dates which belong to that month be more suitably designated by this formula of "*about* the month of *November?*"

I am really pained to see a man of science condescend to such wretched expedients. Carried away by a blind passion, which he, perhaps, disguises to himself as the spirit of nationality, he overlooks that, even if it could be permitted that he should thus virtually antedate Mr. Barlow's experiments, nothing would be gained thereby for the point in question; for, if there is anything in my experiments which can justify the high favour bestowed on them by the Royal Society, it is the proof which they furnish of the immense enlargement which the magnetic properties of bodies experience either when they are made to rotate beneath a magnetic needle in repose, or when the needle vibrates at a short distance from their surface; now this is a result which is no way made to appear by Mr. Barlow's investigations.*

* The inference deduced by Mr. Barlow from his experiments is expressly stated by himself as follows: Phil. Trans. 1825, p. 326. "When any iron body is put in rapid rotation on any line not coinciding with its magnetic axis, a temporary derangement takes place in its magnetic powers, which, in its effects, is equivalent to a new axis of polarisation perpendicular to the place passing through its axis of polarisation and rotation." The formation of a "new axis" is ascribed, in p. 323., to the "cohesive (? coercitive) power of the iron" which is stated to be "such as to resist in a certain degree the inductive powers of the earth," &c. The experiment is, as will be perceived, in a form better suited for exact measurement, that

"In order to live in peace with some of my detractors, I am therefore quite willing to consent to its being henceforward stated in print, contrary to the evidence of the facts, that the experiments of the Woolwich Professor were commenced about the month of November, or even, if it is wished, about the month of October."

I now pass to the consideration of the experiments relative to the real action exercised by all bodies in nature on the magnetic needle in motion,—an action denied by some Italian physicists.

"Messrs. Nobili and Bacelli say that they have made magnetic needles vibrate over non-metallic substances, without finding any appreciable difference between the vibrations performed by the needles when placed over the disks, and when removed from their influence."

If these gentlemen had stated the distance between their needle and the non-metallic disk employed by them, and the number of the vibrations, I might perhaps be enabled to assign the cause of the error into which they have fallen. As it is, all I can do is to oppose to their negation, exact measurements stating the circumstances under which they were obtained. The following paragraph is an extract from my Journal of Experiments:—

"I suspend a magnetic needle horizontally over water, and I draw the needle 53° out of its natural position; being then left to itself, the needle vibrates to and fro on either side of the magnetic meridian, in arcs of diminishing magnitude. I try to seize the moment when the semi-arc has decreased to 43°, and I count how many vibrations have taken place from the commencement.

"When the distance of the under surface of the needle from the surface of the water is ·0256 of an English inch, a reduction of 10° of arc takes place in the course of 30 vibrations. When the distance is 2·055 inches, 60° vibrations are required to effect a similar reduction."

It is impossible to be mistaken in regard to such a difference as this. I may add that it would have been still greater if the

of the physicists, who, after having caused an horizontal needle to deviate from its natural direction by means of a vertical bar of iron, sought to learn whether, after a *sudden* reversion of the vertical bar, the deviation would still remain in the same direction for the first moment.

commencing semi-arc had been 90°. The following results were given when the same needle was vibrated over ice:—

Semi-arc.	Distance from the Surface of Ice.	Number of Vibrations.
	Inches.	
From 53° to 43°	·028	26
"	·050	34
"	1·202	56
"	2·055	60

Over a plate of crown glass, with another glass:

Semi-arc.	Distance from the Surface of Glass.	Number of Vibrations.
	Inches.	
From 90° to 41°	·0358	122
"	·0390	180
"	·1198	208
"	·1580	220

Thus, so far from the magnetic effects of non-metallic substances, such as water, ice, glass, &c., being inappreciable, as Messrs. Nobili and Bacelli have erroneously announced, these effects are, we see, sufficiently powerful to allow us to hope that, by making the experiment with adequate precaution, it will be possible even to show a sensible effect from compressed gases.

Messrs. Nobili and Bacelli have also remarked, that "it follows from Coulomb's experiments, that all substances give some signs of magnetism; which would seem to make it probable, that, in order to discover in bodies the faintest traces of magnetism, Coulomb's method would be found preferable to M. Arago's, as more secure."

I would observe, in reply, 1st, that Coulomb had not extended his attempts to any liquid; and even that his method would not have admitted of his doing so; and that this circumstance of itself gives an altogether peculiar character and value to the proceeding which I employed for rendering manifest the magnetic properties of water: 2dly, that the traces of magnetism perceived by the celebrated physicist who has just been named, were so faint, that, as he himself recognised, it was possible to attribute them to the presence of some ferruginous particles, so minute that the most exact chemical analysis might not be capable of showing their existence. I must next add that my experiments have no relation to those of Coulomb; the magnetic virtues which they serve to manifest are quite of a different nature from those which are measured by causing needles to vibrate between two magnetised bars. Some new facts, which I will presently relate, will, I think, leave no doubt on the subject; I will only say here, that Messrs. Nobili and Bacelli might themselves have drawn the inference from their own experiments. I subjoin the values of the deviations produced by disks, formed of different substances, turning with a given velocity below a magnetised horizontal needle, as stated in the Memoir of the two physicists of Modena.

The disk of	copper	produces	a deviation of	55°
„	zinc	„	„	14°
„	brass	„	„	11°
„	tin	„	„	10°
„	lead	„	„	8°

The succession of different substances in the order of magnetic intensity, as it would result from Coulomb's experiments, (proceeding, as in the above table, from the greater to the less) would be lead, tin, silver, copper, gold; precisely the contrary of the succession given by the experiments of deviation.

All the physicists, including Messrs. Nobili and Bacelli, who had attended to the phenomena produced by the magnetism of bodies in motion, explained them at first very nearly in the same way. If, said they, a needle is suspended horizontally over an indefinite metallic plate, there should be formed below each pole of the needle,—say, for instance, below the north pole,—a pole of an opposite kind—or attractive—produced by the decomposition of the neutral fluid in the plate. When afterwards this plate is made to rotate, this attractive pole is carried in the direction in which the plate is turning; a similar pole is formed afresh below the needle, to be afterwards carried round in its turn, and so on. Now, suppose that these poles are produced almost instantaneously, and that they take some time to disappear, it will follow that the needle will be preceded by a series or succession of poles, which, being all attractive to the pole of the needle, will cause it to deviate from its usual position in the direction in which the movement of the plate takes place.

This mode of explanation* had also presented itself to my

* I think it was M. Duhamel, member of the Academy, who first pro-

mind, when I first communicated the experiments of rotation to the Academy; I did not mention it, however, because an hypothesis which only accounted for the *direction* in which the needle was displaced, did not appear to me to rest on a sufficiently solid basis. I think that what above all required to be explained was that a plate of copper, which, when in a state of repose, scarcely causes a magnetic needle to deviate a second of arc from its ordinary direction, may become capable, by the sole fact of its being put in motion, — the distance remaining the same, — of drawing the needle ninety degrees and upwards from its place; I will frankly own that I had not found the means of explaining this. In regard to the hypothesis in question, I had every reason to be glad that I had not brought it forward; for fresh trials showed me not only that it is insufficient, but that, moreover, it is directly contrary to the results of experiment; I will demonstrate this in a few words.

The south poles, which, according to the theory of Herschel, Babbage, Nobili, Prevost, &c., the north pole of the needle may be said to sow or deposit around the margin of the turning copper-plate, evidently ought, by their combined action, to attract the said north pole of the needle, and tend to draw it nearer to the plate; but I have assured myself, on the contrary, that the component, perpendicular to the plate, of all the forces brought into action by its motion, is a repelling force. Suspend, by means of a thread, a very long magnet hanging vertically to the beam of a balance, counterpoise it by weights of any nature you please, on the opposite side, and make the equilibrium exact; if you then cause a plate of copper, placed below the magnet, to rotate, the equilibrium will no longer subsist; the magnet will seem to have become lighter; it will be lifted up; that is to say, the plate will repel it.

The experiment can be still more easily tried with a dipping needle. When the plane of such a needle is directed exactly towards the centre of the rotating disk, which I suppose always to be horizontal, the needle itself being made horizontal, any movement of rotation around its axis of motion can obviously only result from a force perpendicular to the disk: if we now suppose that one only of the poles of the needle is brought

posed the explanation in question. His letter to the Academy was read on Monday, the 27th of December, 1824, and extracts from it were printed in some of the journals of the 29th.

vertically over the plate, we shall find, as in the experiment of the vertically suspended magnet, that so long as the plate rotates, the pole in question is constantly lifted.

The action which a circular horizontal metallic disk, turning round its centre, exercises on one of the poles of a magnetic needle, may be decomposed into three components, the first vertical, or perpendicular to the disk; the second horizontal, and perpendicular to the vertical plane containing the radius of the disk terminating in the projection of the pole of the needle; and the third, horizontal and parallel to the said radius. The first is, as we have just seen, repellent; the second is the tangential force which imparts the movement of rotation to horizontal needles; the properties of the third component may be studied by using an inclination needle, placed vertically, and in such manner that its axis of rotation shall be situated in a plane perpendicular to one of the radii of the disk; in this position, the needle will only move in virtue of the component directed towards the centre.

Let us conceive such a needle to be vertical when over the centre of the rotating disk; the rotatory movement will naturally not cause it to deviate from its direction. There exists a second point, nearer to the circumference than to the centre, in which the verticality of the needle will also be preserved. Between these two points the lower pole is constantly attracted towards the centre, whatever may be the velocity of rotation; when more distant from the centre it is repelled. The action is still sensible and repellent, when the vertical direction of the prolongation of the needle has passed the circumference of the disk. I might ask how this repelling force, in the direction of the radius, could be deduced from the action of the attracting poles distributed over the upper surface of the metal, if I had not already proved the insufficiency of that theory by the simple fact of the existence of a repelling force perpendicular to the rotating disk.

Faraday, in 1832, was the first to show, by employing a galvanometer of which the wires were placed on different parts of moveable metallic disks, above which there was a fixed magnet, that there are in these metallic disks currents induced by the magnetic needle; and it has been thought that the *complete* explanation of *all* the phenomena which I had discovered could be deduced from thence. I do not participate in this opinion. On the 25th of September, 1844, I communicated my doubts on this subject to the Bureau des Longitudes; the report, or *procès verbal* of the meeting, was to the following effect:—

"M. Arago recalled the experiments made by him long since, on the diminution of the arc of vibration of a magnetic needle, when its vibrations are performed at a small distance from a plate of glass, or of ice (frozen water), or from the surface of a liquid. M. Arago cited the particular circumstances in his experiments, from which it follows that in the particular case of glass, ice, or liquids, the phenomenon does not depend on an *induction*. M. Arago thought that it could only be attributed to a condensation of the atmosphere at the surface of bodies. He indicated experiments which he proposes to undertake for the purpose of putting this result beyond question."

It is not that I mean to assert that electricity is not principally concerned in the phenomena in question; but I say that they cannot be completely explained by the production of currents, vanishing as soon as formed, at the surface only of bodies, which, in this hypothesis, ought necessarily to be very good conductors of the electric fluid supposed to be in rapid circulation at their surface. I have not been the last to show that, in phenomena of rotation, electricity and magnetism produce analogous effects. In 1845, I had to make good my claims against an inaccuracy on this point, which had obtained circulation in the scientific world on the authority of one of the most illustrious men of our age.

In his fine work, Faraday, whose friendship I prize so dearly, attributed to Ampère the discovery of the motion which takes place in a wire passed through by a voltaic current when it is placed horizontally at some distance from a rotating metallic disk. The true epitome of the facts is the following:—

About the beginning of the month of August, 1826, I thought that my experiments of rotation ought to be repeated, substituting currents for magnetic needles. Not having any voltaic pile, I asked my friend Ampère to have the apparatus set up in the Cabinet de Physique of the Collége de France. One of the gentlemen attached to the College, M. Ajasson de Grandsagne, made the necessary arrangements; but the day the first attempt was made, at the very moment when the wire began to move, the axle round which the plate turned, broke. As I was starting the next day for the Pyrenees, I authorised Ampère to continue the experiment. M. Colladon presided over the reconstruction of the instrument, and introduced some important improvements. This time the wire began to move almost at the instant when the copper disk began to turn. Ampère hastened to send me the result which had been obtained.

These explanations did not at first appear to me necessary to be given, for, in publishing the experiment, Ampère had been careful to cite me. Since, however, it has happened that the notice drawn up by this illustrious and lamented physicist has led such a man as Faraday into error, it may not be useless to place Ampère's own letter before the public. I will only quote from it the following passage:—

"Paris, 1st September, 1826.

"You will see in this notice that I have taken care to say that the idea of this experiment is due exclusively to you.

"It remains, my dear and excellent friend, that I should remind you that you promised me, if this experiment was successful, to defend my theory as the true explanation of the phenomena. When this is added to all the rest, and to the calculations in the Memoir now printing in the Memoirs of the Academy, I do not see what objection can remain to be brought against me.

"I would also beg of you, if you find the note which I send you what it should be, to write to M. Savary to insert it in the 'Annales de Chimie et de Physique,' such as it is, excepting only such alterations or additions as you may think are required, and which you are at perfect liberty to make, since the experiment was devised by you."

I will add, what has never been printed in full, that immediately after my return to Paris, I repeated with currents the experiments which had been made before with needles, and with the same results; first in respect to the direction of the forces when I used complete disks, and second in respect to the diminution of the forces when interrupted disks were used.

Having mentioned "interrupted disks," I take the opportunity of repeating here the remarks which I made in 1845, on presenting to the Academy of Sciences a pamphlet by my friend

M. de Haldat, entitled "Histoire du Magnetisme dont les Phenomènes sont rendus sensibles par le Mouvement." The learned secretary of the Academy of Nancy has fallen into one inaccuracy. He reminds his readers, in pp. 11. and 42. of his work, that rotating metallic disks lose a great part of their power when pieces have been cut out from them in the direction of their radii. This fact, from the first, appeared of capital importance; it showed that the phenomena do not depend on purely molecular action. But M. de Haldat is in error in attributing the discovery of this fact to Messrs. Herschel and Babbage. The two English philosophers, in the Memoirs which they have published, declare that their experiments with disks were made "after M. Arago." (See vol. cxv. of the *Phil. Trans.* p. 480.)

In order to decide the question as to the cause of the influence exercised by the magnetic needle in motion upon all bodies, and reciprocally by all bodies in motion on a freely suspended magnetic needle at rest, we must study what takes place with substances reputed to be the worst conductors of electricity, such as resin or gum-lac for example. It would also have to be seen, whether at very small distances from non-conducting bodies (distances of the same order as those at which magnetic bars are made to vibrate) bars of brass, of precisely the same form and dimensions, would not be influenced in their vibration by condensation of the atmospheric air at the surface of the bodies. Experiments of this kind have been undertaken at my request by two of my friends, Messrs. Laugier and Barral. I gave a verbal account of the results obtained to the Academy of Sciences, at its meeting of the 7th of March, 1853. In the state of health in which I now find myself, I can no longer hope to see this investigation carried out to its termination. I must, therefore, content myself with recording here the numerical results which have been found, and with placing them by the side of those which I had myself obtained when my strength and the state of my eyesight permitted me to observe.

On account of the rapidity of the diminution of the arc of vibration of a copper needle which moves only under the influence of the torsion of a platinum wire, it was requisite to make an alteration in the method which I had employed. It was necessary that the vibrations of the copper bar should be at least as rapid as those of a magnetised bar, and that the torsion wire should be sufficiently short and thick. Consequently Messrs. Laugier and Barral sought the diminution of arc produced in the course of a given number of vibrations. I operated, on the contrary (as may have been seen by the numerical results cited above), by counting the number of vibrations performed while the arc of vibration was undergoing a diminution of a certain number of degrees. In other respects, Messrs. Laugier and Barral experimented with the same glass case, and exactly under the same conditions as those which I had selected. Only a few modifications, which those gentlemen thought the apparatus required, were made in it by our skilful artist, M. Brunner.

To avoid any error which might arise from defective centering, M. Laugier observed the amplitude of the arc of vibration on one side, while M. Barral did the same on the opposite side, the mean of the two observations being taken. In my experiments I observed the elongation of the needle, sometimes on the right and sometimes on the left, and took, in a similar manner, the mean of the observations on either side.

The following were the results obtained by Messrs. Laugier and Barral:—

Using a copper bar, suspended by a platinum wire, to which the same amount of torsion was always given, and measuring the diminution of arc taking place in the course of ten vibrations, there was found—

When the bar was placed over an iron disk,

	English Inches.		Degrees.
At a distance of	0·04	a diminution of arc of	49·8
„	0·28	„ „	49·5
„	3·15	„ „	49·0

Over mercury, with another platinum wire giving slower vibrations:—

	English Inches.		Degrees.
At a distance of	0·03	a diminution of arc of	32·0
„	0·09	„ „	32·0
„	0·33	„ „	32·4
Taking away the mercury		- - -	31·6

Over a cake of resin, with a platinum wire giving more rapid vibrations:—

	English Inches.		Degrees.
At a distance of	0·02	a diminution of arc of	95·3
After taking away the cake		- - -	95·1

Over a cake of gum-lac, with the same platinum wire:—

	English Inches.		Degrees.
At a distance of	0·03	a diminution of arc of	98·7
After taking away the cake	-	-	- 97·5

Over the same cake of gum-lac, with a platinum wire giving slower vibrations:—

	English Inches.		Degrees.
At a distance of	0·02	a diminution of arc of	72·25
After taking away the cake	-	-	- 71·20

Each experiment was repeated five times, and the above figures are the mean of the five results, which did not differ from each other more than one or two degrees at the utmost.

It is very evident that the presence or absence of a foreign body had no effect on the vibrations of the copper bar, notwithstanding the small distances employed. If, therefore, very great differences are found when a magnetised bar is employed, they cannot but be attributed to magnetism.

With the magnetised bar, Messrs. Laugier and Barral, after drawing the bar 76 degrees from the magnetic meridian, began to count the vibrations when this distance had been reduced to 71 degrees, and noted the diminution of arc which had taken place when the fiftieth double vibration was accomplished. Their results are given in the following table:—

Substances over which the Bar vibrated.	Distance of the magnetised Bar from the Substances experimented with.	Diminution of the Arc of Vibration in the course of 50 Vibrations.
	English Inches.	*Degrees.*
In air - - - - - -	- -	8·40
Glass vessel - - - - -	0·40	9·80
With 0·275 lbs. of distilled water in the same vessel - - - - -	0·14	12·25
With 0·275 lbs. of distilled water in the same vessel - - - - -	0·04	16·50
With 0·275 lbs. of distilled water in the same vessel - - - - -	0·02	25·00
With 0·550 lbs. of distilled water in the same vessel - - - - -	0·03	24·50
With a cake of resin - - -	0·24	7·25
" " - - -	0·05	23·50
" " - - -	0·02	37·50
With only the box which had contained the resin, and above the wooden bottom of the box - - - - -	0·08	10·50
After putting into the box the resin powdered - - - - -	0·67	10·00

Substances over which the Bar vibrated.	Distance of the magnetised Bar from the Substances experimented with.	Diminution of the Arc of Vibration in the course of 50 Vibrations.
	English Inches.	*Degrees.*
After putting into the box the resin powdered - - - - -	0·055	20·00
After putting into the box the resin powdered - - - - -	0·016	35·00
With a cake of gum-lac, - - -	0·06	13·50
With a cake of gum-lac - - -	0·03	21·50
With a cake of gum-lac - - -	0·022	23·50
With only the box which had contained the lac, and above the bottom of the box -	0·079	10·00
After putting into the box some pulverised lac - - - - - -	0·039	13·50
After putting into the box some pulverised lac - - - - - -	0·020	16·00
After putting into the box some pulverised lac - - - - - -	0·012	17·50

Thus it appeared that bodies which are regarded as the worst conductors of electricity, which resist the passage of ordinary machine electricity, and of the voltaic electricity of the most powerful piles, exercise a very powerful action on a magnetic needle in motion, and that they do so, whether they form a solid uninterrupted layer, or whether they are reduced to an impalpable powder. The action exercised is not the same with the different bodies. It was consequently more than probable that analogous results would be obtained in a vacuum.

This mode of testing the effects exercised by all bodies on a magnetic bar might be applied to examining the effects which would be produced by compressed gases, as I pointed out when I first made known the earliest discovered phenomena of rotation magnetism. I hope that this experiment will some day be made by some physicist.

I will add here to the numbers given above, some results which I obtained with various substances, for the sake of showing that there might be found in this kind of researches a means of measuring the specific action of each substance.

I repeat that I counted the number of vibrations performed between certain determinate arcs.

Substances employed.	Distance of the magnetic Needle from the Substances experimented with.	Number of double Vibrations counted.	Diminution of measured Arcs.
	English Inches.		*Degrees. Degrees. Degrees.*
Plate of glass - - -	0·984	30	35·5 —15·5 =20·0
" - - -	0·984	40	35·5 —12·0 =23·5
" - - -	0·984	50	35·5 — 9·5 =26·0
" - - -	0·984	60	35·5 — 7·5 =28·0
" - - -	0·984	70	35·5 — 6·0 =29·5
" - - -	0·984	80	35·5 — 4·5 =31·0
" - - -	0·049	30	35·75— 9·00=26·75
" - - -	0·049	40	35·75— 5·75=30·00
" - - -	0·049	50	35·75— 4·00=31·75
" - - -	0·049	60	35·75— 2·75=33·00
" - - -	0·049	70	35·75— 2·00=33·75
" - - -	0·984	77	35·5 — 5·0 =30·6
" - - -	0·138	59	35·5 — 5·0 =30·5
" - - -	0·049	43	35·5 — 5·0 =30·5
At a considerable distance from the bottom and sides of a bowl - - -	0·049	70	46·0 — 5·0 =41·0
With water in the bowl -	0·473	63	45·75— 5·0 =40·75
" " - -	0 079	41	46·00— 5·0 =41.00
In an exhausted receiver, without any other substance in it	0·079	174	50·5 — 5·0 =45·5
After letting the air in again -	0·079	98	50·5 — 5·2 =44·8
With a cake of resin in the exhausted receiver - -	0·276	57	50 — 5 =45
Zinc plate - - -	0·177	11	48 — 4 =44
Brass plate - - -	0·177	12	48 — 4 =44
Tin plate - - -	0·177	17	48 — 4 =44
Leaden plate - - -	0·177	28	48 — 4 =44
Distilled water - -	0·473	69	44·5 — 4 =40·5
Salt water - - -	0·473	62	46 — 5 =41
" - - -	0·079	45	46 — 5 =41

I will not add to the number of these citations, which are sufficient to show that there is no class of bodies which can be looked upon as escaping from the influence exercised by a magnetised bar in motion. When it is remembered that the bars employed in my observations were of small dimensions, and that I operated on non-conducting substances, it will, I hope, be seen, on due reflection, that my experiments differ from those devised by Faraday, and that they cannot be completely explained by the simple induction of fugitive currents.

ANIMAL ELECTRICITY.*

I WILL here bring together, under the title of *Animal Electricity*, facts which are in reality very diverse, but which uninformed persons have been fond of associating. As yet electricity has only been clearly evidenced in some animals; attempts have been vainly made to connect with it phenomena taking place in the human body, and on which the human will has appeared to exercise some action in the judgment of persons, either prejudiced, or little attentive to the circumstances.

CHAPTER I.

ON THE SPARK DRAWN FROM THE TORPEDO AND THE GYMNOTUS.

I HAD to occupy myself with this subject on the occasion of a discussion respecting priority, between Messrs. Linari and Matteucci, on experiments made in 1836, having for their object to obtain sparks from the torpedo, and to show that these sparks are really of the same nature as those obtained in laboratories with the ordinary electrical machines, or with voltaic piles.

No one had previously seen the electric spark in experiments made on the torpedo. M. de Humboldt had not succeeded in seeing it, even in operating on the gymnotus in its native country. Walsh, whose numerous attempts had failed so long as they were only made with torpedos, succeeded, in August 1776, in rendering the spark apparent in using the gymnotus. It is worth noticing that this important experiment came to the knowledge of the public, not directly by a memoir of Walsh himself, but through a note written by M. Le Roy. Fahlberg and Ingenhousz also said that they had sometimes obtained the spark during the discharge from an electric eel from Surinam. At the present time every one may have the opportunity of ob-

* A posthumous Work.

serving the same effect, when they are assured that electro-chemical currents have not had any share in the experiments.

It appeared to me that the honour of the new discovery belonged to M. Matteucci, when I saw, in a letter addressed to him by M. Linari, dated the 11th of March, 1836, the following sentence:—"Describe to me, clearly and patiently, the plan of experiment, which you say you have thought of for *drawing the spark from the torpedo.*" It appeared to me probable, that, in making such a request as this, M. Linari would not have failed to announce, or, at the very least, to imply, that he was himself in possession of some particular method of experiment if he really had been on the track of anything new; whereas the letter does not contain the slightest allusion of the kind.

However, a person by whom I have often been contradicted, but whose name a solemn decree of the tribunals of justice has since caused to be erased from the list of the Academy,—presented, in the *compte rendu* of our meetings, some remarks tending to prevent the Academy from voting the insertion of M. Matteucci's Memoir in the "Recueil des Savants Etrangers" (an insertion which is the highest encouragement in our power to offer to the cultivators of science), under the pretext that it had not yet been possible to verify all the experiments described by M. Matteucci.

I remarked, that if this system of proceeding were adopted, it would scarcely ever happen that the Academy could be free to approve the works submitted to it,—at least, in the sciences of observation. Has it ever been imagined that Academic commissions had imposed upon them the obligation of repeating, in all their details, the numerous delicate and difficult experiments which are described in the long Memoirs referred to their examination? *When they can do so,* such commissions verify here and there some prominent points; if this partial verification has a satisfactory result, they admit the rest, but, as is well understood, under the responsibility of the author. And yet more,—the Academy fully adopts, and often causes to be inserted in the "Recueil des Savants Etrangers," Memoirs of which there has been no opportunity of verifying even a single result. For instance, did the Academy require me to proceed to the summits of the Pyrenees, before it honoured with its suffrage the fine geodesical levelling carried by M. Corabœuf along the whole of the chain between the ocean and the Mediterranean? The Commission which was appointed to examine M. Matteucci's Memoir acted conformably to our customs, and did all that could in reason have been asked from it. Such points as it could verify it found correct. It did not occupy itself with the experiment of the lobes of the torpedo (the most simple and perhaps the most easy of all the experiments cited by M. Matteucci), for the very good reason that there are no torpedos at Paris. The Commission notices this in its Report. In my opinion it was scarcely necessary to have done so; the facility of the particular observation, the confirmed exactness of all the rest, the success attained by M. Matteucci in a great number of delicate researches, all afforded a sufficient guarantee; and it is not usual to ask for more. In deciding, in conformity with the opinion of the Commission, and against the opposition to which I have alluded, that the Memoir of M. Matteucci should be inserted in the "Recueil des Savants Etrangers," the Academy testified its just interest in an investigation which connects itself with one of the most delicate points of animal organisation, and has invited observers to turn their attentive inquiries in that direction: this is the honourable office which the Academy has always taken upon itself, and which it is impossible that it should ever have reason to repent. I will conclude by citing a passage, which will show in what terms M. Matteucci's experiments are spoken of beyond the Rhine: it is taken from a letter from M. de Humboldt:—"The thing which has most deeply interested me of late is the great discovery of M. Matteucci on the action of the fourth lobe of the brain of the torpedo."

CHAP. II.

ON A SO-CALLED ELECTRIC GIRL.

I NOW come to some circumstances of animal electricity, of which the truth has not been confirmed, and which are only attributed to a magnetical or electrical cause from the want of due reflection.

At the meeting of the 16th of February, 1846, I laid on the table of the Academy a short note by M. Cholet, and a more detailed note by M. Tanchon, both relating to a young girl of thirteen or fourteen years of age, Angélique Cottin, employed in a manufactory of net gloves, in whom, it was said, extraordinary faculties had developed themselves for about a month past. When M. Cholet presented himself at the Observatory, he was accompanied by the young girl and her parents. He urged me to assure myself without delay, and by my own observation, of the reality of the phenomena described. After some hesitation, I yielded to his wish, as it was possible that this first inspection might lead me, in case of entire failure, to propose to the Academy not to name any commission.

I reported what I saw during a few minutes' interview. Mademoiselle Cottin, on sitting down upon a chair, occasioned it to move violently. I did not distinctly perceive the agitations which are said to be produced, at a distance, in a wooden stool, by the intermediation of an apron. Other observers have thought that such agitations were perceptible. Above all, I could not confirm the reality of any effects on magnetised needles. The repulsive action exercised by Mademoiselle Cottin's left hand on a suspended sheet of paper was not greater than is produced by a great many persons under analogous circumstances. Notwithstanding so many negative results, I did not, however, hesitate to ask the Academy to name a commission, who might verify the facts at leisure. It was for the gentlemen composing it to ascertain the manner in which the movements taking place in the chair were produced. If there was a trick, it was desirable to detect the deception, and thus prevent the public from being led into error. Moreover, M. Tanchon cited in his note some experiments very easy to be repeated, and which did not allow of any equivocal explanation.

The following is the Report of the Commission, which consisted of MM. Arago, Becquerel, Isidore Geoffroy-Saint-Hilaire, Babinet, Rayer, and Pariset: —

"At its meeting of the 16th of February last, the Academy received from M. Cholet and M. le Docteur Tanchon two notes, respecting the extraordinary faculties, said to have developed themselves for about a month past, in a young girl of the département de l'Orne, named Angélique Cottin, of about fourteen years of age. The Academy, conformably to its usual practice, named a commission to examine the alleged facts and report the results. We will acquit ourselves of this duty in very few words.

"It was affirmed that Mademoiselle Cottin exercised a very intense repelling action on bodies of all kinds the moment they were touched by any part of her clothes. It was even said that stools were overturned by means of the simple contact of a silk thread.

"No appreciable effect of this kind was witnessed by the Commission.

"In the accounts communicated to the Academy, it is said that under the influence of this young person's arm, a magnetised needle first vibrated rapidly, and then came to rest at a considerable distance from the magnetic meridian.

"In the presence of the Commission, a delicately suspended magnetic needle did not experience under these circumstances any displacement, either permanent or momentary.

"M. Tanchon thought that Mademoiselle Cottin possessed the faculty of distinguishing the north from the south pole of a magnet, by simply touching the two poles with her fingers.

"The Commission assured themselves, by varied and numerous experiments, that this young girl does not possess the supposed faculty of distinguishing the poles of a magnet by the touch.

"The Commission will not pursue further the enumeration of failures; its members content themselves with declaring, in conclusion, that the only one of the announced facts which was realised in their presence, was that of the sudden and violent movements in chairs in which the young girl sat. Serious suspicions had arisen as to the manner in which these movements were produced, and the Commission determined to subject them to an attentive examination. The Commissioners announced, without disguise, that their examination would be directed to discover what share certain skilful and concealed manœuvres by the hands and feet might have had in the effect witnessed by them. From this moment it was declared that the young girl had lost her attracting and repelling faculties, and that whenever they should reappear we should be apprised. Many days have since elapsed, and the Commission has received no such intimation. We know, however, that Mademoiselle Angélique Cottin is still daily presented in drawing-rooms, where she repeats her experiments.

"Having weighed all these circumstances, the Commission is of opinion that the communications presented to the Academy on the subject of Mademoiselle Angélique Cottin ought to be set aside, or regarded as not having taken place."

CHAP. III.

PHENOMENA OF TURNING TABLES.

FAILURES have never produced any discouragement in public opinion in matters supposed to relate to animal electricity. An example of this has shown itself in regard to the phenomena known under the name of turning tables. They were attributed unhesitatingly to a supposed faculty possessed by animated beings, of developing in inert bodies a peculiar kind of electricity. I had to speak on this subject at the Academy, on the occasion of a communication from its correspondent, M. Séguin, known by the important invention of tubular boilers. It was my duty to speak as I did. But I cited old experiments by Mr. Ellicot, clock-maker, mentioned in the Philosophical Transactions, which have the greatest analogy with the admissible accounts given of turning tables. What is apparently most extraordinary and most difficult to explain in the phenomenon of the tables, is the circumstance that by means of, it may be said, infinitely small impulses, impressed by the fingers on the mass of wood of which the table consists, a very considerable degree of motion should at length be imparted to the table. Well, then, I would remark, that in Mr. Ellicot's experiments, two pendulum clocks, enclosed in separate cases, were suspended from a wooden plank affixed to the same wall, and at a distance of 23½ English inches from each other. At first only one of these two clocks was going; the second clock was at rest; after a certain time this second clock was found to have been set going by the imperceptible vibrations transmitted to its pendulum from the pendulum of the first clock, through the medium of the intervening solid bodies. A very singular circumstance is, that after a certain time longer, while the pendulum of the second clock (the one which had at first been at rest) vibrated in the largest arc which the construction of the clock would permit, the pendulum of the first clock, the one which at first was the only one going, had arrived at a state of entire rest.

I will not enlarge upon the deductions which may be drawn, and which really have been drawn, from the facts just mentioned, because my object has only been to show that there already existed in science instances of communicated motion analogous to those which have been recently presented by turning tables, and of which the explanation does not require any of those mysterious influences to which recourse has been had in the case of the tables.

TERRESTRIAL MAGNETISM.*

CHAPTER I.

NOTICE RELATING TO MY OWN OBSERVATIONS.

NOTHING in the vast domain of terrestrial physics is more obscure and more uncertain than the causes which everywhere occasion the three elements of terrestrial magnetism, viz., the declination, inclination, and intensity of the force, to vary.

The magnificent discoveries which have been made in recent years on the connection of heat and electricity with magnetism, have scarcely taught us anything on the subject of the causes of these singular variations.

Perhaps this want of success may be to be attributed to the ignorance in which we still are, regarding the laws which regulate such great and singular changes; viz., the laws of the phænomena. Thus, for example, previous to 1816, it was not known at Paris by any direct observation, whether the horizontal needle, in its secular movement from east to west, would arrive at a limit which it would not pass, and from whence, after a short pause, it would return to the eastward. The inclination needle gives occasion to similar questions and similar doubts. From the date of the earliest known observations to the present time, the inclination of the needle at Paris, reckoning from the horizontal line, has constantly diminished; but when will this diminution cease? Of this we are ignorant. In regard to the intensity of the force, too few years have elapsed since its absolute value has been determined, for the question of the change which it may undergo to be even approached.

All, therefore, that can now be hoped, is to provide and bring together measurements which will serve as bases for the investigations of our successors. I have been enabled to make

* A posthumous Work.

magnetic observations since 1810. It appeared to me that it was my duty to publish them, and I would not willingly have devolved this care on any one else. However explicit may be the elucidatory notes written in the margins of the original registers, there are always some details which are liable to be omitted, and which no one but the person who has directed the whole can supply. Unfortunately the state of my eyesight forbids my going over the observations myself, and drawing from them their results; and I have, therefore, requested M. Barral to undertake this task.

These observations, to be spoken of in the sequel, have not, generally speaking, been made with the same apparatus; the instruments employed were executed by different artists. This would be a disadvantage if we aimed principally at discussing annual variations; but it will be on the contrary an advantage where the *absolute* inclination, for example, is to be reasoned on; for we are entitled to believe that the unexplained errors, affecting in a constant manner and to the amount of a few minutes inclinations obtained with different needles, will thus be intercompensated, and their effect destroyed.

The observations of the inclination were generally made at the end of the Observatory garden, on a stone pedestal constructed for the purpose. This pedestal was originally in the open air, but it has since been protected by a wooden building in which no iron fastenings have been used.

Some years ago an amphitheatre, of which the zinc roof rests on iron supports, was added to the west side of the Observatory. More recently the east tower of the original Observatory had placed over it a colossal turning roof, into the construction of which a vast quantity of iron enters. These two masses are distant 236 English feet from the pedestal on which the inclinations are measured.

We have every reason to believe, from the results of several trials, that at this distance the iron in question has produced no sensible effect on any of the phenomena of the magnetic needle.

CHAP. II.

VARIATIONS IN THE ELEMENTS OF TERRESTRIAL MAGNETISM.

A NATURAL magnet, or which comes to the same thing, a magnetised needle, properly suspended, always directs itself towards the polar regions. It may easily be imagined how great an advantage mariners ought to derive from this property for their guidance, when clouds or mists veil from them the sun, moon, and stars. Unfortunately, the direction of a compass needle changes with time and place, according to laws of which the knowledge would indeed powerfully contribute to the progress of navigation, but which, judging by the results which some physicists have obtained, would seem to be very complicated. It is true, that the earliest observations to which recourse can be had are too recent and too imperfect to furnish a complete elucidation of so difficult a subject. The care which, for some years past, artists have bestowed on the construction of instruments, has enabled observations to be made with considerable exactness; and these have led to the discovery of several curious phenomena, which will be spoken of in this Notice.

The numbers which determine the geographical, hypsometrical, and climatological characters of places on the globe, do not appear, speaking generally, to undergo the slightest alteration in the course of centuries. It is otherwise with the magnetical elements,—the declination, inclination, and intensity of the earth's magnetic force,—these change visibly at each place, from year to year, and even from hour to hour. The laws of these variations are not perfectly known. Yet is there a subject more interesting to navigation?

Let us establish for a given year the succession of points on the globe where the inclination of the magnetic needle is null. The continuous line passing through all these points is called the *magnetic equator*. This equator, situated partly to the north and partly to the south of the geographic equator, intersects the latter at points called nodes. Well, then, these nodes change their place. By a gradual movement of translation of the magnetic equator, they are transferred from east to west; but in the course of this movement does the magnetic equator itself preserve exactly the same form? This is a question which is not yet perfectly solved.

If we set entirely aside the applications to the interests of navigation, of which these phenomena might become the object, still the changes from year to year of the declination and inclination, including the above-mentioned change of place of the magnetic equator, will not the less continue to be the facts the most astonishing, mysterious, and worthy of interest, embraced within the vast domain of science.

The directive action of the globe is evidently the resultant of the actions of the molecules of which it is composed. How is it that this resultant can be thus variable, while the number, position, and temperature of these molecules, and, so far as we know, all their other physical properties, remain constant? Must we suppose, with Halley, that there exist in the interior of the earth *moveable molecules?* No scientific body can be indifferent to the honour of contributing to the resolution of such questions.

CHAP. III.

LOCAL DEVIATION OF THE COMPASS.

THE masses of iron which enter into the construction of ships and form their chain cables, cast-iron guns, anchors, &c., exercise on the magnetic needle an action, which usually causes it to deviate considerably from the direction which it would take under the sole attraction of the earth's magnetism. This action is not the same in all directions of the ship's head, or on all the courses on which she may be steering; it also changes with the [magnetic] latitude. The compass is therefore only a safe and secure guide, when the condition is fulfilled, of determining experimentally in each ship, when ready to sail, the local deviations of the compass in different azimuths; and of afterwards taking a scrupulous account of the changes which,—all other circumstances remaining the same,—are the inevitable consequences of a change of geographical position.

The experiments by the aid of which the elements of calculation proper to each ship are to be procured are very delicate; but this is no reason for dispensing with them. We believe that at every port at which vessels are equipped, there ought to be a hydrographical engineer charged with the duty of determining the coefficients of each ship. The necessity of this precaution will be readily admitted for iron vessels, when I state, that recently an iron steamer, on her passage from Bordeaux to Brest, found herself in the case of being almost wholly unable to determine her route, owing to the irregularities in the direction of her compass. But we maintain that the same precautions are requisite for ordinary wooden vessels. The waves conceal from us the errors of navigators, as the earth covers those of physicians. We may not, therefore, be able to cite any fatal accident, of which it can be affirmed with entire certainty that it took place in consequence of the local deviation of the compass needle; but we may, at any rate, support our views in some measure by probabilities borrowed from the English navy.

In the winter of 1811-1812, the "Hero," of 74 guns, was lost in the Texel, in coming from the Categat, with several merchantmen under her convoy. Only eight seamen were saved.

The "St. George," of 98 guns, Admiral Reynolds, and the "Defiance," of 74, experienced the same fate on the coast of Jutland. The Admiral, the captain of the "Defiance," and above two thousand sailors were drowned.

In 1810, the "Minotaur," of 64 guns, was wrecked at the mouth of the Texel, on the 22nd of December. Three hundred and sixty seamen perished.

Scoresby thinks there is a very great probability that these four shipwrecks would not have taken place, if the commanders had been acquainted with the means of taking into account the local deviation of the compass.

In 1804, sixty-nine merchant vessels sailed from Cork under the convoy of two English ships of war, the "Carysford" and the "Apollo." On the night of the 2nd of April, when the "Apollo" was by estimation a hundred miles from land, she struck on the coast of Portugal, near Cape Mondego. Twenty-nine merchantmen who had shaped their course by that of the "Apollo" were also wrecked. Nearly three hundred seamen perished in the catastrophe.

This terrible shipwreck was long attributed to the action of currents, but the discussion which Scoresby has gone into on the subject appears to show, that its cause is rather to be ascribed to a want of knowledge on the part of those by whom the course was directed, of the influence of the ship's iron on her compass needle.*

CHAP. IV.

MEANS OF IMPROVING COMPASS OBSERVATIONS AT SEA.

THE compass is doubtless the instrument which has been most useful to navigators. But have magnetic instruments been

* ["Practical Rules" for ascertaining the coefficients of local attraction in a ship, and for forming tables from them showing the true magnetic course corresponding to every compass-course, and the compass-courses corresponding to the true courses, were drawn up under the direction of the Lords of the Admiralty in 1840; and, since that period, have been supplied to all ships and vessels of war in the British service. In addition to the "Practical Rules," an officer was appointed (Captain Edward Johnson, of the Royal Navy, F. R. S., who had previously given much attention to the subject), for the express purpose of personally superintending the execution of the directions which they contained, in all Her Majesty's iron vessels and steamers, and occasionally also in wooden ships in cases where the local attraction was either unusually large, or presented features of unusual difficulty. With the advantage of this personal instruction, in addition to the practical rules, the officers of the ship were expected to be competent to repeat the determination of the coefficients, and to form new tables (which they were strictly enjoined to do) after every considerable change of geographical position, and particularly after passing from the high or middle latitudes to the tropics, or from one hemisphere to the other.

These arrangements are quite in accordance, as will be seen, with M. Arago's wise and sagacious remarks; but the provision which they have made applies solely to the ships and vessels of the *Royal* navy. There exists as yet no similar provision for the great mercantile marine of this country, of which iron ships and steamers now form a notable portion. The Editor gladly avails himself of this opportunity of expressing his entire concurrence in the opinions of M. Arago, that in all large ports, at least, in which vessels are equipped, a competent person should be appointed, whose duties should be,—to select in every ship an advantageous position for a standard compass, combining the two requisites in such selection of a manageable local attraction, and of convenient access for navigating the ship,—to determine experimentally the local deviations of the standard compass in different azimuths,—to instruct the Master how to repeat the same on future occasions,—and to see that he rightly and thoroughly understands the deduction of the true magnetic courses from those of the standard compass, and of the course by the standard compass corresponding to the true course which he desires to steer. The performance of these duties on the part of the person so appointed, to be imperative, at least in all cases of iron ships and steamers, by a regulation that no such ship should be permitted to leave the port until a certificate should be produced that they have been duly performed.]—*Editor.*

rendered as useful as possible to navigation? It is permitted to doubt whether this has been done. First, no use whatsoever has been made of the dipping needle as a means of guidance in the immensity of the ocean. It may be said that the dip is difficult to observe with precision at sea; but even the observations of the declination of the compass are only susceptible of a very moderate degree of precision in a ship under way. The reason in both instruments is, 1st, that when the suspension is delicate, the motion of the needle occasioned by that of the ship is frequent and irregular, so as to make even the mean uncertain; and, 2nd, that all arrangements in the mode of suspension, having for their object the diminution of the mobility of the needles, affect very sensibly the exactness of the observations.

There exists, however, a means, independent of the mode of suspension, of diminishing the number and extent of the vibrations of a needle, by the help of which oscillations of 90° are almost instantaneously reduced to 1° and less, without in the least degree impairing the mobility of the needle. This means is a result of the discovery which I made of the phenomena of rotation-magnetism. Plates of copper, suitably arranged, would effect this object.

Instruments which should secure this valuable practical result would not be difficult of construction, and when prepared, it is highly desirable that they should be submitted to the decisive test of experience under the care of practised seamen. To determine the latitude in cloudy weather by the sole aid of the dipping needle,—and both latitude and longitude by the single observation of the declination,—in parts of the globe where the course and direction of the magnetic lines permit this to be done, would be to introduce a new feature in the art of navigation.*

* [The application of M. Arago's discovery of rotation-magnetism suggested in this Chapter, has been in use in the standard compasses of the British navy since 1839, having been adopted on the recommendation of a committee appointed by the Lords of the Admiralty for improving the compasses of the Royal Navy. *Plates* of copper would have been inconvenient, because the compass requires to be read from *above*, and (on board a man-of-war) to be lighted at night from *below*, that no light may be visible on deck. Therefore, the top and bottom of the compass-bowl are necessarily of glass; but the cylindrical part of the bowl is made of copper one-tenth of an inch thick, carefully turned so as to permit the needle to vibrate freely, but with its poles as near as possible to the copper without touching it. The degree in which the vibrations are checked by being thus made in

CHAP. V.

ON THE DECLINATION.

THE loadstone, or natural magnet, according to the analysis of Bucholz, is a combination of protoxyde and sesqui-oxyde of iron. The most obvious property of the magnet, or loadstone, is the attraction which it exercises on iron. The ancients were acquainted with this, but were entirely ignorant of its directive property. These properties are communicable to iron, nickel, cobalt, and chrome; they become permanent in steel, which is the substance of which ordinary magnets or magnetic needles are made. Pure iron is not susceptible of acquiring permanent magnetism; in order that it may do so, it must be combined with certain proportions of carbon, phosphorus, and sulphur.

It is customary to give the name of *declination* to the angle formed by the direction of the geographical meridian and that of a magnetic needle placed on a vertical point or pivot, or suspended by a thread free from torsion, in such manner that it is horizontal.*

The existence of the declination of the magnetic needle is

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close proximity to the copper is shown by the following experiment made by two of the members of the committee referred to, viz. Captain Sir James Clark Ross and Captain Edward Johnson, both of the Royal Navy. "One of the committee's compass needles was suspended by fibres of silk in a *wooden* bowl of exactly the same dimensions as its own copper bowl. It was then drawn out of the meridian, and set in vibration in an arc of 22°. The length of time which elapsed before the arc of vibration was reduced to 2° was above *eighteen* minutes, and the number of vibrations 134. The wooden bowl was then replaced by the copper one, the needle remaining suspended as before, and the experiment repeated. The arc was now reduced from 22° to 2° in *four* minutes, and the number of vibrations was 30 instead of 134." (Extract from the Report of the Compass Committee, 1840, taken from the original draft in the Editor's possession.)

* [That which M. Arago here terms the declination of the needle is more usually called by British seamen the variation of the needle; and some little displeasure has occasionally been manifested when English writers have employed the more generally used term of "declination." But those who have occasion to discuss the many *variations* (diurnal, annual, secular, &c.), which the variation undergoes, often find an advantage in using the term declination for the element which undergoes such variations, over and above its being the term employed by physicists of all nations.]—*Editor*.

clearly indicated in the manuscript work of Peter Adsiger, existing in the Library of the University of Leyden, and bearing date 1269. In the same work, the author describes the compass as a means of directing the course of a ship at sea. (*Traité du Magnétisme*, par Cavallo, 3rd edition, Supplement.)

It is to Christopher Columbus that we owe the discovery of the change in the declination in the magnetic needle in passing from one part of the globe to another; he remarked it during his first voyage, on the 13th of September, 1492, being then two hundred leagues from the Isle of Ferro. The declination towards the west augmented continually as he advanced westward from that meridian, having been previously towards the east. (*Histoire de Colomb*, vol. i. p. 162., and *Las Casas*, book i. chap. vi.)

CHAP. VI.

ON THE CHANGE WHICH TAKES PLACE IN THE DECLINATION AT A GIVEN PLACE FROM YEAR TO YEAR.

The horizontal magnetic needle makes with the terrestrial meridian an angle which varies from year to year. Its direction appears to oscillate on either side of the terrestrial meridian in arcs of which we are not yet able to determine the amount.

According to the oldest observations made at Paris, the declination was at first easterly. From that time for more than two centuries the needle has moved gradually towards the west, as appears by the following figures:—In 1580, the declination was east, and was equal to 11° 30′. In 1618, it was still east, but not more than 8°. In 1663, the needle pointed direct towards the pole of the earth. It remained two years in this position, and has subsequently moved constantly from the pole towards the west.

The declination of the magnetic needle in 1667, according to the observations of the academicians made on the site where the Observatory was to be built, was on the 21st of June 15 minutes west. (*Acad. des Sciences*, t. i. p. 44.)

	Degrees.	Minutes.
In 1678 the west declination was already	1	30
In 1700 it was - - - -	8	10
In 1767 it was - - - -	19	16
In 1780 it was - - - -	19	55
In 1785 it was - - - -	22	0
In 1805 it was - - - -	22	5

Commencing with 1810, I have made regular determinations of the declination at the Observatory, with instruments by Lenoir and Gambey. I subjoin a table of all the results obtained up to the present year, 1853, indicating the day and hour of each determination. These are particularised, because at a given place the declination is subject to continual change; an important point, to which we shall revert in a special chapter.

Years.	West Declination.	Days and Hours of Observation.	Names of the Observers.
1806	21° 51′	16th May, at Noon.	Bouvard.
1807	22 25	7th October,	"
1808	22 19	7th October,	"
1809	22 6	24th February,	"
1809	21 55	11th August,	"
1810	22 16	13th March, at 1h. P.M.	Arago.
1811	22 25	15th October, at Noon.	"
1812	22 29	9th October, 2h. 30m. P.M.	"
1813	22 28	30th October, Noon.	"
1814	22 34	10th August, Noon.	"
1816	22 25	12th October, 3h. P.M.	"
1817	22 19	10th February, Noon, 30m.	"
1818	22 26	15th October, 9h. A.M.	"
1819	22 29	22nd April, 2h. P.M.	"
1821	22 25	26th October, Noon.	"
1822	22 11	9th October, Noon.	"
1823	22 23	21st November, 1h. 15m. P.M.	"
1824	22 23 15″	13th June, 1h. 15m. P.M.	"
1825	22 12 48	18th August, 8h. 40m. A.M.	"
1825	22 21 31	18th August, Noon.	"
1827	22 20	8th July, 1h. 8m. P.M.	"
1828	22 5 57	7th August, 8h. 7m. A.M.	"
1829	22 12 5	3rd October, 2h. 45m. P.M.	"
1832	22 3	4th March, 11h. 35m. A.M.	"
1835	22 4	9th November, 1h. 8m. P.M.	"
1848	20 41	22nd December, 1h. 45m. P.M.	Laugier & Goujon.
1849	20 34 18	30th November, 1h. 25m. P.M.	Mauvais & Goujon.
1850	20 30 40	4th December, 1h. 45m. P.M.	Laugier & Mauvais.
1851	20 25	16th November, 1h. 2m. P.M.	"
1852	20 19	3rd December, 2h. 12m. P.M.	"

Thus we see that, judging only by these results, it was about 1814 that the magnetic needle reached its greatest westerly declination; since that epoch, it has returned towards the east, but at first only very slowly. As at the end of its westerly excursion its progress was very slow, so the commencement of its course in the opposite direction could not but be slow also.

We were the first to announce (in the "Annuaire" for 1814) "that the progressive movement of the magnetic needle towards the west appeared to have become continually slower of late years, which seemed to indicate that after some little time longer it might become retrograde." "However," we added, "as the needle has already in former times been stationary for several years together, it will be prudent to await ulterior observations before definitively adopting this conclusion."

In 1817 (See "Annales de Chimie et de Physique," 2ème série, t. vi. p. 443), we thought we might dismiss our reserve. We then said: "On the 10th of February, 1817, at one hour after noon, the magnetic needle pointed 22° 19′ to the west of north. This observation, when compared with the results of the two preceding years, seems no longer to leave any doubt as to the retrograde movement of the magnetic needle."

This conclusion was not immediately admitted. Colonel Beaufoy at first thought to invalidate it from the observations which he had made in London from 1817 to 1819. (*Annales de Chimie et de Physique*, t. xi. p. 332.) But this skilful observer soon gave up his first impression, and came entirely into our views, which are now corroborated by a retrogression which has continued for nearly forty years. Our conviction had been based on more than twelve thousand observations, not of absolute declination, but of the needle employed in observing the diurnal variation, which could leave no room for doubt.

It was already sufficiently difficult to imagine what could be the kind of change in the constitution of the globe, which could act during one hundred and fifty-three years, in gradually transferring the resultant of the magnetic forces of the globe from due north to 23° west of north. We see that it is now necessary to explain, moreover, how it has happened that this gradual change has ceased, and has given place to a return towards the preceding state of the globe.

The gradual movement towards the west, as Cassini was the first to recognise, did not take place without several fluctuations.

The following table, borrowed from Gilpin, will show that the London observations had given results analogous to those of Paris, as respects the diminution of the westerly movement.

Year of Observation.	Name of Observers.	Declination observed.	Mean Annual Change of the Declination between these different Epochs.
1580	Burrows	11° 15′ E.	
			7′ 5″
1622	Gunter	6 0	
			9 6
1634	Gellibrand	4 6	
			10 6
1657	Bond	0 0	
			10 2
1665	Gellibrand	1 22 W.	
			9 7
1672	Halley	2 30	
			10 5
1692	„	6 0	
			16 0
1723	Graham	14 17	
			8 1
1748	„	17 40	
			8 4
1773	Heberden	21 9	
			9 3
1787	Gilpin	23 19	
			4 7
1795	„	23 57	
			1 2
1802	„	24 6	
			0 7
1805	„	24 8	

By observations, continued with a highly praiseworthy zeal, from 1817 to 1819, at Bushey Heath, near Stanmore, in 51° 37′ 42″ north latitude, and 1′ 20″·7 west longitude from Greenwich, Colonel Beaufoy came to the conclusion, that the needle reached the limit of its western digression in March, 1819, and that it is now moving eastward. The following is an epitome of his observations:—

		Declination in 1817.	Declination in 1818.	Differences.
January	Morning	-	24° 34′ 2″	
	Noon	-	39 57	
	Evening	-		
February	Morning	-	24 34 22	
	Noon	-	40 51	
	Evening	-		
March	Morning	-	24 33 18	
	Noon	-	41 37	
	Evening	-	33 47	
April	Morning	24° 31′ 52″	24 34 6	+ 2′ 14″
	Noon	44 43	44 50	+ 0 7
	Evening	35 58	36 36	+ 0 58
May	Morning	24 32 20	24 36 18	+ 3 58
	Noon	42 35	45 49	+ 3 14
	Evening	34 45	38 35	+ 3 50
June	Morning	24 31 9	24 33 47	+ 2 38
	Noon	42 14	45 11	+ 2 57
	Evening	34 45	37 40	+ 2 55
July	Morning	24 31 14	24 34 24	+ 3 10
	Noon	42 6	44 59	+ 2 53
	Evening	35 43	38 14	+ 2 31
August	Morning	24 33 2	24 34 40	+ 3 24
	Noon	42 51	45 58	+ 3 7
	Evening	33 45	37 50	+ 4 5

Month	Time	Declination in 1817.	Declination in 1818.	Differences.
September	Morning	24 33 2	24 34 29	+ 1 27
	Noon	41 36	45 22	+ 3 46
	Evening	34 38	37 28	+ 2 50
October	Morning	24 31 6	24 35 26	+ 4 20
	Noon	40 46	33 28	+ 2 42
November	Morning	24 31 49	24 33 24	+ 1 35
	Noon	37 55	41 41	+ 3 36
December	Morning	24 34 3	24 37 4	+ 3 1
	Noon	38 2	41 20	+ 3 18

Month	Time	Declination in 1819.	Declination in 1820.	Differences.
January	Morning	24° 35′ 42″	24° 34′ 6″	— 1′ 36″
	Noon	39 54	37 54	— 2 0
February	Morning	24 34 17	24 32 19	— 1 58
	Noon	39 55	38 7	— 1 48
March	Morning	24 33 18	24 30 47	— 2 31
	Noon	41 42	39 33	— 2 9
	Evening	35 17	33 45	— 1 32
April	Morning	24 32 36	24 30 38	— 1 58
	Noon	43 9	40 29	— 2 40
	Evening	34 59	31 58	— 3 1
May	Morning	24 32 42	24 30 42	— 2 0
	Noon	41 22	40 8	— 1 14
	Evening	34 10	33 0	— 1 10
June	Morning	24 31 28	24 29 50	— 1 38
	Noon	41 41	39 16	— 2 25
	Evening	35 9	33 48	— 1 21
July	Morning	24 32 31	24 28 41	— 3 50
	Noon	42 12	39 0	— 3 12
	Evening	34 24	33 26	— 2 11
August	Morning	24 32 33	24 30 25	— 2 8
	Noon	42 49	40 0	— 2 49
	Evening	34 24	33 14	— 1 10
September	Morning	24 32 29	24 31 16	— 1 13
	Noon	41 35	40 29	— 1 6
	Evening	33 27	32 59	— 0 28
October	Morning	24 33 27	24 31 0	— 2 27
	Noon	40 8	37 33	— 0 35
November	Morning	24 32 42	24 32 23	— 0 19
	Noon	38 43	37 38	— 1 5
December	Morning	24 33 29	24 33 3	— 0 26
	Noon	37 20	36 34	— 0 46

By subtracting the declinations observed in 1819 from those determined at the same hours in 1818, we should also find a column of negative differences, but beginning only from the month of April; the month of April in 1819 is therefore the epoch which Colonel Beaufoy's observations assign as the commencement of the retrograde motion of the magnetic needle.

In order to see at a glance what may be supposed to be the mean rate of easterly motion, we will collect all the observations made at the same hours in each year.

	Mean Declination	Differences
	Mean Declination in 1818.	**Differences between 1818 and 1819.**
Forenoon - - -	24° 34′ 33″	— 1′ 32″
Noon - - -	24 43 26	— 2 34
Afternoon - - -	24 37 10	— 2 27
	Mean Declination in 1819.	
Forenoon - - -	24° 33′ 06″	
Noon - - -	24 40 52	
Afternoon - - -	24 34 43	**Differences between 1819 and 1820.**
	Mean Declination in 1820.	— 1′ 50″
Forenoon - - -	24° 32′ 16″	— 1 48
Noon - - -	24 39 4	— 1 33
Afternoon - - -	24 33 10	

The mean annual retrogression was therefore equal to 1′ 57″.

The total motion towards the east between 1818 and 1820, was, according to this table: —

By the comparison of the morning observations -	3′ 22″
By that of the noon observations - - -	4 22
And lastly, by the evening observations - -	4 0

As these quantities are greater than the errors of observation, they indicate the retrograde movement of the needle with great probability; it is right, however, to remark, that a similar movement towards the east of 3′ appears to have taken place in London between the years 1790 and 1791, and that nevertheless the westerly march recommenced in 1792, and continued from that time in the same direction.

Colonel Beaufoy published in the May number of the "Annals of Philosophy," 1822, a detailed table of observations for the month of March of the same year. The mean declinations resulting from this table were: —

Morning, or 8h. 32m. A.M. - - -	24° 27′ 38″
Noon, or 1h. 29m. P.M. - - -	24 36 36
Evening, or 6h. 20m. P.M. - - -	24 28 45

These numbers, when compared with those of March, 1819, give for the retrograde motion of the north end of the needle during three years: —

By the morning observations - - -		5′ 20″
By those of an hour and a half past noon - -		5 6
And by the evening observations - - -		6 32
	Mean	5 46

whence the mean annual retrograde motion is 1′ 55″.

It would seem that the retrograde motion of the needle towards the west was manifested earlier in the more northern regions than in our parts of the globe: for I find in a memoir by Wlengel ("Annals of Philosophy," July, 1819, p. 57.), the following statement respecting the magnetic declination at Copenhagen:—

1649 - - - -	1° 30′ E.
About 1656 - - - -	0
1672 - - - -	3 35 W.
1806 - - - -	18 25 W.
1817 - - - -	17 56 W. on the 8th of September at 2h. P.M.

In 1737, at Tornea, the declination was 5° 5′ W. by a mean of the indications of four different needles (Maupertuis, *Fig. de la terre*, p. 152.); and in 1695, Bilberg had found it 7° W.

A circumstance worthy of notice, and which results from the preceding tables, is that the declination was null at Copenhagen sooner than in London and Paris, and also null in London sooner than in Paris.

CHAP. VII.

VARIATION OF THE DECLINATION AT DIFFERENT POINTS ON THE SURFACE OF THE GLOBE.

In passing from one place to another on the surface of the globe, we see the declination of the needle vary very sensibly; a fact first remarked by Columbus. In certain parts of the globe—in Europe, for example—the declination is at present westerly; in other parts it is easterly; and for a series of intermediate points, forming lines of no declination, the needle assumes a true north and south direction, or is directed towards the poles of the earth.

Hitherto three lines of no declination have been observed, and have been traced by navigators to more or less high latitudes. They have been drawn on many maps of the globe, but the changes continually taking place in the declination cause their positions and forms to be constantly varying. We have seen above, that one of the lines passed through Paris in 1663; since that period it has been continually advancing towards the west, for it now passes not far from Philadelphia.*

* [This sentence must have been penned before the publication, in 1819, of M. Hansteen's elaborate work entitled "Untersuchungen über den Magnetismus der Erde," or at least before M. Arago had found leisure to study its contents with the attention they deserve. In this work, M. Hansteen has collected all the most trustworthy magnetic observations from the earliest dates, and in reference to those of the *declination* in particular, has formed from them maps of the declination lines for the years 1600, 1700, 1710, 1720, 1730, 1744, 1756, 1787, and 1800, by which we are enabled to trace the general order of the progressive changes in form and position which those lines have undergone with a confidence previously unattainable. We learn from this assemblage and classification of the facts of observation, that the line of no declination referred to in the text—forming two branches, including between them a region of easterly declination which in 1600 overspread the whole of Western Europe, and of which the *western* branch passed over London in or about 1657, and Paris in or about 1663—has been since carried, by a progressive movement of translation, not towards the *west*, as erroneously apprehended by M. Arago, but towards the *east*. In the maps of the subsequent epochs, this system or arrangement of lines continually reappears, occupying ground progressively more and more to the east; and in more recent maps than M. Hansteen's, constructed from observations made subsequent to the publication of the "Magnetismus der Erde," it is seen to have now reached the eastern parts of Siberia, still presenting a similar arrangement of two branches of a line of no declination enclosing between them a region of easterly declination.

As in the middle latitudes of the northern hemisphere (in those parts, at least, which are now in question), the general movement of translation of the declination lines has been from west to east, so in the middle latitudes of the southern hemisphere—in the South Atlantic and Indian oceans, for example,—the general movement of translation of the declination lines during the same period has been in the opposite direction, or from east to west. Thus the South Atlantic branch of the line of no declination which, in 1600, skirted the African shore of the Atlantic from the Equator to the Cape of Good Hope (and prolonged itself still farther to the South in the same direction), has been carried by the movement of translation progressively across the whole breadth of the Atlantic to the position which it is seen to occupy in the map corresponding to the year 1840 (Phil. Trans., 1849.), entering the South American continent from the S.S.W., near Rio de Janeiro, and crossing the north-eastern angle of the South American continent in a prolongation of the same direction, to the equatorial regions, where it is now connected with a North American line of no declination, which passes not far west of Philadelphia. Thus, in the lapse of 250 years, the South Atlantic line of no declination, which in 1600 was connected through the tropics with the easternmost of the two (then European) branches of a line of no declination, has, by the opposite movements of translation which take place in the two hemispheres, been disjoined from that connection, removed across the whole breadth of the At-

The term "*magnetic meridians*" has sometimes been applied to lines, on moving along which with a compass the same angle of declination would constantly be found.

Lines drawn at the surface of the globe in directions always perpendicular to the magnetic meridians, have been called magnetic parallels. Captain Duperrey, in 1836, published maps containing these two kinds of lines, such as they then resulted from the observations of declination. As follows from what we have stated above, such lines vary with the lapse of time.

The greatest declinations of the magnetic needle were those observed during the voyages of Cook and De Langle. Cook, in 60° 51′ S. lat., and 95° 41′ east long., found the compass needle deviate 43° 6′ to the west; and De Langle, about the 62nd degree of N. lat., between Greenland and Labrador, observed a declination of 45°; the needle pointing, therefore, as much to the west as to the north.*

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lantic, and is now joined to a North American line of no declination; whilst, in the same interval, the two branches of what was the European line of no declination in 1600 have been carried by an opposite movement almost to the eastern limits of the Asiatic continent. The disjunction of the South Atlantic line of no declination from the European line, and its connexion with the American line of no declination, appear to have taken place before 1700; and the latter connection has ever since subsisted, although the form of the line, continuous through both hemispheres, has undergone considerable change in consequence of the different movements of translation which affect its different parts.

The phenomena which have been thus referred to are portions of that great system of secular magnetic change which, in a previous chapter (II.), is justly pronounced by M. Arago to be, amongst the facts which Nature presents to us, "the most astonishing, mysterious, and worthy of interest embraced in the vast domain of science." We have indeed made some progress of late years in acquiring a better knowledge of the facts than was previously possessed, and in laying a more substantial foundation for the determination hereafter of the elements of this great and constantly-progressive systematic change in the directive influence of the globe. But all attempts that have hitherto been made to connect the secular magnetic change with any other physical phenomena, either terrestrial or cosmical, have signally failed; and we can therefore only repeat M. Arago's inquiry, "How is it that the directive action of the globe, which is evidently the resultant of the actions of the molecules of which it is composed, can be thus variable, while the number, position, and temperature of these molecules, and, as far as we know, all their other physical properties, remain constant?"] —*Editor.*

* [It is curious that this paragraph, which must evidently have been written before the publication, in 1821, of the Narrative of the British Arctic Expedition in 1819-20, should have been left in its present state by M. Arago without either note or addition; and thus that an Essay bearing M. Arago's

CHAP. VIII.

ANNUAL VARIATION OF THE DECLINATION.

The magnetic needle, besides the general movement which carries it gradually from year to year towards the east or towards the west, — besides the diurnal variation of which we shall speak in the next chapter, — and besides the irregular variations which will engage our attention in a special notice on the Aurora Borealis, — is subject to an annual variation, discovered by Cassini, and which appears to be connected with the positions of the sun relatively to the equinoxes and solstices.

According to Cassini, in the interval from the month of January to that of April, the needle (at Paris) recedes from the North Pole, causing an increase of west declination.

From April to the beginning of July, or during the whole interval between the vernal equinox and the summer solstice, the declination diminishes, or, in other words, the north end of the needle returns towards the pole.

After the summer solstice, and until the next vernal equinox, the needle, according to Cassini, resumes its march towards the west, in such manner as to make its direction in October nearly identical with its direction in May; the westerly movement from October to March being less than in the three preceding months.

Briefly summing up this statement, we find an easterly retrograde movement in the three months from the vernal equinox to the summer solstice, and in the remaining nine months of the

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name, and published in 1854, should still record, as the greatest amount of declination ever observed in the Northern hemisphere, 45° W., observed by De Langle on the coast of Labrador! M. Arago was far too much interested in magnetic researches, and too well-informed respecting them, not to have learned the far more remarkable directions of the compass-needle, which have been observed by all the British expeditions which have passed through Barrow's Strait; and his writings abound in instances in which he has shown no indisposition to recognise the advances in scientific knowledge made by other nations than his own. On every account, therefore, the omission must be regarded as one of pure inadvertence.

Reference has been made in this note more particularly to the Expedition of 1819-20, because it was the first which crossed, in its westerly course in 1819, and recrossed, in its return in 1820, the line of 180° declination, — in other words, the line in which the north end of the needle points due south, and which, in the *amount* of the declination, can never be exceeded.] — *Editor.*

year a general movement in the opposite direction, or towards the west.

As the diurnal variation of a magnetic needle is of very sensible amount, so that many different declinations may be observed in the course of every twenty-four hours, it may be asked which of these different quantities are employed in the establishment of the above laws? I answer that these laws could be established by employing either all the maxima, or all the minima, of the daily observations; but that, in order to render Cassini's results numerically comparable with others which I shall cite later, I have deduced from his data the values of the mean declinations, and have formed the following table from them only.

I ought to state that, both here and in what has gone before, I have taken, as the mean declination of the needle on a given day, the half sum of the greatest and least declination observed on that day; without examining for the present whether that half sum correctly represents the true mean in the mathematical sense of that expression.

The mean declination corresponding to the month is obtained by adding together the means of all the days in the month, and dividing the sum by the number of those days.

Table of Mean Declinations at Paris.

	1784.	1785.	1786.	1787.	1788.	Mean of 5 Years.
January	— 4′ 29″	18′ 19″	27′ 3″	33′ 9″	39′ 31″	22′ 43″
February	— 4 53	20 2	27 36	37 42	41 25	24 22
March	+ 2 53	19 44	28 36	48 59	40 46	28 12
April	+ 3 39	19 12	30 47	49 58	53 21	31 23
May	+ 2 39	17 31	27 51	46 47	49 58	28 57
June	— 2 59	14 26	17 43	40 4	46 46	23 12
July	— 2 31	14 26	20 56	35 26	46 17	22 55
August	— 0 58	15 39	20 39	37 50	45 19	23 42
September	+ 3 13	18 9	24 57	42 33	46 17	26 2
October	+ 9 58	21 11	30 54	47 42	52 6	32 22
November	+12 18	26 32	26 52	35 18	54 42	31 08
December	+13 54	27 13	32 30	39 12	52 1	32 58

The negative numbers contained in the column for 1784, show that in that year the needle pointed to the right of the zero of the scale. The degrees are not given, as they are useless in the present question.

Let us now assemble, in the same manner, the observations made in London by Gilpin, near the epochs of the equinoxes and solstices.

Table of Mean Declinations at London.

Years.	March.			July.			September.			December.		
1793	23°	48′	8″	23°	48′	5″	23°	52′	6″	23°	52′	3″
1795	23	57	5	23	57	1	24	0	4	23	59	4
1796	24	1	1	23	58	7	24	0	1	24	1	3
1797	24	1	5	24	0	2	24	1	4	24	1	3
1798	24	0	6	24	0	0	24	1	4	24	1	4
1799	24	1	1	24	0	6	24	2	9	24	2	3
1800	24	3	6	24	1	8	24	3	6	24	3	3
1801	24	5	2	24	2	8	24	3	8	24	5	4
1802	24	6	9	24	5	3	24	8	7	24	6	8
1803	24	8	0	24	7	0	24	10	5	24	10	7
1804	24	9	4	24	6	0	24	8	9	24	9	0
1805	24	8	7	24	7	8	24	10	0	24	9	4
	24	2	7	24	1	3	24	3	7	24	3	6

These observations, like those at Paris, give a maximum of declination about the vernal equinox, and a minimum at the summer solstice; but the amount of difference, or the amplitude of the oscillation, is here much less. This smaller amount does not seem to me to be capable of explanation by imperfections of the point on which Gilpin's needle was suspended, since the same observations show, in the different seasons, diurnal variations as great as those indicated by a needle suspended by a silken thread.* Without professing to point out in this place the cause of so singular a difference, I may be permitted some comparative remarks which may not be without interest.

The period of 1786, when Cassini was observing, and that of 1800, to which Gilpin's determinations correspond, appear to me to differ essentially from each other as regards magnetism in one single respect only; this is, that in 1786 the annual change of mean declination was 9′, and in 1800 scarcely 1′; now is it not worthy of notice, that the retrograde movement of the needle between the spring equinox and the summer solstice

* The mean values of these diurnal variations in London, according to Gilpin, in the interval comprised between 1793 and 1805 are:

In March	8′	5″
In June	11	2
In July	10	6
In September	8	7
In December	3	7

should have diminished coincidentally with the general movement towards the west? If these two phenomena are really connected with each other, the spring retrogression ought no longer to take place, since we have seen that the west declination has reached its maximum, and is even beginning to diminish. In grouping Colonel Beaufoy's observations, as I have done in the following table, I find a confirmation of my conjecture.

Table of mean Declinations, deduced from Colonel Beaufoy's Observations, corresponding to the different Months of the Years 1817, 1818, 1819, *and* 1820.

	1817.	1818.	1819.	1820.	Mean of the 3 last Years.
January	24° ′ ″	24° 36′ 59″	24° 37′ 48″	24° 36′ 0″	24° 36′ 56″
February		37 37	37 6	35 13	36 39
March		37 27	37 30	35 10	36 42
April	38 47	39 28	37 52	35 33	37 38
May	37 28	41 4	37 2	35 25	37 51
June	36 42	39 29	36 35	34 33	36 52
July	36 40	39 37	37 22	33 51	36 57
August	37 4	40 19	37 41	35 13	37 44
September	37 18	39 25	37 2	35 53	37 27
October	35 53	34 27	38 47	35 17	36 10
November	34 52	37 33	35 43	35 1	36 6
December	36 3	39 12	35 25	34 49	36 29

We here see that the periodical variation discovered by Cassini and confirmed by Gilpin no longer takes place. May not this variation, after having thus ceased, manifest itself afresh after a time, either in a contrary direction or at other periods of the year, when the movement of the needle towards the west shall have become a little more rapid? Future observations will resolve this question; meanwhile I transcribe a table which I find in the Memoirs of the American Academy, containing declinations determined at Salem in the United States, in 1810, by Bowditch. The reader will remark that at Salem the declination is west, and that it has been gradually diminishing, at the rate of about 2′ a year, for many years past.

Mean Declinations.

April, 1810 - - - - -	6° 21′ 21″	West.
May - - - - - -	23 36	„
June - - - - - -	25 42	„
July - - - - - -	28 51	„

August	6°	29′ 44″	West.
September		25 21	„
October		21 42	„
November		19 11	„
December		12 35	„
January, 1811		20 55	„
February		21 19	„
March		20 29	„
April		23 39	„
May		21 38	„

These results no longer offer any trace of Cassini's period; for the declination, so far from diminishing between the spring equinox and summer solstice, increased gradually from April to August. In compensation, a sensible diminution will be remarked between September and December. Might it not be inferred that the period in question still subsists, but that it has transferred itself from the spring to the autumn? Should this conjecture, for which I fully admit Bowditch's few observations form an insufficient basis, be confirmed, the annual variation will appear to be regulated by very simple principles.

When, the declination being west, the needle recedes from year to year from the meridian, it undergoes in the spring a retrograde movement, forming a partial return towards the meridian. (This is what Cassini discovered.)

This retrograde movement is the more extensive as the yearly change of the declination is greater. (This results from the comparison of Cassini's observations with those of Gilpin.)

The variation disappears, and all the months of the year give nearly the same mean declination, when the needle having arrived at the limit of its westerly digression, the yearly change of the declination is null. (Beaufoy's observations.)

Lastly, when the east declination is diminishing from year to year, the only remarkable movement towards the east observed in the needle is in the months from September to December. (Bowditch's observations.)*

* [The observations at the Magnetic Observatory of St. Helena (one of the colonial observatories established by the British Government at the joint instance of the Royal Society and of the British Association for the Advancement of Science) afford just the opportunity which M. Arago would have desired, of testing the general applicability and correctness of the principles by which he had been led to conjecture that the annual variation might be regulated. The conditions at St. Helena are precisely those which are contemplated in the two first of the four concluding paragraphs of this Chapter. First, "the declination is west, and the needle re-

CHAP. IX.

DIURNAL VARIATION OF THE DECLINATION.

THE discovery of the diurnal variation of the magnetic needle goes back to the year 1722, and was made by Graham. Since

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cedes from year to year still more from the meridian;" according to the first paragraph, therefore, there should be "a retrogade movement, or a partial return towards the meridian in the 3 months from the vernal equinox to the summer solstice." And, second, the yearly change of the declination at St. Helena is very considerable, being little less than 8 minutes in the year; the retrograde movement in the 3 months should, therefore, according to the second paragraph, be also considerable in amount.

Before we proceed to the facts, it may be proper to premise that, in combining the observations at St. Helena to obtain their mean results, the mean declination on a given day is taken, not, as in the case of the Paris observations, from the "half sum of the greatest and least declinations observed on that day," but from the arithmetical mean of all the observations in the 24 hours, which are made for that purpose at equidistant intervals. This is the more onerous, but the more exact, method when circumstances permit its accomplishment. With this distinction, and deriving, as M. Arago has done, the mean *monthly* declinations from the sum of all the mean daily declinations in the month divided by the number of days, we have the following mean values for the declination at St. Helena from September 1. 1842, when a series of *hourly* observations commenced, to August 31. 1847, when the hourly series terminated. The declination is west, and the degree, which for convenience is omitted, is always 23.

	1842.	1843.	1844.	1845.	1846.	1847.
January - -		10′·38	16′·24	23′·29	31′·50	39′·57
February -		11·38	17·00	24·92	31·71	40·58
March - -		11·52	17·75	24·87	32·47	41·32
April - -		12·08	18·19	25·33	33·76	42·38
May - -		12·66	18·54	26·22	34·24	42·99
June - -		13·05	18·93	25·85	34·77	42·33
July - -		14·56	19·77	26·67	35·46	42·92
August - -		15·37	20·50	26·89	36·13	44·46
September -	7′·59	15·62	19·55	27·47	36·97	
October - -	8·13	15·54	20·32	28·53	37·66	
November -	9·15	15·86	21·10	29·44	38·10	
December -	9·97	15·48	22·10	30·82	38·66	

On looking at these numbers, we perceive at the first glance the influence of the secular change in the progressive increase of west declination. The progressive increase, however, from month to month is not absolutely without exceptions; and our first business must be to examine the amount of that time, this curious phenomenon has fixed the attention of many observers, and nevertheless it must be confessed that it is

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retrogression in these exceptional cases, and the months in which they occur. They are as follows:

October	1843,	an easterly retrogression of - -	0′·08
December	1843,	" " - -	0 ·38
September	1844,	" " - -	0 ·95
March	1845,	" " - -	0 ·05
June	1845,	" " - -	0 ·37
June	1847,	" " - -	0 ·66

In 60 months, therefore, there were but 6 instances of retrogression, or partial return towards the meridian; and these with no appearance whatsoever of systematic occurrence. In the daily records from which the monthly results are derived, no separation has been made of observations taken at hours when, from the indications of all the magnetometers, it was evident that a magnetic disturbance was in progress; and without this separation, the mean monthly results must necessarily be subject to slight occasional irregularities, such as are here shown. We may, however, diminish the influence of these irregularities in disturbing the judgment, whilst we at the same time preserve the integrity of the record, by combining into a single result, without any separation of disturbed observations, the means of the same month in each of the 5 years; whereby we shall have the progression of the secular change of the declination in the different months of a single year, with the additional advantage of an accuracy derived from 5 years of observation, in which irregularities either occasional or accidental may be supposed to have in some measure compensated each other. The *mean* year is, then, as follows:

September	(1842 to 1846 inclusive) - - -	23°	21′·44
October	(1842 to 1846 inclusive) - - -	23	22 ·04
November	(1842 to 1846 inclusive) - - -	23	22 ·73
December	(1842 to 1846 inclusive) - - -	23	23 ·41
January	(1843 to 1847 inclusive) - - -	23	24 ·20
February	(1843 to 1847 inclusive) - - -	23	25 ·12
March	(1843 to 1847 inclusive) - - -	23	25 ·59
April	(1843 to 1847 inclusive) - - -	23	26 ·35
May	(1843 to 1847 inclusive) - - -	23	26 ·93
June	(1843 to 1847 inclusive) - - -	23	26 ·99
July	(1843 to 1847 inclusive) - - -	23	27 ·88
August	(1143 to 1847 inclusive) - - -	23	28 ·67

There is here no retrogression whatsoever; the increase of west declination is throughout the year progressive from month to month; manifesting that a nearly equal portion of the annual change (which on the average of the 5 years of hourly observation was 7′·88 yearly) takes place in every month of the year.

If we now extend this examination so as to include, in addition to the 5 years of hourly observation, two other periods,—one of 15 months, from June 1. 1841 to August 31. 1842, when the observations were 2 hourly, and one of 21 months, from September 1. 1847 to May 31. 1842, when 5 obserstill enveloped in great obscurity. All physicists admit that in Europe the north end of the needle moves daily from east to

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vations were taken in each day, the hours being such as to give by their combination a true mean value for each day,—we have the results of 8 years or 96 consecutive months, from which we may obtain conclusions of even greater exactness than from a period of 5 years. From the 8 years, then, we find the average rate of secular change in each year to have been 7′·91, and the mean declination in the several months as follows:

June	(1841 to 1848 inclusive) - - -	23° 23′·42
July	(1841 to 1848 inclusive) - - -	23 24·45
August	(1841 to 1848 inclusive) - - -	23 24·91
September	(1841 to 1848 inclusive) - - -	23 25·30
October	(1841 to 1848 inclusive) - - -	23 26·32
November	(1841 to 1848 inclusive) - - -	23 27·07
December	(1841 to 1848 inclusive) - - -	23 27·73
January	(1842 to 1849 inclusive) - - -	23 28·29
February	(1842 to 1849 inclusive) - - -	23 29·23
March	(1842 to 1849 inclusive) - - -	23 29·76
April	(1842 to 1849 inclusive) - - -	23 30·21
May	(1842 to 1849 inclusive) - - -	23 30·69
	Mean, corresponding to December 1.	23 27·28

Here, again, we have no easterly retrogression whatsoever; but, on the contrary, a progressive increase of west declination from month to month, differing even less from an uniform progression than the previous results derived from the shorter period of 5 years; as might, indeed, on the supposition of the *law* being that of an uniform progression, have been expected.

If, in conformity with this indication, we assume the law of the secular change at St. Helena during the period of these observations to be an annual increase of west declination, *taking place by equal portions in each month of the year*, — and if we regard the mean of the 12 months (virtually the mean of the observations of 96 consecutive months) as probably a more exact determination than that of any one of the twelve monthly means from which it is derived (each of which represents only eight monthly means), we shall have, by applying the proper aliquot portions of the annual secular change corresponding to the several months to the mean declination on the 1st December, a series of *calculated* values of the mean declination in each month, which we may view as the most exact deduction which the observations are capable of yielding; and by comparing these with the monthly values *severally observed*, we shall have the best attainable representation of the annual variation deducible from so long a continuance of very careful and exact observation.

	Calculated Values. (*a*)	Observed Values. (*b*)	Difference. (*a*—*b*)
June - -	23° 23′·64	23° 23′·42	+0′·22
July - -	23 24·31	23 24·45	—0·14
August - -	23 24·97	23 24·91	+0·06
September - -	23 25·63	23 25·30	+0·33
October - -	23 26·29	23 26·32	—0·03

west, from sunrise to about an hour after noon, and then returns towards the east; they also admit that the extent of these diurnal oscillations is greater in summer than in winter. But is all this well assured? Is it also true that geographical posi-

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	Calculated Value.	Observed Value.	Difference.
November - -	23° 26′·95	23° 27′·07	—0′·12
December - -	23 27 ·61	23 27 ·73	—0 ·12
January - -	23 28 ·27	23 28 ·29	—0 ·02
February - -	23 28 ·93	23 29 ·23	—0 ·30
March - -	23 29 ·59	23 29 ·76	—0 ·17
April - -	23 30 ·25	23 30 ·21	+0 ·04
May - -	23 30 ·92	23 30 ·69	+0 ·23

The differences are in all cases so small as to give the strongest presumption that the hypothesis upon which the calculation is based, *of an uniform distribution of the secular change in the several months of the year*, represents a true natural law; to be received in preference to the supposition in the text, that, "the declination being west, when the needle recedes from year to year from the meridian, it undergoes a retrograde movement, or a partial return towards the meridian, between the vernal equinox and the summer solstice, which retrograde movement is the more extensive as the yearly change of the declination is greater." It is possible that, by a still longer continuance of the observations at St. Helena than the eight years of which the results have been here given, the differences in the final column, small as they are, might be still further reduced; but, from the character of the signs by which the different months are affected — those where the sun is north of the Equator having all (with the possibly-accidental exception of July) a + sign, and those when the sun is south of the Equator having all a — sign,—it is probable that, *wholly unconnected with the secular change*, there is a small annual variation in the mean declination at St. Helena (*i. e.* the declination resulting from the *mean* of all the hours of every day), the character of which is, that the declination is on the average less westerly from April to September, and more westerly from October to March; the average difference in the two semiannual periods not however exceeding fifteen seconds. A closer examination of the observations than has yet been made, may show whether any, and what portion of this small annual variation may be due to the preponderance of the occasional disturbances in one or the other direction at different seasons; or whether it be in whole or in part the result of the remarkable change in the *diurnal* variation which takes place at St. Helena when the sun passes from one hemisphere to the other.

At stations where phenomena may present themselves such as those referred to by M. Arago—viz., where the mean declination undergoes a progressive change in *the one direction* from the vernal equinox to the summer solstice, and a progressive change in *the opposite direction* of nearly equal amount from the summer solstice to the autumnal equinox, so that "the direction in October may be nearly identical with the direction in May,"—there appears far more reason to believe that the explanation will be found in connection with the season-changes which the diurnal variation undergoes, than in the cause conjectured by M. Arago, viz., a retrogression of the secular change during a portion of the year.] —*Editor*.

tion modifies these phenomena, and that, as some observers have believed, the displacement of the needle in the course of the twenty-four hours is much less near the terrestrial equator than in our climates?

The Petersburg academicians have several times announced that in that city the declination does not vary from the morning to the evening, or from one day to the next, or even from one year to another. Notwithstanding the confidence which the names of Euler, Krafft, &c. inspire, ought so extraordinary an anomaly to be admitted until founded on a great number of observations made with very precise instruments?

When in the observation of the diurnal oscillations of the magnetic needle, exactness is pushed to seconds of a degree, no two days in the year are found perfectly alike. This may be due to the perpetual changes occurring in atmospheric circumstances; but it is useless to attempt hypotheses on this subject until exact and corresponding observations shall have taught us whether these perturbations are local, or whether they occur simultaneously at distant places.

Hitherto the study of magnetic phenomena has been impeded principally by two causes; one, the want of corresponding observations in places sufficiently distant; the other, the imperfection of instruments. The Bureau des Longitudes has recently had a very exact instrument, by Fortin, placed at the Observatory, which will in future much assist the attention this branch of physics so well deserves. For my own part, I have made continuous observations on this subject from 1818 to 1835, the discussion of which will be given in the following Chapter.

If I am not mistaken, there formerly existed in Europe only one place (the observatory of Bushey Heath, near London) where the diurnal variation of the needle was regularly observed. It is to be regretted that its proprietor, Colonel Beaufoy, whose merit all physicists must recognise, should have used a needle supported on a point, instead of a needle suspended to a thread without torsion, as Coulomb had done.

Colonel Beaufoy deduced from his observations the following values for the diurnal variations in the different months of the year. I will remark here, once for all, that the forenoon observations were made generally at forty minutes after eight, those of the middle of the day at twenty minutes after one, and those of the evening at fifty minutes after seven. Therefore the morning variations represent the movement of the needle between 8 h. 40 m. A.M. and 1 h. 20 m. P.M., and those of the evening the contrary movement, which takes place between 1 h. 40 m. and 7 h. 50 m. P.M.

		1817.	1818.	1819.	1820.	Means.
January	Morning		5′ 55″	4′ 12″	3′ 48″	4′ 28″
	Evening					
February	Morning		6 29	5 38	5 48	5 59
	Evening					
March	Morning		8 19	8 24	8 46	8 30
	Evening		7 50	6 25	5 48	6 41
April	Morning	12′ 51″	10 44	10 33	9 31	11 0
	Evening	8 45	8 14	8 10	8 31	8 25
May	Morning	10 15	9 31	8 40	9 26	9 28
	Evening	7 50	7 14	7 12	7 8	7 21
June	Morning	11 5	11 24	10 13	9 26	10 32
	Evening	7 29	7 31	6 32	5 28	6 45
July	Morning	10 52	10 35	9 41	10 19	10 22
	Evening	6 23	6 45	6 35	5 34	6 19
August	Morning	11 35	11 18	10 16	9 35	10 41
	Evening	9 6	8 8	8 25	6 46	8 6
September	Morning	8 34	10 53	9 6	9 13	9 27
	Evening	6 58	7 54	8 6	7 30	7 37
October	Morning	9 40	7 52	6 41	8 33	8 11
	Evening					
November	Morning	6 6	8 17	6 10	5 15	6 25
	Evening					
December	Morning	3 59	4 16	3 51	3 31	3 54
	Evening					

The evening observations are missing in the months of January, February, November, and December, from want of light to see the distant marks.

It may be noticed that the mean extent of the daily oscillations of the needle for the different months of the year is comprised between three and twelve minutes, the maximum amount being in April and May, and the minimum in December.

The extent of the diurnal variation is not, therefore, the same in all months of the year; it is also variable in different parts of the earth.

Many atmospheric circumstances, and, most of all, auroras, exercise a sensible influence on the extent of the diurnal variations of the needle. This extent also seems to diminish in approaching the equator, and perhaps also in approaching places where the absolute declination is very small. At St. Helena and Sumatra, for example, the diurnal variations hardly exceed 2′ or 3′.

But on this point fresh observations are required, to be continued for a sufficient length of time, and to be made with good instruments.

In the *northern* hemisphere that end of a horizontal magnetic needle which is turned towards the north moves:—

From east to west from a quarter after eight in the morning to an hour and a quarter after noon.

From west to east from an hour and a quarter after noon until the evening.

Our hemisphere cannot enjoy any privilege in this respect; what happens in it to the north end of the needle must happen in the southern hemisphere to the south end. Therefore,

In the *southern* hemisphere that end of a horizontal magnetic needle which is turned towards the south must be supposed to move:—

From east to west from a quarter past eight in the morning until an hour and a quarter after noon.

From west to east from a quarter after one in the middle of the day until the evening.

Observation has, indeed, been found to be in accordance with this reasoning.

We will now compare the simultaneous movements of the two supposed needles, referring in both to the same end; *i.e.* the end which points to the north.

As in the *southern* hemisphere the point turned towards the *south* moves:—

From east to west from a quarter after eight to a quarter after one, the north point of the same needle must move at the same time in the opposite direction, therefore finally,

In the *southern* hemisphere the end of the needle which points towards the *north* moves:—

From west to east from a quarter after eight in the morning to a quarter after one in the middle of the day: being precisely the opposite movement to that executed at the same hours by the same, or north, end of the needle in the northern hemisphere.

Suppose an observer quitting Paris and advancing towards the equator, on his way to regions situated in the southern hemisphere. So long as he is in our hemisphere, he sees the north end of the needle perform every morning a movement towards the west; in the opposite hemisphere the north end of the same needle will perform, as we have said, every morning, a movement towards the east. It is impossible that this transition from the western to the eastern movement should take place suddenly; there must necessarily be between the zone in which one of these movements is observed, and that in which the second takes place, a line on which, at the same hours, the needle will neither move to the east nor to the west, that is to say, in which it will remain stationary.

Such a line cannot but exist; but where is it to be found? Is it the magnetic equator, the geographical equator, or some isodynamic line or line of equal magnetic intensity?

Observations continued for many months at places situated in one of the spaces comprised between the geographical and magnetic equators, such as Pernambuco, Payta, Conception, the Pellew Islands, &c., would certainly conduct to the desired solution; but several months of assiduous observation would be necessary; for, notwithstanding the skill of the observer, the short sojourns made by Captain Duperry, at the request of the Academy at Conception and Payta, have left some doubts.*

Science has been enriched during some years past by a considerable number of observations of the diurnal variation of the declination; but they have been principally made on islands, or on the western coasts of continents. Analogous corresponding observations on the eastern coasts of continents would be very useful; they would serve to subject to an almost decisive test the greater part of the explanations by which it has been attempted to account for this mysterious phenomenon.

For my own part, I have always thought that the diurnal variations of the magnetic needle were connected with the march of the sun. I was happy to see my ideas in this respect supported, in a manner, which I placed on record on the 26th of July, 1837, at a meeting of the Bureau des Longitudes, mentioned as follows in the procès verbal of the meeting.

"M. Arago announced the return of M. d'Abbadie. M. d'Abbadie says that he had observed, that, as M. Arago had thought might be the case in the tropical regions, the diurnal variation of the declination changed completely at Pernambuco from the moment when the sun passed from one side of the zenith to the other."

* See note by the Editor at the close of this chapter, p. 346.

In order to give more perfection to the observations of diurnal variation, perhaps it might be advantageous to endeavour to magnify their amplitude. It is for this reason that I would call the attention of physicists to the following analysis of a memoir, read on the 5th and 12th of June, 1823, to the Royal Society, by Mr. Barlow of Woolwich; which analysis I inserted at the time in the "Annales de Chimie et de Physique."

"Mr. Barlow thought that by attenuating the action which the earth exercises on a magnetic needle, as mineralogists are accustomed to do when they wish to discover faint traces of iron in bodies, the diurnal variations would be rendered much more sensible than they are naturally. Following out this idea, he found that the best way of attaining the object was to present to one pole of the needle the same pole of a larger, or bar magnet, and to the other pole of the needle the opposite pole of another such bar. In this way the diurnal variation of a horizontal needle, which had before been only of a few minutes, was raised, first to 3° 40′, afterwards to 7° 0′, and, in short, as high as desired.

"By bringing the two opposite bars nearer to each other and to the needle, the needle may be made to deviate to any amount from the magnetic meridian, and its diurnal variations may be observed in all positions, — *i.e.*, with its north end directed south, east, west, &c., &c. Mr. Barlow always found that the diurnal variations were greatest when the needle pointed east or west; and that they became almost imperceptible when it pointed nearly N.N.W. or S.S.E. From N.N.W. to S., the principal diurnal movement carried the north end of the needle towards the true north; between S.S.E. and N. the same end also moved towards the north. Thus the movements in these two cases were performed in opposite directions.

"A horizontal needle which was made to turn north or south under the influence of bars, when placed inside Mr. Barlow's house, made its principal diurnal excursion towards the north, whereas, in his *garden*, this movement was towards the south. He assured himself that this singular anomaly did not depend on any change in the relative positions of the bars and needle. Having suspected that light might be the cause of the phenomenon, he observed for two successive days with the windows of the room always closed: the irregularity continued, but was lessened. Lastly, imagining that the magnetic force of a stove of metal might undergo a diurnal variation, he had a shell brought into the garden, and placed, relatively to the needle, in a position corresponding to that occupied by the stove in the house. After this change, the maximum effect, instead of taking place at seven in the morning, was observed at four in the afternoon; but the anomaly in the direction of the movement continued.

"Mr. Christie, whose house is at some distance from Mr. Barlow's, remarked a similar anomaly.

"Our author appears disposed to attribute the diurnal variations to a change in the magnetic intensity of the globe produced by the action of the solar rays. This change would depend, as to its amount, on the sun's declination,—that is to say, on the sun's position relatively to the plane of no attraction. The experiment made in the dark room led him to think that the exciting cause of the diurnal variations might be in the luminous, and not in the calorific, rays of the sun.

"Professor Christie thinks, on the contrary, that the change of the declination depends on the heating, and not on the luminous, rays. A change of temperature of one degree of Fahrenheit in the bars placed near the needle altered the position of the latter a degree. By warming one of the bars with the hand, the needle changed its direction two or three degrees, in experiments made in the presence of Messrs. Oersted and Barlow.

"I think Haüy was the first person who pointed out the advantages which, in experiments where it was desired to take cognizance of very small magnetic actions, might be obtained by the use of a delicately suspended needle, of which the directive power should have been weakened by placing near it a bar magnet in a suitable position. (See *Traité des Pierres précieuses*, p. 176. et seq.) Biot has since pointed out this method as suited to augment almost indefinitely the amount of the diurnal variations. (*Traité élémentaire de Physique*, ii. 101. second edition, 1821.) It is, therefore, Biot's project which was realised by Barlow."*

* [The establishment of a magnetic observatory at St. Helena, and the comparison of the changes which take place in the diurnal variation at that intertropical station at different parts of the year, with the corresponding phenomena at Toronto and Hoberton — stations in the middle latitudes re-

CHAP. X.

M. ARAGO'S OWN OBSERVATIONS ON THE DIURNAL VARIATION OF THE DECLINATION AT PARIS, FROM 1818 TO 1835.

THE Bureau des Longitudes established at the Paris Observatory an instrument to be devoted exclusively to the above object; the observations were commenced in September, 1818. In the course of 1819 the steel bar, which was suspended with

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spectively of the northern and southern hemispheres — have tended greatly to dissipate the obscurity in which some points adverted to in this Chapter were involved at the time when M. Arago's Essay was written. The diurnal variation (as its name, indeed, implies) proceeds obviously from some influence, direct or indirect, of the celestial body which "rules the day;" and the knowledge that, at the same hour of *solar* time, and therefore simultaneously in the same meridian, the needle was moving towards the east in the one hemisphere, and towards the west in the other hemisphere, led not unreasonably to M. Arago's supposition, that in some intermediate latitude the needle would move neither to the east nor to the west, but would remain stationary; and that there must exist a line, connecting such points in the different meridians, and therefore encompassing the globe, where diurnal variations would not be found, and the needle would remain stationary throughout the twenty-four hours. Such, however, does not appear to be the fact. The difference previously known, and occasionally adverted to by M. Arago, as existing in the *middle* latitudes between the diurnal variation in *summer* and in *winter*, and supposed by many to be produced in some way or other by the difference of the temperature of those seasons, has been shown, by the observations at St. Helena, to be not a partial but a general phenomenon, and to subsist equally in the tropics where the ordinary distinctions of season cease; causing in such localities a diurnal variation which, in the months when the sun is in the *northern* signs, corresponds in character with the increase of the diurnal variation in the *northern* hemisphere in the same months, and in the months when the sun is in the *southern* signs, a diurnal variation corresponding in character to the increase of the diurnal variation in the *southern* hemisphere in the same months. There is, therefore, no such line as that imagined by M. Arago, where no diurnal variation is found; but within a certain equatorial zone there is an approximation to such a state twice in the year, continuing, however, but for a very few days on each occasion. The occasions are when one of the two semiannual phases of the diurnal variation passes into the other. The epochs of this passage, as shown distinctly by the St. Helena observations, coincide with the equinoctial epochs, when the sun crosses the terrestrial equator from the one hemisphere into the other. The change in the directive influence exercised on the magnetic needle, which accompanies or appears to immediately follow these epochs, is, that between the March and the September equinoxes the north end of the needle is more to the east in the hours of the forenoon, and more to the west in those of the afternoon; and between the September and the March equinoxes, more to its flat surfaces horizontal, experienced a sudden change of direction *without any apparent cause;* the diurnal variation was at the same time reduced to little more than a tenth of its previous value, while the magnetic intensity had considerably increased. When I had found that this new state of things continued, I had the instrument taken down, and after having modified some of its parts, I again suspended the bar to a thread without torsion, but this time edgewise, or with the broader surface vertical, instead of horizontal as before. These alterations only permitted me to commence, in February, 1820, a fresh series of observations, which has been continued with great regularity to November, 1835. My first intention had only been to examine, whether by means of the declination corresponding to the different hours of the day in two successive years, I should find a confirmation of the retrograde movement of the needle towards the north, which detached observations had already indicated. I was afterwards induced to extend the work much beyond what would have been required for this first question, in the hope that by comparing my observations with those which the English navigators purposed to make at the same time in the polar regions, some useful results might be arrived at. The study of the writings of those who had preceded me soon taught me that, notwithstanding the enormous mass of observations of diurnal variation made at different times and places, several capital circumstances

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the west in the forenoon, and to the east in the afternoon, than its mean position in the year at the respective hours,—the change being similar in character, and nearly so in amount, in the middle latitudes of both hemispheres and in the equatorial regions.

In the report which M. Arago made to the Bureau des Longitudes in 1837, no particulars are stated as to the nature of the "complete change" which M. d'Abbadie is said to have observed to take place in the diurnal variation at Pernambuco "at the moment when the sun passed from *one side of the zenith to the other.*" It is probable that the change thus noticed by M. d'Abbadie was the same of which the St. Helena observations have given the more precise character; and it may well have happened, that the means at that gentleman's command may not have enabled him to make a sufficient number of observations, or sufficiently exact, to discriminate between epochs so little removed from each other as the sun's passage of the equator, and his passage of the zenith, at Pernambuco, the latitude of which place is not more than 8° 3′. The situation of St. Helena is more favourable for this purpose, its latitude being 15° 56′; the sun consequently passes from the one side of its zenith to the other about the 5th of February and 6th of November,—epochs sufficiently remote from the equinoxes on the 21st of March and 21st of September.]—*Editor.*

still demanded fresh investigations. It appeared to me, for instance, that the hours of the mean *maxima* and *minima* had not been exactly determined; that it was not known whether these hours were the same at all seasons, &c. &c. I then determined to consult the needle each day, every quarter of an hour for an hour and a half in the morning about the time of *minimum* declination, and every quarter of an hour for an hour and a half after mid-day about the time of the *maximum*.

I have found that at Paris the march of the horizontal needle is *usually* very regular. On every day, during the same week, it makes excursions of equal amplitude, to within a few seconds. The hours of the maxima and minima of declination are so constant that one might really set one's watch by them to within a quarter of an hour. This circumstance has permitted me to give to our observations a great degree of certainty, so that full confidence may be felt in the results deduced from my registers.

[The registers of the diurnal variation of the declination left by M. Arago consist of six large folio volumes, of from 300 to 400 pages each, in all 2076 pages, which, with the exception of about a hundred, are all written by the illustrious astronomer's own hand. It would obviously be impossible to reproduce this immense work here. Thanks to the conscientious and sagacious skill of M. Fédor Thoman, who has been so good as to make the minute and laborious calculations required for their reduction, we are enabled to give an exact *résumé* of this most admirable series of magnetic observations.

In forming the tables of the monthly means of the absolute values of the magnetic declination, and of its diurnal variation during thirteen years, we have followed the course traced by M. Arago himself in the discussion of the observations of his predecessors in the preceding chapters of this notice. The zeal and scientific devotion of M. Fédor Thoman, and his great habit of calculation, afford in themselves a guarantee of the exactness of the figures which we propose to give, and the greater part of which we have, moreover, verified.

The original registers of M. Arago, and also the books containing M. Thoman's calculations, are deposited in the library of the Institute, and can, if desired, be referred to to substantiate the accuracy and fidelity of the deductions.

Most commonly M. Arago made, on an average, eleven observations each day, beginning at seven in the morning and ending at eleven at night. Sometimes we find him observing from hour to hour until half an hour after midnight, and then rising so as to begin the same work again at four the next morning. Under some circumstances we find the observations succeed each other every five or even every three minutes, and there are then more than 150 of them in the same day. On Mondays, which are the days of meeting of the Academy, there is an interval from two to six in the afternoon.

The total number of observations is 52,599. We subjoin a table of the numbers in the different months and years, for the sake of showing the degree of certainty of the calculated means given subsequently. When M. Arago had fully assured himself of the hours of maxima and minima, he was enabled to concentrate the daily observations in the vicinity of those hours, and to cease distributing them at equal intervals through the course of the day:—

Years.	January.	February.	March.	April.	May.	June.
1820		414	469	467	587	548
1821	375	365	493	492	580	517
1822	464	436	513	477	463	427
1823	349	309	397	451	490	448
1824	287	283	367	354	367	442
1825	397	403	431	418	418	417
1826	302	355	444	418	418	396
1827	360	336	327	364	442	396
1828	353	393	409	417	489	426
1829	355	272	372	432	311	392
1830	383	285	473	448	369	352
1831	191	187	260	250	211	230
1835	180	167				
Totals	3996	4205	4955	4988	5244	4991

Years.	July.	August.	September.	October.	November.	December.	Totals of each Year.
1820	509	568	539	474	411	460	5446
1821	484	502	467	451	505	444	5675
1822	399	333	151	365	339	296	4663
1823	465	358	277	292	288	257	4381
1824	400	357	317	374	312	288	4148
1825	421	372	314	74	283	243	4191
1826	384	340	311	399	379	300	4446
1827	403	372	421	419	319	299	4458
1828	421	394	356	413	347	292	4710
1829	506	409	395	827	540	409	5220
1830	243	86	40	111	184	53	3127
1831							1329
1835				215	243		805
Totals	4635	4091	3588	4414	4150	3341	52599

It results from the examination of all the observations, that the horizontal needle makes each day two complete oscillations; and that the declination has, therefore, two *maxima* and two *minima* in each day in the following manner.

1°. Beginning from 11 h. P.M., the north end of the needle moves from west to east, reaches a *minimum* declination at 8¼ h. A.M., and then retrogrades towards the west to attain its *maximum* declination at 1¼ h. P.M.

2°. From 1¼ h. P.M. the needle moves again towards the east, reaches a second minimum between 8 h. and 9 h. P.M., and returns again towards the west to attain its second *maximum* at 11 h. P.M.

The greatest amplitude is that of the semi-oscillation which takes place between 8 h. A.M. and 1 h. P.M.

The march of the needle is not absolutely regular; it seems, on the contrary, to accomplish its change of direction by small oscillations, which are, generally speaking, scarcely sensible. In M. Arago's registers this is indicated in almost all the observations.

We have collected, in the following table, the values of the mean diurnal variation of each month for the thirteen years of observation. The numbers given have been obtained by taking the difference between the greatest and the least declination in each day, and dividing the sum of the differences in a month by the number of days in that month on which observations were made.

Table of the mean monthly Diurnal Variations of the Declination Needle.

Years.	January.	February.	March.	April.
1820		8′ 54″·88	12′ 6″·28	12′ 57″·92
1821	8′ 39″·07	7 26 ·67	11 21 ·38	12 20 ·02
1822	5 8 ·41	6 44 ·11	10 4 ·01	11 19 ·13
1823	5 34 ·04	4 43 ·16	9 42 ·68	11 53 ·57
1824	4 26 ·13	4 45 ·96	9 18 ·55	10 8 ·13
1825	5 26 ·75	8 13 ·45	11 23 ·43	12 54 ·36
1826	5 51 ·17	8 2 ·12	12 16 ·19	12 33 ·78
1827	6 11 ·46	8 14 ·29	11 55 ·43	16 7 ·05
1828	7 34 ·15	10 35 ·36	13 5 ·49	14 44 ·93
1829	11 27 ·92	11 19 ·79	11 58 ·79	14 14 ·99
1830	8 55 ·73	8 22 ·63	14 5 ·92	14 43 ·52
1831	11 49 ·06	8 55 ·82	9 15 ·28	16 13 ·70
1835	6 4 ·82	7 40 ·79		
Means	7 15 ·73	8 0 ·00	11 22 ·79	13 20 ·92

Years	May.	June.	July.	August.
1820	12′ 3″·66	11′ 2″·76	10′ 52″·84	11′ 14″·08
1821	10 39 ·47	10 33 ·39	10 29 ·28	10 39 ·56
1822	10 49 ·66	11 13 ·61	10 17 ·67	10 30 ·59
1823	10 16 ·17	9 51 ·57	10 12 ·25	9 54 ·47
1824	9 15 ·64	10 19 ·73	9 4 ·90	9 51 ·29
1825	11 8 ·09	11 6 ·50	12 26 ·20	12 32 ·84
1826	11 13 ·08	11 57 ·30	10 46 ·48	10 36 ·38
1827	13 7 ·27	12 34 ·24	11 58 ·14	13 7 ·92
1828	13 31 ·03	15 32 ·54	14 17 ·05	13 58 ·75
1829	12 49 ·86	17 19 ·09	14 11 ·53	13 50 ·39
1830	15 50 ·69	12 47 ·06	11 24 ·83	11 58 ·24
1831	13 59 ·84	13 8 ·39		
1835				
Means	12 3 ·70	12 17 ·18	11 27 ·38	11 39 ·50

Years.	September.	October.	November.	December.
1820	11′ 49″·34	8′ 32″·43	8′ 40″·85	6′ 43″·92
1821	9 20 ·80	7 31 ·07	6 9 ·78	3 59 ·19
1822	9 22 ·02	9 35 ·67	6 46 ·07	4 3 ·40
1823	9 14 ·34	7 57 ·64	5 20 ·39	3 31 ·69
1824	8 59 ·00	10 17 ·67	6 57 ·39	4 59 ·53
1825	10 36 ·19	9 25 ·38	6 0 ·33	4 47 ·64
1826	11 8 ·00	10 54 ·81	7 9 ·46	4 39 ·14
1827	12 36 ·39	13 13 ·07	8 54 ·60	7 43 ·05
1828	12 11 ·04	9 26 ·97	6 9 ·12	7 8 ·33
1829	14 59 ·05	16 45 ·25	15 34 ·04	10 17 ·39
1830	13 21 ·40	16 1 ·82	10 54 ·34	10 18 ·70
1831				
1835		12 28 ·35	17 16 ·65	
Means	11 14 ·32	11 0 ·84	8 49 ·42	6 12 ·00

It follows from this table that the mean *maximum* of diurnal variation takes place in April, and the *minimum* in December. But it may also be seen that this phenomenon is very variable for the same months in different years. We may conclude generally that the mean diurnal variation at Paris is between 3′ and 17′.

M. Arago's registers enable the absolute declination for each day to be computed with great exactness, for they contain direct determinations, some of which have been inserted in the "Annuaire du Bureau des Longitudes," and are reported in an earlier page of this volume (p. 324.); and, moreover, all disturbing causes are pointed out and estimated with extreme care.

By taking as the declination on each day the mean between the *maximum* and *minimum* declinations on that day, and as the mean declination of the month the mean of the declinations of each day in the month, we obtain the following table:—

Table of monthly absolute Declinations at Paris from 1820 *to* 1835.

Years.	January.	February.	March.	April.
1820		22° 24′ 48″·24	22° 24′ 22″·71	22° 23′ 56″·61
1821	22° 23′ 11″·52	23 7 ·79	23 0 ·20	23 14 ·70
1822	21 30 ·21	21 57 ·62	22 38 ·31	21 27 ·69
1823	20 31 ·56	19 23 ·09	20 58 ·40	19 41 ·15
1824	21 11 ·51	21 47 ·86	22 48 ·70	22 14 ·46
1825	20 31 ·93	21 1 ·40	21 49 ·11	20 7 ·05
1826	18 51 ·37	19 29 ·54	17 57 ·95	17 26 ·43
1827	16 0 ·28	15 40 ·17	16 37 ·51	15 21 ·27
1828	12 16 ·34	12 0 ·62	13 8 ·16	11 13 ·10
1829	10 19 ·78	10 23 ·89	10 4 ·34	8 56 ·24
1830	8 29 ·96	9 31 ·14	8 30 ·24	7 20 ·92
1831	3 41 ·47	6 11 ·79	3 51 ·57	5 53 ·27
1835	21 56 35 ·56	21 56 5 ·35		

Years.	May.	June.	July.	August.
1820	22° 23′ 28″·26	22° 21′ 15″·23	22° 20′ 55″·32	22° 21′ 42″·66
1821	22 8 ·09	20 56 ·44	21 18 ·42	21 49 ·01
1822	21 2 ·05	19 41 ·15	19 50 ·40	20 13 ·32
1823	19 35 ·43	19 14 ·10	19 17 ·75	19 19 ·62
1824	21 38 ·63	19 37 ·77	19 8 ·30	20 33 ·24
1825	19 55 ·07	19 22 ·44	15 54 ·74	19 8 ·87
1826	16 58 ·94	16 23 ·77	18 54 ·67	17 8 ·57
1827	14 19 ·34	13 21 ·17	12 42 ·81	13 24 ·25
1828	10 26 ·89	10 51 ·11	10 12 ·20	10 29 ·32
1829	7 53 ·47	7 24 ·20	7 1 ·37	7 56 ·84
1830	5 55 ·51	6 23 ·12	5 2 ·48	5 12 ·58
1831	5 21 ·28	3 28 ·46		
1835				

Years.	September.	October.	November.	December.
1820	22° 22′ 52″·25	22° 22′ 10″·62	22° 21′ 46″·02	22° 21′ 27″·37
1821	21 23 ·67	21 24 ·79	21 54 ·63	21 20 ·48
1822	20 58 ·40	20 44 ·18	20 22 ·96	21 5 ·43
1823	19 21 ·12	19 48 ·99	20 7 ·51	19 17 ·37
1824	20 18 ·75	20 39 ·69	20 6 ·12	19 41 ·14
1825	19 19 ·15	19 44 ·12	19 15 ·97	17 52 ·72
1826	17 5 ·85	16 19 ·74	16 9 ·64	15 53 ·08
1827	13 15 ·83	12 32 ·98	12 41 ·78	11 57 ·63
1828	10 53 ·27	10 23 ·99	10 48 ·50	9 57 ·05
1829	8 34 ·26	7 41 ·13	8 15 ·37	9 36 ·19
1830	5 16 ·70	5 3 ·41	5 40 ·74	6 59 ·87
1831				
1835		21 52 32 ·25	21 53 17 ·35	

On attentively examining the table, we perceive —

Two *maxima* about March and September, that is to say, about the equinoxes.

Two *minima* about June and December, that is to say, about the solstices.

The periods found by Cassini (p. 332.) still subsist. In spring and autumn the needle experiences a retrograde movement towards the west; at the solstices it again approaches the geographical meridian. The conclusions which might have been drawn from the observations of Bowditch, which were too few in number, are not confirmed.

It will be seen by the numbers in the above table, that the needle is now progressively approaching nearer and nearer to the geographical meridian; in other words, that the mean declination is diminishing every month. But it will also be seen that observations made during only a single year might mislead, since, for example, the observations of 1824 give results higher than those of 1823 and 1825, so that about this period there was a slight retrogression towards the west. We also see that detached observations cannot teach us anything respecting the real values of the march of the declination.

By taking the means of the twelve monthly declinations in each of the years contained in the above table we obtain the mean declination of the needle at Paris for each year, and can easily compute the annual decrease of the declination in relation to each preceding year. The results of these calculations are contained in the following table:—

Years.	Value of the Mean Declination in each Year.	Yearly Decrease of West Declination.
1820	22° 22′ 42″·30	
1821	22 22 4 ·14	0′ 38″·16
1822	22 20 57 ·64	1 6 ·50
1823	22 19 43 ·01	1 14 ·63
1824	22 20 48 ·85	+1 5 ·84
1825	22 19 45 ·21	1 3 ·64
1826	22 17 8 ·30	2 36 ·91
1827	22 13 59 ·58	3 8 ·72
1828	22 11 3 ·38	2 56 ·20
1829	22 8 40 ·59	2 22 ·79
1830	22 6 37 ·22	2 3 ·37

The mean annual decrease of west declination from 1820 to 1830 is 1′ 36″·51.

The retrograde march or return of the needle towards the geographical meridian was not, however, uniform; it even presented from 1821 to 1823 a marked irregularity, the needle moving in that interval slightly to the westward. More recently there appears a tendency to an accelerated rate of change. As M. Arago remarked in a preceding page (p. 324.), detached observations made once a year cannot be relied upon in judging of this phenomenon.*

* [The mean monthly variations of the declination, in the different months of the ten complete years from 1826 to 1830 inclusive which have been derived by MM. Barral and Thoman from the registers of M. Arago's observations, — and which are now for the first time given to the public, — supply a most valuable result in connection with the remarkable coincidence which, from observations in different parts of the globe between the years 1840 and 1850, has been inferred to subsist between the variation in amount and frequency in different years of the *solar spots*, and a corresponding variation in period and epoch in all those magnetic influences at the surface of the globe, which by their dependence on the hours of solar time lead us to recognise the sun as their primary cause.

In this category we may place together — the diurnal variation of the declination, which, although known to exist at the latter end of the last century, first became the subject of continued observation for several successive years in the hands of M. Arago — the diurnal variations of the inclination and of the intensity of the total magnetic force, which have been studied with appropriate instruments from 1840 to 1850 at the British magnetic observatories with the same care as that of the declination, — and, lastly, those *occasional* disturbances of the declination, inclination, and total force (or magnetic storms as they are sometimes called), which, however apparently irregular in the times of their occurrence, have been shown by the observations at the British observatories to be subject, in respect to frequency and amount of disturbance, to periodical laws depending upon the hours of solar time.

By comparing the amount of variation and disturbance in these several phenomena in the different years between 1840 and 1850 (which were those during which the British system of observation was in operation), it was found, that in all cases of magnetic variation or disturbance in which a dependence on solar hours was traceable, a minimum of variation or disturbance showed itself in the year 1843, and a maximum in 1848; the years antecedent to 1843, and those subsequent to 1848, being also in accordance with this apparent law of periodical increase and decrease; which, as the minimum and maximum are separated by about five years, would constitute a cycle or period of about ten of our years between maximum and maximum, or minimum and minimum.

Now this same period, of about ten of our years, has been shown by the observations of Schwabe of Dessau, commenced in 1826, and still continuing, to characterise an equally remarkable variation in the amount and frequency of the solar spots in different years. When the sun is viewed with a telescope his luminous disk is found to be scarcely ever free from dark or ashy-coloured spots of irregular and variable outline, appearing and disappearing at uncertain times, and having no fixed period of duration. The number of these spots, and the space they occupy on the solar disk, vary considerably

CHAP. XI.

ON THE INCLINATION.

A NON-MAGNETISED steel needle, supported by its centre of gravity, may remain in a horizontal position; but as soon as magnetism is imparted to it it inclines, or dips, very sensibly.

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at different times; and the variations are of a character which can by no means be regarded as accidental, since they follow with great regularity a law of periodical alternate increase and decrease, continuous and progressive from a minimum to a maximum in a period which corresponds nearly with five of our years, and from a maximum to a minimum in a period of similar duration. In the years of minimum the number of distinct groups appearing in the year is usually between 30 and 40; whilst in the years of maximum the number is between 300 and 400. The years of maxima, since the solar spots have been subjected to careful examination, have been 1828, 1837, and 1848; the years of minima, 1833 and 1843.

The full establishment of the existence of a direct connection of this nature, between affections of the sun's disk and the magnetic influence which the sun exercises at the surface of the earth, is a step in cosmical knowledge of such primary importance, that every contribution to it must be hailed with satisfaction; and it is difficult to imagine how it has happened that the evidence which M. Arago's observations furnish, of the existence of a corresponding variation in the magnitude of the diurnal variation of the declination in different years between 1821 and 1830, should have escaped the notice, or have failed to be pointed out by the gentlemen by whom at M. Arago's request his observations were examined for the purpose of making known their results. But since it is so, the deficiency may be here supplied.

The means of the twelve monthly-diurnal variations in the years which are complete in all the months are as follows:—

1821	9′ 05″·9	1826	9′ 45″·7
1822	8 49 ·7	1827	11 18 ·6
1823	8 09 ·4	1828	11 36 ·2
1824	8 12 ·0	1829	13 43 ·7
1825	9 40 ·1	1830	12 23 ·7

The minimum is in 1823—1824, and the maximum in 1829. The increase is progressive and continuous from the minimum to the maximum; and the years before 1823 and after 1829 show portions of a corresponding variation.

The universality, as respects different parts of the globe, of this periodical inequality in the solar magnetic variations, may be judged of by the following table of the amount of the mean diurnal variation, in the years between 1841 and 1851, at Munich, in Europe; Toronto, in North America; and Hobarton, in Van Diemen Island; stations which are as widely apart, and as diverse in circumstances, as can well be.

This phenomenon of the Dip or Inclination of the magnetic needle was observed for the first time by Robert Norman, in 1576. (*Phil. Trans. for* 1738, p. 310.)

In our hemisphere it is the north end of the needle which dips below the horizon; in the southern hemisphere it is the opposite end.

It will readily be conceived that between two such different positions or directions there are many intermediate ones; that is to say, that at the same moment the magnetic inclination is different in different places, and that there must be points where the inclination is null: the line containing these points is called the magnetic equator.

Years.	Munich.	Toronto.	Hobarton.
1841	7′·82	9′·50	8′·28
1842	7 ·08	8 ·67	7 ·75
1843	7 ·15	8 ·90	7 ·66
1844	6 ·61	8 ·87	7 ·84
1845	8 ·13	9 ·41	8 ·39
1846	8 ·81	9 ·27	9 ·06
1847	9 ·55	10 ·40	9 ·93
1848	11 ·15	12 ·11	10 ·63
1849	10 ·64	11 ·77	8 ·13
1850	10 ·44	10 ·88	8 ·57
1851		10 ·15	6 ·65

The results of the observations on the mean diurnal variation of the inclination and of the total force at Toronto and Hobarton (which are the only stations in which the deduction of results is as yet so far advanced), show the same periodical affection. And that the magnetic disturbances partake in the same is shown by the following table, containing the ratios of the aggregate values of disturbances of the declination exceeding a certain amount at Toronto, from the year 1841 to 1848.

Years.	Ratios.	Years.	Ratios.
1841	1·38	1845	0·63
1842	0·91	1846	1·27
1843	0·50	1847	1·42
1844	0·73	1848	1·45

It would be wrong to omit here the notice of an additional circumstance of a highly interesting nature; viz., that a properly directed examination of the observations at Toronto, Hobarton, and St. Helena show that the moon exercises a lunar-diurnal influence on the direction of the magnetic needle, producing, like the sun, but in a much less degree, two easterly maxima and two westerly maxima in every lunar day; but that in the terrestrial magnetic variation occasioned by the *moon* there is not the slightest trace in different years of the decennial period, which is so distinctly marked in *all* the variations which are connected with the *sun*.]—Ed.

The magnetic poles are the points where the inclination needle is vertical.

The name of lines of equal inclination, or isoclinal, is given to lines in passing along which on the earth's surface with an inclinometer, its needle would always be found to make the same angle with the horizon. But, inasmuch as the magnetic inclination at the same place varies with time, it is evident that the isoclinal lines spoken of are liable to displacement and perhaps to change of form. In order to be able to estimate the value which may belong to observations which cannot be absolutely simultaneous, it is necessary to study the modifications which the inclination undergoes at a particular place.

CHAP. XII.

YEARLY CHANGES OF THE INCLINATION.

The inclination of the magnetic needle at Paris diminishes from year to year. This result has been indicated by early observations, giving the following numbers:—

Years.	Inclinations.
1671	75°
1754	72 15′
1776	72 25
1780	71 48
1791	70 52

By careful determinations my illustrious friend Humboldt found in—

1798	69° 51′

Beginning from 1810 I made a great number of determinations of inclination with various instruments. I subjoin the results obtained, which are in each case the mean of four observations taken both before and after the reversal of the poles of the needle.

7th of October, 1810 (between noon and 2 h. P.M., weather cloudy).

Before inverting the poles	68° 47′ 4
After	68 53 ·1
Mean	68 50 ·2

Observers: MM. Humboldt and Arago. (Instrument by Lenoir.)

18th of September, 1813 (from 11 h. A. M. to 11 h. 30 m. A. M.).

Before inverting the poles - - -	68° 31′·5
After - - - - - -	68 39 ·8
Mean	68 35 ·7

Observer: M. Arago.

9th of February, 1817 (about 2 h. P. M.). (Lenoir's Instrument.)

Before inverting the poles - - -	68° 17′·8
After - - - - - -	68 44 ·2
Mean	68 31 ·0

Observers: MM. Arago and Freycinet.

14th of March, 1817 (about 2 h. P. M.).

Before inverting the poles - - -	68° 35′·6
After - - - - - -	68 40 ·1
Mean	68 37 ·8

Observers: MM. Arago and Freycinet.

16th of March, 1817 (about 2 h. P. M.)

Before inverting the poles - - -	68° 34′·3
After - - - - - -	68 31 ·0
Mean	68 32 ·6

Observers: MM. Arago and Freycinet.

26th of June, 1818 (from 1 h. to 3 h. P. M.; fine sky, with a few clouds.). Instrument by Gambey, belonging to Mr. Ritchie.

First Needle.

Before inverting the poles - - -	68° 22′·25
After - - - - - -	68 29 ·75
Mean	68° 26 ·0

Observer: M. Arago.

11th July, 1818 (from 11 h. A.M. to 2 h. 30 m. P.M.; sky cloudy, with a little wind). Mr. Ritchie's instrument by Gambey.

SECOND NEEDLE.

Before inverting the poles - - -	68° 43′·10
After - - - - - -	68 27 ·55
Mean	68 35 ·32

Observer: M. Arago.

11th of March, 1819 (from noon to 3 h. P.M.; sky cloudy). Instrument by Gambey, intended for the University of Cambridge, in the United States.

FIRST NEEDLE.

Before inverting the poles - - -	68° 20′·8
After - - - - - -	68 13 ·5
Mean	68 17 ·2

Observer: M. Arago.

11th of March, 1819.

SECOND NEEDLE.

Before inverting the poles - - -	68° 10′·6
After - - - - - -	68 39 ·4
Mean	68 25 ·0

Observer: M. Arago.

28th of April, 1822. Instrument by Lenoir, the same which was taken by M. Duperrey on his voyage of circumnavigation.

NEEDLE No. 2.

Before inverting the poles - - -	68° 40′
After - - - - - -	68 5
Mean	68 22 ·5

By observations in two azimuths 90° apart, with the same needle 68° 16′.

Observers: MM. Arago and Duperrey.

15th of June, 1822 (from 8 h. to 8 h. 30 m. P.M.). Instrument by Gambey, intended for the University of Abo, in Finland.

NEEDLE No. 1.

Before inverting the poles - -	67° 50′·60
After - - - - - -	68 27 ·65
Mean	68 9 ·1

18th of June, 1822. Same instrument as on the 15th.

NEEDLE No. 2.

Before inverting the poles - - -	68° 15′·6
After - - - - - -	68 8 ·9
Mean	68 12 ·25

With the same needle, in two azimuths 90° apart, 68° 12′·10.
Observer: M. Arago.

11th of November, 1823 (about 2 h. P.M.). Instrument belonging to the Observatory; needle by Gambey.

Before inverting the poles - - -	68° 20′·05
After - - - - - -	67 57 ·10
Mean	68 8 ·6

Observer: M. Arago.

19th of August, 1825 (about 2 h. P.M.; weather cloudy). Observatory instrument; Gambey's needle, marked A.

Before inverting the poles - - -	68° 11′·5
After - - - - - -	67 50 ·5
Mean	68 1 ·0

The observations made in two azimuths 90° apart gave 67° 50′·30.

Observer: M. Arago.

19th of June, 1829 (between 3 h. and 5 h.; sky cloudy). Instrument by Gambey, intended for Freyberg.

NEEDLE No. 1.

Before reversing the poles - - -	67° 45′·9
After - - - - - -	67 44 ·9
Mean	67 45 ·4

Observers: MM. Arago and Reich.

19th of June, 1829 (between $4\frac{1}{4}$ h. and 5 h.).

NEEDLE No. 2.

Before reversing the poles - - -	67°	36′·0
After - - - - - -	67	40 ·8
Mean	67	38 ·4

Observers: MM. Arago and Reich.

21st of June, 1829 (between noon and 2 h. 30 m.). (Same instrument and needle No. 2.)

Before reversing the poles - - -	67°	43′·2
After - - - - - -	67	28 ·9
Mean	67	36 ·0

Observations made in two azimuths 90° apart, 67° 36′·8.
Observer: M. Arago.

22nd of June, 1829 (between 4 h. and 5 h. P.M.; rain and thunder).

FIRST NEEDLE for Freyberg.

Before inverting the poles - - -	67°	44′·5
After - - - - - -	67	40 ·6
Mean	67	42 ·5

24th of June, 1829 (between 11 h. 45 m. A.M. and 1 h. 45 m. P.M.; sky cloudy).

FIRST NEEDLE for Freyburg.

Before reversing the poles - - -	67°	48′·0
After - - - - - -	67	43 ·2
Mean	67	45 ·6

By observations made in two azimuths 90° apart, 67° 44′·7.
Observer: M. Arago.

It is remarkable that the two needles gave results differing 7′ from each other, this difference being nearly the same when the inclination is deduced from two rectangular azimuths. What may be the cause of such an anomaly?

14th of May, 1831 (from 2 h. 30 m. to 3 h. 30 m. P.M.; weather fine). (Instrument made by Gambey for Mr. Encke).

FIRST NEEDLE.

Before reversing the poles - - -	67° 42′·0
After - - - - - - -	67 42 ·7
Mean	67 42 ·3

14th of May, 1831 (from 4 h. to 5 h. P.M.; clear sky.

SECOND NEEDLE.

Before reversing the poles - - -	67° 46′·4
After - - - - - - -	67 41 ·2
Mean	67 43 ·8

Observer: M. Arago.

12th of November, 1831 (between 10 h. and 11 h. A.M.; sky cloudy).

NEEDLE No. 2. intended for Mr. Rudberg.

Before reversing the poles - - -	67° 40′·9
After - - - - - - -	67 36 ·3
Mean	67 38 ·6

Observers: MM. Arago and Rudberg.

12th of November, 1831 (from 2 h. to 3 h. 30 m. P.M.; sky cloudy).

NEEDLE No. 1. by Gambey, intended for Mr. Rudberg at Stockholm.

Before the reversal of the poles - - -	67° 43′·5
After - - - - - - -	67 40 ·7
Mean	67 42 ·1

Observers: MM. Arago and Rudberg.

The above observations would only admit of comparison strictly and with mathematical exactness, if they had all been made at the same season of the year and the same hour of the day; for the inclination, like the declination, is subject to an annual change, and even to a diurnal variation, as I ascertained as early as 1827 by direct observation with microscopes viewing the two extremities of the dipping needle. These observations will be discussed in a separate chapter, and it will be seen that the general phenomenon of the yearly diminution of the inclination is not masked by the diurnal and season variations which are shown to exist. I transcribe determinations recorded in the *Annuaire du Bureau des Longitudes,* which have been made here since I have ceased to follow up these investigations myself.

Date of Observation.	Inclination.
July 3, 1835, 9 a.m. - - -	67° 24′
January 6, 1849, 2 p.m. - -	66 45
December 1, 1849, 3 p.m. - -	66 44
November 28, 1850, 2 p. m. - -	66 37
November 20, 1851, 2. 30′ p.m. -	66 35

Observations made in London have also shown the same phenomenon of a decrease of inclination. In the "Philosophical Transactions for 1806, p. 395.," there is the following table given by Gilpin:—

1786 - -	72° 5′	1797 - -	70° 59′
1787 - -	72 5	1798 - -	70 55
1788 - -	72 4	1799 - -	70 52
1789 - -	71 55	1801 - -	70 36
1790 - -	71 54	1803 - -	70 32
1791 - -	71 24	1805 - -	70 21
1795 - -	71 24		

We cannot venture to predict from a knowledge of the present rate of yearly decrease what will be the future march of the inclination.*

* [To the observations recorded by Gilpin, as having been made in London from 1786 to 1805, may be added three subsequent determinations at epochs considerably distant from each other, made with great care for the purpose of ascertaining the rate of annual decrease of the magnetic inclination during the period comprised by the observations. The first of these, in August, 1821, was made by the editor of this translation in a part of the Regent's Park then used as a nursery garden, and now included in the Botanic Garden. To obviate what was known to be a frequent cause of error in the dipping needles of that period, arising from imperfections of workmanship in the axle, a needle was employed made on a principle suggested by Professor Tobias Mayer, in a memoir published in 1814 in the Transactions of the Royal Society of Göttingen, according to which the centres of motion and of gravity of the needle were designedly separated by means of a weight applied in the perpendicular to the axis of motion; the needle was thus made to rest on different parts of the axle in the different positions in which the circle is placed in the course of the observation, whilst an additional force was derived from the weight of the needle in aid of the magnetic force, in overcoming the influence of other imperfections of workmanship either of the axle or of the supporting planes. By varying the weight and the azimuth in which the observations are made, and by reversing the poles, almost every part of the axle may be made in turn the

CHAP. XIII.

VARIATION OF THE MAGNETIC INCLINATION IN DIFFERENT PARTS OF THE EARTH.

THE inclination varies rapidly in changing the latitude. We have just seen that at Paris the angle with the horizon is about

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point of support, causing errors from irregular curvature, if any exist, to disappear in the mean result. The perfection to which the manufacture of inclinometers has been carried in England in the last twenty years, originally by Mr. Robinson, and subsequently by his successor, Mr. Henry Barrow, has nearly superseded the use of needles on Professor Mayer's construction; but in 1821 English dipping needles of the ordinary construction were comparatively inferior. The observations with Mayer's needle in the Regent's Park in 1821 were repeated on several days between the 3rd and the 10th of August, in order that they might afford a nearer approximation to the true mean dip of the period. The mean of 10 partial results, the particulars of which are recorded in the Phil. Trans. of the year 1822, Art. I. (the Bakerian Lecture), of which the least was 70° 00'·1 and the greatest 70° 05'·9 was 70° 02' 91.

The next determination (in the order of time) of the inclination in London was one which is entitled, from the number of well-practised observers, the excellence of the instruments, and the number of repetitions, to a more than ordinary degree of reliance. It is the determination made in 1837 and 1838, in the course of the Magnetic Survey of Great Britain, and is recorded in the eighth volume of the Reports of the British Association for the Advancement of Science (1839). The observers were Messrs. Robert Were Fox and John Phillips, Captains Sir James Clark Ross and Edward Johnson of the Royal Navy, and the Editor. The instruments were those of Robinson, Gambey, and Jordan. The general mean corresponding to May, 1838, was 69° 17'·3.

With these earlier determinations we have now to compare the result of observations made by the editor, and Mr. John Welsh of the Kew Observatory of the British Association, in August, 1854, in the same month, and at the same spot, in the Botanic Garden in the Regent's Park, in which the observations in 1821—33 years previously—had been made. The purpose on this occasion was twofold; first, to obtain a result for August, 1854, which might be as strictly comparable as possible with that of August, 1821; and second, to ascertain the difference, if any, between the dip in the Regent's Park and at the Kew Observatory, so that the observations which may be henceforward made at Kew may be brought into strict comparison with those made in the Regent's Park in 1821; whereby it may be hoped that the record of the secular change of the inclination, which has now been carried on from 1821 to 1854, may be permanently continued. The observations in 1854 were made with two of Barrow's inclinometers fitted with verniers and microscopes according to the modern English

$66\frac{1}{2}°$; in 15 N. lat. it is only 50°, and at places near the equator the needle is horizontal.

In 1774, in 79° 44′ lat., Captain Phipps found an inclination of 82° 9′. In more recent times, in 1830, Captain James Ross observed at a point where the needle is absolutely vertical. This northern magnetic pole was then in 70° 5′ 17″ N. lat. and 96° 43′ W. long. from Greenwich. The south magnetic pole has not yet been actually reached.

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mode. The instrument distinguished here as the Kew circle is the same as that with which a monthly series will hereafter be made in the magnetic house attached to the Kew Observatory. The other inclinometer was made for Professor Hansteen, of the Observatory at Christiania in Norway. Each instrument is furnished with two needles.

The following are the results obtained:—

Date.	No. of Obs.	Inclinometer.		Inclination.	Locality.
1854 Aug. 11	4	Hansteen	(both needles)	68° 31′·73	Regent's Park.
„ Aug. 12	4	Kew	(both needles)	68 29·36	Regent's Park.
„ Aug. 16	2	Kew	(both needles)	68 29·25	Kew Observatory.
„ Sept. 6	4	Hansteen	(both needles)	68 31·99	Kew Observatory.
„ Sept. 18	2	Kew	(both needles)	68 33·73	Kew Observatory.
„ Sept. 18	2	Hansteen	(both needles)	68 31·04	Kew Observatory.

Mean,—allowing weight for the number of observations,—68° 31′·13. Mean in the Regent's Park, 68° 30′·55; at the Kew Observatory, 68° 31′·6. The difference between the mean result at the two stations being less than the probable error of observation, the mean of the 18 determinations is taken as the inclination in London corresponding to the beginning of September, 1854.

The observations on August 11, 12, and 16 were made by the editor and by Mr. Welsh conjointly. Those on September 6 and 18 by Mr. Welsh alone. The hours were from 11 a.m. to 4 p.m.

For the comparison of the epochs we have, then,—

August, 1821 - - - - - -	70° 02′·91
May, 1838 - - - - - - -	69 17·30
August and September, 1854 - - -	68 31·13

	Years.	Decrease.	Annual Decrease.
Between the 1st and 2nd epochs -	16·75	45′·61	2′·72
Between the 2nd and 3rd epochs -	16·33	46·17	2·83
Between the 1st and 3rd epochs -	33·08	91·78	2·77

Whence we may conclude that the magnetic inclination in London has been diminishing, in the period between 1821 and 1854, at a nearly uniform annual rate of 2′·77; and by comparison with the earlier observations, noticed by M. Arago as having been recorded by Gilpin, it further appears that the average annual rate in the last 33 years has been less than it was in the preceding 33 years.]—Ed.

In a journey to Italy in 1825, I made a few observations of inclination, which I place here:—

Geneva, September 2 - - -	65°	58′	·2
Venice, September 19 - - -	63	55	·4
Florence, September 26 - -	62	58	·6
Florence, September 30 - -	63	9	·5
Turin, October 10 - - -	64	53	·0
Lyons, October 20 - - -	65	39	·2

CHAP. XIV.

CHANGE OF PLACE OF THE MAGNETIC EQUATOR.

THE line of no inclination, or magnetic equator, intersects the terrestrial equator at an acute angle, so that one of its portions is in the northern, and the other in the southern hemisphere.

The points of intersection are called nodes. M. Kupffer, in a memoir on Terrestrial Magnetism, published in 1827, in the "Annales de Chimie et de Physique," 2e série, t. xxv. p. 231. expressed himself in the following manner:—

"It is *proved* by the most recent observations, and principally by the discussion of all the inclinations observed in the voyage of circumnavigation of the 'Coquille,' published by M. Arago in December, 1825, that the magnetic equator moves from east to west."

It was in seeking to conciliate observations of the inclination, made at different and distant periods, at places not far removed from the magnetic equator, that I was led to recognise the progressive advance of the whole of that equator from east to west. The discussion which I went into on this subject will be found in my report of the voyage of the "Coquille," under the command of M. Duperrey, in 1822—1825.*

It is now thought that the movement of the magnetic equator is accompanied by a change of form. The study of other iso-

* [The position, in 1822, of one of the nodes, or points of intersection of the geographical and magnetical equators, was determined by the Editor, by direct observation in May, 1822, to be in the island of St. Thomas on the west coast of Africa, in 6° 45′ east longitude. This determination was probably unknown to M. Arago when he discussed the observations of the "Coquette" in 1825, and to M. Kupffer, when he published his memoir on terrestrial magnetism in 1827; since it was not referred to by either of the gentlemen, whilst it corroborates the deduction they announced of the eastern movement of the node in question.]—ED.

clinal lines, looked at in this view, will offer no less interest. It will be highly curious, when all these lines shall have been laid down on maps, to follow them by the eye in their displacements and their changes of curvature; and important truths may spring to light from such investigations. It will now be understood why we ask for as many observations of inclination as voyagers can furnish.*

It has been frequently agitated whether, as a general question, the inclination needle would indicate at any given place exactly the same dip at the surface of the earth,—at a great height above it, in the air,—and at a great depth below it, in a mine. The absence of uniformity in the character of the soil renders the solution of this question very difficult. Observations made in a balloon are not sufficiently exact. On a mountain the observer is exposed to local attractions which may notably alter the inclinations of the needle without his being aware of it. The same cause of uncertainty attaches to observations in mines. It is not indeed absolutely impossible to determine, for any particular place, what is due to accidental circumstances; but to do so requires instruments of great precision,—the power of going to a distance from the selected place in all directions,—and observations much more often repeated than are ordinarily within the power of a traveller. Observations of this kind are, however, worthy of attention, and may perhaps lead at a future time to some general result.

CHAP. XV.

ON THE INTENSITY OF THE MAGNETIC FORCE.

In all the phenomena of which we have been speaking, the terrestrial globe performs, relatively to the needles, the office

* [Mr. Hansteen's Treatise, "Magnetismus der Erde," published in 1819, contains maps of the isoclinal lines in 1600, 1700, and 1780. For that of 1600, the authorities, though few, are shown to be entitled to much confidence; those for the map of 1700 are much more numerous; but that of 1780 is the first tolerably complete system of the isoclinal lines warranted by observations. These maps will be found of great service to those who may desire to study the movements of translation and the changes of form which these lines have undergone and are undergoing.]—Ed.

of a real magnet. But does the magnetic property of the earth preserve the same degree of intensity at all parts of the globe? Is it probable, as some persons have thought, that, in the same geographical position, it experiences a sensible diminution in ascending into the atmosphere? These important questions present themselves immediately to the mind, but researches upon them belong only to recent times.

We have said above, that when a magnetic needle is freely suspended, it always places itself in a plane called the magnetic meridian; and that when left to itself, after having been diverted from that plane, it reverts to it, making, in so doing, vibrations of greater or less extent on either side of it. The effect of the magnetic force, in producing these vibrations, is analogous to that which gravity exercises on a pendulum in motion; the vibrations will be so much the more rapid as the intensity of the magnetic force is more considerable; and we can take, as the measure of this intensity, the square of the number of vibrations performed by the needle in a given time. Therefore the proportion between the intensities of the magnetic force at any two given places will be equal to the proportion of the squares of the number of vibrations performed by one and the same needle, in the same interval of time, at those two places.

Graham appears to have been the first person who directed his attention to the intensity of the force of terrestrial magnetism; Musschenbroeck made some efforts to solve the question, and Lemonnier showed its importance. But regular observations of intensity date only from the voyages of D'Entrecasteaux and Humboldt, and already observations of this nature have shed a bright light on the so highly complicated, and at the same time so highly interesting, question of terrestrial magnetism. This kind of observation deserves in the highest degree the attention of all voyagers and travellers attached to science, for at the present time the theorist finds himself arrested at each step by the want of exact determinations.

CHAP. XVI.

ON A METHOD OF MEASURING THE VARIATIONS OF TERRESTRIAL MAGNETISM AT DIFFERENT POINTS ON THE GLOBE.

As we have just said, in order to learn the magnetic force of the globe at a given place, we make a horizontal needle vibrate, and we count the number of its vibrations in a given time. But in comparisons of this kind, it is necessary that the needle should have preserved its charge of magnetism unaltered during whatever interval of time may have elapsed between the observations to be compared. At the meeting of the Bureau des Longitudes of the 16th of November, 1825, I proposed a method by which the observer might assure himself of this invariability, by comparing the magnetism of the needle with gravity.

The proceeding which I imagined is grounded on the property which a magnetic needle possesses when placed near a rotating metallic plate, of being carried round by it with a force proportioned to the intensity of its magnetism. By making the experiment in a plane perpendicular to the direction of the dipping-needle, we become independent of the action of the earth's magnetism. Small weights or counterpoises attached to each of the poles of the needles, producing with a given velocity of the plate deviations of 10°, 20°, 30°, &c., will give the measure of the magnetic force of the poles. If we could be satisfied that science possesses the means of reproducing at will iron endowed with exactly the same properties, we might substitute the angular deviation produced by a certain mass of metal for the deviation occasioned by the rotation of the plate. However this may be, a needle preliminarily tested by this latter proceeding will, as is seen, become an excellent means of estimating the periodical and secular changes of intensity to which the magnetic force of our globe may be subject.

[The object which M. Arago had in view in the suggestion at the close of this Chapter, viz., that of obtaining a measure of the force of the earth's magnetism, which should be independent of the magnetic moment of the needle employed in the process, and of expressing it in terms which should enable a strict comparison to be made, in future times, between the force which now prevails at any given spot, and that which may hereafter prevail at the same spot at any subsequent period of the world's history, has been realised by a method of which the first practical exposition was made in a memoir of Gauss's, entitled "Intensitas Vis Magneticæ Terrestris ad Mensuram absolutam revocata. Göttingen, 1833." This method, with many

CHAP. XVII.

ON THE VARIATIONS OF INTENSITY OF THE MAGNETIC FORCE WITH ELEVATION.

THE aerostatic ascents of Biot and Gay-Lussac, formerly executed under the auspices of the Academy, were designed in great part for the examination of this capital question — Has the magnetic force which, at the surface of the earth, acts on a magnetic needle, exactly the same degree of intensity whatever may be the elevation to which the needle is carried?

The observations of two members of our Academy, made in mountainous countries — those of Humboldt, and the still older ones of De Saussure — appeared to show that at the greatest elevations attainable by man the diminution of magnetic force is still inappreciable.

This conclusion* has been recently controverted. It has been remarked that in Gay Lussac's balloon ascent the thermometer which, on the ground, immediately previous to the ascent, marked $87\frac{1}{2}°$, had sunk to 16° in the aerial region in which the needle was made to vibrate a second time; and it is now perfectly well established, that at the same spot, under the action of the same force, the same needle will vibrate more rapidly as the temperature is lower. Thus, for example, M. Kupffer found that a cast steel cylindrical needle, $2\frac{1}{4}$ inches in length, performed 300 vibrations

			Fahr.
In 13′	11″·5	at a temperature of (in round numbers) -	29°
In 13	17 ·5	„ „ „ -	54
In 13	25 ·0	„ „ „ -	90

instrumental modifications to improve its practical applicability, has been adopted and employed for the last fifteen years in magnetic observatories and surveys, and is known usually under the name of the determination of the earth's *absolute* horizontal magnetic force. A description of a convenient form of apparatus for this purpose, to be employed in surveys or in observatories, may be found in the "Manual of Scientific Enquiry," printed by the Lords Commissioners of the Admiralty, for the use of officers in Her Majesty's navy, and travellers in general (2nd edition, 1851. Art. Terrestrial Magnetism. App. No. 1. Unifilar Magnetometer.)] — ED.

* [A paragraph seems to be wanting to state expressly, what is however implied, viz., that the balloon experiments were at first supposed to corroborate the inference, that elevation made no sensible difference in the magnetic force.]—ED.

Therefore, in order to render truly comparable the observations made at the surface of the earth and in the balloon, it would have been necessary to apply to the latter a certain correction corresponding to the difference of temperature, to be subtracted from the magnetic force which they appeared to indicate. Since then, in this ascent, the force attracting the needle on the ground and in the higher regions of the atmosphere appeared to be equal, the true inference is that there was a real diminution of force at the higher point.

A diminution of magnetic force with increasing height seems also to result from M. Kupffer's observations, made in 1829, on ascending Mount Elbrouz, in the Caucasus. In these experiments, the effect of differences of temperature was carefully allowed for: various irregularities in the march of the inclination throw, however, some doubts on the result.

Experiments, having for their object the determination of this question, are therefore still much wanted.

CHAP. XVIII.

ON THE RELATIONS BETWEEN THE INCLINATION AND THE INTENSITY OF THE EARTH'S MAGNETIC FORCE.

MEASURED with the same needle, at the same place, the magnetic intensity is found to change with time. Do these changes depend simply on the variation in the *direction* of the force? As the comparison is made by means of the times of vibration of a *horizontal* needle, it is evident that the greater the inclination in the direction of the earth's force the smaller will be the horizontal component. It follows that there must be at any given place a certain dependence between the variations of magnetic intensity so measured, and the variations of the inclination; but it remains to inquire whether the absolute intensity of the magnetic force does not vary independently of any modification in its direction.

Gilpin, in the Phil. Trans. for 1806, said that the inclination-needle in London does not undergo any sensible diurnal variation.

Hansteen, of Christiania, on the contrary, in the summer of

1820, found, with a very good dipping needle by Dolland, the inclination four or five minutes greater in the morning than in the afternoon: he also thought that the inclination was subject, at Christiania, to an annual variation, making it fifteen minutes greater in summer than in winter.

He had found by the vibrations of a horizontal needle the following results:—

1. That the intensity of the horizontal magnetic force is subject to a diurnal variation.

2. That the minimum of this intensity occurs between ten and eleven in the morning, and the maximum between four and five in the afternoon.

3. A variation in the mean monthly intensities.

4. That the mean intensity about the time of the winter solstice was much greater than that found near the summer solstice.

But in regard to this last result, the author added, that on taking into account the corrections due to the variation of the inclination, the variation in the intensity was found to be merely apparent.

It is to be remarked that in the above results M. Hansteen did not make any allowance for the influence of temperature on the time of vibration of the horizontal needle employed. (*Annales de Physique et de Chimie*, 2e série, t. xvii. p. 126. 1822.)

In the Phil. Trans. for 1823 there is a memoir by Mr. Barlow on diurnal variation. He succeeded, as mentioned in p. 345., in magnifying the amplitude of the diurnal variation by the use of fixed magnets. He tried to apply the same method to the diurnal variations of the inclination, but did not obtain any numerical determination of those variations.

In February, 1825, during Parry's third Arctic voyage, Lieutenant Foster tried to make direct measurement of the variations of the inclination at Port Bowen; he did not succeed, however, because they were too small.

Foster measured the time of vibration of a dipping needle, and having also observed the variations of the horizontal force by means of vibrations, he inferred from his observations that the alterations of the horizontal component proceeded in great part from changes in the inclination.

Kupffer, in 1827, sought to explain the variations of force measured with a horizontal needle by variations in the inclination, but without adducing any experimental proof.

I will now cite some extracts from the reports, or *procès verbaux,* of the sittings of the Bureau des Longitudes, which will show distinctly the course followed by myself for some years with the view of elucidating this question.

Under date 23rd May, 1827:—

"M. Arago announced that the observations of inclination which he has been making for some months, indicate a diurnal variation. The inclination of the needle is from $1\frac{1}{2}$ to 2 minutes greater in the morning than in the evening. The intensity measured by a horizontal needle is greater in the evening than in the morning. It would, therefore, be possible that this variation in the intensity might depend solely on that of the inclination."

19th September, 1827:—

"M. Arago gave an account of simultaneous observations of intensity and inclination made by him since the beginning of the year. The inclination diminishes at the hours when the intensity measured by a horizontal needle increases; but the change is not sufficient to explain the whole of the variation in the intensity."

On the 19th of November, 1828, I was more explicit, as will be seen by the following extract from the *procès verbal:*—

"M. Arago gave fresh details respecting the observations of inclination and intensity which he has made for some years past. The diurnal variation of the inclination is not sufficient to explain the changes of intensity deduced from the observations with the horizontal needle. It may, therefore, be inferred that the absolute magnetic force of the earth varies at the same place at different hours of the day."

CHAP. XIX.

VARIATION OF THE MAGNETIC FORCE AT PARIS.

The preceding authentic deductions permit me to collect here, without discussion, the results which I obtained for the measure of the intensity of the magnetic force.

The observations were made in the morning between eight and nine, and in the evening between six and seven o'clock.

Mean Time of 300 *Vibrations—Morning.*

	1825. Min. Sec.	1826. Min. Sec.	1827. Min. Sec.	1828. Min. Sec.	1829. Min. Sec.
January	- -	- -	11 49·87	11 49·15	11 48·04
February	11 50·98	- -	11 49·38	11 49·23	
March	11 51·17	- -	11 49·72	11 49·35	11 48·37
April	11 51·62	- -	11 50·33	11 49·52	11 48·84
May	- -	- -	11 50·08	11 49·53	
June	11 51·77	- -	11 49·87	11 49·53	
July	- -	- -	11 50·07	11 51·35	11 48·73
August	11 51·63	11 51·72	11 51·01	11 51·57	
September	- -	- -	11 50·35		
October	- -	11 50·80	11 49·94		
November	- -	- -	11 49·48		
December	- -	- -	11 49·07	11 48·87	
Means	11 51·43	11 51·26	11 49·93	11 49·79	11 48·49

Mean Time of 300 *Vibrations — Evening.*

	1825. Min. Sec.	1826. Min. Sec.	1827. Min. Sec.	1828. Min. Sec.	1829. Min. Sec.
January	- -	- -	11 49·86	11 48·78	11 48·08
February	11 50·75	- -	11 49·42	11 48·96	
March	11 51·10	- -	11 49·58	11 48·77	11 47·91
April	11 50·97	- -	11 49·53	11 48·77	11 47·41
May	- -	- -	11 49·36	11 48·69	
June	11 50·99	- -	11 49·29	11 48·72	
July	- -	- -	11 49·45	11 50·64	11 47·95
August	- -	- -	11 49·47	11 50·66	
September	- -	- -	11 49·57		
October	- -	- -	11 49·57		
November	- -	- -	11 49·12		
December	- -	- -	11 48·70	11 48·54	
Means	11 50·95		11 49·41	11 49·17	11 47·84

Thus we see that the horizontal component of the intensity of the magnetic force at Paris is less in the morning than in the evening; and we also see that it augments from one year to the next.

In the years 1827, 1828, and 1829, at the same hours at which I observed the time of 300 vibrations, I also observed the inclination of the magnetic needle and the temperature of the room in which the experiments were made. It is desirable to place these latter data by the side of the numbers which precede them.

Mean of the Inclinations observed during the Determinations of the Horizontal Force in the Morning.

	1827.	1828.	1829.
January - - -	- -	68° 24′·77	68° 19′·66
February - -	68° 29′·73	68° 24 ·65	
March - - -	68 29 ·93	68 23 ·20	68 19 ·28
April - - -	68 35 ·14	68 24 ·39	68 19 ·60
May - - -	68 37 ·29	68 24 ·44	
June - - -	68 35 ·43	68 27 ·50	
July - - -	68 38 ·78	69 13 ·18	68 39 ·80
August - - -	68 55 ·61	69 7 ·20	
September - -	68 43 ·51		
October - -	68 33 ·57		
November - -	68 31 ·00		
December - -	68 30 ·22	68 20 ·47	
Means - -	68 36 ·38	68 34 ·42	68 24 ·58

Mean of the Inclinations observed during the Determinations of the Horizontal Force in the Evening.

	1827.	1828.	1829.
January - - -	- -	68° 23′·91	68° 20′·34
February - -	68° 24′·00	68 24 ·48	
March - - -	68 30 ·50	68 22 ·40	68 18 ·49
April - - -	68 33 ·00	68 22 ·90	68 17 ·50
May - - -	68 35 ·80	68 23 ·22	
June - - -	68 33 ·75	68 26 ·38	
July - - -	68 35 ·27	69 6 ·10	68 30 ·34
August - - -	68 52 ·44	69 6 ·13	
September - -	68 42 ·68		
October - -	68 32 ·95		
November - -	68 31 ·41		
December - -	68 29 ·93	68 18 ·80	
Means - -	68 34 ·70	68 32 ·70	68 21 ·67

I do not give these results with the view of discussing the variations of the inclination, but only for the sake of placing them by the side of the preceding determinations of the variations of the force. I shall give in a separate chapter observations of the diurnal variation of the inclination which I made with great care. The figures in the above table are direct determinations of absolute inclination, made solely on days and at hours when I was determining the intensity of the magnetic force,—viz. on 105 days of observation in 1827, 52 in 1828, and 30 in 1829. The following temperatures were those of the place in which my observations were made on those days and at those hours.*

	Morning.			Evening.		
	1827.	1828.	1829.	1827.	1828.	1829.
January -	3°·9	8°·6	3°·5	3°·7	8°·6	4°·0
February -	1 ·1	7 ·7	-	1 ·4	8 ·1	
March - -	9 ·0	9 ·3	6 ·8	9 ·5	10 ·7	7 ·8
April - -	13 ·4	13 ·0	11 ·8	14 ·3	13 ·9	11 ·7
May - -	17 ·1	17 ·6	-	18 ·3	18 ·3	
June - -	21 ·6	22 ·5	-	22 ·2	23 ·0	
July - -	24 ·9	23 ·6	22 ·6	25 ·9	23 ·9	23 ·2
August -	23 ·4	23 ·1	-	23 ·7	23 ·5	
September -	21 ·2	-	-	21 ·8		
October -	17 ·7	-	-	18 ·3		
November -	12 ·1	-	-	14 ·5		
December -	8 ·9	7 ·9	-	9 ·5	8 ·3	

It will be seen that as the temperature of the instruments was always a little higher in the evening than in the morning, the *increase* of force in the evening cannot be ascribed to that cause, since (as we have seen above from M. Kupffer, p. 371.) the intensity of the magnetic force in a needle *diminishes* when its temperature increases.

The influence of the inclination remains to be considered. We have just seen that the inclination is about 2′ less in the evening than in the morning; this corresponds, indeed, to an increase in the horizontal component of the intensity, but to an increase very far below that which is indicated by the observations. We see, moreover, that the force increased from 1827 to 1828, and from 1828 to 1829, while the inclination was greater in 1828 than either in 1827 or in 1829. I was, therefore, enabled to state with confidence to the Bureau des Longitudes, at their meeting on the 18th of February, 1829, that the absolute intensity of the earth's magnetic force underwent at a given place both diurnal variations and changes from one year to another.

* [As the *relative* temperatures are in this case the only object, it appeared useless to convert them into Fahrenheit's scale; and they are, therefore, left in the centesimal scale in which they are given in the original.]—Ed.

CHAP. XX.

ON THE INTENSITY OF THE EARTH'S MAGNETIC FORCE DURING SOLAR ECLIPSES.

M. Lion, Professor of Physics at Beaune, communicated to the Academy, at its meeting of the 4th of August, 1851, a note relative to the eclipse of the sun on the 28th of July. In this note M. Lion announced that during that eclipse, which, as every one knows, was only partial in France, a horizontal magnetic needle had indicated a considerable change in the magnetic force. No commission was appointed at the time, but M. Lion himself, with the view of removing the doubts which had arisen on his first communication, sent an additional explanatory note on the 11th of August.

Lastly, M. Lion wrote to the Academy a letter, which was inserted entire in the "Compte rendu" of the meeting of the 9th of February, 1852, in which he stated that, according to observations which he had made, the horizontal needle indicates a variation of magnetic force at the time of a solar eclipse, even at places where the eclipse is not visible. He prayed the Academy to appoint a commission to verify his supposed discovery, proposing particularly that this should be done for the invisible eclipse of the 17th of June, 1852. The Academy consented, and charged a commission, consisting of four members, to make the desired verification.

I was one of the commissioners, and undertook the functions of reporter. I requested my assistants, MM. Laugier, Mauvais, Goujon, and Charles Mathieu, to make in my presence the observations of which I subjoin the results.

16th of June, 1852, Paris mean time:—

Time of Observation.		Time of 300 Vibrations of a horizontal Needle.		Temperature (Centesimal).
h.	*m.*	*m.*	*s.*	°
8	33	11	38·9	18·0
8	48	11	34·2	18·0
10	24	11	36·0	18·0
11	28	11	35·1	18·0
0	43	11	34·2	18·0
1	33	11	34·8	18·1
2	34	11	33·6	18·0
3	37	11	33·6	18·0
4	32	11	34·2	18·8

Time of Observation.		Time of 300 Vibrations of a horizontal Needle.		Temperature (Centesimal).
h.	m.	m.	s.	°
5	38	11	34·2	18·8
6	49	11	33·0	18·8
6	57	11	33·0	18·8
7	59	11	34·5	18·5
8	9	11	33·9	18·5

17th of June, 1852, Paris mean time:—

h.	m.	m.	s.	°
9	0	11	34·8	18·1
9	36	11	33·9	18·5
10	15	11	34·2	18·6
10	54	11	33·0	18·7
11	36	11	34·5	18·7
0	40	11	34·2	18·7
1	38	11	34·5	18·7
2	38	11	33·9	18·8
2	58	11	34·8	18·9

(Geocentric commencement of the eclipse at 3 h. 6 m.)

h.	m.	m.	s.	°
3	30	11	35·1	19·1
3	55	11	32·7	19·1
4	16	11	33·0	19·0
5	6	11	33·3	19·2
5	22	11	33·0	19·1
5	30	11	33·0	20·0
5	59	11	35·4	19·6
6	32	11	33·0	19·4

End of the eclipse at 7 h. 12 m.

h.	m.	m.	s.	°
7	22	11	33·0	19·0
7	58	11	33·3	19·0

18th of June, 1852, Paris mean time:—

h.	m.	m.	s.	°
9	29	11	35·5	18·2
10	48	11	34·8	18·5
0	13	11	33·6	18·8
1	32	11	33·6	18·9
3	11	11	33·9	19·2
3	21	11	33·9	19·2
6	43	11	33·6	19·0

The above figures, taken in combination, show that the horizontal needle at Paris did not indicate any sudden and sensible change of magnetic force, either at the commencement of the eclipse, at its termination, or during its continuance. I may add that the inclination needle was most carefully watched without any irregularity being discovered in its indications.

For the sake of brevity these observations are not given. Those which have been reported above sufficiently show that the facts are opposed to M. Lion's conjecture, at least for the invisible eclipse of the 17th of June, 1852. Perhaps, as reporter, I ought to have communicated these results to the other members immediately; but the great reluctance I felt to give pain to a young man of apparently very extensive information led to a delay, during which I received from the author of the memoir a letter from which it seemed that observations made at Beaune had agreed no better with the theoretical expectations which he had announced than those made at Paris had done. Fearing, no doubt, that the publication of the latter would do him some injury in the minds of persons belonging to the small town in which he resides, M. Lion wished that the negative result to which our observations had led might not be published. So far as I was concerned I thought this wish might, without impropriety, be complied with; although I did not consider that, in such complicated researches, an error honestly fallen into by a person living at a distance from any of the great centres of scientific research, ought to tell against him in the mind of any person.

Now, however, when the supposed fact at first announced by M. Lion is cited in certain publications as being in conformity with observation, it would be wrong to remain longer silent, for science has also its claims.

It would seem to follow from a recent letter which has been communicated to me, that M. Lion persists, to a certain degree, in his former ideas, thinking only that some eclipses, or ecliptic conjunctions, may be accompanied by a change in the magnetic force, and others not.

Future observations will enlighten us on this point.

CHAP. XXI.

VARIATION OF THE INCLINATION AND OF THE INTENSITY OF THE MAGNETIC FORCE FROM ONE PLACE TO ANOTHER.

[In this chapter (which was obviously written between twenty and thirty years ago) M. Arago has republished a table of the relative values of the magnetic force in different parts of the globe, which was drawn up by M. Hansteen, and had appeared in "Poggendorf's Annalen" and "Brewster's Edinburgh Journal" for 1826. It has been considered, therefore, unnecessary to reprint it here, more especially as more recent and much more extensive tables may be referred to by the English reader in a report, drawn up at the request of the British Association, "On the Variations of the Magnetic Force observed at different Points of the Earth's Surface," and printed in the volume of the Transactions of the British Association at the Liverpool meeting in 1837.]—Ed.

CHAP. XXII.

DIURNAL VARIATION OF THE MAGNETIC INCLINATION.

Recent treatises on physics still say that the existence of such variations is uncertain. I think that when their authors see the figures extracted from my registers of observation they will regard the phenomenon as completely established.* I communicated the discovery of the fact to the Bureau des Longitudes, at its meeting on the 23rd of May, 1827. Since then the subject has been examined in different observatories. I am indebted to one of the most competent physicists in these sorts of researches, M. Bravais, for a notice of investigations which have been made since my own observations.

The instrument which M. Kupffer caused to be constructed by Gambey for the purpose of observing the diurnal variation of the inclination, was set up at St. Petersburgh on the 19th of August, 1830.

From the observations made with this instrument, the needle of which has its axles cut into knife edges, it would follow that the greatest, or maximum, inclination takes place at ten in the morning, and the least, or minimum, at ten in the evening. The extent of the variation was generally four or five minutes of arc; sometimes, though rarely, seven or eight minutes.

* [The particular treatises referred to by M. Arago are not indicated by him; but it is most certain that the progress which has been made in magnetical knowledge, more particularly in the last fifteen or twenty years, requires special attention on the part of authors who profess to convey systematic information on this branch of science in treatises on physics.]—Ed.

In the new system of magnetic observations, concerted at Gottingen in 1839, there is no instrument for observing the diurnal variation of the inclination. Colonel Sabine, on the occasion of the observations made at Toronto, in Canada, says that by the employment of Gauss's and Lloyd's magnetometers observed simultaneously, the horary variations (for every hour of the twenty-four) of both the horizontal and the vertical components of the magnetic force are followed, from whence the different inclinations, and consequently the diurnal variation of the inclination, are deduced by calculation.

The observations made at Toronto during the years 1840, 1841, and 1842, gave the following diurnal variation:—

Maximum inclination at 10h. A. M.
Minimum inclination at 4h. P. M.
Extent or amplitude of the variation 1′·21.

At Van Diemen Island, during the years 1842—1848, the diurnal variation of inclination deduced by calculation from the diurnal variation of the horizontal and vertical forces gave:—

A *maximum* of *south* inclination at 11h. 30m. A.M.
A *minimum* „ „ at 6 A.M.*

The mean results of ten years of direct observation (January, 1841, to December, 1850)† give for the (*south*) inclination of each month:—

	Degrees.		*Minutes.*
January	70	-	36·85
February	70	-	36·87
March	70	maximum	37·34
April	70	-	36·64
May	70	-	36·47
June	70	-	34·49
July	70	-	35·59

* [By some accident the *secondary* maxima and minima of the diurnal variation of the inclination at Toronto and Van Diemen Island have been omitted in the original French. In the volumes quoted from they are stated as follows:—

Toronto { A secondary maximum of north dip at 10 P.M.
A secondary minimum „ „ 6 A.M.

Van Diemen Island { A secondary minimum of south dip at 5 P.M.
A secondary maximum „ about 10 P.M.]

—ED.

† [The editor has substituted the results of *ten* years of direct observation of the inclination at Van Diemen Island, from the 2nd vol. of the observations of that observatory, for the results of *seven* years which M. Bravais had taken from vol. i. (not having then received vol. ii.). The results for the longer period of observation, although differing very slightly, are, of course, to be preferred.]—ED.

	Degrees.		Minutes.
August	70	minimum	33·91
September	70	-	35·43
October	70	-	35·59
November	70	-	36·45
December	70	-	36·49

There is here, as we perceive, a maximum in March and a minimum in August, with a difference of 3′·43.

At Milan, Mr. Kreil observed the variations of the inclination by means of a needle furnished with a small mirror parallel at once to the magnetic axis and to the axis of rotation of the needle, and reflecting the divisions of a scale parallel to the magnetic axis.

The apparatus is described in the first supplement to the Milan "Ephemerides," p. 181.

During the years 1837 and 1838 he observed at the following hours:—

8 A.M.	4½ P.M.
10½ A.M.	7½ P.M.
1 P.M.	11 P.M.

He found a very small diurnal variation, viz.—

8 A.M.	63°	51′	11″
10½ A.M.	63	51	25
1 P.M.	63	51	14
4½ P.M.	63	51	18
7½ P.M.	63	51	8
11 P.M.	63	51	4

The magnetic observations made in the "Expedition du Nord" by M. Bravais and his fellow observers, include measures of the variations of the vertical and horizontal components of the magnetic force. The diurnal variations of the horizontal force during magnetically calm days gave:—

A principal maximum at 6 P.M.
A principal minimum at 1 A.M.
A secondary maximum at 7 A.M.
A secondary minimum at 11 A.M.

On disturbed days the morning maximum and minimum disappear; on such occasions the principal maximum occurs earlier (about half-past four), and the secondary minimum about midnight, according to M. Bravais. This physicist has shown that in our own and adjacent countries the secondary maximum should occur at six in the morning, and the secondary minimum at noon; and he has cited, in support of this, the observations made by M. Lamont at Munich (1842, 1843).

The diurnal variation of the vertical force could not be determined for calm days: the calmest period is from 7 A.M. to 3 P.M. On disturbed days the disturbances are sometimes positive and sometimes negative from 4 P.M. to midnight, and from midnight to 8 A.M. they are almost always negative; and negative perturbations also preponderate during the rest of the day. The maximum occurs about 2 P.M. and the minimum about 2 A.M.

From these two elements,—viz., variations of the horizontal, and variations of the vertical force,—the diurnal variations of the inclination may be deduced by calculation, and this is what is generally done in the present day.

My observations, on the contrary, were made directly with the instrument, of which the principle was indicated in p. 363.

[M. Arago's observations of the diurnal variation of the inclination are contained in the register books of the diurnal variations of the declination.

In general M. Arago used to concentrate his observations in two parts of the day—*i.e.* between eight and nine in the morning and between six and seven in the evening. But he sometimes made as many as a hundred and fifty observations in a single day, and, from a general view of his researches, the inclination would appear to have each day—

1. A maximum between eight and nine in the morning.
2. A minimum from two to three in the afternoon.
3. A second maximum between eight and nine in the evening.
4. A minimum between eleven in the evening and midnight.

The above occur earlier or later, according to the season and the temperature.

M. Arago's observations of the variations of the inclination are very numerous; they amount to more than twenty thousand, but they were not made with as much regularity as those of the declination. M. Fédor Thoman has been able to calculate completely the four series which correspond to the years 1827, 1828, 1829, and 1830, so as to secure their representing exactly the phenomenon looked for.

1827.

Months.		Means of the Maxima.	Means of the Minima.	Monthly Means.	Mean Amount of Diurnal Variation.
January	-	68° 30′·72	68° 30′·23	68° 30′·47	0′·49
February	-	68 30 ·32	68 28 ·66	68 29 ·49	1 ·66
March -	-	68 30 ·40	68 30 ·00	68 30 ·20	0 ·40
April -	-	68 35 ·17	68 32 ·60	68 33 ·89	2 ·57
May -	-	68 36 ·89	68 35 ·11	68 36 ·00	1 ·78
June -	-	68 36 ·21	68 35 ·05	68 35 ·63	1 ·16
July -	-	68 39 ·79	68 37 ·44	68 38 ·61	2 ·35
August	-	68 55 ·25	68 52 ·74	68 53 ·99	2 ·51
September	-	68 47 ·05	68 42 ·63	68 44 ·84	4 ·42
October	-	68 34 ·68	68 33 ·36	68 34 ·02	1 ·32
November	-	68 32 ·52	68 31 ·20	68 31 ·86	1 ·32
December	-	68 30 ·50	68 29 ·90	68 30 ·20	0 ·60
Means -	-	68 36 ·62	68 34 ·91	68 35 ·77	1 ·71

The lowest minimum occurs in February, and the highest maximum in August. The greatest amounts of diurnal variation were in April and September, and the smallest occurred in March.

1828.

Months.		Means of Maxima.	Means of Minima.	Monthly Means.	Mean Amount of Diurnal Variation.
January	-	68° 24′·19	68° 23′·65	68° 23′·92	0′·54
February	-	68 25 ·10	68 24 ·80	68 24 ·95	0 ·30
March	-	68 23 ·19	68 20 ·50	68 21 ·84	2 ·69
April -	-	68 24 ·31	68 20 ·20	68 22 ·25	4 ·11
May -	-	68 24 ·67	68 23 ·39	68 24 ·03	1 ·28
June -	-	68 27 ·18	68 25 ·37	68 26 ·27	1 ·81
July -	-	69 9 ·20	69 5 ·86	69 7 ·53	3 ·34
August	-	69 7 ·02	69 4 ·70	69 5 ·86	2 ·32
September	-	69 2 ·40	69 0 ·10	69 1 ·25	2 ·30
October	-	68 44 ·21	68 40 ·41	68 42 ·31	3 ·80
November	-	68 25 ·90	68 24 ·40	68 25 ·15	1 ·50
December	-	68 19 ·65	68 18 ·80	68 19 ·22	0 ·85
Means -	-	68 36 ·38	68 34 ·35	68 35 ·36	2 ·03

The lowest minimum occurred in March, and the highest maximum in July. The greatest amount of diurnal variation occurred in April and October, and the smallest in February.

1829.

Months.		Means of Maxima.	Means of Minima.	Monthly Means.	Mean Amount of Diurnal Variation.
January	-	68° 20′·15	68° 18′·70	68° 19′·42	1′·45
February	-	„	„	„	„
March -	-	68 19 ·47	68 16 ·70	68 18 ·08	2 ·77
April -	-	68 20 ·03	68 17 ·35	68 18 ·69	2 ·68
May -	-	„	„	„	„
June -	-	„	„	„	„
July -	-	68 34 ·18	68 30 ·23	68 32 ·20	3 95
August	-	68 24 ·40	68 21 ·02	68 22 ·71	3 ·38
September	-	„	„	„	„
October	-	68 29 ·77	68 27 ·04	68 28 ·40	2 ·73
November	-	68 28 ·53	68 25 ·71	68 27 ·12	2 ·82
December	-	68 27 ·08	68 25 ·35	68 26 ·22	1 ·73
Means -	-	68 25 ·45	68 22 ·76	68 24 ·10	2 ·69

Although four months' observations are wanting, we see that in 1829 there was a maximum in July and a minimum about March,—an inference analogous to that deduced from the observations of the preceding years.

It was particularly during the year 1830 that M. Arago studied the movements of the inclination needle with minute attention. The results present a regular series of more than three thousand observations, which furnish a very exact table of the march of the diurnal variation of the inclination, and of the absolute monthly values of that important element of terrestrial magnetism.

Months.		Means of Maxima.	Means of Minima.	Monthly Means.	Mean Amount of Diurnal Variation.
January	-	68° 26′·10	68° 24′·21	68° 25′·16	1′·89
February	-	68 25 ·87	68 24 ·74	68 25 ·30	1 ·13
March -	-	68 29 ·51	68 27 ·69	68 28 ·60	1 ·82
April -	-	68 35 ·84	68 33 ·04	68 34 ·44	2 ·80
May -	-	68 38 ·18	68 34 ·42	68 36 ·60	3 ·76
June -	-	68 40 ·42	68 36 ·64	68 38 ·53	3 ·78
July -	-	68 39 ·03	68 36 ·29	68 37 ·66	2 ·74
August	-	68 44 ·32	68 41 ·90	68 43 ·11	2 ·42
September	-	68 39 ·77	68 37 ·47	68 38 ·62	2 ·30
October	-	68 40 ·76	68 38 ·41	68 39 ·58	2 ·35
November	-	68 37 ·89	68 35 ·55	68 36 ·72	2 ·34
December	-	68 36 ·23	68 34 ·50	68 35 ·36	1· 73
Means -	-	68 36 ·16	68 33 ·74	68 34 ·95	2· 42

The minimum of inclination was in February, and the maximum in August.

We might, therefore, say that the minimum of inclination nearly coincides with the spring equinox, and the maximum with the summer solstice.

We also see that the diurnal variations of the inclination do not, generally speaking, exceed 3′ or 4′, and that consequently differences of some tens of minutes, found by observations which may not have been made at the same hours and seasons, may yet be looked upon as indicating with sufficient approximation the general march followed by the inclination at a given place. Thus, we find for Paris, as concluded by M. Arago in the notes left by him:—

Year.		Mean Yearly Decrease.
1798	69° 51′	4′·93
1812	68 42	0·43
1828	68 35	7·16
1850	67 9	3·99
1851	66 35	

From 1798 to 1851, or in fifty-three years, there has been a diminution of 3° 16′, or on the mean, 3′ 41″·9 a year.]

Terrestrial magnetism often presents a marvellous harmony in its march at places very remote from each other, but sometimes also anomalies are met with which show that there exist perturbing forces whose nature is unknown to us. We may follow the indications resulting from the varying play of the magnetic forces by the use of instruments of extreme precision which our skilful artists have succeeded in constructing, but under condition of very frequent and very exact observations being executed in a great number of observatories, at times agreed on beforehand and at very short intervals. My illustrious friend, Alexander von Humboldt, has sought to associate many friends of science in the promotion and prosecution of this common work, and Gauss has seconded him in efforts which deserve to be crowned by success. It is necessary that instruments to be employed in the determination of the magnetic force should be strictly comparable with each other, and that great care should be taken in examining whether any and what changes take place in the magnetism of the needles which are employed.*

* [It is evident from the tenor of this paragraph that this part of the "Essay" remains as written previous to the realisation of this view in the systematic researches which have been so extensively carried out in so many countries in the last fifteen years.]—Ed.

AURORA BOREALIS.

CHAPTER I.

DEFINITION OF THE AURORA BOREALIS.

To popular apprehension the highest or ultimate object of meteorology is to enable us to foretel the weather. Looked upon in this point of view, science can as yet only offer abortive attempts, or such as hold out no promise for the future. In other respects, on the contrary, her advances have been assured, rapid, and brilliant. To justify this assertion it might be sufficient to say that at the present time the simple observation of a magnetised needle freely suspended, indicates that there is taking place in distant regions a phenomenon as worthy of the attention of the most learned physicist, as of the admiration of the most uninstructed spectator.

At the commencement of the 17th century, Gassendi gave the name of aurora borealis to a phenomenon which in our climates usually makes its appearance in the northern quarter, manifesting itself at first near the horizon by a faintly shining light analogous to twilight.

The aurora borealis is not like the rainbow, nor like halos, coronas, parhelia, &c., a simple phenomenon of light; it appears to be connected with the magnetic forces of the earth. These forces being variable, as to position at least, it is reasonable to ask whether the aurora borealis has always existed; whether in all preceding centuries it has appeared with the same forms, the same brightness, the same colours, in the same regions of space, &c. &c. We will proceed to consider the phenomenon successively under its different aspects.

CHAP. II.

AURORA BOREALIS OR NORTHERN LIGHTS WERE KNOWN TO THE ANCIENTS.

Pliny is evidently referring to displays of aurora borealis when he speaks as follows of extraordinary luminous appearances dispelling the obscurity of night. He writes: — " Under the consulate of C. Cæcilius and Cn. Papirius (in the year of Rome, 641), and also at some other times, a light was seen to overspread the heavens in the night time, so that a kind of daylight replaced the darkness.

" Under the consulate of L. Valerius and C. Marius (year of Rome, 654), at the time of sunset, a burning shield, from which sparks shot forth, was seen to traverse the heavens from west to east." (Pliny, lib. 2. chap. xxxiii. and xxxiv.)

According to a very learned investigation, due to M. Edouard Biot, the first positive notice of an aurora borealis in the Chinese records, goes back to the year 208 before the Christian era. (*Comptes rendus de l'Academie,* t. xix. p. 829.)

CHAP. III.

ON AURORAS OBSERVED IN THE NORTH.

Displays of aurora are scarcely anywhere so frequent and magnificent as in the regions where the laborious and zealous observers of the Iceland and Lapland expeditions wintered.

It is a pleasure to remark the exemplary perseverance with which several of our young travellers studied this mysterious phenomenon, and the manner in which they availed themselves, in so doing, of the most refined methods of astronomical, geodesical, and physical observation.

In the course of 206 days (from September, 1838, to April, 1839), passed at Bossekop in Finmarken, in 70° north latitude, there were observed 143 auroras, of which 64 took place during the ten-weeks' night of those regions. I borrow with some abridgment some general features from M. Lottin's description of the magnificent phenomenon which he enjoyed such frequent opportunities of witnessing.

Between the hours of four and eight in the evening the light mist which almost habitually prevails towards the north, at about 4° or 6° above the horizon, becomes coloured in its upper portion, or rather, fringed by the light of the aurora which exists behind. This fringe or border then becomes more regular, and forms an imperfectly defined arch, of a pale yellow colour, with diffused edges, and its extremities resting on the ground.

Soon dark streaks manifest themselves at intervals in the luminous matter of the arch, which now rises slowly, its summit remaining nearly in the magnetic meridian.

There are then formed beams, which lengthen and contract, either slowly or instantaneously, and as they shoot upwards their brightness is suddenly increased or lessened. They all appear to converge to a point in the heavens indicated by the direction of the dipping needle; sometimes they quite reach this point, and, uniting there, form a portion of a vast cupola or canopy of light.

The arch continues to rise towards the zenith, undergoing a waving movement as the brightness of each beam successively increases in intensity.

Not unfrequently the arch is merely a long band of auroral beams, waving and dividing itself into several parts; these form graceful curves, which then become nearly closed, and present what have been called "coronæ boreales," whatever may be the part of the heavens which they occupy.

The curves form and unroll like the folds of a serpent; the beams are coloured with various hues, the lower part is of a bright sanguine tint, the middle a pale emerald green, and the rest remains a pale yellow light.

Fresh arches rise from the horizon; as many as nine have been counted; they then close one upon another and disappear towards the south. Sometimes the mass of beams which have already passed beyond the magnetic zenith, returning from the south, unites with beams coming from the north, and together produce the true coronal phenomenon: this is most often elliptic in its form, rarely circular. The corona may also be formed without any previous arch.

Before reaching the southern horizon the coronas fade, and the arches become faint; the beams form patches of pale light, which gradually become more indistinct until they are confounded with the clouds.

CHAP. IV.

AURORAS OBSERVED IN VARIOUS PLACES.

On the 6th of March, 1715—1716, an aurora borealis was observed at Cambridge by Roger Cotes.

The first streamers appeared in the north, but at a quarter after seven streamers shot upwards in all parts of the sky, from north to south. They unitedly formed a species of canopy: their point of reunion was 20° south of the zenith, the azimuth of the point being 10° east of south; the canopy rose from about 10° or 15° above the north horizon, in which direction it was most extensive; on the south side it only went down to 40° above the horizon.

The prismatic colours were sometimes more vivid than those of the brightest rainbow, but they faded in the course of a second of time.

The author attributes the phenomenon to parallel rays, which only appear to converge by an effect of perspective.

Roger Cotes relates that he remarked a very sensible tremulousness in the upper extremities of the luminous rays or streamers of the aurora just described. The streamers were also sometimes crossed by a kind of undulations, rising in the north and parallel to the horizon.

On a previous occasion he had seen a great number of parallel streamers shooting from a luminous patch or cloud in the northern region; sometimes a portion of this cloud detached itself and moved in a direction parallel to the horizon; and this portion carried with itself one or more of the bundles of rays of which we have been speaking, and crossed them successively, continuing, nevertheless, always parallel to them.

To enable those who might desire to do so, to calculate more exactly the position of the aurora borealis of the 6th of March, 1715—1716, Cotes remarked that at a quarter past seven the summit of the canopy was nearly halfway between Castor and Pollux. (*Philosophical Transactions*, 1720, vol. xxxi. p. 66.)

On the 30th of March, 1717, at Rochester, the Rev. Edmund Barrell states that he observed an aurora which was not quite in the north, but a little towards the west. (*Phil. Trans.* vol. xxx. p. 584.)

Martin Folkes observed the same aurora; he estimated the position of the culminating point of the luminous arch to have been 20° to the west of true north. Before the arch disappeared this deviation appeared to be less by several degrees. (*Phil. Trans.* vol. xxx. pp. 196. and 588.)

Halley observed an aurora in London on the 10th of November, 1719. The point of concourse of the luminous rays was 14° south of the zenith, and very near the meridian. The points from which the rays darted were at altitudes of at least 30° or 40° above the horizon; there was no longer any light visible near the horizon, yet the sky was perfectly calm and clear. (*Phil. Trans.* vol. xxx. p. 1099.)

On the 15th of February, 1730, Cramer observed an aurora at Geneva. The base of the luminous arch rested on a chord of about 145°; the middle of the arch was about 15° west of south (at half-past eight in the evening). The altitude of the highest point was from 30° to 40°.

There was seen at the same time, in the south, a luminous band, of which the height varied successively between 45° and 54°. This band, in some respects not unlike a rainbow, but broader, varying in breadth between 14° and 20°, was terminated by two parallel arches at equal intervals from it. Its culminating point was 15° east of south; it was therefore diametrically opposite to the culminating point of the northern arch. The colour of the southern zone was scarlet. In this aurora, contrary to what is usually the case, the light of the stars over which the arches passed was considerably dimmed. The weather was calm, clear, and cold. (*Phil. Trans.* 1730, vol. xxxvi. p. 279.)

On the 9th of October, 1730, Mairan and Cassini, one being at Breuillepont, in Normandy, and the other in Picardy, saw an aurora of the ordinary kind, which a little later, at eight in the evening, began to break up nearly in the middle, and divided into two luminous ovals, inclined to the horizon, each from 15° to 18° long by 5° or 6° wide, the Pleiades being seen between them. Both then became less bright, changed their shape, and soon disappeared.

At the same time Rouché was observing at Poitiers, and very nearly in the same part of the heavens, an aurora whose forms did not appear capable of being reconciled with those described by Mairan and Cassini, after making allowance for parallax. At Poitiers "there was seen first a semicircle, having its diameter turned upwards; the diameter was parallel to the horizon, and more than 20° long. This semicircle afterwards divided into two lesser ones, contiguous to each other, their diameters forming one straight line still parallel to the horizon. These very regular figures did not last long; the two smaller ones became joined into one large and almost complete circle, which lastly became a sort of segment of a circle, terminating in a trident, of which the prongs were very long and very wide apart." (*Academie des Sciences*, 1730, hist. p. 7.)

Lastly, Maraldi, in his description of the same aurora, seen from Paris (*Memoires*, p. 574.), speaks only of two luminous columns, inclined to the horizon, from 16° to 18° long by 5° or 6° broad. One began to diminish at twenty-five minutes after eight, while the other increased.

Dr. Blane reported having observed an aurora borealis at Barbadoes, on the 10th of October, 1780, during a hurricane; it appeared in the north-east. (*Academy of Edinburgh*, 1788, vol. i. p. 34.)

CHAP. V.

ON THE DETERMINATION OF THE HEIGHT OF AURORAL ARCHES.

In our climates, when an aurora is complete — when a part of its light describes a well-marked arch — the culminating point of this arch is in the magnetic meridian, and its two apparent points of intersection with the horizon are at equal angular distances from that meridian.

When luminous streamers shoot from the different parts of the arch, their point of intersection — that which some meteorologists have called the centre of the cupola — is in the magnetic meridian, and precisely in the prolongation of the dipping needle.

It is very important to repeat this kind of observation in many places; less for the sake of establishing a general connection between auroras and terrestrial magnetism, which cannot now be doubted by any one, than for the sake of the light which it may throw on the intimate nature of the phenomenon, and on the geometrical methods by which the absolute height has sometimes been sought to be determined.

These methods, being founded on combinations of parallax, presuppose that it is the same arch which is everywhere seen. I mean by the *same* arch the same material molecules brought by unknown causes to a luminous state! If I am not mistaken, this hypothesis, when examined with duly scrupulous caution, will give occasion to some serious doubts.

The relation between the direction of the auroral arch and that of magnetism, only proves that the phenomenon is placed symmetrically in reference to the magnetic axis of the globe. The kind of displacement which the centre of the cupola undergoes with every change of position in the observer is not to be explained by parallax. This displacement is such that an observer proceeding from Paris towards the north magnetic pole sees the centre of the cupola (situated to the south of his zenith) rise more and more above the horizon; an effect precisely opposite to what would be the case if the cupola were a radiant point, and not a simple effect of perspective.

When it has once been established that in auroras one of the appearances is a pure illusion, it may be asked why it should be unhesitatingly assumed that the luminous arch seen at Paris is the same which is seen at Strasbourg, Munich, Vienna, &c.? Will it not be perceived how great a step would have been made in the theory of these mysterious phenomena, if it were established that every observer sees his own aurora as every one sees his own rainbow? Would there not, moreover, be something gained by disencumbering our meteorological catalogues of a multitude of determinations of height, which in such case would no longer have any real foundation, though due to such great names as Mairan, Halley, Krafft, Cavendish, and Dalton?

Before concluding a chapter in which there has been so much said respecting the absolute height of the matter amidst which the aurora is formed, I must not forget to remark that Captain Parry on one occasion thought he saw luminous streamers proceeding from an aurora projected on a mountain at but a small distance from his ship. This observation deserves to be confirmed and repeated.

The preceding lines, written to form part of the Instructions to be given by the Academy of Sciences, relatively to observations in meteorology and the *physique du globe*, for the scientific Expeditions du Nord, and for Algeria, gave rise to a reclamation of priority from M. Morlet, 13th of April, 1840. I replied in the following manner, in which I see nothing to alter:—

"The surmise that each observer may see his own auroral arch or bow, as every one sees his own rainbow, was put forward more than twenty years ago in professorial lectures on the Physique du Globe, given by me at the Ecole Polytechnique, and at the Observatory. If it were worth while, evidence of this might still be found in the notes taken by the pupils, in the procès verbaux of the Bureau des Longitudes, and even in printed works older by at least ten years than the treatise referred to by M. Morlet. But I can hardly suppose that that gentleman would press the question, for I could, if needful, show him memoirs, more than a century old, in which positive proofs are given that the aurora borealis seen in one place may not be the same which is seen at the same time in another place. I would also prove to him that long before his time it had been seen that it was necessary to examine whether the auroral arch was truly circular or not. I may affirm that Messrs. Lottin, Bravais, and Martins did not need to learn from M. Morlet's writings that the determination of the form of the arch could be perfectly well obtained by measuring the abscissæ and ordinates. The little ground which there is for M. Morlet's reclamations shall not prevent me, however, from saying that the calculations which he has entered into, in examining the circularity or non-circularity of auroral arches by means of old observations, are really interesting."

The details related in the preceding chapter of the different auroras observed in the last century, show sufficiently that in each place there are seen appearances of the same aurora, but modified by the difference of place.

CHAP. VI.

ON THE SOUND ATTRIBUTED TO AURORAS.

Are auroras accompanied by sound? This is a question of fact, respecting which observers are not in accord. We will first take the statements which favour the affirmative.

Here are two passages from the Rev. Jeremy Belknap, taken from the second volume of the "Transactions of the American Society," p. 196:—"Two years ago," (in 1781) "at Dover, in New Hampshire, U. S., while I was attentively examining the streamers from the luminous arch of an aurora borealis, which appeared on a calm night, during frost, I thought I heard a faint rustling noise, like the brushing of silk."

"In March, 1783, the whole sky appeared on fire; luminous streamers seemed to rise from all points, and to converge to the zenith. There was no difference between the south and the north, save it may be that the vapours appeared to rise from points nearer the horizon in the north than in the south. The wind blew fitfully from the west, usually with intervals of two or three minutes between the gusts. In these intervals, I distinctly heard a rustling noise, easily distinguishable from the wind, and which, moreover, would have been quite overcome by that of one of the gusts."

The following passage is found in Cavallo's "Elements of Natural or Experimental Philosophy" (vol. iii. p. 445.):—"Sometimes those coruscations," (the coruscations of the aurora) "when strong, are accompanied by a sort of distinct crackling noise. I remember having myself heard it on more than one occasion."

"The auroras in Greenland are very brilliant; the luminous columns of which they are formed sometimes overspread the whole horizon with colours as bright and as varied as those of the rainbow. These phenomena are rarely seen in the north horizon; they most often appear in the east, or at the zenith. When the auroras appear low down, a crackling is heard, resembling that of the electric spark. The Greenlanders believe that at such times the souls of the dead are fighting in the air." (*Edinb. Encyclopædia*, vol. x. part ii. p. 488. year 1815.)

M. Ramm, Royal Inspector of the Norwegian Forests, wrote, in 1825, to M. Hansteen, that "in 1766, 1767, or 1768, he had heard the noise of an aurora borealis. M. Ramm, who was only about ten years old at the time, remarked this phenomenon while crossing a meadow near which there was no forest. The ground was covered with snow and hoar frost." (Observe in the Narratives of Captain Franklin, that the snow sometimes emits a crackling sound.) "The sound, M. Ramm said, always coincided with the apparition of the bright streamers. But how could this be, since those streamers are incontestably at some considerable height in the atmosphere?" (*Phil. Mag.* March, 1826, p. 177.)

Wargentin relates, in the fifteenth volume of the "Swedish Transactions," that two of his pupils, Dr. Gisler and M. Hellant, who had resided long in the northern parts of the kingdom, made a report to the Academy of Stockholm, of which the principal passages are to the following effect:—

"The matter of which the northern lights consist sometimes comes down so low as to touch the ground. On the summits of high mountains, it affects the faces of travellers in a manner analogous to wind." "I have often," adds Dr. Gisler, "heard the sound of auroras. It is a noise much resembling that of a strong breeze, or the brushing sound produced by some chemical substances when in the act of decomposition. I often thought I perceived the cloud to have a smell of smoke, or of burnt salt." "The Norwegian peasants told the doctor that there sometimes rose from the ground a cold fog, of a greenish-white tint, rendering respiration difficult, and obscuring the sky, but not preventing the distant mountains from being seen; and that this fog ends by giving rise to an aurora." (*Phil. Mag.* March, 1826, p. 178.)

I will next take the observations which make either for a doubtful or a negative answer to the question.

Gmelin (the elder, the botanist) had said in his "Travels in Siberia" (vol. ii. p. 31.), translated by Keralio, that "auroras crackle like flame; but it was not he himself who had heard the noise; he states it only from what he had learnt from the inhabitants of Yenisseisk, in Siberia." According to them, "the hunters who go after foxes assert that the auroras make a noise like that of fireworks, such as to make their dogs crouch down with terror, and refuse to move until the sound has ceased."

Patrin doubts the authenticity of this account, and remarks that it is not customary in Siberia to hunt foxes with dogs (especially at night); they are caught by setting traps for them. Patrin adds, that Pallas, who had travelled six years in Siberia, spoke of this passage of Gmelin's jestingly.

During nine winters passed in different parts of Siberia, Patrin saw many very fine auroras, but never heard any sound from them. He adds, that "neither the Bishop Egede, who had lived fifteen years in Greenland and gave an account of its natural history and meteorology, nor the Pastor Horrebow, who has described a hundred and sixteen auroras observed by him in Iceland, ever make the slightest mention of these noises and cracklings." (*Bibliothèque Britannique*, t. xlv. p. 89. *et seq.*)

"It is scarcely possible to observe the sudden apparitions and great movements of the masses of light of which auroras consist without fancying that they produce a rushing sound; but I am confident that the idea is erroneous, and that there is no actual noise attending the phenomenon. I frequently stood for hours together on the ice, to ascertain this fact, at a distance from any noise but my own breathing, and thus I formed my opinion." (Captain Lyon's *Private Journal*, p. 100.)

Captain Franklin informs us, that at Cumberland House (in 54° north latitude), during frost, and in calm weather, auroras were seen almost every evening; but that not the slightest noise was ever heard, even when the phenomenon was most vivid. The residents at the Factory asserted, on the contrary, that the aurora was often accompanied by *a rustling sound.* But it is so natural to associate the idea of noise with that of rapid movement, that many observers may have yielded to the illusion.

Mr. Winn presented to the Royal Society, in 1772, a memoir, in which he proposed to prove that the appearance of an aurora borealis is a certain prognosticator of a tempest from the south or south-east.

CHAP. VII.

HOURS OF THE AURORAS.

CAPTAIN LYON, in his "Brief Narrative," p. 167, says, that auroras are rarely seen before nine in the evening, and that their maximum intensity is usually about ten.

We shall see further on, that such indications must not be construed strictly.

CHAP. VIII.

CAUSES OF AURORAS.

THE idea of an intimate connection between magnetism and auroras goes back to the last century. A member of our Academy of Sciences, Du Fay, expressed himself in the following terms, in a treatise entitled "Memoire sur l'Aimant," dated 15th of April, 1730, and published in 1732 in the volume of the "Memoires de l'Academie" for 1730 (pp. 147, 148.):—"Mr. Halley, and since him some other physicists, have said that magnetic matter might have some share in the northern lights.

"It may also be added, that, according to the most exact observations, the centre in which the rays of the aurora terminate, almost always declines to the west of north by 14° or 15°, which is about the present amount of the declination of the magnetic needle. If this centre of the rays of the aurora borealis should be found in future to follow the variations of the magnetic needle, this might lead us to something more positive respecting the cause of auroras."

By the centre in which the rays terminate, Du Fay no doubt means the centre of the luminous arch, or the centre of the cupola.

This idea did not meet with an immediate reception; for Garnett remarked that the central point in the aurora borealis is about 10° to the south of the zenith. He imagined, in consequence, that this want of coincidence of the two points does not take place at the equator, and that it increases in approaching the pole. Garnett was not, therefore, aware in 1791 of the connection which exists between the centre of the aurora, and the point towards which the dipping needle is directed. (*Manchester Memoirs*, vol. iv. p. 255.)

It was thought that the accidental inflammation of the hydrogen gas supposed to exist in the upper regions of the atmosphere, might furnish a plausible explanation of the aurora borealis.

In this hypothesis the magnetic properties of the phenomena depended on the iron with which the gas was supposed to be impregnated. (Usher, *Irish Transactions*, vol. ii. p. 190.)

Du Fay, in the memoir which has been cited above, adopts the opinion that the inflammable substances in the upper regions of the atmosphere are of themselves sufficient for the explanation of auroras. He supposes "inflammable substances, dispersed in the atmosphere, some of them perhaps already in a state of inflammation, to be brought by their particular degree of density or weight to that particular distance from the earth at which the magnetic matter is circulating in the greatest abundance; there this torrent, which is rolling towards the north, collects these scattered exhalations from the whole atmosphere, and draws them towards the pole; those which are already inflamed kindle others, or they are ignited by collision only, and the current disposes them in the form of rays such as we see them."

The following remarks and views are more plausible. Any theory which does not rest on facts already assured is entirely without scientific value.

Usher remarked (*Irish Transactions*, vol. xi. p. 191.), that the period of about forty years, pointed out by Mairan, during which there were very few auroras, corresponds by its middle date, 1661, with the time when the declination of the magnetic needle was zero in England and in France.

In 1788 Usher deduced the connection of the aurora with terrestrial magnetism from the position of the cupola, and still more from that of the arch. "The highest point of the arch," said he, "is always in the magnetic meridian."

For my part, I published in December, 1817 (*Annales de Chimie et de Physique*, 2ème série, t. vi. p. 443.), the following note:—"On the 6th of February, towards 6 P. M., a very fine aurora borealis was seen at Paris. We assured ourselves, by direct observation, that the culminating point of the luminous arch was situated exactly in the magnetic meridian."

In the January Number of the "Annales de Chimie et de Physique," 1819, t. x. p. 119., I added the following details:—

"The Petersburg academicians have several times announced that in that city the magnetic declination does not vary either from morning to evening, or from one day to the next, or even from one year to another. Notwithstanding the confidence inspired by such names as those of Euler, Krafft, &c., so extraordinary an anomaly ought not to be admitted until it shall have been assured by numerous observations made with very exact instruments.

"Auroras ought to be placed in the first rank among the causes which sometimes disturb the regular march of the diurnal variations. These do not, even in summer, exceed fifteen or twenty minutes, but when an aurora appears, the magnetic needle is often seen to move in a few instants several degrees from the magnetic meridian. How can an influence so well marked be reconciled to observations from whence it would seem to follow, that the same aurora which makes one needle move suddenly from east to west leaves a neighbouring needle unmoved, or imparts to it a contrary movement?

"During an aurora one often sees in the northern region luminous streamers of different colours shoot from all parts of the horizon. The point in the sky to which these streamers converge is precisely the point towards which a magnetised needle suspended by its centre of gravity directs itself; so that at Paris, where the inclination or dip of the needle is at the present time 68° 40′, this point is 21° 20′ south of the zenith. It has, moreover, been shown that the concentric circular segments, almost similar in form to the rainbow, which are usually seen previous to the luminous streamers of which we have been speaking, have their two extremities resting on two parts of the horizon equidistant from the magnetic meridian, and the culminating points of each arch are exactly in that meridian. From all this it appears incontestably that there is an intimate connection between the causes of auroras and those of terrestrial magnetism. However, it will only be by the help of numerous observations, made simultaneously at different parts of the earth with delicately suspended needles, that it will be possible to attempt to discover how the first of these phenomena modifies the second.

"Hitherto we have not a sufficient number of observations of variations, both because the instruments are costly, and because the observation of the diurnal variation requires a very inconvenient constancy of attention. Field-marshal the Duke of Ragusa, who deems scientific pursuits no unworthy occupation of his leisure, has happily so far made this subject his care as to have had an excellent declinometer, made by Gambey, placed a few months ago at Châtillon sur Seine in Burgundy, and to have provided that in his absence the observations should be made by an intelligent and well-instructed young man, who is also charged with the superintendence of some of the fine agricultural establishments which are now admired in the neighbourhood of the Château of Châtillon. These observations are regularly communicated to us, and will be usefully compared with those which we make at Paris.

"The march of the magnetic needle on the morning of the 31st of October, 1818, offered nothing remarkable, but at noon the declination began to increase more than usual; at 1 P. M. it exceeded its amount on previous days by about 12′, and at half-past 5 the excess was still as much as 7′. After this the needle retrograded abruptly towards the east, and at 8 o'clock the declination was nearly 9′ less than the mean of all the observations at that hour during the rest of the month. Thus the accidental displacements of the needle on the 31st of October were more considerable than its whole regular diurnal oscillation, which in this month is scarcely more than 10′.

"On the same day at Châtillon sur Seine, in the Château of the Duke of Ragusa, the needle experienced between 8 A. M. and 6 P. M. irregular movements perfectly similar to those at Paris.

"Lastly, I find in the observations of Colonel Beaufoy, made at Bushey Heath (1′ 2″ of time west of Greenwich, and in 51° 38′ latitude), that the declination of the needle on the morning of the 31st of October did not differ sensibly from what it had been on preceding days; but at 1 o'clock it was greater than usual by 11′. Observations in the evening are wanting.

"If in connection with these facts we bring into notice a letter dated Bishopwearmouth, Sunderland, which has just been inserted in Dr. Thomson's Journal, and in which Mr. Renney announces having seen an aurora borealis on the 31st of October, 1818, between 7 and 8 in the evening, it will not be doubted that this phenomenon, which was not seen at Paris by reason of clouds, determined the irregular oscillations of the needle observed at Bushey Heath, at the Royal Observatory at Paris, and at Châtillon. It will also be evident that the aurora acts before it is seen on the horizon, and that its influence is exercised simultaneously at considerable distances."

CHAP. IX.

ON AURORAS APPEARING IN DAYLIGHT.

WELL authenticated appearances of auroras during daylight are too few for me to omit the description given of such a phenomenon in the 5th volume of the Royal Society of Edinburgh. The observation is made by the Reverend Patrick Graham, and was made at Aberfoyle, in Perthshire.

"On the 10th of February, 1799, near half-past three in the afternoon, the sun being then a full hour above the horizon, and showing faintly through a leaden coloured atmosphere, I was intently observing a halo surrounding the sun. Whilst I was attending to this appearance, the whole visible hemisphere of the heavens became covered with a light palish vapour, as I at first imagined it to be. It was disposed in longitudinal streaks, extending from the west, by the zenith, and all along the sky towards the east. On examining this appearance more narrowly, I found it to be a true aurora borealis, with all the characters which distinguish that meteor when seen by night, excepting that it was now entirely pale and colourless. The stream of electric matter issued very perceptibly from a cloud in the west; thence diffusing themselves, the rays converged towards the zenith, and diverged again towards every quarter of the horizon; and the coruscations were as instantaneous and as distinctly perceptible as they are by night.

"This appearance continued for more than twenty minutes, when it gradually vanished, giving place to thin scattered vapours, which, towards sunset, began to overspread the sky.

Through the ensuing night I could not discern the smallest trace of aurora borealis."

The detailed catalogues of auroras published by Mairan in the last edition of his *Traité*, does not contain any observation made in the daytime. That learned academician says, "Great auroras usually begin early, a short time after the end of the twilight, and sometimes before." He adds, in another place, "So far as I am aware, the phenomenon never begins in the morning, after midnight, when the nights are pretty long."

In looking over the 2nd volume of the "Memoirs of the Irish Academy," I have found an observation by Dr. Henry Usher, Fellow of the Royal Societies of London and Dublin, which is so far from falling within the limits indicated by Mairan that it was made in the daytime, and quite near the hour of noon.

"On Saturday night, May 24th, 1788, there was a very bright aurora borealis, the coruscating rays of which united, as usual, in the pole of the dipping-needle. I have always observed that an aurora borealis renders the stars remarkably unsteady in the telescope. The next morning, about eleven, finding the stars flutter much, I examined the state of the sky, and saw whitish rays ascending from every part of the horizon, all tending to the pole of the dipping-needle, where, at their union, they formed a small thin and white canopy, similar to the luminous one exhibited by an aurora in the night. These rays coruscated or shivered from the horizon to their point of union.

"These effects were distinctly seen by three different people, and their point of union marked separately by each of them." (*Transactions of the Irish Academy*, vol. ii. p. 189.)

The influence exercised on the magnetic needle by auroras seemed to me to offer a means of deciding whether the phenomenon above described was really and truly an aurora borealis, seen in daylight. I therefore took, from the archives of the Bureau des Longitudes the observations of the diurnal variation made at the Observatory, under the direction of Cassini. I have deduced from them the following results:—

Mean State of the Needle between the 18*th and* 30*th of March*, 1788.

	8h. a.m.	10h. a.m.	Noon.	2h. p.m.	5h. p.m.	9h. p.m.
	35′	39′	42	42″	37′	35′
On the 24th	„	46	37	„	38	36
On the 25th	44	37	44	39	36	45

Ordinarily, the observations, made in the same fortnight, at any one hour, only present discordances of about 2′ or 3′. The observations of the 25th differ sufficiently from the mean, both in their march and in their values,—to give reason to suppose that there was on that day a disturbing force.

The magnetic phenomena support, therefore, Dr. Usher's conclusion.

I have given the observations of the 24th, for the sake of showing that the aurora which appeared at night on that day had already begun to act from the morning. The observation at eight o'clock is wanting, on account of the great oscillations which the needle was making at that part of the day.

The day aurora is described in Usher's note with great clearness. The author is, moreover, known by several other interesting Memoirs, of which I take pleasure in recognising the merits. I might, therefore, be asked, why I should have thought it necessary to seek for indirect proofs that so practised an observer was not mistaken, and that the phenomenon seen by him on the morning of the 25th of May, 1788, really was, as he announced it to be, an aurora borealis? I would reply that, as all meteorologists have remarked, it often happens that bands of very light clouds, in the upper regions of the atmosphere, are disposed in such manner as to appear to converge to a single point; presenting, therefore, an arrangement similar to that of the rays or beams described by Usher. It is true that in this particular case the point of convergence coincided with the pole of the dipping-needle. I will frankly confess that, if this circumstance did not entirely convince me, without further evidence, it is only because the same author had said, in a Memoir on Halos, that the major axis of elliptical halos is also always in a direction parallel to the magnetic needle;—a result which appears to me to be neither true nor probable.

CHAP. X.

ON MAGNETIC INFLUENCES EXERCISED ON A MAGNETISED NEEDLE.

It has been seen in the two preceding chapters, that I had not only shown, as my predecessors had done, that there are certain coincidences between the direction of the magnetic needle and the most marked phenomena of auroras, but also that I had discovered, as early as 1819, that these phenomena affected the movements of the magnetic needle. I was even able to show, in 1822, that former auroras had caused in compass needles movements which, at the time, had either escaped attention, or had remained unexplained. Inferences of such importance fixed my attention on this class of phenomena; and for more than ten years I collected with care all observations of auroras, in order to compare them with my observations of magnetic declination, inclination, and force. I thus found that the three principal phenomena of the magnetic needle were influenced by auroras, and this even when the auroras were not visible at the place where the magnetic elements were observed. The results thus obtained were, at first, contested by several physicists, according to an almost invariable law to which all discoveries are subject. It was not admitted that I had effectually solved the question, either by my own experiments, or by the detailed examination which I had made of the numerous observations of Celsius, Hiorter, Wilcke, Wargentin, Canton, Van Swinden, Cote, Cassini, and Dalton. Those who will read the catalogue of auroras observed in both hemispheres from 1819, drawn up by me from my private correspondence, and from the examination of different scientific journals—taking that catalogue in conjunction with the record of the march of the declination needle at Paris at the same time,—will be satisfied that the opinion which I adopted as early as 1817 was thus shown to be well-founded, not only as regards auroras seen at Paris, but also as regards auroras which never appeared above the horizon of that place.

Some, who did me the honour of occupying themselves with the above opinion, preferred trusting to recollections of verbal communications made by me to the Academy on these important phenomena, rather than to the notes published by me from time to time in the "Annales de Chimie et de Physique." I found among my adversaries an illustrious physicist who has since become my friend. Sir David Brewster, who is a Foreign Member of the Academy of Sciences, (the highest distinction which a scientific man can desire,) will forgive me this recollection, which I cannot efface from the history of science.

However, the various remarks, epigrams, and witticisms (for all such were pressed into the service, even to a comparison bringing in the name of the battle of Navarino), were not directed altogether against what I really did state. Expressions which I did not use have been attributed to me, and printed in italics. It matters little that I have published precisely the contrary of the phrases for which I am blamed; the italics fasten them peremptorily on me.* It would be no doubt to be regretted if such principles of literary criticism should make many proselytes; but passion looks less closely, and permits itself to indulge in a vigorous refutation of experiments not yet published, and only imperfectly known by confidential communications from their author. Leaving these remarks for reflection, I pass without further preamble to a thorough examination of the real question.

My critic deems it strange that I should not have regularly published the Paris magnetic observations in the "Annales de Chimie et de Physique." My answer must be the axiom, that a thing contained must be smaller than that which contains it.

The "Annales" form each year three small octavo volumes, while the magnetic observations made at the Observatory in each year would fill a *thick folio volume.* It must be perfectly evident that monthly means cannot decide the question of the influence of auroras, and that for this purpose detailed observations during each day are indispensable. If, as I wish, these observations are hereafter published, it cannot, for the aforesaid geometrical reason, be in the "Annals," or in any other scientific journal: special publication by the Government is the only way by which my critic's demand could be satisfied. My crime is that of having made too many observations to be published *in extenso.*

I had thought that by giving every year, with the meteorolo-

* I am made to say that predictions of auroras deduced from the movements of the needle have *always* been exact; even italics have here been deemed insufficient, and the word "always" has been printed in very large letters. Now my critic had before him, at this very time, a Memoir in which Mr. Hansteen reproached me, on the contrary, with having admitted that all disturbances might not perhaps proceed from auroras. He might also have read my own statements, to the effect that several of my announcements had not yet been verified; and that, as soon as Parry's and Franklin's Voyages should be published, I would publicly state the results, *whatever they might be.* In the face of these documents, was it worth while to print the word "always" in large letters?

gical report, a list of the days on which the displacement of the magnetic needle had led me to entertain an expectation that an aurora would have been somewhere visible, I should stimulate persons who might have observed such phenomena to publish their remarks. My critic finds fault with these "*predictions*," that being the name which it pleases him to give to my indications; and he complains that I "monopolise" them. He says it is "the duty" of an author to publish observations without delay. There are various ways of understanding duty; and I have found myself more than once in complete disagreement with my critic on this point; I have, therefore, no difficulty in saying that I hold myself free to present my observations to the public, either in whole or in part, only at the time when I shall think them worthy of being submitted to it. As to the "monopoly" which I have assumed to myself (that of discussing my own observations), I venture to hope it will be exempted from the condemnation which is now generally pronounced on whatever bears that name. Even now, after a much occupied career, I do not find time to publish my works, and am obliged to confide the care of doing so to friendly hands. For the rest, if those who have criticised my "predictions" should desire to compete with me in this respect, I will send them with pleasure the three talismans which I employed, viz., a thread of raw silk, a magnetised needle, and a microscope; to which I would add my best wishes for health, zeal, and a large store of patience.

When I first learnt the jealousy entertained of my terrible monopoly of the prediction of auroras, I own to having felt a slight movement of vanity,—unluckily it was of very short duration. The most energetic of my critics declares that my predictions are false, and proceeds to prove it in two ways. 1st, by citing observations of auroras, the appearance of which cannot be made to agree with the march of the needle at Paris; and 2ndly, by pointing out unfulfilled predictions.

The aurora which he considers irreconcilable with the march of the needle at Paris, is that of the 17th of August, 1825. It was observed at Leith at ten in the evening. At 10 P.M. my horizontal needle presented nothing extraordinary, but as it had been notably disturbed in the morning, I had thought that the luminous streamers observed in Scotland at night were the latter part of an aurora occurring in the daytime. I should need to quote a whole page of the critic's writings to show how much contempt this hypothesis excites in his mind. He presents it as a sample of M. Arago's mode of reasoning; and as an instructive example of the distrust with which the views of makers of theories generally ought to be regarded. I am sure he had some sentiments of compassion for the confusion with which he was overwhelming me, although they did not prevent him from giving me a finishing blow, by exclaiming,—"If the aurora at 10 P.M. was the continuation of a day aurora, why was not it seen at Leith between seven o'clock and ten? At all events, why did it not disturb the needle on the evening of the 17th of August?"

On the first point, may I be permitted to reply, that at Leith at 7 P.M., on the 17th of August, the sun has not yet set, and that sunset is there followed by twilight quite strong enough to mask the beams of an ordinary aurora for some considerable time longer; that, moreover, there is no proof that the sky was clear in the northern quarter previous to the observation; and, lastly, that it is possible that the Leith meteorologist may only have put his head out of the window at ten o'clock; for if I am not mistaken, he says, "I saw an aurora borealis at ten," not "an aurora began to appear at ten." Further, is it necessary for a native of the South of France to tell a Scotchman, born and bred amidst the Northern Lights, that an aurora has not always the same degree of brightness throughout the whole period of its apparition; that sometimes it fades so much as to be barely visible for some hours, and then suddenly brightens afresh? I shall wait until it shall be proved to me that none of these circumstances took place on the 17th of August, before I agree to the justice of the reproof addressed to my "mode of reasoning."

The critic flatters himself that he has "entirely overturned" my conclusions by the remark that at 10 P.M. on the 17th of August, while the aurora was visible at Leith, the needle at Paris occupied its usual place; but this is to combat a phantom. I have said, and I maintain, that a considerable aurora is always, or almost always, attended by an irregular deviation of the horizontal needle at Paris; but I never maintained that this needle is out of its usual place during the whole time for which the aurora lasts. As the attendant perturbations of the needle are sometimes to the east and sometimes to the west, during the same aurora, it is, on the contrary, evident that in changing from the one to the other, the needle must pass through its usual position; and an observer who should only see it then might not suspect the existence of a disturbing cause. Will my critic do me the kindness of saying whether this simple remark does not "overturn" his crushing objection?

Now to pass to predictions which have not been realised: I had announced that, judging by the indications of my needle, auroras should have been seen in the north on the night of the 21st of August, 1825—on the morning of the 22nd—during the night of the 26th—and particularly during the night of the 29th. The critic, having consulted the observer at Leith, declared that on the 21st of August, although the weather was fine, and especially on the 26th, there was no aurora. On the 29th, the sky was not favourable; but the other predictions have been falsified by the event. What, then, in presence of these facts, becomes of the sweeping conclusions of M. Arago?

These conclusions will have little to fear when I shall have rectified an error of translation (an unintentional one, I am sure), in my critic's memoir. I had believed, I cannot now tell on what grounds, that the sky had been clouded at Leith on the 21st, 22nd, 26th, and 29th of August; I did not therefore hope that any auroras could have been observed there; and so I had said, "If, with clear weather, observers placed more to the north (*i. e., north of Leith*) have not seen any auroras, on the night of the 29th of August, for example, I shall be forced to admit that there exist other causes, still unknown, which exercise a considerable influence on the march of the magnetic needle."

My critic makes me say, "If the sky was not clouded *at Leith*, and if *the observers there* have not seen any auroras," &c.

It is fair controversy to confound authors by their own words; but strict justice requires that, then more than ever, those words should be reported with exactness. If, as I have erroneously been made to appear to do, I had placed myself at the discretion of the Leith meteorologist, I should now have had nothing to say, since he declares that no aurora was seen either on the 21st, 22nd, or 26th of August; but my appeal had been made to all observers in the north, whoever they might be; and they have answered,—for by the mouth of Mr. Hansteen, my critic's particular friend, they have declared that they saw auroras in the latter part of August. The celebrated Professor of Christiania even thinks that he may affirm that the phenomenon showed itself on the 21st, 22nd, 23rd, and 26th of August; after this, what is it to me that nothing was seen at Leith? I need not enumerate all the causes which may have brought about this negative result; sufficient for me is the positive observation of Mr. Hansteen's correspondent; it proves that in the latter part of August, 1825, my needle spoke truth. Now that my credit as a prophet is thus restored, I will venture on a new prediction, which I am sure will be realised; it is, that my critic will abstain from communicating my categorical answers to the readers of his journal.

After concluding the criticism on my observations, the author, on passing to those of Parry and Foster, exclaims:— "We now arrive at a period of *sound inquiry*, at a period when the magnetic needle and the aurora borealis were observed at the same time, on the same horizon, and by men who had no hypothesis to support," &c. &c.

I abstain from any remark on the two words italicised above: as my critic thought my inquiries unsound he was right in saying so, though he would have done better to have proved it; some surprise may, however, be excited by them, viewed in conjunction with the opening passage in his article, in which he *promised a candid and moderate discussion*; but, as said the poet:—

"Chassez le naturel, il revient au galop."

The inclination needle deserves no less attention than the horizontal needle, but, owing to its much more imperfect mode of suspension, no one has yet succeeded in making out very distinctly whether its position is also subject to diurnal variation. I have thought such an inquiry deserving of fresh attempts, and after many failures I have at last succeeded in determining the daily changes of the inclination, not only by monthly means, but also by the observations of each day. I have thus been enabled to recognise, that the diurnal variation of the magnetic force, determined by the vibrations of a horizontal needle, does not depend entirely upon the changes which the inclination undergoes; that these latter changes would require to be greater than direct observation gives them, if they had to account for the whole; and that we may thus infer that the total force of terrestrial magnetism itself undergoes approximately regular variations every twenty-four hours.

This is, in brief, the result of the analysis of my investigation, which is composed of more than eighty thousand observations. Whenever I have been obliged to be absent, several of my friends have been so kind as to supply my place. I should wish to express to them here my gratitude, but ought I not to wait, in order to do so, until my critic shall have owned that he judged hastily? For my own part, I have no hesitation in declaring, though perhaps it may not be quite polite to do so, that his cutting decisions have neither grazed my self-love, nor in the least weakened my convictions: for the present, therefore, I take to myself the whole responsibility, whether of the inferences which the observations have appeared to me to justify, or of what he deems the unsound direction which I have given to the whole inquiry. After this short digression, I return to the discussion of the memoir of my celebrated critic.

It would seem as if he had had some vague misgivings as to the strength of the objections which I have been combating, for at the close of his memoir he has recourse to authority to cut short the question between us. He speaks as if he deemed that no physicists of any country are any longer free to admit any influence of auroras on magnetic needles, since the Royal Society has awarded the Copley Medal for 1827 to Captain Foster, and especially since Mr. Davies Gilbert, successor to Sir Humphry Davy in the Presidency of the Royal Society, has classed among the results of the skilful observer and navigator who has been just mentioned, the refutation "of a pretended connection between the agitations of the magnetic needle and auroras." (See Anniversary Address in 1828.)

No one has for the Royal Society a more sincere admiration than that which I have always publicly professed. I had imbibed this sentiment, from the perusal of the "Philosophical Transactions," long before that illustrious body had done me the honour of enrolling me among its foreign members. In spontaneously granting me in 1825 the Copley Medal, and in thus calling the attention of physicists to the discoveries in relation to magnetism which I had just made, the Society imposed upon me a highly pleasing duty, that of the most lively gratitude. I might suppose that my critic had reckoned on my thus feeling myself placed in an embarrassing position, and that if I repelled his arguments, I should still feel myself called upon to subscribe to the decisions of a Society who had just bestowed upon me such marks of kindness; but in case such an idea should have been formed, I hasten to say it would be a complete error. I should think myself truly unworthy of the favours I received, if, in a question of science, I allowed any weight whatsoever to personal considerations; if I looked to the source from whence arguments proceed, rather than to their force; and especially if I gave way to decisions destitute of proof. It might have been rather remembered, that, in becoming a Fellow of the Royal Society I should adopt its motto, — *Nullius in verba.* I now proceed unreservedly to that part of the memoir in which my learned critic thought his position rendered unassailable by the imposing authorities arranged on his side.

In my opinion the Royal Society only performed an act of entire justice by awarding their medal to Captain Foster's Memoir. The multitude of observations obtained by that indefatigable officer, the difficulties amidst which they were made, the small distance which divided the several stations from the magnetic pole, all render his work one of the most valuable acquisitions with which science has been enriched for a long time. No one, I think, can desire from me a more frank or full declaration than this: now let us see how far it has compromised my cause.

I will suppose for a moment that, as Captain Foster announces, auroras have no effect on the magnetic needle at Port Bowen. No doubt this would be a very curious fact; but what could be concluded from it as against our observations at Paris? Because thunder is unheard near the Pole shall we infer that it is never heard in France? My critic will be shocked at this comparison; but I am sure that on reflection he will see that I have only made it because it points directly to the fallacy of his argument, — because it shows that a meteorological fact may be only true in the place where it has presented itself. I fancy, however, that I hear him accuse me of having forgotten that Captains Parry and Foster "lived among the very beams of the northern lights," and how can we admit that an aurora acts at a distance when we find it produce no effect near at hand? I answer, that we are ignorant of the manner in which the action in question is exercised; — that it is not impossible that the amount of the inclination may have an important share in it; — and that where the resultant of the terrestrial magnetic force is almost vertical, the perturbing force may become insensible, especially if, as at Port Bowen, the aurora has some tendency to appear simultaneously on all points of the horizon. To generalise under such circumstances, and to apply to the 49th degree of latitude what was observed under the 73rd is evidently to build on the sand.

But I will now go farther, and I believe that I shall establish without much difficulty that Foster's observations do not prove that in the northern regions, in which he observed, the magnetic needle during auroras is less disturbed than at Paris.

At Paris accidental changes in the direction of the needle of 3′ or 4′ of a degree strike the least attentive observer; perturbations of 10′, 15′, or 20′, appear to him enormous, and they really are so when viewed relatively to the ordinary deviations. It is otherwise at Port Bowen, where the declinations observed on one day and those observed at the same hours on the following are ordinarily very different. I will cite an example, taken indiscriminately: —

	Time. h. m.	Position of Needle.
22nd of January	1 1	0° 31′ West.
23rd of „	1 10	1 26 „
24th of „	1 8	0 10 „
25th of „	1 3	0 40 „
26th of „	1 9	1 6 „
27th of „	1 2	0 52 „
28th of „	1 7	0 19 „
29th of „	1 4	0 20 East.
30th of „	1 21	0 2 West.

Among numbers so discordant, what is to be adopted as the regular march of the needle? When from day to day the partial results habitually differ from each other more than half a degree, how should accidental anomalies due to auroras be distinguished, if, as at Paris, they only rise to 10′, 12′, or 15′? Hence it appears to me evident, that if Parry and Foster believed the aurora to produce no effect on the needle in the arctic regions, it is only because they had supposed that such influence would be much more considerable than it really is. Port Bowen is a station but little favourable for this kind of research. My critic would thus only have left to him as authority the decision of the Royal Society as contained in the phrase which I quoted; now I have consulted on this subject one of the most distinguished members of the Committee, and have thus learnt that nothing was decided, or even discussed, as to the action of auroras. The inferences deduced by Foster from his observations rest therefore on his sole authority. The Royal Society neither admitted nor rejected them. The words used in the discourse of the President may appear a little too positive, but they must be taken—and in this way all is explained—simply as the enunciation of an opinion held by the author to whose work the medal was awarded; now I have already pointed out the circumstances which naturally led Foster, notwithstanding his distinguished merit, to draw conclusions which his tables did not really sufficiently warrant.

The above remarks were written during an excursion in Switzerland, fragmentarily, in the course of reading the memoir of the learned physicist of Edinburgh. I own that I attached some importance to the elucidation of a curious question, in regard to which it seems to me that it was attempted to make Science retrograde, and lose a step which she had gained. Afterwards, however, I was led to think for a moment of suppressing my reply, when, on arriving at certain passages which I will now consider, it seemed to me evident that my critic (by a misadventure not unfrequent with critics) had not the slightest idea of the nature of the phenomena to which this discussion relates. The passage I allude to is the following:—

"Lieutenant Foster has given an extract of the diurnal variation as observed with one of his needles for the months of January, February, March, April, and May, 1825. In one column there is given the value of the diurnal variation, and in another all the auroras which were seen are noted; so that we can compare the mean oscillation of the needle corresponding to the periods when there was no aurora, with that of a period during which many auroras were visible (*and thus obtain the united effects of groups of these motions*).*

"The following table will place this in a clear light:—

	Number of Auroras seen.	Mean Value of Diurnal Variation.	Means.
January	14	1° 37′ 30″	1° 37′ 15″
February	14	1 38 0	
March	2	2 14 30	2 14 30
April	0	2 52 44	3 18 42
May	0	3 44 39	

* The few words underlined are given by me in English because I am not sure that I rightly apprehend their meaning; they are not, however, material to the general sense.

"These comparisons present a very curious result, which Foster does not seem to have perceived. *Instead of a disturbing influence, the aurora, in the Arctic regions, appears to exercise upon the magnetic needle a sedative or tranquillising influence.*" *

I really do not know where to begin in the enumeration of the singular ideas (by politeness I blot out the word blunders) which my critic has contrived to comprise in these few lines. As, however, I must make a selection, I will first ask whether, in judging of the comparative number of auroras in the months of winter and of spring by the number which were visible, the reasoning is not precisely as if one were to maintain that during daylight there are fewer stars above the horizon than at night?—for at Port Bowen the night lasts the whole twenty-four hours in January and February, while in April and May the sun scarcely sets. If I next pass to the supposed sedative influence, would not this have to be regarded as an action no less real than that of which the existence was contested? Should not the writer have perceived that in admitting such an influence, he must support it, not only against Mr. Foster, but also against the Royal Society, who, according to him, had adopted Mr. Foster's opinions? The observations attacked by the Scotch critic profess to show, that on a day of aurora the magnetic needle at certain hours in the course of the day marks declinations very different from those which are found at the same hours on days when aurora does not take place. The anomalous declinations are sometimes greater and sometimes less than the ordinary declinations. According to my critic, if I rightly comprehend his "sedative influence," the needle would be arrested in the position in which it should be found by the aurora, and on such days the diurnal variation would always be inferior in amount to that on ordinary days; but the perturbation would not be less real because always exercised in this particular manner. If this was the result to be arrived at, it was scarcely worth while to entitle the memoir intended to criticise me "On the *supposed* Influence of the Aurora Borealis."

But, in truth, the calculation made by the critic, and which, as he remarks, Captain Foster had not thought of making, has

* I beg the reader to notice that the italics here are the critic's own; apparently to mark the importance of the inference, of which, by remarking that Mr. Foster did not perceive it, he declares himself the discoverer.

no reasonable meaning,—as I proceed to attempt to show by an illustration.

Suppose I take all the thermometric observations made at Port Bowen, and deduce from them the mean diurnal variations of temperature for the months of January, February, March, and April. I range these in a vertical column, and place opposite to them the corresponding number of auroras seen in each of the same months. I then see at a glance that in January and February the diurnal variation of the thermometer was very small, and that there was an aurora one day out of two; that in March and April, on the contrary, the temperature changed a great deal in the course of the twenty-four hours, and that auroras had nearly ceased to be noted. Why might I not argue from thence, in my turn, if the critic's mode of reasoning were correct, that auroras exercise a sedative influence on the thermometer? Or if the learned Secretary of the Edinburgh Society does not like this comparison, I can easily furnish him with another. I may say, for instance, if he pleases, that at Brest, in 1825, the tides of January and February were much smaller than those of March and April, by reason of the sedative influence which the auroras, in the two first named months, exercised on the waters of the ocean. Or why may I not equally say that they have influenced the height of the barometer, or even, if need were, the distance between the sun and the earth? I hear my critic exclaim, that all this is absurd. For my part I willingly admit it to be so, but he should pause to consider whether he is not condemning himself by such a sentence?—for, in truth, what change have I made in the reasoning employed by him, except in substituting the words—variations of the thermometer, variations of the tides, variations of the barometer, variations of the distance of the sun, for the words—diurnal variations of the declination of the magnetic needle? My supposed results would deserve neither more nor less belief than the one which he has put forward as a discovery which Foster missed making, although he had brought together all its elements.

The circumstance on which the critic has based his supposed discovery does not belong exclusively either to Port Bowen or to the year 1825. It has been always and everywhere observed. Whether there are auroras or not, the mean diurnal variation of the magnetic needle is *constantly* less in the cold than in the warm months. In this respect the arctic and the temperate regions are perfectly alike. It is not even necessary to have recourse to the monthly means in order to bring out this result. In our climates the daily observations, looked at by themselves, show it clearly. Auroras, which are an accidental phenomenon, have no other influence on this general march than to disturb it occasionally; but as they cause the needle to deviate sometimes on one side and sometimes on the other, scarcely any trace of their action remains in the monthly means. These considerations belong to the first elements of the question, as systematic works on physics bear witness. By what singular circumstance has the learned critic remained ignorant of them? How, at any rate, has he failed to remark in his own table that the diurnal variations in the months of April and May differed much from each other, although his "sedative" power did not show itself in either of those months? How, above all, did he fail to perceive that a phenomenon of daily occurrence could not be attributed to a variable and accidental cause? I could, perhaps, have understood it if the comparison had been made between the fourteen days of January on which the aurora was seen, and the seventeen days in the same month on which it was not seen; but not so a comparison between the months of January and April.

I must now say that I did not long continue to be the only observer of the influence of auroras on magnetic needles.

In 1824, in his voyage to Hudson's Bay, in the "Griper," Captain Lyon remarked that the needles of his ship's compasses were violently agitated during auroras, even when their directive property had almost entirely ceased. (*Brief Narrative*, p. 167.)

My discovery on the influence of auroras has not only been contested; but, as is also often the lot of discoveries, it has been sought to deprive me of a part of whatever merit it might possess. This happened after a note had been published by me in 1825, in the "Annales de Physique," on the influence which different auroras observed in the north of Scotland had exercised on the position of the magnetic needle at Paris. M. Hansteen did me the honour of submitting this note to a critical examination, of which I will give the leading particulars.

The meritorious physicist of Christiania says, first, that my remark on the influence exercised by auroras in places where they are not seen "is not entirely new;" he thinks, however, that it has a "great interest," because it shows that this meteor, differing very much in this respect from rain, thunder, &c. &c., is not the result of an action exercised only over a small extent of atmosphere, but is indeed the effect of a derangement of equilibrium in the whole system of the magnetic forces of the globe.

In order to prove that my remark is not entirely new, the author then goes on to cite observations made on the 5th of April, 1741, by Celsius at Upsala, and by Graham in London. Celsius saw on that day an aurora borealis, during which his horizontal needle was disturbed: Graham observed a similar disturbance of his needle in London, but does not mention any aurora.*

* When I announced in 1825 that auroras act on the magnetic needle, even in places where they are not seen, I abstained from affirming any thing as to the novelty of this observation, although I had not found it in the numerous memoirs which I had consulted.

On reading the first part of Mr. Hansteen's Memoir, I found reason to rejoice that I had not laid claim to any novelty, for I own that at first I attached no importance to the words "*entirely* new" which I regarded simply as an expression of politeness; it being clear that an assertion, such as I had published, cannot be *half new;* I therefore resolved on giving up any kind of pretension to a new discovery, and on rendering entire justice to the first author of the remark, and thenceforward to present my own observations only as confirmatory. When, however, I afterwards came to examine the proofs adduced by M. Hansteen, I felt that I had not invaded the rights of any one, and that there was no ground for any reparation on my part. I regard Mr. Graham's observation in London as unimportant, because he does not say whether an aurora was or was not visible, and there is no evidence that he sought to assure himself on this head; and also, because everything indicates that he was not aware of the connection of this phenomenon with the movements of the magnetic needle. Will not M. Hansteen permit me to add a reason why I think that, even if Mr. Graham's note had contained the particulars which are wanting in it, still no legitimate deduction could have been drawn therefrom relatively to the action which, according to me, is exercised by invisible auroras. It is well established by a multitude of observations, that an aurora seen at a given place often leaves the magnetic needle there in a state of disturbance for a considerable time after it has ceased to appear: now, since in London on the 5th of April, 1741, the needle was considerably agitated in the daytime, there is reason to believe that there was then above the horizon an aurora which the daylight alone prevented from being perceived, and of which the oscillations of the needle in the night were the consequence. This conjecture will appear the more natural, when it is remembered that, at Stockholm itself, Celsius only saw at night faint traces of an aurora which was drawing to its close. In order to prove incontestably the influence of invisible auroras, it is then necessary that at a given place, say Paris, "on a certain day, the sky being perfectly clear, the needle shall have been seen to follow its regular march

I had indicated the days on which the needle was notably deranged at Paris in 1827, without any aurora having been observed at Edinburgh; M. Hansteen has examined his meteorological journals to see whether, farther to the north, at Christiania, the meteor may not have shown itself, and he has found that:—

On the 13th* of March the sky was cloudy; so that aurora could not have been seen.

On the 30th and 31st, the sky was clear, and yet the journal does not record any aurora; but the window from which M. Hansteen observed the state of the sky did not look towards the north. Near Drontheim, where there is an observer, snow was falling on the 30th and 31st of March, and on the 1st of April.

On the 21st of April the sky was quite clouded at Christiania. (I had not spoken of the 21st of April, and do not know on what account it is cited.)

On the 19th and 21st of August the state of the atmosphere would not have permitted an aurora to be seen at the places where M. Hansteen's correspondents were residing.

On the 25th† of August, forty minutes after eleven, an aurora showed itself at Christiania and at Hardenger. M. Holmbæ's journal says, that the aurora was seen several times in the latter part of the month of August, but does not state the actual dates. M. Hansteen thinks it very probable that they were the 21st, 22nd, and 23rd; and adds that, if this were so, we should thenceforth not be obliged to admit, with M.

until night, and that then, and only then, it shall have become notably deranged, that the observer shall have looked scrupulously for any appearance of aurora and have found none, and that an aurora shall have been seen at some other station, situated considerably further to the north." The combination of all these circumstances has taken place so frequently during my observations, that I had full reason to feel no hesitation in submitting to physicists the result to which it conducts, and of which M. Hansteen, by means of this discussion, will have done me the service of, placing the novelty in a clearer light. If, instead of elucidating the question by arguments taken in the nature of things, I had merely wished to reply to criticism, I might have contented myself with remarking, that the learned Norwegian professor, in analysing in his great work the observations of Celsius and Graham, made no mention of the inference which, since the publication of my notice, he has deduced from them.

* I think the 19th must have been meant. I had said nothing of the 13th in my notice in the Annales.

† Should the date not be the 26th of August?

Arago, "that there exist unknown causes (other than auroras), which act on the magnetic needle."*

10th of September. Very brilliant aurora at Christiania.

7th of October. Cloudy sky.

3rd or 4th of November. Aurora at Bergen.

22nd of November. Sky clear at Christiania; yet the Meteorological Journal does not mention any aurora. (An aurora was seen at Leith.)

On the 26th of August, M. Hansteen being near Bornéo, in Lapland, his horizontal needle, at a quarter before ten in the evening, made 300 vibrations in 887 seconds; whereas it usually took only 881 seconds to make that number of vibrations.

"This irregularity," says M. Hansteen, "having coincided with the variation in the declination observed by M. Arago in Paris, shows that the influences of auroras embrace very large extents of country, and that disturbances of the magnetic force take place as well as disturbances of the declination." †

CHAP. XI.

ACTION EXERCISED BY EARTHQUAKES ON THE MAGNETIC NEEDLE.

Although I consider that I have fully demonstrated the influence exercised by auroras on the magnetic needle, I have

* If I should suppose the translation of M. Hansteen's Memoir to have been correctly printed in the English Journal in which I found it, I would remark that my proposition was different from this. I said, "The march of the needle on the 29th of August, 1825, having presented very great anomalies at Paris, if, with serene weather, observers in the North have not seen any aurora, it will be necessary to admit that there exist other causes, still unknown, which also exercise a considerable influence on the march of the magnetic needle." I did not say that I admitted such causes; I only showed under what circumstances it would become necessary for physicists to have recourse to them.

† This result is quite correct; but M. Hansteen's observations do not sufficiently prove it to be so. The inclination needle, as before stated, changes its direction during auroras as well as the declination needle, and hence the time of vibration of horizontal needles would vary: even though the intensity of the magnetic force should remain constant. It is only after correcting the time of vibration for the effect produced by changes of inclination that the intensities corresponding to different hours and different days can be deduced.

not concluded, from my researches, that all irregular variations which might be presented by a compass needle would be due to the appearance of an aurora in some part of the globe. Far otherwise. I have shown that earthquakes occasioned special oscillations of the needle.

The newspapers announced that a considerable earthquake had been felt on the 19th of February, 1822, in Auvergne, at Lyons, and in Switzerland. The shock extended to Paris, where it was felt at a quarter before nine in the morning (apparent time), or a few minutes earlier, and its direction very nearly coincided with that of the magnetic meridian.

I subjoin an extract from the register of the observations of the diurnal variation of the declination needle for the 19th of February, 1822:—

"At eight in the morning the needle appeared perfectly tranquil, even when viewed through the microscope."

"At a quarter after eight, all the circumstances were quite similar; the north end of the needle approached only a few seconds nearer to the geographical meridian.

"Half-past eight. Needle still very tranquil: the march of the north end towards the geographical meridian has ceased; the needle is now at its minimum of declination.

"A quarter to nine. There was not, properly speaking, any observation at this hour, that is to say, no reading of the scale indicating the position of the needle, but I had written in the register as follows:—'The needle is very much agitated. I would even say that nothing similar has ever presented itself before since we have begun to observe the diurnal variation. The movements are so great, that the microscope is unnecessary for observing them; they are seen perfectly well with the naked eye. The circumstance which renders the derangement remarkable is, that the oscillations of the needle are exclusively in the direction of its length.' I can see nothing but an earthquake which can be supposed to have given occasion to a movement of this kind; and even this would require to have been exactly in the direction of the magnetic meridian, that is to say, in a line forming with the geographical meridian an angle of $22\frac{1}{4}°$.

"Nine o'clock. Needle very tranquil. The north point has as yet only retrograded 6″ towards the west.

"Half-past nine. Needle tranquil. The movement to the west continues as usual, gradually, and without shocks.

"The direction in which the oscillations took place at a quarter before nine allowed it to be recognised that the axis of the needle was then in a position exactly intermediate between the two declinations (which, moreover, differed very little from each other) observed at half-past eight and at nine. If electricity, as is generally believed, is concerned in earthquakes, we see that, in the one of this day, at least, it has produced no effect on the declination of the magnetic needle."

I wrote this note at the moment when the great movements of the needle were taking place. Having afterwards learnt that the earthquake shock had been sufficiently strong at Paris to have been felt by persons in their beds, I thought it would be curious to see whether the march of the sidereal clock of the Observatory might not have been affected by it. But the following table shows that the earthquake had absolutely no effect in this respect. The vibrations of the pendulum of the clock are performed in the plane of the meridian.

Daily Rate of the Sidereal Clock of the Observatory.

	m.	s.	
From the 15th to the 16th of February	0	48	gaining.
From the 16th to the 17th of "	0	50	"
From the 17th to the 18th of "	0	45	"
From the 18th to the 19th of "	0	40	"
From the 19th to the 20th of "	0	47	"
From the 21st to the 22nd of "	0	40	"

Mr. Gay sent to me from Valdivia, on the west coast of South America, some details concerning a perturbation of the magnetic needle at the time of the terrible earthquake of February, 1836. The needle was not again disturbed during the numerous shocks—very small, it is true—which have been felt since. Mr. Gay has made observations for a whole year on the diurnal variation of the declination. According to him, that phenomenon has not quite the same march there as in Europe. He says:—"Instead of two daily movements, backwards and forwards, I have always found three: one in the morning, to the east; one in the middle of the day, towards the west; and the other in the evening, again to the east,—this last movement being the complement of the morning one.* The hours of the maxima and the minima differ a little according to seasons, but anomalies are so rare, that I regard the triple movement as permanent in these countries. Can the great chain of the Cordilleras be the principal cause of this constant irregularity? I cannot believe it to be so; but I reckon on ascertaining the truth in this respect in a journey which I expect to make to Mendoza."

CHAP. XII.

AURORA AUSTRALIS.

FORSTER says in his work, that prior to Cook and himself no one had spoken of southern lights, austral auroras; they observed the first one seen by them in 1773, between 58° and 60° of south latitude.

The dates of all their observations of this kind are, 18th, 19th, 20th, 21st, and 26th February, and 15th and 16th March, 1773.

It is now pretty well established that there are as many polar auroras in the southern as in the northern parts of the globe. Every thing gives reason to suppose that the austral auroras, and those which we see in Europe, follow the same laws. This, however, is but conjecture. Therefore when voyagers see an austral aurora in the form of an arch, it is very important that they should note exactly the azimuths of the points of intersection of the arch with the horizon, or if these cannot be observed, that of the culminating or highest point. In Europe this highest point appears to be always in the magnetic meridian of the place of the observer.†

* [The translation is correctly rendered from the French. The diurnal variation which is intended to be described is not very clearly pointed out.] —ED.

† [This generalisation would appear to require modification if it be intended to include countries situated in the north of Europe, such, for example, as Norway. The Proceedings of the Royal Society of May 21. 1840, contain a Report from the Committee of Physics to the Council, communicated by the Council to the Society, on the meteorological observations made by Mr. Thomas, at Alten, in Finmorken, in lat. 69° 58′, in the winters of 1837-38, and 1838-39, from which the following is an extract:—

"The phenomena of the aurora borealis appear to have been observed by

Numerous examinations cited in this volume have proved that northern lights not visible above the horizon of Paris, and of which the existence has only been made known by the narratives of voyages in the polar regions, have strongly influenced the declination, inclination, and intensity shown by magnetic needles at our observatory. Who, therefore, could venture to argue, from the great distance of the southern auroras, that they may not also exercise a disturbing influence on magnetism in our hemisphere? At all events, the care taken by voyagers to keep exact records of these phenomena will enable some light to be thrown on the question.

It would also be desirable that in observatories the observations should be sufficiently near to each other for no disturbance to pass unnoticed.

If my memory does not deceive me, among the meteorologists who have already collected a good number of descriptions of auroras in the southern hemisphere, no one before M. Lafond had seen them to the north of the zenith in the low latitude of 45° S. Without wishing at present to attach undue import-

Mr. Thomas with great assiduity, and recorded with great care. On examining the register, with reference to M. Erman's important remark, that 'in Siberia two kinds of aurora are distinguished, one having its centre in the west, and the other in the east, the latter being the more brilliant,' it is found that twenty-two nights occur in the course of the two winters in which the formation of arches of the aurora is noticed and their direction recorded; of these, *ten* are to the *west*, having their centres rather to the southward of west, the arches extending from N.W. to S.S.E. and S.E.; *seven* are to the *east*, or more precisely to the southward of east, the arches extending from N.E. to S.E. and S.W. Of the five others, *four* are said to be from east to west across the zenith, and cannot, therefore, be classed with either of the preceding, and *one* is noticed generally as being to the north. The facts here recorded appear to afford an evidence of the same nature as those mentioned by M. Erman, as far as regards there being two centres of the phenomena. In respect to the relative brilliancy of the eastern and western auroras, nothing very decided can be inferred from the register. If, as M. Erman supposes, they may be referred respectively to 'les deux foyers magnétiques de l'hemisphère boréal,' it is proper to notice that the position of Alten is nearly midway between those localities."

The magnetic declination at Alten was about 11° west.

It happened to the editor of this translation, when at Lerwick, in the Shetland Islands, in the year 1818, to see two auroral arches existing at the same time, one having its centre or culminating point in the north-west, and the other in the north-east; a fact which convinced him at that early period that auroral arches have not *always* their culminating points in the magnetic meridian of the place of the observer.]—Ed.

ance to the remark, I will add, that at the time of M. Lafond's observations the horizontal needle with which the diurnal variations were followed at the Paris observatory had a very irregular march. I now subjoin the account sent to me by that navigator.

"On the 14th of March, 1831, being in 45° S. lat., and about the meridian of the middle of New Holland, we saw an aurora australis. The auroras seen in the northern hemisphere being called boreal, it seems natural to call those seen in the southern hemisphere austral.

"On the above date (14th January), and in the above-named position of the ship, the sun had set at half-past seven, but up to nine o'clock it was not dark, and even long after that hour, there seemed to be much light about the south horizon and a few degrees above it. Half an hour after midnight luminous streamers appeared in the north-east. They began 30° above the horizon, and were directed towards our zenith. At one o'clock these streamers became much brighter and more luminous, and extended more towards the north. At two o'clock they were at their greatest brightness, and embraced the whole portion of the heavens comprised between the N. N. E. and N. W. by compass, from 20° above the horizon to 10° or 15° beyond our zenith.

"The weather was fine, the sky clear of clouds, and the wind fresh from the S. W.

"The rays or streamers which we saw were formed by a mist, or by uniform and rather opaque clouds; the light was more vivid and strong in the places where the mist seemed thickest; there it had a dark rose colour, which melted in the intervals into white and pale yellow.

"From time to time the beams or streamers wavered or flickered, and one could fancy at such times that one heard a brushing sound, which, however, was nothing but the effect of the sight of this movement on the imagination. At other moments the streamers moved more slowly, and resembled the undulations of a deep sea. To convey a just idea of the spectacle by a comparison, which, though true, may seem but little worthy of so grand and majestic an effect, imagine a vase filled with water placed in a court-yard inclosed by high walls; if, on a fine day, the sun lights up the part of the yard in which the vase stands, the water will reflect the sun's image on the wall which is in shadow. If you shake the vase the liquid contained in it being set in motion will reflect the sun's rays successively in all directions.

"The light from the auroral beams was sufficient to enable rather small print to be read with facility. To convince myself on this point, I had an octavo volume of Firmin Didot's brought to me, and my officers and myself repeatedly passed the book to each other, and all read some lines from it without difficulty.

"At three in the morning the luminous beams gradually disappeared, and were replaced by the light of the dawning day, which already began to appear in the whole of the E. S. E. quarter.

"On the 15th and 16th we saw the same auroral phenomena, but less bright, and of less duration."

CHAP. XIII.

ON AN ARRANGEMENT OF CLOUDS SIMILAR TO THAT OF THE LUMINOUS BEAMS OF AURORAS.

On Sunday, 24th June, 1844, towards half-past eight in the evening, the sky being entirely overcast, there was seen at Paris, towards the south, an apparently circular arch, appearing on an almost uniform stratum of clouds; it was dark, regular, and very extensive, yet did not continue down to the horizon, either to the east or to the west. This arch became blacker and blacker, and more and more defined. A whitish arch was soon formed along the internal border of the dark one, but not throughout its whole extent.

Both above and below this appearance the clouds seemed to be in a state of singular agitation.

The two arches, the white and the black, keeping always contiguous to each other, rose gradually higher above the horizon; towards nine o'clock they reached the zenith, having previously become much fainter; after which they disappeared.

The culminating point of the arch appeared to be in a vertical plane forming an angle of about 20° with the meridian towards the east. As the phenomenon seemed thus to have something of a magnetic character, M. Laugier, from the time when he perceived it, observed the needle for the diurnal variations from minute to minute, but found it undisturbed.

There were seen on different points in the atmosphere traces of polarised light, which evidently did not proceed from moonlight. It would remain to be examined whether they might not be caused by crepuscular light, or twilight.

I ought here to remark that observations made in high latitudes have often shown that clouds assume the forms and position of auroras.

CHAP. XIV.

UNCERTAINTY IN RESPECT TO THE POLARISATION OF THE LIGHT OF AURORAS.

On directing the polariscope, described by me in 1815, on the light of auroras, I have found traces of polarisation. But this simple observation did not authorise me to pronounce that the mysterious phenomenon of which we are treating shows itself to our eyes by reflected light. M. Baudrimont, indeed, thought that he might draw such a conclusion from the observation which he obtained of the light of an aurora borealis visible at Paris on the 22nd of October, 1839, at a quarter past ten at night; but in order to justify this conclusion it would have been necessary to ascertain that the rays observed in my polariscope, or of the striæ in the Savart polariscope described by M. Baudrimont, were not caused by the inevitable admixture of the moonbeams reflected from the molecules of the terrestrial atmosphere, and therefore polarised. It would also have been necessary to take into account the effects which proceed from the multiplied inflections which the beams of the aurora itself may experience in the atmosphere. An exact determination of the direction and apparent intensity of the polarisation in different azimuths might have solved the difficulty, but time did not admit of this. Observations will always be more decisive if not made during moonlight. This inquiry was particularly recommended by the Academy to observers in the scientific expeditions to the north.

I remarked on some passages in M. Baudrimont's communication to the Academy, which were irreconcilable with the laws of the polarisation of light; as, for example, where there seemed to be questions of a polarisation in three planes. I supposed, however, that the confusion was only apparent, arising from a want of clearness in the expression.

M. Baudrimont has complained of my remarks, and has said that his statement was positive to the effect that the light was polarised in three planes which intersected each other in a single point, adding, "I care little for this not being in accordance with the known laws," &c.

Having on my part, also, observed the phenomenon attentively, I could not do otherwise than remark that the light analysed was in truth a compound of the light of the aurora, and of the partially polarised light, arriving at the eye at the same time from the regions of the atmosphere illuminated by the moon, and interposed between the aurora and the observer. The deduction which was drawn from the observations as they were given,—to the effect that the auroral light was polarised,—is not, therefore, authorised by them, since it is quite possible that the polarised atmospheric light might be the sole cause of the phenomenon observed. If an observer were to tell me that on a day when the sky was nearly clear the light of the few detached clouds was polarised, I should ask him, in the same way, how he could distinguish this light from that of the strata of the atmosphere intervening between the cloud and the eye.

M. Baudrimont, on seeing, during the movement of rotation of the polariscope, two series of bands, which, if they existed simultaneously, would intersect each other at right angles, believed that this showed polarisation in two rectangular planes. I must, therefore, tell him that the rays polarised in a single plane present precisely the same phenomenon. This is one of the elementary principles of optics, in regard to which no person is entitled to say, "I care little."

CHAP. XV.

ON THE USEFULNESS OF CATALOGUES OF AURORAS.

MAIRAN has proved that auroras are not always equally frequent, and that there are sometimes long intervals during which none are seen, not only in the temperate zone, but also in Sweden and Norway. According to the same author, these phenomena are at least twice as frequent when the earth is in its perihelion as when it is most distant from the sun. It may, at some time, be interesting to examine whether there is any connection between the cessations and renewals of auroras and of other natural phenomena; and for the sake of facilitating such researches, we have undertaken to draw up a catalogue of such appearances of the aurora borealis as shall have come to our knowledge; using for this purpose the accounts of travellers and the intelligence given in scientific journals, and comparing them with the observations of the magnetic needle at Paris.

[M. Arago has published a part of his catalogue in the "Annales de Chimie et de Physique;" but the larger portion of his remarks and observations is only to be found in the registers of the diurnal variations of the needle, from whence we have faithfully extracted them.]—*French Editors.*

CHAP. XVI.

CATALOGUE OF APPEARANCES OF THE AURORA BOREALIS FROM 1818 TO 1848.

§ 1. 1818.

THE only aurora borealis mentioned in the scientific journals of this year was observed on the 31st of October, between 7 and 8 P.M., at Bishopwearmouth, in Sunderland, by Mr. Renney. In itself it presented nothing extraordinary, but it exercised on the magnetic needle in London, Paris, and at the Duke of Ragusa's château at Châtillon sur Seine, a remarkable influence, which has been spoken of more fully in Chapter VIII.

§ 2. 1819.

February 1.; 30 minutes after midnight. Although the sky was clouded, there were seen between the clouds, in the northern quarter, bright white patches, evidently announcing the existence of aurora. The oscillations of the needle were as great as 10′ 36″.

[Our observations of the diurnal variations of the needle having been interrupted from this period to February, 1820, we can only cite, for that interval, from other sources.]

October 15., aurora observed in Suffolk.

October 17. About 8 h. 50 m. P.M., a rather brilliant aurora was observed at Seathwaite in Cumberland; and in the vicinity of London. This aurora augmented the magnetic declination on the morning of October 17. about fifteen minutes.

October 17. About 8 h. 50 m. P.M., there was observed, at Newton Stewart, in England, a luminous phenomenon, which, from the description, must have been an aurora.

I find in Colonel Beaufoy's published observations, that on the same day the needle used for observing the diurnal variation in England was at a considerable distance from its usual position.

The following is the catalogue of auroras observed during Captain Parry's voyage*:—

20th of October, between six and eight, P.M. Aurora borealis forming a broad arch of irregular white light, extending from N.N.W. to S.S.E., the centre of the arch being 10° to the west of the zenith. It was most bright near the southern horizon.

12th of November. At six P.M. the aurora was seen in a broken, irregular arch, about 6° high in the centre, extending from N.W. by N. to S. by W.

13th of November, from eight P.M. till midnight, it was

* [The appearances of the aurora which are here cited from Captain Parry's Narrative were all observed during the winter passed at Melville Island. The mean declination of the magnetic needle at Winter Harbour was 128° 48′ east of north. Were it the fact that the centre or culminating point of auroral arches is *always* in the magnetic meridian, the arches observed at Winter Harbour ought always to have had their centre or culminating point either S.E. half E., or N.W. half W. But this was by no means the case.]—Ed.

again seen in a similar manner from S.W. to S.E., the brightest part being in the centre, or due south.

15th, 16th, and 18th of November, traces of aurora.

26th of November, in the morning, some vivid coruscations of the aurora borealis were observed from S. to N.W.

14th of December, at six P.M., aurora borealis, forming two concentric arches, passing from the western horizon on each side of the zenith to within 20° of the opposite horizon. (No effect on the electrometer or the magnetic needle.)

17th of December, in the morning, stationary faint light from S.W. to W.S.W.

19th and 20th of December. On the 19th aurora appeared frequently at different hours of the day; on the 20th in the N.W., which was more to the northward than usual.

§ 3. 1820.

Continuing our catalogue from Captain Parry's Narrative, we have:—

8th of January, at half-past five P.M., broken and irregular arch, 10° or 12° high in the centre, extending from N. by W. round by W. to S.S.E.

11th of January, at eight A.M., faint coruscations of the aurora were observed to dart with inconceivable rapidity across the heavens from W.N.W to E.S.E., from horizon to horizon.

14th of January, Mr. Howard, at Stratford (in England), observed, between eleven P.M. and midnight, a brilliant aurora borealis between N.W. and N.

Captain Parry next records:—

15th of January, the only very brilliant display of aurora observed during the whole winter. When first seen, the legs of the arch were nearly north and south of each other. The arch passed a little to the east of the zenith.

3rd of February, at six P.M., faint aurora from S. to S.S.W. At Paris, variations of the needle of 2′ 39″.

8th of February, in the evening, rather bright aurora. Variations at Paris of 5′ 27″.

10th of February, at a quarter after six P.M., an arch extending from S.E. to N.W. by N. (This aurora lasted long, and was rather brilliant.) At Paris, deviation of the needle 9′ 12″.

At Paris, on the 9th of February, at eight in the evening, frequent flashes of lightning were seen in the south; in the west, thick black clouds; the rest of the sky very fine. At nine, the sky became generally overcast; lightning less frequent but more intense. At ten, lightning had ceased. In the south, west, and north, thick clouds, generally not rising more than 30° above the horizon. From half-past nine there was seen in the north-west a rather vivid light round the margin of the cloud, quite distinct from that of the milky way on the left. This light varied in intensity every instant, and disappeared at the end of a quarter of an hour. Needle much agitated. It was only at a quarter after ten that I could get any tolerably certain observation. The variations were as much as 14′ 39″.

11th of February. At half-past eight in the evening, a momentarily rather vivid aurora. At Paris, variation of the needle 19′ 57″.

19th of February. At half-past ten in the evening, rather vivid aurora. At Paris, variations 15′ 54″

8th of March. Faint aurora. At Paris, variations 23′ 51″.

3rd of April. Mr. Scoresby observed the most brilliant aurora he had ever seen in his numerous voyages. (*Greenland Voy.*, p. 17.) At Paris, variations 16′ 41″.

2nd of October. Very faint aurora. At Paris, variation 10′ 55″.

3rd of October. An aurora more brilliant than usual; light equal to that of the full moon. Captain Parry says, no action on the electrometer or magnetic needle. At Paris, variations 8′ 16″.

13th of October. Bright aurora. Arch from N.E. to W.S.W., so as to be nearly bisected by the magnetic meridian. At Paris, variations 7′ 1″.

— of November. From five to nine P.M., fine aurora observed at St. Petersburg.

This phenomenon was not seen at Paris, and I do not know its exact date; but the needle in our observatory having been considerably out of its ordinary position during the whole of the 14th of November, I am much inclined to suppose that this was the day on which the aurora was seen at St. Petersburg. The variations of the declination at Paris were as much as 23′ 33″.

Mr. Forster saw an aurora in England in the night from the 4th to the 5th of December. (See his *Treatise on Clouds.*) At Paris, the changes of declination were 7′ 20″.

§ 4. 1821.

In this year, although the needle at Paris showed anomalous oscillations on the 24th, 25th, and 26th of January; the 4th and 21st of February; the 1st, 13th, 26th, and 30th of March; the 15th of April; the 12th and 19th of May; the 22nd of June; and the 6th and 14th of July, I know of no observations of aurora.

Captain Parry observed an aurora on the night of the 15th of August in Hudson's Bay. He was then in 65° 28′ N. lat. and 50° 18′ W. long. from Greenwich. It consisted of many luminous patches, having, when viewed together, a tendency to form an arch extending from S. S. E. to W.S.W. The pencils of rays shot up from this arch to the zenith. At times, the colour was a deep orange. At Paris, the variations of the declination needle were as much as between 16′ and 17′.

On the 25th of November, there were at Paris oscillations of 10′ 17″, but I do not find anywhere any indications of an aurora.

On the 29th of December, towards midnight, Captain Lyon saw in Hudson's Bay a brilliant aurora in the form of an arch, situated to the south, and directed from east to west. At Paris, the declination needle only varied 2′ 30″.

§ 5. 1822-3.

On the 13th of February, 1822, about eight in the evening, Sir George Mackenzie, in travelling between Nairn and Inverness, saw, in the north, a luminous arch 3° or 4° broad, and extending over about 60°. There were also traces of an arch, broader, less intense, concentric with the former, but of larger diameter. All remained in this state for some time; after which, a vivid light suddenly appeared in the east. Passing rapidly over the space occupied by the arches, it presented those fantastic appearances and wavings of light always seen in brilliant auroras. It is stated positively that the summits of the arches were directly below the pole-star. This would be a very remarkable circumstance if it were a result of measurements taken with an instrument. At eleven, when Sir George Mackenzie left off observing, two luminous concentric arches were still visible.

At this time displays of aurora borealis had become very rare; so much so, that the one above described is the only one mentioned in the scientific journals of the year (1822). I could not learn that it had been seen anywhere in France; but its effect on the magnetic needle was very sensible at Paris in the evening of the 13th, especially towards eleven o'clock. On the following day, the 14th of February, the march of the declination needle was, in the same way, sufficiently irregular to lead me to suppose that the phenomenon of the day before was repeated on that evening.

On the 19th of February, at 8 h. 45 m., the declination needle underwent a movement different from any which I ever remember to have seen in the course of my observations. The needle oscillated with great rapidity, and its movements, which were so considerable as to be seen by the naked eye, were principally in the north and south direction, or in the direction of the length of the needle. I do not see anything but an earthquake which can be supposed to have been the cause of this kind of effect.

On the 15th of April, 1822, about half-past ten P.M., Scoresby, whose vessel was in 65° N. lat. and 5° W. long. from Greenwich, saw an aurora, which commenced in the north, rose gradually to the zenith, and extended to the south, forming a continuous arch. A kind of corona was then formed in the zenith; the light was equal to that of the full moon; streamers of all colours darted from it with extreme rapidity. The most remarkable tints were blue, green, and red. At Paris the variations of the needle were as much as 14′ 53.″

On the 13th of July, at a quarter before ten, there was in the direction of the magnetic meridian and near the horizon, a light, which appeared to me much stronger than twilight could be at that hour. Some clouds, low down, were sensibly tinged with red. At ten the light had almost entirely faded. The variations of the needle were as much as 10′ 55″.

On the 24th of October, the needle varied 22′ 18″, and on the 17th of the same month 2′ 40″ [?]; but we did not learn any aurora being observed.

During the year 1823 I did not observe any aurora at Paris; nor did I learn of any being observed elsewhere. My needle showed a variation of 12′ 38″ on the 20th of January, and 11′ 23″ on the 5th of September.

§ 6. 1824.

On the 21st of January, at half-past nine in the evening, at Leith, a luminous appearance resembling aurora was seen in the north. At Paris the variation was 5′ 18″.

In the night of the 29th of July Captain Lyon saw a faint aurora borealis. He was then near Hudson's Bay. (*Brief Narrative*, p. 16.) Variations at Paris 10′ 8″.

On the 11th of August Captain Lyon saw an aurora in Hudson's Bay; it lasted several hours, and was mostly in the zenith: the light was very vivid, passing successively from the deepest purple to a light blue, to yellow, and to green. (*Brief Narrative*, p. 35.) Variations at Paris 12′ 56″.

On the 13th of August, at Paris, there was a variation of 13′ 15″. Captain Lyon saw no aurora that night, but the sky was only clear for a few moments (pp. 43, 44.).

On the morning of the 9th of September a brilliant aurora was seen near Edinburgh. (*Brewster's Journal*, July, 1825, p. 55.) At Paris, variation 19′ 57″.

On the same day, about midnight, a very brilliant aurora, but lasting only a short time, was observed in Hudson's Bay. It had all the prismatic colours. (*Capt. Lyon*, p. 91.)

On the 29th of September a brilliant aurora was observed near Hudson's Bay by Captain Lyon (p. 134.). At Paris the oscillations did not exceed 4′ 41″, but were very frequent.

Captain Parry records an aurora borealis *in the morning* of the 17th of November, about 45° above the horizon at Port Bowen. In the morning certainly means after midnight. This aurora appears to have disturbed the needle at Paris in the night from the 16th to the 17th, for, with numerous oscillations, the extreme deviation rose to 25′ 25″.

On the 25th of November, at Port Bowen, in 88° 54′ W long. from Greenwich, 73° 13′ N. lat.*, Captain Parry mentions an aurora in the south, forming faint arches. Variation at Paris 4′ 41″.

On the 26th of November the same observer saw, two hours

* [The mean declination of the needle at Port Bowen was 123° 22′ W. of N. In reference to the forty-seven auroras seen at Port Bowen between October, 1824, and March, 1825, Captain Parry remarks, "upon the whole, the auroral arches seem to have been more frequently bisected by the plane of the magnetic, than by that of the true meridian."]—*Editor*.

after midnight, an aurora forming an irregular arch, from S.S.E. to N.W. by N. The arch was sometimes very brilliant, and streamers shot up from it towards the zenith. At Paris, variations 6′ 52″.

On the 27th of November Parry mentions a faint auroral arch E.S.E. to N.N.W. At Paris, at 7 A.M., variation 3′ 17″

On the 1st of December Parry saw a faint aurora in the morning. At Paris the variation was 15′ 17″.

On the 8th of December the weather was clear at Port Bowen, yet no aurora is recorded in that day's journal, although my register at Paris gives a variation of 10′ 27″.

16th of December, at 7 A.M., an aurora from E. S. E. to W.S.W. was seen at Port Bowen. It is not said that it was considerable. At Paris there was a variation of 2′ 58″.

On the 28th, and on the morning of the 29th, the sky was clouded at Port Bowen, so that it was not possible to see an aurora, supposing there to have been one in connection with the derangement of the magnetic needle, which was observed at Paris on the morning of the 29th, occasioning a variation of 7′ 29″.

On the 31st of December there was at Paris a variation of 7′ 1″, but the sky was clouded at Port Bowen.

§ 7. 1825.

At this time auroras were scarcely ever seen in the latitude of Paris. We learn, however, from the narratives of Captains Parry and Franklin, that in the Arctic regions traces more or less vivid were seen almost every evening.

The positive announcement that the phenomenon of the aurora had become much more rare than formerly was thus shown to have been either premature or expressed with too great generality. All that could be inferred would be, that the aurora does not rise so high as in previous years, and only rarely attains the limits of our horizon. Yet even when the luminous bands, arches, and streamers, of which auroras are composed, are not visible at a particular place, they exercise at it an evident influence on the position of the magnetic needle. A comparison of the journals of the two above-named celebrated navigators with our registers of magnetic observations at Paris will not leave a shadow of doubt in this respect. This singular connection certainly deserves to be studied under all its aspects;

but perhaps assiduous researches, continued during many years, may be required before all its details can be rightly apprehended. It is therefore satisfactory to learn that exact observers, Messrs. Coldstream and Foggo, are stationed at Leith in Scotland, near the limit which the auroras now rarely outstrip, and that they keep a careful record of all the phenomena of this kind which appear above their horizon. Their observations will aid us in completing the knowledge which we obtain from other sources.

On the 6th of January the sky is marked as having been clear at Port Bowen; but no aurora is recorded, although the variation at Paris rose to 11′ 32″.

On the 7th of January, at Port Bowen, a brilliant aurora appeared at six P.M., and was seen, but only faintly, through the remainder of the evening. The variations at Paris were 6′ 32″.

On the 11th of January, at Port Bowen, an aurora forming an arch from S.E. to N.W. is recorded; but the hour is not named, nor is it said whether it was brilliant. Variations at Paris 6′ 33″.

On the 12th, at Port Bowen, a rather bright aurora was seen in the morning: variations at Paris 5′ 18″

On the 15th, 16th, 17th, and 18th, at Port Bowen, auroras frequently seen, having a marked tendency to form arches, generally from S.E. to S.W. "Sometimes observed to shoot out brilliant pencilled rays, and sometimes vivid coruscations towards the zenith." At Paris, variations 9′ 31″.

28th of January, faint aurora at Port Bowen. Variations at Paris 56″.

11th of February, at Port Bowen, aurora seen at night. It is not said whether it was faint or bright. At Paris, variations 15′ 45″.

14th, 15th, 16th, and 17th, at Port Bowen, aurora was seen every morning. The journal says it was faint; but I perceive that the sky was generally overcast; therefore the real intensity of the aurora may have been considerable. At Paris, variations 11′ 14″.

22nd, 23rd, and 24th of February, at Port Bowen, aurora seen faintly. Only on the morning of the 23rd it took the form of a brilliant and well-defined arch, with numerous bright spots or patches, from which vivid coruscations shot towards the zenith. At Paris, variations 8′ 53″.

Captain Parry's journal does not mention any aurora on the 4th of March; yet the sky was clear at Port Bowen. At Paris there was a variation of 8′ 53″.

On the 6th of March there was a variation of 11′ 32″ at Paris, but the sky was hazy at Port Bowen.

On the 9th of March, at Port Bowen, there was a bright aurora in the south-west at night. At Paris, variations 7′ 22″.

On the 12th, 13th, and 14th of March, at Port Bowen, aurora in the morning, forming a band of bright light parallel to the horizon, about 45° above it, and between W.N.W. and S.W. At Paris, variations amounted to 11′ 4″.

On the 19th of March, the sky was overcast at Port Bowen, but was clear at Leith. At the latter place the wind was blowing strong from the south, when, at about eight P.M., there was seen in the northern horizon that faint light which is the first indication of an aurora. This light increased in strength until half-past nine; then very bright ascending streamers began suddenly to appear, but they did not reach beyond 65° of altitude. They were usually white or yellowish, but occasionally there were momentarily some that were blue and green. A little before ten o'clock the phenomenon became still more interesting; an arch of brilliant white light appeared in the west, rose gradually, reached the zenith, passed beyond it, and went on to the east. At the zenith its breadth was about 7°, but at 5° or 6° of altitude—below which limits scarcely any traces of it were seen—it terminated almost in a point at either extremity. The arch was stationary, and perfectly continuous for an entire hour; only stars of the first and second magnitude could be seen through it. It then broke up in several places before it faded away. When it disappeared the ascending streamers, which had ceased at the moment of its formation, were again seen to dart upwards with renewed brilliancy. One hour after midnight there remained no traces of the phenomenon.

At Paris, on the same day, from noon to 1 h. 30 m. P.M., the horizontal needle departed suddenly and repeatedly from its usual position by nearly 5′. These irregular movements led me to suppose that an aurora would become visible at night; but although the sky was quite clear, nothing of the kind was seen. At six and at eight P.M. the needle was quiet and without oscillation; nor had it deviated from its usual place at those hours; but at half-past eleven (the very time when the aurora seen at Leith had attained its maximum of brilliancy) the declination had suddenly diminished more than 8′, and the needle was oscillating in large arcs. The extent of the entire diurnal variation on that day rose to 17′ 35″.

The march of the same needle (that used for observing the diurnal variation) with which the preceding observations were made, seems to indicate that there were probably considerable auroras on the 30th and 31st of March and on the 1st of April. Probably clouds prevented Messrs. Foggo and Coldstream from observing them. Although the sky appears to have been tolerably clear on those days at Port Bowen, Captain Parry's Journal does not mention any aurora.

I add, that no aurora is mentioned in Captain Parry's Journal throughout the month of April!

Ought we to suppose that this meteor had suddenly ceased to appear? My needle seems to say the contrary.

I would also point out the 26th of July, when I had a variation of 34′ 46″. I have not learned that any aurora was observed on that day.

On the 17th of August, at 10 P.M., Messrs. Coldstream, and Foggo saw faint traces of aurora.

I suspect that this was the end of a day aurora. I find that on the morning of the 17th, from half-past eight to noon, the declination was constantly about 5′ greater than the mean of the month at those hours; while in the evening the needle had returned to its usual position. The total amount of variation was 12′ 10″.

In this same month of August, the night of the 21st, the morning of the 22nd, the night of the 26th, and especially the night of the 29th, presented great anomalies in the march of the needle. On all those nights I believe the sky at Leith was clouded. If the observers farther to the north had fine weather, and yet saw no aurora on the night of the 29th of August, for example, we shall be obliged to admit that there exist other causes, still unknown, which exercise a considerable influence on the march of the needle. However, auroras were seen in Norway in the latter part of August, and Mr. Hansteen thinks that their real dates were the 21st and 22nd.

On the 26th, at forty minutes past eleven, an aurora was seen at Christiania.

On the 10th of September a very fine aurora was observed about ten in the evening at Leith. At the same hour the horizontal needle at Paris was 10′ distant from its mean position. The whole variation was 15′ 17″.

The same day a brilliant aurora was observed at Christiania, as we learn from Mr. Hansteen.

On the 15th of September, in Davis Straits, lat. 69° 30′, aurora in the south-east quarter, as a bright luminous patch, 5° or 6° above the horizon, occasionally sending up vivid streamers towards the zenith. It appeared in the same manner on several subsequent nights in the south-west, west, and east quarters of the heavens. (*Parry*, p. 170.) At Paris, variation 10′ 36″.

On the 20th of September, Captain Parry saw a bright auroral arch pass across the zenith from S. E. to N. W.*, "appearing to be very close to the ship," and affording so strong a light as to throw the shadows of objects on deck. At Paris, variation 9′ 49″.

On the 24th of September, in lat. 58½° and long. 44½°, Captain Parry described detached masses, like luminous clouds of yellow or sulphur-coloured light, appearing in the east. At nine P. M. the light rose in a narrow band, passing through the zenith and descending again to the western horizon. Afterwards luminous streamers succeeded each other, darting with inconceivable velocity. The intensity of light, during the brightest part of the phenomenon which continued for three quarters of an hour, was scarcely inferior to that of the full moon; some parts of the aurora were greenish. At Paris, variation 9′ 33″.

On the 5th of October, Captain Parry notices that the sky was clouded, but yet, at times, as brightly illuminated by aurora as it would have been by the full moon, in similarly cloudy weather. At Paris, variation 11′ 42″.

7th of October, in the evening, inconsiderable aurora at Leith. (At Paris the observer was absent.)

3rd of November, at Leith, aurora at eleven P. M. The north end of the needle at Paris was, at ten on the same evening, 9′ E. of its mean position. The entire variation was 15′ 8″.

On the same day, Mr. Hansteen mentions an aurora at Bergen, in Norway.

* [Magnetic declination about 60° W.]—*Editor.*

4th of November, Leith. In the evening very vivid and very numerous luminous streamers; they, however, only continued visible for a few minutes, and were not preceded or followed by the diffused light near the horizon which usually accompanies the meteor.

On this day (4th of Nov.) the horizontal needle at Paris made sudden, rather considerable, and very numerous movements, from nine in the morning until two in the afternoon. In the evening it had nearly come back to its mean position. The total variation observed in the day was 9′ 31″. It would seem, therefore, as if the streamers seen by the Scotch observers were the last remains of an aurora which had taken place in the daytime.

22nd of November, Leith. Very fine aurora seen for three hours, notwithstanding bright moonlight. The luminous streamers in shooting upwards reached the zenith.

On this day, at Paris, the needle began to go beyond its usual limits at eleven at night; and on the following morning, the 23rd, at 8 A.M., its north end was more than 3′ to the west of its mean position. During the rest of the day its march was very irregular. The entire diurnal variation was 6′ 24″.

On the same day (23rd of Nov.), Mr. Farquharson saw a fine aurora at Alford, in Aberdeenshire, at half-past ten in the evening. (*Phil. Trans.* 1829, p. 106.)

§ 8. 1826.

In the December number of the Annales de Chimie et de Physique for this year (2e série, t. xxxiii. p. 421.), I inserted the following note, in which I have only modified what there may have been of harshness in the terms of my reply to those who had criticised me:—

"A luminous auroral arch was seen at Carlisle and in Roxburghshire, on the 29th of April, 1826: the aurora was not seen at Gosport, although the sky there was clear.

"At Paris, on the 29th of April, at 7 h. 50 m. P.M., the north end of the needle for observing diurnal variation was 4′ E. of its ordinary position; at half-past eight it had returned back to the west by a sudden movement; at half-past eleven it had resumed, within half a minute, its position at half-past eight. Long experience has taught me that these great oscillations, at hours when the needle is almost always stationary, are a nearly certain indication of the existence of an aurora. Therefore, notwithstanding all the pains which have been taken to throw doubt on this result, of which, be it said, in passing, I am myself far from disputing the strangeness, I venture to predicate that brilliant auroras will have been seen somewhere in the northern regions at the following times:—

"On the 16th of January, 1826; on the 10th and 13th of February; in the course of the day of the 9th of March; on the morning and evening of the 23rd, and on the 29th of the same month; on the 9th and 13th of April; on the night from the 17th to the 18th; the 24th of the same month, &c. &c.

"If my critics are disinclined to wait patiently for the return of the voyagers from the North before pronouncing on the justness of my announcements, I would propose their making immediate inquiries from the masters of whalers, and from observers in the north of Scotland."

In the following year I was enabled to state that, as I had conjectured, an aurora had been seen on the 29th of March, 1826. The letter from the illustrious chemist, Dalton, in which this information was communicated to me, is so interesting, that I cannot refrain from giving it in full:—

"My dear Friend,

"Knowing the interest you take in all that relates to meteorology, I send you the result of an investigation which I have recently made on the height of auroras.

"A very remarkable aurora borealis was seen in the north of England and in Scotland on the 29th of March, 1826, between eight and ten in the evening. It had the form of a rainbow, and embraced in the sky the space comprised between the magnetic east and west. This arch remained almost completely stationary for nearly an hour, or, at least, its movement in a north and south direction was quite insensible.

"The arch was seen on several points of a line of not less than 170 English miles in extent, in the direction of the magnetic meridian; among other places, at Manchester and at Edinburgh. At the southern extremity of this line the culminating point of the arch appeared in the magnetic meridian, in the north, at an angular height of sixty degrees above the horizon. At the northern extremity of the line the culminating point, being also in the magnetic meridian, was at an altitude of fifty-five degrees, but on the southern side. At some intermediate towns observers saw the culminating point in the zenith; and at others, either to the north or to the south of the zenith, according to the latitude of the place.

"According to these data, taken in combination, I. have found the vertical height of the arch to have been 100 English miles, its breadth 8 or 9 miles, and its visible extent from east to west upwards of 500 miles.

"Manchester, 22nd November, 1827."

I must own, however, that I have not received a similar confirmation of all my announcements. I have been enabled to draw up the following list of auroras, and to compare it with my observations.

On the 5th of January an aurora was seen at Königsberg, in the night. (Letter from Mr. Kupffer.) This aurora was also seen at Leith, from as early as seven in the evening, through openings in the clouds. Mr. Coldstream thought that there was also a wide and very luminous arch 25° south of the zenith. (*Edinb. Journ. of Science*, vol. v. p. 190.) Variations at Paris, 9′ 31″.

On the 16th of January an aurora was seen at Leith, between one and two in the morning. (*Edinb. Journ. of Science*, vol. v. p. 190.) At Paris, the extent of the variation was 14′ 2″.

On the 11th of February, aurora said to be seen at Leith in the course of the evening. (*Edinb. Journ. of Science*, vol. v.) May it not have really been the 10th when this aurora was seen? On that day the oscillations of my declination needle were frequent, and the entire amplitude of the variation was 7′ 1″. On the next day, the 11th, the variation was only 4′ 41″.

29th of March. The details given by Dalton respecting the aurora on this day have been seen above. At Paris, the extent of the variation was 29′.

§ 9. 1827.

On the 9th of January, Mr. Marshal saw at Kendal a brilliant aurora.

On this day, the march of the needle at Paris was very irregular. As early as two in the afternoon the north end of the needle was 4′ 30″ more to the west than usual; the deviation continued to be in the same direction until half-past seven; but, at five minutes after eleven, the declination was, on the contrary, 3′ 30″ more easterly than on preceding days. The diurnal variation was 10′ 46″.

The dipping needle also made irregular oscillations; it varied 5′ 9″.

The sky was completely clouded.

On either the 13th or the 18th of January, about six P.M., a luminous arch, situated in the magnetic meridian, and to the north of the zenith, was seen at Gosport. It increased gradually in extent and brightness; after half-past nine its base subtended more than 90°. Streamers, or pencils, of pale red light rose successively from different parts of the arch where considerable accumulations of luminous matter had momentarily formed. Many of these pencils or columns reached an altitude of 48°. At half-past eleven, the phenomenon could still be seen through breaks in the clouds. On the following days nothing was seen.

I find the two above-named dates (13th and 18th) in the same number of the Philosophical Magazine, from which I have taken the above. If the first date, the 13th, were the correct one, it would follow, that the aurora did not sensibly affect the march of the needle at Paris; but if, as I suppose, the 18th be the true reading, then the influence was very great; and contrary to what usually happens, the disturbing effect was first to carry the north end of the needle more to the westward. At half-past six in the evening the declination was 3′ greater than its ordinary amount, and a quarter of an hour later it had increased 1½′ more; at a quarter before twelve, on the contrary, I found it 14′ less than on preceding days; but in the course of the next five minutes, or between 11h. 45m. and 11h. 50m., the needle moved 21′ to the west. The sky was clear.

The other days in the month of January, 1827, on which the magnetic needle showed a sensible derangement in its march were,—Thursday, the 4th (particularly in the morning, and about noon); Thursday, the 25th, the whole evening from six o'clock; and Tuesday, the 30th, in the evening. On the last-named day, I find that Dr. Fielder saw an aurora in Norway. The variation of the declination at Paris was 12′ 47″; that of the inclination rose to 11′ 1″.

On the 17th of February, at eight P.M. at Gosport, according to Mr. Burney, a bright light was seen in the north; it occupied 20° on either side of the magnetic meridian. Luminous columns rose vertically, from time to time, from some clouds which formed here and there. At eleven the phenomenon was suddenly concealed by a heavy fall of snow.

On this day, at Paris, the declination needle presented nothing remarkable in the morning, or until a quarter after one in the afternoon, when the north end was 5′ to the east of its usual position. The variation in the day was 9′ 12″. The sky was clear.

I consider that there is reason to believe, that in this month of February there were auroras on the 3rd, beginning at noon; on the 4th, especially in the morning; on the 18th, in the evening; and on the 19th, about noon.

In March the needle did not experience any great perturbations. The evening of the 8th, the morning of the 9th, a quarter after nine on the evening of the 13th, about noon on the 22nd, and at half-past nine in the evening of the 30th, are the only times when it was 2′ or 3′ out of its usual position.

I have no doubt that the observers in the north saw several auroras in the month of April. The days on which the needle was most disturbed were the 5th, about noon, the 6th, 7th, 22nd, and 24th. There were also sensible deviations on the 12th and the 13th.

If I thought it desirable to continue this enumeration, I should also say that I believe there were auroras on the 2nd and 16th of May; but I prefer reverting to those appearances of the meteor which were actually witnessed, and which have come to my knowledge.

On the evening of the 27th of August an aurora was seen at Perth. The coruscations were very rapid, and, at one moment, overspread almost the entire sky.

On the evening of the same day an aurora was seen at New York, Washington, &c.

At Paris, on this day (27th), at 1h. 6m. P.M., I found the north end of the needle 10′ to the west of its usual position, and it moreover underwent irregular oscillations. At half-past nine in the evening the declination was, on the contrary, about 8′ less than on preceding days at the same hour; the sky was very cloudy. The diurnal variation of the declination was on this day as much as 27′ 8″.

On the evening of the 28th of August an aurora was seen in Roxburghshire.

A brilliant aurora was also observed on the evening of the same day in the United States, and was still visible at sunrise. At ten in the evening two concentric arches were remarked.

At Paris, on this day (the 28th), at one P.M., the declination of the needle was 6′ greater than its usual position on other days. In the evening it was unfortunately only observed once; this was at eleven, when the declination appeared to be 3′ less than usual. Next morning (the 29th), at nine o'clock, the north end of the needle was 12′ to the west of its usual position. At a quarter to ten a further change of 4′ in the same direction had taken place, and the needle now made considerable oscillations, vibrating in arcs of more than 8′. In the evening all was again as usual.

The dipping-needle showed similar influences. During the morning of the 29th the inclination was nearly 6′ greater than on the preceding and following days. In the intensity, also, there was a variation of 5^s in the duration of 500 vibrations.

Auroras were observed over a great part of the United States on the nights of the 27th, 28th, 29th, and 31st of August, 1827.

I subjoin some extracts from the description of these phenomena given by a New York observer, in the Commercial Advertiser:—

On Monday, the 27th of August, a few minutes after the moon had set, a considerable light was seen in the northern part of the heavens, as if from the reflection of a great fire. Soon afterwards there was seen a luminous arch, a little above the horizon: the centre of the arch was nearly under the pole-star. The whole interior of the arch appeared as if occupied by a thick cloud: bright patches formed from time to time at different parts of its contour; from these darted a great number of bright streamers, which underwent a very rapid horizontal or lateral movement from east to west. Later in the night the vertical columns of light appeared quite stationary. The phenomenon had not ceased when the sun rose.

We have seen that this same aurora was observed at Perth, in Scotland, and that it occasioned considerable perturbation in the magnetic needle at Paris.

On the 28th of August, at half-past nine in the evening, there were in the north two concentric arches, a few degrees from each other. The pole-star was in the vertical plane of their culminating points. The upper arch rose gradually higher above the horizon of New York; reached the zenith, where it appeared to remain stationary for some time, passed beyond it about eleven o'clock, and then broke up and disappeared. Vertical columns of light, having rather a rapid movement of translation, carrying them from east to west, showed themselves several times below the great arch of which we have been speaking.

The interior of the lower and smaller arch was occupied, as on the preceding night, by what looked like a thick vapour. At eleven, a large black cloud, driven by a N.W. wind, passed over the luminous border. The observer thought that the cloud and the arch mutually influenced each other. He says, that the parts which came near together appeared in a state of great agitation. At eleven o'clock a considerable number of luminous pencils appeared to dart from different parts of the arch. Lastly, from time to time, the whole space up to the pole-star was momentarily overspread by a very vivid light, similar to that of sheet, or heat, lightnings.

We have already said that this aurora was seen in England, and that at the same time the horizontal and inclination needles were much disturbed.

On the following day (the 29th) the author of the account of which the substance has just been given, remarked a great arch of vapour extending from the S. W. to the N. E. He adds, that for several days the clouds almost constantly arranged themselves into long bands extending between opposite points of the horizon. He also says that the brilliant aurora described by him was not accompanied by any sound, and he was himself persuaded that no such sounds ever occur. On the other hand, I find on this subject in No. 1., Vol. XIV., April, 1828, of the "American Journal of Science," statements to the following effect.

At Rochester, during the auroras in August, 1827, there were distinctly heard reports similar to those which result from the discharge of an electric battery. The observer in the County of St. Laurence states positively that he heard such reports, especially when the luminous bundles of rays were most agitated. The physicists of New Haven and of Yale College also speak of the sound produced by the aurora.

Amidst these contradictory accounts it seems difficult to know what opinion to adopt. I am well aware that it may seem that positive evidence should outweigh a large amount of negative evidence: but if this view be taken, how can we explain that in the winters past by Captains Parry and Franklin, in the very focus of auroras, nothing should ever have been heard?

On Saturday, the 8th of September, M. Heron de Villefosse, Member of the Academy, saw at St. Cloud, at half-past eight in the evening, an aurora in the north-west: the sky was clear and the moon shone brightly.

On the same day (the 8th) a very notable disturbance in the needle for diurnal variation was remarked at Paris as early as noon, when the north end was 13′ to the west of its ordinary place. At $1^h\ 19^m$, the declination was 19′ higher than at the same hour on preceding days. Throughout the rest of the day the needle was much agitated, and the perturbing cause always carried the north end to the west. It was only in the evening, at a quarter after nine, that a deviation in the opposite direction, or towards the east, was observed.

Persons who may still doubt the influence exercised by auroras, will certainly change their opinion when they see the subjoined series of observations made at Paris on the 8th of September.

Hour.		Declination.		
7h.	16m.	22°	9′	2″·8
Noon		22	33	59 ·5
„	20	22	33	12 ·7
„	30	22	35	42 ·4
„	40	22	35	39 ·6
„	45	22	35	39 ·6
„	50	22	37	31 ·8
„	53	22	39	5 ·3
„	57	22	39	33 ·8
1	0	22	40	15 ·4
„	4	22	38	55 ·9
„	7	22	38	37 ·2
„	11	22	39	38 ·1
„	14	22	38	4 ·6

Hour.	Declination.
1h. 16m. - - - -	22° 38′ 18″·6
„ 19 - - - -	22 40 38 ·9
„ 22 - - - -	22 39 33 ·4
„ 24 - - - -	22 40 43 ·6
„ 28 - - - -	22 40 15 ·5
„ 31 - - - -	22 40 10 ·8
„ 35 - - - -	22 39 47 ·4
„ 37 - - - -	22 38 41 ·9
„ 40 - - - -	22 37 33 .9
„ 43 - - - -	22 36 23 ·8
„ 45 - - - -	22 36 19 ·1
„ 50 - - - -	22 34 36 ·2
„ 55 - - - -	22 32 1 ·9
„ 57 - - - -	22 32 34 ·6
2 0 - - - -	22 31 38 ·9
„ 4 - - - -	22 29 51 ·3
„ 8 - - - -	22 30 5 ·6
„ 12 - - - -	22 29 14 ·7
„ 15 - - - -	22 17 41 ·2
„ 20 - - - -	22 18 18 ·6
„ 25 - - - -	22 17 22 ·5
„ 30 - - - -	22 14 10 ·7
„ 35 - - - -	22 14 43 ·5
„ 40 - - - -	22 15 20 ·9
„ 45 - - - -	22 14 15 ·4
„ 50 - - - -	22 14 52 ·8
„ 56 - - - -	22 17 56 ·5
3 0 - - - -	22 18 56 ·0
„ 4 - - - -	22 19 24 ·1
„ 7 - - - -	22 20 1 ·5
„ 11·5 - - -	22 21 7 ·0
„ 13 - - - -	22 22 3 ·0
„ 15 - - - -	22 22 54 ·5
„ 19 - - - -	22 21 7 0
„ 24 - - - -	22 22 54 ·5

The needle scarcely vibrated at all; at each change it was seen to quit its position without any return in the opposite direction.

Hour.	Declination.
3h. 28m. - - -	22° 21′ 55″·9
„ 33 - - - -	22 21 8 ·1
„ 37 - - - -	22 20 31 ·7
„ 44 - - - -	22 20 55 ·1
„ 50 - - - -	22 20 41 ·1
„ 55 - - - -	22 20 31 ·7
4 0 - - - -	22 20 41 ·1
„ 5 - - - -	22 21 23 ·2
„ 50 - - - -	22 16 37 ·9
„ 55 - - - -	22 15 51 ·1
5 0 - - - -	22 19 45 ·0
„ 5 - - - -	22 18 20 ·8
„ 10 - - - -	22 14 40 ·3

Hour.	Declination.
5h. 13m.	22° 22′ 17″·9
„ 15	22 19 10 ·8
6 0	22 14 58 ·2
„ 15	22 12 57 ·7
9 15	22 5 8 ·9
„ 30	22 9 12 ·2

The inclination needle showed the following variations: —

Hour.	Inclination.
7h. 20m. A.M.	68° 56′·5
1 22 P.M.	68 57 ·2
„ 40	68 57 ·8
2 42	68 55 ·5
3 0	68 54 ·1
5 0	68 58 ·0
„ 5	68 59 ·2
6 15	68 58 ·8
9 20	68 54 ·8

Thus the extent of the variations of the declination amounted to 35′ 36″, and of those of the inclination to 5′·1.

On the 9th of September a brilliant aurora was seen in England. The morning had been rainy; the wind blew from the north-east. A little before noon the wind changed to west, the clouds in the north-west dispersed, and the part of the sky which was now clear took the shape of a perfectly well defined segment of a circle which rose gradually to 20° altitude. Beyond this the sky continued overcast; in the semicircular zone of blue sky there were seen from time to time flashes of a faint whitish light. In the evening, between 9 and 10, a very brilliant aurora was seen. The unknown author of the above notice does not doubt that the phenomena seen in the morning were intimately connected with the aurora which showed itself in the evening. (*Journal of the Royal Institution*, January 1828, p. 489.)

The aurora of the 9th of September was seen by Mr. Farquharson, in Aberdeenshire, at eleven in the evening. (*Phil. Trans.* 1829, p. 107.)

The needle for diurnal variation at Paris was considerably disturbed both in the morning and evening, and also in the afternoon of this day. For example, between half-past one and two the declination diminished nearly 7′; at a quarter after six it was about 12′ less than usual.

The extent of the diurnal variation of the declination rose on this day to 21′ 50″; that of the inclination was 2′.

On the 25th of September, the needle, which throughout the day had presented nothing remarkable, being very sensibly disturbed at half-past nine in the evening, I was led to conjecture that there would probably be an aurora somewhere. Soon afterwards I saw luminous clouds dispersed here and there between N.N.W. and N.E.; they were not always equally bright, sometimes they appeared to light up, and in another instant entirely disappeared. Once, these scattered luminosities united, and formed for a few minutes a continuous arch, but little raised above the horizon, and having its culminating point, so far as could be judged, about twenty degrees from the geographical meridian, that is to say, very near the magnetic meridian.

The same phenomenon was seen at Havre; Ostend, in Belgium; Aarau and Zurich, in Switzerland; Gosport and Kendal, in England; in Denmark, and in Sweden; and Professor Cleveland observed it at Brunswick in the United States. The Ostend observer said that the aurora began to appear at eleven, that it still continued at midnight, and that its light reached to the zenith. Mr. Forster, in England, said that he had never seen distant objects so clearly by the light of the full moon as by that of the aurora of the 25th of September.

Professor Cleveland stated that the auroral arch was very bright, that it was situated to the south, and that its greatest height above the southern horizon was only about 35°. Luminous bundles of rays rose from different parts of the arch towards the zenith. During this time there was absolutely no light either in the north or in the north-east. Only, at an altitude of 45°, there were seen some very faintly luminous streamers.

M. Valenciennes saw this aurora between Arras and Doulens. His attention was particularly directed to a luminous sheet of a very bright crimson tint, just above a luminous whitish segment on the verge of the horizon, about the north-west quarter. He also remarked several vertical rays of a golden yellow.

My learned *confrère* has given me the following notice on the subject:—

"I was going from Arras to Doulens, in a direction nearly from east to west. The meteor was before me, a little to my right, therefore about W.N.W. About nine in the evening, the sky being cloudy, I saw several bright points, which I took for flashes of lightning. Near ten o'clock the sky cleared; and the meteor thus becoming visible, I soon recognised an aurora borealis. I saw at a height of ten or fifteen degrees a rather vivid white light extending for some distance along the horizon. Above this there extended a sheet of bright crimson of varying intensity. It seemed as if there were two luminous foci or regions, which spread until they became united, and took a deeper crimson tinge the more they blended. There then rose, from low down on the horizon, three or four bundles of rays of a more golden colour. These seemed to dart across and divide the sheet of red light; both they and it seemed to fade, and then the crimson gradually recovered its intensity, and then faded again, after having been traversed by fresh beams. These beams rose to 30° or 36° above the horizon. The phenomenon lasted until a quarter to twelve. I never saw anything in the heavens so beautiful. It was a magnificent spectacle. The white light near the horizon continued after the rest of the display had ceased, and gave so much light that, as there was no moon, the postilions said they could not at all understand what it could be. They had supposed it to be the light from some place on fire.

"As we ascended the hill of Doulens the heavens were of enchanting serenity and beauty. The north-west horizon, illuminated by an uncertain light, which a little dimmed the stars, contrasted beautifully with the brilliancy of the constellations in the east. Orion was especially admirable."

On this day, the 25th of September, the needle for diurnal variation at the Paris observatory had followed its regular march from the morning to eight in the evening; after which it became disturbed. At nine, I found the declination 7′ less than on preceding nights; ten minutes later it had moved 7′ to the westward. This was succeeded by a movement to the east, so that at a quarter-past ten the north point had moved 14′ nearer the geographical meridian. It then moved progressively in the other direction; and at half-past ten the declination was 12′ higher than that which I had observed a quarter of an hour before.

The changes in the direction of the needle in the vertical plane were no less interesting; for instance, the inclination was 7′ greater at a quarter after ten than at half-past nine. I presented my observations to the Bureau des Longitudes the next day; and their *procès verbal* for that day contains the following note:—"M. Arago observed an aurora borealis last night. The irregular displacement of the needle for diurnal variation had announced to him the phenomenon in the evening."

On the 6th of October a brilliant aurora was seen in several parts of Britain; among others, at Manchester and in Roxburghshire, notwithstanding the light of the moon.

At Paris on that day (the 6th), the needle presented nothing remarkable in its march during the day; but at eight in the evening a sensible diminution of declination showed that it was expedient to multiply observations. I therefore began to note the position of the needle every five minutes, and continued to do so until past eleven. The displacements were exceedingly irregular, but there was no difficulty in observing, for the needle scarcely oscillated. At eight o'clock the declination had been smaller than usual; at twenty minutes after ten it had increased 8′; five minutes later it had decreased by that amount. At thirty-five minutes past ten I found a declination 18′ less than usual; afterwards it repeatedly augmented and diminished, but without ever coming up to the values of the preceding days.

At twelve minutes past eleven, when the declination was least, its anomalous diminution was more than 20′.

The declination needle also underwent sensible displacements on the same day (the 6th of October), between eight o'clock and twenty-four minutes past ten. Observations which I made on the vibrations of a horizontal needle, when properly corrected for the effect of changes of inclination, proved that the magnetic intensity also undergoes variations during auroras.

The observations of the evening compared with those of the morning showed in the intensity of the magnetic force, as determined with a horizontal needle, changes which were not due to a change of inclination. We found:

Time.	Time of 300 Vibrations.	Temperature.	Inclination.
h. *m.*	*m.* *s.*		
8 55 A. M.	11 50·33	18°·9 (cent.)	68° 34′·2
6 0 P. M.	11 50·11	19°·9 "	68° 35′·0
7 54 P. M.	11 50·23	19°·8 "	68° 36′·5

The first number (11 m. 50 s. ·33), both on the account of temperature and on that of the inclination, ought to be less than the third (11 m. 50 s. ·23). On account of the inclination alone the difference should be 0 s. ·63; it is 0 s. ·10 in the opposite direction.

The horizontal needle only began to be disturbed at night: the sky was perfectly clear, but the moon was very bright, and the north-west horizon a little hazy. During the entire evening I sought in vain to discover any traces of aurora; and yet the aurora observed that night in England was cited as having been one of great brilliancy.

Without the combination of these three circumstances, I should not, as I have already said, have been able to deduce legitimately from the above-mentioned observations the inference, that the aurora seen at Manchester, although it remained below the horizon of Paris, yet deranged the needle there.

On the 17th of October, Mr. Burney saw a faint aurora at Gosport.

On this day (17th October) the horizontal needle at Paris began, between one and two in the afternoon, to show some slight anomalies; but in the evening, ten minutes before ten, the derangement became enormous, the declination being then 24′ less than that observed at the same hour on preceding days. In the course of the next fifty minutes, the needle went back (*i. e.*, the declination increased) 19′. The whole amplitude of the diurnal variation on that day rose to 36′ 10″; and that of the inclination to 2′·1.

I learn, by the English journals, that auroras were seen in Roxburghshire on the 18th and 19th of November. According to Mr. Burney, the aurora of the 18th, the least faint of the two, did not rise more than 5° above the horizon of Gosport.

On the 18th the needle at Paris was disturbed, especially in the afternoon. On the 19th, at eleven at night, the declination was less than on other days, at the same hour, by 8′.

Hitherto scientific Journals have not announced any aurora borealis for the month of December, 1827; yet, from the indications given by the magnetic needle, I venture to predict that observers in the north will have seen auroras on Saturday the 29th, and Sunday the 30th.

§ 10. 1828.

On the 18th, 19th, and 20th of January an aurora was seen at Franklin, Hartwick, Albany, and Auburn, in the United States.

The horizontal needle was considerably disturbed, at Paris, on the 17th and 18th, and a little so on the morning of the 19th. The ranges of the diurnal variation of the declination on those days were respectively 10′ 25″, 16′ 13″, and 4′ 50″.

On the 3rd and 19th of February, auroras were observed at Utica, in the United States.

On the 3rd, the needle, at Paris, was only observed once in the course of the evening; therefore we cannot say whether it was disturbed. The diurnal variation was 6′ 40″.

On the 19th, only a very slight perturbation was observed (variation 6′ 14″); but on the 20th, the disturbance was at its maximum, nearly 20′, and very variable; the whole extent of the diurnal variation on that day reached 36′ 19″.

On the 11th and 12th of April, auroras were seen at Hartwick (U. S.). No indications of these phenomena appeared at Paris; probably the auroras were inconsiderable.

I received from Montmorillon a letter signed Gotteland, informing me that, on the 5th of July, 1828, about ten in the evening, luminous masses of different shapes, about the same apparent magnitude as the sun, were repeatedly seen darting upwards from the horizon to the height of about two or three degrees, and disappearing again. This happened nine times in half an hour.

This vague description would scarcely have sufficed to lead me to conjecture, that the phenomenon seen at Montmorillon was an aurora, if the needle at Paris had not been rather disturbed on the evening of the 5th and the morning of the 6th of July; the variations on those days were 7′ 29″, and 11′ 51″.

The disturbance manifested itself as early as noon on the 5th; but, contrary to what more usually happens in disturbances taking place in the day time, the declination was sensibly below its ordinary amount. In the evening, at a quarter to ten, I also found the declination less than at the same hour on the preceding and following days; but, during times of aurora, this is the usual direction of the derangement of the needle in the evening.

I have since found that, on the 5th, an aurora was seen at Albany, at Dutchess, at Lowville, St. Lawrence and Utica (United States).

On the 14th of August an aurora was seen at Clinton in the United States.

On the 14th, at half-past ten, the declination at Paris was notably less than at the same hour on the preceding days.

On the 16th of August a fine aurora was seen at Cambridge, at Lowville, and at Utica (U. S.). A brilliant arch was seen for some time.

On the same day (the 16th) at Paris, the declination was notably greater than usual in the morning and at noon, while in the evening, on the contrary, it was several minutes less than usual. The amplitude of the whole variation was 17′ 9″.

On the 8th of September, at St. Lawrence, half the sky was lit up by very luminous streamers, which rose almost to the zenith.

On this day (the 8th), the needle began to be disturbed in the afternoon, and, as is usually the effect at this part of the day, the disturbance had increased the declination. In the evening, on the contrary, and agreeably to what is, also, an almost general law, the declination was less than its ordinary amount at the same hour; whereas on the next morning it had again become higher than the ordinary amount by 7′. The variation in the day rose to 23′ 23″.

On the 12th of September, an aurora was again seen at Utica. The march of the needle at Paris, on that evening, did not present anything remarkable.

On the 15th of September, at Edinburgh, about nine in the evening, a luminous ray was seen to rise from the west horizon towards the zenith, and soon formed an arch of great beauty. On drawing it upon a globe, it was found that the plane of the arch was perpendicular to the magnetic meridian. At seventeen minutes after nine, the arch passed over the zenith of Edinburgh. It had a slow and gradual movement towards the south; its lowest portions were the most brilliant. Its breadth, at the zenith, was about 5° or 6°.

At Islay House (in Scotland), about ten minutes before nine, the aurora formed a luminous arch extending from S.E. to N.W. Its lower extremities were much less broad than the higher parts. Faint rays darted from it towards the south-west. The arch remained stationary.

Throughout the time during which this appearance lasted there was a brilliant aurora in the south-east—sometimes red, sometimes yellow, and sometimes pale green. (*Edinb. Journal of Science*, No. XIX., p. 177.)

On Monday the 15th of September, the diurnal variation needle at Paris presented no derangement deserving of notice. On the preceding Monday (the 8th of September), on the contrary, it had, as already noticed, much exceeded its usual limits. It might be useful to examine whether there may not have been a mistake in the date.

On the 26th of September, an aurora was seen at Albany, Auburn, Lowville, Clinton, &c. (U. S.).

On the 26th of September, at 10 P.M., the declination at Paris was 9′ less than usual. The variation in the day was 16′ 31″.

On the 27th of September, an aurora seen at Cambridge in the United States was not indicated by anything in the march of the needle at Paris, where the variation in the course of the day was only 7′ 47″.

Messrs. Kater and Moll report that they saw on the 29th of September, at 8 h. 35 m. mean time, a luminous zone extending from E.N.E. to a little south of west. Its extremities went down to the horizon on both sides. Its light was white, very nearly uniform, and of an intensity very superior to that of the milky-way. Its breadth appeared to them to be about 3° 45′. The edges were perfectly well defined, and the margin as bright as the centre. The stars were seen distinctly through it.

The height of the culminating point of the arch was 72°. Taking this result in combination with the points of intersection with the horizon, Mr. Kater finds that the plane of the arch was perpendicular to the magnetic meridian, and that it formed with the horizon an angle equal to the inclination of the magnetic needle. At 8 h. 42 m. mean time, the light began to fade on the eastern side; at 9 h. 22 m. no trace of it remained. During the whole time that it continued visible, the arch remained very quiet. No streamers darted from it. The weather was magnificent. The wind blew from the south-east. Chesfield Lodge, where these observations were made, is in 51° 56′ latitude.

This same phenomenon of the 29th of September has been described, by Mr. T. Forster, of Boreham, in Essex, as an appearance of the zodiacal light. Near eight o'clock that observer did not see the whole arch, but only that part of its light which extended from the western horizon to the zenith, or a very little beyond it; the rest of the arch was scarcely discernible. At half-past eight, and a little to the south of west south west, the luminous zone commenced abruptly at a height of 5° above the horizon, and continued to within 5° of the zenith, so that its total length was only about 90°. (At this same time, Mr. Kater saw the arch quite entire.) Mr. Forster says that the colour was reddish and very vivid. (Mr. Kater describes the light as quite white.) Mr. Forster saw a few auroral streamers in the north; at nine o'clock all had disappeared. What was the cause, I do not say of the difference of position, but of the difference of form in the luminous band at two places so little distant from each other?

The English journals contain a third description dated from Gosport. The observer there (Mr. Burney, I suppose,) saw at seven in the evening a small luminous segment, nearly in the magnetic north (or "north by compass"); its altitude gradually increased, and at nine o'clock was 26°. The extremities of the segment corresponded to a little north of west, and a little north of north-east. Bright streamers, almost perpendicular to the horizon, darted from it, and rose to about 35°; forty were counted in the space of forty minutes. They were either slightly yellowish or very bright red. At a quarter past eight, a mass of light detached itself at all points from the segment in question; five minutes later, it formed a very regular arch $4\frac{1}{2}$° in breadth, its culminating point being at 70° of altitude, and its extremities at the horizon being a little south of west and east-north-east. The eastern part disappeared ten minutes before nine. Bright streamers then darted continually from the western branch. This western branch ceased itself to be visible at five minutes after nine; yet some traces of it could still be perceived near the horizon. The bright segment from which the arch had detached itself was seen up to ten o'clock. Mr. Burney perceived slight signs of aurora on the two following evenings, viz. the 30th of September and the 1st of October.

(I leave to physicists to explain how it happened that Mr. Burney saw so many streamers at the very time when Kater saw no trace of any.)

At King's Lynn Mr. Utting saw that at eight o'clock the centre of the luminous arch passed exactly through α Aquilæ. The greatest altitude was 56°, in a plane forming an angle of

25° with the meridian; thus the culminating point was about S. S. E. Mr. Utting thought that the arch was 2° or 3° broad at the time of its greatest brilliancy, but said that latterly the breadth had increased to 8° or 10°. He made the time of disappearance nine o'clock. (*Ann. of Philos.* Nov. 1828.)

The same phenomenon was observed in the neighbourhood of London from six in the evening to midnight. At six the aurora showed itself in the north-west under the form of a very brilliant segment of a circle resting on the horizon. It disappeared at half-past six, after having risen to 12° of elevation. At seven the aurora reappeared; it was the most intense in the quarter of the magnetic north; bright streamers shot from it perpendicularly, and rose to 20°. At a quarter-past eight all had vanished, but at ten the aurora was again visible. Many streamers rose from the base.

The unknown author of the narrative from which the above is taken was disposed to attribute the successive disappearances of the aurora to the existence of an upper current coming from the N. W., but he does not say how the current could produce such an effect. He remarks, moreover, that a violent N. E. wind was blowing in the evening and in the night. (*Phil. Mag.*, Jan. 1829, p. 77.)

The same aurora was observed at Plymouth by Mr. George Harvey. At ten minutes after eight he saw in the W. S. W. a bright streamer 20° long and 1° broad, and about 20° above the horizon; five minutes later the streamer had already grown considerably larger, and now crossed the meridian 10° south of the zenith. At twenty-seven minutes past eight it almost reached the eastern horizon; the arch was then 4° broad, its two sides were parallel and well-defined, and, with the exception of a few inflections, only seen at its lowest portions, its plane was perpendicular to that of the magnetic meridian, and formed with the horizon an angle equal to the inclination of the magnetic needle. The western part always appeared much the most brilliant. The light was everywhere quiet and steady, except a little tremulousness, perceived at forty-eight minutes after eight o'clock near the Pleiades.

During the whole time the phenomenon lasted, the quadrant included between north and west was lit up by a light equal to that of a bright summer morning when the sun is about to rise. (*Edinb. Journ. of Science*, No. XIX. p. 146.)

Mr. Davies Gilbert, President of the Royal Society, being near Penzance, saw the auroral arch of the 29th of September at eight o'clock. Its plane was perpendicular to the magnetic meridian, and its light was perfectly tranquil.

At Dublin, where the same phenomenon was observed, the culminating point is stated to have been 10° south of the zenith at half-past seven.

In the United States, on the same day, bright streamers are said to have been seen at Albany, Cambridge, St. Lawrence, Utica, and Lowville, as well as a luminous arch.

On the 29th of September, at 6h. 45m. P. M., the declination shown by the diurnal variation-needle at Paris was more than 7′ below that of the preceding days at the same hour. At 10h. 25m. the accidental or abnormal difference had increased to 12′, always in the same direction; the diurnal variation on that day amounted to 20′ 44″.

On the 30th of September, on which day Mr. Burney also observed an aurora at Plymouth, the needle at Paris was very much disturbed throughout the day. For instance, at a quarter before nine in the morning the declination was more than 20′ greater than at the same hour on the preceding and following days; the diurnal variation was 17′ 9″. The aurora was also seen at Dutchess in the United States.

On the 3rd of October an aurora was seen at Cayuga, but nothing remarkable was observed in the march of the declination-needle at Paris. The diurnal variation was only 6′ 33″.

On the 8th of October a brilliant aurora was seen at Albany and at Dutchess. At midnight an arch 5° broad was seen, perpendicular to the magnetic meridian, and rising 10° above the horizon.

On the same day (8th of October) the horizontal needle was much disturbed at Paris; the extent of the variation was 11′ 23″.

On the 11th of October an aurora was seen at Hartwick.

On that evening (11 October) the needle at Paris was not looked at until a quarter-past ten, when it occupied its usual position.

"On *Monday*, the 15th of October, 1828, at Perth, a brilliant aurora borealis was seen in the evening. Afterwards, a few minutes before nine o'clock, a pencil of very vivid light began to show itself on the horizon about the east; it rose gradually, and in a few moments assumed the form of an arch, spanning the whole firmament. At its highest part the breadth of the arch was about 4°, but from thence it gradually lessened, so that towards its lower extremities, and when approaching its points of intersection with the horizon, the arch was hardly visible. These two points of intersection were nearly diametrically opposite to each other, one a little east of north-east, and the other a little west of north-west. The culminating point was 7° south of the zenith of Perth. The axis of the arch (it would have been desirable that the author of the account had employed a more precise term) remained during the whole time for which the phenomenon lasted in the plane of the magnetic meridian (*Edinb. Journal of Science*, Jan. 1829, p. 179.)

On the 15th of October the horizontal needle at Paris was not notably disturbed; this, therefore, would appear to be a case of aurora not exercising any action on the needle. I only say "would appear," because it seems to me possible that there may be an error in the date of the notice published by Mr. Brewster; for it says *Monday the 15th of October;* now the 15th of October was a Wednesday, not a Monday.

"On *Monday* the 29th of October, 1828, an aurora borealis was observed at Perth, between ten and eleven at night. The streamers were remarkably brilliant, and darted upwards to the zenith with inconceivable rapidity. It seemed as if the atmosphere was on fire." (*Edinb. Journ. of Science*, Jan. 1829, p. 179.)

The horizontal needle for diurnal variation at Paris followed pretty regularly its usual course on the 29th of October, affording a second apparent instance of an aurora without influence on the needle. But by some strange fatality, I have again to remark that the 29th of October was Wednesday, not Monday, as stated in the notice in the Edinburgh account. On the 30th of October, the declination needle was somewhat disturbed, and I should say it had also been so on the 8th, 9th, and 26th of the month.

On the 31st of October, at Paris, there was in the north a very remarkable black cloud, which I looked upon as the precursor of an aurora; but no luminous streamers appeared. The needle was about 5′ out of its usual position in the evening. On the 8th of November, an Aurora was seen at Utica U. S.

The observations at Paris were not sufficiently numerous in the evening for me to be able to affirm positively that the horizontal needle was not disturbed; but no derangement appeared in the morning, or at noon, or at a quarter to seven in the evening, or at eleven.

On the 11th of November, Erman saw an aurora at Tobolsk (this is taken from a letter addressed to the Academy). The declination is east at Tobolsk, and the summit of the auroral arch corresponded to N.N.E. At Paris, the needle was slightly out of its usual direction in the evening.

On the 1st of December, an aurora was observed at Manchester, at six in the evening, by Mr. Blackwall. Its light was pale and white. It formed an arch 4° or 5° broad, the plane of which seemed to be perpendicular to that of the magnetic meridian. Its culminating point was about 30° above the horizon. At ten minutes after six, the arch began to fade, and soon disappeared entirely, but ascending streamers were subsequently seen to issue from a faintly luminous appearance situated on the horizon, on the prolongation of the magnetic meridian. At Wirksworth in Derbyshire, the aurora was seen at half-past nine, but without any trace of an arch. Aurora was also seen in the United States, at Chuton, and at Schenectady.

On the same day, an aurora was observed by Erman at Beresow in Siberia (lat. 63° 56′), attended by an increase of magnetic inclination of 8′·5. (*MS. Letter from Erman.*)

Although the declination is east at Beresow, yet Erman says the summit of the arch was N.N.W. If a distinct detached arch is intended to be spoken of, the remark is important; if by an arch is only meant the upper margin of a luminous segment resting on the horizon, hundreds of similar examples have been observed in Europe.

On the 1st of December, the needle for diurnal variation was much disturbed throughout the day. In the morning, the declination was greater than usual, and in the evening, on the contrary, it was smaller. At 11 h. 28 m., the derangement exceeded 22′.

The aurora observed at Beresow by Erman may without doubt be supposed to have been under the influence of the second northern magnetic pole, *i.e.*, the Asiatic pole; like the auroras of Europe, however, it carried the north point of the declination needle to the west of its normal position in the morning, and considerably to the east of it in the evening.

Mr. Blackwall saw at Manchester, on the 26th of December, at 6 P.M., an auroral arch perfectly well defined, and having its plane perpendicular to the magnetic meridian. It rose gradually higher; and at 6 h. 20 m. its culminating point was 20° above the horizon. It repeatedly became alternately stronger and fainter. After its entire disappearance, there remained a faint light in the magnetic north.

The same aurora was seen at Hull, from six to seven o'clock. At the moment of its greatest altitude, the arch appeared to be 25° above the horizon.

At Gosport, during the same time, Mr. Burney saw only a faint auroral light.

At a quarter before seven that evening, the declination at Paris was 9′ less than usual. The derangement lasted but a short time.

Mr. Burney saw, at Gosport, during the month of December, a second aurora, of which he has not given the date. Judging by disturbances of the horizontal needle, we may suppose it to have been on the 3rd, 15th, or 28th.

On the 28th of December, at half-past six in the evening, Mr. Farquharson in Aberdeenshire saw an arch begin to rise in the magnetic north, disperse, and then form again five times in succession. At one moment, three concentric arches were seen. (*Phil. Trans.* 1829, p. 118.)

At Paris, the declination needle was sensibly deranged during the day, beginning from the first observation in the morning. The variation in the day was 15′ 54″.

§ 11. 1829.

On the 2nd of January, Mr. Marshal saw a brilliant aurora at Kendal near Manchester.

On the same day (the 2nd of January), at a quarter before eight in the evening, the needle at Paris pointed about $5\frac{1}{2}'$ more to the east than at the same hour on preceding and following days. At eight o'clock, the irregular deviation was not more than $3\frac{1}{2}'$; at a quarter past ten, all was again as usual.

The aurora of the 2nd of January was also accompanied by an effect on the vertical needle. In winter, this needle scarcely varies from the morning to the evening; but when there is any sensible change, it is a diminution of inclination in the evening as compared with the morning. On the 2nd of January, on the contrary, the inclination increased about 1′. I will only add one remark, which is not without interest; it is that an observer at Paris who should only have looked at the needle at a quarter-past seven and a quarter-past ten, would not have suspected the existence of the aurora. Negative facts regarding the magnetic influence of these luminous phenomena are therefore only to be viewed as important when the observations have been very frequent.

On the 27th of January, an aurora was seen at Cambridge (U. S.). At Paris, the horizontal needle showed the influence of a small but real perturbing action.

On the 30th and 31st of January, auroras are also noted at Cambridge. At Paris there was a sensible movement of the north end of the needle towards the east.

On the 11th of February, my illustrious friend Alexander von Humboldt saw faint traces of an aurora at Berlin.

On the 11th, the horizontal needle at Paris was very notably disturbed. At 25 minutes after noon it was more than 7′ to the west of its ordinary position. At 11h. 45m. P.M., it was nearly $2\frac{1}{2}$′ to the east of its usual place. I cannot say what the *maximum* amount of disturbance may have been, because our register supplies no observation between 5h. 45m. and 11h. 45m. P.M. The whole extent of diurnal variation observed was 14′ 58″.

On the evening of the 21st of March the needle for diurnal variation at Paris was much agitated; the entire variation in the day rose to 18′ 33″. Nothing was seen, however, towards the north to indicate aurora, nor have I found any notice of any aurora on that day in scientific journals. The inclination needle also showed a variation of 2′·2.

On Monday, the 23rd of March, about a quarter after two in the morning, Mr. Thomas Maclear, at Biggleswade, in England, saw a luminous arch rising from the eastern part of the horizon, and moving towards the Great Bear. In the course of two minutes, this arch divided into three branches, then into four, and still later into five; but these were soon reduced to two. These different branches were always joined near the horizon, which is not the usual character of multiple auroral arches; but the circumstance which leaves no doubt as to the nature of the phenomenon is, that luminous streamers coming from the west rose from time to time to 10° above the horizon, and that, notwithstanding the light of the moon, traces of aurora were seen in the north, close to the horizon.

In the morning of the 22nd, the horizontal needle had been notably disturbed. In the evening it was not observed until 10h. 40m.; and the declination was then nearly $3\frac{1}{2}'$ less than at the same hour on the preceding and following days. The entire extent of the diurnal variation was 14′ 39″.

On the 4th of April, an aurora was observed at Utica. (*Brewster's Journal*, Jan. 1831, p. 80.)

On the morning of the 4th, peasants going to Dieppe market, and coming from villages several leagues distant from each other, all saw in the sky a fiery train, appearing very broad below, and terminating in a point. They said it gave as much light as the moon when full.

I am indebted to M. Nell de Bréauté for the knowledge of this phenomenon. Notwithstanding what is imperfect in its description, I am inclined to class it with auroras, because the magnetic needle at Paris presented a remarkable march on the morning of the 4th. The disturbance of the horizontal needle was very sensible in the night of the 3rd, and morning of the 4th. On the night of the 3rd, the needle had pointed too much to the east; and on the following morning the derangement was, on the contrary, to the west. The entire variation observed was 13′ 34″.

An aurora is mentioned in Brewster's Journal (Jan. 1831, p. 80.) as having been seen at Lowville on the 5th of April. On that day at Paris, at a quarter before seven in the evening, the needle pointed 4′ more to the east than usual.

On the 8th of April, another aurora was seen at Lowville. It resembled a bright cloud. A constant light was observed near the horizon for some hours. (*Brewster's Journal*, 1831, p. 80.) At Paris, the needle was much disturbed on the morning of the 9th; it pointed 6′ more to the east than at the same hour the day before.

On the 2nd of May, at Paris, the sky being completely overspread with clouds, there could nevertheless be seen in the northern quarter, at some little distance above the horizon, a rather vivid light contrasting with the darkness of the lower clouds. The declination needle was much disturbed that evening, and the entire diurnal variation amounted to 21′ 40″.

On the 29th of May, a rather faint aurora was seen at St. Lawrence, in America. At Paris, the declination was slightly affected. The total variation in the day amounted to 14′ 21″.

On the 31st of May, an aurora, not very remarkable for intensity, was seen at Utica, in America. In Paris, there was a notable deviation of the needle to the west at 1h. 45m. P.M. The total variation in the day was 13′ 24″.

On the 1st of June, at Cambridge, Franklin, and other places in America, a brilliant aurora was seen, with several concentric arches. At Paris, there was a perturbation to the east in the morning. In the evening the needle was only observed once.

On the 2nd of June, an aurora was seen at Cambridge (U.S.), Utica, &c. At Paris, our registers show a disturbance of the needle towards the east at half-past nine in the evening. No traces of aurora were seen with us, although the sky was clear. The entire diurnal variation observed was 20′ 16″.

On the 7th of June, an aurora is noted as having been seen at Schenectady, in America; but no disturbance is shown by the Paris observations.

On the 14th of June, at St. Lawrence (America), an aurora is mentioned which coincides with a small perturbation to the west, remarked about noon, at Paris. The entire variation observed was 15′ 7″.

On the 21st of June, an aurora was seen at Pough-keepsie (America); it was not accompanied by any notable derangement at Paris. The total diurnal variation was only 8′ 53″.

On the 25th of July a very brilliant aurora was seen in the evening at Kendal, by Mr. Marshal. (*Edin. Journ. of Science,* No. 2., p. 317.) Dalton also saw an aurora at Manchester, at 11 P.M.

The disturbance of the needle at Paris, on the 25th, was much greater in the morning than in the evening. The variation of the declination in the day amounted to 10′ 36″, that of the inclination to 5′.

On the 25th of August an aurora was seen at Pough-keepsie (*Brewster's Journal,* Jan. 1831). The Paris observations, which, indeed, are too few, give the variation of the declination in the day 12′ 28″.

On the 26th of August a brilliant aurora was seen at Cambridge, Utica, and other places in America. At Paris, at 11 P.M., the needle pointed 12′ more to the east than usual, and the total variation was 24′ 10″.

On Friday, the 18th of September, at nine in the evening, a very brilliant aurora was seen at a place in America, situated in 40° 35′ N. lat., and 64° 18′ W. long. from Greenwich. The streamers were very rapid and changeful, both in place and colour, sometimes red, sometimes blue, and of all intermediate tints. (*Silliman's Journal*, vol. xviii., 1830, p. 393.)

The aurora of the 18th was seen at Albany, and at Utica, but nothing is said of its brilliancy. (*Brewster's Journal*, Jan. 1831, p. 81.) At Paris the needle was very sensibly to the west of its usual place at 6h. and 11h. 15m. P.M. The total variation was 15′ 54″.

On the 19th of September, at Manchester, an aurora was seen from half-past eight in the evening. (Communication from Dalton.)

The aurora of the 19th was seen in the United States (in 40° 35′ N. lat., and 64° 18′ W. long. from Greenwich), at about nine in the evening. (*Silliman's Journal*, vol. xviii., 1830, p. 393.) At Albany and Clinton, it formed an arch of about 65° in extent, from which streamers rose towards the zenith. At St. Lawrence, it was seen from eight o'clock to nine. (*Brewster's Journal*, Jan. 1831, p. 81.) At Paris, at 1h. 30m. P.M., the needle pointed 3′ or 4′ more to the west than usual, and at 11 P.M., above 7′ more to the east than usual. The total variation in the day was 20′ 54″.

The Paris newspapers of the 23rd of September announced that a brilliant aurora had shown itself in the night from the 21st to the 22nd; and had been watched by numerous spectators from the bridges up to half-past eleven.

I learnt from Captain Sabine that Mr. Farquharson, in Aberdeenshire, had observed auroras both on the 21st and the 22nd; but had not found his declination needle sensibly disturbed by them.

At Paris, on the 21st of September, the needle was in its usual position at 6 P.M., which was the only time it was observed that evening; but at noon precisely, a disturbance of about 6′ to the westward had been noted. As we have seen, Mr. Farquharson, in Scotland, did not notice any disturbance in his needle; but I believe he did not observe it with attention until the evening.

At Paris, on the 22d of September, the needle was probably very sensibly disturbed in the evening; for at 25 minutes after midnight, its north end pointed more than 4′ more to the east than usual.

On the 26th of September, a brilliant aurora was seen at Albany, at half-past nine in the evening. Streamers darted upwards from it to the pole-star. (*Brewster's Journal,* Jan. 1831, p. 81.) In Aberdeenshire, an aurora was seen, but there appeared to be no action on Mr. Farquharson's needle. (*Phil. Trans.,* 1830, p. 105.) Neither is there any evidence of disturbance of the needle at Paris.

On the 1st of October, in Aberdeenshire, an aurora is recorded without any action on Mr. Farquharson's needle. (*Phil. Trans.,* 1830, p. 105.) At Paris there were some small irregularities in the march of the declination needle between 8 P. M. and midnight.

On the 3d of October, aurora was seen both at Manchester and in Aberdeenshire, without any perturbation being shown by Mr. Farquharson's needle. At Paris, at seven, and a quarter past seven, the north end of the needle was more than 4′ to the west of its mean position corresponding to those hours. No more observations were taken during the remainder of the evening.

It is rare for the needle to be so much and so often disturbed as it was during the last three months of 1829.* I subjoin a list of the days on which the perturbations appear to me to have been so considerable that I believe them to have corresponded to auroras:—

October, 4. 9, 10, 11, 12. 21, 22. 24, 25, and 30.
November, 10. 13, 14. 16. 17, 18, 19. 24, and 26.
December, 7. 14. 19, 20, 21, and 23.

* [Those who have followed with interest the recent discovery of a decennial magnetic period, affecting coincidently the solar spots and the solar magnetic influences sensible at the surface of our globe, will not fail to be impressed with the testimony which is here directly, and in other parts of M. Arago's memoranda incidentally, borne to the greater frequency both of magnetic disturbances and of auroras in the years 1828 and 1829 than in the years immediately preceding or following. These were also the years of maximum apparition of the solar spots. (See *Cosmos*, vol. iii. p. 292., Engl. edit.)] —Ed.

The 6th of October is not among the dates specified in the above list; for on that day the march of the needle had appeared to me to be pretty regular. I had only observed it at twenty minutes after five, at seven, at eight, and at half-past eleven. May there have been, between eight and half-past eleven, a great disturbance neither preceded at 8h., nor followed at 11h. 30′, by any sensible derangement in the declination? This does not seem probable, although such a possibility cannot be denied. However, at the point the question has now reached, cases of aurora without action on the needle would be more interesting to science than cases where the declination is visibly affected. Accordingly, the publication of Mr. Farquharson's observations is awaited with impatience.

I subjoin, as an exemplification, a detailed table of the march of the magnetic needle, at Paris, on the 11th of October, 1829, during one of the auroras seen by Mr. Farquharson in Aberdeenshire:—

Time.		Horizontal Needle.			Inclination Needle.		
h.	m.	°	′	″	°	′	″
7	0	22	4	50	67	39	45
7	35		2	50		39	25
8	0		3	45		41	0
8	15		3	15		41	25
8	40		4	59		41	45
9	0		5	35		42	50
9	25		7	0		42	35
10	0		9	40		42	0
10	30		12	0		43	0
Noon.			14	20		41	20
0	20		14	20		41	5
0	50		14	45		41	0
1	45		13	20		40	55
2	0		12	55		40	20
3	45		13	40		41	25
4	45		12	10		42	15
6	15		3	5		43	10
6	30		8	15		42	55
7	20		6	5		44	15
7	30	21	57	0		43	15
7	35		56	25		41	40
7	40	22	2	40		41	15
7	45		5	15		41	40
7	50		7	30		42	5
7	55		8	50		43	15
8	0		7	45		43	50
8	5		7	30		44	25
8	10		4	10		45	20
8	15	21	56	45		45	50

Time. h. m.	Horizontal Needle. ° ′ ″	Inclination Needle. ′ ″
8 20	53 30	43 45
8 25	58 10	42 40
10 15	22 8 40	44 5
10 30	5 35	43 5
10 45	21 57 30	42 15
10 52	56 45	
11 0	57 0	43 20
11 15	55 40	
11 30	54 45	44 20
11 37	56 25	

When these observations are compared with those which present the appearance of a regular march on the preceding and following days, it is seen that the declination needle was deranged as early as noon on the 11th, and that its direction was then nearly $2\frac{1}{2}'$ too much to the west. The uncertainty in the observations of inclination is under 10″.

After attentively examining the preceding table, great surprise will be felt at learning, that at Alford, in Aberdeenshire, Mr. Farquharson's needle was not disturbed on the 11th of October. He says, in express terms, that from 8h. to 8h. 20m. P.M., his needle was tranquil, and in its ordinary position. In this same interval of time at Paris, the declination varied more than 9′, and was very different from its ordinary value.

On the 17th of October, a brilliant aurora was seen at Manchester, at half-past six in the evening. At Paris, the declination needle presented no remarkable anomalies, at least up to a quarter after seven.

On the 21st of October an aurora is noted as having been seen at Utica, and at Cambridge, U.S. At Paris, at noon, the north end of the horizontal needle was nearly 3′ west of its usual position, and at a quarter before nine in the evening the deviation in the opposite direction, or to the east, was upwards of 5′. The total variation was 16′ 22″.

On the 24th of October an aurora was seen at St. Lawrence, in America, and also at sea in 44° N. lat., and 52° 30′ W. long. from Greenwich, by a young scientific man from Columbia, M. Acosta. At Paris, according to a rule during times of aurora, which appears subject to few exceptions, the disturbance of the needle was to the west in the morning and about noon, and became east in the evening.* At a quarter after eight in

* [At Toronto, in lat. 43° 40′N., and therefore in the same hemisphere and

the morning, the anomaly was 6′; at a quarter after noon, more than 5′; and at a quarter after six in the evening, 13′ or 14′. The total variation was 22′ 27″.

On the 25th of October, an aurora was seen at Kendal and in Aberdeenshire. At Kendal the aurora, according to Mr. Marshal's account, was composed of five parallel bands. At Paris, at half-past seven in the morning, the needle was 5′ west of its usual position; at noon, the disturbance was 6′ in the same direction; and at half-past six in the evening, 6′ in the contrary direction, or to the east. At Alford, Mr. Farquharson's needle showed no disturbance.

On the 27th of October, an aurora is said to have been seen at Delaware in America; but from the description it does not appear certain that the light seen was really that of an aurora. However this may be, no appreciable action was observed at Paris.

At Paris, during the evening of the 30th of October, whitish luminosities were seen towards the north, which in a different locality might have been regarded as indications of aurora; but it is possible that the lamps of Paris may have caused such appearances. There was, moreover, a luminous cloud, varying in brightness, but without changing its place, which remained in the E.N.E. for a very long time. This cloud attracted the attention of many persons. There were no very bright stars in that part of the heavens. The extent of the diurnal variation of the declination was 18′ 15″.

nearly in the same latitude as Paris, the mean effect of the magnetic disturbances on the declination needle is to produce a maximum deflection to the west a little after 7 A. M. The westerly deflection gradually diminishes to 1 P. M., when an increase takes place to a secondary westerly maximum about 3 P. M. Between 5 and 6 P. M. the disturbance-deflection passes from westerly to easterly, and attains a maximum of easterly disturbance a little after 9 P. M., the easterly maximum being in amount almost twice as great as the westerly deflection at 7 A. M. From its maximum about 9 A. M. the easterly deflection progressively diminishes until between 4 and 5 A. M., when the deflection becomes westerly.

At Hobarton, which is nearly in the same latitude, but in the southern hemisphere, the mean effect of the magnetic disturbances on the declination needle is to produce a maximum of *easterly* disturbance a little after 7 A. M. The easterly deflection gradually diminishes to a little after noon, when it again increases, and reaches a secondary easterly deflection at about 4 P. M. Between 6 and 7 P. M. the disturbance deflection passes from easterly to westerly, attains a maximum between 10 and 11 P. M., which in amount is rather more than twice the maximum easterly deflection at 7 A. M. From between 10 and 11 P. M. the westerly deflection progressively diminishes, and passes into easterly soon after 4 A. M.]—Ed.

On the 9th of November a brilliant aurora was seen at Lowville in America. At Paris, there was a considerable disturbance of the needle towards the west in the morning, and from noon to half-past one. In the evening, all was nearly as usual.

On the 17th of November, an aurora was observed in Aberdeenshire. At a quarter-past six, an arch of nebulous light was seen, having its summit in the magnetic meridian at an altitude of 20°. Concentric arches rose successively, and disappeared as soon as they had reached 20° altitude. At eleven o'clock, one-half of one of these arches became very brilliant. At this time, Mr. Farquharson's needle was not disturbed. (*Phil. Trans.*, 1830, p. 102.) At Paris, a westerly disturbance was observed in the morning, and, as usual, an easterly disturbance in the evening. The total variation was 13′ 47″.

On the 18th of November, an aurora was seen in Aberdeenshire as early as six in the evening. At eight o'clock, very bright arches were seen, at 20° altitude, and with vertical streamers. (*Phil. Trans.*, 1830, p. 102.) No effect on Mr. Farquharson's needle. (*Phil. Trans.*, 1830, p. 105.) At Paris, the sky was clear in the evening, but no traces of aurora were seen. The needle was 9′ too much to the east at half-past six in the evening; three minutes later, it had already returned $6\frac{1}{2}'$ towards the west, and at 6h. 37m. P.M., it was a little more to the west than usual, a circumstance worthy of remark, if not for the amount, at least for the direction of the disturbance; for in the evening the perturbation almost always shows itself towards the east. At a quarter to seven, the needle had nearly returned to its usual place, and continued so throughout the rest of the evening. The total variation was 14′ 2″.

On the 19th of November, faint auroras, with streamers rising from time to time to the zenith, were seen at St. Lawrence (in America), and in Aberdeenshire. At Paris, no trace of aurora was perceived, although the sky was clear. In the morning, from half-past seven to ten minutes to eight, the needle was sensibly more to the west than usual. No observations were taken in the evening.

On the 14th of December, an aurora was observed in London from six in the evening, according to a communication from Dalton. The aurora was also very bright in Aberdeenshire, where Mr. Farquharson observed it. (*Phil. Trans.*, 1830.) Mr. Burney saw it also at Gosport. At six o'clock, he observed a bright light in the magnetic north. Fourteen luminous streamers rose from it to from 10° to 20° of altitude. A quarter of an hour afterwards, a well-defined arch had formed. It was 3° broad, and had its highest point at 16° of altitude. Its legs corresponded to a little east of north and a little west of north-west. Several luminous meteors passed across the arch. (*Phil. Mag.*, Feb. 1830.) At Paris, there was a considerable perturbation towards the west at one P.M. and at twenty minutes after one P.M. In the evening, the derangement was towards the east; but it was scarcely more than 2′. The total variation was 13′ 25″.

On the 19th of December, an aurora, which had nothing remarkable about it, was seen at Schenectady. (*Brewster's Journal*, p. 81.) At Alford in Aberdeenshire, at half-past eleven, a very brilliant aurora was seen in the north. The streamers rose up to the zenith. The aurora was seen at the same time on the south horizon. (*Phil. Trans.*, 1830, pp. 103 and 104.) At Paris, the needle was considerably to the west of its usual place from half-past eleven A.M. to half-past two P.M. In the evening, and especially between nine o'clock and midnight, there was also a very notable derangement, but it was towards the east. The total variation was 20′ 54″. At Alford, Mr. Farquharson's needle was also considerably disturbed.

On the 20th of December, in Aberdeenshire, a splendid aurora was observed from half-past eight to eleven. (*Phil. Trans.*, 1830, p. 104.) At Paris, at 1 P.M., there was a derangement to the west of 8′; and at 11 P.M. a derangement to the east of 6′. The total variation amounted to 21′. Mr. Farquharson affirms that his needle was not disturbed; but did he observe it frequently?

On the 28th of December, a brilliant aurora was seen at North Salem in America; but the needle at Paris was not sensibly disturbed.

§ 12. 1830.

On the 25th of January, there was in Aberdeenshire an aurora, which presented a succession of arches which only rose to a small height. From time to time, it was accompanied by bright streamers. At Paris, at 1 P.M., the needle was about 3′ to the west of its usual place. At nine in the evening, the deviation in the opposite direction, or towards the east, was little more than 1′ 5″. The total variation was 10′. No derangement showed itself in Mr. Farquharson's needle; but, if I am not mistaken, as I have already said, that physicist only observed the declination attentively in the evening.

On the 28th of January, Mr. Marshal, at Kendal, saw a very brilliant aurora in the evening. (*Brewster's Journal.*) In Aberdeenshire, very brilliant but low arches were seen at eight o'clock.

	h.	m.				
At Paris at	6	15 P. M.	perturbation to the west, nearly			8′
„	8	25	„		east	4
„	8	27	„		„	10
„	8	30	„		„	12
„	8	35	„		„	10
„	8	37	„		„	9
„	8	45	„		„	9

The total variation was 15′ 17″.

At Alford (in Aberdeenshire) Mr. Farquharson's needle was—

	h.	m.		
At	8	0 P. M.		in its usual position.
	8	30	—	21′ 30″ east.
	9	55	—	vibrating in an arc of 30′.

I cannot compare these observations one by one with those of Paris, not knowing whether Mr. Farquharson employs apparent time (*temps vrai*) as would seem natural, or mean time.

On the 19th of February, Mr. Marshal saw at Kendal an appearance of aurora, but without any perceptible streamer. (*Brewster's Journal.*) At Paris, there was a considerable disturbance to the west, from the morning to three in the afternoon, and to the east at a quarter before ten in the evening. The variation was 13′ 53″.

On the 18th of March, I learn from Dalton that a very beautiful, bright, and elevated aurora was seen at Manchester. At Paris, at twenty minutes before seven in the evening, the needle was 17′ more to the east than usual. The total variation amounted to 25′ 44″.

On the 24th of March, a beautiful aurora was observed in Aberdeenshire. At Paris, the needle did not suffer any notable derangement either in the morning or in the evening. Mr. Farquharson's needle, on the contrary, was much disturbed.

	h. m.		
At	9 5	32′	to the west.
About	9 10	25	to the east.
About	9 15	34	to the west.

On the 19th of April, from nine in the evening to midnight, a very brilliant aurora was seen at Manchester, Edinburgh, York, &c. At Paris, at one in the afternoon, the needle was 3′ more to the west than usual. At 10h. 40m. P.M. the perturbation in the opposite direction, or to the east, was nearly 12′. At that time the sky was clear, but no aurora was seen.

An aurora was said to have been seen on the 24th of April; but Dalton, who gave me the information, had not seen it himself. The needle at Paris, which indeed was not much observed, did not indicate anything remarkable.

The newspapers announced that on the 5th of May, at midnight, a magnificent aurora was seen at St. Petersburg. They said that the rays, or streamers, formed an immense semi-circle in which they appeared successively, red, white, and greenish, then faded almost away, and, a moment after, shone out again, and darted in long lines to the zenith.

What should we understand by the 5th at midnight? the midnight which divides the 4th and 5th of May, or the midnight between the 5th and 6th? In either case, the needle at Paris was affected by the aurora in question.

M. Kupffer saw the aurora at St. Petersburg up to 2h. A.M. on the 6th of May. (*Royal Institution Journal*, No. 2. p. 429.)

At Paris there were great disturbances of the needle in the evening of the 5th of May:

h. m.				
At 8 5	apparent time,	more than	7′	to the east.
At 9 10	„	„	5	„
At 10 10	„	„	5	„
At 10 45	„	„	17	„
At 10 50	„	„	9	„
At 11 0	„	„	9	„
At 11 10	„	„	11	„
At 11 30	„	„	17	to the west (?)
At 11 40	„	„	8	to the east.
At 11 45	„	„	13	„
At 11 52	„	„	19	„
At midnight	„	„	14	„

The next morning there was still disturbance, but it was towards the west. At a quarter before ten, it amounted to nearly 9′. In the evening of the 5th, the inclination needle underwent from time to time variations of as much as 3′ or 4′ in a very few instants. At St. Petersburg, M. Kupffer's horizontal needle was considerably disturbed on the night between the 5th and 6th of May. Although I do not know whether the hours of observation are expressed in apparent or in mean time, yet I think I may affirm that the great movements did not take place at the same times, or even always in the same direction, at St. Petersburg and at Paris. Thus, for example, at half-past eleven the disturbance of the needle at Paris was 17′ west, while at St. Petersburg, at 13h. 20m. (corresponding to 11h. 28m. at Paris,) a disturbance of 12′ to the east was observed.

On the 20th of August, a brilliant aurora was seen at Kendal. The keeper of a lighthouse in Scotland saw auroras on the 7th, 10th, 12th, 13th, 17th, 19th, 20th, 21st, 24th, and 25th of September.

On the 7th of September, an aurora was seen at Gosport, between a quarter to nine and nine; the following day some traces of aurora were still discernible. On the 17th of September, a brilliant aurora was also seen at Gosport. Kupffer saw an aurora on the 13th at St. Petersburg. Unfortunately, during great part of September and the early part of October, the magnetic observations at Paris were interrupted by the observer's illness.

On the 5th of October, an aurora was seen at Gosport.

On that day (the 5th of October) Captain Godreuil, of the ship General Foy, saw a brilliant aurora when in 42° 20′ N. lat., 37° 19′ W. long. from Paris. (*National* of the 28th of October.)

On the 6th of October, an aurora was observed at sea by M. Acosta, in lat. 44°, long. 52°. 30′ from Greenwich. The streamers rose to 50° and 60° of altitude. It ceased suddenly at twenty-five minutes after seven.

On the 16th an aurora was seen at Gosport from ten to half-past ten. The streamers which rose from it shot up to the Great Bear. (*Phil. Mag.*, Dec. 1830.) At Paris, between a quarter to eight and thirty-nine minutes after nine, the horizontal needle was always considerably to the east of its usual place. The sky was clear, but no trace of aurora was seen.

On the 17th of October there was an aurora at Gosport; it did not send out any streamers. (*Phil. Mag.*, Dec. 1830.)

On the 1st of November, at 9 P.M., a brilliant aurora was observed by Mr. Burney at Gosport, between north and west. At 9h. 18m. P.M., streamers began to rise; they were exceedingly bright, although the moon, then almost at the full, was 30° high. (*Phil. Mag.*, Jan. 1831, p. 79.) At Paris, at nine in the evening, the needle was about 8′ to the east of its usual place at that hour. The total variation was 16′ 32″.

On the 4th of November, at Gosport, an aurora was visible from 7h. P.M. The streamers only formed at 8 o'clock, and rose 22° high. At 9 o'clock the aurora had disappeared. The moon was then on the horizon. At Paris there was a sensible disturbance of the needle towards the west at one in the afternoon, and there began to be derangement towards the east from 7h. 40m. P.M. At 7h. 55m. this derangement had become considerable, and it was still in existence at a quarter after ten. The total variation was 18′ 43″.

A faint aurora was observed on the 7th of November at Gosport, between 7h. and 10h. P.M. There were no streamers. (*Phil. Mag.*, Jan. 1831, p. 79.) The total variation at Paris was 22′ 36″.

On the 7th of December an aurora was observed at Christiania by M. Hansteen (this information is from a MS. letter from M. Erman). At Paris there was westerly deviation of the needle of above 15′ at a quarter before two in the afternoon, and of more than 20′ at twenty-five minutes after six. Five minutes after seven the disturbance had become easterly. Between 1h. 20m., and 6h. 23m. P.M., the derangement increased 8′.

A brilliant aurora was observed on the 11th at Gosport, from half-past eight in the evening. At two in the morning, the clouds having dispersed, the phenomenon presented itself in its full splendour. The ascending streamers which issued from it were, at their maximum, 2° broad and 30° high; their hue was red or purple. At eight in the evening at Paris, the needle had been more to the east than usual. The observed variation was 13′ 25″.

On the 12th a faint aurora was seen at Gosport, from 6 P.M. to 10 P.M. It extended from N.N.E. to N.W. The arch which bounded it was 8° high. (*Phil. Mag.*, Feb. 1831.) An aurora was also seen on the 13th and 14th, at Paris; from a quarter before seven on the evening of the 12th the needle was considerably to the east of its usual place. The total variation was 16′ 32″. The next morning, the 13th, at eight, the disturbance was also very sensible, but it was towards the west.

Hansteen wrote thus to Erman on the 29th of December, 1830, "Since the end of July there have been observed at Christiania, 35 auroras, all of which were accompanied by considerable movements of the declination needle." Among the cases in which the disturbance was the greatest, he particularly names the auroras of the 6th and 7th of October. I had been obliged to be absent from Paris at that time.

I have thought it desirable to cite in this catalogue all the disturbances of the magnetic needle perceived at Paris, in order that the reader might be able to decide for himself whether, as Mr. Farquharson thought he had remarked, such disturbances only show themselves at the moment when, in their ascending movement, the luminous parts of the aurora reach the plane perpendicular to the magnetic meridian passing through the inclination or dipping needle. For our part of the world, at least, this supposition does not appear to hold good. The reader may have remarked that in almost all cases the aurora, of which the appearance in the evening was attended by a deviation of the north end of the needle towards the east, had previously occasioned a disturbance in the opposite direction, or towards the west, in the morning hours. It may also be noted, and this circumstance removes all difficulties, that the aurora acts on the magnetic needle at Paris, even when it does not rise above our horizon. (See the 19th of April, 16th and 17th of October, &c.)

Auroras which have only been seen in America, Petersburg, or Siberia, notwithstanding the immense distances which divide us from those regions, have occasioned considerable disturbances in the magnetic needle at Paris. This gives rise to the question, May not the auroras of the southern hemisphere also produce some effect? I thought at first that I had it in my power to answer this question in the affirmative by aid of several austral observations, for which I am indebted to M. Simonoff; but I afterwards discovered that unfortunately on the days on which the Russian navigator saw auroras in the regions of the south pole, the phenomenon also showed itself in the northern parts of the globe.

§ 13. 1831.

On the 7th of January a great aurora was seen at Paris.

During the whole time of the observations made on that evening, the aurora was very apparent. At 7h. 33m., apparent time, there were two very distinct arches. The lower limit of the upper arch passed through Lyra. The culminating point may have been 1° or 2° higher at forty minutes after seven. The east leg of the upper arch as seen from the Observatory, was between the Pantheon and the Val-de-Grace, the west leg was a little to the south of west.

At five minutes before eight, vertical streamers darted from it. Ten minutes later, bands and large spaces of a very intense sanguine hue were seen. The light of the aurora was sufficient to read by.

Sometimes there was one, and sometimes there were two concentric arches. In either case the culminating points corresponded very nearly to the magnetic meridian. The electrometer for atmospheric electricity did not show any trace of electricity in any part of the time during which the phenomenon lasted.

The extent of the diurnal variation of the declination rose to 1° 16′ 33″; and that of the inclination needle to 20′.

After midnight (*i. e.* on the 8th) the arch reformed itself in a regular manner, and rose as before. Though the sky was clouded, yet I thought I could see traces of a luminous arch. The needle continued to be disturbed until the 13th.

On the 9th, an aurora was observed at Buch-holz, near Frankfort on the Oder. The observer, M. Pastorff, says that it began at 7 P.M. on the 7th, and was visible until 2 A.M. on the 9th. Does he mean that it was visible during daylight, for two successive days? The light was very bright, and extended 30° on either side of the magnetic meridian. The diurnal variation of the declination was 33′ 22″.

I point out the 2nd, 10th, and 12th of April as having presented variations in the inclination and declination needles which would lead me to suspect auroras, although I saw no traces of the kind on the sky over Paris.

On the 12th of April I saw two very dark clouds, forming on the starry sky two well-defined arches (the lower one especially so), of which the culminating points were in the magnetic meridian. These bands were certainly clouds, for I saw no stars through them.

On the 19th, between half-past ten and eleven, an aurora was seen at Berlin. Vertical streamers shot up to the zenith; a reddish light was seen on the northern horizon. The culminating point of the luminous portion was nearer the geographical meridian than in the aurora of the 7th of January; but as it is not said that the luminosity was in the form of an arch, I do not think the remark has any great importance. The variation at Paris was 25′ 53″.

On the 9th of December the sky at Paris was cloudy; there was in the north, at the horizon, a black band of clouds, above which appeared a vivid and varying light, which evidently could only proceed from an aurora. The needle was much disturbed, and pointed in the evening several minutes more easterly than usual.

On the 22nd of December, at eight in the evening, I saw in the north, through the clouds, a light which seemed to me to be an evident indication of an aurora. The needle was notably disturbed.

§ 14. 1832-1848.

The magnetic observations made by me in 1832, and in subsequent years, having been often interrupted, owing to different circumstances, I cannot attach the same importance to the description of the series of different auroras recorded since then, that I do to those mentioned in the preceding catalogue. However, I think I may still do a useful service to science by noticing in this place the principal cases of the occurrence of the aurora borealis which have come to my knowledge.

I find in a letter from my friend Alexander von Humboldt, the following communication:—"Although the observations of the influence exercised by auroras, even in places where they are not visible, no longer stand in need of confirmation, yet you will learn with some interest the following fact, which M. Gauss has inserted in Schumacher's 'Astronomical Journal,' No. 276. 'On the 7th of February, 1835, the variations in the direction of the horizontal magnetic needle at Göttingen surpassed all that M. Gauss had previously seen; they were as much as 6 minutes of arc in a minute of time.' Now on this same 7th of

February, M. Feld, Professor of Physics at Braunsberg (in East Prussia), observed a fine aurora borealis, which he has described in Poggendorff's Journal."

A fortunate circumstance, in November, 1835, permitted an additional verification of the action exercised by the aurora borealis on the magnetic needle. At that time the instruments intended to be confided to the skilful officers of the "Bonite" were undergoing careful comparison with those of the Observatory. While these examinations were in progress, on the 17th and 18th of November, the needles for diurnal variation, both in the instrument belonging to the Observatory in the great hall of the Meridian, and in that of the expedition at the southern extremity of the garden, underwent sudden, irregular, and very considerable changes of direction. Although the sky was cloudy, I felt no hesitation in inferring from these motions that an aurora borealis would show itself: this was on the morning of the 17th; and on the 18th the unusual motions of the needles had become so great that, notwithstanding the clouds which entirely covered the sky, observers carefully examined its northern portion for traces of aurora, and were able to perceive vivid and rapidly changing luminosities, discernible even through the thick and unbroken curtain of cloud.

Subsequently to these remarks being placed on record in the register of the Observatory, the English newspapers announced that the aurora borealis had been seen in several towns on the night from the 17th to the 18th of November, and the following night. Thus we have a fresh example added to so many others, of a disturbance of the magnetic needle, evidently produced by the mysterious luminous apparitions of which the magnetic pole seems to be the focus. However, in concluding a communication to the Academy on this subject, I cited the disturbances of the 17th and 18th only, inasmuch as they showed themselves in the course of instrumental examinations undertaken by me at the wish of the Academy; for I considered that for several years previously I had demonstrated and established, by the evidence of a great number of observations, that auroras act on the magnetic needles of Paris even when they do not reach its horizon.*

* [The intelligent reader will have remarked that all that the *facts* cited by M. Arago can be considered to *demonstrate or establish* is the *coincidence* of magnetic disturbances at Paris with auroras visible in other parts of the

The aurora of which I had suspected the existence in the morning of the 18th, solely from the irregular motions of the magnetic needles, was observed by M. Valz, at Nimes, between eight and ten in the evening. At nine o'clock, during the greatest intensity of the phenomenon, red streamers rose to the zenith. A rather strongly luminous space, from which streamers ascended, was seen on the horizon. No arch was formed.

M. Masson of Caen, M. Gachot, lieutenant in the navy, M. Verusmor of Cherbourg, M. Charié, engineer of "Ponts et Chaussées," in the department of Nièvre, and others, also saw the aurora on the 18th of November between eight and nine in the evening. The reddish rays of light gave rise to many mistakes; almost everywhere people ran to extinguish the flames of supposed conflagrations, of which they thought they saw the reflection in the sky.

This aurora was seen at Cahors, which is the most southern point from which observations have reached me.

In the night between the 17th and 18th of November the aurora in London, from a peculiar effect of the atmosphere, presented the appearance of a widely spread conflagration, and throughout the night a dozen fire engines were almost incessantly hurrying to and fro to the places from which the flames seemed to proceed. The meteor was first seen at eleven in the evening of the 17th, and after having been very bright for

globe. Of this coincidence the facts cited leave no doubt; and corresponding facts, in regard to the cotemporaneous occurrence of auroras in one place and magnetic disturbances in another, have been very extensively observed elsewhere. The observations at the British Magnetic Observatories have manifested that magnetic disturbances sometimes continue without intermission for several days together, even for seven or eight days together, the continuity throughout being shown by one or other of the magnetic elements, sometimes by one and sometimes by another. By M. Arago's supposition, viz., that the magnetic disturbance is *caused* by an aurora either at the place itself or in some other place, it would be necessary to suppose that, during the whole of such lengthened periods of disturbance, auroras are visible in some part or other of the earth's surface. This it would manifestly be very difficult either to prove or to disprove. But even were it proved, it would not demonstrate or establish that the aurora is the *cause* of the magnetic disturbance, any more than it would prove that the magnetic disturbance is the cause of the aurora. Much more is required to be known before we can hope to form a *certain* conclusion in regard to the nature and extent of the causal connection which we may readily admit to exist between the two phenomena.]—Ed.

some time disappeared. At three in the morning it re-appeared, a very bright streamer rising, nearly in the north, to 30° above the horizon. At the end of some time the light became fainter; the direction from which it proceeded was from N. W. to N. N. W., from whence people were led to infer that it was not a fire. The aurora continued more or less bright until six in the morning. The sky was clear throughout the whole night.

The aurora was again very bright on the following night (from the 18th to the 19th). Bad weather and a thick fog did not permit the meteor to be seen at Paris; but from ten in the morning it had announced itself, as usual, by a sensible increase of the declination. In the evening, on the contrary, from a quarter before nine to nine, the north end of the needle pointed much nearer to the geographical north than at the same hour on preceding days; but at 7 P. M., — and this is a circumstance very worthy of attention, — the disturbance was positive, and notably increased the declination.

About twenty minutes before midnight a bright and varying luminosity was perceived, even through the curtain of clouds. The total amount of diurnal variation observed was 50′ 12″.

On the 22nd of April, 1836, in 46° 25′ N. lat., and 41° 40′ W. long. (from Greenwich), an aurora was reported by M. A. Duhamel, judge in the islands of St. Pierre and Miquelon. It was remarkable for its intense brightness, which, said the observer, was such as quite to outshine the light of the moon, then at the full.

The year 1836 seems to have shown the phenomenon of the aurora with great frequency, and, at the same time, with every variety of form, brightness, and evolution: this is what was written to M. Biot by Mr. Thomas Edmonston, who observed in Shetland. Among all the auroras recorded, that of the 18th of October is the one which was best seen in our Continent. M. Matteucci observed it at Forli in the Roman States; the following is his report:—

"It was nine in the evening when a slightly reddish light showed itself in the northern region. It embraced an extent of 70 or 80°, and rose to 25° or 30° of altitude; its form was circular; in its least high portions it might be 7° or 8° above the horizon. Twenty-three minutes after its first appearance the light became of a bright crimson colour. A darker central line, which was remarked in it, moved towards the west. The phenomenon disappeared by fading gradually away."

According to M. Bonafous, this aurora was seen simultaneously at Turin and at Chambery, at half-past nine in the evening, in the direction from east to west.

Mr. Wartmann of Geneva has given the following description of the phenomenon as observed by him:—

"At 8h. 31m. P.M., the instant when the phenomenon began, the sky was quite clear, the air perfectly calm, and the moon, seven days old, was shining in the south. Two reddish clouds first showed themselves in the north-west, about 25° or 30° above the horizon; they drew nearer to each other and to the horizon until they were in contact, and in a few minutes, appearing to touch the ground, they had the aspect of an extensive distant conflagration; soon afterwards they took the form of a segment, having its cord resting on the horizon, and extending at least 50°; this segment, which was of a strongly pronounced dark red tinge, especially towards the middle, seemed formed of undulating molecules. Three striæ, or very distinct bundles or pencils of white rays, darted from the centre of the arch in a vertical direction; they expanded a little at the upper part, and rose several degrees above the segment, but without reaching the zenith. There were also other streamers of a pale white, and not very distinct, which could be vaguely seen to shoot towards the limb. At a quarter before nine the aurora was very brilliant, and was in the direction of the magnetic meridian. The segment was then about 24° or 25° high; it reached, and included, the stars β, δ, ε, ζ, η, of the Great Bear, situated near the culminating part of its margin; α of the same constellation was almost beyond the limits of the meteor, while γ, the lowest of the seven stars, was rather deep within it.

"The aurora did not remain stationary in this position: it first advanced slowly, all in one piece, from north-west to north, and as far as 5° north-east, passing over a horizontal arc of about 30°, and its upper extremity traversing all the stars of the Great Bear; afterwards, four minutes before nine, it returned towards its former place, and presented a pale orange-tinged crimson; the segment was now transformed into a kind of elongated spindle, of which the lower extremity touched the horizon, while the upper, segmentary, margin reached the stars in the tail of the Little Bear. This vertical column, 47° high, continued to move towards the north-west, shedding a dark reddish lustre, which became gradually fainter. At nine o'clock it was barely visible, and at five minutes after nine all that could be seen in the atmosphere was a confused light, which a few minutes later had completely disappeared."

Mr. Wartmann also received from Mr. Struve observations of the aurora on the same day. It resulted from their comparison with his own, that at the same moment when he found at Geneva the angular height of the culminating point of the luminous arch 25°, in Livonia it was 90°. Mr. Wartmann deduced from hence, by the method of parallaxes, the inference, that the material particles of the arch were two hundred leagues above the surface of the earth.

On the 18th of February, 1837, an aurora was observed at Meaux (Seine et Marne) by M. Darlu. The phenomenon was principally remarkable for the very red colour of the light. As usual, it considerably disturbed the magnetic needle, but without anything to decide whether the direction of the disturbances has any connection with the position of the points where the light was at its maximum. M. Darlu speaks of an arch which, at a quarter before nine, occupied the south part of the heavens: no such southern arch was seen at Paris. The auroral luminosities seen in the south did not form a continuous zone; they only appeared in isolated places.

The aurora of the 18th of February was also seen at the following towns: —

Atonne, near Meaux	Observers, MM.	Darlu.
Luzarches	„ „	Hahn.
Beauvais	„ „	Zoega.
Versailles	„ „	Gaudin.
Sarreguemines	Observers, MM.	Lhomme. Legoullon. Collignon. Barhaise.
Morlaix	„ „	Pitot de Helles.
Bésançon	„ „	Virlet.
Montpellier	„ „	Auguste Saint Hilaire. Bérard.
Marseilles	„ „	Valz.

My friend Alexander von Humboldt sent me a table of the disturbances of the needle at Göttingen during the appearance of this aurora.

"At 8h. 2m. 30s. the declination was 39′ above its usual value.

"From 9h. 36m. to 9h. 37m. a change of declination of 11′ 31″ was observed."

M. Morren, Professor of Physics of the Royal College of Angers, saw an aurora on the 6th of April, 1837. Towards 8h. P. M. the aurora consisted of a pale light, perpendicular to the horizon and directed towards α Cephei. At 8h. 26m. a new arch, much larger and more luminous than the first, was formed a little more to the west; it covered α and γ Cassiopeiæ. This latter arch was intermitting, losing and recovering its brightness in the course of a few seconds. At 9h. all had disappeared.

During the above observations the sky was clouded at Paris; but the needle was much disturbed.

On the 18th of October, 1837, M. Mandl saw at Paris, from 6h. 5m. to 6h. 30m. P. M., a very red aurora. The sky was entirely overcast at the time; this circumstance might have rendered it doubtful whether the red bands observed by M. Mandl really proceeded from an aurora, if the "Féderal" and "Courier de l'Ain" newspapers had not announced that an aurora had been seen at the same moment at Geneva and at Bourg (at both which places the sky was cloudless), and if, moreover, for final confirmation, the observatory needle had not presented sensible anomalies in the course of the same evening.

This aurora was seen at Stockholm. It is in reference to it that M. Capocci said the clouds often borrow from auroras tinges of colour which have not been sufficiently noticed. He further imagines that the red light with which the surface of the moon sometimes shines during total lunar eclipses is to be attributed to terrestrial polar auroras.

Some photometrical considerations might be brought forward which would, I think, present insurmountable difficulties to the reception of M. Capocci's hypothesis. Nor are meteorologists open to the reproach apparently addressed to them by the Neapolitan astronomer: the effects of the aurora borealis on clouds have long been objects of assiduous observation.

On the night of the 12th to the 13th of November, a brilliant aurora of a reddish colour was seen at Paris by M. de la Pilaye, at Angers by M. Morren, at Antony by M. Faure, at Vendôme by M. Yvon, at Jambles, near Givry (Saône et Loire), by M. Nervaux, between Genoa and Leghorn by M. Chassinat, and at Montpellier by Captain Bérard. When the arch was formed at Montpellier, its upper portion, which could but just be distinguished, seemed to be about 20° or 25° high. Captain Bérard judged that this culminating point was in the geographical, and not in the magnetic, meridian. This circumstance would offer an anomaly respecting which more ample information would be desirable.

On the 23rd of September an aurora was observed at Hamburg by M. Robert.

For the year 1838 I have not received any accounts of observations of auroras; in 1839 they appear, on the contrary, to have been frequent.

M. Quetelet wrote to me that an aurora was observed at Brussels on the 5th of May, 1839, about half-past eleven at night, by his assistant, M. Mailly. Its light was more especially remarkable in the direction of the magnetic meridian. It occupied about an eighth part of the heavens, reckoning round the horizon; the luminous streamers rose, at intervals, to upwards of 50° of altitude.

M. Lalanne, engineer of "Ponts et Chaussées," informed me, in a letter, dated from St. Brice, near Ecouen, that he saw an aurora on the 7th of May, about half-past nine. M. Lalanne notices, among the circumstances by which he was most struck, the bright bundles of rays—red, yellow, and blue—which rose 25° and 30° above the horizon.

According to a letter from Mr. Herrick, from New Haven in Connecticut, twenty-two cases of aurora were observed in that town, between the 1st of January and the 3rd of September, 1839. The aurora of the 3rd of September was very magnificent. The centre of the cörona was 74°, angular height, above the south horizon, which would correspond pretty nearly to the point in the heavens in the prolongation of the line of the dipping needle at New Haven. The needle of a compass was disturbed during the whole time for which the aurora lasted, to such an extent, that it sometimes showed a declination differing 3° from the ordinary mean declination. All the disturbances were of such a kind as to bring the north point of the needle to the east of its usual position.

According to Mr. Herrick, the aurora of the 3rd of September was also seen at New Orleans.

At Paris, the astronomers of the Observatory and M. Fravient saw the aurora of the 3rd at about ten in the evening. M. Quetelet wrote me that he had seen it at Asti in Piedmont, about one in the morning. At Alexandria, it began to be seen at ten in the evening, and it lasted throughout the night.

A remarkable aurora on the 22nd of October was reported to me by M. Darlu of Meaux, M. Chaperon of Strasbourg, M. Coquand, director of the Museum of Natural History at Aix (Bouches du Rhône), M. Valz, director of the Observatory at Marseilles, M. Maniani of the Rovere de Pesaro, M. Matteucci of Rome, and lastly, M. de la Pilaye. The latter thought himself authorised in drawing from the differences of appearance, altitude, and direction, presented by the observations sent from different places, the inference that the phenomenon was really at only a rather low elevation within our atmosphere.

The light of the aurora on this occasion was everywhere red, very vivid, and distributed generally in groups, or patches, without any apparent connection. At the moment when, at Marseilles, it assumed the form of a regular arch, the culminating point of the arch was in the magnetic meridian. At Paris, my brother academician Savary recognised that the planes, in which were included the streamers of a greenish-white which from time to time traversed the red zones, all passed through the part of the heavens to which the inclination needle pointed. The horizontal needle at the Observatory was in a state of continual irregular motion, to and fro, during the whole time for which this phenomenon lasted.

I subjoin an extract from the letter of M. Valz:—

"Towards the pole, there was a light, white cloud, lit up by the full moon. When the red tinge reached the cloud, it made it share in its own colour, so as to give reason to believe that the source of the red hue was between the cloud and the observer, and therefore little distant from the latter. It might be objected that possibly the red rays might colour the cloud in passing through it; but I remarked that the cloud intercepted the view of the stars, which the aurora did not, and thus the above explanation is not admissible."

We have given these few lines from M. Valz's letter, because they point out to astronomers a particular class of observations to which, perhaps, their attention has not been directed with sufficient care. The very important question of the distance of the seat of the auroral light is not to be solved by an isolated observation, the inference from which rests on the hypothesis of the lower surface of the cloud having been horizontal.

M. Necker de Saussure observed auroras in Scotland at the close of the year 1839 and the beginning of 1840, and addressed to me an interesting communication on the subject, from which I extract the following details:—

"The auroras seen in Skye are far grander, more beautiful, and more complex than those seen near Edinburgh. At the latter place, they rarely reached the zenith; in Skye, on the contrary, they almost always passed beyond it and overspread the greater part of the heavens.

"The aurora of the 3rd of September, 1839, was exclusively confined to the south part of the heavens. It is the only instance of the kind which I ever saw.

"It happened frequently, both at Edinburgh and in Skye, that fine displays of aurora occurred on two consecutive evenings.

"On three occasions, I saw the aurora commence before night; the columns of vivid white light detaching themselves from the yellow and orange tint still prevailing in the west. This was at Skye, on the 4th of September, and the 28th of October, 1839, and the 4th of January, 1840.

"I never succeeded in hearing any particular sound,—even during the grandest and most vivid auroras at Skye, where the greatest calm and most profound silence reigned. Yet I collected in the Shetland Islands numerous attestations on the subject; the more remarkable, because they were altogether spontaneous, and in no way influenced by any previous question on my part.

"Persons of different conditions and employments, and living at places far apart in those islands, were all unanimous in saying that when the aurora has a great intensity, it is accompanied by a sound which all were equally unanimous in comparing to that of the flail when corn is thrashed.

"One of the persons employed under the Edinburgh Commissioners of Northern Lights, in taking meteorological observations at the lighthouse at Sumburgh Head, the southern extremity of Shetland, and who has consequently the habit of exact observation, told me, of his own accord, and without anything from me to lead him to it, that this noise was always distinctly heard; and he even added that he had heard it from the inside of one of the rooms in the lighthouse when the shutters were closed, and had said in consequence that there probably was an aurora, which was found to be the case.

"The auroras were several times accompanied by white frost, and the majority were followed by heavy falls of snow or rain, and by violent gusts of wind and tempests. Thus, in the latter respects, my observations would rather tend to confirm the generally received opinion in Scotland, that the northern lights are the forerunners of bad weather and violent gales.

"I have heard Professor J. D. Forbes say that near Edinburgh the fixed stars, even the largest, never appeared to scintillate, except when there was an aurora. My own observations generally confirmed this remark. It is true the fixed stars do not scintillate in these parts, or at least it is only rarely that I have seen, in stars of the first magnitude, a slight scintillation.

"In Skye, on the contrary, all the fixed stars are as bright, and scintillate as vividly, as in the finest evenings of France and Switzerland. It is the same in the rest of the Hebrides, in the Orkneys, the Shetland Islands, and generally along the west coast of the north of Scotland, and throughout the high region of the Highlands. Now, it is to be remarked that in all these parts there are no large towns, and no extensive coal-burning factories; the very sparse population of these solitary regions use for fuel only turf or wood, of which the very light smoke is immediately dispersed, and does not obscure the atmosphere, which is as pure as on the Continent of Europe. But in the Lowlands of Scotland, and on the eastern coast, and in the north-east, where towns, large villages, and factories abound, and where coal is the ordinary fuel, not only do the towns themselves and their immediate neighbourhood have their atmosphere darkened by a thick smoke which the wind drives in one or another direction, but even in the rural districts most distant from the towns, the air may still be perceived to be at all seasons very hazy, by reason of this coal smoke. It is so throughout England, and I would even say that having been pretty often at sea in the German Ocean, which washes the eastern coasts of the British Islands, I have always been struck by the want of clearness in the air, and the misty appearance of the shores. Nothing made it more evident to me that this depended on the coal smoke, than the fact that in looking from the Isle of Arran, and especially from the summits of its mountains, during the finest months of spring and early summer, in 1839, while Arran itself enjoyed the purest air and most serene sky, the opposite shores (the coasts of Ayrshire and Renfrewshire) had constantly over them a band of thick vapours, like a long grey cloud rising 1° or $1\frac{1}{2}$° above the horizon."

According to M. Cagigal, an aurora borealis was observed at Caracas on the 23rd of May, 1840. He remarks that although there have been some rare instances of this phenomenon having been observed at Cuba and at St. Domingo, he does not think that there is any other known example of its occurrence in so low a latitude as that of Caracas.

M. Wartmann has written to me from Geneva, that the periodical aurora of the 18th of October again showed itself very evidently on the 18th of October, 1841.*

At Paris, MM. Laugier and Goujon saw a well-characterised aurora on the 12th of November, 1841, about half-past eleven.

An aurora was also seen in France and Belgium, in the night between the 6th and 7th of May, 1843. Although it did not present anything unusual, yet I will extract from the account sent to the Academy, details which, when compared with accounts from distant places, may perhaps lead to useful conclusions. M. Quetelet wrote to me as follows:—

"During the daytime of the 6th of May, the march of the magnetometer had been very regular, and there was nothing to lead to the anticipation of the phenomenon which was to manifest itself in the evening. After ten o'clock, M. Beaulieu came to tell me, before he left for the night, that the needle showed a very sensible deviation, and I found it, indeed, in a state of extraordinary agitation. I wished to assure myself whether this derangement did not coincide with some meteorological

* [It is singular that this historical summary should contain no mention of the remarkable aurora of the 25th of September, 1841, observed simultaneously at Greenwich, at Toronto in Canada, and at Hobarton in Van Diemen Island, accompanied by disturbances of unusual magnitude in each of the three magnetic elements, observed with appropriate instruments at intervals of two minutes and a half for several hours, as well as at St. Helena and at the Cape of Good Hope, where the magnetic disturbance prevailed, but no aurora was visible. The details of these observations, published shortly after the occurrence of the phenomena, were extensively circulated throughout Europe and America, and attracted very general attention.]—Ed.

phenomenon, and I remarked that the horizon towards the north was strongly lit up; but the light of the moon would not yet permit me to pronounce as to the existence of an aurora.

"Whilst I was pursuing my observations at the magnetometer, which continued to be disturbed, they came to tell me that there was something extraordinary in the sky towards the south (this was at 11^h, 12^m, Mean Time). In the midst of a perfectly serene sky there was seen a kind of whitish cloud, of an elliptic shape, situated in the meridian, and at an altitude of about 60°. The cloud varied every instant in size and in brightness; its abrupt changes had something in them fatiguing to the eye, alternating from the faint light of the Milky Way to the strong illumination of a white cloud nearly effacing the light of the brightest stars situated in the same direction; whilst its outline was always undefined. I thought I saw in this phenomenon the kind of luminous cloud which generally accompanies very intense auroras; and on looking to the north I saw that it was indeed very vividly illuminated, and bright streamers were darting upwards to a considerable height in the magnetic meridian.

"As I had no one with me, and desired while observing the phenomenon also to follow the increasingly deviating indications of the magnetic instruments, it was impossible for me to obtain a view of all the circumstances. About twenty-four minutes after eleven the light which had shown itself in the south and in the meridian had entirely disappeared; and soon afterwards the northern part of the sky also returned to its ordinary state."

This aurora was seen at Paris from a quarter before to a quarter after eleven; its light was sufficiently powerful to vie with that of the full moon, which had not yet descended below the horizon. Two whitish arches were seen, through which the stars were visible. At Rheims differently coloured rays were seen. In the neighbourhood of Dieppe M. Nell de Bréauté, corresponding member of the Academy, saw in the north vertical bands very slightly tinged with orange.

On the 8th of December M. Colla, at Parma, saw a very fine aurora of a reddish colour, the elevation of which, at its most convex part, might be about 6 or 7 degrees. From it rose a luminous column of a yellowish colour, nearly in the direction of the meridian. White spherical-looking patches were, moreover, seen towards the south. The phenomenon was accompanied by a considerable magnetic perturbation of more than 18′.

On the 29th of December, at eight in the evening, an aurora of short duration was seen by M. Coulvier-Gravier.

We now come to 1847.

A brilliant aurora showed itself in the night of the 24th to the 25th of October. It was observed in the north of Germany, at Paris, in the Departement de l'Indre, at Bourges, at Parma in Italy, at Cadiz in Spain, and at Mount Eagle in Ireland. Its appearances were very variable.

At Leipsig, very prolonged rays formed, by their intersection, what it has been agreed to call the *cupola*.

At Paris, M. Faye remarked a whitish curtain, and a little above it a large grey cloud, which rose gradually, continually varying in its forms.

M. Faye, the Leipsig observers, &c., report that there rose from the horizon luminous streamers of a *very well characterised apple green;* but as these were bordered on either side by very vivid rose colour, the green may be supposed to have been an effect of contrast.

M. Faye saw with astonishment the fall of some widely scattered drops of rain; the zenith only was clouded.

M. Goujon satisfied himself that the aurora was attended by a great deviation of the Observatory needle. M. Colla observed the same effect at Parma.

M. Demidoff, at Cadiz, remarked that the luminous clouds always remained separated from the horizon by a zone of entirely clear sky, in which no more luminosity was seen than in any other point in the heavens; there is also reason to note the permanence and immobility of the same clouds when they had ceased to be luminous.

Mr. Cooper, at Mount Eagle in Ireland, saw some beautiful rose-coloured beams; they were paler towards the north, and were quite colourless to the east and west of north. The phenomenon was of great extent, and the point of convergence of the beams was not, on this occasion, in the magnetic meridian.

M. Coulvier-Gravier saw an aurora on the 1st of November, between nine and eleven o'clock.

On the 17th of December, thirty-five minutes after seven in the evening, when the moon shed a bright light, M. Rigault and several other persons, at La Ferté-sous-Jouarre, saw an aurora. It consisted of four patches of bright red, between the Great Bear and the Swan, passing through the Pole-star.

Our learned brother academician, M. de Gasparin, reported his observation of the same aurora as follows:—"I was proceeding to St. Symphorien-en-Laye (Loire) when I saw the sky near the zenith covered by a vast cloud of an intense crimson colour, which I might have taken for the reflection of a conflagration if I had not been aware (having just come down from the heights of Tarare, from whence I overlooked the whole country) that there was nothing of the kind. The cloud completely resembled those seen in the east a little before sunrise.

"As an aurora is reported to have been seen at Blangy (Seine Inférieure) on the same day and at the same hour, the colour of the cloud seen by me was probably caused by the reflection of the auroral light."

The aurora of this date was also observed—at Cirey, by M. Chevandier; at Bourges, by M. Levasseur; at Toulouse, by M. Petit; at Florence, by M. Demidoff.

A letter from Mr. Littrow informed me that on the 18th of October, 1848, an aurora was seen at Kremsmunster, and that during the time of its appearance the declination of the magnetic needle was considerably lessened.

A fine aurora was seen on the 17th of November at Cirey, Havre, Grenoble, Montpellier, Bordeaux, Parma, Venice, Florence, Pisa, and Madrid.

The following particulars were remarked at Montpellier:—

"It was at nine in the evening that the phenomenon reached the phase of its greatest beauty. A luminous band, a little resembling the first break of day, still occupied about 50 degrees of the horizon in the north, and a little more towards the west. In the lower part some clouds contrasted by their blackness with the light of the sky. Above the clouds a red light, at moments very vivid, rose to about 50 degrees of altitude, over an extent of 90 degrees in azimuth. The brightness of the luminous band increased until half-past nine, when it completely effaced the light of the stars in the Great Bear. No intermediate star could be distinguished between Polaris, Lyra, and Capella. The red cloud, in the midst of which Lyra shone with brilliant whiteness, appeared to vary in place, and to undergo changes of intensity.

"But the most remarkable spectacle was that of the beams, or streams of light, which at certain moments darted upwards in a nearly vertical direction, vanishing a few minutes later to re-appear at other points, and preserving during the whole time of their apparition perfect immobility. Some of these, which, as far as the eye could judge, were parallel to the magnetic meridian, reached almost to the zenith. Some among them were of a vivid red, and contrasted with the whiteness of the rest.

"At ten o'clock beams of light succeeded each other at short intervals; but, instead of rising parallel to each other, they appeared to diverge from a point situated below the horizon. The white light had diminished in intensity; the red clouds had extended themselves towards the west, and embraced a space of 150 degrees. The bright star in Aquila shone through the red light, which, towards the east, almost reached the constellation of Auriga.

"During this time the magnetic needle was observed with care, and we assured ourselves of a deviation towards the east of more than 1 degree. The needle showed not abrupt shocks, but slow and irregular variations. The aurora continued until the twilight of the morning made its last traces disappear."

The facts observed at Pisa are of great importance, and I therefore subjoin the whole of the letter written to me by M. Matteucci:—

"Permit me to send you the description of a very fine aurora which showed itself on the evening of the 17th with some rather singular circumstances.

"The sky was clear, and the stars shone brightly: for some days past the temperature of the air had been colder than is usual at this season. I had just passed through the town in going to the office of the Electric Telegraph, situated at the railroad station. As I went I had seen three very brilliant shooting stars pass over the sky in different directions; on the northern quarter a stratum of very thin clouds rested on the horizon, rising from 15 to 20 degrees above it, and diminishing in density in ascending. Nearly at half-past nine o'clock we were surprised, at the Telegraph Office, by the sudden suspension of the performance of the apparatus, which had been perfectly good throughout the day; the same thing happened at the same time at the Florence Station. We tried to get it to work by increasing the strength of the current, and by other means; but in vain. From time to time the needle moved by starts, and stopped again abruptly. The phenomena were exactly similar to those which take place during a thunderstorm.

"At fifty-five minutes after nine I went outside the office in order to observe the state of the sky, which I found continued clear, and I was struck by a reddish light in the northern quarter, above the clouds. I immediately asked the sentry how long it was since this light had appeared, and was told that it had begun to be seen a quarter of an hour before. I ran home as quickly as I could, in order to be able to observe the phenomenon better from a terrace about 130 feet high. The light continued to increase, both in intensity and extent, until half-past ten, when it was of a very intense sanguine red colour. The arrangement into the form of an arch, described in the greater number of observations of aurora, was not seen in this case. Instead of it there were large clouds of a more or less vivid red, sometimes detached and sometimes united, spreading from the north towards the east, and sometimes mounting to the zenith. I twice saw a long beam of lemon-coloured light rise through the red cloud, and issue from it, having its summit in the direction of the magnetic meridian. During the two or three minutes for which this beam lasted, it seemed to have a motion of alternate lengthening and contracting. Stars of the first magnitude were the only ones which were visible through the red auroral light. A very brilliant shooting star traversed the light in the direction from north to east, and almost parallel to the horizon. Gradually the red light diminished in intensity as it spread towards the east, and ten minutes before eleven o'clock it had completely disappeared.

"The sky, towards midnight, became covered by a slight mist. While the aurora lasted the barometric pressure was $30^{\text{in}}\cdot 1718$; the thermometer was $40^{\circ}\cdot 7$ Fahrenheit; Saussure's hygrometer marked 89°, and the wind was light from the south-west.

"The aurora had commenced before I put up the flame electrometer for atmospheric electricity on the terrace. During several minutes I obtained very strong indications of positive electricity; the leaf did but just touch the negative pillar, detach itself, re-touch it again, and so on. After midnight the indications of electricity had become scarcely sensible; the electro-magnetic instruments, which, until midnight, had ceased to act, resumed their ordinary march without the slightest alteration having been made either in the batteries or in the other apparatus."

M. Colla reported from Parma that the greatest brilliancy of the aurora occurred between ten and half-past ten. At certain moments it almost reached the zenith; it extended in a horizontal direction for more than 150°.

He added:—"The magnetic needle, by its extraordinary variations, had predicted the aurora some hours beforehand; the declination had sometimes diminished nearly a degree, and near midnight the diminution even exceeded a degree. While the aurora was most considerable the needle was in continual movement. The magnetic disturbance was renewed on the following day."

Mr. Highton, telegraphic engineer of the London and North Western Railway, on the occasion of this same aurora, remarked the considerable action exercised on the electric telegraph. He says:—"A telegraph passing through the Watford Tunnel, and of which the wires extend 1300 feet beyond the tunnel on one side and 2600 feet on the other, was rendered entirely useless for three hours. The magnet was constantly thrown back on the same side. Such an effect from auroras is usual. It has sometimes shown itself in the daytime, when no aurora was visible; and in one case I was able to follow its action from Northampton, through Shapstone, to Peterborough, on the Eastern line to London."

CHAP. XVII.

CONCLUSION.

Sometimes an interval of several successive years elapses, in which, in the temperate regions, few or no auroras are seen, and in which, in the polar regions, their frequency is proportionally diminished. The true cause of these vicissitudes is entirely unknown to us; but is not this an additional reason for carefully noting all circumstances connected with the appearances of so singular a phenomenon? As the scientific journals which record in different countries the auroras seen there, are not accessible to the greater number of physicists, I have thought it useful to science to publish tables which I had at first drawn up solely for my own private use.

I think the inquiry which I have gone into in the present notice, can leave no remaining doubts as to the intimate connection of aurora borealis and magnetism, and that this magnificent luminous phenomenon is thus seen to connect itself with electricity. It has just been seen by the reader that the action which from 1819 I had announced as exercised both by visible and by invisible auroras on the magnetic needle, also makes its influence sensible on the electric telegraph. My discovery can no longer be contested. I should add, however, that so early as 1827, I had established from the comparison of the movements of the magnetic needles at Kasan, St. Petersburg, Berlin, Freiberg, and Paris, that there is a simultaneity of action of the aurora borealis on the whole of terrestrial magnetism. According to the fine expression of my friend Alexander von Humboldt, "the magnetic tempests announce themselves by the perturbations of the magnetic needle, even when no traces of their luminous manifestation are seen on the celestial vault."

INDEX.

1. THUNDER AND LIGHTNING.

2. ELECTRO MAGNETISM.

3. ANIMAL ELECTRICITY.

4. TERRESTRIAL MAGNETISM.

5. AURORA BOREALIS.

INDEX TO THE PRINCIPAL NOTES ADDED BY THE EDITOR.

* Erratum in this note. For "Coquette" read "Coquille."

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