Max Planck Research Group
Epistemes of Modern Acoustics

Sound & Science: Digital Histories

Smithsonian Institution. Scientific Writings of Joseph Henry, Volume 1. Washington D.C.: The Smithsonian Institution, 1886.

https://acoustics.mpiwg-berlin.mpg.de/node/484

Scan licensed under: CC BY-SA 3.0 DE | Max Planck Institute for the History of Science

MAX PLANCK INSTITUTE
FOR THE HISTORY OF SCIENCE

SCIENTIFIC WRITINGS

OF

JOSEPH HENRY.

VOLUME I.



WASHINGTON:
PUBLISHED BY THE SMITHSONIAN INSTITUTION.
1886.

Ex
Biblioth. Regia
Berolinenſi.

STEREOTYPED AND PRINTED
BY JUDD & DETWEILER,
WASHINGTON, D. C.

INTRODUCTORY NOTE.

In these volumes the principal scientific writings of Joseph Henry are collected for the first time. They include his contributions to various societies and journals, together with notices of a few of his earlier communications which were never published in full. These productions, extending over a long and busy life, are naturally grouped under two periods: the first comprising the record of his researches from 1824 to 1846 (a period of 23 years), during his professorial career at Albany and Princeton; the second that of his scientific work from 1847 to 1878 (a period of 32 years), during his directorship of the Smithsonian Institution at Washington.

It will be observed that Professor Henry's contributions to science were given to the world from time to time throughout more than half a century, and published in widely remote places. Most of them are now scarce, many are practically inaccessible, hardly any individuals and few public libraries can be supposed to possess them all. It is noteworthy, and indeed is characteristic of their author, that he sedulously abstained from publishing any of his researches of the later period or reproducing any of the earlier ones—very important though he knew them to be—through the inviting channel of the "Smithsonian Contributions," or "Miscellaneous Collections," or in any way at the expense of the Smithsonian fund.

It has seemed to the Regents of the Smithsonian Institution that justice to the scientific name and memory of their distinguished Secretary who made the Institution

what it is, no less than a due regard to the history of physical science in this country, and the interests of its present votaries, require that these writings should now be collected and made available: also that their publication and distribution may be fittingly undertaken by the Smithsonian Institution. Accordingly, at a meeting of the Regents, held January 17, 1883, "Dr. Maclean having called the attention of the Board to the fact that the sundry papers of Professor Henry on scientific subjects had not been published in the series issued by the Smithsonian Institution, it was *Resolved*, That the Secretary be requested to have the scientific writings of Prof. Joseph Henry collected and published." At the next stated meeting of the Board, held January 16, 1884, Dr. Asa Gray, Hon. W. L. Wilson, and Prof. S. F. Baird were appointed a committee to supervise the publication of Prof. Henry's writings.

It was decided by them to include in this collection only the published writings of Prof. Henry, and to arrange these chronologically. A departure from this arrangement has been made in transferring the series of papers detailing Henry's extended observations on the phenomena of sound from the position of their successive dates of publication (1873 to 1877), and interpolating them between papers published in 1855. This has been done for the purpose of equalizing the size of the two volumes. The second volume is thus made to commence with the series of Meteorological Essays, which, also published originally in successive years (from 1855 to 1859), are here presented continuously. These Essays, although of a more popular character than the other writings of the author, contain much original observation and generalization, and therefore well deserve a place in this collection.

CONTENTS.

VOLUME I.

PART I. (1824–1846.)

	Year.	Page.
Chemical and mechanical effects of steam	(1824)	1
Refrigeration by rarefaction of air	(1825)	1
Lecture on flame	(1827)	2
On some modifications of the electro-magnetic apparatus	(1827)	3
Topographical sketch of the State of New York	(1829)	8
On the application of the principle of the galvanic multiplier to electro-magnetic apparatus, &c.	(1831)	37
Account of a large electro-magnet, made for Yale College	(1831)	50
On a reciprocating motion produced by magnetic attraction and repulsion. [An electro-magnetic engine.]	(1831)	54
On a disturbance of the earth's magnetism, in connection with the appearance of an aurora-borealis	(1832)	58
The production of electrical currents and sparks from magnetism. [Magneto-electricity.]	(1832)	73
On electrical self-induction in a long helical wire		79
Contributions to electricity. No. I. Description of a convertible galvanic battery	(1835)	80
Facts in reference to the spark, &c., from a long conductor	(1835)	87
On the action of a spiral conductor	(1835)	89
Contributions to electricity. No. II. On the influence of a spiral conductor in increasing electrical intensity, &c.	(1835)	92
Notice of electrical researches: The lateral discharge	(1837)	101
On the lateral discharge of electricity	(1838)	105
Induction currents from ordinary electricity	(1838)	105
On induced currents from ordinary electricity	(1838)	106
Contributions to electricity. No. III. On electro-dynamic induction	(1838)	108
Sect. 1. Conditions which influence self-induction		111
Sect. 2. Conditions which influence secondary currents		114
Sect. 3. Induction of secondary currents at a distance		119

	Year.	Page.
Sect. 4. Effects of interposing different substances between conductors		122
Sect. 5. On induced currents of the third, fourth, and fifth order		127
Sect. 6. Induced currents of the different orders from ordinary electricity		132
Capillary transmission through solids	(1839)	146
Letter on electrical induction	(1839)	147
Contributions to electricity. No. IV. On electro-dynamic induction	(1840)	149
Sect. 1. Induction at the beginning of a galvanic current		151
Sect. 2. On apparently two kinds of electro-dynamic induction		161
Sect. 3. Theoretical considerations on the preceding phenomena		169
On a reciprocating motion produced by galvanic attraction and repulsion. [A galvanic engine.]	(1840)	189
On the evolution of electricity from steam, &c.	(1840)	189
Experiments on phosphorescence	(1841)	191
On a simple form of heliostat	(1841)	192
On the effects of a thunder-storm	(1841)	193
Contributions to electricity. No. V. On electro-static induction, and the oscillatory discharge	(1842)	200
Experiments on phosphorescence	(1843)	204
On a new method of determining the velocity of projectiles. [An electrical chronograph.]	(1843)	212
On the application of the thermo-galvanometer to meteorology	(1843)	215
The theory of the discharge of the Leyden-jar		216
On the cohesion of liquids	(1844)	217
On the origin and classification of the natural motors	(1844)	220
On a peculiar action of fire on iron nails	(1845)	224
Observations on the relative radiation of the solar spots	(1845)	224
On the capillarity of metals	(1845)	228
On the protection of houses from lightning	(1845)	231
On color-blindness. [A review.]	(1845)	233
Experiments on the electrical discharge	(1845)	240
Experiment on the magnetic polarization of light	(1846)	243
On the relation of telegraph lines to lightning	(1846)	244
On the "fountain-ball;" and on the interference of heat	(1846)	254
On the atomic constitution of matter	(1846)	255
On the height of the aurora	(1846)	260

PART II. (1847-1878.)

	Year.	Page.
Programme of organization of the Smithsonian Institution	(1847)	263
On heat, and on a thermal telescope	(1848)	283
On the practical operations of the Smithsonian Institution	(1848)	284
Remarks on the aurora-borealis	(1849)	287
Remarks on the diffusion of vapor	(1849)	288
On the radiation of heat	(1849)	289
Remarks on the expansive energy of lightning strokes	(1850)	290
Remarks on the forms of lightning-rods	(1850)	291
On the phenomena of the Leyden-jar	(1850)	293
On the limit of perceptibility of a direct and reflected sound	(1851)	295
On the theory of the so-called "imponderables"	(1851)	297
The improvement of the mechanical arts. [An address.]	(1853)	306
Thoughts on education. [An address.]	(1854)	325
On the mode of testing building materials, &c.	(1855)	344
On molecular cohesion		352
The effect of mingling radiating substances with combustible materials	(1855)	355
Experiments on the alleged spontaneous separation of alcohol and water	(1855)	360
Remarks on the United States light-house service	(1873)	364
Researches in sound, in relation to fog-signalling	(1874)	370
Introduction		370
Part I. Investigations from 1865 to 1872	(1874)	378
Part II. On some abnormal phenomena of sound	(1874)	416
Part III. Investigations during 1873 and 1874	(1874)	426
Part IV. Investigations in 1875	(1875)	447
Part V. Investigations in 1877	(1877)	487

LIST OF ILLUSTRATIONS IN VOLUME I.

	PAGE.
Schweigger's galvanic "multiplier" (2 figures)	4
Modification of De la Rive's galvanic ring	5
Modification of Ampère's double galvanic rings	6
Modification of Sturgeon's galvanic dipping needle (2 figures)	6
Action of galvanic dipping needle (2 figures)	7
Another modification of the galvanic ring	7
Frame for testing a strong electro-magnet	43
Electro-magnetic engine	55
A convertible galvanic battery (for "quantity" or "intensity")	81
A single element of the galvanic battery	82
Homogeneous connector for "quantity" galvanic battery	82
Alternate or serial connector for "intensity" galvanic battery	83
Plan of the galvanic battery	84
Lateral electrical induction of parallel wires	102
Coils of copper ribbon to develope electrical induction	109
Wire coils to develope electrical induction	110
Arrangement of ribbon coil "No. 1," and wire helix "No. 1"	115
Arrangement of ribbon coil "No. 1" and wire helix "No. 4,"	119
Arrangement of coil and wire helix, with interposed metallic plate	123
Circular plate of lead with segment cut out	124
The same, with edges connected by a magnetizing spiral	124
Arrangement of two pairs of induction coils	127
Arrangement of three pairs of induction coils	129
Another arrangement of double pairs of induction coils	129
Glass cylinder, Leyden jar, and magnetizing spiral	133
Arrangement of double induction coil with bell-glass interposed	136
Arrangement of wires for common electrical induction at a distance	139
Disruptive discharge through detached pieces of tinfoil	142
Diagrams showing character of the successive inductions	143
Arrangement of double induction coils	152
Curve to represent induced electrical current	171

PAGE.

Curve to represent electrical induction 175
Do. do. 176
Do. do. 181
Diagram showing the effect of a favoring wind on sound . . 449
Diagram showing effect of adverse wind on sound . . . 449
Diagram showing the effect of a compound wind on sound . . 450
Sketch map of Block Island 459
Curve of audition, August 10th, 1875 468
Sketch map of Little Gull Island and vicinity . . . 470
Curve of audition, September 2d, 1875 475
Do. " 3d, " 478
Do. " 4th, " 479
Do. " 6th, " 480
Do. " 7th, " 481
Do. " 8th, " 482

SCIENTIFIC WRITINGS OF JOSEPH HENRY.

PART I.

FROM 1824 TO 1846.

SCIENTIFIC PAPERS AND ABSTRACTS.

CHEMICAL AND MECHANICAL EFFECTS OF STEAM.

(Proceedings of the Albany Institute, vol. 1, p. 30.)*

October 30, 1824.

Joseph Henry read a communication on the chemical and mechanical effects of steam, with experiments designed to illustrate the great reduction of temperature in steam of high elasticity when suddenly expanded.

REFRIGERATION BY RAREFACTION OF AIR.

(Proceedings of the Albany Institute, vol. 1, p. 39.)

March 2, 1825.

Joseph Henry read a communication on the production of cold by the rarefaction of air, accompanied with experiments.

One of these experiments most strikingly illustrated the great reduction of temperature which takes place on the sudden rarefaction of condensed air. Half a pint of water was poured into a strong copper vessel, of a globular form, and having a capacity of five gallons—a tube of one-fourth of an inch in caliber, with a number of holes near the lower end

---

*[The "Transactions of the Albany Institute," vol. 1, part i, is dated on page 3, "June, 1828." The title page of the volume bears date "1830." Part ii of same volume is an "Appendix" of 74 (independently numbered) pages, and comprises brief abstracts of proceedings; here cited as "Proceedings" for conciseness.]

and a stop-cock attached to the other extremity, was firmly screwed into the neck of the vessel; the lower end of the tube dipped into the water, but a number of the holes were above the surface of the liquid, so that a jet of air mingled with water might be thrown from the fountain. The apparatus was then charged with condensed air, by means of a powerful condensing pump, until the pressure was estimated at nine atmospheres; during the condensation the vessel became sensibly warm. After suffering the apparatus to cool down to the temperature of the room, the stop-cock was opened; the air rushed out with great violence, carrying with it a quantity of water, which was instantly converted into snow; after a few seconds the tube became filled with ice, which almost entirely stopped the current of air. The neck of the vessel was then partially unscrewed, so as to allow the condensed air to rush out around the sides of the screw; in this state the temperature of the whole atmosphere was so much reduced as to freeze the remaining water in the vessel; the stop-cock and tube at the same time became so cold that the fingers adhered to them, in the same manner that they are sometimes found to stick to the latch of a door on an intensely cold morning. This experiment was exhibited to the Institute within six feet of a large stove, and in a room the temperature of which was not less than eighty degrees of Fahrenheit's thermometer.

LECTURE ON FLAME.

(Proceedings of the Albany Institute, vol. I, p. 59.)

March 21, 1827.

Mr. Joseph Henry delivered a lecture on flame, accompanied with experiments.

ON SOME MODIFICATIONS OF THE ELECTRO-MAGNETIC APPARATUS.

(Transactions of the Albany Institute, vol. I, pp. 22–24.)

Read October 10, 1827.

The subject of electro-magnetism, although one of the most interesting branches of human knowledge, and presenting at this time the most fruitful field for discovery, is perhaps less generally understood in this country than almost any other department of natural science.

Our popular lecturers have not availed themselves of the many interesting and novel experiments with which it can so liberally supply them; and, with a few exceptions, it has not as yet been admitted as a part of the course of physical studies pursued in our higher institutions of learning. A principal cause of this inattention to a subject offering so much to instruct and amuse is the difficulty and expense which formerly attended the experiments—a large galvanic battery, with instruments of very delicate workmanship, being thought indispensable. But this bar to the advancement of electro-magnetism no longer exists, several improvements having been made in the principles and arrangement of the apparatus, which tend considerably to simplify its construction and use. Mr. Sturgeon, of Woolwich, who has been perhaps the most successful in these improvements, has shown that a strong galvanic power is not essentially necessary, even to exhibit the experiments on the largest scale. On the contrary, he has proved that it may be almost indefinitely diminished, provided the magnetic force be proportionately increased. On this principle he has constructed a set of instruments, with large magnets and small galvanic elements, which, from their size and the facility of their operations, are well calculated either for the private study or the public lecture room.*

Mr. Sturgeon's suite of apparatus, though superior to any other, as far as it goes, does not however form a complete

*Annals of Philosophy, new series, vol. 12, page 375.

set; as indeed it is plain that his principle of strong magnets cannot be introduced into every article required, and particularly into those intended to exhibit the action of the earth's magnetism on a galvanic current, or the operation of two conjunctive wires on each other. To form therefore a set of instruments, on a large scale, that will illustrate all the facts belonging to this science, with the least expense of galvanism, evidently requires some additional modification of the apparatus, and particularly in those cases in which powerful magnets cannot be applied. And such a modification appears to me to be obviously pointed out in the construction of Prof. Schweigger's Galvanic Multiplier:* the principles of this instrument being directly applicable to all the experiments in which Mr. Sturgeon's improvement fails to be useful; and to those only can it be successfully applied. The following description of the figures in *Plate* I† will render my meaning sufficiently clear.

Fig. 1, is an apparatus on the plan of the Multiplier, to show the deflection of a large magnetic needle. It consists of a coil of wire, A B, of an oblong form, about ten inches in length and one and a half in width, with a small galvanic element attached to each end; the coil is formed of about twenty turns of fine copper or brass wire, wound with silk,

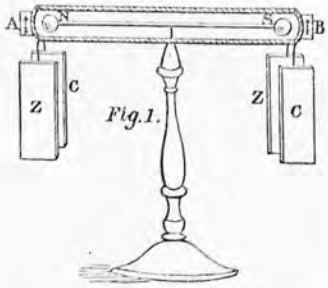


Fig. 1.

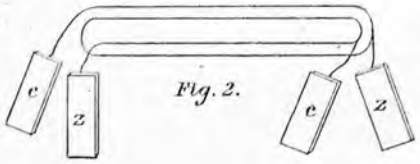


Fig. 2.

to prevent contact, and the whole bound together so as to have the appearance of a single wire. The attachment of the zinc and copper is more plainly shewn in Fig. 2, which

*See Green's Electro-Magnetism, page 30.

†[The figures, copied from the original copper-plate illustration, are here reproduced in the text for facility of reference.]

represents a coil of only two turns of wire: on the left side of the figure the plates are soldered directly to the ends of the wire of the coil; on the right, the plate of zinc Z, is attached to the part of the wire ending with copper on the other side, while the plate of copper on the right corresponds to the zinc on the left. By this arrangement, we can instantly reverse the direction of the currents, and deflect the needle either to the right or left, by merely holding a tumbler of acidulated water so as to immerse one or the other of the double plates into the fluid. The arrows at B, formed of two pieces of card, are intended to show the direction of the currents, and they should point in the course of the wires going from the copper. N S, is the needle, about nine and a half inches long, made by binding together several watch springs, touched separately, so as to form a compound magnet; at the ends are two balls of pith, to show the movement of the needle more plainly. This instrument is complete in itself, and we receive the full effect of the instantaneous immersion of the galvanic element.

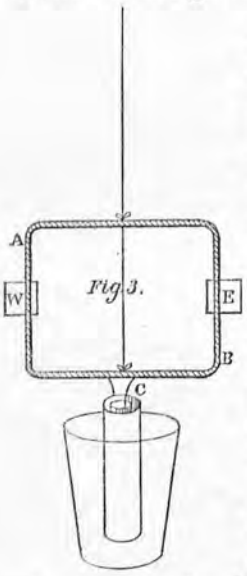


Fig 3.

Fig. 3, represents a modification of De la Rive's ring on a large scale. A B, is a coil about nine inches by six, with a small cylinder of copper, enclosing another of zinc, without bottoms, soldered to its extremities, which end at *c*, the whole being suspended by a fibre of raw silk, so as to swing freely in a cup of acidulated water. When this apparatus is made sufficiently light, it invariably places itself, after a few oscillations, at right angles to the magnetic meridian. W and E, are two pieces of card, with letters on them, to show which side of the coil will turn to the east or west: they may be properly placed by recollecting that the current from the copper to the zinc has a tendency to circulate in a direction contrary to that of the sun.

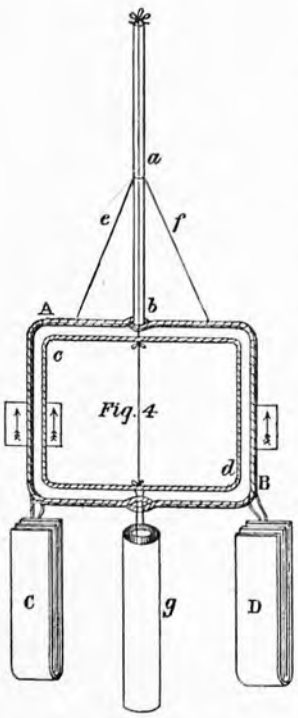


Fig. 4.

Fig. 4, is designed to show the action of two conjunctive wires on each other; A B, is a thick multiplying coil, with galvanic plates attached, in the same manner as shown in Fig. 2; *c d*, is a lighter coil, with a double cylinder, precisely similar to Fig. 3, and suspended within the other by a fibre of silk, passing through a glass tube, (*a*) the end of which is inserted into an opening (*b*) in the upper side of A B; *e f* are two wires supporting the glass tube. When the cylinder *g* and the plates C are placed in vessels of acidulated water, the inner coil will immediately arrange itself so that the currents in both coils will circulate the same way: if the vessel be removed from C, and D placed in the fluid, the coil *c d* will turn halfway round and again settle, with the currents flowing in

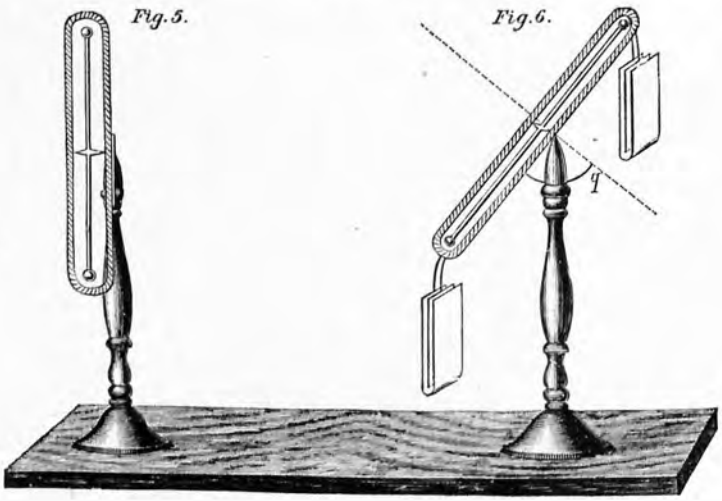


Fig. 5.

Fig. 6.

the same direction. Instead of the cylinder, a separate battery of greater power may be used, by suspending the inner coil, as shown in Fig. 9; *h h* are cups with mercury—the upper wire should turn on a fine steel point.

Fig's 5 and 6, are front and side views of a modification of an instrument, described by Mr. Sturgeon. It consists of a dipping needle, surrounded by a multiplying coil, turned edgewise, but in all other respects similar to that of Fig. 1. If, when the needle is placed in the magnetic meridian, and the coil in the plane of the dip, a galvanic current be passed through it in a direction opposite to that of the sun, the north end of the needle will turn up, as in Fig. 7; but if in

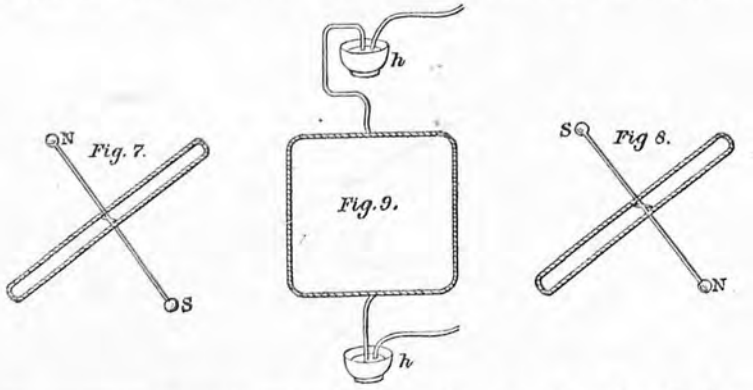


Fig. 7. Fig. 9. Fig 8.

the contrary direction, it will turn down, as Fig. 8. If the coil be placed at right angles to the dip, as shown in the dotted line, Fig. 6, and the current passed in the first mentioned direction, the needle will not alter its position, but will be more firmly fixed in it: if passed in the contrary direction, it will turn half-way round and dip with its south end. The quadrant *q* permits the coil to be readily placed, either in the plane of the dip or at right angles to it.

TOPOGRAPHICAL SKETCH OF THE STATE OF NEW-YORK, DESIGNED CHIEFLY TO SHOW THE GENERAL ELEVATIONS AND DEPRESSIONS OF ITS SURFACE.

(Transactions of the Albany Institute, vol. I, pp. 87–112.)

Read October 28, 1829.

The Topography of the state of New-York, viewed either in relation to that of the continent of North America in general, or only in reference to the space included within its own political boundaries, presents many interesting and peculiar features.

The two great lakes, and their outlets, forming a natural boundary on the north and west; the continued chain of water communication of the Hudson and Lake Champlain, along the whole eastern section; the connected series of smaller lakes in the interior, together with several large streams which rise in the middle of the state, and pass through its southern boundary; all give to the surface of New-York a diversity of aspect, and a facility of internal navigation, possessed by no other section of our own country, and perhaps not surpassed by any of equal extent on the surface of the globe.

The eastern portion of the United States, designated by geographers as the Atlantic slope, is separated from the central part, or the great valley of the Mississippi, by a marked natural division, consisting of a continuous swell or ridge of land extending from Alabama to the south shore of Lake Ontario. This ridge is the true *water shed* of the country, and determines the course of the rivers falling into the Atlantic on the one side, and those into the Mississippi on the other. It has a mean height of about 3000 feet; and cannot be crossed at any point south of the state of New-York, by an elevation of less than two thousand feet above the ocean. Upon the acclivities of this ridge are based an indeterminate number of spurs, hills, and collateral subordinate ridges, which often rise to a much greater height than the crest of the *water shed*. These subordinate ranges are not continuous, but are often cut through by the Atlantic rivers: They have,

however, nearly the same direction as the main ridge; and in passing through North-Carolina and Virginia, assume the form of four principal ranges, nearly parallel to each other. The three westernmost of these mingle together in the northern part of Pennsylvania, and form a mountain chain, which diverges to the east from the great water shed, and in passing through the state of New-York, occupies the space between Seneca lake and the Hudson river. At first sight, it appears to terminate at the valley of the Mohawk; but it soon rises again on the north side of the river, and forms the mountain district between Ontario and Champlain; is afterwards cut through by the valley of the latter, and then passes on towards the sources of the Connecticut. The remaining ridge of the four parallel ones continues separate from the others, and suddenly turns to the east in Pennsylvania, crosses the state of New-Jersey, and is deeply cut through by the Hudson at West-Point, where it forms the highlands of that river. It afterwards passes to the north in nearly a straight line, and forms the dividing ridge between the waters of the Hudson and those of the Connecticut: at the sources of the latter, it mingles with the other mountain chain, and they then together pass on to the northeast, and may be traced even to the coast of Labrador. The opening between these ridges forms a long, deep, and narrow valley, in which is situated the part of the Hudson river between West-Point and Glen's Falls, and the whole of Lake Champlain. South of this state, the several collateral ridges are cut through by the Susquehanna, the Potomac, and several other streams of less magnitude, which rise near the crest of the water shed, and flow with a rapid descent to the ocean. This fact has been stated as something peculiar in the topography of our country, and has given rise to the fallacious hope of finding practicable canal passes through the river vallies from the waters of the Atlantic to those of the Mississippi; but the water shed, in its uninterrupted continuity, every where rises as an insuperable barrier, and the lowest pass yet found south of New-York is elevated more than 2000 feet above the ocean. As a whole, these mountains are known

by the name of the Appalachian system; but the parallel ridges are perhaps most generally referred to as the Alleganies; and these again, in their course, have received different local names, such as the Blue Ridge in Virginia, the Catskill in New-York, and the White Mountains in New-Hampshire. From the above sketch of the great mountain system of our country, the peculiar topographical features of the state of New-York will be readily understood.

The Appalachian system may be said to occupy the principal part of the state; and, indeed, through the whole district, the mountains appear to be only partially interrupted by the vallies of rivers, or depressed by the basins of lakes. The entire surface may perhaps be best described as an elevated tract of country, with indentations in various places below its general level. The most important depressions of the surface are the great basins in which are situated the Lakes Erie and Ontario, and the long narrow valley which contains the Hudson river and Lake Champlain. The two last are connected with each other by a valley occupied by the Mohawk river and the Oneida lake; and with it may be considered as separating the whole mountain system of this state into three principal divisions. The first of these, and the largest of the whole, occupies the space situated south of the Mohawk river and the Ontario valley, and between the Hudson river and Lake Erie. The second is the mountain district north of the Mohawk, and between Lake Champlain and the east end of Lake Ontario. The third division comprises that part of the mountain range on the east side of the Hudson river included within this state. The first division is separated into two parts, by the basins of Seneca and Cayuga lakes, and by an elevated valley extending from the head of the former to the valley of the Chemung or Tioga river, at Newtown.

The western subdivision, or the part of the state between Seneca lake and Lake Erie, is occupied by that portion of the mountain system which we have called the *water shed*. This, in its course from the south, in Pennsylvania and New-York, forms a high table land of about two thousand feet in

mean elevation. The highest part of it comprises the surface of the counties of Steuben, Allegany, Cattaraugus and Chautauqua; and a little to the north of these, it begins to decline, and finally descends, by three principal steps, to its terminations on the south shore of Lake Ontario. The great elevation and geographical importance of this table, may be inferred from the fact, that it gives rise to several streams of water, which find the level of the ocean at points almost as distant as the extremities of the continent. The head branches of the Allegany, of the Genesee, and of the Susquehanna, are all found inosculating with each other in the county of Allegany; while their waters separately mingle with the ocean in the gulf of St. Lawrence, the Chesapeake bay, and the gulf of Mexico. But the following heights, from actual survey, will serve to give a more definite idea of its general elevation.

Chautauqua lake, the largest* sheet of water on this table, and the most elevated of its size in the United States, is 1291 feet above the level of the ocean, and 723 feet higher than Lake Erie, although only eight miles distant: its discharged waters descend to the ocean, along the western declivity of the water shed, through the Ohio and the Mississippi rivers. The lowest pass to the east, over a swell of land near Casadaga outlet in Chautauqua county, is 1720 feet high; and another pass in the same swell is 1972 feet. The lowest notch in the height of land between Elm and Little Valley creeks, in Cattaraugus county, is 1725 feet; and between Little Valley and Big Valley, the lowest pass is 2144 feet above the level of the ocean. Franklinville has an elevation of 1580 feet, and Angelica 1428 feet, although both are situated in vallies. This height of land extends close to the shore of Lake Erie, as it may be seen by the map, that one of the head branches of the Allegany, a tributary of the Ohio, rises within four or five miles of the lake. The surface is not broken, but consists of large swells of land, with broad shallow vallies intervening. The principal indenta-

*It is 18 miles long, contains 16,000 square acres, and discharges 2295 cubic feet of water per minute.—*Whippo's Report.*

tion of the surface, is the valley of the Genesee river, which may be considered as an arm of the Ontario valley, extending into the state of Pennsylvania. The extreme southern branches of this river rise at an elevation of more than 2500 feet.

The space between Seneca lake and the Hudson, and south of the Mohawk, is occupied by the mountain chain formed by the union of the three parallel ridges before mentioned, as mingling in Pennsylvania, and passing through New-York. The surface is much more uneven than that of the part just described, and presents the general appearance of a number of ridges in a north and south direction. The highest of these is the Catskill mountains, which bound the valley of the Hudson on the west, and rise in some places nearly 4000 feet higher than the level of the ocean. The Round Top is 3804, and the High Peak is 3718 feet, above the level of the tide waters of the Hudson.* The principal indentations of the surface of this subdivision of the mountain part of the state, are the vallies of the Susquehanna, the Delaware, and their several branches. By a reference to the map, it will be seen that the Chemung river, the main branch of the Susquehanna, and the Delaware river, when viewed in connexion with each other, present an almost entire water course, extending along the Pennsylvania line, from Painted Post, in Steuben county, to the northwest angle of the state of New-Jersey, the only interruption being the space between the Delaware and the Susquehanna. The vallies in which these rivers are situated, cross the mountains in an east and west direction; but their several tributaries, viz., the two branches of the Susquehanna, the Unadilla and the Chenango rivers, the Owego and the Cayuta creeks, besides several smaller streams, descend to the south, and intersect the principal vallies in a remarkable manner, nearly at right angles to their general course. These streams all rise on a narrow table land, which is situated a little south of the line of the Erie canal, and may be traced on the map as forming the water shed, between the heads of streams

*As measured by Capt. Patridge.

flowing to the north and the south, in an uninterrupted course, from the Catskill mountains to the head of Seneca lake. Along the summit of this table land, are a number of small, but highly elevated lakes, which give a peculiar character to this region. The first of these, from the east, and the largest of the whole, is Otsego lake, the outlet of which forms the Susquehanna river. It is a beautiful sheet of water, surrounded by high hills; is nine miles in length, three in breadth, and elevated 1193 feet above the surface of the ocean. The next is Schuyler's lake, which also gives a branch to the Susquehanna: It is situated a few miles to the west of Otsego lake, in the same county; its exact elevation is not known, but it cannot be less than 1200 feet. The other lakes worthy of notice on this table land, are Cazenovia, Skaneateles and Owasco. These are on the northern declivity, and discharge their waters to the north: they are scarcely as much elevated as the two just mentioned; the first being about 900 feet, the second 840, and the last 670 feet above the level of the ocean. It might be supposed, by an inspection of the map, that Cayuga and Seneca lakes were also highly elevated on this table land; but this is not the case, as the former is only 387 and the latter 447 feet above the level of tide. They in reality occupy two long narrow ravines, which deeply indent the surface of the adjacent country, and are separated from each other by a ridge which rises to the height of more than 800 feet above Cayuga lake. The smaller lakes above mentioned are situated several hundred feet above the highest level of the Erie canal, and form inexhaustible reservoirs to supply it with water.

It may be here remarked, that this is an advantage possessed by no other canal route in this country, as it is a curious feature in the physical geography of the United States, that except in the swamps along the southern sea coast, no lake is to be found east of the Mississippi and south of the latitude of the southern boundary of New-York, while almost every river north of this degree issues from a lake or a pond.*

*Gallatin's Report.

The following tables of ascents and descents will serve to give a correct idea of the general configuration of the surface of the whole of the first division of the state, or that part situated between the Hudson and Lake Erie.

No. 1, is a section in an east and west direction from the Hudson to Lake Erie. It commences at the level of tide in the river, and passes over the several ridges to the village of Bath, in Steuben county, and then crosses the high table land to Lake Erie.—No. 2, also begins on the Hudson, at Kingston landing, and follows principally the valleys of streams along the Pennsylvania line to Bath, where it intersects with No. 1.—Nos. 3, 4, 5, 6, 7, 8 and 9, are sections at right angles to Nos. 1 and 2. The five last, pass from points on the south shore of Ontario up the slope of the great depression which contains this lake, to the summit of the table land, and then down the valley of streams to the Susquehanna and the Allegany rivers.—No. 3, is from a point in the valley of the Mohawk, and passes over the ridge to the head waters of the Susquehanna, and then descends this river to the Pennsylvania line.—No. 4, extends entirely across the state, from the St. Lawrence to the Susquehanna river, and exhibits the deep depression of the Mohawk valley below the level of the ridges on each side.

The several distances given in these tables are in most cases straight lines, measured from point to point on a map, but the elevations are all from actual surveys, made at the expense of the state.

The elevations in table No. 1, between the Hudson river and Bath, are from the survey of William Morell, Esq. The remaining elevations of this table, as well as those in No. 2, are from the personal survey of the writer of this article. The elevations in both these tables were taken under the direction of Messrs. Hammond, Morell and Pitcher, as commissioners to explore the route of a state road through the southern tier of counties, in 1825. No. 3, is from the survey of Dr. William Campbell and De Witt Clinton, Jun. The remaining six tables were taken from the reports and maps of Messrs. Geddes, Roberts, Hutchinson, Young and Whippo,

engineers employed by the canal commissioners to explore the routes of 15 proposed canals, in 1825.

It must be premised with regard to these heights, that as they are points on routes explored for roads and canals, they are the elevations of the lowest passes near the line of survey, and are consequently less than the general height of the several ridges.

No. I.—*Table of Ascents and Descents across the Ridges from Catskill, on the Hudson, to the Village of Bath, in Steuben County, and thence to Lake Erie.*

Route.	Miles.		Feet.	
From the Hudson river, at Catskill, to Madison village		4	rises	184
Cairo	7	11	rises 226	410
Shinglekill at Cairo	0	11	falls 40	370
Catskill mountain summit	13	24	rises 1542	1912
Valley of the Schoharie at Gilboa	10	34	falls 742	1170
Head waters of the Delaware	10	44	rises 716	1886
Delhi on the Delaware	18	62	falls 502	1384
Height of land between the Delaware and Susquehanna	5	67	rises 759	2143
Susquehanna river at the junction of the Oleout creek	17	84	falls 1143	1000
Unadilla river one mile above its junction with the Susquehanna	5	89	falls 27	973
Between Unadilla and Chenango	6	95	rises 657	1630
Valley of the Chenango at Oxford	6	101	falls 669	961
Between Chenango and Tioughnioga or Homer river	13	114	rises 133	1094
Valley of the Tioughnioga at the junction of the Otselic	6	120	falls 159	935
Between Tioughnioga and Owego creek	8	128	rises 445	1380
Valley of the Owego at Richford	7	135	falls 285	1095
Between the Owego and the deep valley of Cayuga lake	4	139	rises 275	1370
Valley of the Cayuga lake at Ithaca	10	149	falls 962	408
Between the Cayuga valley and the Seneca inlet at Catharine landing	11	160	rises 849	1257
Catharine landing	7	167	falls 801	456
Between the Seneca valley and Mud creek, a branch of the Conhocton	9	176	rises 1188	1644
Valley of Mud creek one mile below Mud lake	4	180	falls 528	1116
Between Mud creek and Conhocton	6	186	rises 463	1579
Conhocton valley at the village of Bath	4	190	falls 489	1090
Between Conhocton and Canisteo	7	197	rises 750	1840
Canisteo valley at Arkport	9	206	falls 646	1194
Between the Canisteo and Genesee	8	214	rises 868	2062
Genesee valley at Angelica	10	224	falls 634	1428
Between the Genesee valley and Oil creek	13	237	rises 59	1487

No. I.—*Table of Ascents and Descents, &c.*—Continued.

Route.	Miles.		Feet.		
Oil creek valley, a tributary of the Allegany	2	239	falls	39	1448
Between Oil creek and Ellicottville	12	251	rises	696	2144
Ellicottville, on a tributary of the Allegany	8	259	falls	630	1514
Between Ellicottville and the Conewango	3	262	rises	621	2135
Conewango valley at the junction of Clear creek,	15	277	falls	885	1250
Between Conewango valley and Chautauque lake	8	285	rises	716	1966
Chautauque lake	18	303	falls	675	1291
Between Chautauque and Lake Erie	1	304	rises	61	1352
Lake Erie at Portland harbor	7	311	falls	787	565

No. II.—*Table of Ascents and Descents from the Hudson, at Kingston Landing, to Bath, in Steuben County, by the route of the valleys of the Rondout Creek, the Beaver Kill, the east branch of the Delaware, and the east and west branches of the Susquehanna.*

Route.	Miles.		Feet.		
Hudson river, at the junction of the Rondout, to Kingston village		2	rises	188	188
Warwasing	21	23	rises	123	311
Sullivan county line on the Rondout	10	33	rises	462	773
Height of land between the Rondout and Neversink	6	39	rises	896	1669
Neversink river	2	41½	falls	357	1312
Height between the Neversink and Beaver kill	2½	44	rises	768	2080
Junction of the Beaver kill and the east branch of the Delaware	24	68	falls	1062	1018
Junction of the east and west branches of the Delaware	7	75	falls	96	922
Deposit on west branch of the Delaware	11	86	rises	82	1004
Height of land between the Delaware and the Susquehanna	6	92	rises	684	1688
Susquehanna at Windsor	4	96	falls	775	913
Height across the Great Bend of the Susquehanna	5	101	rises	644	1557
Binghampton on the Susquehanna	9	110	falls	721	836
Owego on the Susquehanna	18	128	falls	32	804
State line above Tioga Point	15	143	falls	19	785
Newtown on the Chemung or Tioga	13	156	rises	51	836
Painted Post at the junction of Tioga and Conhocton	14	170	rises	106	942
Bath on the Conhocton	17	187	rises	148	1090

NOTE.—The numbers in the first column of figures are the distances from point to point—those in the second, are the total distances. The third column of figures gives the ascents and descents; and the fourth, the elevations of the several points above the level of tide water in the Hudson.

The last six stations in the above table, or those from Binghamton to Bath inclusive, are along the valley of the two great branches of the Susquehanna. The elevations opposite these stations give 900 feet as the mean height of the bottom of this valley, but the mountains on each side rise from five hundred to a thousand feet higher. These mountains are some of the high ridges whose elevations are given in table No. 1, and which here retain about the same elevation.

No. III.—*Table of Ascents and Descents from the valley of the Mohawk, through Otsego Lake, and down the valley of the Susquehanna to the Pennsylvania line.*

Route.	Miles.		Feet.		
Fort Plain, on Erie canal					304
Lake Summit, in Springfield		16½	rises	1048	1352
Head of Otsego lake	3	19½	falls	159	1193
Along Otsego lake to its outlet	9	28½	level		1193
Mouth of Oats creek	3½	32	falls	12	1181
Crippen's Ville, at the dam	12	44	falls	23	1158
Opposite the mouth of Charlotte river	7	51	falls	80	1078
Pennsylvania line	59	110	falls	178	900

No. IV.—*Table of Ascents and Descents on nearly a direct line from Ogdensburgh, on the St. Lawrence, to Binghamton, on the Susquehanna, by the way of the Black river, and across the valley of the Mohawk; thence to the head of the Chenango river; and down the same to its mouth.*

Route.	Miles.		Feet.		
Ogdensburgh, on the St. Lawrence					226
Indian river, near the village of Antwerp		34	rises	233	459
Black river, above the falls at the village of Carthage	14	48	rises	234	693
Along the valley of Black river to foot of High falls, near mouth of Moosic river	27	75	rises	10	703
Summit between Black river and the Mohawk, near Boonville	9	84	rises	432	1135
Erie canal at Rome, and highest part of the Mohawk and Oneida lake valley	18	102	falls	710	425
Head of Chenango valley, at Hamilton village	26	128	rises	730	1155
Along the Chenango river to the forks	42	170	falls	208	947
Binghamton, on the Susquehanna	10	180	falls	111	836

No. V.—*Table of Ascents and Descents from Lake Ontario along the Oswego River, through the Tully Lakes, and down the Tioughnioga River to the Susquehanna.*

Route.	Miles.		Feet.	
Lake Ontario, at mouth of Oswego river				231
Outlet of Onondaga lake		28	rises 130	361
Erie canal at Syracuse	7	35	rises 38	399
Tully lakes, town of Tully	18	53	rises 795	1194
Forks of the streams near Homer village	12	65	falls 98	1096
Chenango forks	29	94	falls 149	947
Junction of Chenango and Susquehanna, at Binghamton	10	104	falls 111	836

No. VI.—*Table of Ascents and Descents from Little Sodus Bay, on Lake Ontario, to Owego, on the Susquehanna, along Cayuga Lake and the valley of Owego Creek.*

Route.	Miles.		Feet.	
Little Sodus bay, on Lake Ontario				231
Montezuma, on the Erie canal		31	rises 49	380
Outlet of Cayuga lake	6	37	rises 7	387
Along the lake to its head	36	73	level	387
Summit between Cayuga lake and Owego creek	6	79	rises 594	981
Susquehanna, at Owego	20	99	falls 185	796

The elevation of Owego, according to table No. 2, is 804 feet, which differs eight feet from that given in the above table. This small discrepancy is owing to the circumstance of the elevations in these two tables being the results of surveys entirely independent of each other, and which intersect at Owego, after a circuit from the Hudson, of more than 300 miles. Table No. 2, also intersects with No. 4, at Binghamton, and with No. 7, at Newtown. At the former place the difference was only the fraction of a foot, and at the latter less than two feet. These facts show with what precision measurements of this kind can be made, and what reliance may be placed on the correctness of the elevations of the several points given in these tables.

No. VII.—*Table of Ascents and Descents from Great Sodus Bay, on Lake Ontario, along Seneca Lake and the route of the Chemung Canal to Newtown, on the Chemung or west branch of the Susquehanna River.*

Route.	Miles.		Feet.		
Lake Ontario, at Great Sodus bay					231
Lyons, on the Erie canal		15	rises	170	401
Outlet of Seneca lake, near Geneva	12	27	rises	46	447
Along the lake to its head	34	61	level		447
Summit between the lake and Chemung river	8	69	rises	443	890
The Chemung at Newtown	10	79	falls	53	837

No. VIII.—*Table of Ascents and Descents from Lake Ontario along the valley of the Genessee River, to the mouth of Black Creek in Allegany County, and thence to Olean, on the Allegany River, along Oil and Black Creeks.*

Route.	Miles.		Feet.		
Mouth of Genessee river					231
Erie canal, at Rochester		8			506
Squaque hill	29	37	rises	68	574
Gardow flats	6	43	rises	76	650
Head of the Great Falls in the town of Nunda	8	51	rises	453	1103
Mouth of Black creek	16	67	rises	162	1265
Summit level between Black and Oil creeks*	10	77	rises	221	1486
Olean on the Allegany	13	90	falls	78	1408

No. IX.—*Table of Ascents and Descents from the mouth of Oak Orchard Creek, on Lake Ontario, in nearly a direct line to Olean, on the Allegany, by the route of Batavia, the Tonnewanta Creek, Lime Lake, and the valley of Ischua Creek.*

Route.	Miles.		Feet.		
Lake Ontario, at the mouth of Oak Orchard creek					231
Albion, on the Erie canal		8	rises	275	506
Tonnewanta creek, at Batavia	17	25	rises	377	883
Attica, along Tonnewanta creek	11	36	rises	71	954
Dividing ridge between Tonnewanta and Cattaragus creeks	18	54	rises	526	1480
Lime lake†	14	68	rises	143	1623
Olean Point, on the Allegany, along the valley of Ischua and Oil creek	27	95	falls	214	1409

*This summit is a marsh—the discharged waters of which find the level of the ocean in the Gulf of St. Lawrence and the Gulf of Mexico.

†This lake, according to Mr. Roberts' report, is 1642 feet above tide. According to the same report, Beaver lake, in the town of China, is 1704 feet.

It is evident from these tables, that the mountain system occupies the entire width of the southern part of the state, between the Hudson and Lake Erie. The section given in Table No. 1, exhibits a mean elevation, after the first 13 miles from the Hudson, of 1400 feet, and presents no height less than 935 feet, except at its extremities, and in the two places where the survey descends into the deep ravines in which are situated Cayuga and Seneca lakes. If this section had passed a few miles to the south of the head of Seneca lake, the lowest point would have been 890 feet, which is the highest part of the bottom of a valley extending from this lake to the Chemung river. The mean elevation of the several ridges, crossed by the same section, is 1700 feet. And as these elevations are the lowest notches near the line of the survey, they may be considered as being but little higher than the general elevation of the surface of the country.

The second division of the mountain district of the state, or that on the north side of the Mohawk and Oneida valley, and between Lake Ontario and Champlain, has not been as minutely explored by topographical surveys for roads and canals, as the division we have already described; but the surface is known to be traversed, in a northeast direction, by at least five or six parallel ridges. The position of the principal one of these, beginning in Oneida county, may be traced on the map, between the heads of streams flowing to the right and left of its course through the middle of Herkimer and Hamilton counties, and the northern part of Essex, near the sources of the Hudson. The lowest pass across this ridge, between the valley of the Black river and the head waters of the Mohawk, is shown in table No. 4, and is elevated 1135 feet above the level of tide water. The lowest notch between West Canada creek and the Black river, is elevated 1226 feet, and between Fish creek and Salmon river, near where the ridge commences, the pass is 659* feet high. One of the peaks of this ridge, called the White Face, rises to the height of 2686 feet; and the general elevation of the country in the middle part of Hamilton county, has been

*Judge Geddes' report.

estimated at from 1800 to 2000 feet above the level of the ocean.

The mountains of this section are often described as an isolated group, entirely disconnected from the Appalachian system, which is generally considered as terminating in New-York, at the valley of the Mohawk river and Oneida lake. But when we view their relative positions, and the general direction of their several ridges, we must at once be convinced that they are, with all the other mountains in this state, only a part of the great chain which traverses the United States from Alabama to Maine. Indeed, the existence of a separate mountain group in any part of our national territory, has been reasonably doubted; and, strictly speaking, such a phenomenon is perhaps not to be found on the surface of the globe.

The third division, or that portion of the state on the east side of the Hudson, is situated principally on the western acclivity of the ridge which has been described as continuing distinct from the other subordinate ridges of the mountain system, and crossing the Hudson in the vicinity of West-Point, forming the Highlands of the river, and afterwards the dividing ridge between the Hudson and the Connecticut. The crest of this ridge passes to the north, on the east side of the boundary of New-York, in New-England, and has a mean elevation of more than 2000 feet. One of the lowest notches yet explored, is at Washington summit, in Massachusetts, on the route of the contemplated rail-way from Boston to Albany, and is elevated 1480 feet above the level of tide water in Boston harbor. This mountain range is known by various names in different parts of its course: before it crosses the Hudson, it is called the Blue Ridge; in Massachusetts and Connecticut, the Taghonnuc Range; and in Vermont, the Green Mountains. But as it lies principally without this state, a more particular description would be foreign to our purpose.

From the foregoing sketch, the truth of our remark must be evident, that the whole surface of the state of New-York is a mountain tract of country, indented in several places

below its general level, by the great depressions, in which are situated the waters of its principal lakes and rivers. The most important depressions, as we have already observed, are the basins of Lake Erie and Ontario, the valley in which is situated the Oneida lake and the Mohawk river, and that which contains the Hudson river and Lake Champlain. The basins of Lake Erie and Ontario are only parts of the immense St. Lawrence basin, which contains the five great western lakes, and bounds a principal part of the northern frontier of the Union. As this interesting depression of country is intimately connected with the topography of this state, we will dwell a few moments on some of its general features. Commencing at the Gulf of St. Lawrence it extends almost to the head waters of the Mississippi, a distance of nearly 1800 miles. In its whole depression it is computed to contain 511,930 square miles of surface, 72,930 of which is covered with water. It may be described as consisting of three great but unequal divisions; the upper, the middle, and the lower sub-basins. The first of these is in the form of a rhomb, and has an area of about 90,000 square miles, more than one-fourth of which is occupied by the waters of Lake Superior. The next, or middle sub-basin, occupies a quadrangular area of at least 160,000 square miles, and contains the three central lakes, viz: Huron, Michigan and Erie, in its lowest depressions. The surface of the lower sub-basin has an area of about 260,000 square miles, and is covered in part by the waters of Lake Ontario and St. Lawrence river.

Lakes Michigan and Huron are immense chasms, the bottoms of which, in some places, sink to the almost incredible depth of 1000 feet below their surface, and more than 300 feet below the level of the ocean. This is an interesting fact in the physical geography of the country; as these lakes are probably the lowest depressions on the continental surface of the earth. The surface of Lake Erie is elevated 565 feet above the level of the Atlantic ocean, 76 below Lake Superior, and 35 lower than the general level of Michigan and Huron. Its bottom, which is seldom depressed more

than 200 feet below its surface, is composed of alluvial deposit, probably washed down from the upper lakes by the continued action of a rapid current. Lake Ontario is elevated 231 feet above the level of the ocean: its mean depth has been estimated at 492 feet, although, in the middle, attempts have been made with 300 fathoms without striking soundings.* The St. Lawrence river, which connects this system of lakes with the Atlantic ocean, is the second river in magnitude in America, being no less than ninety miles wide at its mouth, and navigable for ships of the largest size, 400 miles from the ocean: Its whole length, from Lake Ontario to its mouth, is 692 miles.†

The following table, compiled from Darby's Geographical View of the United States, gives in a connected form, the elevation and extent of the several waters of the St. Lawrence basin.

No. X.—*Table of Elevation, mean Depth, Length, Breadth and Area, of the several collections of Water in the great St. Lawrence basin.*

	Elevation above tide level.	Mean depth.	Mean length.	Mean breadth.	Area.
	Feet.	*Feet.*	*Miles.*	*Miles.*	*Sq. miles.*
Lake Superior	641	900	300	80	24,000
Lake Huron	596	900	200	95	19,000
Lake Michigan	600	900	300	50	15,000
Lake Erie	565	120	230	35	8,030
Lake Ontario	231	492	180	30	5,400
River St. Lawrence and smaller lakes	------	20	------	------	1,500
Total water surface	------	------	------	------	72,930

The several slopes of the St. Lawrence basin, not covered by water, have been estimated to be sufficient to sustain a population of thirty millions of inhabitants. But the most interesting fact connected with this great depression is the vast quantity of fresh water contained in its several reser-

*Dr. Bigsby's sketch of the topography of Lake Ontario. Philosoph. Mag. and Annals, vol. 5, page 4.

†Darby.

voirs. From the data furnished by the above table, which may be considered as an approximation to truth, we find that the whole amount of water is 10,500 cubic miles; more than one half of the fresh water on the surface of the globe.*

The discharged waters of the upper lakes, in passing from the middle to the lower sub-basin of the St. Lawrence, are precipitated over the great falls of Niagara. This celebrated cataract has been rendered so familiar to almost every person, by the pen and pencil of the many travellers who have visited it, that a formal description, in this sketch, would be entirely unnecessary. About 20 miles below Lake Erie the Niagara river narrows, and the rapids commence: these are of such force and velocity, that their noise, agitation and fury constitute an object of as much curiosity as the falls themselves. On the very brink of the precipice, is situated Goat island, which contains about eighty acres, and extending up the stream, divides the waters. At this place the Niagara river, nearly half a mile wide, and flowing with immense velocity, is precipitated headlong over a perpendicular ledge of rocks, into an almost unfathomable abyss below. The height of the falls, from the surface of the water above to that of the water below, is 151 feet on the Canada side, and 164 feet on the American. The descent of the country from Lake Erie to Ontario, is principally by a step, not at the falls, but at Lewiston, several miles below. The surface on each side is a level plain, through which the Niagara river passes below the falls, in a deep chasm, nearly a mile wide, with almost perfect mural sides. In viewing the position of the falls, and the features of the country around, it is impossible not to be impressed with the idea, that this great natural race-way has been formed by the continued action of the irresistible current of the Niagara, and that the falls, beginning at Lewiston, in the course of ages have worn back the rocky strata to their present site.

The distances and descents along the Niagara river, from Lake Erie to Lake Ontario, from actual survey, on the American side, are as follows:

*See Edinburgh Encyclopedia, Article "Phys. Geog.," page 605.

From Lake Erie to the head of the rapids	distance 20 miles,	fall 15 feet.
Thence to the falls	1	51
The falls	--	164
From the falls to Lewiston, at the mouth of the chasm	7	104
Thence to Lake Ontario	7	2
Total	35 miles,	fall 336 feet.

The annexed table of elevations and distances, through the whole extent of the St. Lawrence basin, in connexion with the tables already given, will show its depression below the mountain surface of the country.

No. XI.—*Table of Ascents and Distances through the St. Lawrence basin, from the Gulf of St. Lawrence to the western angle of Lake Superior.*

Route.	Miles.		Feet.		
Up St. Lawrence river to the head of tide water		450			
Lake Ontario level	200	650	rises		231
Lake Erie level	175	825	rises	334	565
Lake Huron level	340	1165	rises	31	596
Lake Superior level	240	1405	rises	45	641
Mouth of St. Louis river into the western angle of Lake Superior	380	1785	level		641

The slopes of the lower subdivision of the St. Lawrence basin, which descend to the shores of Lake Ontario, occupy a considerable portion of the state of New-York. Beginning near the eastern extremity of Lake Erie, the boundary or edge of this sub-basin may be traced on the map along the heads of streams falling into Lake Ontario, through the southern part of the counties of Erie and Genessee, to the valley of the Genessee river, which is an arm of the St. Lawrence basin, stretching up into the high lands of Pennsylvania. From the Genessee river, the edge of the basin curves to the southeast around the southern extremities of Seneca and Cayuga lakes, including the four smaller lakes which lie a little to the west of these. The deep ravines in which are situated Seneca and Cayuga lakes may also be

considered as arms or branches of the principal basin, separated from each other by a high ridge. From the head of Cayuga lake, the edge of the basin turns suddenly to the north along the lake, and passes in a northeasterly direction through the northern part of Cortland county, a little south of Skaneateles lake, in nearly a straight line to the Little Falls on the Mohawk river. Here it suffers, for the first time in the course that we have described, an interruption, and an outlet appears to have been forcibly broken through into the lower valley of the Mohawk, by some tremendous convulsion of nature. From the Little Falls, the edge of the basin may be traced along the sources of the Mohawk river, Fish creek and the Salmon river, to the valley of the Black river, which may be considered a branch of the St. Lawrence basin, extending back almost to the valley of the Mohawk. From the Black river to St. Regis the remaining part of the basin in this state is the narrow slope of land along the St. Lawrence river, and the several valleys through which descend the Grass, the Racket, and the St. Regis rivers.

From the foregoing description of the southern boundary of the lower subdivision of the St. Lawrence basin, it evidently comprises the richest and most fertile part of the state, and includes the minor basins of the Genessee country, of the Oneida lake, and the valley of the Mohawk river as far east as the Little Falls. It is also evident from the data before given, that the mean elevation of the high land, forming the boundary just described, must be at least 1600 feet above the level of the ocean. On the north side of the lake in Canada,* the edge of the basin probably rises to nearly the same height, and as the bottom of Lake Ontario, in the deepest places, sinks 900 feet below its surface, or more than 600 feet below the level of the ocean, it follows that this collection of water occupies the lower part of an immense hollow, the deepest depressions of which are more than two thousand feet below the general level of the surrounding mountain surface. As this hollow is situated with its longer diameter directly across the mountain system, it lays bare to

*See Bigsby's Sketch.

the view on its southern side the different strata of rocks which deeply interlay the surface of the country to the south, and presents a geological section in this state, perhaps not less interesting than that at Paris, London, or Rome.

The lowest pass from the ocean into the St. Lawrence basin throughout its whole extent, except the bed of the St. Lawrence river, is through the valleys of the Hudson and Mohawk rivers. The highest part of this pass is near the Little Falls, and is elevated only 425 feet above the level of tide water.

The elevations of the lowest passes to the south, between the waters of Lake Ontario and those of the Susquehanna and the Allegany rivers, are given in tables Nos. 5, 6, 7, 8 and 9. The lowest of these is shown in table No. 7, where the Seneca lake approaches to within 18 miles of the Chemung river, and is separated from it by an intervening elevation of 443 feet above the lake, or 890 feet above the ocean. The pass through which the Ohio canal is being directed is 395 feet above the level of the lake. But the lowest pass to the south from any of the western lakes is that between the Chicago, a small stream emptying into the southern end of Lake Michigan, and the river Des Plaines, a branch of the Illinois. The summit is here only 17 feet above Lake Michigan, or about 617 feet above the ocean.* This is the most surprising and important hydrographical feature of our country; as, comparatively speaking, it here requires but a slight effort of art to give a new outlet to the upper lakes, and to divert a portion of the waters of Superior and Michigan from their present channel of the St. Lawrence to that of the Mississippi. Indeed, two of the plans reported by the canal commissioners of the State of Illinois, are to cut entirely through the barrier, and to supply the summit of a canal through this pass with water directly from Lake Michigan.

From the elevations of the several notches in the height of land that surround Lake Ontario, we may infer the curious fact, that if a narrow barrier of sufficient height were to exist

* Report of the Canal Commissioners of the state of Illinois, 1825.

across the St. Lawrence river above Quebec, and another at the Little Falls on the Mohawk, Lake Ontario would be on the level of Lake Superior; the falls of Niagara would disappear, and these two lakes would be merged in one immense inland sea. That this has actually been the state of things at some remote period in the history of our globe, is a favorite opinion of many; and indeed the appearance of the two outlets, particularly that at the Little Falls, and the nature of the surface of the different slopes of the lower basin, are not unfavorable to this hypothesis.*

No. XII.—*Table of Ascents and Distances on the line of the Erie Canal, through the Mohawk valley, from the mouth of the river to Little Falls, and thence along the St. Lawrence basin to Lake Erie.*

Route.	Miles.		Feet.		
Mouth of the Mohawk to Schenectady	....	21	rises	226	226
Head of Little Falls	58	79	rises	142	368
Beginning of the long level of Utica	12	91	rises	57	425
Along that level to its end, near Syracuse	69½	160½	level		425
Montezuma, at the Seneca river	36½	197	falls	45	380
Beginning of Rochester level	64	261	rises	126	506
Along that level to Lockport and Lake Erie level	63	324	rises	59	565
Along that level to Lake Erie	30	354	level		565
The whole length of the canal, from Albany to Lake Erie, is 363 miles. The junction of the Hudson and Mohawk is nine miles above Albany.					

That part of the above section between Utica and Lake Erie, presents a remarkable uniformity of elevation, with only one intervening depression of 45 feet at the Seneca river. The great length of its levels is also a striking feature of the Erie canal: the Utica level is 69½ miles long, and the Rochester level extends a distance of 63 miles. These facts, however, are both readily explained from a consideration of the circumstance that the canal passes from the Little Falls to Lake Erie along the slope of the St. Lawrence basin, the gradual descent of which to the north is highly favorable to the graduation of a line to the most uniform elevation.

*Appendix to Cuvier's Theory of the Earth, American edit., page 332.

The following are the elevations of the principal lakes in this state, included within the boundaries of the lower sub-basin of the St. Lawrence:

	Above Lake Ontario.	Above tide water.
Crooked lake in Yates and Steuben counties	487	718
Canandaigua lake	437	668
Seneca lake at Geneva	216	447
Cayuga lake	156	387
Oneida lake	144	375
Cross lake	139	370
Onondaga or Salt lake	130	361

The discharge waters of all these reservoirs pass into Lake Ontario, through the Oswego river.*

After the lower sub-basin of the St. Lawrence, the principal depression of surface connected with the topography of this state, is that containing the Hudson river and Lake Champlain. This depression is a long, deep and narrow vale, extending through the country, in a direct line from the ocean near New-York, to the valley of the St. Lawrence river, a distance of 380 miles. That part north of the Highlands at West-Point, is formed by an opening between two of the Allegany ranges; and is bounded on the one side by the Catskill ridges and the mountains on the north side of the Mohawk, and on the other by the range which we have described as forming the separating ridge between the Hudson and the Connecticut. There are only three lateral passes from this valley. The most important of these is the lower valley of the Mohawk, which may be considered as an arm of the Hudson and Champlain valley, extending back as far as the Little Falls; and thus forming a pass from the Hudson, through the Appalachian mountains, into the great St. Lawrence basin. The highest part of this pass, as we have before observed, is only 425 feet above tide water. The next pass is the valley through which the Delaware and Hudson canal has been constructed. It extends from the Hudson, near the village of Kingston, to the Delaware river; and is elevated in the highest part, 500 feet above the level of the Hudson. The other pass is also between the same rivers,

*It is a curious fact, that this river is the common drain of 15 lakes.

and is through a spacious valley bounded by the Catskill ridge on the one side and the mountains forming the Highlands on the other. The elevation of the summit is 430 feet above the Hudson and 207 above the Delaware.

The most remarkable and peculiar feature of the Hudson and Champlain valley, is its great and uniform depth below the general level of the surface of the adjoining country. The highest part of the bottom of this valley, throughout its whole extent, is on the intervening space between the Hudson and Lake Champlain, and is elevated only 147 feet above the level of tide in the river, and 54 feet above the surface of the lake. From this surprising fact, we learn that an obstruction in the channel of the Hudson at the entrance of the Highlands, near Newburgh, of only 150 feet in height, would turn the current of the river to the north, and cause its waters to descend to the gulf of St. Lawrence, through the outlet of Lake Champlain and the St. Lawrence river. The appearance of the mountain pass at the Highlands, is highly favorable to the supposition, that the Hudson has in reality forced its way through this impeding barrier, and thus gained a more direct passage to the ocean.

It has been justly remarked by an able geographer, that there is but one pass on the earth having a specific resemblance to this valley. Scotland is divided into two unequal sections, by what is well expressed by the term *glen*, signifying a deep vale between high and steep hills. This glen extends from the Atlantic ocean to the German sea, a distance of 120 miles, and has no summit higher than 70 feet, although bounded on each side by high mountains. Each of these passes is occupied by lakes and rivers which follow the general direction of the glen, and both have been rendered navigable by means of canals and other artificial improvements.

Viewed as a whole, the Hudson and Champlain valley may be more minutely described as consisting of two unequal sub-basins: the one containing Lake George, Lake Champlain, and the Chambly river; the other, the Hudson river below Glen's falls. Lake George is a narrow sheet of

water, lying in an apparent rend in the adjacent mountains; is thirty-four miles long, and from one to three miles wide. It discharges its waters into Lake Champlain, through a descent of nearly 200 feet. Lake Champlain, which forms the most important part of the upper sub-basin, is 109 miles long, and from one-half mile to twelve miles wide; its depth nearly corresponds to that of Huron and Michigan; while its surface is elevated only 93 feet above the level of tide water. Surrounded by imposing mountain scenery, the traveller on this lake imagines himself raised to Alpine heights, and can scarcely be convinced that a descent of less than one hundred feet would depress him to the level of the ocean. Lake Champlain is connected with the river St. Lawrence by the Chambly river on the north, and with the Hudson river on the south, by the artificial communication of the Champlain canal. The intervening distance between the Hudson river and the lake is only 22 miles; but the whole length of the canal, from its junction with the Erie canal, is 64 miles, 39 of which is along the side of the river.

The other division of the Hudson and Champlain valley, is the deep basin of the Hudson; and this may again be described as consisting of two subdivisions. The first of these includes the lower valley of the Mohawk, and the slopes of land on each side of the Hudson, from Glen's falls to the entrance of the Highlands near Newburgh. The sandy plain between Albany and Schenectady, is an upper shelf of the lower valley of the Mohawk, the southern boundary of which is a continuation of the Catskill mountains, and is seen in travelling between these cities, stretching along the horizon in a northwesterly direction towards the Mohawk river. This plain has a mean elevation of 320 feet, and suddenly declines into the valley of the Hudson by a precipitous step nearly parallel to the river. The capitol at Albany is built on the very edge of this step; and the Mohawk, in passing over the same depression, forms the Cohoes or great fall of the river. A similar shelf exists on each side of the Hudson, from Albany down to the Highlands. The country rises abruptly from the river to upwards

of two hundred feet, and then sweeps backwards with a very gentle rise to the mountain chain. On this shelf are situated all the cities and villages along the river, with the exception of Troy, which is the only place on the Hudson erected on the alluvial flat.

The lower or southern sub-basin of the Hudson, is a section of country highly interesting to the political geographer. It includes all that part of the state south of the Highlands, (except Long-Island,) as well as a part of New-Jersey. Its greatest width is from the southern sources of the Raritan river, to the eastern head of Croton river, in Putnam county, a distance of about 100 miles.

No. XIII.—*Table of Ascents and Distances through the Hudson and Champlain valley, from the Ocean, at New-York, to the St. Lawrence River.*

Route.	Miles.		Feet.		
New-York to the mouth of the Mohawk		154			
Level at Stillwater	14	168	rises	99	99
Level at Fort Miller	17	185	rises	18	117
Beginning of summit level at Fort Edward, nearly opposite to Glen's Falls	8	193	rises	30	147
Along that level to Fort Ann	12	205	rises		147
Lake Champlain, at Whitehall	12	217	falls	54	93
Along the lake to its outlet, near the 45° of north lat.	110	327	falls	33	90
Down the Chambly or Sorel river to its junction with the St. Lawrence, 40 miles above the head of tide water	70	397	falls	55	35

The Hudson river, which occupies so important a part of the Hudson and Champlain valley, is in itself one of the most interesting water courses on the surface of the globe; and as a navigable inlet to the vast and fertile regions of the west, demands a more particular notice than the limit of this article can afford to any other river in the state. It is formed of two principal branches: the Hudson proper, and the Mohawk. Each of these deserves particular attention, as contributing to supply the waters of our northern and western canals.

The Mohawk rises west of Oneida lake, flows south about twenty miles, and then suddenly turns to the southeast at Rome, where it falls on the bottom of what has been called the upper valley of the Mohawk. At this place, in high floods, the waters of the river divide: one part passing down the channel to the Hudson, and the other through Wood creek into Oneida lake, and thence to Lake Ontario. From Rome to the foot of Little Falls, a distance of 37 miles, the river descends 97 feet. Here the river descends through a narrow pass to the lower valley of the Mohawk, and offers incontestible evidence of having forcibly broken its way through the primitive rocks: the ledges on each side bear striking marks of the action of water at a height of more than 40 feet above the present level of the stream. The whole fall of the river, from Rome to its mouth, as may be seen by table No. 5, is 425 feet, in a distance of 116 miles; 78 feet of this descent is passed by the cataract of the Cohoes, one mile above its junction with the Hudson.

The two most remote branches of the Hudson proper, have their sources in the marshy regions of Hamilton and Essex counties. These united with each other, and the Sacandaga river, form a stream of considerable magnitude, which is first precipitated over a ledge of rocks called the Great falls, and afterwards down Glen's falls into the deep valley of the Hudson and Champlain basin. The length of what may be called the upper Hudson, from its extreme source to this place, is about 120 miles; and from here to its junction with the Mohawk is 40 miles, with a fall of 147 feet.

The Hudson, after its reception of the Mohawk, from its peculiar character, has been defined by some geographers as a long narrow bay. The periodical rising of the tides to the height of two feet at Albany—the great volume of water, and the gentleness of the current, which, under ordinary circumstances, is reversed by the ascending tide, are indeed the several characters of a bay; but it nevertheless possesses all the distinctive properties of a river, and when swelled by the spring floods, pours a rapid and immense torrent to the ocean. The oscillation of the tide in this river, is an

interesting phenomenon. It is not caused, as in the main ocean, by the direct action of the sun and moon, but is produced by a vast wave, propelled by the force of the Atlantic tide, along the slightly inclined plane of the bed of the river. The crest of this wave passes through the whole distance of 151 miles, between New-York and Troy, in from seven to nine hours.

The comparative importance of the Hudson, as a great commercial inlet to the western territory of the union, may be inferred from the fact, that it is the only Atlantic river, with the exception of the St. Lawrence, that has not its navigation soon interrupted by a precipitate descent from the mountain chain. At the Highlands the Hudson penetrates the primitive rock, and admits the ocean tide one hundred miles to the interior of the ridge, at whose foot, in every other Atlantic river, it is stopped.* Its tributary, the Mohawk, as we have seen, occupies the bottom of a depression which deeply indents the remaining ridges of the Appalachian mountains, and thus connects by an easy pass the valley of the Hudson with the basin of the St. Lawrence. Nature has thus done more by the valleys of the Hudson and the Mohawk, and that to the south of Lake Michigan, towards uniting the waters of the Atlantic with those of the Mississippi, than the utmost efforts of art can ever hope to accomplish in any other part of the country.

The importance of these peculiar topographical features was duly appreciated by the projectors of our canal policy, and the Erie and Champlain canal, with those in contemplation for uniting the former with the waters of the Susquehanna and Lake Ontario, fully develope the natural facilities for internal navigation possessed by this state.

In a physical point of view, these works produce changes which it could scarcely have been believed that the power of man could have accomplished. The waters of the Tioga river, which now entirely contribute to swell the volume of the Susquehanna, by the construction of the artificial channel of the Chemung canal, will in part be conducted to

*Gallatin's Report.

Seneca lake, and thence with the discharged waters of this reservoir, to the gulf of St. Lawrence. On the summit level of the Champlain canal, the waters of the upper Hudson are turned back to the north, and instead of mingling, as formerly, with the Atlantic ocean in the bay of New-York, now mix with the sea in the straits of Bellisle.

NOTE.—For the accompanying plate of the comparative elevation of the principal mountain ridges and peaks in this state, we are indebted to the politeness of DAVID H. BURR, Esq. It forms a part of a general map of the state, which together with an atlas containing a map and statistical table of each county in the state, has just been published by the above named gentleman.

This work is an important acquisition to the topographical knowledge of our state; and as it is intimately connected with the subject of the preceding article, the following extracts from the author's preface may not be improper in this place. "The legislature of New-York, in 1827, upon the recommendation of Governor Clinton, passed an act directing that whenever a set of maps was compiled on this plan, and delivered to the surveyor-general and comptroller, they should revise and correct the same; and that when they were satisfied with their accuracy, should publish them at the expense of the state. The legislature at the same and subsequent sessions, made liberal appropriations to defray the expenses, at the same time giving the author permission to make use of all documents deposited in any of the public offices of the state, or of the several towns and counties, which he should deem necessary in the completion of the work."

"During its progress, the surveyor-general addressed circulars to the supervisors of the several towns, requiring them to furnish surveys of the same, that their boundaries might be correctly described in the revised statutes. The information so obtained was furnished by the surveyor-general to the author, and has been used in the present work. When

the author had rendered the work as perfect as these authorities and his own personal observations enabled him to do, it was delivered to the surveyor-general and comptroller, for revision and correction, pursuant to the act before mentioned."

"Circulars were again addressed by the surveyor-general to the several supervisors, enclosing maps of their respective towns, and requesting them to point out the errors, if any, and also to suggest such additions as might be necessary to render the work more full and perfect. These circulars were in most instances returned with much useful information, which enabled the surveyor-general, with his previous knowledge, to correct such errors as had escaped the observation of the author. This work, therefore, comprises not alone the geographical knowledge of a single individual, but that of many, and those the best informed by their vocations of any in the state."*

*This article was prepared as an introduction to the atlas of the state of New-York published by David H. Burr. The section of mountains is principally from my own survey. (*MS. note by J. H.*)

[The plate showing the sectional elevations of the principal mountains in New York, (referred to in the preceding page,) has been omitted in the present re-print.]

ON THE APPLICATION OF THE PRINCIPLE OF THE GALVANIC MULTIPLIER TO ELECTRO-MAGNETIC APPARATUS, AND ALSO TO THE DEVELOPMENT OF GREAT MAGNETIC POWER IN SOFT IRON, WITH A SMALL GALVANIC ELEMENT.*

(Silliman's American Jour. of Science, January, 1831, vol. XIX, pp. 400–408.)

For a long time after the discovery of the principal facts in electro-magnetism, the experiments in this interesting department of science could be repeated only by those who were so fortunate as to possess a large and expensive galvanic apparatus. Mr. Sturgeon, of Woolwich, did much towards making the subject more generally known, by showing that when powerful magnets are used, many of the most interesting experiments can be performed with a very small galvanic combination. His articles of apparatus, constructed on this principle, are of a much larger size, and more convenient, than any before used. They do not, however, form a complete set, as it is evident that strong magnets cannot be applied to every article required, and particularly to those intended to exhibit the action of terrestrial magnetism on a galvanic wire, or the operation of two galvanic wires on each other.

In a paper, published in the *Transactions of the Albany Institute*, June, 1828, I described some modifications of apparatus, intended to supply this deficiency of Mr. Sturgeon, by introducing the spiral coil on the principle of the galvanic multiplier of Prof. Schweigger, and this I think is applicable in every case where strong magnets cannot be used. The coil is formed by covering copper wire, from $\frac{1}{40}$ to $\frac{1}{20}$ of an inch in diameter, with silk; and in every case, which will permit, instead of using a single conducting wire, the effect is multiplied by introducing a coil of this wire, closely turned upon itself. This will be readily understood by an example: thus, in the experiment of Am-

* The term galvanic element is used in this paper to denote a single pair of galvanic plates.

pere, to shew the action of terrestrial magnetism on a galvanic current, instead of using a short single wire suspended on steel points; 60 feet of wire, covered with silk, are coiled so as to form a ring of about 20 inches in diameter, the several strands of which are bound together by wrapping a narrow silk ribbon around them. The copper and zinc of a pair of small galvanic plates are attached to the ends of the coil, and the whole suspended by a silk fibre, with the galvanic-element hanging in a tumbler of diluted acid. After a few oscillations, the apparatus never fails to place itself at right angles to the magnetic meridian. This article is nothing more than a modification of De la Rive's ring on a larger scale.

Shortly after the publication mentioned, several other applications of the coil, besides those described in that paper, were made in order to increase the size of electro-magnetic apparatus, and to diminish the necessary galvanic power. The most interesting of these, was its application to a developement of magnetism in soft iron, much more extensively, than to my knowledge had been previously effected by a small galvanic element.

A round piece of iron, about $\frac{1}{4}$ of an inch in diameter, was bent into the usual form of a horse-shoe, and instead of loosely coiling around it a few feet of wire, as is usually described, it was tightly wound with 35 feet of wire, covered with silk, so as to form about 400 turns; a pair of small galvanic plates, which could be dipped into a tumbler of diluted acid, was soldered to the ends of the wire, and the whole mounted on a stand. With these small plates, the horse-shoe became much more powerfully magnetic, than another of the same size, and wound in the usual manner, by the application of a battery composed of 28 plates of copper and zinc, each 8 inches square. Another convenient form of this apparatus was contrived, by winding a straight bar of iron 9 inches long with 35 feet of wire, and supporting it horizontally on a small cup of copper containing a cylinder of zinc,—when this cup, which served the double purpose of a stand and the galvanic element,

was filled with dilute acid, the bar became a portable electro-magnet. These articles were exhibited to the Institute in March, 1829.

The idea afterwards occurred to me, that a sufficient quantity of galvanism was furnished by the two small plates, to develope, by means of the coil, a much greater magnetic power in a larger piece of iron. To test this, a cylindrical bar of iron, $\frac{1}{2}$ an inch in diameter, and about 10 inches long, was bent into the form of a horse-shoe, and wound with 30 feet of wire; with a pair of plates containing only $2\frac{1}{2}$ square inches of zinc, it lifted 14 lbs. avoirdupois. At the same time, a very material improvement in the formation of the coil suggested itself to me, on reading a more detailed account of Prof. Schweigger's galvanometer, and which was also tested with complete success upon the same horse-shoe; it consisted in using several strands of wire, each covered with silk, instead of one:—agreeably to this construction, a second wire, of the same length as the first, was wound over it, and the ends soldered to the zinc and copper in such a manner that the galvanic current might circulate in the same direction in both, or in other words, that the two wires might act as one; the effect by this addition was doubled, as the horse-shoe, with the same plates before used, now supported 28 lbs.

With a pair of plates 4 inches by 6 inches, it lifted 39 lbs., or more than 50 times its own weight.

These experiments conclusively proved that a great developement of magnetism could be effected by a very small galvanic element, and also that the power of the coil was materially increased by multiplying the number of wires, without increasing the length of each.

The multiplication of the wires, increases the power in two ways; first, by conducting a greater quantity of galvanism, and secondly, by giving it a more proper direction, for since the action of a galvanic current is directly at right angles to the axis of a magnetic needle, by using several shorter wires, we can wind one on each inch of the length of the bar to be magnetized, so that the magnetism of each

inch will be developed, by a separate wire; in this way the action of each particular coil becomes very nearly at right angles to the axis of the bar, and consequently, the effect is the greatest possible. This principle is of much greater importance when large bars are used. The advantage of a greater conducting power from using several wires might in a less degree be obtained by substituting for them one large wire of equal sectional area, but in this case the obliquity of the spiral would be much greater and consequently the magnetic action less; besides this, the effect appears to depend in some degree on the number of turns which is much increased by using a number of small wires.*

In order to determine to what extent the coil could be applied in developing magnetism in soft iron; and also to ascertain, if possible, the most proper length of the wires to be used—

A series of experiments was instituted joinly by Dr. Philip Ten Eyck and myself. For this purpose 1060 feet (a little more than $\frac{1}{5}$ of a mile) of copper wire of the kind called bell-wire, .045 ($\frac{45}{1000}$) of an inch in diameter, were stretched several times across the large room of the Academy.

Experiment 1. A galvanic current from a single pair of plates of copper and zinc two inches square, was passed through the whole length of the wire, and the effect on a galvanometer noted;—From the mean of several observations, the deflection of the needle was 15°.

Exp. 2. A current from the same plates was passed through half the above length (or 530 feet) of wire, the deflection in this instance was 21°.

By a reference to a Trigonometrical table, it will be seen that the natural tangents of 15° and 21° are very nearly in the ratio of the square roots of 1 and 2, or of the relative lengths of the wires in these two experiments.

* Several small wires conduct more common electricity from the machine than one large wire of equal sectional area; the same is probably the case though in a less degree, in galvanism.

The length of the wire forming the galvanometer may be neglected, as it was only 8 feet long. This result agrees remarkably with the law discovered by Mr. Ritchie and published in the last No. of the *Journal of the Royal Institution of Great Britain.*

Exp. 3. The galvanometer was now removed, and the whole length of the wire attached to the ends of the wire of a small soft iron horse-shoe, $\frac{1}{4}$ of an inch in diameter, and wound with about 8 feet of copper wire with a galvanic current from the plates used in Experiments 1 and 2; the magnetism was scarcely observable in the horse-shoe.

Exp. 4. The small plates were removed and a battery composed of a piece of zinc plate 4 inches by 7 inches surrounded with copper, was substituted; when this was attached immediately to the ends of the 8 feet of wire wound round the horse-shoe, the weight lifted was $4\frac{1}{2}$ lbs.: when the current was passed through the whole length of wire (1060 feet) it lifted about half an ounce.

Exp. 5. The current was passed through half the length of wire (550 feet) with the same battery, it then lifted 2 oz.

Exp. 6. Two wires of the same length as in the last experiment were used, so as to form two strands from the zinc and copper of the battery: in this case the weight lifted was 4 oz.

Exp. 7. The whole length of the wire was attached to a small trough on Mr. Cruickshanks' plan, containing 25 double plates, and presenting exactly the same extent of zinc surface to the action of the acid as the battery used in the last experiment. The weight lifted in this case was 8 oz.; when the intervening wire was removed and the trough attached directly to the ends of the wire surrounding the horse-shoe it lifted only 7 oz. From this experiment, it appears that the current from the galvanic trough is capable of producing greater magnetic effect on soft iron after traversing more than $\frac{1}{5}$ of a mile of intervening wire, than when it passes only through the wire surrounding the magnet. It is possible that the different states of the trough, with respect to dryness, may have exerted some influence

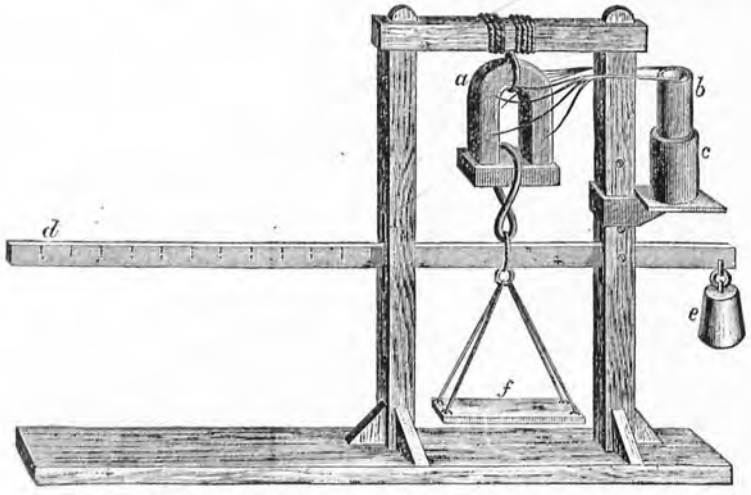
on this remarkable result; but that the effect of a current from a trough, if not increased, is but slightly diminished in passing through a long wire, is certain. A number of other experiments would have been made to verify this had not our use of the room been limited, by its being required for public exercises.

On a little consideration however, the above result does not appear so extraordinary as at the first sight, since a current from a trough possesses more "projectile force," to use Prof. Hare's expression, and approximates somewhat in intensity to the electricity from the common machine. May it not also be a fact that the galvanic fluid, in order to produce the greatest magnetic effect, should move with a small velocity, and that in passing through one fifth of a mile, its velocity is so retarded as to produce a greater magnetic action? But be this as it may, the fact, that the magnetic action of a current from a trough is, *at least*, not sensibly diminished by passing through a long wire, is directly applicable to Mr. Barlow's project of forming an electro-magnetic telegraph;* and it is also of material consequence in the construction of the galvanic coil. From these experiments, it is evident that in forming the coil we may either use one very long wire or several shorter ones as the circumstances may require: in the first case, our galvanic combinations must consist of a number of plates so as to give "projectile force;" in the second, it must be formed of a single pair.

In order to test on a large scale, the truth of these preliminary results, a bar of soft iron, 2 inches square and 20 inches long, was bent into the form of a horse-shoe, 9½ inches high, the sharp edges of the bar were first a little rounded by the hammer, it weighed 21 lbs.; a piece of iron from the same bar weighing 7 lbs. was filed perfectly flat on

*[In a statement made by Prof. Henry, in March, 1857, he says: "Not being familiar with the history of the attempts made in regard to this invention, I called it 'Barlow's project,' while I ought to have stated that Mr. Barlow's investigation merely tended to disprove the possibility of a telegraph."]

one surface, for an armature or lifter; the extremities of the legs of the horse-shoe were also truly ground to the surface of the armature: around this horse-shoe 540 feet of copper bell wire were wound in 9 coils of 60 feet each; these coils were not continued around the whole length of the bar, but each strand of wire, according to the principle before mentioned, occupied about two inches and was coiled several times backward and forward over itself; the several ends of the wires were left projecting and all numbered, so that the first and the last end of each strand might be readily distinguished. In this manner, we formed an experimental magnet on a large scale, with which several combinations of wire could be made by merely uniting the different projecting ends. Thus, if the second end of the first wire be soldered to the first end of the second wire, and so on



[Frame for testing strength of electro-magnet.]

a, the magnet covered with linen, the ends of the wires projecting so as to be soldered to the galvanic element *b*. *c*, a cup with dilute acid on a moveable shelf. *d*, a graduated lever. *e*, a counterpoise. *f*, a scale for supporting weights; when a small sliding weight on the lever is not used, a second galvanic element is attached to the apparatus so that the poles of the magnet can be instantly reversed: this is omitted in the figure.

By inverting the large magnet, it sets in motion a very large revolving cylinder of March and Ampère.

through all the series, the whole will form a continued coil of one long wire. By soldering different ends, the whole may be formed into a double coil of half the length, or into a triple coil of one third the length, &c. The horse-shoe was suspended in a strong rectangular wooden frame 3 feet 9 inches high and 20 inches wide, an iron bar was fixed below the magnet so as to act as a lever of the second order; the different weights supported, were estimated by a sliding weight in the same manner as with a common steelyard. (See the sketch of the magnet.)

In the experiments immediately following,* a small single battery was used, consisting of two concentric copper cylinders, with zinc between them; the whole amount of zinc surface exposed to the acid from both sides of the zinc was $\frac{3}{6}$ of a square foot; the battery required only half a pint of dilute acid for its submersion.

Exp. 8. Each wire of the horse-shoe was soldered to the battery in succession, one at a time; the magnetism developed by each was just sufficient to support the weight of the armature, weighing 7 lbs.

Exp. 9. Two wires, one on each side of the arch of the horse-shoe, were attached; the weight lifted was 145 lbs.

Exp. 10. With two wires, one from each extremity of the legs, the weight lifted was 200 lbs.

Exp. 11. With three wires, one from each extremity of the legs, and the other from the middle of the arch, the weight supported was 300 lbs.

Exp. 12. With four wires, two from each extremity, the weight lifted was 500 lbs. and the armature; when the acid was removed from the zinc, the magnet continued to support, for a few minutes, 130 lbs.

Exp. 13. With six wires, the weight supported was 570 lbs.; in all these experiments, the wires were soldered to the galvanic element; the connexion, in no instance, was formed with mercury.

Exp. 14. When all the wires, (nine in number,) were attached, *the maximum weight lifted was* 650 *lbs.* and this aston-

* All the weights in this series of experiments are avoirdupois.

ishing result, it must be remembered, was produced by a battery containing only $\frac{2}{5}$ of a square foot of zinc surface, and requiring only half a pint of diluted acid for its submersion.

Exp. 15. *A small battery, formed with a plate of zinc* 12 *inches long and* 6 *inches wide, and surrounded by copper, was substituted for the galvanic element used in the last experiment; the weight lifted in this case was* 750 *lbs.* This is probably the maximum of magnetic power which can be developed in this horse-shoe, as with a large calorimotor, containing 28 plates of copper and zinc, each 8 inches square, the effect was not increased, and indeed we could not succeed in making it lift as much as with the small battery.

The strongest magnet of which we have any account, is that in the possession of Mr. Peale, of Philadelphia; this weighs 53 lbs. and lifted 310 lbs. or about six times its own weight. Our magnet weighs 21 lbs. and consequently lifts more than thirty-five times its own weight; it is probably, therefore, the most powerful magnet ever constructed.

This, however, is by no means the maximum, which can be produced by a small galvanic element, as in every experiment we have made the power increases by increasing the quantity of iron; with a bar similar to the one used in these experiments, but of double the diameter, or of 8 times the weight, the power would doubtless be quadruple, and that too without increasing the size of the galvanic element.

Exp. 16. *In order to ascertain the effect of a very small galvanic element on this large quantity of iron, a pair of plates, exactly one inch square, was attached to all the wires; the weight lifted was* 85 *lbs.*

The following experiments were made with wires of different lengths, on the same horse-shoe.

Exp. 17. With 6 wires, each 30 feet long, attached to the galvanic element, the weight lifted was 375 lbs.

Exp. 18. The same wires used in the last experiment, were united so as to form 3 coils of 60 feet each; the weight supported was 290 lbs. This result agrees nearly with that of Exp. 11, though the same individual wires were not used;

from this it appears, that 6 short wires are more powerful than 3 of double the length.

Exp. 19. The wires used in Exp. 10, but united so as to form a *single* coil of 120 feet of wire, lifted 60 lbs.; while in Exp. 10, the weight lifted was 200 lbs.: this is a confirmation of the result in the last experiment.

Exp. 20. The same wires used in the last experiment were attached to a small compound battery, consisting of two plates of zinc and two of copper, after the plan of Prof. Hare, and containing exactly the same quantity of zinc surface, as the element in the last experiment; in this case the weight lifted was 110 lbs., or nearly double that in the last. This result is in strict accordance with that of Exp. 7; the two plates having more "projectile force," and thus producing a greater effect with a long wire.

In these experiments a fact was observed, which appears somewhat surprizing: when the large battery was attached and the armature touching both poles of the magnet, it was capable of supporting more than 700 lbs. but when only one pole is in contact it did not support more than 5 or 6 lbs., and in this case we never succeeded in making it lift the armature (weighing 7 lbs.). This fact may perhaps be common to all large magnets, but we have never seen the circumstance noticed of so great a difference between a single pole and both.

A number of experiments were also made with reference to the best form of the iron to receive magnetism, but no very satisfactory results were obtained; of these however, the following are considered as not uninteresting.

Exp. 21. A cylindrical bar of iron weighing 13 oz. 4½ drachms, and bent into a horse-shoe, was covered with two coils of wire each 60 feet long; with the small battery used in the last experiment, it lifted 42 lbs.

Exp. 22. A rectangular flat bar $\frac{15}{16}$ of an inch wide, and $\frac{1}{5}$ of an inch thick, also bent into a horse-shoe, weighing 9 oz. 3 dr., and of exactly the same surface as the bar used in the last experiment, with the same wires and battery, lifted 35 lbs

Exp. 23. A piece of a gun-barrel, little less than inch in diameter and about 8 inches long, and from $\frac{1}{12}$ to $\frac{1}{15}$ of an inch thick, weighing 8 oz. $3\frac{2}{3}$ dr. (with the wires and battery as before,) lifted 40 lbs.

From the last experiment, it appears that a given quantity of iron in the form of a hollow cylinder, is capable of receiving more magnetism than that of a solid cylinder of less diameter; but it is evident from Exp. 21, that a solid bar of the same diameter as the gun-barrel, and of greater weight, would have lifted more: perhaps the gun-barrel was not sufficiently thick for the full development of magnetism, which, according to Barlow's experiments, resides near the surface.*

A series of experiments† was separately instituted by Dr. Ten Eyck in order to determine the maximum development of magnetism in a small quantity of soft iron: from these the following interesting results were obtained.

Experiment 1. A horse-shoe of round iron $\frac{56}{100}$ of an inch in diameter, 4 inches long, weighing 2314 grains and wound with 23 ft. copper-wire, diameter $\frac{45}{1000}$ of an inch, with a pair of one inch plates, lifted 19 lbs. 5 oz. 6 dwt. 16 grs.; with a pair of 4 inch plates, lifted 25 lbs. 6 oz. 5 dwt.; with the cylindrical element used in Exps. 8, 9 and 10 of former series, it lifted 42 lbs. 6 oz. 8 dwt. 8 grs., or 105 times its own weight.

Exp. 2. A horse-shoe of round iron $\frac{1}{4}$ inch in diameter, $3\frac{1}{2}$ inches in length weighing 310 grains, and wound with 15 ft. copper wire, diameter $\frac{45}{1000}$ inch, with a pair of one inch plates, lifted 3 lbs. 11 oz. 7 dwt. 22 grs.; with 4 inch plates it lifted 5 lbs. 5 oz. 12 dwt. 12 grs.; with the cylindrical element 8 lbs. 2 oz. 8 dwt. 18 grs., or 152 times its own weight.

Exp. 3. A horse-shoe formed of a flat bar $2\frac{1}{10}$ inches long $\frac{3}{10}$ in. broad and $\frac{6}{100}$ in. thick, weighing 84 grains, and wound with 16 feet of brass wire, $\frac{32}{60}$ of an inch in diameter, with a pair of one inch plates, lifted 5 lbs. 2 oz. 3 dwt. 8 grs.;

* See Barlow's Essay on Magnetic Attractions, page 50.

† Troy weight is used in these experiments.

with 4 inch plates, it lifted 2 lbs. 10 oz. 2 dwt. 12 grs.; with the cylindrical element, it lifted 2 lbs. 10 oz. 13 dwt. 2 grs., or 198 times its own weight.

Exp. 4. A horse-shoe of round iron, slightly flattened, one inch in length, diameter, (before flattening) $\frac{6}{100}$ inch, weight 6 grains, and wound with 3 feet brass wire same diameter as that of No. 3, with a pair of one inch plates, lifted 2 oz. 15 dwt. 1 gr.; with four inch plates, lifted 3 oz. 17 dwt. 10 gr.; with the cylindrical element, lifted 5 oz. 5 dwt. 4 grs., or 420 times its own weight.

In this last result the ratio of the weight lifted, to the weight of the magnet is much greater than any we have ever seen noticed; the strongest magnet we can find described is one worn by Sir Isaac Newton in a ring, weighing 3 grains; it is said to have taken up 746 grs. or nearly 250 times its own weight. M. Cavallo has seen one of 6 or 7 grs. weight which was capable of lifting 300 grs. or about 50 times its own weight. From these experiments it is evident, that a much greater degree of magnetism can be developed in soft iron by a galvanic current, than in steel by the ordinary method of touching.

Most of the results given in this paper were witnessed by Dr. L. C. Beck, and to this gentleman we are indebted for several suggestions, and particularly that of substituting cotton well waxed for silk thread, which in these investigations, became a very considerable item of expense; he also made a number of experiments with iron bonnet-wire, which, being found in commerce already wound, might possibly be substituted in place of copper:—the result was that with very short wire the effect was nearly the same as with copper, but in coils of long wire with a small galvanic element, it was not found to answer. Dr. Beck also constructed a horse-shoe of round iron, one inch in diameter, with four coils on the plan before described; with one wire it lifted 30 lbs., with two wires—60 lbs., with three wires—85 lbs., and with four wires—112 lbs.

While we were engaged in these investigations, the last No. of the *Edinburgh Journal of Science* was received, containing

Prof. Moll's paper on Electro-Magnetism. Some of his results are, in a degree, similar to those here described: his object, however, was different, it being only to induce strong magnetism on soft iron with a powerful galvanic battery. The principal object in these experiments was to produce the greatest magnetic force, with the smallest quantity of galvanism. The only effect Prof. Moll's paper has had over these investigations, has been to hasten their publication: the principle on which they were instituted was known to us nearly two years since, and at that time exhibited to the Albany Institute.

AN ACCOUNT OF A LARGE ELECTRO-MAGNET,* MADE FOR THE LABORATORY OF YALE COLLEGE.

(*Extract of a letter to Prof. Silliman, accompanying the Magnet.*)

(Silliman's American Journal of Science, April, 1831, vol. xx. pp. 201–203.)

The magnet is constructed on precisely the same principles as that described in the last number of the Journal. It weighs 59½ lbs. avoirdupois, (exclusive of the copper wire which surrounds it,) and was formed from a bar of Swede's

---

*[This magnet is now arranged in its frame, in the laboratory of Yale College. Being myself out of town when the instrument arrived, the necessary experiments and fixtures were satisfactorily made by C. U. Shepard, (Chemical Assistant) and Dr. Titus W. Powers, of Albany, who was so obliging as to bring the magnet to New Haven. There has not been time (as the magnet came just as this No. was finishing) to do any thing more than make a few trials, which have however fully substantiated the statements of Prof. Henry. He has the honor of having constructed by far, the most powerful magnets that have ever been known, and his last, weighing, armature and all, but 82½ lbs., sustains over a ton. It is eight times more powerful than any magnet hitherto known in Europe, and between six and seven times more powerful than the great magnet in Philadelphia. We understand that the experiments described in the last No. of this Journal, (except those ascribed to Dr. Ten Eyck) were devised by Professor Henry alone, who (except forging the iron) constructed the magnet with his own hand. The plan of the frame, and the fixtures, and the drawing in the last No., were done by Dr. Ten Eyck. In the Yale College magnet, the plan was drawn by Professor Henry, and the iron forged under his direction. The length of the wires being agreed upon, the winding was done by Dr. Ten Eyck, and the experiments were mutually performed.—*Editor of Journal.*]

[It may be worth while to state a single experiment, which I made with a view to learn the chemical effects of this instrument. As its magnetic flow was so powerful, I had strong hopes of being able to aacomplish the decomposition of water by its means. My experiment, however, which was made as follows, proved unsuccessful. The battery being immersed, to the extremities of the magnet were applied two broad, polished plates of iron, terminating in flattened wires, which were united with the wires of the ordinary apparatus for decomposing water, and the contact heightened by the use of cups of mercury: not the slightest decomposition was, however, observable. Aware, that had any chemical effect been produced, this arrangement could have decided nothing, (except perhaps from the degree of energy in the decomposition) as respects the point whether simple magnetism is

iron three inches square and thirty inches long. Before bending the bar into the shape of a horse-shoe, it was flattened on the edges, so as to form an octagonal prism, having a perimeter of $10\frac{3}{4}$ inches. The other dimensions of the magnet, as measured before winding it with wire, are as follows:— perpendicular height of the exterior arch of the horse-shoe, $11\frac{3}{4}$ inches; around the outside from one pole to the other, $29\frac{9}{10}$ inches; internal distance between the poles, $3\frac{1}{2}$ inches.

The armature or lifter is formed from a piece of iron from the same bar, not flattened on the edges; it is nearly 3 inches square, $9\frac{1}{2}$ inches long, and weighs 23 lbs. The upper surface is made perfectly flat, except about an inch in the middle where the angles are rounded off so as to form a groove, into which the upper part of a strong iron stirrup, surrounding the armature, fits somewhat loosely. The weight to be supported is fastened to the lower part of the stirrup, and by means of the groove is made to bear directly on the centre of the armature.

For the purpose of suspending the magnet, a piece of round iron with an eye on one end, is firmly screwed into the crown of the arch and is attached to the cross beam of a frame, similar to that figured in the last number of the Journal.

The magnet is wound with 26 strands of copper bell-wire, covered with cotton thread 31 feet long; about 18 inches of the ends are left projecting, so that only 28 feet actually surround the iron; the aggregate length of the coils is therefore 728 feet. Each strand is wound on a little less than an inch; in the middle of the horse-shoe it forms three thick-

---

adequate to decompose water, since it might under these circumstances be attributed to the electricity from the battery, I had determined in a second experiment, had the first proved successful, to have interrupted the galvanic flow by a non-conductor; in which case, had the decomposition ensued, pure magnetism might have been considered as the decomposing agent. But as my preliminary experiment was unsuccessful, I proceeded no farther; I hope, however, to resume the research hereafter, under more favorable circumstances. C. U. Shepard.]

nesses of wire, and on the ends or near the poles it is wound so as to form six thicknesses.

Two small galvanic batteries are soldered to the wires of the magnet, one on each side of the supporting frame, in such a manner as to cause the poles to be instantaneously reversed, by merely dipping the batteries alternately into acid. To render these as compact as possible, they are formed of concentric copper cylinders, with cylinders of zinc plates interposed, and so united as to form but one galvanic pair. Each of these batteries presents to the action of the acid, (measuring both surfaces of the plate,) $4\frac{7}{9}$ square feet: they are 12 inches high and about 5 inches in diameter.

In experimenting with this magnet, a battery containing $\frac{3}{5}$ of a square foot of zinc surface was first attached to the wires; with this the magnet could not be made to support more than 500 lbs. Another battery was then substituted for the above, containing about three times the same quantity of zinc surface; with this, at the first instant of immersion, the magnet sustained 1600 lbs.; after the acid was removed, it continued to support, for a few minutes, 450 lbs.; and in one experiment, three days after the battery had been excited, more than 150 lbs. were added to the armature* before it fell. It was evident from these experiments, that this magnet required a considerably larger quantity of zinc surface in proportion to its weight, to magnetize it to saturation, than that described in the former paper. Accordingly the two batteries, before mentioned as containing $4\frac{7}{9}$ square feet, were prepared. With one of them, at the first immersion, the magnet readily supported 2000 lbs. A sliding weight was then attached to the bar; the battery was suffered to become perfectly dry, and on immersing it again, the magnet supported 2063 lbs. The effect of a larger battery was not tried.

To test its power of inducing magnetism on soft iron, two

* [The armature of 23 lbs. applied when the battery is immersed, only for an inch and an instant, remains day after day without falling, although the galvanic coils are perfectly dry.—*Editor of Journal.*]

pieces of round iron $1\frac{1}{4}$ inches in diameter and 12 inches long, were interposed between the extremities of the magnet and the armature: with this arrangement, when one of the batteries was immersed, the pieces of iron became so powerfully magnetic as to support 155 lbs.

To exhibit the effects produced by instantaneously reversing the poles, the armature was loaded with 56 lbs., which added to its own weight made 89 lbs.: one of the batteries was then dipped into the acid and immediately withdrawn, when the weight of course continued to adhere to the magnet; the other battery was then suddenly immersed, when the poles were changed so instantaneously that the weight did not fall. That the poles were actually reversed in this experiment, was clearly shown by a change in the position of a large needle placed at a small distance from the side of one extremity of the horse-shoe.

P. S.—Last autumn, I commenced a series of observations on the magnetic intensity of the earth at Albany, and intend to begin a new series next month; the apparatus used was that sent by Capt. Sabine to Prof. Renwick, and was mentioned in the Journal, vol. XVII, p. 145. I have constructed a similar apparatus for myself, and intend to pay considerable attention to the subject.

ON A RECIPROCATING MOTION PRODUCED BY MAGNETIC ATTRACTION AND REPULSION.

(Silliman's American Journal of Science, July, 1831, vol. xx, pp. 340–343.)

To the Editor:

SIR:—I have lately succeeded in producing motion in a little machine by a power, which, I believe, has never before been applied in mechanics—by magnetic attraction and repulsion.

Not much importance, however, is attached to the invention, since the article, in its present state, can only be considered a philosophical toy; although, in the progress of discovery and invention, it is not impossible that the same principle, or some modification of it on a more extended scale, may hereafter be applied to some useful purpose. But without reference to its practical utility, and only viewed as a new effect produced by one of the most mysterious agents of nature, you will not, perhaps, think the following account of it unworthy of a place in the *Journal of Science.*

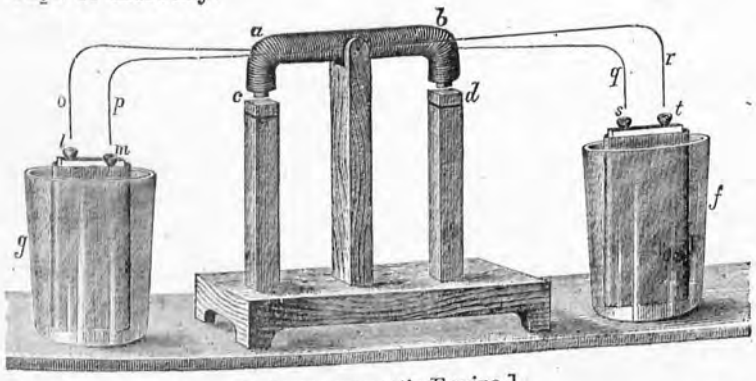
It is well known that an attractive or repulsive force is exerted between two magnets, according as poles of different names, or poles of the same name, are presented to each other.

In order to understand how this principle can be applied to produce a reciprocating motion, let us suppose a bar magnet to be supported horizontally on an axis passing through the centre of gravity, in precisely the same manner as a dipping needle is poised; and suppose two other magnets to be placed perpendicularly, one under each pole of the horizontal magnet, and a little below it, with their north poles uppermost; then it is evident that the south pole of the horizontal magnet will be attracted by the north pole of one of the perpendicular magnets, and its north pole repelled by the north pole of the other: in this state it will remain at rest, but if, by any means, we reverse the polarity of the horizontal magnet, its position will be changed and the extremity,

which was before attracted, will now be repelled; if the polarity be again reversed, the position will again be changed, and so on indefinitely: to produce, therefore, a continued vibration, it is only necessary to introduce, into this arrangement, some means by which the polarity of the horizontal magnet can be instantaneously changed, and that too by a cause which shall be put in operation by the motion of the magnet itself; how this can be effected, will not be difficult to conceive, when I mention that instead of a permanent steel magnet in the moveable part of the apparatus, a soft iron galvanic magnet is used.*

The change of polarity is produced simply by soldering to the extremities of the wires which surround the galvanic magnet, two small galvanic batteries in such a manner that the vibrations of the magnet itself may immerse these alternately into vessels of diluted acid; care being taken that the batteries are so attached that the current of galvanism from each shall pass around the magnet in an opposite direction.

Instead of soldering the batteries to the ends of the wires, and thus causing them at each vibration to be lifted from the acid by the power of the machine, they may be permanently fixed in the vessels, and the galvanic communication formed by the amalgamated ends of the wires dipping into cups of mercury.



[Electro-magnetic Engine.]

* For a method of constructing the galvanic magnet on an improved plan, see my paper in vol. XIX, p. 400 of this Journal. [*Ante*, p. 37.]

The whole will be more readily understood by a reference to the annexed drawing: *a b* is the horizontal magnet, about seven inches long, and movable on an axis at the centre: its two extremities when placed in a horizontal line, are about one inch from the north poles of the upright magnets *c* and *d*. *g* and *f* are two large tumblers containing diluted acid, in each of which is immersed a plate of zinc surrounded with copper. *l*, *m*, *s*, *t*, are four brass thimbles soldered to the zinc and copper of the batteries and filled with mercury.

The galvanic magnet *ab* is wound with three strands of copper bell-wire, each about twenty-five feet long; the similar ends of these are twisted together so as to form two stiff wires, which project beyond the extremity *b*, and dip into the thimbles *s*, *t*.

To the wires *q*, *r*, two other wires are soldered so as to project in an opposite direction, and dip into the thimbles *l*, *m*. The wires of the galvanic magnet have thus, as it were, four projecting ends; and by inspecting the figure it will be seen that the extremity *p*, which dips into the cup *m* attached to the copper of the battery in *g* corresponds to the extremity *r* connecting with the zinc in *f*.

When the batteries are in action, if the end *b* is depressed until *q*, *r* dips into the cups *s*, *t*, *ab* instantly becomes a powerful magnet, having its north pole at *b*; this of course is repelled by the north pole *d*, while at the same time it is attracted by *c*, the position is consequently changed, and *o*, *p* comes in contact with the mercury in *l*, *m*; as soon as the communication is formed, the poles are reversed, and the position again changed. If the tumblers be filled with strong diluted acid, the motion is at first very rapid and powerful, but it soon almost entirely ceases. By partially filling the tumblers with weak acid, and occasionally adding a small quantity of fresh acid, a uniform motion, at the rate of seventy-five vibrations in a minute, has been kept up for more than an hour: with a large battery and very weak acid, the motion might be continued for an indefinite length of time.

The motion, here described, is entirely distinct from that produced by the electro-magnetic combination of wires and magnets; it results directly from the mechanical action of ordinary magnetism: galvanism being only introduced for the purpose of changing the poles.

My friend, Prof. Green, of Philadelphia, to whom I first exhibited this machine in motion, recommended the substitution of galvanic magnets for the two perpendicular steel ones. If an article of this kind was to be constructed on a large scale, this would undoubtedly be the better plan, as magnets of that kind can be made of any required power; but for a small apparatus, intended merely to exhibit the motion, the plan here described is perhaps the most convenient.

ON A DISTURBANCE OF THE EARTH'S MAGNETISM, IN CONNECTION WITH THE APPEARANCE OF AN AURORA BOREALIS, AS OBSERVED AT ALBANY, APRIL 19TH, 1831.*

(Silliman's American Journal of Science, April, 1832; vol. XXII, pp. 143–155.)

That the aurora has some connection with the magnetism of the earth, was asserted as early as the middle of the last century; and since that time many observations have been recorded tending to confirm this position. 1. It has been observed that when the aurora appears near the northern horizon in the form of an arch the middle of this is not in the direction of the true north, but in that of the magnetic needle at the place of observation, and that when the arch rises towards the zenith it constantly crosses the heavens at right angles, not to the true, but to the magnetic meridian. This fact is most obvious where the variation of the needle is great. 2. When the beams of the aurora shoot up so as to pass the zenith, which is sometimes the case, the point of their convergence is in the direction of the prolongation of the dipping needle at the place of observation. 3. It has also been observed that during the appearance of an active and brilliant aurora the magnetic needle often becomes restless, varies sometimes several degrees, and does not resume its former position until after several hours.

From the above facts, it has been generally inferred that the aurora is in some way connected with the magnetism of the earth; and that the simultaneous appearance of the meteor, and the disturbance of the needle, are either related as cause and effect, or as the common result of some more general and unknown cause.

The subject is however involved in much obscurity; and there are some facts which tend to throw doubt on the connection of the two phenomena. The accurate and valuable observations of Col. Beaufoy in England, continued for several years, add nothing towards establishing the fact of the

* Communicated to the Albany Institute, January 26, 1832.

magnetic influence of the aurora; and in the scientific expeditions under Capt. Parry to the north, in the peculiar regions, as it would appear, of this meteor, no unusual disturbance of the needle was observed to accompany the aurora, although the apparatus was visited every hour in the day, and sometimes oftener, when any thing rendered it desirable. Indeed, so far from producing a disturbing effect, Dr. Brewster concludes, from a comparison of the observations, that the aurora, in the arctic regions, seems rather to exercise a sedative influence.*

On the other hand, Dr. Richardson states, from his own observations, made at Bear Lake, during six successive months of the years 1825–6, and again in 1826–7, that the aurora does influence the magnetic needle. "A careful review of the daily register," says he, "has led me to form the following conclusion: That brilliant and active coruscations cause a deflection of the needle almost invariably, if they appear through a foggy atmosphere, and if prismatic colors are exhibited; on the contrary, when the atmosphere is clear, and the aurora presents a dense steady light of a yellow color, and without motion, the needle is often unaffected." †

In this state of knowledge, every additional fact becomes of some importance. The following communication, it is therefore hoped, may be useful, either in directing the attention of observers in this country to the subject, or in corroborating similar observations made in other quarters of the globe.

In September, 1830, I commenced a series of observations, for Professor Renwick, of Columbia College, to determine the magnetic intensity at Albany. In the course of these, I unexpectedly witnessed a disturbance of the magnetism of the earth, in connection with an appearance of an aurora, which on some accounts appears interesting.

The needles used in these observations were those mentioned in Capt. Sabine's letter to Prof. Renwick, published

* Edinburgh Philosophical Journal of Science, vol. 8.

† Edinburgh New Philosophical Journal, vol. 5.

in the 17th volume of the *American Journal of Science.* One of these, it will be recollected, formerly belonged to Prof. Hansteen, of Norway, and the other to Capt. Sabine. They were suspended, according to the method of Hansteen, in a small mahogany box, by a single fibre of raw silk. The box was furnished with a glass cover, and had a graduated arc of ivory on the bottom to mark the amplitude of the vibrations. It had also two small circular windows, diametrically opposite to each other, through which the oscillations of the needle could be seen.

In using this apparatus, the time of three hundred vibrations was noted by a quarter second watch, well regulated to mean time; a register being made at the end of every tenth vibration, and a mean deduced from the whole, taken as the true time of the three hundred vibrations. Experiments carefully made with this apparatus, were found susceptible of considerable accuracy; as the individual observations, after a small correction for temperature, give a result, except in a few instances, differing from the mean of a number made under similar circumstances, by a quantity not greater than one part in nearly a thousand.

The observations were repeated daily, when the weather would permit, from the latter part of September to the last of November, either at the hours of 12 noon, or between 5 and 6 P. M.* I was always assisted in making them by the same person,—my relative, Mr. Stephen Alexander,—to whose skill and experience I am much indebted for any accuracy they may possess.

In April, 1831, a new series was commenced, to determine if the needles still indicated the same degree of magnetic intensity. No material difference was observed, except in the following instance, when a remarkable anomaly was exhibited.

On the 19th of April, at 12 o'clock noon, an observation was made with the Hansteen needle, the result of which differed only the fractional part of a second from the usual

*These times were chosen only on account of being most convenient.

mean rate of this needle. At 6 o'clock P. M. the same day, another observation was made with the same needle, and apparently under the same circumstances; but a remarkable change was now observed in the time of its making three hundred vibrations, indicating a great increase in the magnetic intensity of the earth. It was at first supposed that the needle had accidentally been placed contiguous to some ferruginous substance; but on a most careful investigation, nothing could be discovered which would tend in the least degree to explain the cause of the phenomenon. The experiment was made at the usual place, with the box containing the needle resting on a post permanently fixed for the purpose, in the Academy Park, at a sufficient distance from every disturbing object, and with the usual precaution of divesting the person of all articles of iron, such as keys, knives, &c.

At about 9 o'clock in the evening, or three hours after the above observation, an unusual appearance was noticed in the *southern* part of the heavens, which was shortly afterwards recognized as an arch of the aurora. It was about nine degrees in breadth, with the vertex of the arch twenty degrees above the horizon. At this time the northern part of the sky was covered with light fleecy clouds. At forty-five minutes past nine, the clouds partially disappeared, and disclosed the whole northern hemisphere entirely occupied with coruscations of the aurora, shooting up past the zenith, and apparently all converging to the same point. The actual formation of a *corona* might probably have been observed, but for a dark cloud which remained stationary a little south of the zenith. The idea for the first time now occurred to me, that this uncommonly brilliant appearance of the aurora might possibly be connected with the magnetic disturbance observed at 6 o'clock; and in order to test this, the apparatus was again placed on the post in the Academy Park, and an observation made during the most active appearance of the meteor.

The result of the observation was however entirely different from that anticipated; for *instead of still indicating, as at* 6

o'clock, an uncommonly high degree of magnetic intensity, it now showed an intensity considerable lower than usual.

Observations were also made on the 20th and 21st, but no disturbance was again noticed; the intensity had resumed its former state.

The following table exhibits the observed times of three hundred vibrations, with the mean temperature and aspect of the weather during each observation:

Day.	Time of 300 vibrations.	Mean temperature.	Weather.
April 19th, 12 h. noon	980s.75	66½° F.	Cloudy, rain A. M.
" 19th, 6 h. P. M.	968s.65	61°	Clear.
" 19th, 10 h. P. M.	982s.20	52°	Broken clouds.
" 20th, 6 h. P. M.	978s.68	51½°	Clear.

The above observations may be reduced approximately to the uniform temperature of 60°, by the formula,

$$T = T[1 \pm 0.000165(t' \pm t)],^*$$

(T being time, t temperature in degrees of Fahrenheit,) which was deduced from experiments on a similar needle. The relative intensities may also be readily calculated, since they are reciprocally as the squares of the times of the vibrations. In this way, by assuming as unity the time observed on the 20th, we have the following results:

Day.	Time of 300 vibrations at temperature of 60.	Relative intensities.
April 19th, 12 h. noon	979s.94	1.00022
" 19th, 6 h. P. M.	968s.49	1.02401
" 19th, 10 h. P. M.	983s.50	0.99299
" 20th, 6 h. P. M.	980s.05	1.00000

From the mean of several observations made with this needle in April, I consider its time of three hundred vibrations for this month, and in an undisturbed state of terres-

* This formula was obtained by Hansteen.

trial magnetic intensity, to be nine hundred and seventy nine seconds. The accidental errors in the above observations do not probably exceed in any case one second.

At the time of registering the above observations, I had not seen the following remark of Prof. Hansteen, which was subsequently met with in the 12th volume of the *Edinburgh Philosophical Journal*:—"A short time before the aurora borealis appears," says Prof. Hansteen, "the intensity of the magnetism of the earth is apt to rise to an uncommon height; but so soon as the aurora begins, in proportion as its force increases, the intensity of the magnetism of the earth decreases, recovering its former strength by degrees, often not till the end of twenty-four hours."* This statement, founded on observations made in Norway, is a precise description of the phenomenon observed in Albany; and should it be found a general, or even a frequent occurrence, that a great increase of intensity precedes the appearance of the aurora, it would perhaps reconcile many apparent discrepancies in the different accounts of magnetic influence of the meteor.

Prof. Hansteen also remarks, in the same paper, that "The polar lights seem to be the effect of an uncommonly high magnetic intensity, which lets itself off, as it were, by the aurora, and thus sinks under its common strength." Nothing however can with certainty be deduced from these observations, in reference to this supposition; since the magnetic intensity at any place, as exhibited by the vibrations of the horizontal needle, may change while the absolute force or intensity of the whole earth remains the same. If we represent by F the whole force in the direction of the dipping needle, by δ the dip in degrees, and by H the horizontal force, we shall have, by a well known law,

$$F = \frac{H}{\text{cosin } \delta}$$

*I find the same observation has also been made by Humboldt; and also a similar one by Van Swinden, who remarks, that the variation of the needle increases when the aurora borealis is approaching. Journal Royal Institution. Young's Natural Philosophy, vol. 2, p. 442.

In this formula it is evident that F may remain constant, although H is caused to vary by a change in the value of *cosin* δ. The fact therefore of a variation in the absolute intensity, can only be determined by combining the observations of the vibrations of the horizontal needle with simultaneous observations on the dipping needle.

If we suppose F constant during the change of horizontal intensity as observed at Albany, we may, by means of the above formula, calculate the change in declination or dip required to produce the observed difference in the horizontal intensity. Assuming $\delta = 75°$, (the dip at Albany nearly,) and $H =$ to the horizontal intensity observed at 6 o'clock, we can readily find the value of F; and since this value is supposed constant, by substituting it in the expression

$$\text{cosin } \delta = \frac{H'}{F}$$

in which H' represented the intensity observed at 10 o'clock, we shall have the value of δ (the dip) corresponding to the latter intensity. In this way, the change observed in the horizontal intensity at the time of the aurora, gives 28′ 48″ as the deviation of the needle in the plane of the dip.

The aurora which appeared in connection with this magnetic disturbance, was probably one of the most interesting ever observed in this country, particularly from the circumstance of the actual formation of a *corona*, which was seen in several parts of this State. My friend Prof. Joslin, of Union College, who happened to be in New York at the time, has furnished me with the following account:

"The aurora borealis of 19th April, as it appeared in the city of New York at 9 P. M., was peculiarly interesting, on account of the meeting of the luminous columns in the magnetic meridian, at the point in the direction of the dipping needle towards which they usually tend. The luminous matter occupied the whole northern half of the visible celestial hemisphere, and was very much condensed near the point of convergence. Some of the eastern coruscations were at times transiently curved, as though their middle parts (as was probably the case) were driven eastward by the

impulse of the westerly breeze which was blowing at the time. A luminous band was at one time extended across the heavens, at right angles to the meridian, and 30° south of the zenith. This had at times an oscillatory motion in a north and south direction. It passed near the moon, around which was one of the large halos. The sky had been previously clear. The converging rays appeared to meet at the star δ Leonis."

By computing the position of δ Leonis for 9 o'clock on the evening of the 19th, its altitude was found to be 70° 25′, and its azimuth 11° 27′ east. A small error in time however would make a great difference in the azimuth. The dip of the needle at New York is 73°, and the variation probably between 4° and 5°, as it is $6\frac{3}{4}$° at Albany.

The aurora was also seen by Dr. William Campbell, at Cherry Valley. He describes it as very brilliant, and assuming a variety of forms; at one time appearing as a stupendous arch, crossing the heavens from east to west; at another, radiating from a point south of the zenith. The Rev. Mr. Thummel, of the Hartwick Seminary, at his residence in Otsego county, likewise observed the same aurora. He describes it as radiating in every direction from a nucleus near the zenith, which appeared clear and compact for some time, when it began to move, and darted forth rays in every direction like crystals.

MARCH 6, 1832.

Since the foregoing was communicated to the Institute, several particulars have been learned in reference to the subject, which, on some accounts, are deemed interesting. The Annual Meteorological Reports of the different Academies in the State of New York, to the Regents of the University, have been received; and from them it appears that the aurora of the 19th of April was visible over the whole extent of the State, and probably considerably west of it. It is described as being very brilliant at Lewiston on the Niagara river, extending high, and farther to the south than any before observed. In the eastern part of the State, it was seen at most of the Academies along the Hudson, and at Eras-

mus Hall, on Long Island. It also appeared brilliant at Potsdam in St. Lawrence county, the most northern Academy in the State. It was probably not seen very extensively in the States east of New York, as I am informed the weather in the eastern part of New England was cloudy at the time, accompanied with rain. The aurora is described as shooting up to the zenith at North Salem; and at Middlebury as consisting of coruscations in almost every part of the visible heavens. At Fairfield, it illuminated nearly the whole heavens; a number of bows, commencing in the northwest, passed south of the zenith, and terminated in the northeast. An interesting account is given of its appearance at Utica, where it is described as rising at one time in streams of light, of purple, yellow, green, and other colors, and exhibiting a rapid horizontal motion, passing and repassing like a company of dancers. The actual intersection of the beams so as to form the appearance called the *corona*, is mentioned as having been seen in the city of New York, at Hartwick, Cherry Valley, Hudson, and Prattsburg in Steuben county.

The only plausible explanation of the formation of the *corona*, is that which supposes the beams of the aurora to consist of cylindrical portions of some kind of matter, which becomes luminous as it passes into the higher regions of the atmosphere; and that the cylindrical beams shoot up from many points of the earth's surface, nearly parallel to each other, and in the direction of the dipping needle. Being at different distances from the observer, they appear of different elevations; and sometimes, when seeming to overlap each other, they form continued streaks of light in every part of the visible heavens. The corona, according to this hypothesis, is the perspective projection on the sky, of the beams which are shooting up at the same instant on all sides of the observer, and which, being all parallel to the dipping needle, appear to converge as it were to a vanishing point, situated, in the State of New York, about 15° south of the zenith. If this hypothesis be correct, (and it seems a strict geometrical deduction from actual appearances,) it would follow that on the evening of the 19th of April,

beams of auroral matter, were shooting up from every part of the surface of the State of New York.

But the most interesting circumstance in reference to this aurora, is that which I have learned from the December number of the *Journal of the Royal Institution of Great Britain*, viz., the fact of a disturbance of terrestrial magnetism being observed by Mr. Christie in England, on the same evening, and at nearly the same time the disturbance was witnessed in Albany, and that too in connection with the appearance of an aurora.

Mr. Christie had adjusted a magnetic needle for the express purpose of observing the effect when an aurora should appear, but was not so fortunate as to be able to make any observations with it until the evening of the 19th of April. His apparatus consisted of a light needle six inches long, suspended within a compass box by a fine brass wire $\frac{1}{600}$ of an inch in diameter, and twenty-three inches long. The needle was deflected from the magnetic meridian by the repulsive action of two bar magnets placed on opposite sides of it; so that instead of pointing to the magnetic north, it settled in the direction of N. 37° W. As the needle assumed this position in consequence of the attractive force of the earth, and the repulsive force of the magnets, a deviation from the north towards the west would indicate a diminution of the terrestrial horizontal intensity, and a deviation towards the north an increase in that intensity, the intensity of the magnets remaining the same. At 10 o'clock P. M. on the evening of the 19th, during the appearance of the aurora, Mr. Christie found the needle vibrating between N. 43° 40′ W. and N. 42° 40′ W. At 10h. 15m. its direction was N. 34° W. It continued to approach the north until 10h. 37½m. when it pointed N. 33° 30′ W. It again receded from the pole, and at 10h. 40m. vibrated between N. 37° W. and N. 36° W. The next morning at 7h. 20m. the needle pointed N. 40° W. From this brief abstract of Mr. Christie's observations, it will be seen that the horizontal intensity was less than usual at 10 o'clock; that it increased until 10h. 37½m. when it was greater than in its undisturbed state;

and that it again decreased, and was less than usual the next morning at 7h. 20m.

By adding five hours to the time of the observations made at Albany, we shall have nearly the corresponding time at Mr. Christie's residence in Woolwich. These times being 6h. and 10h. P. M. will therefore correspond with 11h. P. M. and 3h. A. M. of time at Woolwich. From this it appears that the observations at Albany were made at a period of absolute time between the last observation of Mr. Christie on the evening of the 19th, and the morning of the 20th. The only interesting result however which apparently can be drawn from a comparison of the observations, is that at both places there was a disturbance of terrestrial intensity at the same time; the intensity rising above and sinking below its usual state at each, although these changes did not occur in the same order at both places.

I am not aware that a simultaneous disturbance of terrestrial magnetism, in connexion with an aurora, has ever before been noted at two places so distant from each other. Nor do I think the co-incidence in this case in the least degree accidental. On the contrary, it appears to me highly probable that the disturbing cause was not only common to both places, but was also active at the same time in a great portion of the northern part of the globe. A brilliant aurora is by no means a local phenomenon. That of the 28th of August, 1827, was visible over nearly the whole of the northern States, in Canada, and also from some part of the Atlantic ocean. But what places the extensive and simultaneous appearance of the aurora in a more striking point of view than any in which it perhaps was ever before exhibited, is the comparison of the notices of the aurora given under the monthly meteorological reports in the *Annals of Philosophy* for 1830 and 1831, and the Reports of the Regents of the University of the State of New York for the same period. By inspecting these two publications, it will be seen that from April 1830 to April 1831 inclusive, the aurora borealis was remarkably frequent and brilliant, both in Europe and in this country; and *that most of the auroras*

described in the Annals for this time, particularly the brilliant ones, were seen on the same evening in England and in the State of New York.

The particular days on which the aurora appeared in England, are not mentioned in the Annals, except when the aurora is considered on some accounts interesting. By comparing those which are thus noticed with the Regents' Reports, the following results are obtained:

The first aurora mentioned in the *Annals* for 1830, occurred on the 19th of April. A particular description is given of its appearance in England, and also a notice of its being seen in Scotland. In the State of New York, a brilliant aurora was extensively seen on the same evening. Accounts are given of it from Auburn, Cambridge, Canajoharie, Cayouga, Franklin, Hudson, Lansingburgh, Lowville, Oxford, Pompey, Rochester, Union, Cazenovia, and Utica.

The second aurora noticed in the *Annals*, is that of the 20th of August. An aurora was also seen in the State of New York, at Lowville, Pompey, Cazenovia, and is particularly described as presenting an unusual appearance at Utica.

The next aurora which appeared worthy of a particular notice in the *Annals*, happened on the 7th of September; and the same evening an aurora was seen at Lewiston in Niagara county. On the 17th of the same month an aurora was also observed in England, and the same time at Pompey, St. Lawrence and Utica.

Under the report of the meteorology for the month of October, in the *Annals*, two auroras are described as appearing, one on the evening of the 5th, and the other on that of the 16th. These were both seen in the State of New York, the first at Utica, and the second at Lowville.

Two auroras are particularly mentioned as appearing in England in November; but no corresponding ones are noticed in the Report of the Regents, as having been seen in the State of New-York.

In the meteorological reports for the month of December, in the *Annals*, there are five auroras mentioned. The most

interesting of these happened on the 11th, and exhibited peculiar appearances. At one time, from a segment of the horizon of 70 degrees in extent, there emanated several flame-colored perpendicular columns, some of which were 2 degrees wide and 30 in altitude: these were succeeded by others, which ultimately exhibited red and purple tints. Many persons in England saw the aurora, and described it as exhibiting an awful appearance from a mixture of the colors. The most brilliant aurora which appeared in the State of New York during 1830, happened on the same evening. At Albany, it extended nearly 90 degrees around the northern horizon; and at one time, a row of bright columns rose from an arch, and extended upwards, some of them nearly to the north star. The columns from the western limb of the arch were slightly tinged with redness; all the others were white. At Lowville, flashes of light are described as arising from the north to the zenith, and thence descending half-way to the southern horizon. It was brilliant at Auburn, Dutchess, Erasmus Hall, Lansingburgh, Hartwick, Lewiston, North Salem, Plattsburgh, Rochester, St. Lawrence, Union, and Utica. An aurora also appeared on the 12th of the same month, and a brilliant one was likewise seen in the State of New York, at Auburn, Dutchess, Franklin, Fredonia, Ithaca, Lansingburgh, Lewiston, Middlebury, North-Salem, Plattsburgh, Pompey, St. Lawrence, Utica. Faint auroras are also mentioned as appearing in England on the 13th and 14th, and another on the evening of the 25th; but no corresponding ones are described in the Regents' Report.

In 1831, the first aurora described in the *Annals* is that of the 7th of January; "and of all the auroræ boreales," says the author, "that have been observed here (in England) the last twenty years, (some say forty,) this was the most extensive, the most beautiful in colors, and the most interesting on account of the singular phenomena which it displayed, in the number of distinct luminous bows which were presented in the course of the night." Several communications are given on the subject of this aurora, in the *Annals of Phi-*

losophy, and the *Journal of the Royal Institution*. It was seen at Paris, and at Brussels. A particular description is given of its appearance in Utrecht, by Prof. Moll. On inspecting the Reports for 1831, I find that an aurora was seen in the State of New-York, at places in the extreme east and west part of the State—at North-Salem on the east side of the Hudson river, and Fredonia near Lake Erie; and intermediate to these places, at Utica, and Pompey. The *Annals* also mention that faint auroras were seen on the evening preceding and following, and also an aurora on the 11th. An aurora was noticed at several places in New York on the evening of the 6th, but none on that of the 8th or 11th.

No auroras are mentioned in the *Annals* under the meteorology for February, but three are noticed for March; the first, an interesting one, appeared on the 7th; the second, on the 8th; and the third, a bright one, on the 11th. By referring to the Reports of the Regents, it will be seen that auroras were observed on the same evening in several places in the State of New York.

The next aurora mentioned in the *Annals* is that of the 19th of April, which has been the principal subject of this paper. An interesting account is given of its appearance in England, which states that at one time there was a grand display of about ten long active streamers along an arch of the aurora, several of which ascended to an altitude of sixty degrees; and when most active, many passed beyond the zenith, exhibiting at the same time several prismatic colors. At 10 o'clock, the arch of the aurora extended 150 degrees. The extensive appearance of this aurora in the State of New York, and the magnetic disturbance accompanying it, have already been sufficiently described.

The above co-incidences appear too numerous to admit the supposition that they are merely accidental, particularly when it is recollected that there are many causes to prevent the co-temporaneous appearance of an aurora being recorded at two distant places, although it exists at both. While it is observed at one place, it may be obscured by clouds, or may escape the notice of the meteorological observer, at the other.

Besides this, the co-incidences occurred on the evenings when the aurora was most brilliant, and consequently when its action might be supposed most extensive. These simultaneous appearances of the meteor in Europe and America would therefore seem to warrant the conclusion, that the aurora borealis cannot be classed among the ordinary local meteorological phenomena, but that it must be referred to some cause connected with the general physical principles of the globe; and that the more energetic actions of this cause, whatever it may be, affects simultaneously a great portion of the northern hemisphere.

ON THE PRODUCTION OF CURRENTS AND SPARKS OF ELECTRICITY FROM MAGNETISM.

(Silliman's American Journal of Science, July, 1832; vol. XXII, pp. 403–408.)

Although the discoveries of Oersted, Arago, Faraday, and others, have placed the intimate connection of electricity and magnetism in a most striking point of view, and although the theory of Ampere has referred all the phenomena of both these departments of science to the same general laws, yet until lately one thing remained to be proved by experiment, in order more fully to establish their identity; namely, the possibility of producing electrical effects from magnetism. It is well known that surprising magnetic results can readily be obtained from electricity, and at first sight it might be supposed that electrical effects could with equal facility be produced from magnetism; but such has not been found to be the case, for although the experiment has often been attempted, it has nearly as often failed.

It early occurred to me, that if galvanic magnets on my plan were substituted for ordinary magnets, in researches of this kind, more success might be expected. Besides their great power, these magnets possess other properties, which render them important instruments in the hands of the experimenter; their polarity can be instantaneously reversed, and their magnetism suddenly destroyed or called into full action, according as the occasion may require. With this view, I commenced, last August, the construction of a much larger galvanic magnet than, to my knowledge, had before been attempted, and also made preparations for a series of experiments with it on a large scale, in reference to the production of electricity from magnetism. I was however at that time accidentally interrupted in the prosecution of these experiments, and have not been able since to resume them, until within the last few weeks, and then on a much smaller scale than was at first intended. In the mean time, it has been announced in the 117th number of the *Library of Useful Knowledge*, that the result so much sought after

has at length been found by Mr. Faraday of the Royal Institution. It states that he has established the general fact, that when a piece of metal is moved in any direction, in front of a magnetic pole, electrical currents are developed in the metal, which pass in a direction at right angles to its own motion, and also that the application of this principle affords a complete and satisfactory explanation of the phenomena of magnetic rotation. No detail is given of the experiments, and it is somewhat surprising that results so interesting, and which certainly form a new era in the history of electricity and magnetism, should not have been more fully described before this time in some of the English publications; the only mention I have found of them is the following short account from the *Annals of Philosophy* for April, under the head of Proceedings of the Royal Institution:

"Feb. 17.—Mr. Faraday gave an account of the first two parts of his researches in electricity; namely, Volta-electric induction and magneto-electric induction. If two wires, *A* and *B*, be placed side by side, but not in contact, and a Voltaic current be passed through *A*, there is instantly a current produced by induction in *B*, in the opposite direction. Although the principal current in *A* be continued, still the secondary current in *B* is not found to accompany it, for it ceases after the first moment, but when the principal current is stopped then there is a second current produced in *B*, in the opposite direction to that of the first produced by the inductive action, or in the same direction as that of the principal current.

"If a wire, connected at both extremities with a galvanometer, be coiled in the form of a helix around a magnet, no current of electricity takes place in it. This is an experiment which has been made by various persons hundreds of times, in the hope of evolving electricity from magnetism, and as in other cases in which the wishes of the experimenter and the facts are opposed to each other, has given rise to very conflicting conclusions. But if the magnet be withdrawn from or introduced into such a helix, a current of electricity is produced *whilst the magnet is in motion*, and is rendered evident by the deflection of the galvanometer. If a single wire be passed by a magnetic pole, a current of electricity is induced through it which can be rendered sensible."*

*[Phil. Mag.; and Annals of Philosophy; April, 1832: vol. xi, p. 300.]

Before having any knowledge of the method given in the above account, I had succeeded in producing electrical effects in the following manner, which differs from that employed by Mr. Faraday, and which appears to me to develop some new and interesting facts. A piece of copper wire, about thirty feet long and covered with elastic varnish, was closely coiled around the middle of the soft iron armature of the galvanic magnet, described in vol. XIX of the *American Journal of Science*, and which, when excited, will readily sustain between six hundred and seven hundred pounds. The wire was wound upon itself so as to occupy only about one inch of the length of the armature which is seven inches in all. The armature, thus furnished with the wire, was placed in its proper position across the ends of the galvanic magnet, and there fastened so that no motion could take place. The two projecting ends of the helix were dipped into two cups of mercury, and there connected with a distant galvanometer by means of two copper wires, each about forty feet long. This arrangement being completed, I stationed myself near the galvanometer and directed an assistant at a given word to immerse suddenly, in a vessel of dilute acid, the galvanic battery attached to the magnet. At the instant of immersion, the north end of the needle was deflected 30° to the west, indicating a current of electricity from the helix surrounding the armature. The effect however appeared only as a single impulse, for the needle, after a few oscillations, resumed its former undisturbed position in the magnetic meridian, although the galvanic action of the battery, and consequently the magnetic power was still continued. I was however much surprised to see the needle suddenly deflected from a state of rest to about 20° to the east, or in a contrary direction when the battery was withdrawn from the acid, and again deflected to the west when it was re-immersed. This operation was repeated many times in succession, and uniformly with the same result, the armature the whole time remaining immovably attached to the poles of the magnet, no motion being required to produce the effect, as it appeared to take place only in consequence of the instantaneous development

of the magnetic action in one, and the sudden cessation of it in the other.

This experiment illustrates most strikingly the reciprocal action of the two principles of electricity and magnetism, if indeed it does not establish their absolute identity. In the first place, magnetism is developed in the soft iron of the galvanic magnet by the action of the currents of electricity from the battery, and secondly the armature, rendered magnetic by contact with the poles of the magnet, induces in its turn currents of electricity in the helix which surrounds it; we have thus as it were electricity converted into magnetism and this magnetism again into electricity.

Another fact was observed which is somewhat interesting inasmuch as it serves in some respects to generalize the phenomena. After the battery had been withdrawn from the acid, and the needle of the galvanometer suffered to come to a state of rest after the resulting deflection, it was again deflected in the same direction by partially detaching the armature from the poles of the magnet to which it continued to adhere from the action of the residual magnetism, and in this way, a series of deflections, all in the same direction, was produced by merely slipping off the armature by degrees until the contact was entirely broken. The following extract from the register of the experiments exhibits the relative deflections observed in one experiment of this kind.

At the instant of immersion of the battery,	deflection	40° west.
" " emersion "	"	18° east.
Armature partially detached,	"	7° east.
Armature entirely detached,	"	12° east.

The effect was reversed in another experiment, in which the needle was turned to the west in a series of deflections by dipping the battery but a small distance into the acid at first and afterwards immersing it by degrees.

From the foregoing facts, it appears that a current of electricity is produced, for an instant, in a helix of copper wire surrounding a piece of soft iron whenever magnetism is induced in the iron; and a current in an opposite direction when the magnetic action ceases; also that an instantaneous current in one or the other direction accompanies every change in the magnetic intensity of the iron.

Since reading the account before given of Mr. Faraday's method of producing electrical currents I have attempted to combine the effects of motion and induction; for this purpose a rod of soft iron ten inches long and one inch and a quarter in diameter, was attached to a common turning lathe, and surrounded with four helices of copper wire in such a manner that it could be suddenly and powerfully magnetized, while in rapid motion, by transmitting galvanic currents through three of the helices; the fourth being connected with the distant galvanometer was intended to transmit the current of induced electricity; all the helices were stationary while the iron rod revolved on its axis within them. From a number of trials in succession, first with the rod in one direction then in the opposite, and next in a state of rest, it was concluded that no perceptible effect was produced on the intensity of the *magneto-electric* current by a rotary motion of the iron combined with its sudden magnetization.

The same apparatus however furnished the means of measuring separately the relative power of motion and induction in producing electrical currents. The iron rod was first magnetized by currents through the helices attached to the battery and while in this state one of its ends was quickly introduced into the helix connected with the galvanometer; the deflection of the needle in this case was seven degrees. The end of the rod was next introduced into the same helix while in its natural state and then suddenly magnetized; the deflection in this instance amounted to thirty degrees, showing a great superiority in the method of induction.

The next attempt was to increase the *magneto electric* effect while the magnetic power remained the same, and in this I was more successful. Two iron rods six inches long and one inch in diameter, were each surrounded by two helices and then placed perpendicularly on the face of the armature, and between it and the poles of the magnet, so that each rod formed as it were a prolongation of the poles, and to these the armature adhered when the magnet was excited. With this arrangement, a current from one helix produced a deflection of thirty-seven degrees; from two helices both on the same

rod fifty two degrees, and from three fifty nine degrees; but when four helices were used, the deflection was only fifty five degrees, and when to these were added the helix of smaller wire around the armature, the deflection was no more than thirty degrees. This result may perhaps have been somewhat affected by the want of proper insulation in the several spires of the helices, it however establishes the fact that an increase in the electric current is produced by using at least two or three helices instead of one. The same principle was applied to another arrangement which seems to afford the maximum of electric development from a given magnetic power; in place of the two pieces of iron and the armature used in the last experiments, the poles of the magnet were connected by a single rod of iron, bent into the form of a horse-shoe, and its extremities filed perfectly flat so as to come in perfect contact with the faces of the poles; around the middle of the arch of this horse-shoe, two strands of copper wire were tightly coiled one over the other. A current from one of these helices deflected the needle one hundred degrees, and when both were used the needle was deflected with such force as to make a complete circuit. But the most surprising effect was produced when instead of passing the current through the long wires to the galvanometer, the opposite ends of the helices were held nearly in contact with each other, and the magnet suddenly excited; in this case a small but vivid spark was seen to pass between the ends of the wires and this effect was repeated as often as the state of intensity of the magnet was changed.

In these experiments the connection of the battery with the wires from the magnet was not formed by soldering, but by two cups of mercury which permitted the galvanic action on the magnet to be instantaneously suspended and the polarity to be changed and rechanged without removing the battery from the acid; a succession of vivid sparks was obtained by rapidly interrupting and forming the communication by means of one of these cups; but the greatest effect was produced when the magnetism was entirely destroyed and instantaneously reproduced by a change of polarity.

It appears from the May No. of the *Annals of Philosophy* that I have been anticipated in this experiment of drawing sparks from the magnet by Mr. James D. Forbes of Edinburgh, who obtained a spark on the 30th of March; my experiments being made during the last two weeks of June. A simple notification of his result is given, without any account of the experiment, which is reserved for a communication to the Royal Society of Edinburgh; my result is therefore entirely independent of his and was undoubtedly obtained by a different process.

Electrical self-induction in a long helical wire.

I have made several other experiments in relation to the same subject, but which more important duties will not permit me to verify in time for this paper. I may however mention one fact which I have not seen noticed in any work, and which appears to me to belong to the same class of phenomena as those before described; it is this: when a small battery is moderately excited by diluted acid, and its poles which should be terminated by cups of mercury, are connected by a copper wire not more than a foot in length, no spark is perceived when the connection is either formed or broken; but if a wire thirty or forty feet long be used instead of the short wire, though no spark will be perceptible when the connection is made, yet when it is broken by drawing one end of the wire from its cup of mercury, a vivid spark is produced. If the action of the battery be very intense, a spark will be given by the short wire; in this case it is only necessary to wait a few minutes until the action partially subsides, and until no more sparks are given from the short wire; if the long wire be now substituted a spark will again be obtained. The effect appears somewhat increased by coiling the wire into a helix; it seems also to depend in some measure on the length and thickness of the wire. I can account for these phenomena only by supposing the long wire to become charged with electricity, which by its re-action on itself projects a spark when the connection is broken.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. I.

DESCRIPTION OF A GALVANIC BATTERY FOR PRODUCING ELECTRICITY OF DIFFERENT INTENSITIES.

(Transactions American Philosophical Society, n. s., vol. v, pp. 217–222.)*

Read January 16th, 1835.†

The following account of a Galvanic Battery, constructed under my direction for the Physical Department of the College of New Jersey, is submitted to the American Philosophical Society, with the intention of referring to it in some communications which I purpose making on the subject of Electricity and Magnetism. It is hoped however that the arrangement and details of the instrument, in themselves, will be found to possess some interest, since they have been adopted in most cases after several experiments and much personal labor.

The apparatus is intended to exhibit most of the phenomena of Galvanism and all those of Electro-Magnetism, on a large scale, with one battery. It was constructed to illustrate the several facts of these branches of science to my class, and also to be used as a convenient instrument of research in all cases where no very great degree of intensity is required.

The several parts of this battery are not soldered together forming one permanent galvanic arrangement, but are only temporarily connected by means of movable conductors and cups of mercury. The whole is constructed with reference to the principle well understood of producing electricity of greater or less intensity, by a change in the method of uniting the several elements with each other.

The apparatus consists of eighty-eight elements or pairs, composed of plates of rolled zinc nearly one eighth of an inch thick, nine inches wide, and twelve inches long, inserted into copper cases open at top and bottom. Eleven of these

*[The title-page of this volume bears date 1837.]

†[The date given in the "Transactions" is January 14. This appears from the Minutes to be a typographic error.]

elements are suspended together from two cross pieces of wood, and the whole number is thus arranged in eight sets, of eleven in each. These are supported by the ends of the cross pieces in a strong wooden frame, so as to be immersed in eight separate troughs: they thus form as many independent batteries, which can be used separately or together as the occasion may require. Each trough is divided into eleven cells by wooden partitions coated with cement. If one of the cells be charged with dilute acid, a single element may be excited without producing action in any other part of the battery. Each set or battery may also be lifted separately from the frame by its cross pieces, without disturbing the other parts of the apparatus.

The elements remain stationary, while the troughs are raised to them on a movable platform by the common application of a wheel and pinion.

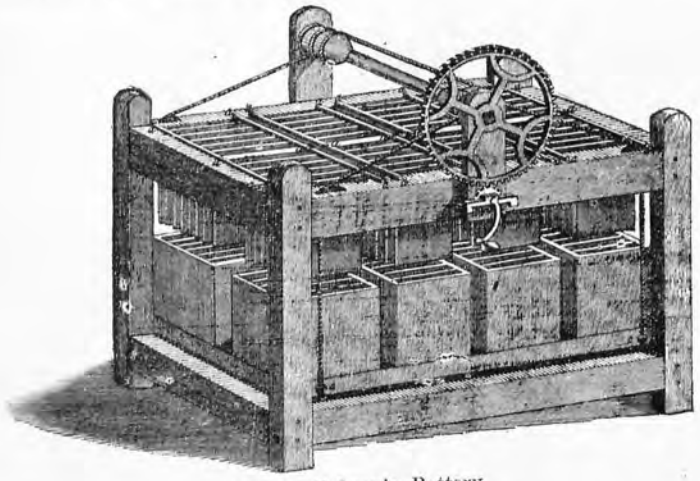


FIG. 1.—Galvanic Battery.

The general arrangement of the whole may be seen at once by a reference to the perspective drawing, fig. 1: *a a*, &c., represent the cross pieces resting on the upper part of the frame of the machine; *c c* is the movable platform.

A perspective view of one of the elements on a larger scale is given in fig. 3. *a a* are two cups of cast copper, with



FIG. 3.

a broad stem on the bottom; one soldered to the zinc plate, and the other to the copper case. The cavity in these cups is about three eighths of an inch wide, a little more than an inch long, and half an inch deep. The cups being well amalgamated and partially filled with mercury, receive the ends of the copper conductors which unite the several elements.

For the purpose of suspension, a slip of copper, *b b*, with a hole in it, is soldered to each upper corner of the copper case; these fit loosely into a mortice or narrow groove in the cross pieces, and are secured by a pin of copper wire. When the pins are withdrawn, a single element may be removed from any part of the series, without disturbing the remainder.

The zinc plate is fastened into its copper case, without touching, by a piece of wood at each corner with a groove in it to receive the edge of the plate. The grooves in the two lower pieces of wood terminate at about a quarter of an inch from the lower end, and thus form shoulders, which prevent the plate from slipping down; while the wood itself is supported by a flange, formed by bending in the lower edges of the corner of the case.

There are two principal sets of connectors; the first is formed of bars of cast copper thirteen inches long, an inch wide, and about an eighth of an inch thick. On the lower side of these are eleven broad projections, which fit loosely



FIG. 4.—Homogeneous Connector.

into a row of cups on the plates of zinc or copper. Fig. 4 represents one of these connectors with a thimble soldered on the upper side for the purpose of attaching a conductor, which may serve as a pole.

There are two of these for each of the eight batteries, and when in their places, one unites all the zinc, and the other all the copper, so that the battery becomes a "calorimotor" of a single element or pair. If with this arrangement the several batteries be connected, zinc to zinc and copper to copper, by conductors reaching from one to the other, the whole apparatus of eighty-eight elements becomes a large "calorimotor" of a single pair; but if the copper of the first be united to the zinc of the second, and so on, it then forms a "calorimotor" of eight elements, and by a simple change may be reduced to one of four, or of two elements.

The other set of connectors consists of short pieces of thick copper plate, the ends of which are bent down at right angles, so as to dip into the cups of mercury: they connect the copper of one element with the zinc of the next. Ten of these, intended to unite the elements of one battery, are shown in



FIG. 5.—Alternate or Serial Connector.

fig. 5. They are attached crosswise to a slip of harness leather, which, by its pliability, permits them to fit loosely into the cups, while it enables the whole set to be removed as one piece. When these connectors are in their places, and the several batteries united, the copper pole of the one, with the zinc pole of another, and so on, the whole series forms a "deflagrator" of eighty-eight elements.

The different arrangements of the several connectors will be readily understood by a reference to the plan drawing, fig. 2, which exhibits one-half of the whole apparatus arranged as a "deflagrator" of forty-four elements, and the other half as a "calorimotor" of four pairs. By closely inspecting the drawing, it will be seen that the connexion in the upper half of the figure is from the copper of the first element to the zinc of the next, and so on through the entire series of forty-four elements. In the lower half the union of copper and zinc takes place only between the poles of the dif-

ferent batteries; the several elements of which are united so as to act as one plate of copper and one of zinc. The four

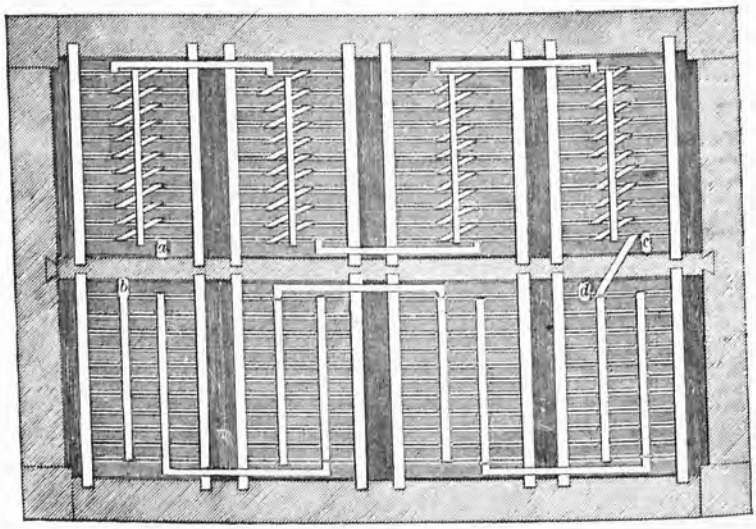


FIG. 2.—Plan of Battery.

batteries therefore will act together as a "calorimotor" of four elements. The arrangement, as given in the drawing, is intended to illustrate by one figure the two sets of connectors; but such an arrangement becomes interesting in practice in determining the effect of the conjoined actions of batteries producing electricity of different intensities.

The circuit of the connections as given in the figure is complete except at *a b;* the two plates at this point form the poles of the battery. A set of poles however may be formed at any other point of the circuit, by making an interruption at that place. In the same way two or more sets may be formed. It furnishes an interesting and instructive experiment to place a pair of large decomposing plates at *a b* and another at *c d*. When only one of these is plunged into a saline solution, the circuit being interrupted at the other pair, no effect is produced; but as soon as this other is plunged into a similar solution, a copious decomposition simultaneously takes place at both. Also the co-temporaneous action in each element of the battery is pleasingly shown by placing

at the same time several large magnetic needles on the different parts of the apparatus. These instantly change their direction when the second pair of decomposing plates touch the solution.

At first sight it might be supposed that there would be some difficulty in entering the several plates into their respective cells, but this is obviated by the precise movement of the platform on which the troughs stand. Its horizontal position is adjusted by four screws (*c c* fig. 1), and its corners slide in grooves in the upright posts of the large frame. Besides this, when the plates are once entered, they are not required to be entirely withdrawn from the cells until the end of the series of experiments; since the acid descends as the plates are withdrawn, and finally fills but little more than three-fourths of the capacity of the cells. When a plate accidentally catches on the side of the cell, the battery to which it belongs is gently raised in its place and the plate adjusted.

This apparatus readily furnishes the means of making comparative experiments on the difference produced by partial and perfect insulation. When no higher degree of intensity is required than that afforded by eight pairs of plates, perfect insulation is obtained by the eight separate troughs. In higher degrees of intensity the partitions in the troughs furnish the means of perfectly insulating forty-eight of the elements: this is effected by simply charging with acid every other cell in each of the troughs, and connecting the corresponding element by conductors, which pass over the intermediate elements without touching them: with this arrangement we have six cells in each trough separated from one another by a cell without acid, or in effect by a stratum of air. For comparison with these a set of troughs has been constructed without partitions.

The want of perfect insulation is not very perceptible in the common experiments of the deflagration of large and perfect conductors; but where the decomposition of a liquid is attempted, or the battery required to act on a small or imperfect conductor, the loss of power is very great, the apparatus

partially discharging itself through its own liquid, and the intensity at the poles does not increase with a short interruption of the current.

There is also considerable loss on account of imperfect insulation even in the case of low intensity, and when the poles are connected by a perfect conductor. In one experiment with an arrangement of five pairs, and the poles united by a conductor composed of thirty strands of copper bell-wire, each forty feet long, the loss was found to be at least one-seventh, as measured by the quantity of zinc surface required to be immersed in order to produce the same magnetic effect. I would infer from this that the most perfect of all Dr. Hare's ingenious galvanic arrangements is that in which the elements dip into separate glass vessels, as this combines perfect insulation with the power of instantaneous immersion.

A variety of experiments have been made during the past year with this instrument on several points of Galvanism and Electro-magnetism, which will be communicated to the Society as soon as my engagements will permit me to repeat and arrange them for publication

FACTS IN REFERENCE TO THE SPARK, ETC., FROM A LONG CONDUCTOR UNITING THE POLES OF A GALVANIC BATTERY.*

(Journal of the Franklin Institute, March, 1835, vol. xv, pp. 169, 170.)

Extract from the proceedings of the stated meeting of the American Philosophical Society, January 16, 1835.†

The following facts in reference to the spark, shock, &c., from a galvanic battery of a single pair when the poles are united by a long conductor, were communicated by Prof. Joseph Henry, and those relating to the spark were illustrated experimentally:

1. A long wire gives a more intense spark than a short one. There is however with a given surface of zinc a length beyond which the effect is not increased; a wire of one hundred and twenty feet gave about the same intensity of spark as one of two hundred and forty feet.

2. A thick wire gives a larger spark than a smaller one of the same length.

3. A wire coiled into a helix gives a more vivid spark than the same wire when uncoiled.

4. A ribbon of copper, coiled into a flat spiral, gives a more intense spark than any other arrangement yet tried.

---

*To the Committee on Publications.

Gentlemen: The American Philosophical Society, at their last stated meeting, authorized the publication of the following abstract of a verbal communication made to the Society by Professor Henry on the sixteenth of January last. A memoir on this subject has been since submitted to the Society containing an extension of the subject, the primary fact in relation to which was observed by Professor Henry as early as 1832, and announced by him in the *American Journal of Science* (vol. xxii, p. 408). Mr. Faraday having recently entered upon a similar train of observations, the immediate publication of the accompanying is important, that the prior claims of our fellow-countryman may not be overlooked.

Very respectfully, yours,

A. D. Bache,

One of the Secretaries Am. Philos. Soc.

Philadelphia, *Feb.* 7th, 1835.

†[The Minutes of the Am. Phil. Soc. from 1743–1837 have only recently been published, (1885,) the first volume of the published "Proceedings" commencing with 1838.]

5. The effect is increased, by using a longer and wider ribbon, to an extent not yet determined. The greatest effect has been produced by a coil ninety-six feet long and weighing 15 lbs.; a larger conductor has not been received.

6. A ribbon of copper, first doubled into two strands and then coiled into a flat spiral, gives no spark, or a very feeble one.

7. Large copper handles, soldered to the ends of a coil of ninety-six feet, and these both grasped, one by each hand, a shock is felt at the elbows, when the contact is broken in a battery of a single pair with one and a half feet of zinc surface.

8. A shock is also felt when the copper of the battery is grasped with one hand and one of the handles with the other; the intensity however is not as great as in the last case. This method of receiving the shock may be called the direct method; the other, the lateral one.

9. The decomposition of a liquid is effected by the use of the coil with a battery of a single pair, by interrupting the current and introducing a pair of decomposing wires.

10. A mixture of oxygen and hydrogen is also exploded by means of the coil, and breaking the contact, in a bladder containing the mixture.

11. The property of producing an intense spark is induced, on a short wire, by introducing at any point of a compound galvanic current a large flat spiral, and joining the poles by the short wire.

12. A spark is produced when the plates of a single battery are separated by a foot or more of diluted acid.

13. Little or no increase in the effect is produced by inserting a piece of soft iron into the centre of a flat spiral.

14. The effect produced by an electro-magnet, in giving the shock, is due principally to the coiling of the long wire which surrounds the soft iron.

[The foregoing article was re-printed in Silliman's American Journal of Science, July, 1835, vol. XXVIII, pp. 327–329.]

APPENDIX TO THE ABOVE—ACTION OF A SPIRAL CONDUCTOR.

(Silliman's Am. Journal of Science, July, 1835, vol. XXVIII, pp. 329–331.)

To Prof. Silliman.

SIR: With this I send you a copy of a paper communicated by me to the American Philosophical Society, on the influence of a spiral conductor in increasing the intensity of electricity from a galvanic arrangement of a single pair. As the part of the Transactions which contains the paper has not yet been distributed, I regret that I am not at liberty to request you to insert the article for more general diffusion in your valuable Journal. An abstract however of the principal facts was ordered to be published, and appeared in the March number of the *Franklin Journal.* A copy was also sent by Prof. Bache for insertion in the *American Journal;* but as it did not appear in the last number, you will confer a favor by inserting it in the next.*

Should you wish to repeat the experiments, you will find them most interestingly exhibited with one of Dr. Hare's "calorimotors." If a galvanic current of very low intensity from this instrument be transmitted through a spiral conductor formed of copper ribbon about one inch wide, from sixty to one hundred feet long, well covered with silk, and the several spires closely wound on each other, the "calorimotor" will be almost converted into a "deflagrator." One end of the conductor being attached to a pole of the battery, and the other brought in contact with or rubbed along the edge of a plate of metal attached to the other pole, a vivid deflagration will be produced, even when the plates are immersed in a mixture containing not more than one part of acid to five hundred parts of water.

If a copper cylinder of about two inches in diameter and four or five inches long, to serve as a handle, be attached to each end of the spiral by an intervening piece of copper wire and these cylinders grasped with moistened hands, a series of shocks will be felt when one end of the conductor

* [Then mislaid, but now inserted; as above.—*Ed.*]

is drawn across the edges of the zinc plates, the other end being in contact with the copper pole.

Another method of producing the shocks is to place the spiral between two batteries, each of a single pair, so as to connect the copper of one with the zinc of the other. If the extreme poles of this compound arrangement be terminated by the copper handles, and these be brought in contact, holding one in each hand, a deflagration of the metal will be produced, and a thrilling sensation, scarcely supportable, felt in each arm. The effect is much increased if the handles are rough. Two cylinders of cast zinc terminating the poles were found to produce the greatest effect when rubbed on each other.

To exhibit these phenomena in a striking manner a galvanic battery of considerable size is required. I have used one for the purpose containing about forty feet of zinc surface, estimating both sides of the plate. This battery was first immersed for a short time in a strong solution of acid, to dissolve the coating of oxide, and then removed to a vessel containing pure water. The small quantity of acid adhering to the plates was sufficient to produce, by means of the spiral, the deflagration of the metals, which would shock and snap for many hours in succession, while with a short conductor the battery in the same state gave no signs of electricity.

This will be found an economical method of exhibiting some very interesting experiments with the calorimotor. After having shown the ordinary heating powers of the instrument with strong acid, transfer the plates to a trough containing pure water, and the action of the coil may be shown for an almost indefinite time, at little or no expense of zinc or acid.

The spiral produces no increased effect when applied to a galvanic trough of one hundred four-inch plates. If however a coil of five or six hundred feet of wire be substituted, an increase of action will be manifest. The length of the coil must be in some ratio to the "projectile" force of the electricity, and also the quantity to the thickness of the conductor, in order to produce a maximum result. Thus, when

a small battery is used with a large conductor, it must be charged with strong acid.

The action of the spiral conductor depends on the inductive principle of an electric current discovered by Mr. Faraday, and is consequently intimately connected with the whole subject of magneto-electricity.

If a magnet be fitted up in the ordinary manner, with a spool of wire covered with silk around the keeper, the intensity of the shock will be astonishingly increased if the current generated in the spool be transmitted through a coil of several hundred feet of fine wire surrounding the legs of the magnet. To produce this effect however it is necessary that the wire on the spool and that around the magnet should at first form a continuous closed circuit, and that this be interrupted at the same instant that the keeper is detached, so that the induced current may pass entirely through the body.

The intense shock may also be given by generating a current with one magnet, and accelerating it by passing it around a second magnet.

Professor Emmet, of the University of Virginia, more than two years since, made the interesting discovery that the magneto-electric current is much increased in intensity by passing it through a portion of the generating magnet. This interesting fact, which he has applied with much success to improve the magneto-electric machine, may undoubtedly be referred to the same cause as the action of the spiral, and I have succeeded in modifying the application of it in several ways.

These magnetic experiments were made on the first or second day of May last, while on a visit to Philadelphia, with the large magnet belonging to the museum, and kindly lent to me for the purpose by Mr. Peale. They were made with the assistance of my friend, Mr. Lukens; but as I have not had an opportunity of verifying them, I cannot at present give a more detailed account. I have also made some preparations for applying the same principle to increase the action of a thermo-electric current.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. II.

ON THE INFLUENCE OF A SPIRAL CONDUCTOR IN INCREASING THE INTENSITY OF ELECTRICITY FROM A GALVANIC ARRANGEMENT OF A SINGLE PAIR, ETC.

(Transactions American Philosophical Society, n. s., vol. v, pp. 223–231.)

Read February 6th, 1835.

In the *American Journal of Science* for July, 1832, I announced a fact in Galvanism which I believe had never before been published. The same fact however appears to have been since observed by Mr. Faraday, and has lately been noticed by him in the November number of the *London and Edinburgh Journal of Science* for 1834.

The phenomenon as described by me is as follows: "When a small battery is moderately excited by diluted acid, and its poles, terminated by cups of mercury, are connected by a copper wire not more than a foot in length, no spark is perceived when the connection is either formed or broken; but if a wire thirty or forty feet long be used instead of the short wire, though no spark will be perceptible when the connection is made, yet when it is broken by drawing one end of the wire from its cup of mercury, a vivid spark is produced. If the action of the battery be very intense, a spark will be given by a short wire; in this case it is only necessary to wait a few minutes until the action partially subsides, and until no more sparks are given from the wire; if the long wire be now substituted a spark will be again obtained. The effect appears somewhat increased by coiling the wire into a helix; it seems also to depend in some measure on the length and thickness of the wire. I can account for these phenomena only by supposing the long wire to become charged with electricity, which by its re-action on itself projects a spark when the connection is broken."*

The above was published immediately before my removal from Albany to Princeton, and new duties interrupted for

*Silliman's Journal of Science, vol. 22, page 408. [See *ante*, p. 79.]

a time the further prosecution of the subject. I have however been able during the past year to resume in part my investigations, and among others, have made a number of observations and experiments which develop some new circumstances in reference to this curious phenomenon.

These, though not as complete as I could wish, are now presented to the Society, with the belief that they will be interesting at this time on account of the recent publication of Mr. Faraday on the same subject.

The experiments are not given in the precise order in which they were first made, but in that which I deem best suited to render them easily understood; they have however been repeated for publication in almost the same order in which they are here given.

1. A galvanic battery, consisting of a single plate of zinc and copper, and exposing one and a half square feet of zinc surface, including both sides of the plate, was excited with diluted sulphuric acid, and then permitted to stand until the intensity of the action became nearly constant. The poles connected by a piece of copper bell-wire of the ordinary size and five inches long, gave no spark when the contact was broken.

2. A long portion of wire, from the same piece with that used in the last experiment, was divided into equal lengths of fifteen feet, by making a loop at each division, which could be inserted into the cups of mercury on the poles of the battery. These loops being amalgamated and dipped in succession into one of the cups while the first end of the wire constantly remained in the other, the effect was noted. The first length, or fifteen feet, gave a very feeble spark, which was scarcely perceptible. The second, or thirty feet, produced a spark a little more intense, and the effect constantly increased with each additional length until one hundred and twenty feet were used; beyond this there was no perceptible increase; and a wire of two hundred and forty feet gave a spark of rather less intensity. From other observations I infer that the length necessary to produce a maximum result, varies with the intensity of the action of the battery, and also with its size.

3. With equal lengths of copper wire of unequal diameters, the effect was greater with the larger; this also appears to depend in some degree on the size of the battery.

4. A length of about forty feet of the wire used in experiments first and second was covered with silk and coiled into a cylindrical helix of about two inches in height and the same in diameter. This gave a more intense spark than the same wire when uncoiled.

5. A ribbon of sheet copper nearly an inch wide, and twenty-eight and a half feet long, was covered with silk, and rolled into a flat spiral similar to the form in which woollen binding is found in commerce. With this a vivid spark was produced, accompanied by a loud snap. The same ribbon uncoiled gave a feeble spark similar in intensity to that produced by the wire in experiment third. When coiled again the snap was produced as at first. This was repeated many times in succession, and always with the same result.

6. To test still farther the influence of coiling, a second ribbon was procured precisely similar in length and in all other respects to the one used in the last experiment. The effect was noted with one of these coiled into a flat spiral and the other uncoiled, and again with the first uncoiled and the second coiled. When uncoiled each gave a feeble spark of apparently equal intensity, when coiled, a loud snap. One of these ribbons was next doubled into two equal strands, and then rolled into a double spiral with the point of doubling at the centre. By this arrangement the electricity, in passing through the spiral, would move in opposite directions in each contiguous spire, and it was supposed that in this case the opposite actions which might be produced would neutralize each other. The result was in accordance with the anticipation; the double spiral gave no spark whatever, while the other ribbon coiled into a single spiral produced as before a loud snap. Lest the effect might be due to some accidental touching of the different spires, the double spiral was covered with an additional coating of silk, and also the other ribbon was coiled in the same manner; the effect with both was the same.

7. In order to increase if possible the intensity of the spark while the battery remained the same, larger spirals were applied in succession. The effect was increased until one of ninety-six feet long, an inch and a half wide and weighing fifteen pounds, was used. The snap from this was so loud that it could be distinctly heard in an adjoining room with the intervening door closed. Want of materials has prevented me from trying a larger spiral conductor than this, but it is probable that there is a length which, with a given quantity and intensity of galvanism, would produce a maximum effect. When the size of the battery is increased, a much greater effect is produced with the same spiral. Thus when the galvanic apparatus, described in the first article, is arranged as a "calorimotor" of eight pairs, the snap produced on breaking contact, with the spiral last described, resembled the discharge of a small Leyden jar highly charged.

8. A handle of thick copper was soldered on each end of the large spiral at right angles to the ribbon similar to those attached to the wires in Pixii's magneto-electric machine for giving shocks. When one of these was grasped by each hand, and the contact broken, a shock was received which was felt at the elbows, and this was repeated as often as the contact was broken. This shock is rather a singular phenomenon, since it appears to be produced by a lateral discharge, and it is therefore important to determine its direction in reference to the primary current.

9. A shock is also received when the copper of the battery is grasped by one hand, and the handle attached to the copper pole of the ribbon with the other. This may be called the direct shock, since it is produced by a part of the direct current. It is however far less intense than that produced by the lateral discharge.

10. When the poles were joined by two coils, connected by a cup of mercury between them, a spark was produced by breaking the circuit at the middle point, and when a pair of platina wires was introduced into the circuit with the large coil and immersed in a solution of acid, decomposition took place in the liquid at each rupture of contact, as was shown

by a bubble of gas given off at each wire. It must be recollected that the shocks and the decomposition here described were produced by the electricity from a single pair of plates.

11. The contact with the poles of the battery and the large spiral being broken in a vessel containing a mixture of hydrogen and atmospheric air, an explosion was produced.

I should also mention that the spark is generally attended with a deflagration of the mercury, and that when the end of the spiral is brought in contact with the edge of the copper cup or the plate of the battery, a vivid deflagration of the metal takes place. The sides of the cup sometimes give a spark when none can be drawn from the surface of the mercury. This circumstance requires to be guarded against when experimenting on the comparative intensities of sparks from different arrangements. If the battery formerly described [fig. 1, page 81] be arranged as a "calorimotor," and one end of a large spiral conductor be attached to one pole, and the other end drawn along the edge of the connector, fig. 4, a series of loud and rapid explosions is produced, accompanied by a brilliant deflagration of the metal, and this takes place when the excitement of the battery is too feeble to heat to redness a small platina wire.

12. A number of experiments were made to determine the effect of introducing a cylinder of soft iron into the axis of the flat spiral, in reference to the shock, the spark, &c.; but no difference could be observed with the large spiral conductor; the effect of the iron was merged in that of the spiral. When however one of the smaller ribbons was formed into a hollow cylindrical helix of about nine inches long, and a cylinder of soft iron an inch and a half in diameter was inserted, the spark appeared a little more intense than without the iron. The obliquity of the spires in this case was unfavorable to their mutual action, while the magnetism was greater than with the flat spiral, since the conductor closely surrounded the whole length of the cylinder.

I would infer from these experiments, that some effects heretofore attributed to magneto-electric action are chiefly due to the re-action on each other of the several spires of the coil which surround the magnet.

13. One of the most singular results in this investigation was first obtained in operating with the large galvanic battery [fig. 2, page 84]. The whole instrument was arranged as a "calorimotor" of eight pairs, and a large spiral conductor introduced into the circuit at *c d*, while a piece of thick copper wire, about five inches long, united the poles at *a b*. In this state an explosion or loud snap was produced, not only when the contact was broken at the spiral, but also when one end of the short wire, at the other extremity of the apparatus, was drawn from its cup. All the other short movable connectors of the battery gave a similar result. When the spiral was removed from the circuit, and a short wire substituted, no effect of the kind was produced. From this experiment it appears that the influence of the spiral is exerted through at least eight alternations of zinc, acid, and copper, and thus gives to a short wire, at the other extremity of the circuit, the power of producing a spark.

14. The influence of the coil was likewise manifest when the zinc and copper plates of a single pair were separated from each other to the distance of fourteen inches in a trough without partitions, filled with diluted acid. Although the electrical intensity in this case must have been very low, yet there was but little reduction in the apparent intensity of the spark.

The spiral conductor produces however little or no increase of effect when introduced into a galvanic circuit of considerable intensity. Thus when the large spiral used in experiments seven, eight, &c., was made to connect the poles of two Cruickshanks troughs, each containing fifty-six four inch plates, no greater effect was perceived than with a short thick wire; in both cases in making the contact a feeble spark was given, attended with a slight deflagration of the mercury. The batteries at the same time were in sufficiently intense action to give a disagreeble shock. It is probable however that if the length of the coil were increased in some proportion to the increase of intensity, an increased effect would still be produced.

In operating with the apparatus described in the last exper-

iment, a phenomenon was observed in reference to the action of the battery itself, which I do not recollect to have seen mentioned, although it is intimately connected with the facts of Magneto-electricity, as well as with the subject of these investigations, viz.: When the body is made to form a part of a galvanic circuit composed of a number of elements, a shock is of course felt at the moment of completing the circuit. If the battery be not very large, little or no effect will be perceived during the uninterrupted circulation of the galvanic current; but if the circuit be interrupted by breaking the contact at any point, a shock will be felt at the moment, nearly as intense as that given when the contact was first formed. The secondary shock is rendered more evident, when the battery is in feeble action, by placing in the mouth the end of one of the wires connected with the poles; a shock and flash of light will be perceived when the circuit is completed, and also the same when the contact is broken at any point, but nothing of the kind will be perceived in the intermediate time, although the circuit may continue uninterrupted for some minutes. This I consider an important fact in reference to the action of the voltaic current.

The phenomena described in this paper appear to be intimately connected with those of Magneto-electricity, and this opinion I advanced with the announcement of the first fact of these researches in the *American Journal of Science*. They may I conceive be all referred to that species of dynamical *Induction* discovered by Mr. Faraday, which produces the following phenomenon, namely: when two wires, *A* and *B*, are placed side by side, but not in contact, and a voltaic current is passed through *A*, there is a current produced in *B*, but in an opposite direction. The current in *B* exists only for an instant, although the current in *A* may be indefinitely continued; but if the current in *A* be stopped, there is produced in *B* a second current, in an opposite direction however to the first current.

The above fundamental fact in Magneto-electricity appears to me to be a direct consequence of the statical principles of "Electrical *Induction*" as mathematically investigated by

Cavendish, Poisson, and others. When the two wires *A* and *B* are in their natural state, an equilibrium is sustained by the attractions and repulsions of the two fluids in each wire; or, according to the theory of Franklin and Cavendish, by the attractions and repulsions of the one fluid, and the matter of the two wires. If a current of free electricity be passed through *A*, the natural equilibrium of *B* will be disturbed for an instant, in a similar manner to the disturbance of the equilibrium in an insulated conductor, by the sudden addition of fluid to a contiguous conductor. On account of the repulsive action of the fluid, the current in *B* will have an opposite direction to that in *A*; and if the intensity of action remains constant, a new state of equilibrium will be assumed. The second state of *B* however may perhaps be regarded as one of tension, and as soon as the extra action ceases in it, the fluid in *B* will resume its natural state of distribution, and thus a returning current for an instant be produced.

The action of the spiral conductor in producing sparks, is but another case of the same action; for since action and reaction are equal and in contrary directions, if a current established in *A* produces a current in an opposite direction in *B*, then a current transmitted through *B* should accelerate or increase the intensity of a current already existing in the same direction in *A*. In this way the current in the several successive spires of the coil may be conceived to accelerate, or to tend to accelerate each other; and when the contact is broken, the fluid of the first spire is projected from it with intensity by the repulsive action of the fluid in all the succeeding spires.

In the case of the double spiral conductor, in experiment six, the fluid is passing in an opposite direction; and according to the same views, a retardation or decrease of intensity should take place.

The phenomenon of the secondary shock with the battery, appears to me to be a consequence of the law of Mr. Faraday. The parts of the human body contiguous to those through which the principal current is passing, may be considered as in the state of the second wire *B*; when the principal current

ceases, a shock is produced by the returning current of the natural electricity of the body.

If this explanation be correct, the same principle will readily account for a curious phenomenon discovered several years since by Savary, but which I believe still remains an isolated fact. When a current is transmitted through a wire, and a number of small needles are placed transverse to it, but at different distances, the direction of the magnetic polarity of the needles varies with their distance from the conducting wire. The action is also periodical; diminishing as the distance increases, until it becomes zero; the polarity of the needles is then inverted, acquires a maximum, decreases to zero again, and then resumes the first polarity; several alternations of this kind being observed.* Now this is precisely what would take place if we suppose that the principal current induces a secondary one in an opposite direction in the air surrounding the conductor, and this again another in an opposite direction at a great distance, and so on. The needles at different distances would be acted on by the different currents, and thus the phenomena described be produced.

The action of the spiral is also probably connected with the fact in common electricity called the lateral discharge: and likewise with an appearance discovered some years since by Nobili, of a vivid light, produced when a Leyden jar is discharged through a flat spiral.

The foregoing views are not presumed to be given as exhibiting the actual operation of nature in producing the phenomena described, but rather as the hypotheses which have served as the basis of my investigations, and which may further serve as formulæ from which to deduce new consequences to be established or disproved by experiment.

Many points of this subject are involved in an obscurity which requires more precise and extended investigation; we may however confidently anticipate much additional light from the promised publication of Mr. Faraday's late researches in this branch of science.

*Cumming's Demonferrand, page 247; also Edinburgh Journal of Science, October, 1826.

NOTICE OF ELECTRICAL RESEARCHES—THE LATERAL DISCHARGE.

(Report of British Association, 1837, vol. VI, part ii, pp. 22–24.)*

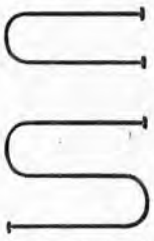
September, 1837.

The primary object of these investigations was to detect, if possible, an inductive action in common electricity analogous to that discovered in a current of galvanism. For this purpose an analysis was instituted of the phenomena known in ordinary electricity by the name of the lateral discharge. Professor Henry was induced to commence with this from some remarks by Dr. Roget on the subject. The method of studying the lateral spark consisted in catching it on the knob of a small Leyden phial, and presenting this to an electrometer. The result of the analysis was in accordance with an opinion of Biot that the lateral discharge is due only to the escape of the small quantity of redundant electricity which always exists on one or the other side of a jar, and not to the whole discharge. The Professor then stated several consequences which would flow from this, namely, that we could increase or diminish the lateral action by the several means which would affect the quantity of redundant or as it may be called free electricity, such as an increase of the thickness of the glass, or by substituting for the small knob of the jar a large ball. But the arrangement which produces the greatest effect is that of a long fine copper wire, insulated parallel to the horizon, and terminated at each end by a small ball. When sparks are thrown on this from a globe of about a foot in diameter, the wire at each discharge becomes beautifully luminous from one end to the other, even if it be a hundred feet long. Rays are given off on all sides perpendicular to the axis of the wire. In this arrangement the electricity of the globe may be considered nearly all as free electricity; and as the insulated

* [Re-printed in Silliman's American Journal of Science, April, 1838, pp. 16–18.]

wire contains its natural quantity, the whole spark is thrown off in the form of a lateral discharge. But to explain these phenomena more fully, Professor Henry remarked that it appeared necessary to add an additional postulate to our theory of the principle of electricity, namely, a kind of momentum, or inertia without weight. By this he would only be understood to express the classification or generalization of a number of facts which would otherwise be insulated. To illustrate this, he stated that the same quantity of electricity could be made to remain on the wire if gradually communicated; but when thrown on in the form of a spark it is dissipated, as before described. Other facts of the same kind were mentioned; and also that we could take advantage of the principle to procure a greater effect in the decomposition of water by ordinary electricity. The fact of a wire becoming luminous by a spark was noticed by the celebrated Van Marum more than fifty years ago; but he ascribed it to the immense power of the great Haarlem machine. The effect however can be produced, as before described, by a cylinder of Nairn's construction of seven inches in diameter, a globe of a foot in diameter being placed in connection with the prime conductor to increase its capacity.

Some experiments were next described in reference to the induction of the lateral action of different discharges on each other. When the long wire is arranged in two parallel but continuous lines by bending the wire the outer side of each wire only becomes luminous. When formed into three parallel lines by a double bend the middle portion of the wire does not become luminous, the outer sides only of the outer lines of wire exhibit the rays. When the wire is formed into a flat spiral the outer spiral alone exhibits the lateral discharge, but the light in this case is very brilliant; the inner spirals appear to increase the effect by induction.



Professor Henry stated that a metallic conductor intimately connected with the earth at one end, does not silently conduct the electricity thrown in sparks on the other end.

In one experiment described a copper wire one-eighth of an inch in diameter was plunged at its lower end into the water of a deep well, so as to form as perfect a connection with the earth as possible. A small ball being attached to the upper end and sparks passed on to this from the globe before mentioned a lateral spark could be drawn from any part of the wire, and a pistol of Volta fired even near the surface of the water. This effect was rendered still more striking by attaching a ball to the middle of the perpendicular part of a lightning rod, put up according to the directions given by Gay-Lussac. When sparks of about an inch and a half in length were thrown on the ball corresponding lateral sparks could be drawn not only from the parts of the rod between the ground and the ball, but from the part above even to the top of the rod. Some remarks were then made on the theory of thunder-storms, as given by the French writers, in which the cloud is considered as analogous in action to one coating of a charged glass, the earth the other coating, and the air between as the non-conducting glass. One very material circumstance has been overlooked in this theory, namely, the great thickness of the intervening stratum and the consequent great quantity of free or redundant electricity in the cloud. This must modify the nature of the discharge from the thunder-cloud, and lead to doubt if it be perfectly analogous to the discharge from an ordinary Leyden jar, since the great quantity of redundant electricity must produce a comparatively greater lateral action; and hence, possibly, the ramifications of the flash and other similar phenomena may be but cases of the lateral discharge.

Some facts were then mentioned on the phenomena of the spark from a long wire charged with common or atmospheric electricity. It is well known that the spark in this case is very pungent, resembling a shock from a Leyden jar. The effect does not appear to be produced, as is generally supposed, by the high intensity of the electricity at the ends of the wire by mere distribution, since this is incompatible with the shortness of the spark. In one experiment fifteen persons, joining hands, received a severe shock, (while stand-

ing on the grass,) from a long wire. One of the number only touched the conductor. The spark in this case was not more than a quarter of an inch long. Several other analogous facts were mentioned, and the suggestion made that the whole were probably the result of an inductive action in the long wire, similar to that observed in a long galvanic current. The subject now required further investigation.

Professor Henry concluded by observing that the facts he had given in this communication were such as must have been noticed by every person who is in the habit of experimenting on ordinary electricity; but he believed these had never been studied in this connection. He was anxious to direct the attention of the Section to the subject as one which appeared to afford an interesting field of research, particularly in connection with the recent discoveries of the surprising inductive actions of galvanic currents.

THE LATERAL DISCHARGE OF ELECTRICITY.

(Proceedings of the American Philosophical Society, vol. I, p. 6.)*

February 16, 1838.

Professor Henry made a verbal communication on the lateral discharge of electricity while passing along a wire, as in the Leyden experiment, or communicated directly to an insulated wire, or to a wire connected with the earth, and detailed various experiments proving that free electricity is not, under any circumstances, conducted silently to the earth.

INDUCTION CURRENTS FROM ORDINARY ELECTRICITY.

(Proceedings of the American Philosophical Society, vol. I, p. 14.)

May 4, 1838.

Dr. Patterson read a letter from Professor Henry, of Princeton, dated May 4, 1838, announcing that in recent experiments he has produced directly from ordinary electricity currents by induction analogous to those obtained from galvanism, and that he has ascertained that these currents possess some peculiar properties; that they may be increased in intensity to an indefinite degree, so that if a discharge from a Leyden jar be sent through a good conductor a shock may be obtained from a contiguous but perfectly insulated conductor more intense than one directly from the jar. Professor Henry remarks that he has also found that all conducting substances screen the inductive action, and that he has succeeded in referring this screening process to currents induced for a moment in the interposed body.

*[The title-page of vol. I (comprising the proceedings from Jan., 1838, to Dec., 1840,) bears date 1840.]

INDUCED CURRENTS FROM ORDINARY ELECTRICITY.

(Proceedings of the American Philosophical Society, vol. 1, pp. 54–56.)

November 2, 1838.

Professor Henry read a paper entitled "Contributions to Electricity and Magnetism, No. 3. On the Phenomena of Electro-dynamic Induction."

The primary object of the investigation undertaken by the author was the discovery of induced currents from ordinary electricity similar to those produced by galvanism. Preparatory to this, a new investigation was instituted of the phenomena of galvanic induction, and the result of this, forms perhaps the most important part of the communication.

The first section of the paper refers to the conditions which influence the induction of a current on itself, as in the case of a long wire and a spiral conductor. These are shown to depend on the intensity and quantity of the battery current, and on the length, thickness, and form of the conductor.

The next section examines the conditions necessary to the production of powerful secondary currents, and also the changes which take place in the same when the form of the battery and the size and form of the conductor are varied.

The important fact is shown that not only a current of intensity can be induced by one of quantity, but also the converse—that a current of quantity can be produced by one of intensity.

The third section relates to the effect of interposing different substances between the conductor which transmits the current from the battery, and that which is arranged to receive the induced current. All good conducting substances are found to screen the inducing action, and this screening effect is shown, by the detail of a variety of experiments, to be the result of the neutralizing action of a current induced in the interposed body. This neutralizing

current is separately examined, and its direction found to be the same as that of the battery current. The question is then raised, how two currents in the same direction can counteract each other? An answer to this question is given in a subsequent part of the paper.

The fourth section relates to the discovery of induced currents of the third, fourth, and fifth orders; that is, to the fact that the second current is found capable of inducing a third current, and this latter again another, and so on. The properties of these new currents are next examined, and the screening influence is found to take place between them; quantity is induced from intensity, and conversely; magnetism is developed in soft iron, decomposition is effected, and intense shocks are obtained, even from the current of the fourth order. A remarkable and important fact is stated in reference to the direction of these currents. If the direction of the battery current and that of the second be called *plus*, then the direction of the third current will be *minus*, of the fourth current *plus*, of the fifth *minus*, and so on. The application of the fact of these alternations is made to the explanation of the phenomenon of screening before mentioned, and also to the improvement of the magneto-electrical machine.

The last part of the paper relates to the discovery of secondary currents, and of currents of the several orders, in the discharge of ordinary electricity. Shocks are obtained from these, the screening influence of good conductors is shown to take place, magnetism is developed, and the alternations in the direction are found to exist as in the currents from galvanic induction. Some remarkable results are given in reference to the great distance at which the induction takes place. Experiments are detailed in which needles were made magnetic when the conductors were removed to the distance of twelve feet from each other.

Prof. Henry made a verbal communication during the course of which he illustrated experimentally the phenomena developed in his paper.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. III.

ON ELECTRO-DYNAMIC INDUCTION.

(Transactions American Philosophical Society, n. s., vol. VI, pp. 303–337.)*

Read November 2, 1838.

INTRODUCTION.

1. Since my investigations in reference to the influence of a spiral conductor, in increasing the intensity of a galvanic current, were submitted to the Society, the valuable paper of Dr. Faraday, on the same subject, has been published, and also various modifications of the principle have been made by Sturgeon, Masson, Page, and others, to increase the effects. The spiral conductor has likewise been applied by Cav. Antinori to produce a spark by the action of a thermo-electrical pile; and Mr. Watkins has succeeded in exhibiting all the phenomena of hydro-electricity by the same means. Although the principle has been much extended by the researches of Dr. Faraday, yet I am happy to state that the results obtained by this distinguished philosopher are not at variance with those given in my paper.

2. I now offer to the Society a new series of investigations in the same line, which I hope may also be considered of sufficient importance to merit a place in the Transactions.

3. The primary object of these investigations was to discover, if possible, inductive actions in common electricity analogous to those found in galvanism. For this purpose a series of experiments was commenced in the spring of 1836, but I was at that time diverted, in part, from the immediate object of my research, by a new investigation of the phenomenon known in common electricity by the name of the lateral discharge. Circumstances prevented my doing anything further, in the way of experiment, until April last, when most of the results which I now offer to the Society were obtained. The investigations are not as complete in several points as I could wish, but as my duties will not per-

* [The title page of vol. VI bears date 1839.]

mit me to resume the subject for some months to come, I therefore present them as they are; knowing, from the interest excited by this branch of science in every part of the world, that the errors which may exist will soon be detected, and the truths be further developed.

4. The experiments are given nearly in the order in which they were made; and in general they are accompanied by the reflections which led to the several steps of the investigation. The whole series is divided, for convenience of arrangement, into six sections, although the subject may be considered as consisting, principally, of two parts. The first relating to a new examination of the induction of galvanic currents; and the second to the discovery of analogous results in the discharge of ordinary electricity.

5. The principal articles of apparatus used in the experiments, consist of a number of flat coils of copper ribbon, which will be designated by the names of coil No. 1, coil No.

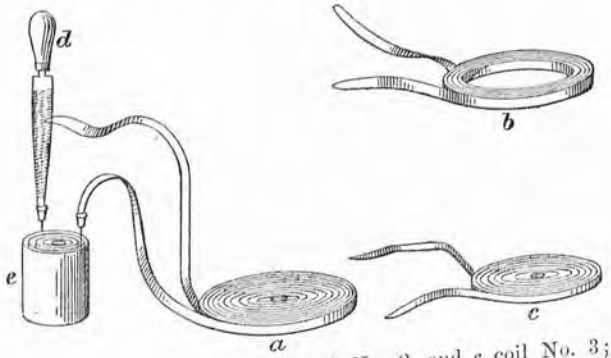


FIG. 1.—*a* represents coil No. 1, *b* coil No. 2, and *c* coil No. 3; *e* the battery, *d* the rasp.

2, &c.; also of several coils of long wire; and these, to distinguish them from the ribbons, will be called helix No. 1, helix No. 2, &c.

6. Coil No. 1 is formed of thirteen pounds of copper plate, one inch and a half wide and ninety-three feet long. It is well covered with two coatings of silk, and was generally used in the form represented in Fig. 1, which is that of a flat spiral sixteen inches in diameter. It was however some-

times formed into a ring of larger diameter, as is shown in Fig. 4, Section III.

7. Coil No. 2 is also formed of copper plate, of the same width and thickness as coil No. 1. It is however only sixty feet long. Its form is shown at *b*, Fig. 1. The opening at the centre is sufficient to admit helix No. 1. Coils No. 3, 4, 5, 6, &c., are all about sixty feet long, and of copper plate of the same thickness, but of half the width of coil No. 1.

8. Helix No 1 consists of sixteen hundred and sixty yards of copper wire, $\frac{1}{49}$th of an inch in diameter. No. 2, of nine

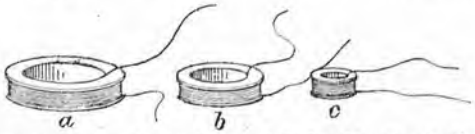


FIG. 2.—*a* represents helix No. 1, *b* helix No. 2, *c* helix No. 5.

hundred and ninety yards; and No. 3, of three hundred and fifty yards, of the same wire. These helices are shown in Fig. 2, and are so adjusted in size as to fit into each other; thus forming one long helix of three thousand yards: or, by using them separately, and in different combinations, seven helices of different lengths. The wire is covered with cotton thread, saturated with beeswax, and between each stratum of spires a coating of silk is interposed.

9. Helix No. 4 is shown at *a*, Fig. 4, Section III; it is formed of five hundred and forty-six yards of wire, $\frac{1}{49}$th of an inch in diameter, the several spires of which are insulated by a coating of cement. Helix No. 5 consists of fifteen hundred yards of silvered copper wire, $\frac{1}{125}$th of an inch in diameter, covered with cotton, and is of the form of No. 4.

10. Besides these I was favored with the loan of a large spool of copper wire, covered with cotton, $\frac{1}{16}$th of an inch in diameter, and five miles long. It is wound on a small axis of iron, and forms a solid cylinder of wire, eighteen inches long, and thirteen in diameter.

11. For determining the direction of induced currents, a magnetizing spiral was generally used, which consists of about thirty spires of copper wire, in the form of a cylinder, and so small as just to admit a sewing needle into the axis.

12. Also a small horseshoe is frequently referred to, which is formed of a piece of soft iron, about three inches long, and $\frac{3}{8}$ths of an inch thick; each leg is surrounded with about five feet of copper bell wire. This length is so small, that only a current of electricity of considerable quantity can develop the magnetism of the iron. The instrument is used for indicating the existence of such a current.

13. The battery used in most of the experiments is shown in Fig. 1. It is formed of three concentric cylinders of copper, and two interposed cylinders of zinc. It is about eight inches high, five inches in diameter, and exposes about one square foot and three quarters of zinc surface, estimating both sides of the metal. In some of the experiments a larger battery was used, weakly charged, but all the results mentioned in the paper except those with a Cruickshank trough, can be obtained with one or two batteries of the above size, particularly if excited by a strong solution. The manner of interrupting the circuit of the conductor by means of a rasp *b*, is shown in the same Figure.

SECTION I.

Conditions which influence the induction of a Current on itself.

14. The phenomenon of the spiral conductor is at present known by the name of the induction of a current on itself, to distinguish it from the induction of the secondary current, discovered by Dr. Faraday. The two however belong to the same class, and experiments render it probable that the spark given by the long conductor is, from the natural electricity of the metal, disturbed for an instant by the induction of the primary current. Before proceeding to the other parts of these investigations, it is important to state the results of a number of preliminary experiments, made to determine more definitely the conditions which influence the action of the spiral conductor.

15. When the electricity is of low intensity, as in the case of the thermo-electrical pile, or a large single battery weakly excited with dilute acid, the flat ribbon coil No. 1, ninety-

three feet long, is found to give the most brilliant deflagrations, and the loudest snaps from a surface of mercury. The shocks, with this arrangement, are however very feeble, and can only be felt in the fingers or through the tongue.

16. The induced current in a short coil, which thus produces deflagration, but not shocks, may, for distinction, be called one of quantity.

17. When the length of the coil is increased, the battery continuing the same, the deflagrating power decreases, while the intensity of the shock continually increases. With five ribbon coils, making an aggregate length of three hundred feet, and the small battery, Fig. 1, the deflagration is less than with coil No. 1, but the shocks are more intense.

18. There is however a limit to this increase of intensity of the shock, and this takes place when the increased resistance or diminished conduction of the lengthened coil begins to counteract the influence of the increasing length of the current. The following experiment illustrates this fact. A coil of copper wire $\frac{1}{16}$th of an inch in diameter, was increased in length by successive additions of about thirty-two feet at a time. After the first two lengths, or sixty-four feet, the brilliancy of the spark began to decline, but the shocks constantly increased in intensity, until a length of five hundred and seventy-five feet was obtained, when the shocks also began to decline. This was then the proper length to produce the maximum effect with a single battery, and a wire of the above diameter.

19. When the intensity of the electricity of the battery is increased, the action of the short ribbon coil decreases. With a Cruickshank's trough of sixty plates, four inches square, scarcely any peculiar effect can be observed, when the coil forms a part of the circuit. If however the length of the coil be increased in proportion to the intensity of the current, then the inductive influence becomes apparent. When the current, from ten plates of the above mentioned trough, was passed through the wire of the large spool (10), the induced shock was too severe to be taken through the body. Again, when a small trough of twenty-five one-inch plates, which

alone would give but a very feeble shock, was used with helix No. 1, an intense shock was received from the induction, when the contact was broken. Also a slight shock in this arrangement is given when the contact is formed, but it is very feeble in comparison with the other. The spark however with the long wire and compound battery is not as brilliant as with the single battery and the short ribbon coil.

20. When the shock is produced from a long wire, as in the last experiments, the size of the plates of the battery may be very much reduced, without a corresponding reduction of the intensity of the shock. This is shown in an experiment with the large spool of wire (10). A very small compound battery was formed of six pieces of copper bell-wire, about one inch and a half long, and an equal number of pieces of zinc of the same size. When the current from this was passed through the five miles of the wire of the spool, the induced shock was given at once to twenty-six persons joining hands. This astonishing effect placed the action of a coil in a striking point of view.

21. With the same spool and the single battery used in the former experiments, no shock, or at most a very feeble one, could be obtained. A current however was found to pass through the whole length, by its action on the galvanometer; but it was not sufficiently powerful to induce a current which could counteract the resistance of so long a wire.

22. The induced current in these experiments may be considered as one of *considerable* "*intensity*," and *small* "*quantity*."

23. The form of the coil has considerable influence on the intensity of the action. In the experiments of Dr. Faraday, a long cylindrical coil of thick copper wire, inclosing a rod of soft iron, was used. This form produces the greatest effect when magnetic reaction is employed; but in the case of simple galvanic induction, I have found the form of the coils and helices represented in the figures most effectual. The several spires are more nearly approximated, and therefore they exert a greater mutual influence. In some cases, as will be seen hereafter, the ring form, shown in Fig. 4, is most effectual.

24. In all cases the several spires of the coil should be well insulated, for although in magnetizing soft iron, and in analogous experiments, the touching of two spires is not attended with any great reduction of action; yet in the case of the induced current, as will be shown in the progress of these investigations, a single contact of two spires is sometimes sufficient to neutralize the whole effect.

25. It must be recollected that all the experiments with these coils and helices, unless otherwise mentioned, are made without the reaction of iron temporarily magnetized; since the introduction of this would in some cases interfere with the action, and render the results more complex.

SECTION II.

Conditions which influence the production of Secondary Currents.

26. The secondary currents, as it is well known, were discovered in the introduction of magnetism and electricity, by Dr. Faraday, in 1831. But he was at that time urged to the exploration of new, and apparently richer veins of science, and left this branch to be traced by others. Since then however attention has been almost exclusively directed to one part of the subject, namely, the induction from magnetism, and the perfection of the magneto-electrical machine: and I know of no attempts, except my own, to review and extend the purely electrical part of Dr. Faraday's admirable discovery.

27. The energetic action of the flat coil, in producing the induction of a current on itself, led me to conclude that it would also be the most proper means for the exhibition and study of the phenomena of the secondary galvanic currents.

28. For this purpose coil No. 1 was arranged to receive the current from the small battery, and coil No. 2 placed on this, with a plate of glass interposed to insure perfect insulation; as often as the circuit of No. 1 was interrupted, a powerful secondary current was induced in No. 2. The arrangement is the same as that exhibited in Fig. 3, with the exception that in this the compound helix is represented as receiving the induction, instead of coil No. 2.

29. When the ends of the second coil were rubbed together, a spark was produced at the opening. When the same ends

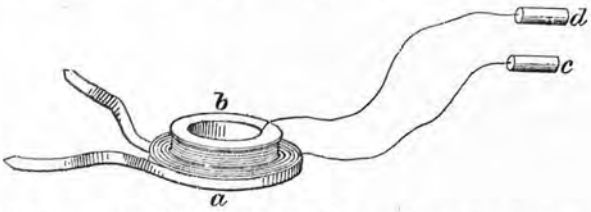


FIG. 3.—*a* represents coil No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

were joined by the magnetizing spiral (11), the enclosed needle became strongly magnetic. Also when the secondary current was passed through the wires of the iron horseshoe (12), magnetism was developed; and when the ends of the second coil were attached to a small decomposing apparatus, of the kind which accompanies the magneto-electrical machine, a stream of gas was given off at each pole. The shock however from this coil is very feeble, and can scarcely be felt above the fingers.

30. This current has therefore the properties of one of moderate "intensity," but considerable "quantity."

31. Coil No. 1 remaining as before, a longer coil, formed by uniting Nos. 3, 4, and 5, was substituted for No. 2. With this arrangement, the spark produced when the ends were rubbed together, was not as brilliant as before; the magnetizing power was much less; decomposition was nearly the same, but the shocks were more powerful, or in other words the "intensity" of the induced current was increased by an increase of the length of the coil, while the "quantity" was apparently decreased.

32. A compound helix, formed by uniting Nos. 1 and 2, and therefore containing two thousand six hundred and fifty yards of wire, was next placed on coil No. 1. The weight of this helix happened to be precisely the same as that of coil No. 2, and hence the different effects of the same quantity of metal in the two forms of a long and short conductor, could be compared. With this arrangement the magnetizing effects,

with the apparatus before mentioned, disappeared. The sparks were much smaller, and also the decomposition less, than with the short coil; but the shock was almost too intense to be received with impunity, except through the fingers of one hand. A circuit of fifty-six of the students of the senior class, received it at once from a single rupture of the battery current, as if from the discharge of a Leyden jar weakly charged. The secondary current in this case was one of small quantity, but of great intensity.

33. The following experiment is important in establishing the fact of a limit to the increase of the intensity of the shock, as well as the power of decomposition, with a wire of a given diameter. Helix No. 5, which consists of wire only $\frac{1}{125}$th of an inch in diameter, was placed on coil No. 2, and its length increased to about seven hundred yards. With this extent of wire, neither decomposition nor magnetism could be obtained, but shocks were given of a peculiarly pungent nature; they did not however produce much muscular action. The wire of the helix was further increased to about fifteen hundred yards; the shock was now found to be scarcely perceptible in the fingers.

34. As a counterpart to the last experiment, coil No. 1 was formed into a ring of sufficient internal diameter to admit the great spool of wire (11), and with the whole length of this (which, as has before been stated, is five miles) the shock was found so intense as to be felt at the shoulder, when passed only through the forefinger and thumb. Sparks and decomposition were also produced, and needles rendered magnetic. The wire of this spool is $\frac{1}{16}$th of an inch thick, and we therefore see from this experiment, that by increasing the diameter of the wire, its length may also be much increased, with an increased effect.

35. The fact (33) that the induced current is diminished by a further increase of the wire, after a certain length has been attained, is important in the construction of the magneto-electrical machine, since the same effect is produced in the induction of magnetism. Dr. Goddard of Philadelphia, to whom I am indebted for coil No. 5, found that when its

whole length was wound on the iron of a temporary magnet, no shocks could be obtained. The wire of the machine may therefore be of such a length, relative to its diameter, as to produce shocks, but no decomposition; and if the length be still further increased, the power of giving shocks may also become neutralized.

36. The inductive action of coil No. 1, in the foregoing experiments, is precisely the same as that of a temporary magnet in the case of the magneto-electrical machine. A short thick wire around the armature gives brilliant deflagrations, but a long one produces shocks. This fact, I believe, was first discovered by my friend Mr. Saxton, and afterwards investigated by Sturgeon and Lentz.

37. We might, at first sight, conclude, from the perfect similarity of these effects, that the currents which, according to the theory of Ampere, exist in the magnet, are like those in the short coil, of great quantity and feeble intensity; but succeeding experiments will show that this is not necessarily the case.

38. All the experiments given in this section have thus far been made with a battery of a single element. This condition was now changed, and a Cruickshank trough of sixty pairs substituted. When the current from this was passed through the ribbon coil No. 1, no indication, or a very feeble one, was given of a secondary current in any of the coils or helices, arranged as in the preceding experiments. The length of the coil, in this case, was not commensurate with the intensity of the current from the battery. But when the long helix, No. 1, was placed instead of coil No. 1, a powerful inductive action was produced on each of the articles, as before.

39. First, helices No. 2 and 3 were united into one, and placed within helix No. 1, which still conducted the battery current. With this disposition a secondary current was produced, which gave intense shocks but feeble decomposition, and no magnetism in the soft iron horseshoe. It was therefore one of intensity, and was induced by a battery current also of intensity.

40. Instead of the helix used in the last experiment for receiving the induction, one of the coils (No. 3) was now placed on helix No. 1, the battery remaining as before. With this arrangement the induced current gave no shocks, but it magnetized the small horseshoe; and when the ends of the coil were rubbed together, produced bright sparks. It had therefore the properties of a current of quantity; and it was produced by the induction of a current, from a battery, of intensity.

41. This experiment was considered of so much importance, that it was varied and repeated many times, but always with the same result; it therefore establishes the fact *that an "intensity" current can induce one of "quantity,"* and, by the preceding experiments, the converse has also been shown, that *a "quantity" current can induce one of "intensity."*

42. This fact appears to have an important bearing on the law of the inductive action, and would seem to favor the supposition that the lower coil, in the two experiments with the long and short secondary conductors, exerted the same amount of inductive force, and that in one case this was expended (to use the language of theory) in giving a great velocity to a small quantity of the fluid, and in the other in producing a slower motion in a larger current; but in the two cases, were it not for the increased resistance to conduction in the longer wire, the quantity multiplied by the square of the velocity would be the same. This however is as yet a hypothesis, but it enables us to conceive how intensity and quantity may both be produced from the same induction.

43. From some of the foregoing experiments we may conclude, that the quantity of electricity in motion in the helix is really less than in the coil, of the same weight of metal; but this may possibly be owing simply to the greater resistance offered by the longer wire. It would also appear, if the above reasoning be correct, that to produce the most energetic physiological effects, only a small quantity of electricity, moving with great velocity, is necessary.

44. In this and the preceding section, I have attempted

to give only the general conditions which influence the galvanic induction. To establish the law would require a great number of more refined experiments, and the consideration of several circumstances which would affect the results, such as the conduction of the wires, the constant state of the battery, the method of breaking the circuit with perfect regularity, and also more perfect means than we now possess of measuring the amount of the inductive action; all these circumstances render the problem very complex.

SECTION III.

On the Induction of Secondary Currents at a distance.

45. In the experiments given in the two preceding Sections, the conductor which received the induction, was separated from that which transmitted the primary current by the thickness only of a pane of glass; but the action from this arrangement was so energetic, that I was naturally led to try the effect at a greater distance.

46. For this purpose coil No. 1 was formed into a ring of about two feet in diameter, and helix No. 4 placed as is shown

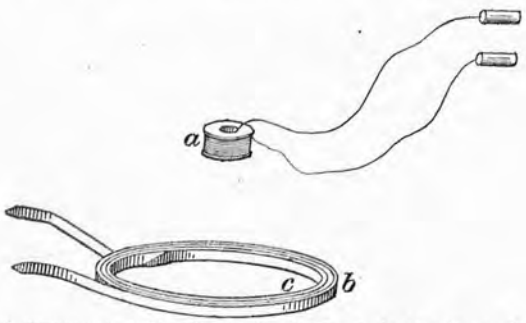


FIG. 4.—*a* represents helix No. 4, *b* coil No. 1, in the form of a ring.

in the figure. When the helix was at the distance of about sixteen inches from the middle of the plane of the ring, shocks could be perceived through the tongue, and these rapidly increased in intensity as the helix was lowered, and when it reached the plane of the ring they were quite severe. The effect however was still greater, when the helix was

moved from the centre to the inner circumference, as at *c*: but when it was placed without the ring, in contact with the outer circumference, at *b*, the shocks were very slight; and when placed within, but its axis at right angles to that of the ring, not the least effect could be observed.

47. With a little reflection, it will be evident that this arrangement is not the most favorable for exhibiting the induction at a distance, since the side of the ring, for example, at *c*, tends to produce a current revolving in one direction in the near side of the helix, and another in an opposite direction in the farther side. The resulting effect is therefore only the difference of the two, and in the position as shown in the figure; this difference must be very small, since the opposite sides of the helix are approximately at the same distance from *c*. But the difference of action on the two sides constantly increases as the helix is brought near the side of the ring, and becomes a maximum when the two are in the position of internal contact. A helix of larger diameter would therefore produce a greater effect.

48. Coil No. 1 remaining as before, helix No. 1, which is nine inches in diameter, was substituted for the small helix of the last experiment, and with this the effect at a distance was much increased. When coil No. 2 was added to coil No. 1, and the currents from two small batteries sent through these, shocks were distinctly perceptible through the tongue, when the distance of the planes of the coils and the three helices, united as one, was increased to thirty-six inches.

49. The action at a distance was still further increased by coiling the long wire of the large spool into the form of a ring of four feet in diameter, and placing parallel to this another ring, formed of the four ribbons of coils No. 1, 2, 3, and 4. When a current from a single battery of thirty-five feet of zinc surface was passed through the ribbon conductor, shocks through the tongue were felt when the rings were separated to the distance of four feet. As the conductors were approximated, the shocks became more and more severe; and when at the distance of twelve inches, they could not be taken through the body.

50. It may be stated in this connection, that the galvanic induction of magnetism in soft iron, in reference to distance, is also surprisingly great. A cylinder of soft iron, two inches in diameter and one foot long, placed in the centre of the ring of copper ribbon, with the battery above mentioned, becomes strongly magnetic.

51. I may perhaps be excused for mentioning in this communication that the induction at a distance affords the means of exhibiting some of the most astonishing experiments, in the line of *physique amusante*, to be found perhaps in the whole course of science. I will mention one which is somewhat connected with the experiments to be described in the next section, and which exhibits the action in a striking manner. This consists in causing the induction to take place through the partition wall of two rooms. For this purpose coil No. 1 is suspended against the wall in one room, while a person in the adjoining one receives the shock, by grasping the handles of the helix, and approaching it to the spot opposite to which the coil is suspended. The effect is as if by magic, without a visible cause. It is best produced through a door, or thin wooden partition.

52. The action at a distance affords a simple method of graduating the intensity of the shock in the case of its application to medical purposes. The helix may be suspended by a string passing over a pulley, and then gradually lowered down towards the plane of the coil, until the shocks are of the required intensity. At the request of a medical friend, I have lately administered the induced current precisely in this way, in a case of paralysis of a part of the nerves of the face.

53. I may also mention that the energetic action of the spiral conductors enables us to imitate, in a very striking manner the inductive operation of the magneto-electrical machine, by means of an uninterrupted galvanic current. For this purpose it is only necessary to arrange two coils to represent the two poles of a horseshoe magnet, and to cause two helices to revolve past them in a parallel plane. While a constant current is passing through each coil, in opposite

directions, the effect of the rotation of the helices is precisely the same as that of the revolving armature in the machine.

54. A remarkable fact should here be noted in reference to helix No. 4, which is connected with a subsequent part of the investigation. This helix is formed of copper wire, the spires of which are insulated by a coating of cement instead of thread, as in the case of the others. After being used in the above experiments, a small discharge from a Leyden jar was passed through it, and on applying it again to the coil, I was much surprised to find that scarcely any signs of a secondary current could be obtained.

55. The discharge had destroyed the insulation in some part, but this was not sufficient to prevent the magnetizing of a bar of iron introduced into the opening at the centre. The effect appeared to be confined to the inductive action. The same accident had before happened to another coil of nearly the same kind. It was therefore noted as one of some importance. An explanation was afterwards found in a peculiar action of the secondary current.

SECTION IV.

On the Effects produced by interposing different Substances between the Conductors.

56. Sir H. Davy found, in magnetizing needles by an electrical discharge, that the effect took place through interposed plates of all substances, conductors and non-conductors.* The experiment which I have given in paragraph 51 would appear to indicate that the inductive action which produces the secondary current might also follow the same law.

57. To test this the compound helix was placed about five inches above coil No. 1, Fig. 5, and a plate of sheet iron, about $\frac{1}{10}$th of an inch thick, interposed. With this arrangement no shocks could be obtained; although, when the plate was withdrawn, they were very intense.

58. It was at first thought that this effect might be pe-

* Philosophical Transactions, 1821.

culiar to the iron, on account of its temporary magnetism; but this idea was shown to be erroneous by substituting a

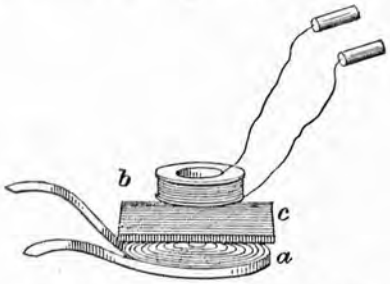


FIG 5.—*a* represents coil No. 1, *b* helix No. 1, and *c* an interposed plate of metal.

plate of zinc of about the same size and thickness. With this the screening influence was exhibited as before.

59. After this a variety of substances was interposed in succession, namely, copper, lead, mercury, acid, water, wood, glass, &c.; and it was found that all the perfect conductors, such as the metals, produced the screening influence; but nonconductors, as glass, wood, &c., appeared to have no effect whatever.

60. When the helix was separated from the coil by a distance only equal to the thickness of the plate, a slight sensation could be perceived even when the zinc of $\frac{1}{10}$ of an inch in thickness was interposed. This effect was increased by increasing the quantity of the battery current. If the thickness of the plate was diminished, the induction through it became more intense. Thus a sheet of tinfoil interposed produced no perceptible influence; also four sheets of the same were attended with the same result. A certain thickness of metal is therefore required to produce the screening effect, and this thickness depends on the quantity of the current from the battery.

61. The idea occurred to me that the screening might, in some way, be connected with an instantaneous current in the plate, similar to that in the induction by magnetic rotation, discovered by M. Arago. The ingenious variation of this principle by Messrs. Babbage and Herschel, furnished me with a simple method of determining this point.

62. A circular plate of lead was interposed, which caused the induction in the helix almost entirely to disappear. A slip of the metal was then cut out in the direction of a radius of the circle, as is shown in Fig. 6. With the plate in this condition, no screening was produced; the shocks were as intense as if the metal were not present.

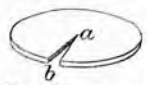


FIG. 6.—*a* represents a lead plate, of which the sector *b* is cut out.

63. This experiment however is not entirely satisfactory, since the action might have taken place through the opening of the lead; to obviate this objection, another plate was cut in the same manner, and the two interposed with a glass plate between them, and so arranged that the opening in the one might be covered by the continuous part of the other. Still shocks were obtained with undiminished intensity.

64. But the existence of a current in the interposed conductor was rendered certain by attaching the magnetizing spiral by means of two wires to the edge of the opening in the circular plate, as is shown in Fig. 7. By this arrangement the latent current was drawn out, and its direction obtained by the polarity of a needle placed in the spiral at *b*.



FIG. 7.—*a* represents a lead plate, *b* the magnetizing spiral.

65. This current was a secondary one, and its direction, in conformity with the discovery of Dr. Faraday, was found to be the same as that of the primary current.

66. That the screening influence is in some way produced by the neutralizing action of the current thus obtained, will be clear, from the following experiment. The plate of zinc before mentioned, which is nearly twice the diameter of the helix, instead of being placed between the conductors, was put on the top of the helix, and in this position, although the neutralization was not as perfect as before, yet a great reduction was observed in the intensity of the shock.

67. But here a very interesting and puzzling question occurs. How does it happen that two currents, both in the same direction, can neutralize each other? I was at first

disposed to consider the phenomenon as a case of real electrical interference, in which the impulses succeed each other by some regular interval. But if this were true the effect should depend on the length and other conditions of the current in the interposed conductor. In order to investigate this, several modifications of the experiments were instituted.

68. First a flat coil (No. 3) was interposed instead of the plates. When the two ends of this were separated, the shocks were received as if the coil were not present; but when the ends were joined, so as to form a perfect metallic circuit, no shocks could be obtained. The neutralization with the coil in this experiment was even more perfect than with the plate.

69. Again, coil No. 2, in the form of a ring, was placed not between the conductors, but around the helix. With this disposition of the apparatus, and the ends of the coil joined, the shocks were scarcely perceptible, but when the ends were separated, the presence of the coil has no effect.

70. Also when helix No. 1 and 2 were together submitted to the influence of coil No. 1, the ends of the one being joined, the other gave no shock.

71. The experiments were further varied by placing helix No. 2 within a hollow cylinder of sheet brass, and this again within coil No. 2 in a manner similar to that shown in Fig. 12, which is intended to illustrate another experiment. In this arrangement the neutralizing action was exhibited, as in the case of the plate.

72. A hollow cylinder of iron was next substituted for the one of brass, and with this also no shocks could be obtained.

73. From these experiments it is evident that the neutralization takes place with currents in the interposed or adjoining conductors of all lengths and intensities, and therefore cannot, as it appears to me, be referred to the interference of two systems of vibrations.

74. This part of the investigation was, for a time, given up almost in despair, and it was not until new light had been obtained from another part of the inquiry, that any further advances could be made towards a solution of the mystery.

75. Before proceeding to the next Section, I may here state that the phenomenon mentioned, paragraph 54, in reference to helix No. 4, is connected with the neutralizing action. The electrical discharge having destroyed the insulation at some point, a part of the spires would thus form a shut circuit, and the induction in this would counteract the action in the other part of the helix; or in other words, the helix was in the same condition as the two helices mentioned in paragraph 70, when the ends of the wire of one were joined.

76. Also the same principle appears to have an important bearing on the improvement of the magneto-electrical machine: since the plates of metal which sometimes form the ends of the spool containing the wire, must necessarily diminish the action, and also from experiment of paragraph 72 the armature itself may circulate a closed current which will interfere with the intensity of the induction in the surrounding wire. I am inclined to believe that the increased effect observed by Sturgeon and Calland, when a bundle of wire is substituted for a solid piece of iron, is at least in part due to the interruption of these currents. I hope to resume this part of the subject, in connection with several other points, in another communication to the Society.

77. The results given in this Section may, at first sight, be thought at variance with the statements of Sir H. Davy, that needles could be magnetized by an electrical discharge with conductors interposed. But from his method of performing the experiment, it is evident that the plate of metal was placed between a straight conductor and the needle. The arrangement was therefore similar to the interrupted circuit in the experiment with the cut plate (62), which produces no screening effect. Had the plate been curved into the form of a hollow cylinder, with the two ends in contact, and the needle placed within this, the effect would have been otherwise.

SECTION V.

On the Production and Properties of induced Currents of the Third, Fourth, and Fifth order.

78. The fact of the perfect neutralization of the primary current by a secondary, in the interposed conductor, led me to conclude that if the latter could be drawn out, or separated from the influence of the former, it would itself be capable of producing a new induced current in a third conductor.

79. The arrangement exhibited in Fig. 8 furnishes a ready

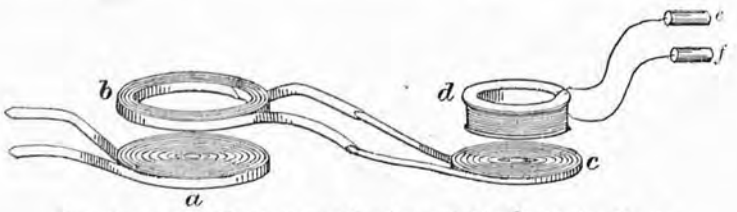


FIG. 8.—*a* coil No. 1, *b* coil No. 2, *c* coil No. 3, *d* helix No. 1.

means of testing this. The primary current, as usual, is passed through coil No. 1, while coil No. 2 is placed over this to receive the induction, with its ends joined to those of coil No. 3. By this disposition the secondary current passes through No. 3; and since this is at a distance, and without the influence of the primary, its separate induction will be rendered manifest by the effects on helix No. 1. When the handles *e*, *f*, are grasped a powerful shock is received, proving the induction of a tertiary current.

80. By a similar but more extended arrangement, as shown in Fig. 9, shocks were received from currents of a fourth and fifth order; and with a more powerful primary current, and additional coils, a still greater number of successive inductions might be obtained.

81. The induction of currents of different orders, of sufficient intensity to give shocks, could scarcely have been anticipated from our previous knowledge of the subject. The secondary current consists as it were of a single wave

of the natural electricity of the wire, disturbed but for an instant by the induction of the primary; yet this has the power of inducing another current, but little inferior in energy to itself, and thus produces effects apparently much greater in proportion to the quantity of electricity in motion than the primary current.

82. Some difference may be conceived to exist in the action of the induced currents, and that from the battery, since they are apparently different in nature; the one consisting, as we may suppose, of a single impulse, and the other of a succession of such impulses, or a continuous action. It was therefore important to investigate the properties of these currents, and to compare the results with those before obtained.

83. First, in reference to the intensity, it was found that with the small battery a shock could be given from the current of the third order to twenty-five persons joining hands; also shocks perceptible in the arms were obtained from a current of the fifth order.

84. The action at a distance was also much greater than could have been anticipated. In one experiment shocks from the tertiary current were distinctly felt through the tongue, when helix No. 1 was at the distance of eighteen inches above the coil transmitting the secondary current.

85. The same screening effects were produced by the interposition of plates of metal between the conductors of the different orders, as those which have been described in reference to the primary and secondary currents.

86. Also when the long helix is placed over a secondary current generated in a short coil, and which is therefore, as we have before shown, one of quantity, a tertiary current of intensity is produced.

87. Again, when the intensity current of the last experiment is passed through a second helix, and another coil is placed over this, a quantity current is again produced. Therefore in the case of these currents, as in that of the primary, a *quantity current can be induced from one of intensity, and the converse.* By the arrangement of the apparatus as shown in Fig. 9, these different results are exhibited at once.

The induction from coil No. 3 to helix No. 1 produces an intensity current, and from helix No. 2 to coil No. 4 a quantity current.

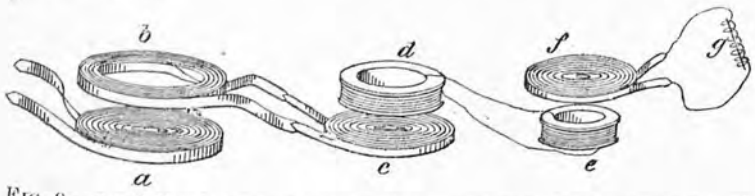


FIG. 9.—*a* coil No. 1, *b* coil No. 2, *c* coil No. 3, *d* helix No. 1, *e* helix No. 2 and 3, *f* coil No. 4, and *g* magnetizing spiral.

88. If the ends of coil No. 2, as in the arrangement of Fig. 8, be united to helix No. 1 instead of coil No. 3, no shocks can be obtained; the quantity current of coil No. 2 appears not to be of sufficient intensity to pass through the wire of the long helix.

89. Also, no shocks can be obtained from the handles attached to helix No. 2, in the arrangement exhibited in Fig.

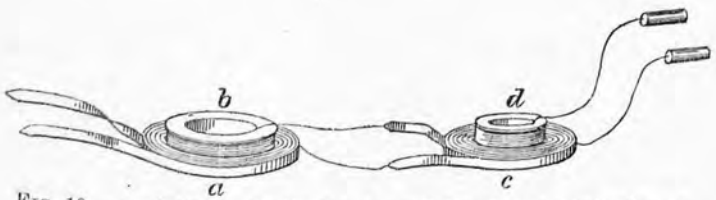


FIG. 10.—*a* coil No. 1, *b* helix No. 1, *c* coil No. 3, and *d* helix No. 3.

10. In this case the quantity of electricity in the current from the helix appears to be too small to produce any effect, unless its power is multiplied by passing it through a conductor of many spires.

90. The next inquiry was in reference to the direction of these currents, and this appeared important in connection with the nature of the action. The experiments of Dr. Faraday would render it probable, that at the beginning and ending of the secondary current, its induction on an adjacent wire is in contrary directions, as is shown to be the case in the primary current. But the whole action of a secondary current is so instantaneous, that the inductive effects at the

beginning and ending cannot be distinguished from each other, and we can only observe a single impulse, which however may be considered as the difference of two impulses in opposite directions.

91. The first experiment happened to be made with a current of the fourth order. The magnetizing spiral (11) was attached to the ends of coil No. 4, Fig. 9, and by the polarity of the needle it was found that this current was in the same direction with the secondary and primary currents.* By a too hasty generalization, I was led to conclude, from this experiment, that the currents of all orders are in the same direction as that of the battery current, and I was the more confirmed in this from the results of my first experiments on the currents of ordinary electricity. The conclusion however caused me much useless labor and perplexity, and was afterwards proved to be erroneous.

92. By a careful repetition of the last experiment, in reference to each current, the important fact was discovered, that *there exists an alternation in the direction of the currents of the several orders, commencing with the secondary.* This result was so extraordinary, that it was thought necessary to establish it by a variety of experiments. For this purpose the direction was determined by decomposition, and also by the galvanometer, but the result was still the same; and at this stage of the inquiry I was compelled to the conclusion that the directions of the several currents were as follows:

Primary current	+
Secondary current	+
Current of the third order	—
Current of the fourth order	+
Current of the fifth order	—

93. In the first glance at the above table, we are struck with the fact that the law of alternation is complete, except between the primary and secondary currents, and it appeared

*It should be recollected that all the inductions which have been mentioned were produced at the moment of breaking the circuit of the battery current. The induction at the formation of the current is too feeble to produce the effects described.

that this exception might possibly be connected with the induced current which takes place in the first coil itself, and which gives rise to the phenomena of the spiral conductor. If this should be found to be *minus*, we might consider it as existing between the primary and secondary, and the anomaly would thus disappear. Arrangements were therefore made to fully satisfy myself on this point. For this purpose the decomposition of dilute acid and the use of the galvanometer were resorted to, by placing the apparatus between the ends of a cross wire attached to the extremities of the coil, as in the arrangement described by Dr. Faraday (ninth series;) but all the results persisted in giving a direction to this current the same as stated by Dr. Faraday, namely, that of the primary current. I was therefore obliged to abandon the supposition that the anomaly in the change of the current is connected with the induction of the battery current on itself.

94. Whatever may be the nature or causes of these changes in the direction, they offer a ready explanation of the neutralizing action of the plate interposed between two conductors, since a secondary current is induced in the plate; and although the action of this, as has been shown, is in the same direction as the current from the battery, yet it tends to induce a current in the adjacent conducting matter of a contrary direction. The same explanation is also applicable to all the other cases of neutralization, even to those which take place between the conductors of the several orders of currents.

95. The same principle explains some effects noted in reference to the induction of a current on itself. If a flat coil be connected with the battery, of course sparks will be produced by the induction, at each rupture of the circuit. But if in this condition another flat coil, with its ends joined, be placed on the first coil, the intensity of the shock is much diminished, and when the several spires of the two coils are mutually interposed by winding the two ribbons together into one coil, the sparks entirely disappear in the coil transmitting the battery current, when the ends of the other are joined. To understand this, it is only necessary to mention

that the induced current in the first coil is a true secondary current, and it is therefore neutralized by the action of the secondary in the adjoining conductor; since this tends to produce a current in the opposite direction.

96. It would also appear from the perfect neutralization which ensues in the arrangement just before described, that the induced current in the adjoining conductor is more powerful than that of the first conductor; and we can easily see how this may be. The two ends of the second coil are joined, and it thus forms a perfect metallic circuit; while the circuit of the other coil may be considered as partially interrupted, since to render the spark visible the electricity must be projected as it were through a small distance of air.

97. We would also infer that two contiguous secondary currents, produced by the same induction, would partially counteract each other. Moving in the same direction, they would each tend to induce a current in the other of an opposite direction. This is illustrated by the following experiment: helix No. 1 and 2 were placed together, but not united, above coil No. 1, so that they each might receive the induction; the larger was then gradually removed to a greater distance from the coil, until the intensity of the shock from each was about the same. When the ends of the two were united, so that the shock would pass through the body from the two together, the effect was apparently less than with one helix alone. The result however was not as satisfactory as in the case of the other experiments; a slight difference in the intensity of two shocks could not be appreciated with perfect certainty.

SECTION VI.

The production of induced Currents of the different Orders from ordinary Electricity.

98. Dr. Faraday, in the ninth series of his researches, remarks that "the effect produced at the commencement and the end of a current (which are separated by an interval of time when that current is supplied from a voltaic apparatus) must occur at the same moment when a common electrical

discharge is passed through a long wire. Whether if it happen accurately at the same moment they would entirely neutralize each other, or whether they would not still give some definite peculiarity to the discharge, is a matter remaining to be examined."

99. The discovery of the fact that the secondary current, which exists but for a moment, could induce another current of considerable energy, gave some indication that similar effects might be produced by a discharge of ordinary electricity, provided a sufficiently perfect insulation could be obtained.

100. To test this a hollow glass cylinder, Fig. 11, of about

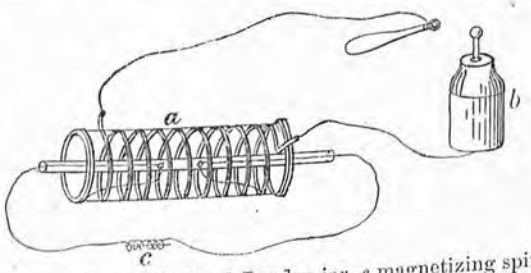


FIG. 11.—*a* glass cylinder, *b* Leyden jar, *c* magnetizing spiral.

six inches in diameter, was prepared with a narrow ribbon of tinfoil, about thirty feet long, pasted spirally around the outside, and a similar ribbon of the same length, pasted on the inside; so that the corresponding spires of the two were directly opposite each other. The ends of the inner spiral passed out of the cylinder through a glass tube, to prevent all direct communication between the two. When the ends of the inner ribbon were joined by the magnetizing spiral (11), containing a needle, and a discharge from a half gallon jar sent through the outer ribbon, the needle was strongly magnetized in such a manner as to indicate *an induced current through the inner ribbon in the same direction as that of the current of the jar.* This experiment was repeated many times, and always with the same result.

101. When the ends of one of the ribbons were placed very nearly in contact, a small spark was perceived at the

opening, the moment the discharge took place through the other ribbon.

102. When the ends of the same ribbon were separated to a considerable distance, a larger spark than the last could be drawn from each end by presenting a ball, or the knuckle.

103. Also if the ends of the outer ribbon were united, so as to form a perfect metallic circuit, a spark could be drawn from any point of the same, when a discharge was sent through the inner ribbon.

104. The sparks in the two last experiments are evidently due to the action known in ordinary electricity by the name of the lateral discharge. To render this clear, it is perhaps necessary to recall the well known fact, that when the knob of a jar is electrified positively, and the outer coating is connected with the earth, then the jar contains a small excess of positive electricity beyond what is necessary to perfectly neutralize the negative surface. If the knob be put in communication with the earth, the extra quantity, or the free electricity, as it is sometimes called, will be on the negative side. When the discharge took place in the above experiments, the inner ribbon became for an instant charged with this free electricity, and consequently threw off from the outer ribbon, by ordinary induction, the sparks described. It therefore became a question of importance to determine whether the induced current described in paragraph 100 was not also a result of the lateral discharge, instead of being a true case of a secondary current analogous to those produced from galvanism. For this purpose the jar was charged, first with the outer coating in connection with the earth, and again with the knob in connection with the same, so that the extra quantity might be in the one case *plus* and in the other *minus;* but the direction of the induced current was not affected by these changes; it was always the same, namely, from the positive to the negative side of the jar.

105. When however the quantity of free electricity was increased, by connecting the knob of the jar with a globe about a foot in diameter, the intensity of magnetism ap-

peared to be somewhat diminished, if the extra quantity was on the negative side; and this might be expected, since the free electricity, in its escape to the earth through the ribbon, in this case would tend to induce a feeble current in the opposite direction to that of the jar.

106. The spark from an insulated conductor may be considered as consisting almost entirely of this free or extra electricity, and it was found that this was also capable of producing an induced current, precisely the same as that from the jar. In the experiment which gave this result, one end of the outer ribbon of the cylinder (100) was connected with the earth, and the other caused to receive a spark from a conductor fourteen feet long, and nearly a foot in diameter. The direction of the induced current was the same as that of the spark from the conductor.

107. From these experiments it appears evident that the discharge from the Leyden jar possesses the property of inducing a secondary current precisely the same as the galvanic apparatus, and also that this induction is only so far connected with the phenomenon of the lateral discharge as this latter partakes of the nature of an ordinary electrical current.

108. Experiments were next made in reference to the production of currents of the different orders by ordinary electricity. For this purpose a second cylinder was prepared with ribbons of tinfoil, in a similar manner to the one before described. The two were then so connected that the secondary current from the first would circulate around the second. When a discharge was passed through the outer ribbon of the first cylinder, a tertiary current was induced in the inner ribbon of the second. This was rendered manifest by the magnetizing of a needle in a spiral joining the ends of the last mentioned ribbon.

109. Also by the addition, in the same way, of a third cylinder, a current of the fourth order was developed. The same result was likewise obtained by using the arrangement of the coils and helices shown in Fig. 9. For these experiments however the coils were furnished with a double coat-

ing of silk, and the contiguous conductors separated by a large plate of glass.

110. Screening effects precisely the same as those exhibited in the action of galvanism were produced by interposing a plate of metal between the conductors of different orders, Figures 8 and 9. The precaution was taken to place the plate between two frames of glass, in order to be assured that the effect was not due to a want of perfect insulation.

111. Also analogous results were found when the experiments were made with coils interposed instead of plates, as described in paragraph 68. When the ends of the interposed coils were separated, no screening was observed, but when joined, the effect was produced. The existence of the induced current, in all these experiments, was determined by the magnetism of a needle in a spiral attached to one of the coils.

112. Likewise shocks were obtained from the secondary current by an arrangement shown in Fig. 12. Helices No.

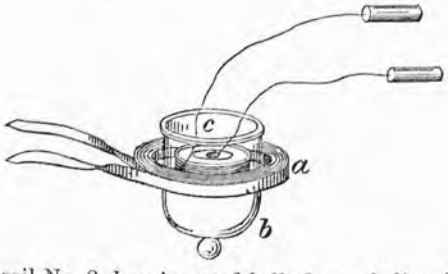


FIG. 12.—*a* coil No. 2, *b* an inverted bell glass, *c* helices No. 2 and 3.

2 and No. 3 united are put within a glass jar, and coil No. 2 is placed around the same. When the handles are grasped, a shock is felt at the moment of the discharge, through the outer coil. The shocks however were very different in intensity with different discharges from the jar. In some cases no shock was received, when again, with a less charge, a severe one was obtained. But these irregularities find an explanation in a subsequent part of the investigation.

113. In all these experiments, the results with ordinary and galvanic electricity are similar. But at this stage of the investigation there appeared what at first was considered a

remarkable difference in the action of the two. I allude to the direction of the currents of the different orders. These, in the experiments with the glass cylinders, instead of exhibiting the alternations of the galvanic currents (92), were all in the same direction as the discharge from the jar, or in other words, they were all *plus*.

114. To discover, if possible, the cause of this difference, a series of experiments was instituted; but the first fact developed, instead of affording any new light, seemed to render the obscurity more profound. When the directions of the currents were taken in the arrangement of the coils (Fig. 9) the discrepancy vanished. *Alternations were found the same as in the case of galvanism.* This result was so extraordinary that the experiments were many times repeated, first with the glass cylinders, and then with the coils; the results however were always the same. The cylinders gave currents all in one direction; the coils in alternate directions.

115. After various hypotheses had been formed, and in succession disproved by experiment, the idea occurred to me that the direction of the currents might depend on the distance of the conductors, and this appeared to be the only difference existing in the arrangement of the experiments with the coils and the cylinders.* In the former the distance between the ribbons was nearly one inch and a half, while in the latter it was only the thickness of the glass, or about $\frac{1}{20}$th of an inch.

116. In order to test this idea, two narrow slips of tinfoil, about twelve feet long, were stretched parallel to each other, and separated by thin plates of mica to the distance of about $\frac{1}{50}$th of an inch. When a discharge from the half gallon jar was passed through one of these, an induced current in the same direction was obtained from the other. The ribbons were then separated, by plates of glass, to the distance of $\frac{1}{20}$th of an inch; the current was still in the same direction, or *plus*. When the distance was increased to about $\frac{1}{8}$th of an inch, no induced current could be obtained; and when they

* This idea was not immediately adopted, because I had previously experimented on the direction of the secondary current from galvanism, and found no change in reference to distance.

were still further separated the current again appeared, but was now *found to have a different direction, or to be minus.* No other change was observed in the direction of the current; the intensity of the induction decreased as the ribbons were separated. The existence and direction of the current, in this experiment, were determined by the polarity of the needle in the spiral attached to the ends of one of the ribbons.

117. The question at this time arose, whether the direction of the current, as indicated by the polarity of the needle, was the true one, since the magnetizing spiral might itself, in some cases, induce an opposite current. To satisfy myself on this point a series of charges, of various intensity and quantity, from a single spark of the large conductor to the full charge of nine jars, were passed through the small spiral, which had been used in all the experiments, but they all gave the same polarity. The interior of this spiral is so small, that the needle is throughout in contact with the wire.

118. The fact of a change in the direction of the induced current by a change in the distance of the conductors, being thus established, a great number and variety of experiments were made to determine the other conditions on which the change depends. These were sought for in a variation of the intensity and quantity of the primary discharge, in the length and thickness of the wire, and in the form of the circuit. The results were however in many cases anomalous, and are not sufficiently definite to be placed in detail before the Society. I hope to resume the investigation at another time, and will therefore at present briefly state only those general facts which appear well established.

119. With a single half gallon jar, and the conductors separated to a distance less than $\frac{1}{20}$th of an inch, the induced current is always in the same direction as the primary. But when the conductors are gradually separated, there is always found a distance at which the current begins to change its direction. This distance depends certainly on the amount of the discharge, and probably on the intensity, and also on the length and thickness of the conductors. With a battery of eight half gallon jars, and parallel wires of about ten feet long, the change in the direction did not

take place at a less distance than from twelve to fifteen inches, and with a still larger battery and longer conductors, no change was found, although the induction was produced at the distance of several feet.

120. The facts given in the last paragraph relate to the inductive action of the primary current; but it appears from the results detailed in paragraphs 110 and 114, that the currents of all the other orders also change the direction of the inductive influence with a change of the distance. In these cases however the change always takes place at a very small distance from the conducting wire; and in this respect the result is similar to the effect of a *primary current* from the discharge of a small jar.

121. The most important experiments, in reference to distance, were made in the lecture room of my respected friend Dr. Hare of Philadelphia, with the splendid electrical apparatus described in the Fifth Volume (new series) of the Transactions of this Society. The battery consists of thirty-two jars, each of the capacity of a gallon. A thick copper wire of about $\frac{1}{10}$th of an inch in diameter and eighty feet in length, was stretched across the lecture room, and its ends brought to the battery, so as to form a trapezium, the longer side of which was about thirty-five feet. Along this side a wire was stretched of the ordinary bell size, and the extreme ends of this joined by a spiral, similar to the arrangement shown in Fig. 13. The two wires were at first placed within the distance of about an inch, and afterwards constantly separated after each discharge of the whole battery through the thick wire. When a break was made in the second wire at *a*, no magnetism was developed in a needle in the spiral at *b*, but when the circuit was complete, the needle at each discharge indicated a current in the same direction as that of the battery. When the distance of the two

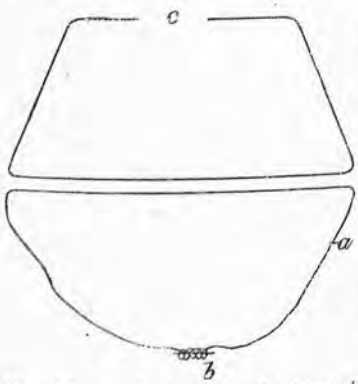


FIG. 13.—*c* place of the battery, *b* spiral.

wires was increased to sixteen inches, and the ends of the second wire placed in two glasses of mercury, and a finger of each hand plunged into the metal, a shock was received. The direction of the current was still the same, but the magnetism not as strong as at a less distance.

122. The second wire was next arranged around the other, so as to enclose it. The magnetism by this arrangement appeared stronger than with the last; the direction of the current was still the same, and continued thus, until the two wires were at every point separated to the distance of twelve feet, except in one place where they were obliged to be crossed at the distance of seven feet, but here the wires were made to form a right angle with each other, and the effect of the approximation was therefore (46) considered as nothing. The needle at this surprising distance was tolerably strongly magnetized, as was shown by the quantity of filings which would adhere to it. The direction of the current was still the same as that of the battery. The form of the room did not permit the two wires to be separated to a greater distance. The whole length of the circuit of the interior large wire was about eighty feet; that of the exterior one hundred and twenty. The two were not in the same plane, and a part of the outer passed through a small adjoining room.

123. The results exhibited in this experiment are such as could scarcely have been anticipated by our previous knowledge of the electrical discharge. They evince a remarkable inductive energy, which has not before been distinctly recognized, but which must perform an important part in the discharge of electricity from the clouds. Some effects which have been observed during thunder storms, appear to be due to an action of this kind.

124. Since a discharge of ordinary electricity produces a secondary current in an adjoining wire, it should also produce an analogous effect in its own wire; and to this cause may be now referred the peculiar action of a long conductor. It is well known that the spark from a very long wire, although quite short, is remarkably pungent. I was so fortunate as to witness a very interesting exhibition of

this action during some experiments on atmospheric electricity made by a committee of the Franklin Institute, in 1836. Two kites were attached, one above the other, and raised with a small iron wire in place of a string. On the occasion at which I was present, the wire was extended by the kites to the length of about one mile. The day was perfectly clear, yet the sparks from the wire had so much projectile force (to use a convenient expression of Dr. Hare) that fifteen persons joining hands and standing on the ground, received the shock at once, when the first person of the series touched the wire. A Leyden jar being grasped in the hand by the outer coating, and the knob presented to the wire, a severe shock was received, as if by a perforation of the glass, but which was found to be the result of the sudden and intense induction.

125. These effects were evidently not due to the accumulated intensity at the extremities of the wire, on the principles of ordinary electrical distribution, since the knuckle required to be brought within about a quarter of an inch before the spark could be received. It was not alone the quantity, since the experiments of Wilson prove that the same effect is not produced with an equal amount of electricity on the surface of a large conductor. It appears evidently therefore a case of the induction of an electrical current on itself. The wire is charged with a considerable quantity of feeble electricity, which passes off in the form of a current along its whole length, and thus the induction takes place at the end of the discharge, as in the case of a long wire transmitting a current of galvanism.

126. It is well known that the discharge from an electrical battery possesses great divellent powers; that it entirely separates, in many instances, the particles of the body through which it passes. This force acts, in part, at least, in the direction of the line of the discharge, and appears to be analogous to the repulsive action discovered by Ampere, in the consecutive parts of the same galvanic current. To illustrate this, paste on a piece of glass a narrow slip of tinfoil, cut it through at several points, and loosen the ends from the glass

at the places so cut. Pass a discharge through the tinfoil from about nine half gallon jars; the ends, at each separation, will be thrown up, and sometimes bent entirely back, as if by the action of a strong repulsive force between them. This will be understood by a reference to Fig. 14; the ends are shown bent back at *a*, *a*, *a*, *a*. In the popular experiment of the pierced card, the bur on each side appears to be due to an action of the same kind.

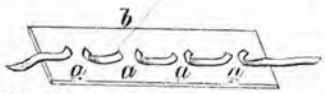


FIG 14.—*b* glass plate; *a*, *a*, *a*, *a*, openings in tinfoil.

127. It now appears probable, from the facts given in paragraphs 119 and 120, that the table in paragraph 92 is only an approximation to the truth, and that each current from galvanism, as well as from electricity, first produces an inductive action in the direction of itself, and that the inverse influence takes place at a little distance from the wire.

128. To test this the compound helix was placed on coil No. 1, to receive the induction, and its ends joined to those of the outer ribbon of tinfoil of the glass cylinder, while the magnetizing spiral was attached to the ends of the inner riband. A feeble tertiary current was produced by this arrangement, which in two cases gave a polarity to the needle indicating a direction the same as that of the primary current. In other cases the magnetism was either imperceptible or *minus*. With an arrangement of two coils of wires around two glass cylinders, one within the other, the same effect was produced. The magnetism was less when the distance of the two sets of spires was smaller, indicating, as it would appear, an approximation to a position of neutrality. These results are rather of a negative kind, yet they appear to indicate the same change with distance in the case of the galvanic currents as in that of the discharge of ordinary electricity. The distance however at which the change takes place would seem to be less in the former than in the latter.

129. There is a perfect analogy between the inductive action of the primary current from the galvanic apparatus and of that from the larger electrical battery. The point of change, in each, appears to be at a great distance.

130. The neutralizing effect described in Section IV may now be more definitely explained by saying that when a third conductor is acted on at the same time by a primary and secondary current (unless it be very near the second wire) it will fall into the region of the *plus* influence of the former, and into that of the *minus* influence of the latter; and hence no induction will be produced.

131. This will be rendered perfectly clear by Fig. 15, in which *a* represents the conductor of the primary current, *b* that of the secondary, and *c* the third conductor. The characters + + +, &c., beginning at the middle of the first conductor and extending downwards, represent the constant *plus* influence of the primary current, and those + 0 — —, &c., beginning at the second conductor, indicate its inductive influence as changing with the distance. The third conductor, as is shown by the figure, falls in the *plus* region of the primary current, and in the *minus* region of the secondary, and hence the two actions neutralize each other, and no apparent result is produced.

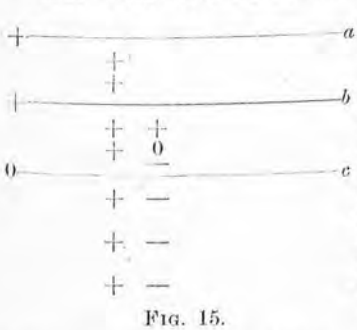


FIG. 15.

132. Fig. 16 indicates the method in which the neutralizing effect is produced in the case of the secondary and tertiary currents. The wire conducting the secondary current is represented by *b*, that conducting the tertiary by *c*, and the other wire, to receive the induction from these, by *d*. The direction of the influence, as before, is indicated by + 0 — —, &c., and the third wire is again seen to be in the *plus* region of the one current, and in the *minus* of the other. If however *d* is placed sufficiently near *c*, then neutralization will

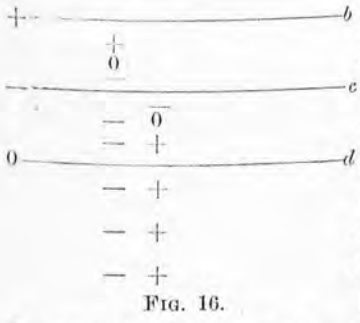


FIG. 16.

not take place, but the two currents will conspire to produce in it an induction in the same direction. A similar effect would also be produced were the wire *c*, in Fig. 15, placed sufficiently near the conductor *b*.

133. Currents of the several orders were likewise produced from the excitation of the magneto-electrical machine. The same neutralizing effects were observed between these as in the case of the currents from the galvanic battery, and hence we may infer that also the same alternations take place in the direction of the several currents.

134. In conclusion, I may perhaps be allowed to state, that the facts here presented have been deduced from a laborious series of experiments, and are considered as forming some addition to our knowledge of electricity, independently of any theoretical considerations. They appear to be intimately connected with various phenomena, which have been known for some years, but which have not been referred to any general law of action. Of this class are the discoveries of Savary on the alternate magnetism of steel needles placed at different distances from the line of a discharge of ordinary electricity,* and also the magnetic, screening influence of all metals, discovered by Dr. Snow Harris, of Plymouth.† A comparative study of the phenomena observed by these distinguished *savants*, and those given in this paper, would probably lead to some new and important developments. Indeed every part of the subject of electro-dynamic induction appears to open a field for discovery, which experimental industry cannot fail to cultivate with immediate success.

NOTE.

On the evening of the meeting at which my investigations were presented to the Society, my friend, Dr. Bache of the Girard College, gave an account of the investigations of Professor Ettingshausen, of Vienna, in reference to the improvement of the magneto-electric machine, some of the results of which he had witnessed at the University of Vienna,

* Annales de Chimie et de Physique, 1827.

† Philosophical Transactions, 1831.

about a year since. No published account of these experiments has yet reached this country, but it appears that Professor Ettingshausen had been led to suspect the development of a current in the metal of the keeper of the magneto-electric machine, which diminished the effect of the current in the coil about the keeper, and hence to separate the coil from the keeper by a ring of wood of some thickness, and afterwards, to prevent entirely the circulation of currents in the keeper, by dividing it into segments, and separating them by a non-conducting material. I am not aware of the result of this last device, nor whether the mechanical difficulties in its execution were fully overcome. It gives me pleasure to learn that the improvements, which I have merely suggested as deductions from the principles of the interference of induced currents (76), should be in accordance with the experimental conclusions of the above named philosopher.*

*[Re-printed in Silliman's American Journal of Science, March, 1840, vol. XXXVIII, pp. 209–243.]

CAPILLARY TRANSMISSION THROUGH SOLIDS.

(Proceedings of the American Philosophical Society, vol. 1, pp. 82, 83.)*

March 15, 1839.

Professor Henry made a verbal communication relating to a phenomenon of capillary action which had fallen under his notice.

A lead tube of about half an inch in diameter and eight inches long happened to be left with one end immersed in a cup of mercury, and on inspection a few days afterwards it was observed that the mercury had disappeared from the cup, and was found on the floor at the other end of the tube. Struck with the phenomenon, I again filled the cup with mercury; the next morning the same effect was exhibited. The mercury had again passed over through the tube, apparently like water through a capillary siphon, and was again found on the floor.

On cutting the tube into pieces, it was evident that the mercury had not passed along the hollow axis, but had apparently been transmitted through the pores of the solid metal. To determine this, a lead rod of about seven inches long and a quarter of an inch in diameter was bent into the form of a siphon. The shorter leg was immersed in a watch-glass filled with mercury, and a similar glass placed under the end of the longer leg, to receive the metal which might pass over. At the end of twenty-four hours a globule of mercury was perceived at the lower end; and in the course of five or six days all the mercury passed over, leaving a crop of beautiful arborescent crystals of an amalgam of lead in the upper glass.

The mercury did not pass along the surface of the wire, since the lead exhibited, externally, but little change of appearance, although the progress of the penetration could be traced by a slight variation of the color of the oxide on the surface.

The action is much influenced by the texture of the lead. When a rod of cast lead, of the same size and form, was

*[Re-printed in Silliman's American Journal of Science, December, 1839, vol. XXXVIII, pp. 180, 181.]

substituted for the one before described, the globule of mercury did not make its appearance at the lower end until about forty days, and all the mercury of the upper glass had not yet (after three months) entirely disappeared.

The penetration takes place much more readily in the direction of the laminæ of the metal than across them. A plate of thick sheet lead was formed into a cup, and mercury poured into this; and it was found that before a drop had passed directly through, the mercury oozed out all around the edge of the plate.

Professor Henry stated that he had in progress a variety of experiments to investigate this action, and if any results of importance were obtained he would communicate them to the Society.

LETTER ON ELECTRICAL INDUCTION.

(Proceedings of the American Philosophical Society, vol. I, pp. 134–136.)

October 18, 1839.

The following extract from a letter addressed by Professor Henry to Professor Bache was read, announcing the discovery of two distinct kinds of dynamic induction by a galvanic current.

"Since the publication of my last paper, I have received through the kindness of Dr. Faraday a copy of his fourteenth series of experimental researches, and in this I was surprised to find a statement directly in opposition to one of the principal results given in my paper. It is stated in substance in the 59th paragraph of my last communication to the American Philosophical Society, that when a plate of metal is interposed between a galvanic current and a conductor the secondary shock is neutralized. Dr. Faraday finds, on the contrary, under apparently the same circumstances, that no effect is produced by the interposition of the metal. As the fact mentioned forms a very important part of my paper, and is connected with nearly all the phenomena described subsequently to it, I was anxious to investigate the cause of the discrepancy between the results obtained by Dr. Faraday, and those found by myself. My

experiments were on such a scale, and the results so decided, that there could be no room for doubt as to their character; a secondary current of such intensity as to paralyze the arms having been so neutralized by the interposition of a plate and ribbon of metal, as not to be perceptible through the tongue. I was led by a little reflection to conclude that there might exist a case of induction similar to that of magnetism, in which no neutralization would take place; and I thought it possible that Dr. Faraday's results might have been derived from this. I have now however found a solution to the difficulty in the remarkable fact that an electrical current from a galvanic battery exerts two distinct kinds of dynamic induction. One of these produces, by means of a helix of long wire, intense secondary shocks at the moment of breaking the contact, and feeble shocks at the moment of making the contact. This kind of induction is capable also of being neutralized by the interposition of a plate of metal between two conductors. The other kind of induction is produced at the same time from the same arrangement, and does not give shocks, but affects the needle of the galvanometer. It is of equal energy at the moment of making contact and of breaking contact, and is not affected by the introduction of a plate of copper or zinc between the conductors.* The phenomena produced by the first kind of induction form the subject of my last paper, as well as that of the previous one; while it would appear from the arrangement of Dr. Faraday's experiments that the results detailed in his first series and those in the fourteenth were principally produced by the second kind of induction. Although I may be too sanguine in reference to the results of the discovery, yet I cannot refrain from adding that it appears to lead to a separation of the electrical induction of a galvanic current from the magnetical, and that it is a step of some importance towards a more precise knowledge of the phenomena of magneto-electricity."

*Since writing the account of the two kinds of induction I have found that the second kind, although not screened by a plate of copper or zinc, is affected by the introduction of a plate of iron. In the cases of the first kind of induction iron acts as any other metal.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. IV.

ON ELECTRO-DYNAMIC INDUCTION. (CONTINUED.)

(Transactions American Philosophical Society, n. s., vol. VIII, pp. 1–35.)*

Read June 19, 1840.

INTRODUCTION.

1. In the course of my last paper, (No. III,) it was stated that the investigations which it detailed were not as complete in some parts as I could wish, and that I hoped to develop them more fully in another communication. After considerable delay, occasioned by alterations in the rooms of the physical department of the college, I was enabled to resume my researches; and since then I have been so fortunate as to discover a series of new facts belonging to different parts of the general subject of my contributions. These I have announced to the Society at different times, as they were discovered, and I now purpose to select from the whole such portions as relate particularly to the principal subject of my last paper, namely, the induction at the beginning and ending of a galvanic current, and to present them as a continuation, and in a measure as the completion of this part of my researches. The other results of my labors in this line will be arranged for publication as soon as my duties will permit me to give them a more careful examination.

2. In the course of the experiments I am about to describe, I have had occasion to repeat and vary those given in my last paper, and I am happy to be able to state, in reference to the results, that except in some minor particulars which will be mentioned in the course of this paper, I have found no cause to desire a change in the accounts before published. My views however of the connection of the phenomena have been considerably modified, and I think rendered much more definite by the additional light which the new facts have afforded.

3. The principal articles of apparatus used in these experiments are nearly the same as those described in my last

*[The title-page of vol. VIII bears date 1843.]

paper, namely, several flat coils, and a number of long wire helices. (No. III, 6, 7, 8.*) I have however added to these a constant battery, on Prof. Daniell's plan, the performance of which has fully answered my expectations, and confirmed the accounts given of this form of the instrument by its author. It consists of thirty elements, formed of as many copper cylinders, open at the bottom, each five inches and a half in height, three inches and a half in diameter, and placed in earthen cups. A zinc rod is suspended in each of these, of the same length as the cylinders, and about one inch in diameter. The several elements are connected by a thick copper wire, soldered to the copper of one element, and dipping into a cup of mercury on the zinc of the next. The copper and zinc as usual are separated by a membrane, on both sides of which is placed a solution of one part of sulphuric acid in ten parts of water; and to this is added, on the side next the copper, as much sulphate of copper as will saturate the solution. The battery was sometimes used as a single series, with all its elements placed consecutively, and at others in two or three series, arranged collaterally, so as to vary the quantity and intensity of the electricity as the occasion might require.

4. The galvanometers mentioned in this paper, and referred to in the last, are of two kinds; one, which is used with a helix, to indicate the action of an induced current of intensity, consists of about five hundred turns of fine copper wire, covered with cotton thread, and more effectually insulated by steeping the instrument in melted cement, which was drawn into the spaces between the spires by capillary attraction. The other galvanometer is formed of about forty turns of a shorter and thicker wire, and is always used to indicate an induced current, of considerable quantity, but of feeble intensity. The needle of both these instruments is suspended by a single fibre of raw silk.

5. I should also state, that in all cases where a magnetizing spiral is mentioned in connection with a helix, the

*The numerals II or III included in parentheses refer to the corresponding Nos. of my previous Contributions.

article is formed of a long, fine wire, making about one hundred turns around the axis of a hollow piece of straw about two inches and a half long: also the spiral mentioned in connection with a coil, is formed of a short wire which makes about twenty turns around a similar piece of straw. The reason of the use of the two instruments in these two cases is the same as that for the galvanometers, under similar circumstances, namely, the helix gives a current of intensity, but of small quantity, while the coil produces one of considerable quantity, but of feeble intensity.

SECTION I.

On the Induction produced at the moment of the Beginning of a Galvanic Current, &c.

6. It will be recollected that the arrangement of apparatus employed in my last series of experiments gave a powerful induction at the moment of breaking the galvanic circuit, but the effect at making the same was so feeble as scarcely to be perceptible. I was unable in any case to get indications of currents of the third or fourth orders from the beginning induction, and its action was therefore supposed to be so feeble as not materially to affect the results obtained.

7. Subsequent reflection however led me to conclude that in order to complete this part of my investigations, a more careful study of the induction at the beginning of the current would be desirable; and accordingly on resuming the experiments, my attention was first directed to the discovery of some means by which the intensity of this induction might be increased. After some preliminary experiments, it appeared probable that the desired result could be obtained by using a compound galvanic battery, instead of the single one before employed. In reference to this conjecture, the constant battery before mentioned (3) was constructed, and a series of experiments instituted with it, the results of which agreed with my anticipation.

8. In the first experiment, coil No. 2, which it will be remembered (No. III, 7,) consists of a copper ribbon about sixty feet long, coiled on itself like the main spring of a watch,

was connected with the compound battery, and helix No. 1, (No. III, 8,) formed of one thousand six hundred and sixty yards of fine copper wire, was placed on the coil to receive the induction, as is shown in figure 3, which is again inserted here for the convenience of the reader.

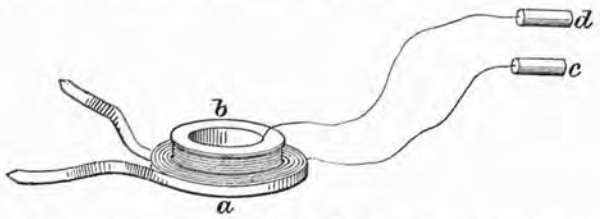


FIG. 3.—*a* represents coil No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

This arrangement being made, currents of increasing intensity were passed through the coil by constantly retaining one of its ends in the cup of mercury forming one extremity of the battery, and successively plunging the other end into the cups which served to form the connections of the several elements of the battery. With the current from one element, the shock at breaking the circuit was quite severe; but at making the same it was very feeble, and could be perceived in the fingers only, or through the tongue. With two elements in the circuit, the shock at the beginning was slightly increased; with three elements the increase was more decided, while the shock at breaking the circuit remained nearly of the same intensity as at first, or was comparatively but little increased. When the number of elements was increased to *ten*, the shock at making contact was found fully equal to that at breaking, and by employing a still greater number, the former was decidedly stronger than the latter, the difference continually increasing until all the thirty elements were introduced into the circuit.

9. In my last paper, a few experiments are mentioned as being made with a compound battery of Cruickshank's construction; but from the smallness of its plates, and the rapidity with which its power declined, I was led into the error of supposing that the induction at the ending of

the current, in the case of a short coil, was diminished by increasing the intensity of the battery; (see paragraph 19, of No. III;) but by employing the more perfect instrument of Professor Daniell in the arrangement of the last experiment, I am enabled to correct this error, and to state that the induction at the ending remains nearly the same, when the intensity of the battery is increased. If the induction depends in any degree on the quantity of current electricity in the conductor, then a slight increase in the induction should take place, since according to theory the current is somewhat increased in quantity, in the case of a long coil, by the increase of the intensity of the battery. Although very little, if any, difference could be observed in the intensity of the shock from the secondary current, yet the snap and deflagration of the mercury appeared to be greater from the primary current, when *ten* elements of the battery were included in the circuit, than with a single one. The other results which are mentioned in my last paper in reference to the compound battery are I believe correctly given.

10. The intensities of the different shocks in the foregoing experiments were compared by gradually raising the helix from the coil, (see Fig. 3,) until on account of the distance of the conductors, the shock in one case would be so much reduced as to be scarcely perceptible through the fingers or the tongue, while the shock from another arrangement, but with the same distance of the conductors, would be evident perhaps in the hands. The same method was generally employed in the experiments in which shocks are mentioned as being compared, in the other parts of this paper.

11. Experiments were next made to determine the influence of a variation in the length of the coil, the intensity of the battery remaining the same. For this purpose, the battery consisting of a single element, and the arrangement of the apparatus as represented in Fig. 3, the coil was diminished in length from sixty feet to forty-five, then to thirty, and so on. With the first mentioned length the shock, at making contact with the battery, was of course very feeble, and could be felt only in the tongue; with the

next shorter length it was more perceptible, and increased in intensity with each diminution of the coil, until a length of about fifteen feet appeared to give a maximum result.

12. The diminution of the intensity of the shock in the last experiment, after the length of the coil was diminished below fifteen feet, was due to the diminution of the number of spires of the coil, each of which, by acting on the helix, tends to increase the intensity of the secondary current, unless the combined length of the whole is too great for the intensity of the battery. That this is the fact is shown by the following experiment: the helix was placed on a single spire or turn of the coil, and the length of the other part of the copper ribbon, which did not act on the helix, was continually shortened, until the whole of it was excluded from the circuit; in this case the intensity of the shock at the beginning was constantly increased. We may therefore state generally, that at the beginning of the battery current, the induction of a unit of its length is increased by every diminution of the length of the conductor.

13. In the experiment given in paragraph 11, the intensity of the shock at the *ending* of the battery current diminishes with each diminution of the length of the coil; and this is also due to the decrease of the number of the spires of the coil, as is evident from an experiment similar to the last, in which the helix was placed on a coil consisting of only two turns or spires of copper ribbon; the shock at the ending, with this arrangement, was comparatively feeble, but could be felt in the hands. Different lengths of coil No. 2 were now introduced into the same circuit, but not so as to act on the helix; but although these were varied from four or five feet to the whole length of the coil, (sixty feet,) not the least difference in the intensity of the shock could be perceived. We have therefore the remarkable result, that the intensity of the ending induction of each unit of length of the battery current is not materially altered, at least within certain limits, by changing the length of the whole conductor. From this we would infer that the shock depends more on the intensity of the action than on the quantity of

the current, since we know that the latter is diminished in a given unit of the conductor by increasing the length of the whole.

14. We have seen (8) that with a circuit composed of ten elements of the compound battery and the coil No. 2, the shock, at the beginning of the current, was fully equal to that at the ending. It was however found that if in this case the length of the coil was increased, this shock was diminished; and we may state as an inference from several experiments, that however great may be the intensity of the electricity from the battery, the shock at the beginning may be so reduced, by a sufficient increase of the length of the primary circuit, as to be scarcely perceptible.

15. It was also found that when the thickness of the coil was increased, the length and intensity of the circuit remaining the same, the shock at the beginning of the battery current was somewhat increased. This result was produced by using a double coil; the electricity was made to pass through one strand, and immediately afterwards through both; the shock from the helix in the latter case was apparently the greater.

16. By the foregoing results we are evidently furnished with two methods of increasing at pleasure the intensity of the induction at the beginning of a battery current;—the one consisting in increasing the intensity of the source of the electricity, and the other in diminishing the resistance to conduction of the circuit while the intensity remains the same.

17. The explanation of the effects which we have given, relative to the induction at the beginning, is apparently not difficult. The resistance to conduction in the case of a long conductor and a battery of a single element is so great that the full development of the primary current may be supposed not to take place with sufficient rapidity to produce the instantaneous action on which the shock from the secondary current would seem to depend. But when a battery of a number of elements is employed, the poles of this, previous to the moment of completing the circuit, are in a state

of electrical tension; and therefore the discharge through the conductor may be supposed to be more sudden, and hence an induction of more intensity is produced.

18. That the shock at both making and breaking the circuit in some way depends on the rapidity of formation and diminution of the current is shown by the following experiment, in which the tension just mentioned does not take place, and in which also the current appears to diminish more slowly. The two ends of the coil were placed in the two cups which formed the poles of the battery, and permanently retained there during the experiment; also, at the distance of about six inches from—say the right hand end of the coil, a loop was made in the ribbon, which could be plunged into the cup containing the left hand end. With this arrangement, and while only the two extreme ends of the coil were in connection with the cups of mercury, of course the current passed through the entire length of the ribbon of the coil; but by plunging the loop into the left hand cup, the whole length of the coil, except the six inches before mentioned, was excluded from the battery circuit. And again, when the loop was lifted out of the cup, the whole length was included. In this way the current in the coil could be suddenly formed and interrupted, while the poles of the battery were continually joined by a conductor, but no shock with either a single or a compound battery could be obtained by this method of operation.

19. The feebleness of the shock at the beginning of the current, with a single battery and a long coil, is not entirely owing to the cause we have stated, (17,) namely the resistance to conduction offered by the long conductor, but also depends in a considerable degree (if not principally) on the adverse influence of the secondary current, induced in the primary conductor itself, as is shown by the result of the following experiment. Helix No, 1 was placed on a coil consisting of only three spires or turns of copper ribbon; with this, the shock both at making and breaking the circuit with a single battery could be felt in the hands. A compound coil was then formed of the copper ribbons of coils No. 3 and 4 rolled

together so that the several spires of the two alternated with each other, and when this was introduced into the circuit so as not to act on the helix by its induction, and the battery current passed through (for example) coil No. 3, the shock at making contact with the pole of the battery was so much reduced as to be imperceptible in the hands, while the shock at breaking the contact was about the same as before this addition was made to the length of the circuit. The ends of coil No. 4 were now joined so as to produce a closed circuit, the induced current in which would neutralize the secondary current in the battery conductor itself; and now the shock at making the contact was nearly as powerful as in the case where the short conductor alone formed the circuit with the battery. Hence the principal cause of the feebleness of the effect at the beginning of the battery current is the adverse action on the helix of the secondary current produced in the conductor of the battery circuit itself. The shock at the breaking of the circuit in this experiment did not appear affected by joining or separating the ends of coil No. 4.

20. Having investigated the conditions on which the inductive action at the beginning of a battery current depends, experiments were next instituted to determine the nature of the effects produced by this induction: and first, the coils were arranged in the manner described in my last paper, (No. III, 79,) for producing currents of the different orders. The result with this arrangement was similar to that which I have described in reference to the ending induction, namely, currents of the third, fourth, and fifth orders were readily obtained.

21. Also, when an arrangement of apparatus was made similar to that described in paragraph 87 of my last paper, it was found that a current of intensity could be induced from one of quantity and the converse.

22. Likewise, the same screening or rather neutralizing effect was produced, when a plate of metal was interposed between two consecutive conductors of the series of currents, as was described (No. III section IV) in reference to the ending

induction. In short, the series of induced currents produced at the beginning of the primary current appeared to possess all the properties belonging to those of the induction at the ending of the same current.

23. I may mention in this place that I have found in the course of these experiments that the neutralizing power of a plate of metal depends in some measure on its superficial extent. Thus a broad plate which extends in every direction beyond the helix and coil, produces a more perfect screening than one of the same metal and of the same thickness, but of a diameter only a little greater than that of the coil.

24. The next step in the investigation was to determine the direction of the currents of the different orders produced by the beginning induction; and for this purpose the magnetizing spirals (5) were used, and the results obtained by these verified by the indications of the galvanometer. It should be stated here, as a fact which was afterwards found of some importance, that although the needle of the galvanometer was powerfully deflected when the instrument was placed in the circuit of the secondary current, yet a very feeble effect was produced on it by the action of a current of the third, fourth, or fifth order. The directions however of these currents, as indicated by the feeble motions of the needle, were the same as those given by the magnetizing spiral.

25. The direction of the different currents produced at the making of the battery current, as determined by these instruments, is as follows, namely: the direction of the secondary current is, as stated by Dr. Faraday, adverse to that of the primary current, and also the direction of each succeeding current is opposite to that of the one which produced it. We have therefore from these results, and those formerly obtained, (No. III, 92,) the following series of directions of currents, one produced at the moment of beginning, and the other at that of ending of the battery current.

	At the Beginning.	At the Ending.
Primary current	+	+
Secondary current	—	+
Current of the third order	+	—
Current of the fourth order	—	+
Current of the fifth order	+	—

26. These two series, at first sight, may appear very different, but with a little attention, they will be seen to be of the same nature. If we allow that the induction at the ending of a galvanic battery should be opposite to that at the beginning of the same, then the sign at the top of the second column may be called minus instead of plus, and we shall have the second series —+ — + alternating precisely like the first.

27. In connection with the results given in the last two paragraphs, it is due to Mr. Sturgeon that I should state, that in a letter addressed to me and published in the *Annals of Electricity*, he has predicted from his theory, that I would find on examination the series of alternation of currents for the beginning induction which I have here given. I may however here add, it appears to me that this result might have been predicted without reference to any theory. There was no reason to suppose the induction at the beginning would be different in its nature from that at the ending, and therefore the series which would be produced from the former might be immediately inferred from that belonging to the latter, by recollecting that the direction of the induction at the beginning should be opposite to that at the ending. I do not wish it to be supposed however from this remark, that I had myself drawn any inference from my experiments as to the alternations of currents which might be produced by the beginning induction; the truth is, that this action was so feeble with the arrangement of apparatus I employed, that I supposed it could not produce a series of currents of the different orders.

28. In the course of the experiments given in this section, I have found that a shock can be produced without using a coil, by arranging about ten elements of the battery in the form of a circle, and placing the helix within this. The

shock was felt in the hands at the moment of closing the circuit, but the effect at opening the same was scarcely perceptible through the tongue. An attempt was also made to get indications of induction by placing the helix within a circle of dilute acid, connected with a battery instead of a coil, but the effect if any was very feeble.

29. I have shown, in the second number of my Contributions, that if the body be introduced into a circuit with a battery of one hundred and twenty elements, without a coil, a thrilling sensation will be felt during the continuance of the current, and a shock will be experienced at the moment of interrupting the current by breaking the circuit at any point. This result is evidently due to the induction of a secondary current in the battery itself, and on this principle the remarkable physiological effects produced by Dr. Ure, on the body of a malefactor, may be explained. The body, in these experiments, was made to form a part of the circuit, with a compound galvanic apparatus in which a series of interruptions was rapidly made by drawing the end of a conductor over the edges of the plates of the battery. By this operation a series of induced currents must have been produced in the battery itself, the intensity of which was greater than that of the primary current.

30. In this connection I may mention that the idea has occurred to me that the intense shocks given by the electrical fish may possibly be from a secondary current, and that the great amount of nervous organization found in these animals may serve the purpose of a long conductor.* It appears to me, that in the present state of knowledge, this is the only way in which we can conceive of electricity so intense being produced in organs imperfectly insulated and immersed in a conducting medium. But we have seen that an original current of feeble intensity can induce, in a long wire, a secondary current capable of giving intense shocks, although the several strands of the wire are separated from each other only by a covering of cotton thread. Whatever

*Since writing the above, I have found that M. Masson has suggested the same idea, in an interesting thesis lately published.

may be the worth of this suggestion, the secondary current affords the means of imitating the phenomena of the shock from the electrical eel, as described by Dr. Faraday. By immersing the apparatus (Fig. 3) in a shallow vessel of water, the handles being placed at the two extremities of the diameter of the helix, and the hands plunged into the water parallel to a line joining the two poles, a shock is felt through the arms; but when the contact with the water is made in a line at right angles to the last, only a slight sensation is felt in each hand, but no shock.

31. Since the publication of my last paper, I have exhibited to my class the experiment (No. III, Sec. III) relative to the induction at a distance on a much larger scale. All my coils were united so as to form a single length of conductor of about four hundred feet, and this was rolled into a ring of five and a half feet in diameter, and suspended vertically against the inside of the large folding doors which separate the laboratory from the lecture room. On the other side of the doors, in the lecture room, and directly opposite the coil, was placed a helix, formed of upwards of a mile of copper wire, one sixteenth of an inch in thickness, and wound into a hoop of four feet in diameter. With this arrangement, and a battery of one hundred and forty-seven square feet of zinc surface divided into eight elements, shocks were perceptible in the tongue, when the two conductors were separated, to the distance of nearly seven feet; at the distance of between three and four feet, the shocks were quite severe. The exhibition was rendered more interesting by causing the induction to take place through a number of persons standing in a row between the two conductors.

SECTION II.

On apparently two kinds of Electro-dynamic Induction.

32. The investigations arranged under this head had their origin in the following circumstances. After the publication of my last paper, I received, through the kindness of Dr. Faraday, a copy of the fourteenth series of his Researches, and in this I was surprised to find a statement which ap-

peared in direct opposition to one of the principal facts of my communication. In paragraph 59, I state in substance that when a plate of metal is interposed between the coil transmitting a galvanic current, and the helix placed above it to receive the induction, the shock from the secondary current is almost perfectly neutralized. Dr. Faraday, in the extension of his new and ingenious views of the agency of the intermediate particles in transmitting induction, was led to make an experiment on the same point, and apparently, under the same circumstances, he found that it "makes not the least difference whether the intervening space between the two conductors is occupied by such insulating bodies as air, sulphur, and shell-lac, or such conducting bodies as copper and other non-magnetic metals."

33. As the investigation of the fact mentioned above forms an important part of my paper, and is intimately connected with almost all the phenomena subsequently described in the communication I was of course anxious to discover the cause of so remarkable a discrepancy. There could be no doubt of the truth of my results, since a shock from a secondary current which would paralyze the arms was so much reduced by the interposition of plates of metal as scarcely to be felt through the tongue.

34. After some reflection however the thought occurred to me that induction might be produced in such a way as not to be affected by the interposition of a plate of metal. To understand this, suppose the end of a magnetic bar placed perpendicularly under the middle of a plate of copper, and a helix suddenly brought down on this; an induced current would be produced in the helix by its motion towards the plate, since the copper, in this case, could not screen the magnetic influence. Now, if we substitute for the magnet a coil through which a galvanic current is passing, the effect should be the same. The experiment was tried by attaching the ends of the helix to a galvanometer,* and the

*The arrangement will be readily understood by supposing in Fig. 3, the handles removed, and the ends of the helix joined to the ends of the wire of a galvanometer; also, by a plate of metal interposed between the helix and the coil.

result was as I expected: when the coil was suddenly brought down on the plate the needle swung in one direction and when lifted up, in the other; the amount of deflection being the same, whether the plate was interposed or not.

35. It must be observed in this experiment, that the plate was at rest, and consequently did not partake of the induction produced by the motion of the helix. From my previous investigations, I was led to conclude that a different result would follow, were a current also generated in the plate by simultaneously moving it up and down with the helix. This conclusion however was not correct, for on making the experiment, I found that the needle was just as much affected when the plate was put in motion with the helix as when the latter alone was moved.

36. This result was so unexpected and remarkable, that it was considered necessary to repeat and vary the experiment in several ways. First, a coil was interposed instead of the plate, but whether the coil was at rest or in motion with the helix, with its ends separated or joined, the effect on the galvanometer was still the same; not the least screening influence could be observed. In reference to the use of the coil in this experiment, it will be recollected that I have found this article to produce a more perfect neutralization than a plate.

37. Next, the apparatus remaining the same, and the helix at rest during the experiment, currents were induced in it by moving the battery attached to the coil up and down in the acid. But in this case as in the others the effect on the galvanometer was the same, whether the plate or the coil was interposed or not.

38. The experiment was also tried with magneto-electricity. For this purpose, about forty feet of copper wire, covered with silk, were wound around a short cylinder of stiff paper, and into this was inserted a hollow cylinder of sheet copper, and into this again, a short rod of soft iron; when the latter was rendered magnetic, by suddenly bringing in contact with its two ends the different poles of two magnets, a current was of course generated in the wire, and this as before

was found to affect the galvanometer to the same degree, when the copper cylinder was interposed, as when nothing but the paper intervened.

39. The last experiment was also varied by wrapping two copper wires of equal length around the middle of the keeper of a horse-shoe magnet, leaving the ends of the inner one projecting, and those of the outer attached to a galvanometer. A current was generated in each by moving the keeper on the ends of the magnet, but the effect on the galvanometer was not in the least diminished by joining the ends of the inner wire.

40. At first sight, it might appear that all these results are at variance with those detailed in my last paper, relative to the effect of interposed coils and plates of metal. But it will be observed that in all the experiments just given, the induced currents are not the same as those described in my last communication. They are all produced by motion, and have an appreciable duration, which continues as long as the motion exists. They are also of low intensity, and thus far I have not been able to get shocks by any arrangement of apparatus from currents of this kind. On the other hand, the currents produced at the moment of *suddenly* making or breaking a galvanic current, are of considerable intensity, and exist but for an instant. From these and other facts presently to be mentioned, I was led to suppose that there are two kinds of electro-dynamic induction; one of which can be neutralized by the interposition of a metallic plate between the conductors, and the other not.

41. In reference to this surmise, it became important to examine again all the phenomena of induction at suddenly making and breaking a galvanic current.* And in connection with this part of the subject, I will first mention a fact which was observed in the course of the experiments given in the last section, on the direction of the induced currents of different orders. It was found that though the indications of the galvanometer were the same as those of the spiral, in reference to the direction of the induced currents,

*See my last paper. (No. III.)

yet they were very different in regard to the intensity of the action. Thus, when the arrangement of the apparatus was such that the induction at making the battery circuit was so feeble as not to give the least magnetism to the needle, and so powerful at the ending as to magnetize it to saturation, the indication of the galvanometer was the same in both cases.

42. Also, similar results were obtained in comparing the shock and the deflection of the galvanometer. In one experiment for example the shock was so feeble at making contact that it could scarcely be perceived in the fingers, but so powerful at the breaking of the circuit as to be felt in the breast; yet the galvanometer was deflected about thirty-five degrees to the right, at the beginning of the current, and only an equal number of degrees to the left, at the ending of the same.

43. In another experiment, the apparatus being the same as before, the magnetizing spiral and the galvanometer were both at once introduced into the circuit of the helix. A sewing needle being placed in the spiral, and the contact with the battery made, the needle showed no signs of magnetism, although the galvanometer was deflected thirty degrees. The needle being replaced, and the battery circuit broken, it was now found strongly magnetized, while the galvanometer was moved only about as much as before in the opposite direction.

44. Also, effects similar to those described in the last two paragraphs were produced when the apparatus was so arranged as to cause the induction at the beginning of the battery current to predominate. In this case the galvanometer was still almost equally affected at making and breaking battery contact, or any difference which was observed could be referred to a variation in the power of the battery during the experiment.

45. Another fact of importance belonging to the same class has been mentioned before, (24,) namely, that the actions of the currents of the third, fourth, and fifth orders produce a very small effect on the galvanometer, compared with that

of the secondary current; and this is not on account of the diminishing power alone of the successive inductions, as will be evident from the following experiment: By raising the helix from the coil, in the arrangement of the apparatus for the secondary current, the shock was so diminished as to be inferior to one produced by the arrangement for a tertiary current, yet while with the secondary current the needle was deflected twenty-five degrees, with the tertiary it moved scarcely more than one degree; and with the currents of the fourth and fifth orders the deflections were still less, resembling the effect of a slight impulse given to the end of the needle.

46. With the light obtained from the foregoing experiments, I was the more fully persuaded that some new and interesting results might be obtained by a re-examination of my former experiments, on the phenomena of the interposed plate of metal, in the case where the induction was produced by making and breaking the circuit with a cup of mercury; and in this I was not disappointed. The coil (Fig. 3) being connected with a battery of ten elements, the shocks, both at making and breaking the circuit, were very severe; and these as usual were almost entirely neutralized by the interposition of a zinc plate. But when the galvanometer was introduced into the circuit instead of the body, its indications were the same whether the plate was interposed or not; or in other words the galvanometer indicated no screening, while, under the same circumstances, the shocks were neutralized.

47. A similar effect was observed when the galvanometer and the magnetizing spiral were together introduced into the circuit. The interposition of the plate entirely neutralized the magnetizing power of the spiral, in reference to tempered steel, while the deflections of the galvanometer were unaffected.

48. In order to increase the number of facts belonging to this class, the last experiments were varied in several ways; and first, instead of the hard steel needle, one of soft iron wire was placed in the spiral, with a small quantity of iron filings almost in contact with one of its ends. The plate

being interposed, the small particles of iron were attracted by the end of the needle, indicating a feeble, temporary development of magnetism. Hence the current which moves the needle, and is not neutralized by the interposed plate, also feebly magnetizes soft iron, but not hard steel.

49. Again, the arrangement of apparatus being as in paragraph 46, instead of a plate of zinc, one of cast iron, of about the same superficial dimensions, but nearly half an inch thick, was interposed; with this, the magnetizing power of the spiral, in reference to tempered steel, was neutralized; and also the action of the galvanometer was much diminished.

50. Another result was obtained by placing in the circuit of the helix, (Fig. 3,) at the same time, the galvanometer, the spiral, and a drop of distilled water; with these the magnetizing power of the spiral was the same as without the water, but the deflection of the galvanometer was reduced from ten to about four degrees. In addition to these the body was also introduced into the same circuit; the shocks were found very severe, the spiral magnetized needles strongly, but the galvanometer was still less moved than before. The current of low intensity, which deflects the needle of the galvanometer in these instances, was partially intercepted by the imperfect conduction of the water and the body.

51. To exhibit the results of these experiments with still more precision, an arrangement of apparatus was adopted similar to that used by Dr. Faraday, and described in the fourteenth series of his Researches, namely, a double galvanometer was formed of two separate wires of equal length and thickness, and wound together on the same frame; and also a double magnetizing spiral was prepared by winding two equal wires around the same piece of hollow straw. Coil No. 1, connected with the battery, was supported perpendicularly on a table, and coils Nos. 3 and 4 were placed parallel to this, one on each side, to receive the induction, the ends of these being so joined with those of the galvanometer and the spiral that the induced current from the one

coil would pass through the two instruments, in an opposite direction to that of the current from the other coil. The two outside coils were then so adjusted, by moving them to and from the middle coil, that the induced currents perfectly neutralized each other in the two instruments, and the needle of the galvanometer and that in the spiral were both unaffected when the circuit of the battery was made and broken. With this delicate arrangement the slightest difference in the action of the two currents would be rendered perceptible; but when a zinc plate was introduced so as to screen one of the coils, the needle of the galvanometer still remained perfectly stationary, indicating not the least action of the plate, while the needle in the spiral became powerfully magnetic. When however a plate of iron was interposed instead of the one of zinc, the needle of the galvanometer was also affected.

52. From the foregoing results it would seem that the secondary current, produced at the moment of the sudden beginning or ending of a galvanic current, by making and breaking contact with a cup of mercury, consists of two parts, which possess different properties. One of these is of low intensity, can be interrupted by a drop of water, does *not* magnetize hardened steel needles, and is *not* screened by the interposition of a plate of any metal, except iron, between the conductors. The other part is of considerable intensity, is *not* intercepted by a drop of water, develops the magnetism of hardened steel, gives shocks, and *is* screened or neutralized by a closed coil, or a plate of any kind of metal. Also, the induced current produced by moving a conductor towards or from a battery current, and that produced by the movement up and down of a battery in the acid, are of the nature of the first mentioned part, while the currents of the third, fourth, and fifth orders partake almost exclusively of the properties of the second part.*

---

*[The above paper was reprinted in Silliman's American Journal of Scienc, April, 1841, vol. XLI, pp. 117–152. Also, in Sturgeon's Annals of Electricity, etc., vol. VII, pp. 21–56. Also, in the London and Edinburgh Philosophical Magazine, June, 1841, vol. XVIII, pp. 482–514.]

Postscript.

53. The principal facts and conclusions of this section were announced to the Society in October, 1839, and again, in June last, presented in the form in which they are here detailed. Since then however I have had leisure to examine the subject more attentively, and after a careful comparison of these results with those before given, I have obtained the more definite views of the phenomena which are given in the following section.

SECTION III.

Theoretical Considerations relating to the Phenomena described in this and the preceding Communications.

Read November 20, 1840.

54. The experiments given in number III of my Contributions were merely arranged under different heads, and only such inferences drawn from them as could be immediately deduced without reference to a general explanation. The addition however which I have since made to the number of facts, affords the means of a wider generalization; and after an attentive consideration of all the results given in this and the preceding papers, I have come to the conclusion that they can all be referred to the simple laws of the induction at the beginning and the ending of a galvanic current.

55. In the course of these investigations the limited hypotheses which I have adopted have been continually modified by the development of new facts, and therefore my present views, with the further extension of the subject, may also require important corrections. But I am induced to believe, from its exact accordance with all the facts, so far as they have been compared, that if the explanation I now venture to give be not absolutely true, it is so at least in approximation, and will therefore be of some importance in the way of suggesting new forms of experiment, or as a first step towards a more perfect generalization.

56. To render the laws of induction at the beginning and the ending of a galvanic current more readily applicable to

the explanation of the phenomena, they may be stated as follows: 1. During the time a galvanic current is increasing in quantity in a conductor, it induces, or tends to induce, a current in an adjoining parallel conductor in an opposite direction to itself. 2. During the continuance of the primary current in full quantity, no inductive action is exerted. 3. But when the same current begins to decline in quantity, and during the whole time of its diminishing, an induced current is produced in an opposite direction to the induced current at the beginning of the primary current.

57. In addition to these laws, I must frequently refer to the fact, that *when the same quantity of electricity in a current of short duration is passed through a galvanometer, the deflecting force on the needle is the same, whatever be the intensity of the electricity.* By intensity is here understood the ratio of a given quantity of force to the time in which it is expended;* and according to this view, the proposition stated is an evident inference from dynamic principles. But it does not rest on considerations of this kind alone, since it has been proved experimentally by Dr. Faraday, in the third series of his Researches.

58. In order to form a definite conception of the several conditions of the complex phenomena which we are about to investigate, I have adopted the method often employed in physical inquiries, of representing the varying elements of action by the different parts of a curve. This artifice has been of much assistance to me in studying the subject, and without the use of it at present, I could scarcely hope to present my views in an intelligible manner to the Society.

59. After making these preliminary statements, we will now proceed to consider the several phenomena; and first, let us take the case in which the induction is most obviously produced in accordance with the laws as above stated (56), namely, by immersing a battery into the acid, and also by withdrawing it from the same. During the time of the descent of the battery into the liquid, the conductor connected with it is constantly receiving additional quantities

*Or, more correctly speaking, the ratio of two quantities of the same species, representing the force and time.

of current electricity, and each of these additions produces an inductive action on the adjoining secondary conductor. The amount therefore of induced current produced during any moment of time will be just in proportion to the corresponding increase in the current of the battery during the same moment. Also, the amount of induction during any moment while the current of the battery is diminishing in quantity will be in proportion to the decrease during the same moment.

60. The several conditions of this experiment may be represented by the different parts of the curve, *A*, *B*, *C*, *D*, Fig. 17, in which the distances *A a*, *A b*, *A c*, represent the times during which the battery is descending to different depths into the acid; and the corresponding ordinates, *a g*, *b h*, *c B*, represent the amount of current electricity in the battery conductor corresponding to these times. The differences of the ordinates, namely, *a g*, *m h*, *n B*, express the increase in the quantity of the battery current during the corresponding moments of time represented by *A a*, *a b*, *b c*; and since the inductive action (59) is just in proportion to the increase, the same differences will also represent the amount of induced action exerted on the secondary conductor during the same moments of time.

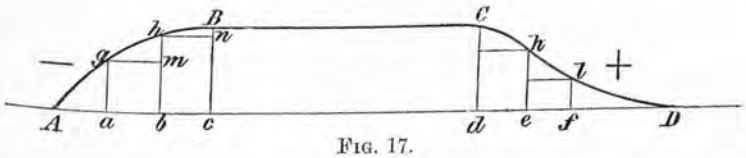


FIG. 17.

61. When the battery is fully immersed in the acid, or when the current in the conductor has reached its state of maximum quantity, and during the time of its remaining constant, no induction is exerted; and this condition is expressed by the constant ordinates of the part of the curve *B C*, parallel to the axis. Also, the inductive action produced by each diminution of the battery current, while the apparatus is in the progress of being drawn from the acid, will be represented by the differences of the ordinates at the other end, *C*, *D*, of the curve.

62. The sum of the several increments of the battery current, up to its full development, will be expressed by the ordinate c B, and this will therefore also represent the whole amount of inductive action exerted in one direction at the beginning of the primary current; and, for the same reason, the equal ordinate, $C\,d$, will represent the whole induction in the other direction at the ending of the same current. Also, the whole time of continuance of the inductive action at the beginning and ending will be represented by $A\,c$ and $d\,D$.

63. If we suppose the battery to be plunged into the acid to the same depth, but more rapidly than before, then the time represented by $A\,c$ will be diminished, while the whole amount of inductive force expended remains the same; hence, since the same quantity of force is exerted in a less time, a greater intensity of action will be produced (57), and consequently a current of more intensity, but of less duration, will be generated in the secondary conductor. The intensity of the induced currents will therefore evidently be expressed by the ratio of the ordinate $c\,B$ to the abscissa $A\,c$. Or, in more general and definite terms, the intensity of the inductive action at any moment of time will be represented by the ratio of the rate of increase of the ordinate to that of the abscissa for that moment.*

64. It is evident from the last paragraph, that the greater or less intensity of the inductive action will be immediately presented to the eye by the greater or less obliquity of the several parts of the curve to the axis. Thus, if the battery be suddenly plunged into the acid for a short distance, and then gradually immersed through the remainder of the depth, the varying action will be exhibited at once by the form of $A\,B$, the first part of the curve, Fig. 17. The steepness of the part $A\,g$ will indicate an intense action for a

*According to the differential notation, the intensity will be expressed by $\frac{dy}{dx}$. In some cases the effect may be proportional to the intensity multiplied by the quantity, and this will be expressed by $\frac{dy^2}{dx}$, x and y representing as usual the variable abscissa and ordinate.

short time $A\ a$, while the part $g\ B$ denotes a more feeble induction during the time represented by $a\ c$. In the same way, by drawing up the battery suddenly at first, and afterwards slowly, we may produce an inductive action such as would be represented by the parts between C and D of the ending of the curve.

65. Having thus obtained representations of the different elements of action, we are now prepared to apply these to the phenomena. And first, however varied may be the intensity of the induction expressed by the different parts of the two ends of the curve, we may immediately infer that a galvanometer, placed in the circuit of the secondary conductor, will be equally affected at the beginning and ending of the primary current; for, since the deflection of this instrument is due to the whole amount of a current, whatever may be its intensity (57), and since the ordinates $c\ B$ and $C\ d$, which represent the quantity of induction in the two directions, are equal, and consequently the amount of the secondary current, therefore the deflection at the beginning and ending of the battery current will in all cases be equal. This inference is in strict accordance with the results of experiment; for however rapidly or slowly we may plunge the battery into the acid, and however irregular may be the rate at which it is drawn out, still, if the whole effect be produced within the time of one swing of the needle, the galvanometer is deflected to an equal degree.

66. Again, the intensity of one part of the inductive action, for example that represented by $A\ g$, may be supposed to be so great as to produce a secondary current capable of penetrating the body, and of thus producing a shock* while the other parts of the action, represented by $g\ B$ and $C\ D$, are so feeble as to effect the galvanometer only. We would then have a result the same as one of those given in the last section (42), and which was suppposed to be produced by two kinds of induction; for if the shock were referred to as the test of the existence of an induced current, one would be

*The shock depends more on the intensity than on the quantity. See paragraph 13.

found at the beginning only of the battery current, while, if the galvanometer were consulted, we would perceive the effects of a current as powerful at the ending as at the beginning.

67. The results mentioned in the last paragraph cannot be obtained by plunging a battery into the acid; the formation of the current in this way is not sufficiently rapid to produce a shock. The example was given to illustrate the manner in which the same effect is supposed to be produced, in the case of the more sudden formation of a current, by plunging one end of a conductor into a cup of mercury permanently attached to a battery already in the acid, and in full operation. The current in this case—rapid as may be its development, cannot be supposed to assume *per saltum* its maximum state of quantity; on the contrary, from the general law of continuity, we would infer that it passes through all the intermediate states of quantity, from that of no current, (if the expression may be allowed,) to one of full development; there are however considerations of an experimental nature which would lead us to the same conclusion, (18,) (90,) and also to the further inference that the *decline* of the current is not instantaneous. According to this view therefore the inductive action at the beginning and the ending of a primary current, of which the formation and interruption are effected by means of the contact with a cup of mercury, may also be represented by the several parts of the curve, Fig. 17.

68. We have now to consider how the rate of increase or diminution of the current, in the case in question, can be altered by a change in the different parts of the apparatus; and first, let us take the example of a single battery and a short conductor, making only one or two turns around the helix; with this arrangement a feeble shock, as we have seen, (11,) will be felt at the making, and also at the breaking of the circuit. In this case it would seem that almost the only impediment to the most rapid development of the current would be the resistance of the metal to conduction; and this we might suppose would be more rapidly overcome

by increasing the tension of the electricity; and accordingly we find that if the number of elements of the battery be increased, the shock at making the circuit will also be increased, while that at breaking the circuit will remain nearly the same. To explain however this effect more minutely, we must call to mind the fact before referred to, (17,) that when the poles of a compound battery are not connected, the apparatus acquires an accumulation of electricity, which is discharged at the first moment of contact, and which in this case would more rapidly develop the full current, and hence produce the more intense action on the helix at making the circuit.

69. The shock, and also the deflection of the needle, at breaking the circuit with a compound battery and a short coil, (9,) appear nearly the same as with a battery of a single element, because the accumulation just mentioned, in the compound battery, is discharged almost instantly, and according to the theory (71) of the galvanic current, leaves the constant current in the conductor nearly in the same state of quantity as that which would be produced by a battery of a single element; and hence the conditions of the ending of the current are the same in both cases. Indeed, in reference to the ending induction, it may be assumed as a fact which is in accordance with all the experiments, (9, 13, 73, 74, 75, 76, &c.,) as well as with theoretical considerations,* *that when the circuit is broken by a cup of mercury, the rate of the diminution of the current, within certain limits, remains the same, however the intensity of the electricity or the length of the conductor may be varied.*

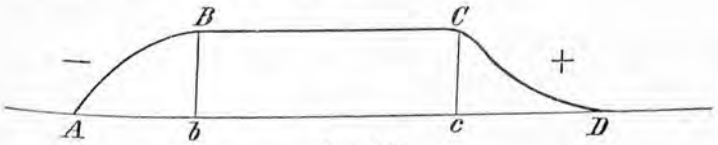


FIG. 18.

70. The several conditions of the foregoing examples are exhibited by the parts of the curves, Figs. 18 and 19. The

* Adopting here the theory of Ohm.

gradual development of the current in the short conductor, with a single battery, and the gradual decline of the same, are represented by the gentle rise of $A\,B$ and fall of $C\,D$, Fig. 18; while, in the next Fig., (19,) the sudden rise of $A\,B$ indicates the intensity which produces the increased shock, after the number of elements of the battery has been increased. The accumulation of the electricity, which almost instantly subsides, is represented by the part $B\,c\,e$,

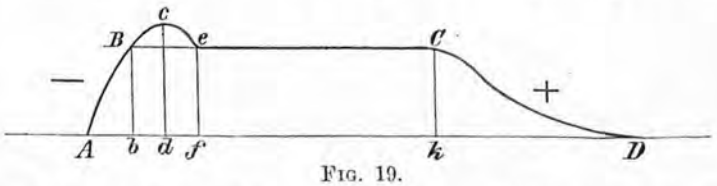


FIG. 19.

Fig. 19, and from this we see, at once, that although the shock is increased by using the compound battery, yet the needle of the galvanometer will be deflected only to the same number of degrees, since the parts $B\,c$ and $c\,e$ give inductive actions in contrary directions, and both within the time of the single swing of the needle, and consequently they will neutralize each other. The resulting deflecting force will therefore be represented by $e\,f$, which is equal to $C\,k$, or to $b\,B$, in Fig. 19. The intensity of the shock at the breaking is represented as being the same in the two figures, by the similarity of the rate of descent of the part $C\,D$ of the curve in each.

71. We have said (69) that the quantity of current electricity in a short conductor and a compound battery, after the first discharge, is nearly the same as with a single battery. The exact quantity, according to the theory of Ohm, in a unit of length of the conductor, is given by the formula—

$$\frac{n\,A}{r\,n + R}.$$

In this, n represents the number of elements; A, the electro-motive force of one element; r, the resistance to conduction of one element; and R, the length of the conductor, or rather, its resistance to conduction in terms of r. Now when R is very small in reference to $r\,n$, as is the case with a very

short metallic conductor, it may be neglected, and then the expression becomes

$$\frac{n\,A}{r\,n} \text{ or } \frac{A}{r};$$

and since this expresses the quantity of current electricity in a unit of the length of the circuit, with either a single or a compound battery, therefore with a short conductor the quantity of current electricity in the two cases is nearly the same.

72. Let us next return to the experiment with a battery of a single element, (68,) and instead of increasing the intensity of the apparatus, as in the last example, let the length of the conductor be increased; then the intensity of the shock at the beginning of the current, as we have seen, (14,) will be diminished, while that of the one at the ending will be increased. That the shock should be lessened at the beginning, by increasing the length of the conductor, is not surprising, since as we might suppose, the increased resistance to conduction would diminish the rapidity of the development of the current. But the secondary current, which is produced in the conductor of the primary current itself, as we have seen, (19,) is the principal cause which lessens the intensity of the shock; and the effect of this, as will be shown hereafter, may also be inferred from the principles we have adopted.

73. The explanation of the increased shock at the moment of breaking the circuit with the long conductor, rests on the assumption before mentioned, (69,) that the velocity of the diminution of a current is nearly the same in the case of a long conductor as in that of a short one. But to understand the application of this principle more minutely, we must refer to the change which takes place in the quantity of the current in the conductor by varying its length; and this will be given by another application of the formula before stated, (71.) This, in the case of a single battery, in which n equals unity, becomes

$$\frac{A}{r+R};$$

and since this, as will be recollected, represents the quantity

of current electricity in a unit of length of the conductor, we readily infer from it that by increasing the length of the conductor, or the value of R, the quantity of current in a unit of the length is lessened. And if the resistance of a unit of the length of the conductor were very great in comparison with that of r, (the resistance of one element of the battery,) then the formula would become

$$\frac{A}{R},$$

or the quantity in a single unit of the conductor would be inversely as its entire length, and hence the amount of current electricity in the whole conductor would be a constant quantity, whatever might be its length. This however can never be the case in any of our experiments, since in no instance is the resistance of R very great in reference to r, and, therefore, according to the formula, (73,) the whole quantity of current electricity in a long conductor is always somewhat greater than in a short one.

74. Let us however in order to simplify the conditions of the induction at the ending of a current, suppose that the quantity in a unit of the conductor is inversely at its whole length, or in other words that the quantity of current electricity is the same in a long conductor as in a short one; and let us also suppose for an example that the length of the spiral conductor, (Fig. 3,) was increased from one spire to twenty spires; then, if the velocity of the diminution of the section of the current is the same (69) in the long conductor as in the short one, the shock which would be received by submitting the helix to the action of one spire of the long coil would be nearly of the same intensity as that from one spire of the short conductor; the quantity of induction however as shown by the galvanometer, should be nearly twenty times less; and these inferences I have found in accordance with the results of experiments, (75.) If however instead of placing the helix on one spire of the long conductor, it be submitted at once to the influence of all the twenty spires, then the intensity of the shock should be twenty times greater, since twenty times the quantity of

current electricity collapses (if we may be allowed the expression) in the same time, and exerts at once all its influence on the helix. If in addition to this we add the consideration that the whole quantity of current electricity in a long conductor is greater than that in a short one, (73,) we shall have a further reason for the increase of the terminal shock, when we increase the length of the battery conductor.

75. The inference given in the last paragraph relative to the change in the quantity of the induction, but not in the intensity of the shock from a single spire, by increasing the whole length of the conductor, is shown to be true by repeating the experiment described in paragraph 13. In this, as we have seen, the intensity of the shock remained the same, although the length of the circuit was increased by the addition of coil No. 2. When however the galvanometer was employed in the same arrangement, the whole quantity of induction, as indicated by the deflection of the needle, was diminished almost in proportion to the increased length of the circuit. I was led to make this addition to the experiment (13) by my present views.

76. The explanation given in paragraph 74 also includes that of the peculiar action of a long conductor, either coiled or extended, in giving shocks and sparks from a battery of a single element, discovered by myself in 1832; (see No. II.) The induction in this case takes place in the conductor of the primary current itself, and the secondary current which is produced is generated by the joint action of each unit of the length of the primary current. Let us suppose for illustration that the conductor was at first one foot long, and afterwards increased to twenty feet. In the first case, because the short conductor would transmit a greater quantity of electricity, the secondary current produced by it would be one of considerable quantity, or power to deflect a galvanometer; but it would be of feeble intensity, for although the primary current would collapse with its usual velocity, (69,) yet, acting on only a foot of conducting matter, the effect (74) would be feeble. In the second case, each foot of the twenty feet of the primary current would severally

produce an inductive action of the same intensity as that of the short conductor, the velocity of collapse being the same; and as they are all at once exerted on the same conductor, a secondary current would result of twenty times the intensity of the current in the former case.

77. To render this explanation more explicit, it may be proper to mention that a current produced by an induction on one part of a long conductor of uniform diameter, must exist of the same intensity in every other part of the conductor; hence, the action of the several units of length of the primary current must re-enforce each other, and produce the same effect on its own conductor that the same current would if it were in a coil, and acting on a helix. I need scarcely add, that in this case, as in that given in paragraph 74, the whole amount of induction is greater with the long conductor than with the short one, because the quantity of current electricity is greater in the former than in the latter.

78. We may next consider the character of the secondary current, in reference to its action in producing a tertiary current in a third conductor. The secondary current consists (as we may suppose) in the disturbance for an instant of the natural electricity of the metal, which subsiding leaves the conductor again in its natural state; and whether it is produced by the beginning or ending of a primary current, its nature, as we have seen, (22,) is the same. Although the time of continuance of the secondary current is very short, still we must suppose it to have some duration, and that it increases, by degrees, to a state of maximum development, and then diminishes to the normal condition of the metal of the conductor; the velocity of its development, like that of the primary current, will depend on the intensity of the action by which it is generated, and also perhaps in some degree, on the resistance of the conductor; while, agreeably to the hypothesis we have assumed, (69,) the velocity of its diminution is nearly a constant quantity, and is not affected by changes in these conditions; hence, if we suppose the induction which produces the secondary current to be sufficiently intense, the velocity of its development will ex-

ceed that of its diminution, as in the example of the primary current from the intense source of the compound battery of many elements. Now this is the case with the inductions which produce currents of the different orders, capable of giving shocks or of magnetizing steel needles; the secondary currents from these are always of considerable intensity, and hence their rate of development must be greater than that of their diminution, and, consequently, they may be represented by a curve of the form exhibited in Fig. 20, in which

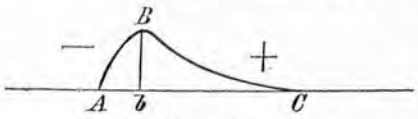


FIG. 20.

there is no constant part, and in which the steepness of *A B* is greater than that of *B C*. There are however other considerations, which will be noticed hereafter, (89,) which may affect the form of the part *B C* of the curve, rendering it still more gradual in its descent, or in other words which tend to diminish the intensity of the ending induction of the secondary current.

79. It will be seen at once, by an inspection of the curve, that the effect produced in a third conductor, and which we have called a tertiary current, is not of the same nature as that of a secondary current. Instead of being a single development in one direction, it consists of two instantaneous currents, one produced by the induction of *A B*, and the other by that of *B C*, in opposite directions, of equal quantities, but of different intensities. The whole quantity of induction in the two directions, will each be represented by the ordinate *B b*, and hence they will nearly neutralize each other, in reference to their action on the galvanometer, in the circuit of the third conductor. I say they will *nearly* neutralize each other, because, although they are equal in quantity, they do not both act in absolutely the same moment of time. The needle will therefore be slightly affected; it will be impelled in one direction—say to the right, by the induction of *A B*, but, before it can get fairly under

way, it will be arrested, and turned in the other direction, by the action of *B C*. This inference is in strict accordance with observation; the needle, as we have seen, (24,) starts from a state of rest, with a velocity which apparently would send it through a large arc, but before it has reached perhaps more than half a degree, it suddenly stops, and turns in the other direction. As the needle is first affected by the action of *A B*, it indicates a current in the adverse direction to the secondary current.

80. Although the two inductions in the tertiary conductor nearly neutralize each other, in reference to the indications of the galvanometer, yet this is far from being the case with regard to the shocks, and the magnetization of steel needles. These effects may be considered as the results alone of the action of *A B;* the induction of *B C* being too feeble in intensity to produce a tertiary current of sufficient power to penetrate the body, or overcome the coercive power of the hardened steel. Hence, in reference to the shock, and magnetization of the steel needle, we may entirely neglect the action of *B C*, and consider the tertiary excitement as a single current, produced by the action *A B;* and because this is the beginning induction, (56,) the tertiary current must be in an opposite direction to the secondary. For a similar reason, a current of the third order should produce in effect a single current of the fourth order, in a direction opposite to that of the current which produced it, and so on; we have here therefore a simple explanation of the extraordinary phenomenon of the alternation of the directions of the currents of the different orders, as given in this and the preceding paper. (See paragraph 25.)

81. The operation of the interposed plate, (32, 47, 48, &c.,) in neutralizing the shock, and not affecting the galvanometer, can also be readily referred to the same principles. It is certain, that an induced current is produced in the plate (No. III, 64,) and that this must re-act on the secondary, in the helix; but it should not alter the total amount of this current, since for example at the ending induction, the same quantity of current is added to the helix while the current

in the plate is decreasing, as is subtracted while the same current is increasing. To make this more clear, let the inductive actions of the interposed current be represented by the parts of the curve, Fig. 20. The induction represented by *A B* will re-act on the current in the helix, and diminish its quantity, by an amount represented by the ordinate *b B;* but the induction represented by *B C*, will act in the next moment, on the same current, and increase its quantity by an equal amount, as represented by the same ordinate *B b;* and since both actions take place within a small part of the time of a single swing of the needle, the whole deflection will not be altered, and consequently, as far as the galvanometer is concerned, the interposition of the plate will have no perceptible effect.

82. But the effect of the plate on the shock, and on the magnetization of tempered steel, should be very different; for, although the quantity of induction in the helix may not be changed, yet its intensity may be so reduced, by the adverse action of the interposed current, as to fall below that degree which enables it to penetrate the body, or overcome the coercive force of the steel. To understand how this may be, let us again refer for example to the induction which takes place at the ending of a battery current; this will produce, in both the helix and the plate, a momentary current in the direction of the primary current, which we have called *plus;* the current in the plate will re-act on the helix, and tend to produce in it two inductions, which as before may be represented by *A B*, and *B C*, of the curve, Fig. 20; the first of these, *A B*, will be an intense action, (78,) in the *minus* direction, and will therefore tend to neutralize the intense action of the primary current on the helix; the second, (*B C*,) will add to the helix an equal quantity of induced current, but of a much more feeble intensity, and hence the resulting current in the helix will not be able to penetrate the body; no shock will be perceived, or at least a very slight one, and the phenomena of screening will be exhibited.

83. When the plate of metal is placed between the con-

ductors of the second and third orders, or between those of the third and fourth, the action is somewhat different, although the general principle is the same. Let us suppose the plate interposed between the second and third conductors; then the helix, or third conductor, will be acted on by four inductions, two from the secondary current and two from the current in the plate. The direction and character of these will be as follows, on the supposition that the direction of the secondary current is itself *plus:*

The beginning secondary ..... intense and ..... *minus.*
The ending secondary ..... feeble and ..... *plus.*
The beginning interposed ..... intense and ..... *plus.*
The ending interposed ..... feeble and ..... *minus.*

Now if the action, on the third conductor, of the first and third of the above inductions be equal in intensity and quantity, they will neutralize each other; and the same will also take place with the action of the second and fourth, if they be equal, and hence in this case, neither shock nor motion of the needle of the galvanometer would be produced. If these inductions be not precisely equal, then only a partial neutralization will take place, and the shock will be merely diminished in power; and also the needle will perhaps be very slightly affected.

84. If in the foregoing exposition we throw out of consideration the actions of the feeble currents which cannot pass the body, and which consequently are not concerned in producing the shock, then the same explanation will still apply which was given in the last paper, (No. III, 94,) namely, in the above example, the helix is acted on by the minus influence of the secondary, and the plus influence of the interposed current.

85. We are now prepared to consider the effect on the helix (Fig. 3,) of the induced currents produced in the conductor of the primary current itself. These are true secondary currents, and are almost precisely the same in their action as those in the interposed plate. Let us first examine the induced currents at the beginning of the primary, in the case of a long coil and a battery of a single element.

Its action on the helix may be represented by the parts of the curve, Fig. 20. The first part, *A B*, will produce an intense induction opposite to that of the primary current; and hence the action of the two will tend to neutralize each other, and no shock, or a very feeble one, will be produced. The ending action of the same induced current, which is represented by *B D*, restores to the helix the same quantity of current electricity (but in a feeble state) which was neutralized by *A B*, and hence the needle of the galvanometer will be as much affected as if this current did not exist. These inferences perfectly agree with the experiment given in paragraph 19. In this, when the ends of the interposed coil were joined so as to neutralize the induced current in the long conductor, the shock at the beginning of the primary current was nearly as powerful as with a short conductor, while the amount of deflection of the galvanometer was unaffected by joining the ends of the same coil.

86. At first sight it might appear that any change in the apparatus which may tend to increase the induction of the primary current (16) would also tend to increase in the same degree the adverse secondary in the same conductor; and that hence the neutralization mentioned in the last paragraph would take place in all cases; but we must recollect that if a more full current be suddenly formed in a conductor of a given thickness, the adverse current will not have as much space as it were for its development, and therefore will have less power in neutralizing the induction of the primary than before. But there is another and perhaps a better reason, in the consideration that in the case of the increase of the number of elements of the battery, although the rapidity of the development of the primary current is greater, yet the increased resistance which the secondary meets with, in its motion against the action of the several elements, will tend to diminish its effect. Also by diminishing the length of the primary current, we must diminish (76) the intensity of the secondary, so that it will meet with more resistance in passing the acid of the single battery, and thus its effects be diminished.

87. The action of the secondary current in the long coil at the *ending* of the primary current, should also at first sight produce the same screening influence as the current in the interposed plate; but on reflection it will be perceived that its action in this respect must be much more feeble that that of the similar current at the beginning; the latter is produced at the moment of making contact, and hence it is propagated in a continuous circuit of conducting matter, while the other takes place at the *rupture* of the circuit, and must therefore be rendered comparatively feeble by being obliged to pass through a small portion of heated air; very little effect is therefore produced on the helix by this induction, (19.) The fact that this current is capable of giving intense shocks, when the ends of a long wire which is transmitting a primary current, are grasped at the time of breaking the circuit, is readily explained, since in this case the body forms with the conductor a closed circuit, which permits the comparatively free circulation of the induced current.

88. It will be seen that I have given a peculiar form to the beginning and ending of the curves, Figs. 17, 18, &c. These are intended to represent the variations which may be supposed to take place in the rate of increase and decrease of the quantity of the current, even in the case where the contact is made and broken with mercury. We may suppose, from the existence of analogous phenomena in magnetism, heat, &c., that the development of the current would be more rapid at first than when it approximates what may be called the state of current saturation, or when the current has reached more nearly the limit of capacity of conduction of the metal. Also, the decline of the current may be supposed to be more rapid at the first moment, than after it has lost somewhat of its intensity, or sunk more nearly to its normal state. These variations are indicated by the rapid rise of the curve, Fig. 17, from A to g, and the more gradual increase of the ordinates from h to B; and by the rapid diminution of the ordinates between C and l, and the gradual decrease of those towards the end of the curve.

89. These more minute considerations, relative to the form of the curve, will enable us to conceive, how the time of the ending of the secondary current, as we have suggested, (78,) may be prolonged beyond that of the natural subsidence of the disturbance of the electricity of the conductor on which this current depends. If the development of the primary current is produced by equal increments in equal times, as would be the case in plunging the battery (59) into the acid with a uniform velocity, then the part *A B* of the curve Fig. 17 would be a straight line, and the resulting secondary current, after the first instant, would be one of constant quantity during nearly the whole time represented by *Ac;* but if the rate of the development of the primary current be supposed to vary in accordance with the views we have given in the last paragraph, then the quantity of the secondary current will begin to decline before the termination of the induction, or as soon as the increments of the primary begin to diminish; and hence the whole time of the subsidence of the secondary will be prolonged, or the length of *b C*, Fig. 20, will be increased, the descent of *B C* be more gradual, and the intensity of the ending induction of the secondary current be diminished, (see last part of paragraph 78.)

90. Besides the considerations we have mentioned, (88,) there are others of a more obvious character, which would also appear to affect the form of particular parts of the curve. And first we might perhaps make a slight correction in the drawing of Figs. 17, 18, &c., at the point *A*, in consideration of the fact that the very first contact of the end of the conductor with the surface of the mercury is formed by a point of the metal, and hence the increment of development should be a little less rapid at the first moment than after the contact has become larger; or in other words, the curve should perhaps start a little less abruptly from the axis at the point *A*. Also, Dr. Page has stated* that he finds the shock increased by spreading a stratum of oil over the surface of the mercury; in this case it is probable that the ter-

*Silliman's American Journal of Science.

mination of the current is more sudden, on account of the prevention of the combustion of the metal by means of the oil, and the fact that the end of the conductor is drawn up into a non-conducting medium.

91. The time of the subsidence of the current, when the circuit is broken by means of a surface of mercury, is very small, and probably does not exceed the ten-thousandth part of a second, but even this is an appreciable duration, since I find that the spark at the ending presents the appearance of a band of light of considerable length, when viewed in a mirror revolving at the rate of six hundred times in a second; and I think the variations in the time of the ending of a current under different conditions may be detected by means of this instrument.

92. Before concluding this communication, I should state that I have made a number of attempts to verify the suggestion given in my last paper, (No. III, 127,) that an inverse induction is produced by a galvanic current by a change in the distance of the conductors, but without success. These attempts were made before I had adopted the views given in this section, and since then I have found (80) a more simple explanation of the alternation of the currents.

93. In this number of my Contributions, the phenomena exhibited by the galvanic apparatus have alone been discussed. I have however made a series of experiments on the induction from ordinary electricity, and the re-action of soft iron on currents; and I think that the results of these can also be referred to the simple principles adopted in this paper; but they require further examination before being submitted to the public.

ON A RECIPROCATING MOTION PRODUCED BY GALVANIC ATTRACTION AND REPULSION.

(Proceedings of the American Philosophical Society, vol. I, p. 301.)

November 20, 1840.

Prof. Henry described an apparatus for producing a reciprocating motion by the repulsion in the consecutive parts of a conductor, through which a galvanic current is passing, and made some remarks in reference to the electro-magnetic engine invented by him in 1831,* and subsequently described by Dr. Ritchie, of London. The machine referred to had been applied recently by Prof. Henry in his experiments.

ON THE EVOLUTION OF ELECTRICITY FROM STEAM, ETC.

(Proceedings of the American Philosophical Society, vol. I, pp. 322, 324.)

December 18, 1840.

[Dr. Patterson called the attention of the Society to the subject of the evolution of electricity from steam, mentioned at the last meeting, and stated that the experiments made lately in England had been successfully repeated by Mr. Peale, Mr. Saxton, and himself, at the United States Mint. - - - - He thought it most probable that the electricity, in these experiments, was evolved by the condensation of the steam. - - - -]

Prof. Henry stated that he had not seen the sparks from steam; but that he had obtained feeble electricity from a small ball, partly filled with water, and heated by a lamp. He agreed with Dr. Patterson in the opinion that the source of the electricity was the change of state,—but from water to vapor. There was however some doubt on the subject. Pouillet had denied the evolution of electricity from the evaporation of pure water. The facts were interesting, particularly on account of the great intensity of the electricity.

*[Silliman's American Journal of Science, July, 1831, vol. xx, p. 340.—*Ante*, page 54.]*

The results obtained by the philosophers, which had been mentioned, indicated electricity of very feeble tension, which could only be observed by the most delicate instruments, but here the sparks were an inch in length.

If the vaporization of the water were shown to be the source of the electricity, Prof. Henry thought the phenomena might be readily explained by the beautiful theory of Becquerel, in regard to the production of the great intensity of the electricity in the thunder cloud. According to this theory, each particle of the vapor carries up with it into the atmosphere the free electricity which it receives at the moment of the change of state: this being diffused through the whole capacity of the air is of very feeble intensity although of great quantity; but the condensation of the vapor in a cloud affords a continuous conductor, and consequently the electricity of all the particles of the interior, according to the well known principles of distribution, rushes to the surface of the cloud, and hence the great intensity of the lightning. Agreeably with this hypothesis, the insulated conductor, placed in the steam, would act not only as a collector, but also as a condenser of the free but feeble electricity of the vapor.

Prof. Henry further stated, in relation to this subject, that he had been informed by several persons, that they had obtained sparks of electricity from a coal stove during the combustion of anthracite. A case had been stated to him several years ago, which he mentioned to his friend Professor Bache, who informed him that a similar one had fallen under his own notice, in which however Prof. Bache had succeeded in tracing the electricity to the silk shirt of the person who drew the spark. Another case had lately been reported to him by an intelligent gentleman, of a stove burning bituminous coal on board of a steamboat on the Ohio, which afforded amusement to all the passengers, during the voyage, by giving sparks of electricity whenever it was touched.

In connection with the facts that had been stated of the production of electricity from steam, Prof. Henry observed that he was now inclined to believe that electricity may also

be evolved during the combustion of coal in a stove. But what (he asked) is the source of electricity in this case? Is it combustion, the evaporation of the moisture, or the friction of the hot air on the interior of the pipe?

EXPERIMENTS ON PHOSPHORESCENCE.

(Proceedings of the American Philosophical Society, vol. II, p. 46.)*

April 16, 1841.

Professor Henry mentioned that he had recently repeated some experiments of Becquerel and Biot on phosphorescence, the results of which demonstrate the existence of an emanation from incandescent bodies, particularly when in an electrical state, of a character not heretofore known. He promised to give a more full account of these at a future meeting of the Society.

[* The title-page of vol. II, (comprising the proceedings from Jan., 1841, to May, 1843,) bears date 1844.]

ON A SIMPLE FORM OF HELIOSTAT.

(Proceedings of the American Philosophical Society, vol. II, pp. 97, 98.)

September 17, 1841.

Professor Henry exhibited to the Society a simple form of the Heliostat, or instrument for throwing a stationary beam of light into a darkened room.

He stated that this article of apparatus, which is indispensable in delicate experiments on light, is in its usual form a very complex instrument and consequently very expensive, while the one to which the attention of the Society was directed is very simple, and cost scarcely more than the tenth part of the price of one of the old form.

It was made in accordance with the plan given by Dr. Thomas Young in the first volume of his Lectures on Natural Philosophy, which consists in reflecting a beam of light into the room in a line parallel to the axis of the earth, and then causing it to retain this direction by giving the reflector a rotary motion equal to the apparent motion of the sun. The instrument consists of a flat block of mahogany, about nine inches long and five inches wide, on which is placed in an inclined position, the wheel-work of a common pocket watch. This serves to give rotary motion to a brass wheel of about five inches diameter, which is so geared into the large wheel of the watch as to make one turn in twenty-four hours. The axis of this wheel is a steel rod, carrying on its upper end a small mirror, which can be set in any position by means of a universal joint. The watch-work and the wheel are attached to the mahogany block by a hinge, so that the axis of the wheel can be inclined to the horizon at an angle precisely equal to the latitude of the place where the instrument is to be used.

The adjustment of the instrument is very simple. It is placed on the outside of the window, with the axis of the wheel parallel to the axis of the earth; a meridian line having been traced on the window-sill for this purpose.

The mirror is then set so that the beam of light is thrown into the room in a line forming the prolongation of the axis of the wheel, which is readily effected by means of a mark previously made on the opposite wall. The beam will preserve this direction during the day, since the mirror and the sun revolve with the same velocity, and are therefore comparatively at rest. The only motion of the beam in reference to terrestrial objects is one of rotation on its own axis. If the required direction of the beam is different from that of the first reflection, a second mirror is used.

Professor Henry's object in exhibiting this article to the Society, was to render this simple contrivance more generally known in our country. He stated that the original invention probably belongs to Dr. Young; that it was at least published by him in 1807, although an account of the same instrument is given in the *London Philosophical Magazine* for 1833, as a new invention by Mr. Potter. The details of the instrument exhibited differ from those proposed by Mr. Potter, in the addition of a hinge and clamp-screw, by which the axis may be adjusted to the angle of the latitude. The instrument was constructed by an ingenious watch-maker at Princeton; and its whole cost, including the watch-work, was but sixteen dollars.

ON THE EFFECTS OF A THUNDER-STORM.

(Proceedings of the American Philosophical Society, vol. II, p. 111–116.)

November 5, 1841.

Professor Henry gave an account of some observations he had made on the effects of a thunder-storm which visited Princeton on the evening of the 14th of July, 1841.

Storms of this kind (he said) are not very frequent at Princeton; but two severe ones have passed immediately over the place within the last nine years, and the lightning has struck but twice in the village during that time. It is thought by some of the inhabitants that damage by lightning was more frequent some years ago than it has been

lately; and the idea has been suggested that the water of the canal, which passes to the south of the place, may have had some effect in determining the course of the cloud. Be this as it may, the thunder-storm generally comes from the south-west, and before it reaches the village it usually divides into two parts, one of which passes along the edge of the Rocky Hill, and the other along the valley of Stonybrook, so that the principal part of the storm seldom passes immediately over the village; and when it does thus pass it is generally at a great elevation, and the thunder is not so loud as that which the observer has been in the habit of hearing at the north. In connection with this remark, Professor Henry mentioned that he has several times observed the lightning assume a beautiful violet color, similar to that of the vapor of iodine, and this was particularly the case during a storm which occurred during the 12th of April, 1840. On this occasion, although the cloud and the flashes appeared directly overhead, yet the sound of the thunder seemed to come from a distance. The peculiar color may perhaps receive a sufficient explanation by referring it to the fact of the discharge taking place at a great altitude, and consequently in comparatively rarified air, as in the case of the color exhibited by the spark through a vessel partially exhausted.

The storm of the evening of the 14th of July, was said to be more severe than any which had visited Princeton for twenty years before. It commenced between 7 and 8 o'clock, and lasted about three hours: the thunder was almost continuous, but except in two or three cases it was not very near. Several buildings and other objects were struck in the vicinity of Princeton; and also Mrs. Hamilton's house, which is situated in the village, about twenty rods west of the college, on the opposite side of the way. It seemed a little surprising that this house should be singled out, since the buildings on either side are considerably higher, although at a few rods distance, and in front of the one to the west is a number of tall trees. The house is also furnished with a lightning rod; but this, like most of the rods erected in the country, is not formed in accordance with the most scientific

principles. The front of Mrs. Hamilton's house is parallel with the main street, and is nearly in an east and west direction. The building is of brick, with a shingle roof, and is two stories high; it has on the front, three upper windows, and two windows and a door below; the latter being immediately under the western upper window. The chimney is on the eastern end, and the lightning conductor is supported against this. The rod is formed of round iron, three-eighths of an inch thick, and the several parts of it are imperfectly connected by hooks and eyes. It appears to be merely thrust into the ground to the depth of about two feet, and is terminated above by three prongs instead of one, the points of which are blunted by long exposure, but do not exhibit any appearance of fusion. The top of the rod is not more than six feet above the ridge of the roof; and since the house is about thirty feet long, the farther end of the ridge is unprotected. A point, according to the experiments of Mr. Charles, can only protect a circular space, the radius of which is not greater than twice the height of the point above the plane to be protected.

The lightning, according to the accounts of several persons, came from a cloud situated to the southwest, and the discharge did not strike the most elevated part of the building, but the western end of the horizontal wooden gutter which extends along the front of the house under the eaves. This point is at the greatest possible distance from the extremity of the lightning rod, and perhaps was as near to the cloud as any other part of the building. The discharge immediately divided itself into two parts: one of these, and probably the larger, passed along the gutter, which must have been filled with water at the time, to the eastern end of the same, and then down to the earth along an ordinary tinned iron pipe or conductor, which conveys the water from the gutter to the pavement below. Marks of its passage were observed along the gutter, and particularly near the end next the metallic conductor. The other part of the discharge passed immediately downward through the end of the gutter which first received the shock, to the casing of the window

below; and was probably thus deflected out of its course by the attraction of the iron hinges and bolts of the shutters. Its course to the ground was further traced along the casings on each side of the front door. The wood was cracked at every place where a nail happened to be in the line of the discharge, and at some places the lightning appeared merely to pass along the surface making a groove in the wood of about one-eighth of an inch in width, and six or seven inches long; several of these grooves were observed on the side casings of the door. Three panes of glass were broken in the window above the door, and the pieces were thrown inward. The entrance within the door was filled with dust, and a strong sulphurous odor was preceptible for an hour or more after. No marks of a discharge were found at the foot of the lightning rod.

During the storm, several women were alone in the house, and at the time it was struck three of these were in the front room in the second story, and consequently near the line of the discharge along the gutter. Two of them were on a bed placed against the partition wall, opposite to the front, and the third one was standing on the floor about eight feet from the front window, with her face to the same. Those on the bed were unaffected; but the one on the floor stated that she felt a sensation on her right ear, as if it had been touched with a live coal; at the same time she felt a rushing sensation down her side and perceived a flash at her foot, and a forked spark in the air between her and the nearest window. One of the persons on the bed also stated that she saw the forked spark in the air, and that the one standing on the floor appeared to her for an instant as if surrounded with light. The outside shutters of the window opposite to which she was standing, were closed, and also one leaf of the shutters of the window farther east. The western window, or that from which the glass was broken, was not in the same room, but in a small adjoining one, over the main entrance from the front door. The chamber door was shut at the time, and no marks of the entrance of the electricity into the room could be found on the walls or on the casings of the two windows.

The principal facts here detailed, although perhaps not unusual occurrences, afford interesting illustrations of the action of electrical induction. First, the horizontal gutter and the vertical tin pipe, both filled with water, formed a long continuous electrical conductor, extending from the point where the lightning first struck to the lower farther corner of the front of the house; and this conductor, on account of its length, would be intensely affected by the induction of the distant cloud, or rather by that of the approaching discharge. If the electricity of the cloud, were positive, then that of the water in the nearest end of the gutter would be negative, and consequently a powerful attraction would determine the lightning on the point where it struck. The house, under these circumstances, might have been damaged even had the rod been much higher than it was, and its connection with the earth much more perfect.

Again, the phenomena exhibited to the women in the upper chamber were also most probably due to inductive action. After a proper allowance for imperfect observation, occasioned by the fright and confusion of the moment, it is still evident that the one on the floor was in some degree affected by the discharge, although none of the electricity of the cloud actually entered the room, since no traces of it were to be found on the walls or other parts. The effects may therefore be referred to the inductive action of the lightning at a distance and through the wall as it passed along the gutter across the front of the house. When a shock of electricity from a Leyden jar is passed through a slip of tinfoil pasted on one side of a pane of glass, the hand on the other side will receive a slight sensation from the lateral induction through the glass. In the same way, it may be supposed that the effects perceived by these persons were due to the disturbance for an instant of the natural electricity of the chamber by the passage of a large charge along the outside of the house.

The discharge, as has before been stated, came from the southwest, and in its passage it crossed obliquely some houses on the opposite side of the street. In one of these, two persons

were sensibly affected by the shock; and another, in a room with the windows closed, according to her own statement, saw sparks of electricity on the floor. The same explanation will also apply to these effects.

During the same storm another house,* about three miles southwest of the village, was struck, and this also was furnished with an imperfect conductor. The upper part of the rod had been broken, and it hung down, so that no part was above the chimney. The lightning struck the eastern chimney, which was on the end of the house opposite to that to which the rod was attached, and passed down the inside of the flue to the kitchen fire-place, in which wood was burning at the time. It threw down a great quantity of soot, filled the lower rooms with smoke, and diffused, according to the account, a strong smell of gunpowder.

A part of the charge passed to the outside through the thick stone wall which forms the back of the chimney, and was evidently attracted by the iron hoop of a large cask which was nearly against the wall. It made a triangular hole, as if the stone and mortar had been burst outwards by an explosive force, and this was directly opposite the nearest part of the hoop. It then descended along the cask to the ground, breaking off all the wooden hoops in its course, while those of iron were undisturbed. The house is about sixty feet long; and from the state of the rod the greater part of this distance might be considered as unprotected. The stroke fell on the end most remote from the approaching storm, and probably the lightning was drawn to this chimney rather than the other on account of the heated air which was escaping from it at the time.

Effects were also produced in this case which can only be explained on the principles of induction. Three persons, the man of the house, his wife, and son, all took refuge on a bed in a room separated from that through which the chimney passes, and upwards of twenty feet from the line of the electrical discharge. They were all lying across the bed, with their feet hanging down the side, and they each received

* The dwelling-house of Mr. Henry Philip.

a shock in the knees and lower joints of the legs. The wife stated that the feeling was precisely like that which she had experienced from a shock from an electrical jar. No marks of the entrance of any part of the discharge from the cloud were found on the plastering or any other parts of the room; the effect can therefore only be accounted for by a sudden disturbance of the equilibrium of the natural electricity of the space within the room.

The induction of an electrical cloud is often exerted at an astonishing distance. It has long been known that a delicate gold-leaf electrometer is sometimes affected by the presence of an electrical cloud immediately overhead; but Dr. Ellet, professor of chemistry in the college of South Carolina, has informed him that if one of Dr. Hare's single-leaf electrometers be furnished with a pointed metal rod attached to the cap, and then placed on the sill of an open window in the upper story, the leaf will be seen to touch the ball at the moment of a flash, although the lightning is several miles distant.

CONTRIBUTIONS TO ELECTRICITY AND MAGNETISM. No. V.

ON INDUCTION FROM ORDINARY ELECTRICITY; AND ON THE OSCILLATORY DISCHARGE.*

(Proceedings of the American Philosophical Society, vol. II, pp. 193-196.)

June 17, 1842.

Professor Henry presented the record of a series of experiments on induction from ordinary electricity, as the fifth number of his Contributions to Electricity and Magnetism. Of these experiments he gave an oral account, of which the following is the substance.

In the third number of his Contributions he had shown on this subject: 1. That the discharge of a Leyden battery through a conductor, developed in an adjoining parallel conductor an induced current, analogous to that which, under similar circumstances, is produced by a galvanic current. 2. That the direction of the induced current, as indicated by the polarity given to a steel needle, changes its sign with a change of distance of the two conductors, and also with a change in the quantity of the discharge of electricity. 3. That when the induced current is made to act on a third conductor, a second induced current is developed, which can again develop another, and so on through a series of successive inductions. 4. That when a plate of metal is interposed between any two of the consecutive conductors, the induced current is neutralized by the adverse action of a current in the plate.

The direction of the induced currents in all the author's experiments was indicated by the polarity given to steel needles enclosed in a spiral, the wire of which formed a part of the circuit. But some doubts were reasonably entertained of the true indications of the direction of a current by this means, since M. Savary had announced in 1826, that when several needles are placed at different distances above a

*[The full Memoir was not printed in the "Transactions of the Am. Philosophical Society."]

wire through which the discharge of a Leyden battery is passed, they are magnetized in different directions, and that by constantly increasing the discharge through a spiral, several reversions of the polarity of the contained needles are obtained.

It was therefore very important before attempting further advances in the discovery of the laws of the phenomena, that the results obtained by M. Savary should be carefully studied; and accordingly the first experiments of the new series relate to the repetition of them. The author first attempted to obtain them by using needles of a larger size, Nos. 3, and 4, such as he had generally employed in all his previous experiments; but although nearly a thousand needles were magnetized in the course of the experiments, he did not succeed in getting a single change in the polarity. The needles were always magnetized in a direction conformable to the direction of the electrical discharge. When however very fine needles were employed he did obtain several changes in the polarity in the case of the spiral, by merely increasing the quantity of the electricity, while the direction of the discharge remained the same.

This anomaly which has remained so long unexplained, and which at first sight appears at variance with all our theoretical ideas of the connection of electricity and magnetism, was after considerable study satisfactorily referred by the author to an action of the discharge of the Leyden jar which had never before been recognized. The discharge, whatever may be its nature, is not correctly represented (employing for simplicity the theory of Franklin) by the single transfer of an imponderable fluid from one side of the jar to the other; the phenomena require us to admit *the existence of a principal discharge in one direction, and then several reflex actions backward and forward, each more feeble than the preceding, until the equilibrium is obtained.* All the facts are shown to be in accordance with this hypothesis, and a ready explanation is afforded by it of a number of phenomena which are to be found in the older works on electricity, but which have until this time remained unexplained.

The same action is evidently connected with the induction of a current on its own conductor, in the case of an open circuit, such as that of the Leyden jar, in which the two ends of the conductor are separated by the thickness of the glass. And hence, if an induced current could be produced in this case, one should also be obtained in that of a second conductor, the ends of which are separated; and this was detected by attaching to the ends of the open circuit a quantity of insulated metal, or by connecting one end with the earth.

The next part of the research relates to a new examination of the phenomena of the change in the direction of the induced currents, with a change of distance, &c. These are shown to be due to the fact that the discharge from a jar does not produce a single induced current in one direction, but several successive currents in opposite directions. The effect on the needle is principally produced by two of these: the first is the more powerful, and in the adverse direction with that of the jar; the second is less powerful, and in the same direction with that of the jar. To explain the change of polarity, let us suppose the capacity of the needle to receive magnetism to be represented by $\pm$ 10, while the power of the first induced current to produce magnetism is represented by — 15, and that of the second by + 12; then the needle will be magnetized to saturation or to — 10, by the first induced current, and immediately afterwards all this magnetism will be neutralized by the adverse second induction, and a power of + 2 will remain; so that the polarity of the needle in this case will indicate an induced current in the same direction as that of the jar. Next, let the conductors be so far separated, or the charge so much diminished, that the power of the first current to develop magnetism may be reduced to — 8, while that of the second current is reduced to + 6, the magnetic capacity of the needle remaining the same. It is evident then that the first current will magnetize the needle to — 8, and that the second current will immediately afterwards neutralize 6 of this, and consequently the needle will retain a magnetism of — 2, or will indicate an induced current in an opposite direction to that of the jar.

In extending the researches relative to this part of the investigations, a remarkable result was obtained in regard to the distance at which inductive effects are produced by a very small quantity of electricity; a single spark from the prime conductor of the machine, of about an inch long, thrown on the end of a circuit of wire in an upper room, produced an induction sufficiently powerful to magnetize needles in a parallel circuit of wire placed in the cellar beneath, at a perpendicular distance of thirty feet with two floors and ceilings, each fourteen inches thick, intervening. The author is disposed to adopt the hypothesis of an electrical *plenum*, and from the foregoing experiment it would appear that the transfer of a single spark is sufficient to disturb perceptibly the electricity of space throughout at least a cube of 400,000 feet of capacty; and when it is considered that the magnetism of the needle is the result of the difference of two actions, it may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from a flint and steel in the case of light.

The author next alludes to a proposition which he advanced in the second number of his Contributions, namely, that the phenomena of dynamic induction may be referred to the known electrical laws, as given by the common theories of electricity; and he gives a number of experiments to illustrate the connection between statical and dynamical induction.

The last part of the series of experiments relates to induced currents from atmospheric electricity. By a very simple arrangement, needles are strongly magnetized in the author's study, even when the flash is at the distance of seven or eight miles, and when the thunder is scarcely audible. On this principle, he proposes a simple self-registering electrometer, connected with an elevated exploring rod.

EXPERIMENTS ON PHOSPHORESCENCE.

(Proceedings of the American Philosophical Society, vol. III, page 38–44.)*

May 26, 1843.

Professor Henry presented a communication "On Phosphorogenic Emanation," and illustrated by numerous diagrams the experiments which he had made on the subject.

It has long been known, that when the diamond is exposed to the direct light of the sun, and then removed to a dark place, it shines with a pale bluish light, which has received the name of phosphorescence. The effect is not peculiar to the diamond, but is common to a long list of substances, among which the sulphuret of lime (Homberg's phosphorus) is the most prominent. It is also an old fact, mentioned by Canton, that the phosphorescence is excited by exposing the substance to the light of the electrical discharge.

About three years ago, M. Becquerel, of the French Institute, repeated the experiment of Canton, and discovered the remarkable fact, that the phosphorescence is excited in a very feeble degree, or not at all, when a plate of glass or mica is interposed between the spark and the sulphuret of lime, although the effect is not apparently diminished when a plate of rock crystal or one of sulphate of lime is similarly interposed. Or in other words he found that substances equally transparent do not equally well transmit the exciting cause of the phosphorescence. Hence the old explanation of the glowing of the diamond, namely, that it is owing to the light which has been absorbed and is again given off in the dark, could no longer be admitted; and Becquerel inferred from his experiments, that the exciting cause of the

* [The title-page of vol. III, (comprising the proceedings only from May 25 to May 30,) bears date 1843. The proceedings occupying a series of special meetings held during the mornings and evenings, were appointed to commemorate the hundredth anniversary of the American Philosophical Society: and the volume containing them was published in advance of the preceding volume II.]

phosphorescence was due to an impression made on the lime by a radiation from the electrical spark, differing essentially from light, and to which he gave the name of the "phosphorogenic emanation."

Biot afterwards made a series of experiments on the "permeability" of different substances, in reference to this emanation as it exists in the beams of the sun; and still later, Matteucci, the clebrated Italian experimental philosopher, has investigated and extended the same subject. The younger Becquerel also has published a memoir on the constitution of solar spectrum, including its phosphorogenic properties. From the notices of the labors of these *savans* on this subject, as they were adverted to, it appears that all their experiments (with the exception of those before mentioned as made by M. Becquerel) were confined to the solar radiation, and consequently they do not lessen the importance of a careful examination of the properties of the same emanation, as derived from a different source, and having a different intensity.

The investigations detailed in this communication relate almost exclusively to the emanation as derived from the electrical spark. The apparatus employed in the experiments was a Leyden jar, of the capacity of about half a gallon; and this was charged each time, so as to give a spark between the rounded ends of two thick wires of about an inch in length. The sulphuret of lime was exposed to the light of the spark at different distances, in shallow leaden pans. The first experiments relate to an examination of a considerable number of substances, in regard to their permeability by the emanation. The results of these, which were given at the close of the communication, will serve to corroborate the inference of M. Becquerel, that the exciting cause of luminous appearance of the lime is not identical with ordinary light.

The next experiments are in reference to the propagation of this emanation. Two slits, of about the one-twelfth of an inch wide and an inch long, were made in two screens of sheet brass, and these slits were placed in the same plane

with the path of the spark. After the discharge, the sulphuret of lime under the opening was observed to be marked with a narrow line of light well defined at its edges, and shaded off at its ends into a penumbra; the appearance being precisely in accordance with the laws of a radiation in straight lines from a narrow line of emanation.

Experiments were next made to determine whether the radiation of the emanation takes place with the same intensity from every point of the length of the spark, or whether it is confined to the two extremities, or the poles of the discharging wires. For this purpose the slits were turned at right angles to their former position, so that the emanation could only reach the lime from a single point of the spark. The experiments with this arrangement showed that the radiation is from each point of the line of the spark, but that it is much more intense from the two extremities. This curious result was verified by another arrangement, which allowed the impressions from different points of the spark to be at once compared with each other. Three slips were cut in a thick plate of mica, and this was placed immediately above the line, so that one of the slits was directly under the end of each wire, and the other midway between the other two. When the discharge was passed over the plate, the lime under the middle slit exhibited a feeble phosphorescence for two or three seconds, and then became dark, while that under the slits at the end of the spark continued to glow for more than a minute. This effect did not appear to be due to the diffusion of the spark at the middle of its course, since the discharge was from a Leyden jar, and the spark as is usual in this case, appeared as a single line of light, of the same intensity and width throughout its whole length.

The phosphorescence was excited at a much greater distance than was at first thought possible. In a perfectly dark room, the light was observed for a few moments when the pan containing the lime was removed to the distance of ten feet from the point of discharge. The intensity of the light and the time of continuance however diminished very rapidly with an increase of distance.

To determine whether the emanation obeys the laws of the reflection of light, a piece of common looking-glass was so arranged with the path of the spark, the slits in the screens, and the pan of lime, that the angle of reflection could be compared with the angle of incidence: but with this arrangement no impression on the lime could be obtained; the want of permeability in the glass apparently preventing any reflection from the silvered side of the mirror. A plate of polished black glass was next used, so as to get the reflection from the anterior surface: the result however was of the same negative character as before. It would therefore appear, that glass neither reflects nor transmits the phosphorogenic emanation, except in a very small degree. When a metallic mirror was employed, a well defined line of light was impressed on the lime from the reflected emanation, and from the position of this it was found that the two angles were equal.

The refraction and dispersion of the emanation were readily obtained, by employing for the purpose a prism of rock salt, instead of one of glass. The dispersion was shown by the conversion of the narrow line of light, by means of the prism, into a broad band.

The next question was in reference to the polarization of the phosphorogenic emanation; and in obtaining a satisfactory answer to this, several difficulties were encountered. Attempts were first made to polarize the beam by passing it through tourmaline; but it was found that this substance is less permeable to the emanation than even glass or mica. Nicol's polarizing prisms were next employed, but no impression could be made on the lime through two of them; and since the emanation is not reflected by glass, and the polarization from polished metal is very feeble, these substances could not be employed in the process. At length an indirect method was adopted which gave positive results. This was founded on an experiment of Melloni, in his interesting researches on radiant heat. A pile of exceedingly thin plates of mica, prepared according to the method of Professor Forbes, of Edinburgh, was placed between the spark

and the pan containing the lime, with its plane at right angles to the line joining the middle of the two. In this position of the pile, no impression was made on the lime by the electrical discharge; but when the plane of the pile was inclined to the line just mentioned, so as to form with it the polarizing angle, a luminous spot was excited.

By this change of the position of the pile, the thickness of the path to be traversed by the phosphorogenic beam was considerably lengthened; and yet the permeability was much increased. This remarkable result could only be the effect of the successive polarization of the several parts of the beam as they passed the several films of mica, and were thus prepared for a more ready transmission by the succeeding films.

After the emanation was found to be polarizable, it was important to determine if the intensity of the action on the lime would be different, in case the beam were transmitted through crystals in different directions in reference to their optical axis, but no difference could be observed, when the beam was passed through crystals of carbonate of lime, and of quartz—parallel, and perpendicular to the axis.

From the foregoing results it is evident that the exciting cause of the phosphorescence of the sulphuret of lime is an emanation possessing the mechanical properties of light, and yet so different in other respects as to prove the want of identity. That the same emanation also differs from heat is manifest from the fact that the lime becomes as luminous under a plate of alum as under a plate of rock salt, although these substances are almost entirely opposite in their property of transmitting heat.

Some experiments were also made to compare the phosphorogenic emanation with the chemical radiation. For this purpose a sensitive daguerreotype plate and a pan of sulphuret of lime were exposed together to the light of the sky for five seconds. The plate by this exposure was marked with a photograpic impression, but little or no effect was produced on the lime. Another sensitive plate and the same pan of lime were similarly exposed to the light of an electrical discharge; the lime was now observed to glow, while

no impression was produced on the plate. When however the plate was exposed very near to a succession of sparks, continued for ten minutes, with a plate of mica interposed, an impression was made.

The sulphuret of lime was also exposed for several minutes to the direct light of the full moon, without any phosphorescent effect. A sensitive plate, similarly exposed, according to the statement of Dr. Draper, receives a photographic impression. These experiments, although not sufficiently extensive, appear to indicate that the phosphorogenic emanation is distinct from the chemical, and that it exists in a much greater quantity in the electrical spark than either the luminous or the chemical emanation.

Professor Henry remarked that in considering these emanations as distinct, he had reference only to the classification of the phenomena, for if they be viewed in accordance with the undulatory hypothesis they may all be considered as the results of waves, differing in length and amplitude, and possibly also slightly differing in the direction of vibration.

The phosphorescence of the lime may also be excited by exposure to the light of a burning coal, and in this case the emanation is also screened by a plate of mica. It was also found that the magneto-electrical spark from a surface of mercury excites the luminous condition of the sulphuret, and it has long been known that heat, applied to the bottom of the vessel containing the article, produces the same effect.

To determine whether the phosphorescence could be excited by electro-dynamic induction, a quantity of the sulphuret was placed between two plates of quartz, and a covered copper wire was wound around the whole, so that the lime occupied the axis of a spiral. But when a discharge of electricity was passed through the wire the lime gave no indications of phosphorescence; the same negative result was also obtained when the sparks were passed through the bottom of the leaden pan.

It has been supposed that the phosphorescence of the lime is due to the disturbance of the electricity of the mass of the substance, and the continuance of the light to the subse-

quently slow restoration of the equilibrium. The result however of the following experiment would seem to be at variance with this explanation. The lime was thrown into a tumbler of water, and sank to the bottom, but in this situation, when the spark was passed over the surface of the liquid, it became as luminous and the effect appeared to remain as long as when the exposure took place in the air.

The author stated that some of the experiments described by him can be repeated with common chalk, although it is not as sensitive as the sulphuret of lime. Some pieces of it however become luminous at a considerable distance, and it is not improbable that the chalk cliffs of England are sometimes rendered phosphorescent by flashes of lightning during a thunder storm.

But the substance which gives the most brilliant light, although the light does not continue so long, and is not as easily excited as that from the lime, is the sulphate of potassa. When exposed to the discharge of a jar highly charged, at the distance of a few inches below the spark, it glows for a few seconds with a beautiful azure light; and as this salt is not readily acted on by liquids, it was used to determine the permeability of different substances, by placing a crystal of the salt in the liquid to be tested.

It has long been known that the sulphate of potassa often emits flashes of light during the progress of its crystallization; and it is probable that other substances, which are known to emit light under the same circumstances, may also be rendered phosphorescent at a distance by the electrical emanation.

The following is a list of the substances which have been examined by Professor Henry, with reference to their permeability by the phosphorogenic emanation:

TRANSPARENT SOLIDS.

Permeable.

Ice,	Sulphate of baryta,
Sulphate of lime,	Sulphate of potassa,
Quartz,	Sulphate of soda,

TRANSPARENT SOLIDS. (CONTINUED.)

Permeable.

Borax,
Citric acid,
Rochelle salt,
Common salt,
Alum,
Horn (pellucid),
Wax, do.

Imperfectly permeable.

Tourmaline,
Mica,
Flint glass,
Crown glass,
Saltpetre,
Tartaric acid,
Hyposulphate of soda,
Copal,
Camphor.

TRANSPARENT LIQUIDS.

Permeable.

Water,
Solution of alum,
Solution of ammonia,
Sulphate of magnesia,
Nitrate of ammonia,
And all weak solutions.

Imperfectly permeable.

Muriatic acid,
Sulphuric acid,
Nitric acid,
Phosphoric acid,
Sulphate of zinc,
Sulphate of lead,
Acetate of zinc,
Arsenious acid,
Ammonia,
Spirits of turpentine,
Alcohol,
Ether,
Oil of aniseed,
Acetate of lead.

ON A METHOD OF DETERMINING THE VELOCITY OF PROJECTILES.

(Proceedings of the American Philosophical Society, vol. III, pp. 165–167.)*

May 30, 1843.

Professor Henry read a communication "On a New Method of determining the Velocity of Projectiles."

The new method proposed by the author, consists in applying the instantaneous transmission of an electrical action, to determine the time of the passage of the ball between two screens, placed at a short distance from each other, in the path of the projectile. For this purpose the observer is provided with a revolving cylinder, moved by clock-work at the rate of at least ten turns in a second; and of which the convex surface is divided into a hundred equal parts, each part therefore indicating in the revolution the thousandths part of a second. Close to the surface of this cylinder which revolves horizontally, are placed two galvanometers, one at each extremity of a diameter, the needles of these being furnished at one end with a pen for making a dot with printers' ink on the revolving surface.

To give motion to the needles at the proper moment, each galvanometer is made to form a part of the circuit of a galvanic current, which is completed by a long copper wire passing to one of the screens, and crossing it several times, so as to form a grating, through which the ball cannot pass without breaking the wire, and thus stopping the current. During the continuance of the galvanic action, the marking end of the needle is turned from the revolving cylinder a few degrees, and pressed immovably against a "steady pin" by the well known deflecting power of the electrical current; but the moment the current is stopped by the breaking of the long conductor, in the passage of the ball through the screen, the marking end of the needle is projected against the cylinder by the action of a fine spiral spring, similar to

*[Re-printed in Walker's Electrical Magazine, 1845, vol. I, pp. 350–352.]

the hair spring of a watch, coiled around the centre pin which supports the needle, and having an elastic force a little less than the deflecting power of the electrical current. The relative position of the dots thus formed gives the time of the passage of the ball through the space between the screens, and indicates the velocity at this part of the course.

The degree of deflection of the needle can be increased or diminished by turning a screw, which alters the position of the "steady pin," and the tension of the spiral spring can also be changed by an arrangement like that of the regulator of a watch.

In order that the position of the dots on the surface of the cylinder may exactly indicate the required interval of time, it is necessary that the time occupied by each needle in starting from rest and moving across the small arc to strike against the cylinder, should be precisely equal. If this be not the case, then the difference of these times will be the error of the instruments. This must however be exceedingly small, since the whole range of the end of the needle need not be more than the one-twentieth of an inch—and the precise amount of error can readily be determined by experiment.

To adjust the apparatus for use, the galvanometers must be so placed that the two dots may be impressed on the cylinder, diametrically opposite each other when the instrument is at rest. The cylinder being then put in motion, the two circuits of long wire are placed together, so that they can be broken at the same instant by lifting a wire common to both from a cup of mercury. If, after breaking the circuits, the dots are still found in the same relative position, no further adjustment or correction will be required: but if this is not the case, then the springs may be altered until the dots are found in their proper positions; or the difference may be noted, and this constantly applied in each actual experiment as an index error.

To prevent the dot from the first galvanometer being confounded with that from the second, the two instruments are placed one below the other in different horizontal planes.

In order that the pen may not describe a line on the cylinder, re-entering into itself, and thus obliterate the dot first impressed, it may be found necessary to give the cylinder a slow ascending motion, so that a spiral instead of a circle would be marked on its surface. A chronometer for measuring minute portions of time, with a motion of this kind is described in Young's *Natural Philosophy*, vol. I, page 191.

To prevent agitations of the air, the whirling apparatus with the galvanometer may be placed in the vacuum of an air pump; and that part of the conducting wire which crosses the screen may be separated at each crossing, the ends being again united by slightly twisting them together, and the conduction being preserved by proper amalgamation, so that the force necessary to break the circuit may not sensibly lessen the velocity of the ball.

Various other methods may be devised for impressing a mark on the revolving cylinder, at the moment of the rupture of the galvanic current by the passage of the ball through the screen. But the following, which has suggested itself to Professor Henry since the meeting of the Society, and has been communicated by him to the Reporter, may be regarded as among the best. It dispenses with the galvanometers, and produces the mark by a direct electrical action.

A part of the long wire which leads to the screen is coiled around a bundle of soft iron wire; and over this is coiled another long wire, so as to produce an intense secondary current, on the principle of the common coil machine. One extremity of the secondary circuit is connected with the axis of the cylinder, and the other is made to terminate almost in contact with the revolving surface, which in this modification of the instrument is surrounded by a ruled or graduated paper. It is obvious that the secondary current which is induced by the interruption of the primary circuit, will pierce or mark the paper band at the moment of the screen being broken. There is no difficulty in effecting

such a current of sufficient intensity to mark the paper, since in some of his experiments on induction, he has developed one which gave a spark between a point and a surface of nearly a fourth of an inch in length.

The terminal points of the wires from the two screens may be placed very near each other in the same horizontal plane: if then the cylinder revolving horizontally has at the same time a slow ascending motion, the relative position of the dots on the paper will give the number of whole turns and parts of a turn, made by the cylinder while the ball is passing between the two screens. In the same way the terminal points of wires from a number of different pairs of screens may be made to impress their marks on the surface of the same cylinder, and the velocity of the ball at the different points of its path may in this way be determined by a single experiment.

ON THE APPLICATION OF THE THERMO-GALVANOMETER TO METEOROLOGY, ETC.

(Proceedings of the American Philosophical Society, vol. IV, pp. 22, 23.)*

November 3, 1843.

Professor Henry made an oral communication in regard to the application of Melloni's thermo-electric apparatus to meteorological purposes, and explained a modification of the parts connected with the pile, to which he had been led in the course of his researches. He had found the vapors near the horizon powerful reflectors of heat; but in the case of a distant thunderstorm he had found that the cloud was colder than the adjacent blue space.

---

* [The title-page of vol. IV, (comprising the proceedings from June, 1843, to December, 1847,) bears date 1847.]

Theory of the discharge of the Leyden jar.

Referring to the theory of the discharge of the Leyden jar, which he had submitted to the Society some time since,* Professor Henry examined some apparent objections to it, resulting from the researches of Matteucci. The effect produced on the galvanometer by the discharge of a battery is due to the retardation of the lesser waves of electricity, a fact which indicates the cause of Matteucci's results, when a card was pierced by the currents induced in a neighboring wire conductor forming an open circuit.

The speaker described several experiments on the direct and return stroke, showing that equilibrium was restored by the same succession of oscillations; large and small needles placed in spirals forming part of an electrical circuit, being magnetized in different directions. The disturbance of the electrical *plenum* by a discharge of electricity was referred to as explanatory of the induction which takes place, and the subject was applied to the explanation of various phenomena; among others, the light appearing in well authenticated cases about persons and objects in the neighborhood of a discharge of lightning in its direct passage, and suggestions were made as to the most effectual mode of protecting powder houses, etc., from the effects of lightning.

Professor Henry examined in the same connection whether currents or ordinary electricity pass actually at the surface, or like galvanic electricity, through the mass of the conductor, and he concluded that the law of conduction developed by Ohm cannot apply to the case of surface passages, as these are indicative of ordinary electricity.

* Contributions No. V, June 17, 1842.

ON THE COHESION OF LIQUIDS.

(Proceedings of the American Philosophical Society, vol. IV, pp. 56, 57.)
April 5, 1844.

Professor Henry made a verbal communication relative to "the cohesion of liquids."

He stated that very erroneous ideas are given as to the constitution of matter in the ordinary books on Natural Philosophy. The passage of a body from a solid to a liquid state is generally attributed to the neutralization of the attraction of cohesion by the repulsion of the increased quantity of heat, the liquid being supposed to retain a small portion of its original attraction, which is shown by the force necessary to separate a surface of water from water, in the well known experiment of a plate suspended from a scale beam over a vessel of the liquid. It is however more in accordance with all the phenomena of cohesion to suppose, instead of the attraction of the liquid being neutralized by the heat, that the effect of this agent is merely to neutralize the polarity of the molecules so as to give them perfect freedom of motion around every imaginable axis. The small amount of cohesion (53 grains to the square inch) exhibited in the foregoing experiment, is due, according to the theory of capillarity of Young and Poisson, to the tension of the exterior film of the surface of water drawn up by the elevation of the plate. This film gives way first, and the strain is thrown on an inner film, which in turn is ruptured, and so on until the plate is entirely separated, the whole effect being similar to that of tearing water apart atom by atom.

Reflecting on this subject, he had thought that a more correct idea of the magnitude of the molecular attraction might be obtained by studying the tenacity of a more viscid liquid than water. For this purpose he had recourse to soap-water, and attempted to measure the tenacity of this liquid by means of weighing the quantity of water which adhered to a bubble of this substance just before it burst, and by determining the thickness of the film from an obser-

vation of the color it exhibited in comparison with Newton's scale of thin plates. Although experiments of this kind could only furnish approximate results, yet they showed that the molecular attraction of water for water, instead of being only about 53 grains to the square inch, is really several hundred pounds, and is probably equal to that of the attraction between the molecules of ice. The effect of dissolving the soap in the water is not (as might at first appear,) to increase the molecular attraction, but to diminish the mobility of the molecules, and thus to render the liquid more viscid.

ON THE COHESION OF LIQUIDS. (CONTINUED.)

(Proceedings of the American Philosophical Society, vol. IV, pp. 84, 85.)

May 17, 1844.

Professor Henry made a second communication on the subject of cohesion.

He had prosecuted his experiments on the soap-bubble to a further extent, and had arrived at a number of results which appeared to him of some interest in reference to capillarity, a subject which had given rise to a greater diversity of opinion than any other part of natural philosophy. As an evidence of its present unsettled state, he mentioned the fact that the last edition of the *Encyclopædia Britannica* contained two articles on this subject under different names; one by Dr. Young, and the other by Mr. Ivory, which explain the phenomena on entirely different physical principles.

According to the theory of Young and Poisson, many of the phenomena of liquid cohesion, and all those of capillarity, are due to a contractile force existing at the free surface of the liquid, and which tends in all cases to urge the liquid in the direction of the radius of curvature towards the centre, with a force inversely as this radius. According to this theory the spherical form of a dew drop is not the effect of the attraction of each molecule of the water on every

other, as in the action of gravitation in producing the globular form of the planets, (since the attraction of cohesion extends only to an unappreciable distance,) but is due to the contractile force which tends constantly to enclose the given quantity of water within the smallest surface, namely that of a sphere. Professor Henry finds a contractile force perfectly similar to that assumed by this theory in the surface of the soap-bubble; indeed, the bubble may be considered a drop of water with the internal liquid removed, and its place supplied by air. The spherical form in the two cases is produced by the operation of the same cause. The contractile force in the surface of the bubble is easily shown by blowing a large bubble on the end of a wide tube, say an inch in diameter; as soon as the mouth is removed, the bubble will be seen to diminish rapidly, and at the same time quite a forcible current of air will be blown through the tube against the face. This effect is not due to the ascent of the heated air from the lungs with which the bubble was inflated, for the same effect is produced by inflating with cold air, and also when the bubble is held perpendicularly above the face, so that the current is downward.

Many experiments were made to determine the amount of this force, by blowing a bubble on the larger end of a glass tube in the form of the letter **U**, and partially filled with water; the contractile force of the bubble, transmitted through the enclosed air, forced down the water in the larger leg of the tube, and caused it to rise in the smaller. The difference of level observed by means of a microscope, gave the force in grains per square inch, derived from the known pressure of a given height of water. The thickness of the film of soap water which formed the envelope of the bubble, was estimated as before by the color exhibited just before bursting. The results of these experiments agree with those of weighing the bubble, in giving a great intensity to the molecular attraction of the liquid, equal at least to several hundred pounds to the square inch. Several other methods were employed to measure the tenacity of the film, the general results of which were the same; the

numerical details of these are reserved however until the experiments can be repeated with a more delicate balance.

The comparative cohesion of pure water and soap water was determined by the weight necessary to detach the same plate from each; and in all cases the pure water required the greater force. The want of permanency in the bubble of pure water is therefore not due to feeble attraction, but to the perfect mobility of the molecules, which causes the equilibrium, as in the case of the arch without friction of parts, to be destroyed by the slightest extraneous force.

Several other experiments with films of soap water were also described, which afford striking illustrations of the principles of capillarity, and which apparently have an important bearing on the whole subject of cohesion.*

ON THE ORIGIN AND CLASSIFICATION OF THE NATURAL MOTORS.

(Proceedings of the American Philosophical Society, vol. IV, pp. 127–129.)

December 20, 1844.

Professor Henry made an oral communication in regard to some speculations in which he had indulged relative to the classification and origin of mechanical power.

He stated that he was indebted for the origin of this train of thought to some remarks made by Mr. Babbage in his work on the economy of machinery, and to the late researches of the German and French chemists on the subject of vital chemistry; indeed all the views contained in the communication might perhaps be found in detached portions in different works, but he believed that they had never before been brought together and presented as a whole.

He defined mechanical power to be that which is capable of overcoming a constant resistance, and of producing a continued motion; or in the language of the engineer, it is that which can be employed to "do work." It is here used in a

*[Reprinted in Silliman's American Journal of Science, October, 1844. Vol. XLVIII, pp. 215–217. Also in the London and Edinburgh Philosophical Magazine, June, 1845. Vol. XXVI, pp. 541–543.]

more restricted sense than force, which is applied as a more general term, to whatever tends to produce or resist motion. The following list of mechanical powers he believed would be found to include all the prime movers employed at the present time, either directly or indirectly, in producing mechanical changes in matter, and all these could be referred to two sources:

Class 1st	Water power Tide power Wind power	Referable to celestial disturbance.
Class 2d	Steam and other powers developed by combustion. Animal power.	Referable to that which is called vital or organic action.

These natural motive principles are not always directly employed in producing work, but are sometimes used to develop other power by disturbing the natural equilibrium of other forces, and in this way they give rise to a class of mechanical motors which may be called intermediate powers. It will be evident on a litile reflection that the forces of gravity, cohesion and chemical attraction, with those of the "imponderable" agents of nature, so far as they belong to the earth, all tend to produce a state of stable or permanent equilibrium at the surface of our planet—that in all cases before the energies of these forces can be exhibited, the disturbing effect of some extraneous force is required, hence these principles in themselves are not the primary sources of power, but are merely secondary agents in producing mechanical effects, or in other words it will be found that while the approximate source of every power is the force exerted by matter in its passage from an unstable to a stable state of equilibrium, yet in all cases it may be referred beyond this to a force which disturbed a previously existing quiescence. As an example we may take the case of water power, in which the mechanical effects are approximately due to the return of the water to a state of stable equilibrium on the surface of the ocean, but the cause of the continued motion is the force which produced the original disturbance, and which elevates the liquid in the form of vapor. Also in the

phenomena of combustion the immediate source of the power evolved in the form of heat, is the passage from an unstable state into one of stable combination of the carbon and hydrogen of the fuel and the oxygen of the atmosphere, but this power may ultimately be resolved into the force which caused the separation of these elements from their previous combination in the state of carbonic acid and water.

Now the only forces of any importance which operate at the surface of the earth to counteract the tendency to a general state of stable equilibrium are those derived from two sources, namely, *celestial disturbance*, and what is called *vital action;* and hence all mechanical power, as well as all activity on the surface of the globe, may be referred to these two sources. The only exception to this generalization is the comparatively limited effect of volcanic action, which is a power, from whatever source it may be derived, that must tend to exhaust itself.

Thus far the author considered his conclusions founded on well-established physical law, and perhaps here the true spirit of inductive philosophy would admonish him to stop; but they who are disposed to continue the speculation, and to consider the results of the late researches of the German and French chemists as well-established truths, may extend the generalization so as to reduce all mechanical motion on the surface of the earth to a source from without. Thus, according to Liebig, Dumas and Boussingault, the mechanical power exerted by animals is due to the passage of organized matter in the body from an unstable to a stable equilibrium; and as this matter is derived in an unstable state from vegetables, and the elements of these again from the atmosphere, it would therefore appear to follow, that animal power is referable to the same sources as that from the combustion of fuels, namely, the original force which separates the elements of the plants from their stable and original combination with the oxygen of the atmosphere. But what is this power which furnishes the plant with the material of its growth? Is it due to a constantly created vital power; or since its effects are never directly exhibited but in the presence of light, may

not the opinion of many chemists of the present day be adopted, namely, that it is due to the decomposing energy of the sun's rays, which are found to exhibit a wonderful decomposing effect in cases where no vital phenomena are present.

If this hypothesis be adopted, it must be supposed that vitality is that mysterious principle which propagates a form and arranges the atoms of organizable matter, while the power with which it operates, as well as that developed by the burning fuel and the moving animal, is a separate force, derived from the divellent power of the sunbeam. It is true that this is as yet little more than a mere hypothesis, and as such forms no part of positive science, but it appears to be founded on a clear physical analogy, and may therefore form the basis of definite philosophical research.

ON A PECULIAR ACTION OF FIRE ON IRON NAILS.

(Proceedings of the American Philosophical Society, vol. IV, p. 173.)

June 20, 1845.

[Dr Patterson exhibited a mass of nails melted together at the fire in Pittsburg, presenting a series of united tubes.]

Professor Henry stated that he had received a similar mass from the New York fire, and found that the action of the fire had changed the nails to a certain depth, leaving a core unchanged, which had afterwards fallen or been drawn out, leaving the hollow tube.

OBSERVATIONS ON THE RELATIVE RADIATION OF THE SOLAR SPOTS.

(Proceedings of the American Philosophical Society, vol. IV, pp. 173–176.)

June 20, 1845.

Professor Henry made a verbal communication of a series of experiments made by himself and Professor Alexander, relative to the spots on the sun.

His attention was directed to the subject by an article in the September number of the *Annales de Chimie*, by M. Gautier, upon the influence of the spots on the sun on terrestrial temperature. It is well known that Sir William Herschel entertained the idea that the appearance of solar spots was connected with a more copious emission of heat, and that the seasons during which they were most abundant were most fruitful in vegetable productions, and pursuing this idea he was led to trace an analogy between the price of corn and the number of solar spots during several successive periods. The result of this investigation, so far as it was extended, seemed to favor the views of this distinguished philosopher. A mode of investigation of this kind however is not susceptible of any great degree of accuracy; the price of corn is subject to so many other causes of variation besides that of solar temperature that little reliance can be placed on this condition alone.

M. Gautier has attempted to investigate the influence of solar spots on terrestrial temperature, by comparing the temperature of several places on the earth's surface during the years in which the spots were most abundant with those in which the smallest number were perceptible. From all the observations collected it seems to be indicated that during the years in which the spots were the greatest in number the heat had been a trifle less; but the results are far from being sufficiently definite to settle the question; and M. Gautier remarks that a greater number of years of observation at a greater number of stations will be necessary to establish a permanent connection between these phenomena.

The idea occurred to Professor Henry that much interesting information relative to the sun might be derived from the application of a thermo-electric apparatus to a picture of the solar disc produced by a telescope on a screen in a dark room. This idea was communicated to Professor Alexander, who readily joined in the plan for reducing it to practice. It was agreed that they should first attempt to settle the question of the relative heat of the spots as compared with the surrounding luminous portions of the sun's disc. The first experiments were made on the 4th of January, 1845. Mr. Alexander had observed a few days previous a very large spot, more than 10,000 miles in diameter, near the middle of the disc. To produce the image of this spot a telescope of four inches aperture and four and a half feet focus was placed in the window of a dark room with a screen behind it, on which the image of the spot was received. The instrument was placed behind the screen with the end slightly projecting through a hole made for the purpose, and a small motion of the telescope was sufficient to throw the image of the spot off or on the end of the pile. The spot was very clearly defined, and might have been readily daguerreotyped had the telescope been furnished with an equatorial movement. The form of the penumbra of the spot as it appeared on the screen was that of an irregular oblong about two inches in one direction and an inch and a half in the other. The dark central spot within the penumbra

was nearly square, of about three-fourths of an inch on the side, and a little larger than the end of the thermo-pile.

The method of observation consisted in first placing, for example, a portion of the picture of the luminous surface of the sun in connection with the face of the pile, and after noting the indication of the needle of the galvanometer the telescope was then slightly moved so as to place the dark part of the spot directly on the face of the pile, the indication of the needle being again noted. In the next set of experiments the order was reversed, the picture of the spot at the beginning was placed in connection with the pile and afterwards a new part of the luminous portion of the disc was made to occupy the same place.

The thermo-electrical apparatus used in these experiments, was made by Ruhmkorff, of Paris; and in order to render the galvanometer more sensitive, two bar magnets, arranged in the form of the legs of a pair of dividers, were placed with the opening downward, in a vertical plane, above the needle, so that by increasing or diminishing the angle the directive power of the needle could be increased or diminished, and consequently the sensibility of the instrument could be varied, and the zero point changed at pleasure.

In the present experiments, in order to mark more definitely the difference in temperature, after the needle had been deflected by the heat of the sun, the magnetic bars above mentioned were so arranged as to repel it back to near zero point, so that it might in this position receive the maximum effect of any variation in the electrical current.

Twelve sets of observations were made on the first day, all of which, except one, gave the same indication, namely, that *the spot emitted less heat than the surrounding parts of the luminous disc.* The following is a copy of the record made at the time of observation. The degrees are those marked on the card of the galvanometer and are of course arbitrary:

Spot, 3° ¼.	Sun, 5° ¼.
Sun, 4° ½.	Spot, 4°.
Sun, 3°.	Spot, 4° ½.
Spot, 1° ¾.	Sun, 5°.

Spot, 2°.	Sun, 4° ½.
Sun, 3°.	Spot, 3° ¾.
Sun, 2° ½.	Sun, 2°.
Spot, 2°.	Spot, 3° ¼. *
Spot, 2°.	Spot, 0° ¾.
Sun, 2° ¼.	Sun, 2° ½.
Spot, 4° ¾.	Sun, 1° ¼.
Sun, 5°.	Spot, 0°.

The change in the temperature during the intervals of observation, is due to the variations in the temperature of the room differently affecting the two extremities of the pile.

In consequence of cloudy weather, another set of observations was not obtained until the 10th of January, and at this time the spot had very much changed its appearance; the penumbra, while it retained its dimensions in one direction, was much narrowed in the other, and the dark part was separated into two small ones; also the sky was not perfectly clear and therefore the results were not as satisfactory as those of the previous observations; the indications were however the same as in the other sets, exhibiting a less degree of heat from the spots.

Cloudy weather prevented other observations on the heat of different parts of the sun, particularly a comparison between the temperature of the centre and the circumference of the disc, which would have an important bearing on the question of an atmosphere of the sun. The observations will be continued, and any results of interest which may be obtained, will be communicated to the Society.

* At this observation a slight cloud probably passed over the sun's disc.

ON THE CAPILLARITY OF METALS.

(Proceedings of the American Philosophical Society, vol. IV, page 176–178.)

June 20, 1845.

Professor Henry gave an account of some observations he had made on capillarity, in addition to those he had before communicated to the Society on the same subject.

In 1839* he presented the results of some experiments on the permeability of lead to mercury; and subsequent observation had led him to believe that the same property was possessed by other metals in reference to each other. His first attempt to verify this conjecture was made with the assistance of Dr. Patterson, at the United States Mint. For this purpose a small globule of gold was placed on a plate of sheet iron, and submitted to the heat of an assaying furnace; but the experiment was unsuccessful; for although the gold was heated much above melting point it exhibited no signs of sinking into the pores of the iron. The idea afterward suggested itself that a different result would have been obtained had the two metals been made to adhere previous to heating, so that no oxide could have been formed between the surfaces. In accordance with this view he inquired of Mr. Cornelius, of Philadelphia, if in the course of his experience in working silver-plated copper in his extensive manufactory of lamps he had ever observed the silver to disappear from the copper when the metal was heated. The answer was that the silver always disappears when the plate is heated above a certain temperature, leaving a surface of copper exposed; and that it was generally believed by the workmen that the silver evaporates at this temperature.

Professor Henry suggested that the silver, instead of evaporating, merely sunk into the pores of the copper, and that by carefully removing the surface of the latter, by the action of an acid the silver would re-appear. To verify this by ex-

*[Proc. Am. Phil. Soc. vol. I, p. 82. See *ante*, page 146.]

periment, Mr. Cornelius heated one end of a piece of thick plated copper to nearly the melting point of the metal; the silver at this end disappeared, and when the metal was cleaned by a solution of dilute sulphuric acid, the end which had been heated presented a uniform surface of copper, whilst the other end exhibited its proper coating of silver. The unsilvered end of the plate was next placed, for a few minutes, in a solution of muriate of zinc, by which the exterior surface of copper was removed, and the surface of silver was again exposed. This method of recovering the silver (before the process of plating silver by galvanism came into use) would have been of much value to manufacturers of plated ware, since it often happened that valuable articles were spoiled, in the process of soldering, by heating them to a degree at which silver disappears.

It is well known to the jeweller that articles of copper, plated with gold, lose their brilliancy after a time, and that it can be restored by boiling them in ammonia; this effect is probably produced by the ammonia acting on the copper, and dissolving off its surface so as to expose the gold, which, by diffusion, has entered into the copper.

A slow diffusion of one metal through another probably takes place in cases of alloys. Silver coins, after having lain long in the earth, have been found covered with a salt of copper. This may be explained by supposing that the alloy of copper, at the surface of the coin, enters into combination with the carbonic acid of the soil, and being thus removed, its place is supplied by a diffusion from within; and in this way it is not improbable that a considerable portion of the alloy may be exhausted in the process of time; and the purity of the coin be considerably increased.

Perhaps also the phenomenon of what is called *segregation*, or the formation of nodules of flint in masses of carbonated lime, and of indurated marl in beds of clay, may be explained on the same principle. In breaking up these masses it is almost always observed that a piece of shell, or some extraneous matter, occupies the middle, and probably formed the nucleus around which the matter was accumulated by attrac-

tion. The difficulty consists in explaining how the attraction of cohesion, which becomes insensible at sensible distances, should produce this effect. To explain this let us suppose two substances uniformly diffused through each other by a slight mutual attraction, as in the case of a lump of sugar dissolved in a large quantity of water, every particle of the water will attract to itself its proportion of sugar, and the whole will be in a state of equilibrium. If the diffusion at its commencement had been assisted by heat, and this cause of the separation of the homogeneous particles no longer existed, the diffusion might be one of unstable equilibrium; and the slightest extraneous force, such as the attraction of a minute piece of shell, might serve to disturb the quiescence, and draw to itself the diffused particles which were immediately contiguous to it. This would leave a vacuum of the atoms around the attracting mass, for example, as in the case of the sugar, there would be a portion of the water around the nucleus deprived of sugar; this portion of the water would attract its portion of sugar from the layer without, and into this layer the sugar from the layer next without would be diffused, and so on until, through all the water, the remaining sugar would be uniformly diffused. The process would continue to be repeated, by the nucleus again attracting a portion of the sugar from the water immediately around it, and so on until a considerable accumulation would be formed around the foreign substance.

We can in this way conceive of the manner by which the molecular action, which is insensible at perceptible distances, may produce results which would appear to be the effect of attraction acting at a distance.

ON THE PROTECTION OF HOUSES FROM LIGHTNING.

(Proceedings of the American Philosophical Society, vol. IV, pp. 179, 180.)

June, 20, 1845.

Professor Henry made a communication relative to a simple method of protecting from lightning buildings covered with metallic roofs.

On the principle of electric induction, houses thus covered are evidently more liable to be struck than those furnished either with shingle or tile. Fortunately however they admit of very simple means of perfect protection. It is evident, from well-established principles of electrical action, that if the outside of a house were encased entirely in a coating of metal, the most violent discharge which might fall upon it from the clouds would pass silently to the earth without damaging the house, or endangering the inmates. It is also evident, that if the house be merely covered with a roof of metal, without projecting chimneys, and this roof were put in metallic connection with the ground, the building would be perfectly protected. To make a protection therefore of this kind the Professor advises that the metallic roof be placed in connection with the ground by means of the tin or copper gutters which serve to lead the water from the roof to the earth. For this purpose it is sufficent to solder to the lower end of the gutter a ribbon of sheet copper, two or three inches wide, surrounding it with charcoal, and continuing it out from the house until it terminates in moist ground. The upper ends of these gutters are generally soldered to the roof; but if they are not in metallic contact, the two should be joined by a slip of sheet copper. The only part of the house unprotected by this arrangement will be the chimneys; and in order to secure these it will only be necessary to erect a short rod against the chimney, soldered at its lower end to the metal of the roof, and extending fifteen or twenty inches above the top of the flue.

Considerable discussion in late years has taken place in reference to the transmission of electricity along a conductor; —whether it passes through the whole capacity of the rod, or

is principally confined to the surface. From a series of experiments presented to the American Philosophical Society, by Professor Henry, on this subject, it appears that the electrical discharge passes, or tends to pass, principally at the surface; and as an ordinary sized house is commonly furnished with from two to four perpendicular gutters (generally two in front and two in the rear), the surface of these will be sufficient to conduct silently, the most violent discharge which may fall from the clouds.

Professor Henry also stated that he had lately examined a house struck by lightning, which exhibited some effects of an interesting kind. The lightning struck the top of the chimney, passed down the interior of the flue to a point opposite a mass of iron placed on the floor of the garret, where it pierced the chimney; thence it passed explosively, (breaking the plaster,) into a bedroom below, where it came in contact with a copper bell-wire, and passed along this horizontally and silently for about six feet; thence it leaped explosively through the air a distance of about ten feet, through a dormer window, breaking the sash, and scattering the fragments across the street. It was evidently attracted to this point by the upper end of a perpendicular gutter, which was near the window. It passed silently down the gutter, exhibiting scarcely any mark of its passage until it arrived at the termination, about a foot from the ground. Here again an explosion appeared to have taken place, since the windows of the cellar were broken. A bed in which a man was sleeping at the time, was situated against the wall, immediately under the bell-wire; and although his body was parallel to the wire, and not distant from it more than four feet, he was not only uninjured, but not sensibly affected. The size of the hole in the chimney, and the fact that the lightning passed along the copper wire without melting it, show that the discharge was a small one, and yet the mechanical effects, in breaking the plaster, and projecting the window frame across the street, were astonishingly great.

These effects the Professor attributes to a sudden repulsive energy, or expansive force developed in the air along the

path of the discharge. Indeed, he conceives that most of the mechanical effects often witnessed in cases of buildings struck by lightning, may be referred to the same cause. In the case of a house struck within a few miles of Princeton, the discharge entered the chimney, burst open the flue, and passed along the *cock-loft* to the other end of the house; and such was the explosive force in this confined space, that nearly the whole roof was blown off. This effect was in all probability due to the same cause which suddenly expands the air in the experiment with Kinnersly's electrical air thermometer.

ON COLOR-BLINDNESS.*

(From the Princeton Review, vol. XVII, pp. 483–489.)

July, 1845.

It is an interesting fact in reference to the dependence of at least one class of our knowledge—on sensation, that many persons are born with defective vision and yet remain for years of their lives without being conscious of the deficiency. We know a gentleman who had probably been always near sighted, but who did not discover the peculiarity of his vision until the age of twenty-five, when it was accidentally made known by looking at a distant object through a concave lens. Many persons whose eyes are sound and capable of exercising the most delicate functions, are permanently unable to distinguish certain colors. And the number of such persons is much more considerable than we would be led to imagine from the little attention this defect of vision has excited. It is often unknown to the individual himself, and indeed only becomes revealed by comparing his powers of discriminating different colors, with those of other persons.

---

*1. Observations on color-blindness, or insensibility to the impression of certain colors. By Sir David Brewster. Philosophical Magazine.

2. Memoir on Daltonism, (or color-blindness.) By M. Elie Wartmann, Professor of Natural Philosophy in the Academy of Lausanne, &c. Scientific Memoirs.

The eye also under some circumstances may lose its sensibility for particular colors, or be thrown into such an unusual state as to present all objects to the mind under the appearance of a false color. Thus, if a person looks fixedly for a time at a bright red object and then turns his eye to a white wall, he will perceive a green image of the red object depicted on the white surface. A lady of our acquaintance was once thrown into alarming but laughable paroxysm of terror by an effect of this kind. She had been for some hours attentively sewing on a bright crimson dress, when her attention was directed towards her child, who in its sport had thrown itself on the carpet; its face appeared of the most ghastly hue, and the affrighted mother screamed in agony, that her child was in convulsions: the other inmates of the house hastened to her assistance, but they were surprised to find the little one smiling in perfect health. The sanity of the mother became the natural object of solicitude, until the effect was properly referred to the impression made on her eye by the crimson cloth.

Phenomena of this kind are known by the name of accidental colors; they have long attracted the attention of the natural philosopher, but the explanation of them is still involved in considerable uncertainty. The hypothesis which has been most generally adopted is that the eye by long attention to a particular color becomes fatigued with this and is incapable after a time of distinctly perceiving it; while it retains its full power of perception in reference to a fresh color. The consequence of this is that when the eye is directed to a white surface, after having attentively regarded a red object, green must appear; because white may be considered as a compound of red and green, and when the perception of the red is destroyed, the green must become visible. This explanation, however well it may apply to some of the phenomena, is not sufficient for the whole. Accidental colors can be perceived in the eye itself in perfect darkness. This is shown by steadily regarding for a short time a brilliant lamp, and then covering the eyes with the hands so as to exclude all external light, a luminous spot

will be perceived which passes in succession through all the colors of the rainbow.

Of the real cause of these appearances we are as yet almost entirely ignorant. Professor Plateau, of Ghent, has indeed referred them all to a few simple principles; but these appear to us rather expressions of the law of succession of the phenomena, than physical explanations of them. We do not however at this time intend to dwell on this class of phenomena, but to give a succinct account of those peculiarities of vision, in which abnormal perceptions of color are permanent, and which are fully treated of in the memoirs, the titles of which stand at the head of this article.

The peculiarity of vision called *color-blindness*, and sometimes *Daltonism*, may generally be referred to two classes. 1. Those in which all impression of color, except white and black, are wanting. 2. Those in which the individual can perceive certain simple colors, but is not able properly to distinguish between them. There are persons, strange as it may appear, in whom the sense of primary color is entirely deficient, and who, in place of red, yellow, and blue, see nothing but different degrees of white and black. Professor Wartmann gives a number of cases of this kind. The most ancient of those he finds described is that by Dr. Tuberville, in 1684, of a woman about 32 years of age, who came to consult the Dr. about her sight, which though excellent in other respects, gave her no impression in reference to color, except white and black. Spurzheim mentions a family, all the members of which could distinguish only different shades of white and black. An account is given by Mr. Huddart of a shoemaker, in Cumberland, who could distinguish in different colors only a greater or less intensity of light, calling all bright tints white and all dull ones black. His peculiarity of vision was unknown to him until one day, while a boy, playing in the street, he found a stocking, and for the first time was struck with the fact that it was called by his companions red, whereas to his mind it was capable of no farther description than that designated by the word stocking; he was thus led to conclude that there was some-

thing else besides the form and position in the leaves and fruit of a cherry tree, perceived by his playmates but not seen by himself. Two of his brothers had the same imperfection, while two other brothers, his sisters, and other relatives, had the usual condition of vision.

Of the other class the cases are much more numerous; we shall however give only a few examples. Mr. Harvey, of Plymouth, mentions a tailor who could see in the rainbow but two tints, namely, yellow and bright blue. Black appeared to him in general—green, sometimes crimson; light blue appeared like dark blue, crimson, or black; green was confounded with black and brown; carmine, red, lake, and crimson,—with blue.

But the most interesting case of this kind is that of the celebrated chemical philosopher, Dr. Dalton, of England. He published an account of his own case and that of several others in the Transactions of the Manchester Society, in 1794. Of the seven colors of the rainbow he could distinguish but two, yellow and blue; or at most, three, yellow, blue, and purple. He saw no difference between red and green; so that he thought the color of a laurel leaf the same as that of a stick of red sealing wax. A story is told of his having, on one occasion, appeared at the Quaker meeting, of which he was a member, in the usual drab coat and smallclothes of the sect, with a pair of flaming red-colored stockings to match. Whatever may be the truth in reference to this story, we have the assertion of Professor Whewell, that when Dr. Dalton was asked with what he would compare the scarlet gown with which he had been invested by the university, he pointed to the trees, and declared that he perceived no difference between the color of his robe and that of their foliage. Dr. Dalton found nearly twenty persons possessed of the same peculiarity of vision as himself; and among the number the celebrated metaphysician, Dugald Stewart, who could not distinguish a crimson fruit, like the Siberian crabapple, from the leaves of the tree on which it grew otherwise than by the difference in its form.

On account of the prominence conferred on this defect of vision by Mr. Dalton's publication, the continental philoso-

phers gave it the name of *Daltonism*. To this name however several British writers have strongly objected. If this system of names were once allowed, say they, there is no telling where it would stop: the names of celebrated men would be connected, not with their superior gifts or achievements, but with the personal defects which distinguish them from their more favored but less meritorious contemporaries. Professor Whewell proposed the term *Idiopts*, signifying peculiarity of vision; but to this name Sir David Brewster properly objected, that the important consonant *p* would be very apt to be omitted in ordinary pronunciation, and so the last state of the Idiopt would be worse than the first. The name *color-blindness*, suggested by Sir David, although not in all cases free from objection, is perhaps better than any we have seen proposed.

It has already been stated that the number of persons affected with color-blindness is much more considerable than is generally imagined. They are often themselves ignorant of their imperfection of vision, particularly when it is restricted to the want of power to discriminate between colors nearly related to each other. Professor Seebeck found five cases among the forty boys who composed the two upper classes of a gymnasium of Berlin. Professor Prevost, of Geneva, stated that they amounted to one in twenty; and Professor Wartmann does not think this estimate much exaggerated

Observations on this peculiarity of vision have as yet been confined, so far as we know, to Europe, with the exception of two cases described by Dr. Hays, of Philadelphia, in the Proceedings of the American Philosophical Society. It has also as yet been found only among the white race, although sufficient observations have not been made to render it probable that it is confined to this variety of the human family. The question has been asked, whether there is any external sign by which to detect, with simple inspection of the visual organ, a case of color-blindness. Professor Wartmann remarks, that he would not venture to give an answer to this question in all cases in the negative. I have observed, says

he, in the case of *Daltonians* whose eyes are brown, of the color which the English call hazel, a golden lustre of a peculiar tint, when the eye was viewed under an incidence of some obliquity.

Color-blindness is found much more common among men than women. Out of one hundred and fifty registered cases, there are but six of females, and one of these is doubtful. It has been conjectured that needle-work on a variety of colored articles might be the means of counteracting the tendency to this defect, as well as to produce a delicacy of discrimination of different shades of color not possessed by those otherwise employed. But in answer to this it has been remarked, that in the case of "Daltonians" engaged in painting, there has been found but little, if any, improvement of the condition of vision; and the very employment of the females on works which require a constant comparison of color would daily reveal cases of blindness of this kind did it frequently exist in the female sex. This peculiarity of vision is principally congenital. Professor Wartmann has found but two exceptions. In one of these, colors were perceived in the usual manner, until at the ninth year; when the boy received a violent blow on the head, which fractured the skull, and rendered a surgical operation necessary. The fact however that three of the brothers of this individual were affected with the same kind of vision renders it probable that he was constitutionally pre-disposed to this peculiarity.

With regard to hereditary pre-disposition there are some persons in whom this defect of vision occurs, whose relatives have never been known to be affected with it; others appear to have inherited it from their fathers through several generations, both on the maternal and paternal side. The boy before mentioned, as becoming color-blind at the age of nine years, was the eldest of eleven children,—seven males and four females; these were singularly divided into two sets, one of which consisted of individuals with blonde hair, and all the males with defective vision; the other, of those with red hair and ordinary power of vision.

Dr. Seebeck, as well as Professor Wartmann, has made a series of experiments to determine whether a person of this peculiarity of vision possesses the power of perceiving differences in colors which appear identical to us. The result of the investigations of both these philosophers was that he does not. Another problem has also been solved by the last-mentioned gentleman, in reference to the difference between a person with this defective vision and one of ordinary conditioned sight, in the perception of complementary colors. He found that colors which we regard as complementary, or such as when mingled together produce white, do not appear as such to those affected with this abnormal vision. They are not however insensible to accidental colors, but the feeling which results from the fatigue of attempting to produce these appears to be more painful in them than in us.

Various hypotheses have been advanced by different persons for the explanation of color-blindness. Mr. Dalton supposed that his peculiarity of vision, as well as that of those whom he had examined, depended on the fact that the vitreous or principal humour of the eye, in these cases, instead of being colorless and transparent was tinged with a blue. After his death, in obedience to his own instruction, his eyes were examined by his medical attendant, Mr. Ransome, but the vitreous humour was not found to exhibit any tinge of blue; on the contrary, it was of a pale yellow color. Objects viewed through it were not changed in color as they should have been had the hypothesis been true. Indeed, were the supposition correct, the same effect should be produced by blue spectacles, which is known not to be the case.

Stewart, Herschel, and others are of the opinion that this malady of vision is attributable to a defect in the *sensorium* itself, which renders it incapable of appreciating the differences between the rays on which the sensation of color depends. Sir David Brewster conceives that the eye, in the case of color-blindness, is insensible to the colors at one end of the spectrum, just as the ear of certain persons is insensible to sounds at one extremity of the scale of musical notes, while it is perfectly sensible to all other sounds. He knows

nothing about the *sensorium* or its connection with, or mode of operation upon, the nerves of sensation; and from the analogy of sight and hearing he has no hesitation in predicting that there may be found persons whose color-blindness is confined to one eye, or at least is greater in one eye than in the other. "Nor is this (says he) wholly a conjecture from analogy, for my own right eye, though not a better one than the left, which has no defect whatever, is more sensible to red light than the left eye." The case is precisely analogous with respect to his ears, for certain sounds; and no person, it is presumed, will maintain that there is a sensorium for each ear and each eye.

Whatever may be the cause of the inferiority, there exists a very easy means of compensating it to a certain extent. This method, first used by Dr. Seebeck, consists in viewing colored objects through colored media. Suppose the medium to be a piece of red glass; the impression of a red body and a green one on the eye of a person like Dr. Dalton, would be different, although with the naked eye they would be the same. The red glass would intercept much more of the light of the green object than of the red one, and hence the two would be readily distinguishable by a difference in the intensity of the illumination of the two objects. Nothing can equal the surprise, says Professor Wartmann, of a *Daltonian* when the errors which he commits every day in the appreciation of colors are thus disclosed to him.

EXPERIMENTS ON ELECTRICAL DISCHARGE.

(Proceedings of the American Philosophical Society, vol. IV, pages 208, 209.)

November 7, 1845.

Professor Henry communicated the result of a series of experiments on electricity made last winter. They had reference, first, to the discharge of electricity through a long wire connected with the earth at the farther end; secondly, to the discharge of a jar through a wire; and, thirdly, to an attempt to account for the phenomena of dynamic induction.

He first showed that when a charge of electricity is given

to one end of a wire, the different parts of the wire become charged successively, as though a wave of electricity passed along it. He then showed that the charge passed along the surface of the wire, and not through its whole mass, as was supposed from the analogy of galvanic conduction. Hence he inferred that dynamical electricity obeys the same laws as the statical. He then detailed some experiments upon the passage of electricity through plates, and showed that when a charge was transmitted across a plate the tension was greatest at the edges, the electricity apparently exercising a self-repelling action; while if the charge were passed through two pieces of tinfoil, these slips attract each other.

Professor Henry believes that it may be justly inferred from these experiments, that the attraction is due to ponderable matter, while the repulsion is due to electricity; thus showing that electricity is a separate principle, and not a mere property of matter.

He next passed to the subject of the discharge of a jar. It was necessary, in his experiments, to get rid of the free electricity arising from the thickness of the glass, and it occurred to him that this might be done by removing the knob, and making the coating upon the inside of less area than that upon the outside. With this arrangement, when the discharge was made through a long wire, and a test jar brought near it during discharge, a bright spark passed; but upon approaching the jar to a delicate electrometer it gave no indications of free electricity. Reflecting upon this, and upon an experiment of Professor Wheatstone's, he was led to believe that the jar is discharged by two waves, a negative and a positive one, starting simultaneously from the two ends of the wire. To prove this he broke the wire, and interposed a pane of glass dusted with red lead and sulphur; two figures of positive and negative electricity were produced. He made several other experiments tending to prove this same fact. He showed how these experiments serve to explain that of Dr. Priestly, where a spark was found to pass between the ends of a long bent wire, the ends being brought within a few inches of each other.

He next passed to the connection between statical and dynamical induction. Statical induction has heretofore been observed only at short distances. His first experiment proved that it could be observed at the distance of nineteen feet, the floor of a chamber intervening, showing that statical induction takes place at great distances, though not at so great distances as the dynamical. He then explained his views of the nature of dynamical induction. When a spark is thrown upon a wire it passes in a wave, whose length might be determined if we knew the velocity of electricity. Now, if we have another parallel wire, a negative wave will be formed in this, and the two waves will travel simultaneously in the same direction. But this is equivalent to a positive induced wave in the opposite direction. In this way the phenomena accompanying the discharge of a jar are easily explained. Again, if we conceive that in a galvanic battery the discharge consists of a series of such waves, we may very simply explain the phenomena of galvanic induction.

EXPERIMENT ON THE MAGNETIC POLARIZATION OF LIGHT.

(Proceedings of American Philosophical Society, vol. IV. pp. 229, 230.)

January 16, 1846.

Dr. Patterson read a portion of a letter from Professor Henry in which he describes the manner in which he had repeated the experiment of Mr. Faraday on the magnetic polarization of light.

"- - - This consists in producing in pure water and other liquids a new arrangement of particles by which they become possessed of the property of circular polarization during the time a current of galvanism is circulating around them. The arrangement I employed was as follows: A tube of glass was filled with pure water and the ends closed with plates of glass; this was placed in the axis of an iron tube, and this again inserted into the axis of a coil consisting of about eight hundred feet of copper wire. The ends of the iron tube were closed with corks, through one of which was passed a Nicol's prism, and in the axis of the other was fastened a plate of tourmaline. This tube being directed to the clear sky, and the tourmaline, which was placed next the eye, so turned that it presented a dark field of view, a current of galvanism from twenty-two cups of a Daniell's battery was passed through the coil. At the moment of making the communication with the battery the field became light, and when the circuit was broken it again appeared dark. A slight rotation of the tourmaline also produced darkness while the galvanic current was passing, which indicated a twist in the plane of polarization of the prolonged beam. The same effect was produced without the iron tube but not to the same extent."

ON THE RELATION OF TELEGRAPH LINES TO LIGHTNING.

(Proceedings of the American Philosophical Society, vol. IV, pp. 260–268.)

June 19, 1846.

[A letter from S. D. Ingham to Dr. Patterson was read, detailing cases in which the telegraph wires were struck by lightning, and asking the attention of the Society to some interesting questions connected with the mode in which the wires may be affected by electricity.]

Professor Henry, to whom the letter was referred, made the following report:

The action of the electricity of the atmosphere on the wires of the electrical telegraph is at the present time a subject of much importance, both on account of its practical bearing, and the number of purely scientific questions which it involves. I have accordingly given due attention to the letter referred to me, and have succeeded in collecting a number of facts in reference to the action in question. Some of these are from the observations of different persons along the principal lines, and others from my own investigations during a thunder storm on the 19th of June, when I was so fortunate as to be present in the office of the telegraph in Philadelphia, while a series of very interesting electrical phenomena was exhibited. In connection with the facts derived from these sources, I must ask the indulgence of the Society in frequently referring, in the course of this communication, to the results of my previous investigations in dynamic electricity, accounts of which are to be found in the Proceedings and Transactions of this Institution.

From all the information on the subject of the action of the electricity of the atmosphere on the wires of the telegraph, it is evident that effects are produced in several different ways.

1. The wires of the telegraph are liable to be struck by a direct discharge of lightning from the clouds, and several cases of this kind have been noticed during the present season. About the 20th of May the lightning struck the

elevated part of the wire which is supported on a high mast at the place where the telegraph crosses the Hackensack river. The fluid passed along the wire each way from the point which received the discharge, for several miles, striking off at irregular intervals down the supporting poles. At each place where the discharge to a pole took place a number of sharp explosions were heard in succession, resembling the rapid reports of several rifles. During another storm, the wire was struck in two places in Pennsylvania, on the route between Philadelphia and New York; at one of these places twelve poles were struck, and at the other eight. In the latter case the remarkable fact was observed that every other pole escaped the discharge; and the same phenomenon was observed, though in a less marked degree, near the Hackensack river. In some instances the lightning has been seen coursing along the wire in a stream of light; and in another case it is described as exploding from the wire at certain points, though there were no bodies in the vicinity to attract it from the conductor.

In discussing these, and other facts to be mentioned hereafter, we shall for convenience adopt the principles and language of the theory which refers the phenomena of electricity to the action of a fluid of which the particles repel each other, and are attracted by the particles of other matter. Although it cannot be affirmed that this theory is an actual representation of the cause of the phenomena, as they are produced in nature, yet it may be asserted that it is, in the present state of science, an accurate mode of expressing the laws of electrical action, so far as they have been made out; and that though there are a number of phenomena which have not as yet been referred to this theory, there are none which are proved to be directly at variance with it.

That the wires of the telegraph should be frequently struck by a direct discharge of lightning is not surprising when we consider the great length of the conductor, and consequently the many points along the surface of the earth through which it must pass, peculiarly liable to receive the discharge from the heavens. Also, from the great length of the con-

ductor, the more readily must the repulsive action of the free electricity of the cloud drive the natural electricity of the conductor to the farther end of the line, thus rendering more intense the negative condition of the nearer part of the wire, and consequently increasing the attraction of the metal for the free electricity of the cloud. It is not however probable that the attraction, whatever may be its intensity, of so small a quantity of matter as that of the wire of the telegraph, can of itself produce an electrical discharge from the heavens, although, if the discharge were started by some other cause, such as the attraction of a large mass of conducting matter in the vicinity, the attraction of the wire might be sufficient to change the direction of the descending bolt, and draw it, in part or in whole, to itself. It should also be recollected, that on account of the perfect conduction, a discharge on any part of the wire must affect every other part of the connected line, although it may be hundreds of miles in length.

That the wire should give off a discharge to a number of poles in succession is a fact I should have expected from my previous researches on the lateral discharge of a conductor transmitting a current of free electricity. In a paper on this subject, presented to the British Association in 1837,* I showed that when electricity strikes a conductor explosively it tends to give off sparks to all bodies in the vicinity, however intimately the conductor may be connected with the earth. In an experiment in which sparks from a small machine were thrown on the upper part of a lightning rod, erected in accordance with the formula given by the French Institute, corresponding sparks could be drawn from every part of the rod, even from that near the ground. In a communication since made to this Society, I have succeeded in referring this phenomenon to the fact, that during the transmission of a quantity of electricity along a rod, the surface of the conductor is charged in succession, as it were, by a wave of the fluid, which, when it arrives opposite a given point, tends to give off a spark to a neighboring body for the same reason

*[Report of British Association, 1837. See *ante*, page 101.]

that the charged conductor of the machine gives off a spark under the same circumstances.

It might at first be supposed that the redundant electricity of the conductor would exhaust itself in giving off the first spark, and that a second discharge could not take place; but it should be observed that the wave of free electricity, in its passage, is constantly attracted to the wire by the portion of the uncharged conductor which immediately precedes its position at any time; and hence but a part of the whole redundant electricity is given off at one place, the velocity of transmission of the wave as it passes the neighboring body, and its attraction for the wire, preventing a full discharge at any one place. The intensity of the successive explosions is explained by referring to the fact, that the discharge from the clouds does not generally consist of a single wave of electricity, but of a number of discharges along the same path in rapid succession, or of a continuous discharge which has an appreciable duration; and hence the wire of the telegraph is capable of transmitting an immense quantity of the fluid thus distributed over a great length of the conductor.

The remarkable facts of the explosions of the electricity into the air, and of the poles being struck in interrupted succession, find a plausible explanation in another electrical principle which I have established, namely, in all cases of the disturbance of the equilibrium of the electrical *plenum* which we must suppose to exist throughout all terrestrial space, the state of rest is attained by a series of diminishing oscillations. Thus, in a discharge of a Leyden jar, I have shown that the phenomena exhibited cannot be explained by merely supposing the transfer of a quantity of fluid from the inner to the outer side of the jar; but in addition to this we are obliged to admit the existence of several waves, backwards and forwards, until the equilibrium is attained. In the case of the discharge from the cloud, a wave of the natural electricity of the metal is repelled each way from the point on which the discharge falls, to either end of the wire, is then reflected, and in its reverse passage meets in

succession the several waves which make up the discharge from the cloud. These waves will therefore interfere at certain points along the wire, producing, for a moment, waves of double magnitude, and will thus enhance the tendency of the fluid at these points to fly from the conductor. I do not say that the effects observed were actually produced in this way; I merely wish to convey the idea that known principles of electrical action might, under certain circumstances, lead us to anticipate such results.

2. The state of the wire may be disturbed by the conduction of a current of electricity from one portion of space to another, without the presence of a thunder-cloud; and this will happen in case of a long line, when the electrical condition of the atmosphere which surrounds the wire at one place is different from that at another. Now it is well known that a mere difference in elevation is attended with a change in the electrical state of the atmosphere. A conductor, elevated by means of a kite, gives sparks of positive electricity on a perfectly clear day; hence, if the line of the telegraph passes over an elevated mountain ridge, there will be continually, during clear weather, a current from the more elevated to the lower points of the conductor.

A current may also be produced in a long level line by the precipitation of vapor, in the form of a fog, at one end, while the air remains clear at the other; or by the existence of a storm of rain or snow at any point along the line, while the other parts of the wire are not subjected to the same influence.

Currents of sufficient power to set in motion the marking machine of the telegraph have been observed, which must have been produced by some of these causes. In one case the machine spontaneously began to operate without the aid of the battery while a snow-storm was falling at one end of the line, and clear weather existed at the other. On another occasion a continued stream of electricity was observed to pass between two points at a break in the wire, presenting the appearance of a gaslight almost extinguished. A con-

stant effect of this kind indicates a constant accession of electricity at one part of the wire, and a constant discharge at the other.

3. The natural electricity of the wire of the telegraph is liable to be disturbed by the ordinary electrical induction of a distant cloud. Suppose a thunder-cloud driven by the wind in such a direction as to cross one end of the line of the telegraph at the elevation—say of a mile; during the whole time of the approach of the cloud to the point of its path directly above the wire, the repulsion of the redundant electricity with which it is charged would constantly drive more and more of the natural electricity of the wire to the farther end of the line, and would thus give rise to a current. When the cloud arrived at the point nearest to the wire, the current would cease for a moment; and as the repulsion gradually diminished by the receding of the cloud, the natural electricity of the wire would gradually return to its normal state, giving rise to a current in an opposite direction. If the cloud were driven by the wind parallel to the line of the telegraph a current would be produced towards each end of the wire, and these would constantly vary in intensity with the different positions of the cloud. Although currents produced in this way may be too feeble to set in motion the marking apparatus, yet they may have sufficient power to influence the action of the current of the battery so as to interfere with the perfect operation of the machine.

4. Powerful electrical currents are produced in the wires of the telegraph by every flash of lightning which takes place within many miles of the line, by the action of dynamic induction; which differs from the action last described in being the result of the influence of electricity *in motion* on the natural electricity of the conductor. The effect of this induction, which is the most fruitful source of disturbance, will be best illustrated by an account of some experiments of my own, presented to the Society in 1843. A copper wire was suspended by silk strings around the ceiling of an upper

room so as to form a parallelogram of about sixty feet by thirty on the sides; and in the cellar of the same building, immediately below, another parallelogram of the same dimensions was placed. When a spark from an electrical machine was transmitted through the upper parallelogram an induced current was developed in the lower one, sufficiently powerful to magnetize needles, although two floors intervened, and the conductors were separated to the distance of thirty feet. In this experiment no electricity passed through the floors from one conductor to the other; the effect was entirely due to the repulsive action of the electricity in motion in the upper wire on the natural electricity of the lower. In another experiment two wires, about 400 feet long, were stretched parallel to each other between two buildings; a spark of electricity sent through one produced a current in the other, though the two were separated to the distance of 300 feet; and from all the experiments it was concluded that the distance might be indefinitely increased, provided the wires were lengthened in a corresponding ratio.

That the same effect is produced by the repulsive action of the electrical discharge in the heavens is shown by the following modification of the foregoing arrangement. One of the wires was removed and the other so lengthened at one end as to pass into my study and thence through a cellar window into an adjacent well. With every flash of lightning, which took place in the heavens within at least a circle of twenty miles around Princeton, needles were magnetized in the study by the induced current developed in the wire. The same effect was produced by soldering a wire to the metallic roof of the house, and passing it down into the well; at every flash of lightning a series of currents, in alternate directions, was produced in the wire.

I was also led, from these results, to infer that induced currents must traverse the line of a railroad, and this I found to be the case. Sparks were seen at the breaks in the continuity of the rail with every flash of a distant thunder cloud.

Similar effects, but in a greater degree, must be produced on the wire of the telegraph, by every discharge in the

heavens; and the phenomena which I witnessed on the 19th of June in the telegraph office in Philadelphia were, I am sure, of this kind. In the midst of the hurry of the transmission of the congressional intelligence from Washington to Philadelphia, and thence to New York, the apparatus began to work irregularly. The operator at each end of the line announced at the same time a storm at Washington, and another at Jersey City. The portion of the circuit of the telegraph which entered the building, and was connected with one pole of the galvanic battery, happened to pass within the distance of less than an inch of the wire which served to form the connection of the other pole with the earth. Across this space, at an interval of every few minutes, a series of sparks in rapid succession was observed to pass; and when one of the storms arrived so near Philadelphia that the lightning could be seen, each series of sparks was found to be simultaneous with a flash in the heavens. Now we cannot suppose, for a moment, that the wire was actually struck at the time each flash took place; and indeed it was observed that the sparks were produced when the cloud and flash were at a distance of several miles to the east of the line of the wire. The inevitable conclusion is that all the exhibition of electrical phenomena witnessed during the afternoon was purely the effects of induction, or the mere disturbance of the natural electricity of the wire at a distance, without any transfer of the fluid from the cloud to the apparatus.

The discharge between the two portions of the wire continued for more than an hour, when the effect became so powerful that the superintendent, alarmed for the safety of the building, connected the long wire with the city gas pipes, and thus transmitted the current silently to the ground. I was surprised at the quantity and intensity of the current; it is well known that to affect a common galvanometer with ordinary electricity requires the discharge of a large battery; but such was the quantity of the induced current exhibited on this occasion that the needle of an ordinary vertical galvanometer, with a short wire, and apparently of little sensibility, was moved several degrees.

The pungency of the spark was also, as might have been expected, very great. When a small break was made in the circuit, and the parts joined by the forefinger and thumb the discharge transmitted through the hand affected the whole arm up to the shoulder. I was informed by the superintendent that on another occasion a spark passed over the surface of the spool of wire surrounding the legs of the horse-shoe magnet at right angles to the spires; and such was its intensity and quantity that all the wires across which it passed were melted at points in the same straight line as if they had been cut in two by a sharp knife.

The effects of the powerful discharges from the clouds may be prevented, in a great degree, by erecting at intervals along the line, and aside of the supporting poles, a metallic wire connected with the earth at the lower end, and terminating above at the distance of about half an inch from the wire of the telegraph. By this arrangement the insulation of the conductor will not be interfered with, while the greater portion of the charge will be drawn off. I think this precaution of great importance at places where the line crosses a river, and is supported on high poles. Also in the vicinity of the office of the telegraph where a discharge, falling on the wire near the station, might send a current into the house of sufficient quantity to produce serious accidents. The fate of Professor Richman, of St. Petersburg, should be recollected, who was killed by a flash from a small wire, which entered his house from an elevated pole, while he was experimenting on atmospheric electricity.

The danger however which has been apprehended from the electricity leaving the wire and discharging itself into a person on the road is, I think, very small; electricity, of sufficient intensity to strike a person at the distance of eight or ten feet from the wire, would, in preference, be conducted down the nearest pole. It will however in all cases be most prudent to keep at a proper distance from the wire during the existence of a thunder storm in the neighborhood.

It may be mentioned as an interesting fact, derived from two independent sources of information, that large numbers of small birds have been seen suspended by the claws from the wire of the telegraph. They had, in all probability, been instantaneously killed, either by a direct discharge, or an induced current from a distant cloud while they were resting on the wire.

Though accidents to the operators, from the direct discharge, may be prevented by the method before mentioned, yet the effect on the machine cannot be entirely obviated; the residual current which escapes the discharge along the perpendicular wires must neutralize, for a moment, the current of the battery, and produce irregularity of action in the apparatus.

The direct discharge from the cloud on the wire is, comparatively, not a frequent occurrence, while the dynamic inductive influence must be a source of constant disturbance during the seasons of thunder storms; and no other method presents itself to my mind at this time for obviating the effect, but that of increasing the size of the battery, and diminishing the sensibility of the magnet so that at least the smaller induced currents may not be felt by the machine. It must be recollected that the inductive influence takes place at a distance through all bodies, conductors and non-conductors; and hence no coating that can be put upon the wire will prevent the formation of induced currents.

I think it not improbable, since the earth has been made to act the part of the return conductor, that some means will be discovered for insulating the single wire beneath the surface of the earth; the difficulty in effecting this is by no means as great as that of insulating two wires, and preventing the current striking across from one to the other. A wire buried in the earth would be protected in most cases from the effect of a direct discharge; but the inductive influence would still be exerted, though perhaps in a less degree.

The wires of the telegraph are too small and too few in number to affect, as some have supposed, the electrical con-

dition of the atmosphere by equalizing the quantity of the fluid in different places, and thus producing a less changeable state of the weather. The feeble currents of electricity which must be constantly passing along the wires of a long line may however, with proper study, be the means of discovering many interesting facts relative to the electrical state of the air over different regions.*

ON THE "FOUNTAIN-BALL," AND ON THE INTERFERENCE OF HEAT.

(Proceedings of the American Philosophical Society, vol. IV, p. 285.)

October 16, 1846.

Professor Henry laid before the Society the results of some investigations that he had lately made on two questions in physical science, and a theory of the causes of the phenomena observed.

The well known phenomenon of a ball resting on a jet of water he ascribed to the action of three different causes: 1st, to the adhesion of the water to the ball: 2d, to the adhesion of the water to itself: 3d, to the tendency of water to move in a straight line and also to the principle of action and re-action.

He had also made experiments in regard to the interference of heat for the purpose of discovering whether certain phenomena of interference of light were exhibited as well in the case of heat. He found it to be so, and that two rays of heat may be thrown on each other so as to produce a reduction of temperature.

---

*[Re-printed in Silliman's American Journal of Science, 1847, vol. III, pp. 25–32. Also in the London and Edinburgh Philosophical Magazine, 1847, vol. XXX, pp. 186–194.]

ON THE ATOMIC CONSTITUTION OF MATTER.

(Proceedings of the American Philosophical Society, vol. IV, pp. 287–290.)

November 6, 1846.

The reference to a paper presented at the preceding meeting of the Society led Professor Henry to make some remarks on the corpuscular hypothesis of the constitution of matter.

He stated that this subject has occupied attention at every period of the history of science, and though at first sight speculations of this kind might appear to belong exclusively to the province of the imagination, yet in reality he considered this hypothesis a fruitful source of valuable additions to our knowledge of the actual phenomena of the physical world. Though simple insulated facts may occasionally be stumbled upon by a lucky accident, the discovery of a series of facts or of a general scientific principle is in almost all cases the result of deductions from a rational antecedent hypothesis, the product of the imagination—founded it is true on a clear analogy with modes of physical action the truth of which have been established by previous investigation.

In constructing an hypothesis of the constitution of matter the simplest assumption, and indeed the only one founded on a proper physical analogy, is that the same laws of force and motion which govern the phenomena of the action of matter in masses pertains to the minutest atoms of these masses.

It is a well established fact that portions of matter at a distance tend to approach each other, and when they are brought very near, to separate, and still nearer, again to approach, and so on through several alternations. In the present state of science we consider these actions as ultimate facts to which we give the name of attracting and repelling forces, and without attempting to go behind them we may study their laws of variation as to intensity and direction under different circumstances and particularly in reference to a change of distance. Bodies or masses of matter are also subjected to fixed laws of motion which have been classed

under three heads, namely, the law of inertia or tendency to resist a change of state and to move in a straight line with a constant velocity, the law of the co-existence of separate motions, and the law of the equality of action and reaction.

The explanation of a mechanical phenomenon consists in its analysis and the reference of its several parts to the foregoing laws of force and motion, and as no phenomenon, whether it relates to masses or the minutest portions of matter is fully explained until it can be referred to one or more of these laws it follows that any corpuscular hypothesis which does not ascribe to each atom of matter the property of obedience to the same laws must be defective. It was for this reason that in printing a syllabus of his lectures about two years ago he was induced to make some additions to the assumptions on which the corpuscular hypothesis of Boscovich is founded. According to this celebrated hypothesis, a portion of matter consists of an assemblage in space of an indefinite number of points kept at a given distance by attracting and repelling forces: these points have relative position but not magnitude, and are merely centers of action of the forces which affect our senses, and since all our knowledge of matter is derived from the action of these forces, to infer that these points are anything more than the centers of forces is going beyond our premises.

This hypothesis readily explains the statical properties of bodies, such as elasticity, porosity, impenetrability, solidity, liquidity, crystallization, resistance to compression when a force is applied to either side of the body, etc.; but it fails to account for the dynamic phenomena of *masses* of matter, or those which are referable to the three laws of motion. It is not therefore enough that we assume, as the elements of matter, an assemblage of points in space from which merely emanate attracting and repelling forces; we must also suppose these points to be endowed with *inertia*, or a tendency to resist a change of state, whether of rest or motion, and a tendency to move in a straight line; also to possess the property of preserving the effects of a number of impulses, as well as that of transferring motion from one point

to another, the one losing as much motion as the other gains. But the admission of the existence of points with such qualities brings us back to the Newtonian hypothesis of matter.

According to the view we have given, a portion of matter consists of an assemblage of indivisible and indestructible atoms endowed with attracting and repelling forces, and with the property of obedience to the three laws of motion. All the other properties, and indeed all the mechanical phenomena of matter, so far as they have been analyzed, are probably referable to the action of such atoms, arranged in groups of different orders, namely, of ultimate atoms, chemical atoms, simple molecules, compound molecules, particles, etc.; the distance in all cases between any two atoms being much greater than the diameter of the atoms or molecules.

In order that we may bring all the phenomena of the "imponderable" agents of nature, (as they are called,) under the category of the laws of force and motion, we are obliged to assume the existence of an ætherial medium formed of atoms, which are endowed with precisely the same properties as those we have assigned to common matter; and this assumption leads us to the inference, that matter is diffused through all space.

That something exists between us and the sun, possessing the properties of matter, may be inferred from the simple fact that time is required for the transmission of light and heat through the intervening space. The phenomena of the transmitted motion, in these cases, are perfectly represented by undulations, in a medium composed of very minute atoms of ordinary matter, endowed with all the mechanical properties we have mentioned. Indeed, the motion is analogous—though not precisely similar—to the transmission of sound through air; the time however in the two cases being very different. Light passes the space between us and the sun in about eight minutes, while sound (through air) would require $13\frac{3}{4}$ years to perform the same journey. This difference in velocity is however readily explained by a difference in density and elasticity of air, and the ætherial medium. That the phenomena of light and heat from the sun are not the

effect of transmission of mere force, (without intervening matter,) such as that of attraction and repulsion, is evident from the fact that these actions require no perceptible time for their transmission to the most distant part of the solar system. If the sun were at once to be annihilated the planet Neptune would, at the same instant begin to move in a tangent to its present orbit. Also, the phenomena of electricity and magnetism involve the consideration of time; the discharge of the former through a copper wire is transmitted with about the velocity of light, and the development of the latter in an iron bar is attended with a change in the ponderable molecules of the metal which requires time for its completion.

According to the foregoing rules, we may assume with Newton, the existence of one kind of matter diffused throughout all space, and existing in four states, namely, the ætherial, the aëriform, the liquid, and the solid. This method of presenting the atomic hypothesis of the constitution of matter, may at first sight appear startling; but on a little reflection, it will be found a necessary consequence of the attempt to explain the mechanical phenomena of matter by an assemblage of separate atoms. It may be objected to the assumption of one kind of matter that the fact of the imponderable nature of light, heat, electricity and magnetism require at least two kinds of matter; but if we adopt the theory of undulation, the phenomena of the "imponderables" (as they are called) are merely the results of the motions of the atoms of the ætherial medium combined in some cases with the motion of the atoms of the body; and since the vibrations of the atoms of a mass of matter do not increase the attraction of the earth on the mass, an increase of temperature in a body cannot change its weight; and also because the ætherial medium fills all space, a portion of this medium can no more exhibit weight than a quantity of air when weighed in the midst of the atmosphere.

The points here noticed, relate merely to the fundamental conceptions of the corpuscular or atomic constitution of matter, and not to the arrangement of the atoms into sys-

tems of groups, which are necessary to represent the varied and complicated mechanical and chemical phenomena exhibited in the physical changes going on around us. Though he could not at this time attempt to give any details of application of this hypothesis, he drew attention to one class of facts of which it is important to furnish an expression in the arrangement of the atoms. He alluded to the facts of polarity, or those which exhibit the action of opposite forces at the extremities of molecules or of masses. The north and south poles of two magnets, brought together, neutralize each other; the attraction of one is balanced by the repulsion of the other, and the point of junction is without action on a third ferruginous body. In the same manner apparently, two chemical elements which enter into combination exhibit a neutralizing effect, which indicates the existence of polar forces in the phenomena of chemical action. Nothing however is perceptible of this kind in the effects of gravitation; the action of two particles on each other does not interfere with the action at the same time of these two on any number of other particles.

In conclusion it should be remembered that the legitimate use of speculations of this kind is not to furnish plausible explanations of known phenomena, or to present old knowledge in a new and more imposing dress, but to serve the higher purpose of suggesting new experiments and new phenomena, and thus to assist in enlarging the bounds of science and extending the power of mind over matter; and unless the hypothesis can be employed in this way, however much ingenuity may have been expended in its construction, it can only be considered as a scientific romance worse than useless, since it tends to satisfy the mind with the semblance of truth, and thus to render truth itself less an object of desire.

ON THE HEIGHT OF THE AURORA.

(Proceedings of the American Philosophical Society, vol. IV, p. 370.)

December 3d, 1846.

Professor Henry made a communication relative to some observations on the aurora borealis, with the object of determining the height of the meteor. The result of the observations tended to establish the fact that the arch of the aurora like the rainbow is a local phenomenon, each observer seeing a different object.

SCIENTIFIC WRITINGS OF JOSEPH HENRY.

PART II.

FROM 1847 TO 1878.

SCIENTIFIC PAPERS AND ABSTRACTS.

PROGRAMME OF ORGANIZATION OF THE SMITHSONIAN INSTITUTION.

(From the First Annual Report of the Secretary to the Board of Regents.)*

December 8, 1847.

GENTLEMEN: - - - In accordance with my instructions I consulted with men of eminence in the different branches of literature and science, relative to the details of the plan of organization, and arranged the various suggestions offered in the form of the accompanying programme. This, after having been submitted to a number of persons in whose knowledge and judgment I have confidence, is now presented to the Board, with the concurrence of the Committee on Organization, for consideration and provisional adoption. I regret that I could not give the names of those whose suggestions have been adopted in the programme; the impossibility of rendering justice to all has prevented my attempting this. Many of the suggestions have been offered by different persons independently of each other. - - -

The introduction to the programme contains a series of propositions suggested by a critical examination of the will of Smithson, to serve as a guide in judging of the fitness of any proposed plan for carrying out the design of the testator. - - -

That all the propositions will meet with general approval cannot be expected; and that this organization is the best that could be devised is neither asserted nor believed. To produce *à priori* a plan of organization which shall be found

*[The Plan adopted by the Board of Regents, December 13, 1847.]

to succeed perfectly in practice, and require no amendment, would be difficult under the most favorable circumstances, and becomes almost impossible where conflicting opinions are to be harmonized and the definite requirements of the Act of Congress establishing the Institution are to be observed. It is not intended that the details of organization as given in the programme, should be permanently adopted without careful trial; they are rather presented as suggestions to be adopted provisionally, and to be carried into operation gradually and cautiously, with such changes from time to time as experience may dictate.

INTRODUCTION.

General considerations which should serve as a guide in adopting a Plan of Organization.

1. Will of Smithson. The property is bequeathed to the United States of America, "to found at Washington, under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men."

2. The bequest is for the benefit of mankind. The Government of the United States is merely a trustee to carry out the design of the testator.

3. The Institution is not a national establishment, as is frequently supposed, but the establishment of an individual, and is to bear and perpetuate his name.

4. The objects of the Institution are—1st, to increase—and 2d, to diffuse—knowledge among men.

5. These two objects should not be confounded with one another. The first is to enlarge the existing stock of knowledge by the addition of new truths; and the second, to disseminate knowledge, thus increased, among men.

6. The will makes no restriction in favor of any particular kind of knowledge; hence all branches are entitled to a share of attention.

7. Knowledge can be increased by different methods of facilitating and promoting the discovery of new truths; and

can be most extensively diffused among men by means of the press.

8. To effect the greatest amount of good, the organization should be such as to enable the Institution to produce results in the way of increasing and diffusing knowledge, which cannot be produced either at all or so efficiently by the existing institutions in our country.

9. The organization should also be such as can be adopted provisionally, can be easily reduced to practice, receive modifications, or be abandoned in whole or in part, without a sacrifice of the funds.

10. In order to compensate, in some measure, for the loss of time occasioned by the delay of eight years in establishing the Institution, a considerable portion of the interest which has accrued should be added to the principal.

11. In proportion to the wide field of knowledge to be cultivated, the funds are small. Economy should therefore be consulted in the construction of the building; and not only the first cost of the edifice should be considered, but also the continual expense of keeping it in repair, and of the support of the establishment necessarily connected with it. There should also be but few individuals permanently employed by the Institution.

12. The plan and dimensions of the building should be determined by the plan of the organization, and not the converse.

13. It should be recollected that mankind in general are to be benefited by the bequest, and that therefore all unnecessary expenditure on local objects would be a perversion of the trust.

14. Besides the foregoing considerations, deduced immediately from the will of Smithson, regard must be had to certain requirements of the act of Congress establishing the Institution. These are, a library, a museum, and a gallery of art, with a building on a liberal scale to contain them.

SECTION I.

Plan of Organization of the Institution in accordance with the foregoing deductions from the will of Smithson.

To increase knowledge: It is proposed—

1. To stimulate men of talent to make original researches by offering suitable rewards for memoirs containing new truths; and,

2. To appropriate annually a portion of the income for particular researches, under the direction of suitable persons.

To diffuse knowledge: It is proposed—

1. To publish a series of periodical reports on the progress of the different branches of knowledge; and,

2. To publish occasionally separate treatises on subjects of general interest.

DETAILS OF THE PLAN TO INCREASE KNOWLEDGE.

I. *By stimulating researches.*

1. Rewards consisting of money, medals, &c., offered for original memoirs on all branches of knowledge.*

2. The memoirs thus obtained to be published in a series of volumes, in a quarto form, and entitled "Smithsonian Contributions to Knowledge."

3. No memoir on subjects of physical science to be accepted for publication which does not furnish a positive addition to human knowledge, resting on original research; and all unverified speculations to be rejected.

4. Each memoir presented to the Institution to be submitted for examination to a commission of persons of reputation for learning in the branch to which the memoir pertains, and to be accepted for publication only in case the report of this commission is favorable.

5. The commission to be chosen by the officers of the Institution, and the name of the author (as far as practicable) concealed, unless a favorable decision be made.

---

*[In the annual report for 1855, this clause was changed to read—"1. Facilities afforded for the production of original memoirs on all branches of knowledge."]

6. The volumes of the memoirs to be exchanged for the transactions of literary and scientific societies, and copies to be given to all the colleges and principal libraries in this country. One part of the remaining copies may be offered for sale, and the other carefully preserved, to form complete sets of the work, to supply the demand from new institutions.

7. An abstract, or popular account of the contents of these memoirs to be given to the public through the annual report of the Regents to Congress.

II. *By appropriating a part of the income annually to special objects of research.*

1. The objects and the amount appropriated to be recommended by counsellors of the Institution.

2. Appropriations in different years to different objects; so that in course of time each branch of knowledge may receive a share.

3. The results obtained from these appropriations to be published, with the memoirs before mentioned, in the volumes of the "Smithsonian Contributions to Knowledge."

4. Examples of objects for which appropriations may be made: (*a.*) System of extended meteorological observations for solving the problem of American storms. (*b.*) Explorations in descriptive natural history, and geological, magnetical, and topographical surveys, to collect materials for the formation of a Physical Atlas of the United States. (*c.*) Solution of experimental problems, such as a new determination of the weight of the earth, of the velocity of electricity, and of light; chemical analyses of soils and plants; collection and publication of scientific facts accumulated in the offices of Government. (*d.*) Institution of statistical inquiries with reference to physical, moral, and political subjects. (*e.*) Historical researches and accurate surveys of places celebrated in American history. (*f.*) Ethnological researches, particularly with reference to the different races of men in North America; also explorations and accurate surveys of the mounds and other remains of the ancient people of our country.

DETAILS OF THE PLAN FOR DIFFUSING KNOWLEDGE.

I. *By the publication of a series of reports, giving an account of the new discoveries in science, and of the changes made from year to year in all branches of knowledge not strictly professional.*

1. These reports will diffuse a kind of knowledge generally interesting, but which at present is inaccessible to the public. Some of the reports may be published annually, others at longer intervals, as the income of the Institution or the changes in the branches of knowledge may indicate.

2. The reports are to be prepared by collaborators eminent in the different branches of knowledge.

3. Each collaborator to be furnished with the journals and publications, domestic and foreign, necessary to the compilation of his report; to be paid a certain sum for his labors, and to be named on the title-page of the report.

4. The reports to be published in separate parts so that persons interested in a particular branch can procure the parts relating to it without purchasing the whole.

5. These reports may be presented to Congress for partial distribution, the remaining copies to be given to literary and scientific institutions, and sold to individuals for a moderate price.*

II. *By the Publication of separate treatises on subjects of general interest.*

1. These treatises may occasionally consist of valuable memoirs, translated from foreign languages, or of articles

*The following are some of the subjects which may be embraced in the reports:

I. PHYSICAL CLASS.—1. Physics, including astronomy, natural philosophy, chemistry and meteorology. 2. Natural history, including botany, zoology, geology, &c. 3. Agriculture. 4. Application of science to arts.

II. MORAL AND POLITICAL CLASS.—5. Ethnology, including particular history, comparative philology, antiquities, &c. 6. Statistics and political economy. 7. Mental and moral philosophy. 8. A survey of the political events of the world; penal reform, &c.

III. LITERATURE AND THE FINE ARTS.—9. Modern literature. 10. The fine arts and their application to the useful arts. 11. Bibliography. 12. Obituary notices of distinguished individuals.

prepared under the direction of the Institution, or procured by offering premiums for the best exposition of a given subject.

2. The treatises should in all cases be submitted to a commission of competent judges previous to their publication.

3. As examples of these treatises, expositions may be obtained of the present state of the several branches of knowledge mentioned in the table of reports. Also of the following subjects, suggested by the Committee on Organization, viz: the statistics of labor, the productive arts of life, public instruction, &c.

SECTION II.

Plan of Organization, in accordance with the terms of the resolutions of the Board of Regents, providing for the two modes of increasing and diffusing knowledge.

1. The Act of Congress establishing the Institution contemplated the formation of a library and a museum; and the Board of Regents, including these objects in the plan of organization, resolved to divide the income into two equal parts.

2. One part to be appropriated to increase and diffuse knowledge by means of publications and researches, agreeably to the scheme before given. The other part to be appropriated to the formation of a library and a collection of objects of nature and of art.

3. These two plans are not incompatible with one another.

4. To carry out the plan before described, a library will be required, consisting, 1st, of a complete collection of the transactions and proceedings of all the learned societies in the world; 2d, of the more important current periodical publications, and other works necessary in preparing the periodical reports.

5. The Institution should make special collections, particularly of objects to verify its own publications.

6. Also a collection of instruments of research in all branches of experimental science.

7. With reference to the collection of books, other than those mentioned above, catalogues of all the different libraries in the United States should be procured, in order that the valuable books first purchased may be such as are not to be found in the United States.

8. Also catalogues of memoirs and of books in foreign libraries, and other materials, should be collected for rendering the Institution a centre of bibliographical knowledge, whence the student may be directed to any work which he may require.

9. It is believed that the collections in natural history will increase by donations as rapidly as the income of the Institution can make provision for their reception, and therefore it will seldom be necessary to purchase any articles of this kind.

10. Attempts should be made to procure for the gallery of art, casts of the most celebrated articles of ancient and modern sculpture.

11. The arts may be encouraged by providing a room free of expense for the exhibition of the objects of the Art-Union, and other similar societies.

12. A small appropriation should annually be made for models of antiquities, such as those of the remains of ancient temples, &c.

13. For the present, or until the building is fully completed, besides the Secretary, no permanent assistant will be required, except one to act as librarian.

14. The duty of the Secretary will be the general superintendence—with the advice of the Chancellor and other members of the establishment—of the literary and scientific operations of the Institution; to give to the Regents annually an account of all of the transactions; of the memoirs which have been received for publication; of the researches which have been made; and to edit, with the assistance of the librarian, the publications of the Institution.

15. The duty of the Assistant Secretary, acting as librarian, will be, for the present, to assist in taking charge of the collections, to select and purchase, under the direction of

the Secretary and a committee of the Board, books and catalogues, and to procure the information before mentioned; to give information on plans of libraries, and to assist the Secretary in editing the publications of the Institution and in the other duties of his office.

16. The Secretary and his assistants, during the session of Congress, will be required to illustrate new discoveries in science, and to exhibit new objects of art; also distinguished individuals should be invited to give lectures on subjects of general interest.

17. When the building is completed, and when in accordance with the act of Congress, the charge of the National Museum is given to the Smithsonian Institution, other assistants will be required.*

Explanation and Illustration of the Programme.

Though the leading propositions of the Programme have been fully discussed by the Board, yet it will be important to offer some remarks in explanation and illustration of them in their present connection.

That the Institution is not a national establishment, in the sense in which institutions dependent on the Government for support are so, must be evident when it is recollected that the money was not absolutely given to the United States, but intrusted to it for a special object, namely: the establishment of an institution for the benefit of men, to bear the name of the donor, and consequently to reflect upon his memory the honor of all the good which may be accomplished by means of the bequest. The operations of the Smithsonian Institution ought therefore to be mingled as little as possible with those of the Government, and its funds should be applied exclusively and faithfully to the increase and diffusion of knowledge among men.

That the bequest is intended for the benefit of men in general, and that its influence ought not to be restricted to a

*[Re-printed in Silliman's American Journal of Science, September, 1848, vol. VI (2d series), pp. 288–292.]

single district, or even nation, may be inferred not only from the words of the will, but also from the character of Smithson himself; and I beg leave to quote from a scrap of paper in his own hand the following sentiment bearing on this point: "The man of science has no country; the world is his country—all men his countrymen." The origin of the funds, the bequest of a foreigner, should also preclude the adoption of a plan which does not, in the words of Mr. Adams, "spread the benefits to be derived from the institution not only over the whole surface of this Union, but throughout the civilized world." "Mr. Smithson's reason for fixing the seat of this institution at Washington obviously was, that *there* is the seat of Government of the United States, and *there* the Congress by whose legislation, and the Executive through whose agency, the trust committed to the honor, intelligence, and good faith of the nation, is to be fulfilled." The centre of operations being permanently fixed at Washington, the character of this city for literature and science will be the more highly exalted in proportion as the influence of the Institution is more widely diffused.

That the terms *increase* and *diffusion* of knowledge are logically distinct, and should be literally interpreted with reference to the will, must be evident when we reflect that they are used in a definite sense, and not as mere synonyms, by all who are engaged in the pursuits to which Smithson devoted his life. In England there are two classes of institutions, founded on the two ideas conveyed by these terms. The Royal Society, the Astronomical, the Geological, the Statistical, the Antiquarian Societies, all have for their object the increase of knowledge; while the London Institution, the Mechanics' Institution, the Surrey Institution, the Society for the Diffusion of Religious Knowledge, the Society for the Diffusion of Useful Knowledge, are all intended to diffuse and disseminate knowledge among men. In our own country, also the same distinction is observed in the use of the terms by men of science. Our colleges, academies, and common schools, are recognized as institutions partially intended for the diffusion of knowledge, while the

express object of some of our scientific societies is the promotion of the discovery of new truths.

The will makes no restriction in favor of any particular kind of knowledge; though propositions have been frequently made for devoting the funds exclusively to the promotion of certain branches of science having more immediate application to the practical arts of life, and the adoption of these propositions has been urged on the ground of the conformity of such objects to the pursuits of Smithson; but an examination of his writings will show that he excluded from his own studies no branch of general knowledge, and that he was fully impressed with the important philosophical fact that all subjects of human thought relate to one great system of truth. To restrict therefore the operations of the Institution to a single science or art, would do injustice to the character of the donor, as well as to the cause of general knowledge. If preference is to be given to any branches of research, it should be to the higher and apparently more abstract; to the discovery of new principles rather than of isolated facts. And this is true even in a practical point of view. Agriculture would have forever remained an empirical art, had it not been for the light shed upon it by the atomic theory of chemistry; and incomparably more is to be expected as to its future advancement from the perfection of the microscope than from improvements in the ordinary instruments of husbandry.

The plan of increasing and diffusing knowledge, presented in the first section of the programme, will be found in strict accordance with the several propositions deduced from the will of Smithson, and given in the introduction. It embraces, as a leading feature, the design of interesting the greatest number of individuals in the operations of the Institution, and of spreading its influence as widely as possible. It forms an active organization, exciting all to make original researches who are gifted with the necessary power, and diffusing a kind of knowledge, now only accessible to the few, among all those who are willing to receive it. In this country, though many excel in the application of

science to the practical arts of life, few devote themselves to the continued labor and patient thought necessary to the discovery and development of new truths. The principal cause of this want of attention to original research, is the want, not of proper means, but of proper encouragement. The publication of original memoirs and periodical reports, as contemplated by the programme, will act as a powerful stimulus to the latent talent of our country, by placing in bold relief the real laborers in the field of original research, while it will afford the best materials for the use of those engaged in the diffusion of knowledge.

The advantages which will accrue from the plan of publishing the volumes of the Smithsonian Contributions to Knowledge, are various. In the first place, it will serve to render the name of the founder favorably known wherever literature and science are cultivated, and to keep it in continual remembrance with each succeeding volume, as long as knowledge is valued. A single new truth, first given to the world through these volumes, will forever stamp their character as a work of reference. The contributions will thus form the most befitting monument to perpetuate the name of one whose life was devoted to the increase of knowledge, and whose ruling passion, strong in death, prompted the noble bequest intended to facilitate the labors of others in the same pursuit.

Again, the publication of a series of volumes of original memoirs will afford to the Institution the most ready means of entering into friendly relations and correspondence with all the learned societies in the world, and of enriching its library with their current transactions and proceedings. But perhaps the most important effect of the plan will be that of giving to the world many valuable memoirs, which on account of the expense of the illustrations could not be otherwise published. Every one who adds new and important truths to the existing stock of knowledge must be of necessity to a certain degree in advance of his age. Hence the number of readers and purchasers of a work is generally in the inverse ratio of its intrinsic value; and

consequently authors of the highest rank of merit are frequently deterred from giving their productions to the world on account of the pecuniary loss to which the publication would subject them. When our lamented countryman, Bowditch, contemplated publishing his Commentary on La Place he assembled his family and informed them that the execution of this design would sacrifice one-third of his fortune, and that it was proper his heirs should be consulted on a subject which so nearly concerned them. The answer was worthy the children of such a father: "We value," said they, "your reputation more than your money." Fortunately in this instance the means of making such a sacrifice existed, otherwise one of the proudest monuments of American science could not have been given to the world. In the majority of cases however those who are most capable of extending human knowledge are least able to incur the expense of the publication. Wilson, the American ornithologist, states in a letter to Michaux that he has sacrificed everything to publish his work: "I have issued," he says, "six volumes and am engaged on the seventh, but as yet I have not received a single cent of the proceeds." In an address on the subject of natural history by one of our most active cultivators of this branch of knowledge we find the following remarks, which are directly in point: "Few are acquainted with the fact that from the small number of scientific works sold, and the great expense of plates, our naturalists not only are not paid for their labors but suffer pecuniary loss from their publications. Several works on different branches of zoology now in the course of publication will leave their authors losers by an aggregate of $15,000. I do not include in this estimate works already finished—one, for instance, the best contribution to the natural history of man extant, the publication of which will occasion its accomplished author a loss of several thousand dollars. A naturalist is extremely fortunate if he can dispose of two hundred copies of an illustrated work, and the number of copies printed rarely exceeds two hundred and fifty." It may be said that these authors have their reward in the reputation which they thus purchase; but

reputation should be the result of the talents and labor expended in the production of a work, and should not in the least depend upon the fact that the author is able to make a pecuniary sacrifice in giving the account of his discoveries to the public.

Besides the advantage to the author of having his memoir published in the Smithsonian Contributions free of expense, his labors will be given to the world with the stamp of approval of a commission of learned men, and his merits will be generally made known through the reports of the Institution. Though the premiums offered may be small, yet they will have considerable effect in producing original articles. Fifty or a hundred dollars awarded to the author of an original paper will in many instances suffice to supply the books, or to pay for the materials, or the manual labor required in prosecuting the research.

There is one proposition of the programme which has given rise to much discussion, and which therefore requires particular explanation. I allude to that which excludes from the contributions all papers consisting merely of unverified speculations on subjects of physical science. The object of this proposition is to obviate the endless difficulties which would occur in rejecting papers of an unphilosophical character; and though it may in some cases exclude an interesting communication, yet the strict observance of it will be found of so much practical importance that it cannot be dispensed with. It has been supposed from the adoption of this proposition that we are disposed to undervalue abstract speculations; on the contrary, we know that all the advances in true science — namely, a knowledge of the laws of phenomena — are made by provisionally adopting well-conditioned hypotheses, the product of the imagination, and subsequently verifying them by an appeal to experiment and observation. Every new hypothesis of scientific value must not only furnish an exact explanation of known facts, but must also enable us to predict in kind and quantity—the phenomena which will be exhibited under any given combination of circumstances. Thus, in the case of the undulatory hypothesis of light, it was inferred as a log-

ical consequence that if the supposition were true that light consisted of waves of an ætherial medium, then two rays of light like two waves of water under certain conditions should annihilate each other, and darkness be produced. The experiment was tried, and the anticipated result was obtained. It is this exact agreement of the deduction with the actual result of experience that constitutes the verification of an hypothesis, and which alone entitles it to the name of a theory, and to a place in the transactions of a scientific institution. It must be recollected that it is much easier to speculate than to investigate, and that very few of all the hypotheses imagined are capable of standing the test of scientific verification.

As it is not our intention to interfere with the proceedings of other institutions, but to co-operate with them so far as our respective operations are compatible, communications may be referred to learned societies for inspection, and abstracts of them given to the world through the bulletins of these societies; while the details of the memoirs and their expensive illustrations are published in the volumes of the Smithsonian Contributions. The officers of several learned societies in this country have expressed a willingness to co-operate in this way.

Since original research is the most direct way of increasing knowledge, it can scarcely be doubted that a part of the income of the bequest should be appropriated to this purpose, provided suitable persons can be found, and their labors be directed to proper objects. The number however of those who are capable of discovering scientific principles is comparatively small; like the poet, they are "born, not made," and, like him, must be left to choose their own subject, and wait the fitting time of inspiration. In case a person of this class has fallen on a vein of discovery, and is pursuing it with success, the better plan will be to grant him a small sum of money to carry on his investigations, provided they are considered worthy of assistance by competent judges. This will have the double effect of encouraging him in the pursuit, and of facilitating his progress. The Institution however need not depend upon cases of

this kind even if they were more numerous than they are, for the application of its funds in the line of original research. There are large fields of observation and experiment the cultivation of which, though it may afford no prospect of the discovery of a principle, can hardly fail to produce results of importance both in a practical and a theoretic view. As an illustration of this remark, I may mention the case of the investigations made a few years ago by a committee of the Franklin Institute of Philadelphia. The Secretary of the Treasury of the United States placed at the disposal of this society a sum of money for the purpose of making experiments with reference to the cause of the explosion of steam boilers. A committee of the society was chosen for this purpose which adopted the ingenious plan of writing to all persons in the United States engaged in the application of steam, and particularly to those who had observed the explosion of a steam-boiler. In this way opinions and suggestions in great variety as to the cause of explosions were obtained. The most plausible of these were submitted to the test of experiment; the results obtained were highly important, and are to be found favorably mentioned in every systematic work on the subject of steam which has appeared in any language within the last few years. New and important facts were established; and what was almost of as much consequence, errors which had usurped the place of truth were dethroned.

In the programme examples are given of a few subjects of original research to which the attention of the Institution may be turned. I will mention one in this place, which may deserve immediate attention. I allude to a small appropriation made annually for researches with reference to the remains of the ancient inhabitants of our country. This is a highly interesting field, and what is done in regard to it should be done quickly. Every year the progress of civilization is obliterating the ancient mounds; cities and villages are rising on the spots they have so long occupied undisturbed; and the distinctive marks of these remains are every year becoming less and less legible.

In carrying out the spirit of the plan adopted, namely

that of affecting men in general by the operations of the Institution, it is evident that the principal means of diffusing knowledge must be the *Press*. Though lectures should be given in the city in which Smithson has seen fit to direct the establishment of his Institution, yet as a plan of general diffusion of knowledge the system of lectures would be entirely inadequate; every village in our extended country would have a right to demand a share of the benefit, and the income of the Institution would be insufficient to supply a thousandth part of the demand. It is also evident that the knowledge diffused should if possible not only embrace all branches of general interest, so that each reader might find a subject suited to his taste, but also that it should differ in kind and quality from that which can be readily obtained through the cheap publications of the day. These requisites will be fully complied with in the publication of the series of reports proposed in the programme. A series of periodicals of this kind, posting up all the discoveries in science from time to time, and giving a well digested account of all the important changes in the different branches of knowledge is a desideratum in the English language. The idea is borrowed from a partial plan of this kind in operation in Sweden and Germany; and for an example of what the work should be I would refer to the annual report to the Swedish Academy of its perpetual secretary, Berzelius, on physical science. The reports can be so prepared as to be highly interesting to the general reader and at the same time of great importance to the exclusive cultivator of a particular branch of knowledge. Full references should be given in foot-notes to the page, number or volume of the work from which the information was obtained, and where a more detailed account can be found. It is scarcely necessary to remark that the preparation of these reports should be entrusted only to persons profoundly acquainted with the subjects to which they relate—namely, to those who are devoted to particular branches while they possess a knowledge of general principles. Sufficient explanations should be introduced to render the report intelligible to the general reader without

destroying its scientific character. Occasionally reports may be obtained from abroad—as for example accounts of the progress of certain branches of knowledge in foreign countries, and these may be translated if necessary and incorporated into other reports by some competent person in this country.

Besides the reports on the progress of knowledge, the programme proposes to publish occasionally brief treatises on particular subjects. There are always subjects of general interest of which brief expositions would be of much value. The preparation of these however should be intrusted to none but persons of character and reputation, and should be subjected to a revision by competent and responsible judges before they are given to the public. They may be presented in the form of reports on the existing state of knowledge relative to a given subject, and may sometimes consist of memoirs and expositions of particular branches of literature and science, translated from foreign languages. The reports and treatises of the Institution, sold at a price barely sufficient to pay the expenses of printing, will find their way into every school in our country, and will be used not as first lessons for the pupil, but as sources of reliable information for the teacher.

The second section of the programme gives, so far as they have been made out, the details of the part of the plan of organization directed by the act of Congress establishing the Institution. The two plans, namely, that of publication and original research, and that of collections of objects of nature and art, are not incompatible, and may be carried on harmoniously with each other. The only effect which they will have on one another is that of limiting the operation of each, on account of the funds given to the other. Still, with a judicious application and an economical expenditure of the income, and particularly by rigidly observing the plan of finance suggested by Dr. Bache, in the construction of the building, much good may be effected in each of the two branches of the Institution. To carry on the operations of the first a working library will be required, consisting of the past volumes and the transactions and proceedings of all the

learned societies in every language. These are the original sources from which the most important principles of the positive knowledge of our day have been drawn. We shall also require a collection of the most important current literature and science for the use of the collaborators of the reports; most of these however will be procured in the exchange for the publications of the Institution, and therefore will draw but little from the library fund.

The collections of the Institution, as far as possible, should consist of such articles as are not elsewhere to be found in this country, so that the visitors at Washington may see new objects, and the spirit of the plan be kept up, of interesting the greatest possible number of individuals. A perfect collection of all objects of nature and of art, if such could be obtained and deposited in one place, would form a museum of the highest interest; but the portion of the income of the bequest which can be devoted to the increase and maintenance of the museum will be too small to warrant any attempt toward an indiscriminate collection. It is hoped that in due time other means may be found of establishing and supporting a general collection of objects of nature and art at the seat of the General Government, with funds not derived from the Smithsonian bequest. For the present it should be the object of the Institution to confine the application of the funds, first, to such collections as will tend to facilitate the study of the memoirs which may be published in the Contributions, and to establish their correctness; secondly, to the purchase of such objects as are not generally known in this country, in the way of art and the illustration of antiquities, such as models of buildings, &c.; and thirdly, to the formation of a collection of instruments of physical research which will be required both in the illustration of new physical truths and in the scientific investigations undertaken by the Institution.

Much popular interest may be awakened in favor of the Institution at Washington by throwing the rooms of the building open on stated evenings during the session of Congress for literary and scientific assemblies, after the manner of the weekly meetings of the Royal Institution in London.

At these meetings, without the formality of a regular lecture, new truths in science may be illustrated, and new objects of art exhibited. Beside these, courses of lectures may be given on particular subjects by the officers of the Institution, or by distinguished individuals invited for the purpose. - - -

Preparations have been made for instituting various lines of physical research. Among the subjects mentioned in the programme as an example for the application of the funds of the Institution is terrestrial magnetism. I need scarcely say that this is a subject of high interest not only in a theoretical point of view, but also in its direct reference to navigation, and to the various geodetical operations of civil and military life. - - -

Another subject of research mentioned in the programme, and which has been urged upon the immediate attention of the Institution, is that of an extensive system of meteorological observations, particularly with reference to the phenomena of American storms. Of late years, in our country, more additions have been made to meteorology than to any other branch of physical science. Several important generalizations have been arrived at, and definite theories proposed, which now enable us to direct our attention with scientific precision to such points of observation as cannot fail to reward us with new and interesting results. It is proposed to organize a system of observations which shall extend as far as possible over the North American continent; and in order to effect this, it will be necessary to engage the co-operation of the British Government. - - -

The present time appears to be peculiarly auspicious for commencing an enterprise of the proposed kind. The citizens of the United States are now scattered over every part of the southern and western portion of North America, and the extended lines of telegraph will furnish a ready means of warning the more northern and eastern observers to be on the watch for the first appearance of an advancing storm.*

* [Reprinted in Silliman's American Journal of Science, November, 1848, vol. VI, pp. 305–317.]

ON HEAT, AND ON A THERMAL TELESCOPE.*

(Silliman's American Journal of Science, January, 1848, vol. v, pp. 113, 114.)

Professor Henry showed the analogy between light and heat, by stating that as two rays of *light* might be so opposed as to produce *darkness*, so two rays of *heat* might be so opposed as to produce *cold*. The facts with regard to heat as well as light therefore show that the theory of undulation is not an imagination, but the expression of a *law*. The minimum of heat, as proved by his experiments with the thermo-electric pile, does not correspond with the minimum of light. Among flames there are many which give but little light, but which give great heat; as for example the flame of hydrogen. The amount of *radiant* heat and *radiant* light were found to be about the same.

The spots on the sun are colder than the surrounding surface; and its surface is variously heated. This result he obtained by a very simple experiment of throwing the disc of the sun on a screen, and placing the very sensitive thermo-electric pile before its different parts. He had not yet concluded his experiments on the sun, and had not measured the comparative heating powers of the centre and circumference, from the results of which observations very important consequences would be drawn.

This apparatus he fitted to a common pasteboard tube, covered with gilt paper externally, and blackened internally, with which he measured the heat of distant objects. He could detect the heat of a man's face a mile off; that of a house five miles off. He thus discovered that the coldest spot of the sky is at the zenith. One day, on directing his

*[An abstract of a paper read before the Association of American Geologists and Naturalists, at its eighth annual meeting, held at Boston, September, 1847. The only published report however of the proceedings of this meeting of the Association appears to be that given in Silliman's American Journal of Science, above quoted. At this meeting, it was agreed by the body to resolve itself into the "American Association for the Advancement of Science;" and the new organization held its first meeting at Philadelphia on the following year, September 20, 1848.]

tube to a cloud, from which flashes of lightning proceeded, he was astonished to find it indicated a great degree of *cold;* he afterwards found out that a considerable quantity of hail had fallen from this cloud.

He was not satisfied with the appearances of heat supposed to have been derived from the moon. The heat that other observers have got, is probably the reflected heat of the sun, and not the moon's proper heat.

PRACTICAL OPERATIONS OF THE SMITHSONIAN INSTITUTION.

(From the Second Annual Report of the Secretary to the Board of Regents.)

December 13, 1848.

GENTLEMEN: By a resolution of the Board of Regents, at their last annual meeting, I was charged with the execution of the details of the programme which had been provisionally adopted, and was directed to report annually to the Board the progress made in the execution of the duty assigned to me. In accordance with this resolution, I present the following statement of the operations of the past year. - - -

It was recommended in my last report that the details of the plan should be adopted provisionally, and should be carried into operation gradually and cautiously, with such changes, from time to time, as experience might dictate. The Institution is not one of a day, but is designed to endure as long as our Government shall exist; and it is therefore peculiarly important that in the beginning we should proceed carefully, and not attempt to produce immediate effects at the expense of permanent usefulness. The process of increasing knowledge is an extremely slow one, and the value of the results of this part of the plan cannot be properly realized until some years have elapsed. - - -

In the publication of the first volume of the Contributions, the question occurred as to the propriety of securing the copyright to the Institution. I had not an opportunity of conferring with the Executive Committee on this point, and was therefore obliged to settle it on my own responsibility.

I concluded that it would be more in accordance with the spirit of the Institution to decide against the copyright. The knowledge which the Smithsonian Institution may be instrumental in presenting to the world should be free to all who are capable of using it. The re-publication of our papers ought to be considered as an evidence of their importance, and should be encouraged rather than prohibited. - - -

An appropriation of one thousand dollars was made at the last meeting of the Board, for the commencement of a series of meteorological observations, particularly with reference to the phenomena of American storms.

It is contemplated to establish three classes of observers among those who are disposed to join in this enterprise. One class, without instruments, to observe the face of the sky as to its clearness, the extent of cloud, the direction and force of wind, the beginning and ending of rain, snow, &c. A second class, furnished with thermometers, who, besides making the observations above mentioned, will record variations of temperature. The third class, furnished with full sets of instruments, to observe all the elements at present deemed important in the science of meteorology. It is believed that much valuable information may be obtained in this way with reference to the extent, duration, and passage of storms over the country, though the observer may be possessed of no other apparatus than a simple wind-vane.

As a part of the system of meteorology, it is proposed to employ, as far as our funds will permit, the magnetic telegraph in the investigation of atmospherical phenomena. By this means, not only notice of the approach of a storm may be given to distant observers, but also attention may be directed to particular phenomena, which can only be properly studied by the simultaneous observations of persons widely separated from each other. For example, the several phases presented by a thunder storm, or by the aurora borealis, may be telegraphed to a distance, and the synchronous appearances compared and recorded in stations far removed from each other. Also, by the same means, a single observatory, at which constant observations are made during

the whole twenty-four hours, may give notice to all persons along the telegraphic lines, of the occurrence of interesting meteorological phenomena, and thus simultaneous observations be secured. The advantage to agriculture and commerce to be derived from a knowledge of the approach of a storm, by means of the telegraph, has been frequently referred to of late in the public journals. And this, we think, is a subject deserving the attention of the General Government. - - -

Under the head of original researches, I may recall to the attention of the Regents the fact of my having been directed to continue my own investigations on physical science, and to report occasionally to the Board my progress therein. In the course of last year, I found an opportunity while at Princeton to commence a series of investigations on radiant heat, which apparently produced some results of interest, but which my subsequent engagements have prevented me from fully developing. I was also directed to cause to be made a series of experiments on the economical value of building material. - - -

The Smithsonian Contributions are intended to consist of entirely original additions to the sum of human knowledge, and are to be principally exchanged for the transactions of learned societies, and to be distributed among public institutions. The reports, on the other hand, are to be of a more popular kind, and are intended for as wide a distribution as the funds of the Institution, or the means of publishing them may permit. They will give an account of the progress of the different branches of knowledge in every part of the world, and will supply a desideratum in English literature.

The objects of the Smithsonian Institution are not educational. The press in our country already teems with elementary works on the different branches of knowledge, and to expend our funds in adding to these, would be to dissipate them without perceptible effect.

ON THE AURORA BOREALIS.*

(Proceedings American Association, Adv. of Science, vol. II, pp. 11, 12.)

August 14, 1849.

Professor Henry said: The paper of Professor Secchi seems to me to be one of considerable interest. It contains a number of ingenious suggestions, which may lead to new results. One fact alluded to in this paper is highly important, and though taken for granted since the days of Franklin, has only lately been fully established. I allude to the connection of the Aurora with electricity. Besides the observation mentioned in the preceding paper, I am informed by Mr. Herrick, of New Haven, that an electrical action had been observed at that place on the wires of the telegraph at the time of the appearance of the Aurora. The same fact has also been observed in England and on the continent, during the last year. It is highly desirable to ascertain whether this action is one of actual transfer of electricity from the space at one end of the wire to that at the other, or whether it is an inductive action of the Aurora at a distance, disturbing for an instant the electrical equilibrium of the wire. This could be readily determined by the character of the action on the needle of a galvanometer.

There was an Aurora last night visible at this place, which exhibited some peculiarities not frequently observed, (so far as I am informed) in this latitude. These were pointed out to me by Dr. A. D. Bache, and are similar in a degree to the appearances observed in Siberia. The Aurora, in these high latitudes, frequently presents the appearance of a number of concentric scrolls or curtains, the general axis of which is parallel to the dipping-needle. The Aurora of last night consisted, while we were observing it, of a number of parallel beams which together formed the skeleton of an arch with an irregular curtain border at the lower edge.

I may mention to the Association that the Smithsonian Institution, in connection with an extended system of meteor-

*[Remarks on a communication by Professor Angelo Secchi, of Georgetown College, D. C., to the Association, on "The Aurora Borealis."]

ology which it has undertaken to establish, has issued directions for the observation of the Aurora. These directions are similar to a set issued by the directors of the observatory at Toronto, for observers in Canada. The observations made in the two countries will thus form one extended system. The proprietors of the several telegraph lines have offered to grant us the use of their wires for meteorological purposes, and it is hoped when the lines are completed, and we have established a set of observers, extending, for example, from Toronto to Washington, or even farther south, we shall be able to study the phenomenon of the Aurora with more precision than it has ever been studied. On a long line extending north and south, the observer, for example, at Toronto, having noticed an Aurora, may call the attention to it of all the observers along the line, and thus the extent of the visibility, and the simultaneous appearance of any peculiar phase of the meteor, may be readily determined.

ON THE DIFFUSION OF VAPOR.*

(Proceedings American Association, Adv. of Science, vol. II, pp. 127, 128.)

August 16, 1849.

Professor Henry remarked that he was much interested in the experiment of Professor Horsford, in which vapor was shown to pass through a tube filled with air. It is well known that, according to the theory of Dalton, air and vapor are vacuums to each other. This theory is certainly in accordance with all the statical phenomena of the diffusion of vapor, but does not as well represent the dynamic effects. So great is the resistance to diffusion through a narrow tube, that Professor Espy has concluded that the theory is incorrect, and that diffusion of vapor cannot take place without the aid of a current of air. Professor Horsford's experiment proves that a diffusion does take place through a tube, but in this case the force of diffusion may be considered a maximum.

*[Remarks on a communication by Professor E. N. Horsford, to the Association, "On the Moisture, Ammonia, and Organic Matter of the Atmosphere."]

If the force is much less, the effect does not take place. Several years ago I placed a small quantity of water in a retort, and joined the beak of this to the open beak of another retort filled with air. The retort containing water was placed within a room kept constantly at a mean temperature of about 65°, while the body of the other retort was without a window, and constantly at a mean temperature of not more than 40°. Though the apparatus was suffered to remain thus, during a whole winter, not a single drop of water passed over. The force of diffusion due to the difference of tension in the two retorts was in this case too small to overcome the resistance of the atoms to a passage between each other.

ON THE RADIATION OF HEAT.

(Proceedings of the American Philosophical Society, vol. v, p. 108.)

October 19, 1849.

Professor Henry communicated some experiments which he had made upon the subject of the radiation of heat. It occurred to him, from the constitution of the atmosphere, that if the air were a good radiator of heat, the higher temperatures below and the lower above could not be permanent. By placing a thermo-multiplier before a flame, interposing a screen of wood with a hole through it, radiation from the flame was perceived becoming less as the flame was lowered, and still existing, though in small quantities, from the heated air above the flame. He also repeated the experiments upon the radiation of heat from flames. The radiation of heat from the flame of hydrogen is but small, as is its radiation of light. This radiation is much increased by placing a solid in the flame. This is in accordance with Count Rumford's assertion, that clay balls placed in the fire increased the amount of heat.

Professor Henry also mentioned some experiments which he had made some years ago upon the reflection of heat from ice with a concave mirror of that substance.

ON THE EXPANSIVE ENERGY OF LIGHTNING STROKES.*

(Proceedings American Association, Adv. of Science, vol. IV, pp. 7, 10.)

August 19, 1850.

Professor Henry mentioned an instance of an explosion during the passage of an electrical discharge through a house, from which fact he had been led to the same conclusions on this point with Professor Olmsted. He had himself made a series of experiments to ascertain whether the hypothesis is true or not. The results were attained by means of Kinnersley's air thermometer. His investigations convinced him that the effect is due to a sudden repulsive energy imparted to the air. He cited several instances—some of which were noticed by himself at Princeton, where the roof of one house was blown off, and the side of another blown out. He considered that the great mechanical effects of an electrical discharge are due in most cases to an expansive or repulsive power in the air. He had made some interesting experiments in galvanism, the effects of which he referred to the same cause. - - -

Professor Henry mentioned instances where ordinary electrical discharges had affected a circle of twenty miles in diameter. By means of an apparatus simply constructed for the occasion, he had succeeded in magnetizing a needle by a flash of lightning so far off that he could not hear the thunder. He explained the apparatus. He considered that every flash of such electricity produces effects to great distances, and may perhaps effect half the globe.

*[Remarks on a communication, by Professor Dennison Olmsted, of Yale College, to the Association, on "Notes of some points of Electrical Theory." Professor Olmsted illustrated by several observations the expansive energy of lightning strokes, and also the occurrence of the lateral discharge from good conductors well connected with the earth.]

ON THE FORMS OF LIGHTNING-RODS.*

(Proceedings American Association, Adv. of Science, vol. IV, pp. 39–42.)

August 20, 1850.

Professor Henry said the question of balls and points had not been fully settled. If electricity acts inversely as the square of the distance, then on the principle of central forces, the induction on a sphere at a distance from the cloud would be the same as if all the matter of the sphere were concentrated in its centre, and consequently the attraction of the ball or sphere on the electricity of the distant cloud would be the same as that of a point. When however the inducing body, or the discharge itself, came near the rod, it would be much more strongly deflected by the point than by the ball, because the former would be electrified by induction to a much greater degree of intensity, for the same amount of electricity which would be diffused over the surface of the ball would be condensed in the point, and hence it would tend to rupture the air, and thus give a more easy passage to the discharge.

His attention had been directed to the action on a ball, by the fixture on the dome of the Capitol at Washington, of a lantern, terminated by a ball. This apparatus had been erected at a great expense, for the purpose of lighting the public grounds. It consisted of a mast reaching to the height of ninety feet above the apex of the dome of the Capitol, terminated by a lantern about five feet in diameter and six or seven feet high. In this were jet gas burners, equal in illuminating power, according to the statement of the projector of the arrangement, to six thousand wax candles.

After the whole apparatus had been prepared, the speaker was requested to give an opinion as to the effect which the lightning might have upon it. His answer was, that

*[Remarks on a communication by Professor Elias Loomis, to the Association, "On the proper height of Lightning-rods;" in which a reference was made to the question of single or multiple points to the rod.]

it would attract the lightning from the heavens, and though the building might be protected by good conductors from the lantern to the earth, yet no protection which the present state of science could devise would be as safe as no exposure; the very idea of protection involving that of a less degree of danger. Though in the case of the ordinary lightning-rod the lightning is seldom or never attracted from the cloud by the conductor, yet in this case the great height of the mast, the height of the dome above the ground, and the elevated position of the building itself, gave a total elevation bearing a considerable ratio to the height of the cloud: add to this, the great amount of metallic surface, and, above all, the large gas burner, and we have an arrangement well calculated to elicit a discharge from the cloud, when under ordinary influences no effect of the kind would take place. It is well known to the Section that the best apparatus for collecting atmospheric electricity is a long pole, with a wire along it, and a lantern at the upper end. The fixture on the Capitol was indeed an exploring apparatus on a magnificent scale. The result was such as had been anticipated. The first thunder-storm which passed over the city after the erection of the lantern, discharged itself upon it, put out the light, and when the whole was taken down, several perforations were found melted in the copper ball which surmounted the lantern.

In this case the induction from the cloud took place over the whole surface of the lantern, and the attraction was in proportion to the number of particles in the surface of the metal. The principal action was however due to the stream of heated air from the burning gas. - - -

[Professor Olmstead mentioned the case of several inverted tin pans placed in a straight line on a bench in the path of the electric discharge, and that they were perforated on opposite sides as if by a bullet.]

Professor Henry thought the phenomenon was in accordance with known electrical action. If a number of conductors are placed in succession in the path of a discharge, the end of the first, to which the lightning is passing, will be-

come highly negative, while the other end of the same conductor must be highly positive; also, the first end of the second conductor will be negative, and the other end positive, and so on. The lightning therefore will enter the metal with much greater intensity than that with which it will pass along the conductor; and hence a hole may be melted at the point of entrance; for the same reason another hole might be expected at the point of exit, and in this way the perforations of the pans might be explained. The electricity did not pass through the space from side to side of the pan, as a bullet would have done, but took the circuit around the inverted bottom of the vessel.

He stated that in all cases when an electrical discharge passes through a conductor, the point at which the fluid enters, and that at which it passes out, are both marked with evidence of more intense action.

When a disruptive discharge takes place through the air between two conductors, in many cases a part of the matter of each conductor is transferred to the other. Professor Henry said that he had received accounts from different sources of a remarkable phenomenon connected with this action. In the case of a person killed many years ago by lightning, while standing near to the whitewashed wall of a room, the discharge took place between his body and the wall, and on the latter was depicted, in dark color, an image of his person. Other cases of the same kind had been observed.

ON THE PHENOMENA OF THE LEYDEN JAR.

(Proceedings American Association, Adv. of Science, vol. IV, pp. 377, 378.)

August 24, 1850.

Professor Henry gave an account of his investigation of the discharge of a Leyden jar. This was a part of a series of experiments he had made a few years ago on the general subject of the dynamic phenomena of ordinary or frictional electricity. On this subject he had made several thousand experiments. He had never published these in full, but had given brief notices of some of them in the Proceedings of

the American Philosophical Society. All the complex phenomena he had observed could be referred to a series of oscillations in the discharge of the jar. If we adopt the hypothesis of a single fluid, then we shall be obliged to admit that the equilibrium of the fluid after a discharge takes place by a series of oscillations, gradually diminishing in intensity and magnitude. He had been enabled to show effects from five of these waves in succession. The means used for determining the existence of these waves was that of the magnetization of steel needles, introduced into the axis of a spiral. A needle of this kind it is well known is susceptible of receiving a definite amount of magnetism, which is called its saturation. Now if the needle be of such a size as to be magnetized to saturation by the principal discharge, it will come out of the spiral magnetized to a less degree than that of saturation, by the amount of the adverse influence of the oscillations in the opposite direction to that of the principal discharge. If the quantity of electricity be increased, the power of the second wave may be so exalted that the needle will exhibit no magnetism; the whole effect of the first or principal wave will be neutralized by the action of the second. If the quantity of electricity be greater than this, then the needle will be magnetized in an opposite direction. If the electricity be still more increased, the needle will again exhibit a change in its polarity, and so on in succession, as the power of the successive waves is increased.

These experiments had been made several years ago, but he had not given them in detail to the public, because he had wished to render them more perfect. For the last three and a half years all his time and all his thought had been given to the details of the business of the Smithsonian Institution. He had been obliged to withdraw himself entirely from scientific research; but he hoped—now the Institution had got under way, and the Regents had allowed him some able assistants,—that he would be enabled, in part at least, to return to his first love—the investigation of the phenomena of nature.

ON THE LIMIT OF PERCEPTIBILITY OF A DIRECT AND REFLECTED SOUND.

(Proceedings American Association Adv. of Science, vol. v, pp. 42, 43.)

May, 6, 1851.

Professor Henry stated that at the meeting of the Association at Cambridge* he had made a communication relative to the application of the principles of acoustics to the construction of rooms intended for public speaking. In that communication he had stated, as an important proposition, that when two portions of the same sonorous wave reach the ear of an auditor,—one directly from the origin of the sound, and the other indirectly,—after one or more reflections, if the two do not differ in the paths they travel by a difference greater than a given quantity, the two sounds will re-enforce each other, and one louder sound will be perceived. If however the interval is greater than a certain limit, the two sounds will appear distinct, or an echo will be perceived.

As an illustration, suppose a speaker to stand before a wall at the distance of say ten feet: in this case the audience in front would hear but one sound. The direct and the reflected impulse meet the ear within the limit which he has called the limit of perceptibility. This limit—a knowledge of which is of considerable practical importance—may either be expressed in time or in space. The simplest method of obtaining its amount is that of clapping the hands, while standing before a perpendicular wall; if the distance of the observer be sufficient, an echo will be heard. If in this case the observer gradually approach the wall and continue to make the sound, at a definite point the echo will cease to be perceived, and the two sounds will appear as one. If the distance from the wall be now measured, twice the distance found will give the limit of perceptibility in space. If the same quantity be divided into the space through which the

*[This communication, made August 21, 1849, was reported by title only.—*Proceed. Am. Assoc.*, vol. II, p. 432.]

wave of sound is known to travel in a second, we shall have the limit of perceptibility in time.

The foregoing plan is the most simple—but not the most accurate—method of arriving at the quantity sought. The better plan is to employ another person to produce the sound, while the observer is stationary at the distance—at least 150 feet from the wall. The person who produces the sound being placed between the observer and the wall, at such a distance from the latter as to give a distinct echo, he is then directed gradually to approach the wall until the echo and the direct sound become one. The distance measured, as before mentioned, will give the limit required.

From a series of experiments on this plan, he found the limit of perceptibility to vary from about 60 to 80 feet, or in other words, the distance from the wall at which the echo ceased was from 30 to 40 feet. This will give from the $\frac{1}{20}$ to the $\frac{1}{15}$ part of a second, in time, for the ear to distinguish the difference of two successive sounds.

The experiments, when made under the same circumstances, gave the same result, almost within a single foot; but when a different source of sound was employed and different observers, there was observed a difference of results, giving the limits between $\frac{1}{20}$ and $\frac{1}{15}$ of a second. The limit was less with a sound produced by an instrument which gave a sudden crack, without perceivable prolongation, such as is produced by an ordinary watchman's rattle when made to emit a single crack. This difference may be explained by taking into consideration the actual length of the sonorous wave. If a sound occupies $\frac{1}{4}$ of a second, (which is about the time required for the utterance of a short, single syllable,) the length of this sonorous wave will be about 300 feet, and hence, when the distance travelled by the two sounds is not more than 80 feet to and from the wall, the two waves must overlap through a considerable portion of their whole length, and will be only separated at the two extremities. The portion of over-lapping may therefore determine the limit of perceptibility, and this again is combined with the fact of the continuance of a sonorous impression on the nerve of the ear.

ON THE THEORY OF THE SO-CALLED IMPONDERABLES.

(Proceedings American Association Adv. of Science, vol. VI, pp. 84–91.)

August 21, 1851.

Professor Henry said: In studying the phenomena of matter we commence with observing the action of masses upon each other, and from this we deduce laws. These, with regard to mechanical philosophy, are five in number, viz., the two laws of force, attraction and repulsion, varying with some function of the distance; and secondly, the three laws of motion, viz., the law of inertia, of the co-existence of motions, and of action and re-action. Of these laws we can give no explanation; they are at present considered as ultimate facts to which all mechanical phenomena are referred, or from which they are deduced by logical inference. The existence of these laws, as has been said, is deduced from the phenomena of the operations of matter in masses; but we apply them by analogy to the minute and invisible portions of matter which constitute the atoms or molecules of gases, and we find that the inferences from this assumption are borne out by the results of experience.

Indeed, the minutest portions of matter must be endowed with properties analogous to masses of the same kind of matter. An attempt has however been made by Boscovich to refer all the mechanical properties of matter to portions of space, filled with associated points, endowed with attracting and repelling forces, varying and alternating with changes of distances. In a communication to the American Philosophical Society,* I have shown that this hypothesis, which is at the present time adopted by many, is insufficient to explain all the facts. Matter thus constituted would indeed exhibit the phenomena of elasticity, compressibility, porosity, affinity, etc.; but it would not exhibit an obedience to the three laws of motion, namely, inertia, the co-existence or composition of motions, and action and re-action. We must

*[November 6, 1846. See *ante*, p. 255.]

therefore superadd to the hypothetical points of Boscovich these other conditions; but in so doing we arrive at a constitution of matter precisely similar to that adopted by Newton, namely, a system of indivisible and indestructible atoms endowed with the essential properties of matter in masses. Indeed, this is the only hypothesis which we can adopt in strict accordance with analogy, reasoning from the known to the unknown.

Besides the phenomena of the action of invisible atoms of gases on each other we have a large class known under the general name of the phenomena of the "imponderables." This name has been given because it is supposed that it is necessary to refer them to hypothetical fluids not subjected to the ordinary laws of force and motion. The term *imponderable* however expresses a quality with reference to the constitution of such fluids not warranted by the facts. A mass of air poised in air has no weight, and in this case may be considered imponderable. In the same way, if we suppose an elastic medium to pervade all space, any portion of this will be imponderable, even were our balances sufficiently delicate to detect its absolute weight. The existence of an elastic medium pervading all space is assumed in order that the phenomena of light, heat, electricity, and magnetism may be brought within the category of the laws of force and motion, and that we may be able to apply the principles of analytical mechanics in the way of deducing consequences to be afterwards tested by an actual appeal to experiment. Without assumptions of this kind it is impossible to arrive at the general expressions which constitute science in the proper sense of the term.

It is not necessary that an hypothesis be absolutely true in order that it may be adopted as an expression for a generalization for the purpose of explaining and predicting new phenomena; it is only necessary that it should be well conditioned in accordance with known mechanical principles. We have a remarkable instance of this in the Newtonian theory of emission of light. According to this, light is first considered as consisting of atoms of matter moving with

immense velocity, but subject to mechanical laws. The inference from this assumption is, that meeting obliquely a reflecting surface the atoms will rebound as would a perfectly elastic ball, making the angle of incidence equal to the angle of reflection. This fact being established by experiment, all the phenomena of reflected light are deduced mathematically as mechanical consequences from the primary assumption. Again, it is discovered that a ray of light, in entering obliquely a new medium, changes its direction; and this is readily explained by adding to the previous hypothesis the second condition, that the atoms of light, like all other matter, are subject to attraction, and that they are, in consequence of this, accelerated or retarded in velocity at the moment of entering the new medium. From this assumption readily flows the law of the permanency of the ratio of the sine of the angle of refraction to that of incidence.

In the progress of discovery it is further found that a ray of light is separated into different colors; and in order to explain this agreeably to the same analogies, we are obliged to admit that there are different kinds of atoms of light, with different properties, and moving with different velocities. Further, it is discovered that light, in passing by the edges of different bodies, produces fringes and other phenomena known by the name of diffraction. To explain these another supplementary hypothesis must be added, namely, that the atoms of light are alternately attracted and repelled by the variation in their distance from the solid body near which they pass. Another class of phenomena, denominated by Newton "fits of easy refraction and easy reflection," induce the assumption that the atoms of light are not homogeneous in property on all sides, but that each possesses an attracting and repelling pole; and that in their passage through space they are constantly revolving on axes perpendicular to the line joining their poles. Again, the discovery of Malus requires another supplementary hypothesis in order to a mechanical conception of the phenomena first observed by him. To explain these we must admit that the atoms of light possess different properties on different sides, in addition

to different properties at different ends. But now the original theory of emission, at first a simple mechanical conception, becomes so loaded with supplementary hypotheses that as a whole it is unwieldy, and we are induced to look for some other possible hypothesis which shall equally well connect the phenomena in accordance with known mechanical principles, and not be subject to the same charge of complexity. Such an assumption is found in the present received undulatory theory of light.

In reviewing the foregoing sketch of the rise, growth, and abandonment of the theory of emission we see that an hypothesis, though not absolutely true, may serve an important purpose in the way of the definite conception of old phenomena, and in the discovery and prediction of new; and indeed in some cases, paradoxical as it may appear, a false hypothesis, from its ease of application, may be of more use than one which is absolutely true. Man, with his finite faculties, cannot hope in this life to arrive at a knowledge of absolute truth; and were the true theory of the universe, or in other words, the precise mode in which Divine Wisdom operates in producing the phenomena of the material world, revealed to him, his mind would be unfitted for its reception; it would be too simple in its expression, and too general in its application, to be understood and applied by intellects like ours.

It may be asked why theories, so apparently different as those of emission and undulation, should both lead to the discovery of new truths? The answer is that the former is involved in the latter, and that all the supplementary hypotheses we have mentioned have their representation in the different phases of wave-motion. Thus an undulation is reflected in the same manner as an elastic ball; a change in velocity also takes place in the undulation on entering a new medium; and the fits of Newton are represented by lengths of waves, and the polarization of Malus by transverse vibrations reduced to the same or parallel planes. The undulatory theory is a more general expression, and contains truths which are not to be logically deduced from the theory of

emission. In order however that this theory may enable us to discover the greatest number of new phenomena, and assist us in ascertaining the more precise relations of known facts, it is necessary that all its parts should be definitely conditioned with reference to established mechanical principles. The phenomena of light and heat, and of chemical and phosphorogenic emanation from the sun, by strict analogy lead us to infer that something possessing inertia, and obedience generally to the laws of force and motion, must exist between us and this luminary. All the phenomena are best explained and predicted by supposing this something to consist of an elastic medium, the atoms of which, in a normal state, are distributed uniformly through space and retained in position by attracting and repelling forces. An ætherial medium, constituted in this manner, will admit of vibrations of different characters and of different forms; for example, if an impulse be given to an atom in a given direction, it will cause in succession a motion to be transmitted to the series of atoms which are found in the same line, and thus longitudinal undulations will be produced; also the motion of the atom to which the impulse is given will cause it to approach the atoms of the medium on the sides of the line just mentioned, and thus rows of atoms on all sides of the first row will be thrown into a state of transverse vibration. Similar systems of vibrations must also take place in air; but such is the constitution of the human ear that it takes cognizance only of longitudinal vibrations, and such the function of the human eye that it is only affected by transverse undulations. Besides these there may be other vibrations compound of the two; and in this way other emanations than those which have yet been observed may be conceived to exist.

The science of electricity, as left by Cavendish and Æpinus, and as expounded by Hauy and Robison, was (next to astronomy) one of the most perfect of the physical sciences. All the known phenomena of statical electricity were referred to the mechanical action of two species of matter; the atoms of each being self-repellant and attractive of the atoms of the

other; one of these is called the electrical fluid, and the other ordinary matter. For the generalization of the same phenomena Dufay assumed three principles, two species of electrical, and one of ordinary matter. From either of these mechanical conceptions, could be deduced all the facts then known.

It would appear however that the tendency of the present day is to the accumulation of facts rather than to their critical examination, or the discovery of general expressions by which to represent them. Electricity and magnetism at the present time consist of almost a chaos of isolated phenomena which can scarcely be called scientific. Most of these however, I am convinced, are capable of being referred to the theory of Franklin or to that of Dufay, with the addition of a few supplementary hypotheses analogous to those which we have seen were added to the theory of emission. For example, we shall be obliged to admit that in some cases inductive effects are propagated wave-fashion; and in others, that a change in the condition of the ponderable matter plays an important part. Thus, as I mentioned at the last meeting of the Association, I have found that in the discharge of a Leyden jar through a metallic wire a series of rebounds between the inside and the outside of the jar takes place precisely in the same way as the equilibrium would be restored by a series of waves were a quantity of air, condensed in one vessel, suffered to discharge itself into another in which a vacuum previously existed. I have also shown that during this discharge a series of inductions take place, extending to a surprising distance on all sides of the wire; and as these are the results of currents in alternate directions, they must produce in surrounding space a series of plus and minus motions analogous to if not identical with undulations.

Next, that a change in the condition of the matter itself is required for the explanation of certain phenomena will be evident from the following experiment: If portions of the same current of galvanism be sent through two parallel wires, or if portions of the same discharge from a Leyden jar be transmitted simultaneously through two parallel strips of

platina foil, an attraction in both cases will be exhibited. If however the surface of a large circular metallic plate be covered at intervals with short needles placed parallel to each other, and a discharge of electricity be sent along the diameter of the circle at right angles to the needles, on examination, they will be found magnetized with different degrees of intensity. Those in the direct line of the discharge will exhibit a slight degree of polarity, while those at the circumference of the plate will show a much greater amount of magnetic force; proving that the electrical discharge, instead of passing in the shortest line between the two points, has divided itself into two portions, each passing at as wide a distance as possible from the other. This phenomenon is in strict accordance with the hypothesis that the plate has been traversed by an elastic fluid, the particles of which, being self-repellant, have separated as far as possible from each other; and it can therefore be referred to the action of a fluid co-existing with, but independent of, ordinary matter; while the phenomenon of the attraction of the two parallel conductors before mentioned can only be explained by a change in the condition of the gross matter itself combined perhaps with the action of an elastic fluid. I ought to state in this place that my friend Dr. Hare, from purely theoretical considerations, independent of experiment, has arrived at a similar conclusion.

There is another phenomenon which I may mention as producing a change in the properties of matter during the instantaneous passage of an electrical discharge. At the moment of the passage through the atmosphere of a discharge of electricity, the particles of the air are suddenly endowed with a surprisingly energetic repulsive tendency, to which is mainly to be attributed the mechanical effects produced by a discharge of lightning passing through a building. Also in the development of magnetism in a bar of iron or steel a change takes place in the ponderable molecules of the metal; this is evident from the fact that at the moment of magnetization a wave of undulation, capable of producing an audible sound, is transmitted along the bar; and again,

when the iron is de-magnetized, (if the expression may be allowed,) a similar change in the position of the molecules is indicated.

In the explanation of the statical phenomena of electricity we may either adopt the hypothesis of one or of two fluids, the mechanical results which are logically deduced from either being the same; in the case of the former we have one movable and one fixed principle; in that of the latter we have two movable fluids and a fixed medium. It is evident that the mechanical results will be the same in the two theories, provided we suppose the absolute motion of the one fluid to be equivalent to the sum of the motions of the two fluids. Though either theory may be adopted with reference to the statical phenomena, the theory of one fluid is more readily applicable to the facts connected with electricity in motion, and particularly that part of the theory which assumes the activity of ordinary matter may hereafter be fruitful in new deductions.

The discoveries of the last few years have tended more and more to show the intimate connection of all the phenomena of the "imponderables;" and indeed we cannot avoid the conclusion, forced upon us by legitimate analogy, that they all result from the different actions of one all-pervading principle. Take, for illustration, the following example of the development of the several classes of phenomena. An iron rod rapidly hammered becomes red hot, or in other words, emits heat and light. The same rod insulated by a non-conductor and struck with another non-conductor exhibits electrical attraction and repulsion. Again, if this rod be struck with a hammer while in a vertical position it becomes magnetic. We have here the evolution of the four classes of phenomena by a simple agitation of the atoms. We cannot, in accordance with the known simplicity of the operations of nature, for a moment imagine that these different results are to be referred to as many different and independent principles.

If we refer all these phenomena to one elastic medium it will be necessary, in order to explain the facts of electricity

and magnetism, that we suppose this medium to be capable of accumulation or condensation in certain portions of space, and of being lessened in quantity or rarefied in other portions; also, that in its return to its normal condition an actual transfer of the medium takes place. It follows from these assumptions that the fluid withdrawn from one portion of space must leave an equivalent deficiency in another, or in other words, that the amount of positive action must be equal in all cases to that of the negative. Further, since it appears from observation that the ætherial medium can only be condensed or accumulated in certain places by the insulating powers of ordinary matter, no electrical phenomena can be exhibited except in connection with such matter; hence electrical action cannot be expected in the regions of celestial space.

The most difficult phenomena for which to invent a plausible mechanical explanation, connected with this subject, are those of the attraction of the two wires transmitting a current of electricity, and the transverse action of a galvanic wire on a magnetic needle. The theory of Ampère, though an admirable expression of a generalization of the phenomena of electromagnetism, is wanting in that strict analogy with known mechanical actions which is desirable in a theory intended to explain phenomena of this kind.

In conclusion I would again revert to the importance, in the adoption of mechanical hypotheses, of conditioning them in strict accordance with the operations of matter under the known laws of force and motion as exhibited in time and space.

THE IMPROVEMENT OF THE MECHANICAL ARTS.

CLOSING ADDRESS AT THE EXHIBITION OF THE METROPOLITAN MECHANICS' INSTITUTE, OF WASHINGTON.

[From a pamphlet edition, published by the M. M. Institute, in 1853.]

Delivered March 19, 1853.

At the close of the Exhibition of the Metropolitan Mechanics' Institute it becomes my duty to offer some remarks relative to the objects and organization of the association; and in addition to these, I shall beg leave to call your attention to some points which present themselves more prominently to my mind, amidst the extended field of the history of mechanical inventions.

The object of this Institute is twofold: first, the improvement of mechanics and artists; second, the improvement of arts and inventions.

These two objects are inseparably connected, and the one necessarily follows as a consequence of the other. Whatever tends to develop the mind of the workman tends to advance the condition of his art. Every material operation and every invention is founded on some law of nature, and the more intimately the operator is acquainted with the principles of his art the better is he fitted to improve it. Without a knowledge of science the practice of art is mere empiricism, often involving operations which are not only unnecessary to the production of the desired result, but frequently detrimental.

The savage who recovers his health after drinking from a mineral spring considers his cure due not alone to the efficacy of the water, but also to the position of his body at the time of drinking, whether facing the east or the west, to the number of draughts, and perhaps in some cases he deems it necessary previously to propitiate the spirit of the fountain by a sacrifice of some object of value. We need not go to savage life for examples of this kind. In many parts of our own country—even among men otherwise intelligent—certain mechanical and agricultural operations are connected

with superstitious observances of the most ridiculous and inconvenient character. A knowledge of principles serves to eliminate these errors, to point out the necessary and essential conditions of practice, and to facilitate the introduction of improved methods. On the other hand, every improvement in the mechanic arts, as a general rule, tends to elevate the character of the artisan, and to render his employment more intellectual.

It is proposed in this Institute to improve the workman by lectures, collections of specimens of natural history, a library and a reading-room, and to advance the arts by exhibitions and by the examination of such new inventions as may be submitted to the judgment of the Institute. For conducting this part of the plan of operation a permanent Committee of Science and Arts will be formed, combining (among its members) theoretical knowledge with practical skill.

There is no place in the United States (taking in view the number of its mechanics) so well adapted to support an efficient institution of this kind as the city of Washington. It offers numerous examples of ingenious inventions and processes. The models of the Patent Office, the instruments of the Observatory, of the Coast Survey, of the Topographical Bureau; the processes of the navy-yard and of the arsenal are illustrations of the useful arts readily accessible and of the most instructive kind. Moreover, no city of the Union of the same size can command so large a number of scientific men, namely, those belonging to the army and the navy, and the institutions established here. Any association which tends to bring these into harmonious co-operation with the practical mechanics of the place may, and I doubt not will, be productive of important results.

The Institute has commenced its existence under very auspicious circumstances, and has found favor with the wise and the liberal. Notwithstanding this, the enterprise is not unfraught with danger, and those who were instrumental in establishing it assumed a responsibility of no small weight. They evoked a power which may be determined on good or

on evil; which, while it is capable of conferring blessings on this city and this country, may be the means of propagating error, and of administering to the selfish ends of designing men. There is no city in which a society of this kind requires to be more strictly guarded against baneful influences. The partisan politician may attempt to make it the stepping-stone of his political advancement. The pseudo-inventor, who seeks to enrich himself by pirating the labors of humble and unobtrusive genius, and the speculator, who wishes to impress Congress with the importance to the country and to the world, of a scheme intended to benefit himself alone, will be untiring in their endeavors to obtain the certificates and recommendations of the Institute. They will approach its judges and its committees with soft words and insinuating manners, and will not hesitate to offer bribes in such sophistical terms that, while cupidity is excited, the conscience is lulled to rest.

The location of this Institute at the seat of government of this vast Union will turn all eyes upon it, and will consequently tend to give it corresponding power and influence. But it must be recollected that in proportion to the conspicuousness of the position occupied by institutions or individuals is their responsibility to society increased. The higher they stand the more secure must be the principles on which they are supported. When men build upon a false foundation, the greater their elevation the more certain is their fall.

There is no place in this country where *motives* and *acts* are more critically examined than in the city of Washington. There is none in which *capacity, honesty of purpose,* and a *prudent, straightforward course* are more necessary to continued success, and none in which *deviations* from *right,* whether intentional or otherwise, are more readily detected and exposed.

The mere organization of the Institute, however well it may have been done, is not sufficient for its perpetuity and usefulness. It requires the constant application of individual effort to sustain it; the unwearied labor of a few master spirits to infuse the constant supply of vital energy; and

these must be men of high moral principle, not only strictly honest, but above suspicion of the contrary. They owe a strict accountability to the members of the Institute for the manner in which the income is expended, and to the world for the mode in which the high duty of acting as judges of the merit of inventions has been discharged. The task of the judges, and of the committee of science and the arts, to whom discoveries and inventions are referred, is one of delicacy and difficulty, and should not be entrusted to those unacquainted with the principles on which the proposition to be examined depends, or who do not possess the mental and moral qualifications necessary to the formation of a correct judgment. It is for the benefit of the community that the truth should prevail, and that the merits and defects of an invention should be rendered distinctly manifest. Where merit exists it should receive due credit, but not exaggerated praise. The simple statement of what has been accomplished is all that is needed, though it may not be all which a generous spirit is impatient to bestow. Nobleness of mind springs forward with ardor to meet every indication of a similar kind wherever it appears. The whole duty of the committee however in this case may be expressed in two words—*strict justice*. This is what every judge ought to give, and more than this no man ought to desire to receive.

It will often become the duty of the committee of examination of subjects of science and art to repress the premature zeal of visionary inventors. We need only examine the records of the Patent Office to be convinced of the immense expense of time and money continually lavished on futile attempts to innovate and improve. We may safely venture to affirm that out of every fifty propositions for improvements in arts or mechanics forty-nine at least are either useless or old. The object should be to distinguish and to adopt the good and reject the bad. But while pruning the luxuriant fruit of uncultivated invention care must be taken to perform the task with gentleness, and to show that the intention is to give additional vigor to the healthful branches and not to injure the parent plant.

There can be no reality in science if at this late day it cannot predict that certain proposed inventions are impossible, as well as declare that others are in accordance with established principles. An honest expression of opinion on such points, though it be met with the accusation of repressing the march of improvement, is necessary in order to save the public from having its attention perpetually distracted by the excitement of fallacious expectations, and the credulous from embarking their all in schemes which must end in disappointment and ruin.

One of the most fruitful sources of error and deception with regard to inventions arises from misconceptions of the nature and application of mechanical power, and this is one of the points on which I wish to arrest your attention for a few minutes. We understand by the term mechanical power that which moves machinery, transports heavy bodies, shapes the raw material into useful forms, and to use the short but expressive phrase of the mechanic, "*that which does work.*" Mechanical power, when properly understood, is a condition or state of matter. Thus a quantity of burning fuel, a moving mass of water or of air, are bodies in the condition of power, and by communicating a portion of their motion to other bodies they produce in them certain changes which are denominated *work*. The change thus produced is the measure of the amount of power in a given quantity of matter. For example, the number of bushels of grain which can be ground during the combustion of a bushel of coal is the measure of the amount of power in this quantity of fuel.

Power is always expended in doing work, and it is in the highest degree absurd to think of applying it to useful purposes without exhausting it. Every change of condition, every transformation of matter, every new motion, and every manifestation of life is at the expense of some motive power which, having performed its part, is forever neutralized.

Power is always the product of nature. God has not vouchsafed to man the means of its primary creation. It is found in the moving air and the rapid cataract—in the burning coal—the heaving tide; man transfers it from these to other

bodies and renders it the obedient slave of his will—the patient drudge, which in a thousand ways administers to his wants, his convenience, and his luxuries, and enables him to reserve his own energy for the higher purpose of the development of his mind and the expression of his thoughts.

The following is a list of all the primary powers which as yet have been used by man in accomplishing his varied purposes in the wide domain of practical life. These are:

1. Water power,
2. Wind power,
3. Tide power,
4. The power of combustion, and
5. The power of vital action.

To this list may hereafter be added the power of the volcano and the internal heat of the earth; and besides these science at the present time gives no indications of any other. These are denominated primary powers, though in reality, when critically studied, they may all, except the two last mentioned, be referred to actions from without the earth, and principally to emanations from the sun.

Gravitation, electricity, galvanism, magnetism, and chemical affinity can never be employed as original sources of power. At the surface of the earth they are forces of quiescence, the normal condition of which must be disturbed before they can manifest power, and then the work which they are capable of performing is only the equivalent of the power which was communicated to them.

There is no more prevalent and mischievous error than the idea that there is in what are called the "imponderables" a principle of spontaneous activity. Heat is the product of chemical action, and electricity only manifests power when its equilibrium is disturbed by an extraneous force, and then the effect is only proportional to the disturbing cause. It was for this reason that the existence of electricity remained so long unknown to man. Though electricity is not in itself a source of power, yet from its extreme mobility and high elasticity it affords the means of transmitting power with

scarcely any loss and almost inconceivable velocity to the greatest distance. A wave of disturbance starting from the impulse given at the battery will traverse the circumference of the earth in less time than I have been occupied in stating the fact.

Besides electricity and the principles before mentioned, there are other agents employed between the primary power and the *work*, namely, the elastic force of steam, of air, and of springs; also various instruments called machines. But these must not be confounded, as they frequently are, with the sources of power. It is not the engine which is the source of motion of the cars, nor yet the steam, but the repulsive energy imparted to the expanding water from the burning fuel.

A machine is an intermediate instrument to transmit, to modify, and to apply power; and with the exception of the power consumed in wearing away the rubbing parts—(that is, in producing friction,) and the small portion imparted to the air, the amount of power transmitted is just equal to that received.

The human body is itself an admirably contrived complex machine, furnished with levers, pulleys, cords, valves, and other appliances for the application and modification of the power derived from the food. It is, in fact, a locomotive engine, impelled by the same power which under another form gives activity and energy to the iron horse of the railway. In both, the power is derived from combustion of the carbon and hydrogen of the organic matter employed for food or fuel. In both the direction of power is under the influence of an immaterial, thinking, willing principle called the soul. But this must not be confounded, as it frequently is, with the motive-power. The soul of a man no more moves his body than the soul of the engineer moves the locomotive and its attendant train of cars. In both cases the soul is the directing, controlling principle, not the impelling power. Let, for example, a locomotive engine be placed upon the track, with water in the boiler and fire in the grate—in short, with all the potentials of motion, and it will still remain

quiescent. In this state let the engineer enter the tender and touch the valve; the machine instantly becomes instinct with life and volition; it has now a soul to govern its power and direct its operations; and indeed as a whole it may be considered as an enormous animal, of which the wheels and other parts are additions to the body of the engineer.

The facts I have given as to the source of power and its application rest upon the widest and best-established inductions of physical science, and a knowledge of them is absolutely essential to every one who desires to improve the art of applying the powers of the elements to useful purposes. And yet,—if we are to judge from the constant announcement in the papers of new motors, of machines moved by centrifugal force, of engines to do a large amount of work with the expenditure of an infinitesimal quantity of power, of contrivances by which electricity is to develop itself and do work by its own force,—we shall be convinced that on projects which are in opposition to the best-established truths of science hundreds of thousands of dollars are squandered and years of thought and labor wasted. One cause of error of this kind is the unfortunate name which was originally given to, and is still retained by, certain elementary machines, viz., the lever, the wheel and axle, the inclined plane, the pulley, and the screw. These are employed separately as instruments for the application of power, or in combination as the elementary parts of complex machines. Every tyro in science knows that they have no power in themselves, yet the name, *mechanical powers*, by which they are designated tends to perpetuate a pernicious error long after the fallacy is understood.

A machine, as I have before stated, is an instrument to apply and modify power, and to effect changes in the form and texture of matter denominated *work*. The combination of the elementary parts of machines so as to produce any desired motions has been studied with much success, and the whole reduced to rules. The diffusion of a general knowledge of these would much facilitate invention and prevent the necessity of the individual who devotes his mind to the

improvement of machines beginning anew instead of building on what has been done before him.

Every complex machine consists of parts which may be classified as follows:

1st. The receivers of the power—such as the buckets and other parts of the water-wheel, the vane of the windmill.

2d. The transmitters and modifiers of the motion, viz., wheels, pinions, levers, pistons, screws, &c.

3d. The supporters—such as the frames, the friction-rollers, &c.

4th. Regulators to render the motion uniform.

5th. Operators or parts applied immediately to the matter on which the work is to be done.

The preparation and publication of charts of the elementary parts of machines and their combinations would do important service to the practical mechanic, and is an object among many others worthy of the attention of this Institute.

The most important source of mechanical power among those we have mentioned, and which promises almost to supersede all others, is that of burning coal. This material—like a watch wound up, is matter in a state of power, or in a state of unstable equilibrium, ready to rush into combination with the oxygen of the atmosphere as soon as the initial action is given, and to evolve power in the form of heat until the whole is consumed. It has been proved that on an average *four ounces* of coal is sufficient to draw—on a railway, one ton a mile. It has also been found by experiment that a man working on a tread-mill continuously for eight hours will elevate one and a half million of pounds one foot high. Now, good Cornish engines will perform the same work by the expenditure of the power of a pound and a half of coal. It follows from these data that about five tons of coal would evolve as much power during its combustion as would be equal to the continued labor of an able-bodied man for twenty years, at the rate of eight hours per day; or in other words, to the average power of a man during the active period of his life. Providence has therefore stored away in the form of coal, for the use of man, an incalculable amount

of mechanical power. Beneath the soil of our own great coal basins there reposes power equivalent to the united force of myriads of giants, ready (like Aladdin's Genius) to be called into activity by the lamp of science, and as its obedient slave to build cities, to transport palaces, or to remove mountains. There is no other locomotive power over which man has any prospect of control in the least degree comparable with this.

I have made these remarks with reference to power, because mistakes on this subject are so frequent and so fatal. Allow me, in the next place, to call your attention to some other points having a direct bearing upon the progress of the mechanic arts.

In order that an important invention may be successful, two conditions must be favorable: First: It must be possible; that is, the scientific principle on which it is to be founded must be known. Second: The invention must be wanted; or in other words, it must be called for by the character and intelligence of the times, or rendered especially desirable in a particular place by some peculiarity of climate, topography, &c.

With reference to the first position, it may be said that in accordance with the well-known laws of permutation, an almost infinite number of new combinations or inventions may be formed from the present stock of scientific knowledge. This is true: but the inventions thus produced must be restricted as to kind, and though they be unlimited in number they are not so as to character. No combination of known principles, before the discovery of galvanism, was sufficient for the invention, by the most ingenious synthetical mind, of the electro-magnetic telegraph; but after the discoveries made by Galvani and Oersted, this invention became possible.

In the history of the progress and development of a branch of science a condition is reached when its principles become applicable to some practical purpose, and it is instructive to observe how at this period it suddenly assumes in the public mind a high degree of importance. The man who makes

the application, though he may not have spent a tithe of the labor and thought on the subject which was bestowed on it by those who brought it to its practical state, is crowned as the discoverer of the whole. After this however, competitors arise who claim a share of the reward, if not the honor of the invention. These labor to show that the first inventor derived his ideas from the discoverers. The public mind then takes another turn, and is disposed to do injustice on the other side, and it is only after a series of oscillations in public opinion that the true state of the case becomes generally known and acquiesced in.

With reference to the second proposition we may state that so important an element is the state of public intelligence in regard to the success of an invention that many of the most important processes of art have been more the result of the actual spirit and want of the age than the product of the ingenuity and knowledge of an individual; and in such cases the invention is frequently brought forth simultaneously by a number of different individuals. The art of printing may be placed in this category. At a certain period in the history of the world this invention was loudly called for by the pressing necessities and peculiarities of the times. It was then produced: but had the attempt been made at an earlier date to introduce it, the result would probably have been a failure. We have a similar example in the application of steam to navigation. The world had for years before this invention been in possession of the steam engine, and a boat had even been propelled by steam on the Clyde, in Great Britain, but the invention was not appreciated. Neither the time nor the place was favorable to its introduction, and it was reserved for our country, with its immense plexus of navigable rivers and its broad expanse of internal lakes, to call for this addition to the art of locomotion, and for the genius of Fulton to give a successful response. Even in this case the importance of the invention was so manifest, and its means of attainment so simple, that several competitors contended for the prize; and had any accident happened to retard for a few weeks the completion of Fulton's first boat

he would have been anticipated in the result of his enterprise by the fortunate experiment of the elder Stevens. In making this statement I would not wish to detract from the real merit of individuals; they have sufficient claims for remuneration and reputation in being among the first to appreciate properly the value of the improvement, and to avail themselves at the earliest point of time of the necessary means of accomplishing it. I may remark in passing that from the foregoing views and statements it is plain that the steamboat is emphatically an American invention. It was in this country that premiums were first offered for its production, and on the Hudson, in 1807, it was first reduced to practice. It was not adopted in England until 1812, and not until 1816 in France.

From a want of a knowledge of the state of science, and a due consideration of the proper time and place, many ingenious minds have wasted their energies in fruitless labor, waged with fortune an unequal war, and sunk into the grave the victims of disappointed hopes. Such men are frequently said to "live before their time;" but it remains to be proved whether in the aggregate of cases they have done more good or evil, and whether they most deserve our admiration or our pity. A premature, and consequently an unsuccessful, attempt often so prejudices the public mind against an invention that when the proper time actually arrives for its introduction public sentiment is found arrayed against it, and difficulties have to be overcome which would not have existed had the first essay never been made.

The man of true genius never lives before his time; he never undertakes impossibilities, and always embarks in his enterprise at the suitable place and period. Though he may catch a glimpse of the coming light as it gilds the mountain top, long before it has reached the eyes of his contemporaries, and though he may hazard a prediction as to the future, he *acts* with the present.

There are some partial exceptions to this rule, and among them I would mention with high respect that of Oliver Evans, than whom no man in this country has ever done

more to improve the art of locomotion. He indeed predicted that steam wagons would be used on common roads, and made attempts to reduce his idea to practice. The time however for the introduction of this invention has not even *yet* arrived, and at present we see no prospect of its coming. But he was more successful in the invention of the American high-pressure engine, which was so essential to the development of the vast resources of the interior regions of our continent. This engine was at the time of its introduction admirably adapted, in its cheapness, simplicity of arrangement, smallness of dimensions, and great power, to the abundance of fuel, the extent of transportation, and the primitive state of the arts in our country. The low-pressure engine used by Fulton was procured from England; and had steam navigation been confined to the employment of the complex and expensive machines of this class, the Mississippi and its tributaries would have remained for years unnavigated, except by the canoe of the native or the flat-boat of the pioneer.

The invention and introduction of the high-pressure engine required the application of genius, energy, and courage. The use of high steam had been proposed in England, but had been discarded on account of the supposed danger attendant on its use, and it was reserved for this country to demonstrate its practical importance. Without precursory labors equivalent to those of Evans the present railway locomotive would not have been in existence.

It gives me pleasure to pay this passing tribute to the memory of one to whom our country owes so deep a debt of gratitude, and whose name deserves a more conspicuous place than it now holds in the history of American inventions.

Every age of the world since the commencement of the historic period has been characterized by some leading or dominant idea, and each age has bequeathed something of value to—or made some abiding impression on—that which followed.

The great characteristic of the present time is the application of science to art; or in other words, the development of the inventive faculty of the human mind. The last cen-

tury was equally if not more fertile in the discovery of the great principles of nature from which we are now reaping so rich a harvest of practical results, but a knowledge of these was not then so interwoven with the thoughts of the common mind as to render them available for purposes of art. Indeed the facts and elementary principles of science, as well as the application of the rules which have been deduced from its higher generalizations, are now so familiar that art has become vain of her attainments, has set herself up as the architect of her own fortune, and disregards the counsel of her more learned and sagacious sister. Such a course however is usually accompanied with its own punishment. The new edifices, designed by empiricism, are generally unstable structures, and most frequently involve the ruin of the builder in their fall.

It is true many valuable inventions have been founded on the accidental discovery of simple facts; but such inventions can never be perfected unless the principles of science on which they are based are known. It is also true that many arts may be successfully practiced by persons entirely ignorant of the principles of these arts. We have a notable example of this in the art of navigation, and in many of the processes of engineering. The practical man in these cases employs rules and deductions furnished by abstract science, in the application of which he often becomes more expert than the original author; but sure progress in art cannot be obtained without anterior or contemporaneous progress in science. The inventor, to insure his success, must consult the discoverer, and the practical skill of the one be directed by the theoretical knowledge of the other.

After what has been said in different parts of this address it may be superfluous to give a formal definition of discovery and invention; but these terms are so frequently confounded with each other, and their misuse so much connected with error, that it is necessary they should be clearly defined, even at the risk of prolixity.

By a *discovery* in science is understood the development of a knowledge of the existence of some principle in nature not

before known or but partially understood; while the term *invention* indicates the application of this knowledge, either simply or in combination with other knowledge, to some useful purpose in the arts. For example, Franklin *discovered* the principle of electrical *induction*, or the action at a distance of a charged body on a conductor, and on this founded his *invention* of the lightning-rod.

It sometimes happens that the peculiar characteristic of mind and training necessary to the successful prosecution of these two branches of labor are found combined in the same individual. Of a happy combination of this kind James Watt affords a striking example, the like of which will become more common in proportion as the means of intellectual improvement afforded to workmen are extended. Generally however the two faculties exist in the greatest degree of development in separate individuals. The successful investigation of a new principle in science generally requires much previous study and preparation and a logical training, which few men—however vigorous may be their native intellect, can dispense with, and to acquire which the opportunities of the workmen are inadequate. On the other hand the successful introduction to common use of an invention requires a contest with the world from which the sensitive student of abstract science shrinks with repugnance. I consider these remarks of some importance, because in this country, where there is so great a demand for immediate practical results, the value of labor in the line of abstract science is not properly appreciated or encouraged.

We have said that every age of the world has bequeathed something of value to that which followed, and we may add that it is doubtful whether any great truth has ever been lost: though some may have apparently lain dormant for a time, yet they have continually produced results. Some arts have undoubtedly fallen into disuse, because they are no longer required, or because they have been superseded by more perfect processes. We however think it can be clearly established that modern science is capable of re-producing every invention of ancient art, and at an indefinite economy of human time and human labor.

I know we are frequently referred to the immense masses of stone transported and wrought by ancient art, which are found among the ruins of Baalbec and Thebes, and are frequently told that the management of these would far transcend the skill and power of modern engineers. Such assertions are however rather intended to convey an idea of the impression produced upon the beholder of these venerable ruins than a declaration of absolute truth. As a sufficient illustration of this we may mention the fact that in New York large buildings of brick and stone are moved from place to place while the inhabitants remain undisturbed within; or we may point to the Menai Strait tubular bridge, a structure of cast-iron several hundred tons in weight, suspended in mid-air over a chasm more than a hundred feet deep.

The pyramid of Cheops is said to have employed the power of 100,000 men for twenty years in its erection; but, vast as is this pile, were the steam-engines employed in one of our large cities directed to the task of rearing one of equal magnitude the whole would be accomplished in a few weeks.

I have said that no arts of importance have been lost, but perhaps this assertion is rather too general. There is one which may be considered an exception: I allude to the ancient art possessed by the few of enslaving and brutalizing the many, the art by which a single individual, invested with the magic of kingly power, was enabled to compel thousands of his subjects, through the course of a long reign, like beasts of burden, to haul materials and heap up huge piles of stone, which might transmit to posterity the fact that a worm like himself had lived and died. The pyramids of Egypt, venerable as they are with the age of accumulated centuries, are melancholy monuments of human degradation, of human vanity and cruelty.

There are certain processes of thought which require individual exertion rather than combined effort for their development. There are certain arts in which perfection depends on the genius and skill of the individual rather than on the condition of the race. Such are oratory, poetry, painting,

and sculpture. In these if an individual excel he excels for himself; his skill is not transferable, though his example may serve to awaken the same taste in many of his contemporaries and successors. For the development of these arts the individualism of the Greeks was well adapted, and they were accordingly advanced by this people almost, if not quite, to their maximum state of perfection.

These results of the labors of the ancients in the development of the beautiful have not been lost; on the contrary, they will ever remain impressed upon the human mind. The marble of the Parthenon may be reduced to atoms, and scattered to the winds of Heaven, but its form is imperishable. The moderns do not surpass the examples of the fine arts bequeathed to them by the ancients, because it would be idle to attempt to add to that which is perfect,—to paint the lily, or to gild refined gold. But they have invented tools and processes by which copies of these precious relics may be multiplied indefinitely, with unerring precision, by the application, not of manual skill, but of physical labor.

This union of the industrial with the fine arts vastly enlarges the influence of the latter, and enables them to be appreciated and genius to be admired by millions whom their single productions would never reach. There are at this time more minds enthusiastically alive to the beauty of ancient art than there were in the days of Phidias. Nothing then of importance with reference to art has been lost, but, on the contrary, much has been gained.

In these remarks we seek not to disparage the past, nor to unduly exalt the present. The character of the world, as it now exhibits itself in its mental and moral development, its knowledge of nature, and its skill in arts, is the result of all the impressions made on it from the earliest dawn of civilization to our own day. In the case of an individual every impression to which his mind is subjected, either from external nature or his own mental operations, or those of his fellow-men, produces an indelible effect, modifying all the previous impressions, and co-operating with them to form the peculiarities of his mental and moral character. An

analogous effect is produced on the whole human family during the successive ages of its existence.

By these remarks we do not wish to draw upon ourselves the imputation of advocating the inevitable progress of the human race. The world is subject to evil impressions as well as good, and whatever advance is made in the line of true progress will not be the result of a blind law of necessity, but of a providential design through human agency and properly-directed human labor. Without labor nothing of value can be accomplished. It is the essential pre-requisite of well-being, the original curse which proves a blessing in disguise. The remark has been properly made that could all the wants of man be supplied without labor there would be reason to fear that he would become a brute for the want of something to do, rather than a philosopher from an abundance of leisure. In all countries where nature does the most, man does the least. The sterile soil and the inclement sky seem to be the stimulants to mental and physical exertion, when once the necessary impulse has been given. True progress does not consist in obviating the necessity of labor, but in changing, by means of improvements in the arts, its character in rendering it more conducive to the supply of the wants and comforts of man, and to the development of his mental and moral nature.

We have received from the past a rich treasure of knowledge, the product of the body and mind, gathered under difficulties and danger, and improved by the thought and the experience of years. Our great object should be to purify this knowledge from error, to reduce it to its essential and simple elements, and to transmit it with the greatest amount of new truth to our successors. We should however recollect that accumulated knowledge, like accumulated capital, increases at compound interest, and that therefore each generation is bound to add much more largely to the common stock than that which immediately preceded it. It is the high privilege, as well as the sacred duty, of every one of us to labor for the improvement of ourselves and our fellowmen, and to endeavor to the utmost of our ability to leave

the world at least a little wiser and better than we found it. But in order to success in this effort, we must cultivate other provinces of thought than merely those which belong exclusively to the development of our knowledge of the external world. There are other regions of a higher and holier nature, without the cultivation of which no true progress can be made.

THOUGHTS ON EDUCATION.

INTRODUCTORY DISCOURSE BEFORE THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF EDUCATION.*

(From the American Journal of Education, 1855, vol. I, pp. 17–31.)

Delivered December 27, 1854.

No subject of human thought has perhaps received more attention than that of education. Every one has the material for speculating in regard to it in his own experience; but individual experience is too limited a basis on which to found a general theory of instruction, and besides this, (paradoxical as it may appear,) an individual is perhaps less able to judge correctly of the effects of the course of instruction to which he has been subjected than another person. No one can tell what he would have been under a different course of training, and the very process which he condemns may perhaps have been the one best suited to develop the peculiarities of mind which have led to his success in life; and indeed in some very rare instances the want of all training of a systematic kind may be the best condition under Providence for producing an entirely original character. Shakespeare's genius might have been shackled by the scholastic curriculum of Oxford or Cambridge: but these cases are extremely rare, for genius itself, like the blossoms of the aloe, is the solitary production of a century.

I bring forward my own views on education with diffidence. First, because I have read scarcely any thing on the subject, and what I shall say may be considered commonplace; secondly, because my views may in some respects be at variance with what are regarded as the established principles of the day. But important truths cannot be too often presented, and when re-produced by different minds under different circumstances they can scarcely fail to awaken new

*[Introductory Address delivered by the retiring President of the Association for the Advancement of Education, at its Fourth Annual Session, held at Washington, D. C., in December, 1854.]

trains of thought and renewed attention; and again, if the propositions which I maintain are erroneous, I desire that they may be discussed and disproved before they are given more widely to the public. What I shall advance may be viewed as suggestions for consideration, rather than propositions adequately proved.

In the establishment of a principle it is of the first importance that all probable suggestions relative to it may be subjected to critical examination, and tried by the test, as far as possible, of experience; it is in this way that science is advanced.

The first remark which may be made in regard to education is that it is a forced condition of mind or body. As a general rule it is produced by coercion,—at the expense of labor on the part of the educator, and of toil and effort on the part of the instructed. That there is no royal road to learning is an aphorism as true now as it was in the days when first uttered. God has placed a price on that which is valuable, and those who would possess a treasure must earn it at the expense of labor. Intellectual as well as material wealth can only be purchased at the price of toil. It is true the child may be induced to learn his task by the prospect of reward; by emulation; by an appeal to his affections; but all these, in some cases, are ineffectual, and recourse must be had to the stimulus of the rod. I do not by this remark intend to advocate a general recourse to corporeal coercion. It should be used sparingly, perhaps only in extreme cases, and for the purpose of eradicating a vicious habit. The philosophy of its use in this case is clear. We associate pain with the commission of an improper act, and thus prevent its recurrence.

I have said that education is a forced condition of mind or body. The child, if left to itself, would receive no proper development, though it might be surrounded with influences which would materially affect its condition. The savage never educates himself mentally; and were all the educational establishments of the present day abolished, how rapidly would our boasted civilization relapse into barbarism.

Another important fact is that every generation must educate and give character to the one which follows it, and that the true progress of the world in intelligence and morality consists in the gradual improvement of the several generations as they succeed each other. That great advance has been made in this way, no one can doubt who views the facts of history with an unprejudiced mind; but still the improvement has not been continuous. There have been various centers and periods of civilization. Egypt, Greece, and Rome, though they have left an impress upon the world which extends even to our time, and modifies all the present, have themselves "mouldered down." It appears therefore that civilization itself may be considered as a condition of unstable equilibrium, which requires constant effort to be sustained, and a still greater effort to be advanced. It is not, in my view, the manifest destiny of humanity to improve by the operation of an inevitable, necessary law of progress; but while I believe that it is the design of Providence that man should be improved, this improvement must be the result of individual effort, or of the combined effort of many individuals, animated by the same feeling, and co-operating for the attainment of the same end. The world is still in a degraded condition; ignorance, want, rapine, murder, superstition, fraud, uncleanliness, inhumanity, and malignity abound. We thank God however that he has given us the promise, and in some cases the foretaste—of a happier and holier condition; that he has vouchsafed to us as individuals, each in his own sphere, the privilege, and has enjoined upon us the duty, of becoming his instruments, and thus co-workers in ameliorating the condition of ourselves and our fellow-men; and above all that he has enabled us through education to improve the generations which are to follow us. If we sow judiciously in the present, the world will assuredly reap a beneficent harvest in the future; and he has not lived in vain who leaves behind him as his successor—a child better educated morally, intellectually, and physically than himself. From this point of view the responsibilities of life are immense. Every individual by his

example and precept, whether intentionally or otherwise, does aid or oppose this important work, and leaves an impress of character upon the succeeding age which is to mould its destiny for weal or woe in all coming time.

Civilization itself, as I have before observed, is a state of unstable equilibrium which, if not supported by the exertions of individuals, resembles an edifice with a circumscribed base, which becomes the more tottering as we expand its lateral dimensions, and increase its height. Modern civilization is founded on a knowledge and application of the moral, intellectual, and physical laws by which Divine wisdom governs the universe. The laws of morality have been revealed to us, but they require constant enforcement and habitual observance. The laws of the intellectual and material universe have been discovered by profound study and years of incessant labor, and unless they are taught in purity and freed from error they fail to produce their legitimate result. But the illustration and enforcement of the laws of morality require the exertions of men of high talents and profound learning; and a true knowledge of the laws of nature can be imparted only by minds that have long been devoted to their study. Therefore a large number of highly educated men whose voice may be heard, and whose influence may be felt, is absolutely necessary to sustain the world in its present moral and intellectual development. The world however is not to be advanced by the mere application of truths already known; but we look forward, particularly in physical science, to the effect of the development of new principles. We have scarcely as yet read more than the title page and preface of the great volume of nature, and what we do know is nothing in comparison with that which may yet be unfolded and applied; but to *discover* new truths requires a still higher order of individual talent. In order that civilization should continue to advance, it therefore becomes necessary that special provision should be made for the *actual increase* of knowledge, as well as for its diffusion; and that support should be afforded, rewards given, and honors conferred, on those who *really add* to the sum of human knowledge.

This truth however is not generally appreciated, and the tendency is to look merely at the immediate results of the application of science to art, and to liberally reward and honor those who simply apply known facts rather than those who *discover* new principles.

From what we have said it would appear that, in order that civilization should remain stationary, it is absolutely necessary that the great truths which have been established should not become diluted, obscured, or forgotten; that their place should not be usurped by error; or in other words, that the great principles of science, which have been established through long years of toil and nights of vigilance, should not be superseded by petty conceits, by hasty and partial generalizations, and by vague speculations or empirical rules. Further, that civilization should not retrograde, it is indispensably necessary that the great truths of morality should not only be theoretically taught and intellectually apprehended, but actively, constantly, and habitually applied. But this state of things can only exist by means of the efforts of individuals actuated by a generous, liberal, and enlightened philanthropy. Unfortunately however the tendency of civilization, from the increase of wealth and security, is to relax individual effort. Man is naturally an indolent being, and unless actuated by strong inducements or educated by coercion to habits of industry, his tendency is to supineness and inaction. In a rude state of society an individual is dependent upon his own exertions for the protection of himself, his family, and his property; but as civilization advances, personal effort is less required, and he relies more and more on law and executive government. Moreover, as wealth and elementary education become more general without a corresponding increase of higher instruction, the voice of the profound teacher becomes less and less audible; his precepts and admonitions less and less regarded; he is himself obliged to comply with popular prejudices and conform to public opinion, however hastily formed or capricious such an opinion may be. Hence the tendency to court popular favor, to be influenced by it, rather than attempt to

direct it. Hence charlatanism and the various dishonest efforts to gain notoriety rather than a true reputation—so frequently observed. Knowledge has arrived at such a stage of advancement that a division of labor in regard to it is necessary. No one can be learned in all the branches of human thought; and the reputation of an individual therefore ought to rest on the appreciation of his character by the few—comparatively, who have cultivated the same field with himself. But these are not generally the dispensers of favor, and consequently he who aspires to wealth or influence seeks not their approbation, but the commendation and applause of the multitude. It is impossible that those who are actively engaged in the business of life should have time for profound thought. They must receive their knowledge, as it were, at second hand; but they are not content under our present system of education with the position of students; they naturally aspire to that of teachers; and every one who has learned the rudiments of literature or science becomes ambitious of authorship and impatient for popular applause. Knowledge in this way becomes less and less profound in proportion to its diffusion. In such a condition of things it is possible that the directing power of an age may become less and less intelligent as it becomes more authoritative, and that the world may be actually declining in what constitutes real moral, and intellectual greatness, while to the superficial observer it appears to be in a state of rapid advance. I do not affirm that this is the case at present. I am merely pointing out tendencies.

The present is emphatically a reading age; but who will venture to say that it is proportionately a *thinking* age? The sum of positive knowledge is embraced in but few books, and small would be the library necessary to contain the essence of all that is known. We read too much and too quickly to read understandingly. The world is gorged with intellectual food, and healthful digestion is comparatively unknown. Too many books are published; I do not mean to say that too many *standard works* are printed, but by far too many silly, superficial, and bad books are sent

forth from the teeming press of our day. The public mind is distracted amidst a multiplicity of teachers and asks in vain for TRUTH. But few persons can devote themselves so exclusively to abstract science as fully to master its higher generalizations, and it is only such persons who are properly qualified to prepare the necessary books for the instruction of the many. I cannot for a moment subscribe to the opinion which is sometimes advanced that superficial men are best calculated to prepare popular works on any branch of knowledge. It is true that some persons have apparently the art of simplifying scientific principles; but in the great majority of cases this simplification consists in omitting all that is difficult of comprehension. There is no task more responsible than that of the preparation of an elementary book for the instruction of the community, and no one should embark in such an undertaking who is not prompted by a higher motive than a mere love of notoriety, or the more general incentive, a hope of commercial success. He should love the subject upon which he intends to write, and by years of study and habitual thought have become familiar with its boundaries, and be enabled to separate the true and the good from that which is merely hypothetical and plausible.

In this connection I may mention the evils which result from literature and science becoming objects of merchandise, and yet not amenable to the laws of trade. I allude to the international copyright system. The tendency of the present condition of copyright law between England and America is greatly to debase literature, to supply cheap books, and not to impart profound wisdom or sound morality. English books are republished in this country and American books are reprinted in England because they are *cheap*, and not because they are GOOD. Literary and scientific labor must be properly remunerated or the market will be supplied with an inferior article. The principles of free trade are frequently improperly applied to this question. The protection required and demanded by the literary man is not that of a premium on his work, but the simple price which it ought

to bear in the market of the world. He asks that the literary product of the foreigner may be paid for in order that justice may be done his brother, and also that he himself may re-receive a proper remuneration for his own labors. Would there be any manufactories of cloth, think you, in this country if the tailor had the means and inclination to procure free of cost all the material of the garments which he supplies to his customers? And can it be supposed that valuable literary works will be produced among us so long as our publishers are allowed to appropriate without remuneration the labors of the foreigner? The want of an international copyright law has, I know, produced a very unfavorable effect upon higher education in this country. It has prevented the preparation of text-books better suited to the state of education among us than those which are republished from abroad and adopted in many of our institutions of learning.

Another result of the wide diffusion of elementary knowledge without a proper cultivation of the higher intellectual faculties, and an inculcation of generous and unselfish principles, is the inordinate desire for wealth. To acquire power and notoriety in this way requires the least possible amount of talents and intelligence, and yet success in this line is applauded, even if obtained by a rigid application of the dishonest maxim that "*all is fair in trade*." We have a notable example of this fact in the autobiography of an individual who glories in his shame and unblushingly describes the means by which he has defrauded the public. No one who has been called upon to disburse public money can have failed to be astonished at the loose morality on the part of those who present claims for liquidation. The old proverb here is very generally applied, namely, "the public is a goose, and he is a fool who does not pluck a feather!" A full treasury, instead of being considered a desirable or healthy state of the nation, should be regarded as the precursor of a diseased condition of the public morals. That the tendencies which I have mentioned do to a greater or less extent exist, and that they require the serious consider-

ation of the enlightened statesman and the liberal-minded and judicious friend of education, must be evident to every one who seriously and without prejudice observes the habits of the times.

The proper appreciation of profound learning and abstract science is not as a general rule what it ought to be. The most authoritative teacher is the editor of a newspaper. Whatever may have been his previous training, or however circumscribed his field of thought, he is the umpire to decide upon all questions even of the most abstract science or the most refined casuistry.

The question may be asked with solicitude—Are the tendencies we have mentioned inevitable? Are there no means of counteracting them? And is our civilization to share the fate of that of Egypt, Greece, and Rome? Is humanity destined to a perpetual series of periodical oscillations of which the decline is in proportion to the elevation? We answer, No! Though there have been oscillations, and will be again, they are like those which constitute the rising flood-tide of the ocean, although separated by depressions, each is higher than the one which preceded it. Something may have been *lost* at intervals; but on the whole more has been and will be *gained*. But how is this to be effected? The man of science and literature, the educator, and the Christian teacher, together with the enlightened editor, must combine their efforts in a common cause, and through the influence of the press, the school, the college, and the pulpit,—send forth a potential voice which shall be heard above the general clamor.

Common school or elementary education is the basis on which the superstructure of the plan of true progress should be established; but it must be viewed in its connection with a general system, and not occupy exclusively the attention and patronage of governments, societies, and individuals; liberal means must also be provided for imparting the most profound instruction in science, literature, and art.

In organizing new States and Territories the amplest provision ought to be made for all grades of education; and it

possible, every individual should have the opportunity offered him of as much mental culture as he is capable of receiving, or desirous of acquiring; notwithstanding comparatively few may have the industry and perseverance necessary to the highest attainment. It is also of the first importance, that modes of instruction be examined and thoroughly discussed, in order that what is valuable in the past should be retained, and what is really an improvement in the present, be judiciously and generously applied.

Having presented some general suggestions in regard to the bearing of education and the efforts of individuals on the progress of humanity, I now propose to offer for consideration a few observations on the theory of the process of instruction.

It may be surprising that the theory of an art so long practiced as that of education should not be definitely settled; but strange as it may appear, the fact is certain that few writers fully agree as to what is the true plan and process of education. No art can be perfect unless it rests upon a definite conception of fundamental principles; or in other words, unless its theory be well established upon a general law of nature. The laws which govern the growth and operations of the human mind are as definite and as general in their application as those which apply to the material universe; and it is evident that a true system of education must be based upon a knowledge and application of these laws. Unfortunately however psychologists have not classified and exhibited them in a form sufficiently definite to render their application easy, and the directors of education have too often considered merely the immediate practical result which might follow a particular course of training rather than that which would be conducive to the highest development of the individual. In this condition of the theory of education, I have myself ventured to speculate upon the subject, and though I may have nothing new of value to offer, it is my duty at this time to make such suggestions as may furnish topics of discussion or serve to illustrate established truths.

The theory which I would present for your consideration and critical examination, and which appears to me to be in accordance with the results of experience, may be briefly expressed as follows:

The several faculties of the human mind are not simultaneously developed, and in educating an individual we ought to follow the order of nature, and to adapt the instruction to the age and mental stature of the pupil. If we reverse this order, and attempt to cultivate faculties which are not sufficiently matured, while we neglect to cultivate those which are, we do the child an irreparable injury. Memory, imitation, imagination, and the faculty of forming mental habits exist in early life, while the judgment and the reasoning powers are of slower growth. It is a fact abundantly proved by observation that the mere child by the principle which has been denominated *sympathetic imitation* may acquire the power of expressing his desires and emotions in correct and even beautiful language without knowing or being able to comprehend the simplest principles of philology. He even seizes, as if by a kind of instinct, upon abstract terms, and applies them with ease and correctness; but as life advances the facility of verbal acquisition declines, and with some it entirely disappears. Hence the plan appears to me to be wise and in accordance with nature which makes the acquisition of language an essential part of early elemental education. The same child which acquires almost without effort his vernacular tongue may by a similar process be taught to speak the principal ancient and modern languages. He may also acquire the art of the accountant, and be taught by proper drilling to add long columns of figures with rapidity and correctness without being able to comprehend the simplest abstract principles of number and magnitude. Moreover, it is well known that the memory may be stored at a very early age with valuable rules and precepts, which in future life may become the materials of reflection and the guiding principles of action; that it may be furnished with heroic sentiments and poetic illustrations, with "thoughts which breathe and words that

burn," and which long after will spontaneously spring up from the depths of the mind, at the proper moment, to embellish and to enforce the truths of the future author, statesman, or divine.

But the period of life when acquisitions of this kind are most readily made is not that in which the judgment and reasoning powers can be most profitably cultivated. They require a more advanced age, when the mind has become more matured by natural growth and better furnished with the materials of thought.

Mental education consists in the cultivation of two classes of faculties, viz., the intellectual and the moral.

Intellectual instruction, of which we shall first speak, should have at least three objects:—

1. To impart facility in performing various mental operations.

2. To cultivate the imagination and store the memory with facts and precepts; and

3. To impart the art of thinking, of generalization, of induction and deduction.

The most important part of elementary mental instruction, and that which I have placed first in the foregoing classification, is that of imparting expertness in the performance of certain processes which may be denominated mental arts. Among these arts are spelling, reading, penmanship, drawing, composition, expertness in the first rules of arithmetic, and in the use of different languages. These can only be imparted by laborious drilling on the part of the teacher, and by acquired industry and attention on the part of the pupil. The practice in each case must be so long continued, and the process so often repeated, that it becomes a mental habit, and is at length performed with accuracy and rapidity almost without thought. It is only in early life, while the mind is in a pliable condition, that these mental facilities can most readily and most perfectly be acquired, whereas the higher principles of science, on which these arts depend, can only be thoroughly understood by a mind more fully matured. Expertness in the performance of an art does not de-

pend on a knowledge of its principles, and can be readily acquired without reference to them. The most expert accountants are frequently and perhaps generally those who have no knowledge of the philosophy of figures. On the other hand, a profound acquaintance with the principles of an art may exist without the ability to apply it in practice. I have known of mathematicians who were unable to perform with accuracy and dispatch the processes which constitute the application of the simple rules of multiplication and addition. The same is the case with the art of composition. A most learned rhetorician is not necessarily a fluent and pleasing writer.

The acquisition therefore of these arts should be the principal and prominent object of the primary or common school, and nothing ought to be suffered to usurp their place. Unfortunately the drilling which is at first required to induce the mental habit is so laborious and tedious to the teacher, and in most cases so irksome and distasteful to the pupil, that there is a tendency in our schools, and (I am sorry to say) a growing one, to neglect them, and to substitute other objects of more apparent—but of less intrinsic value. This is not only an irreparable injury to the individual, but also to the public. All the practical operations of life in which these processes are concerned (and they apply to all except those of mere handicraft skill) are badly performed. I may venture to say that the general substitution of instruction in the mere *rationale* of the rules of arithmetic without a proper drilling in the practice would produce more bankruptcies than all the changes of tariffs or fluctuations of trade.

It is an important principle, which should be kept in view by the teacher, that although the practice of an art is at first difficult and requires at each step an effort of mind, yet every repetition renders it easier, and at length we come to exercise it not only without effort, but as a pleasurable gratification of an habitual act. Perseverance therefore in this cause will ultimately receive a grateful reward. It should be impressed upon the minds of the directors of elementary education that the teacher who neglects to train his pupils

to expertness in these processes, or who merely does enough in this way to awaken a distaste, and who fails to overcome this condition of mind by subsequent judicious drilling, is unworthy of his high vocation, and should give place to a more industrious or more philosophical instructor.

All the processes we have enumerated, besides various manipulations and bodily exercises necessary to health, refinement, and convenience, may be taught previous to the age of ten or twelve years. At the same time the memory may be educated to habits of retention and precision; and for this purpose definite, and if possible elegantly expressed rules should be chosen, to be committed without the slightest deviation, and so impressed upon the memory that they will ever after remain a portion of the mental furniture of the man, always ready to be called up when needed, and always to be depended upon for accuracy. The mere understanding of the rule, and the power of being able to express it in a vague and indefinite way in original language, is in my judgment, not of itself sufficient. The memory is an important faculty of the mind, and is susceptible of almost indefinite cultivation. It should however in all cases be subservient to the judgment.

Habits of observation may also be early cultivated, and a boy at the age of twelve years may be taught to recognize and refer to its proper class almost every object which surrounds him in nature; and indeed the whole range of descriptive natural history may be imparted previous to this age.

Nothing, in my opinion, can be more preposterous or mischievous than the proposition so frequently advanced, that the child should be taught nothing but what it can fully comprehend, and the endeavor in accordance with this, to invert the order of nature, and attempt to impart those things which cannot be taught at an early age, and to neglect those which at this period of life the mind is well adapted to receive. By this mode we may indeed produce remarkably intelligent children who will become remarkably feeble men.

The order of nature is that of art before science, the entire concrete first, and the entire abstract last. These two extremes should run gradually into each other, the course of instruction becoming more and more logical as the pupil advances in years.

Thus far we have principally considered only the education of the habits and the memory, and it is particularly to these that the old system of drilling is peculiarly applicable. I know that this custom has, to a considerable degree, fallen into disuse, and the new and less laborious system of early precocious development been substituted in its stead. In this respect the art of instruction among us has retrograded rather than advanced, and "Young America," though a very sprightly boy may fail to become a very profound man!

I would not however by the foregoing remarks have it inferred that the reasoning faculties of the child should not receive due attention, and that clear conceptions of the principle of every process taught should not be elucidated and explained, as far as he is able to understand them; but that the *habits* and the *memory* should be the main objects of attention during the early years of the pupils' course. The error of the old system consisted in continuing the drilling period too long, and in not shading it off gradually into that of the logical, or what might be called the period of the acquisition and use of general principles.

The last part of mental education as given in our classification is that which relates to the cultivation of the judgment and the reasoning powers. These faculties of the mind, as we have repeatedly said, are latest in arriving at maturity, and indeed they may be strengthened continually and improved progressively through a long life, provided they have been properly directed and instructed in youth and early manhood.

They should be exercised in the study of mathematical analysis and synthesis; in deducing particular facts in a logical form from general principles; and instructed in the process of discovering new truths. The cultivation of the imagination should also be considered an essential part of

a liberal education, and this may be spread over the whole course of instruction, for like the reasoning faculties the imagination may continue to be improved until late in life.

From the foregoing remarks it will be evident that I consider the great object of intellectual education to be, not only to teach the pupil how to *think*, but how to *act* and to *do*, and I place great stress upon the early education of the *habits*. And this kind of training may be extended beyond the mental processes to the moral principles; the pupil may be taught on all occasions habitually and promptly, almost without thought, to act properly in any case that may occur, and this in the practical duties of life is of the highest importance. We are frequently required to act from the impulse of the moment, and have no time to deduce our course from the moral principles of the act. An individual can be educated to a strict regard for truth, to deeds of courage in rescuing others from danger, to acts of benevolence, of generosity, and justice; or on the other hand, though his mind may be well stored with moral precepts, he may be allowed to fall into opposite habits alike prejudicial to himself and to those with whom he is associated. He may "know the right, and yet the wrong pursue."

Man is the creature of habit; it is to him more than second nature; but unfortunately, while bad habits are acquired with readiness, on account of the natural desire to gratify our passions and appetites, good habits can only be acquired by unremitting watchfulness and labor. The combined habits of individuals form the *habits of a nation*, and these can only be moulded, as I have before said, by the coercive labor of the instructor judiciously applied.

The necessity of early and judicious moral training is often referred to, but its importance is scarcely sufficiently appreciated. The future character of a child, and that of the man also, is in most cases formed, probably, before the age of seven years. Previously to this time impressions have been made which shall survive amid all the vicissitudes of life—amid all the influences to which the individual may be subjected, and which will outcrop, as it were,

in the last stage of his earthly existence, when the additions to his character, made in later years, have been entirely swept away. In connection with this point I may mention one idea which has occurred to me, and which I have never seen advanced; but which, if true, invests the subject of early impressions with a fearful interest. The science of statistics shows that certain crimes which are common in the seasons of youth disappear, comparatively, with advancing age, and re-appear again toward the close of life; or in other words, that the tendencies to indulgences in disorders of imagination, and habits which were acquired in the early life of a vicious youth, or one exposed to evil associations, though they may be masked and kept in subjection by the judgment and the influences of position and reputation during early manhood, middle life, and first decline, resume their sway and close the career of the man who has perhaps for years sustained a spotless reputation—with ignominy and shame. How frequently do cases of this kind present themselves! I have now in my mind's eye an individual who for forty years was known and esteemed as a model of honor, purity, and integrity, but who at the age of seventy committed a crime which consigned his name to infamy. Depend upon it, this man was subjected to evil influences in early life, and the impressions then made, though neutralized by the conditions and circumstances which afterwards surrounded him, were never effaced, and when the latter ceased to produce their restraining effects, the former resumed their original sway. Pursuing this train of thought we would conclude that the child is not merely the father of the man, but more emphatically, the father of the *old* man; that the term second childhood has a more extended signification than that of the mere decline of the faculties. It also should convey the idea that the tendency of the dispositions and propensities of individuals is to return to the condition of earlier life. This principle is important also in an historical point of view. The aged, though they may forget the occurrences of middle and after life, recall with vivid distinctness the impressions of childhood, and thus the grand-

father with senile garrulity, transmits the history of his early times, as it were, across an intervening generation to his grandson. This again makes an indelible impression upon the plastic mind of his youthful auditor, to be alike transmitted to *his* children of the third generation. Abundant examples might be adduced to illustrate the proposition of the vivid recurrence of the effects of early impressions apparently effaced. Persons who have for long years been accustomed to speak a foreign language, and who have forgotten the use of any other, have frequently been observed to utter their dying prayers in their mother tongue.

In this country, so far as I have observed, the course of education is defective in two extremes; it is defective in not imparting the mental habits or facilities which can most easily be acquired in early life, and it is equally defective in the other extreme, in not instructing the student, at the proper period, in processes of logical thought, or deductions from general principles. While elementary schools profess to teach almost the whole circle of knowledge, and neglect to impart those essential processes of mental art of which we have before spoken, our higher institutions, with some exceptions, fail to impart knowledge, except that which is of a superficial character. The value of facts, rather than of general principles, is inculcated. The one however is almost a consequence of the other. If proper seeds are not sown, a valuable harvest cannot be reaped.

The organization of a system of public education in accordance with my views would be that of a series of graded schools, beginning with the one in which the mere rudiments of knowledge are taught, and ending with that in which the highest laws of mind and matter are unfolded and applied. Every pupil should have the opportunity of passing step by step through the whole series, and honors and rewards should be bestowed upon those who graduated in the highest school. Few however as I have said before, would be found to possess the requisite talent and perseverance necessary to finish a complete course. But at whatever period the pupil may abandon his studies, he should be

found fitted for some definite pursuit or position in life, and be possessed of the moral training necessary to render him a valuable citizen and a good man.

These are some of the subjects which I commend for discussion at the present meeting of the Association. The great aim should be to enforce the importance of thorough early training and subsequent high education. It should be our object to bring more into repute profound learning, and to counteract the tendency to the exclusive diffusion of popular and mere superficial knowledge. We should endeavor to enlarge the pyramid of knowledge by symmetrical increments, by elevating the apex, and expanding the base, always observing the conditions of stable equilibrium.

ON THE MODE OF TESTING BUILDING MATERIALS, AND AN ACCOUNT OF THE MARBLE USED IN THE EXTENSION OF THE UNITED STATES CAPITOL.

(Proceedings American Association Adv. of Science, vol. IX, pp. 102–112.)*

August 16, 1855.

A commission was appointed by the President of the United States, in November, 1851, to examine the marbles which were offered for the extension of the United States Capitol, which consisted of General Totten, A. J. Downing, the Commissioner of Patents, the Architect, and myself. Another commission was subsequently appointed, in the early part of the year 1854, to repeat and extend some of the experiments, the members of which were General Totten, Professor Bache, and myself.

A part of the results of the first commission was given in a report to the Secretary of the Interior, and a detailed account of the whole of the investigations of these committees will ultimately be presented in full in a report to Congress; and I propose here merely to present some of the facts of general interest, or which may be of importance to those engaged in similar researches.

Though the art of building has been practiced from the earliest times, and constant demands have been made in every age, for the means of determining the best materials, yet the process of ascertaining the strength and durability of stone appears to have received but little definite scientific attention; and the members of the commission, who had never before made this subject a special object of study, were surprised with unforeseen difficulties at every step of their progress, and came to the conclusion that the processes usually employed for solving these questions are still in a very unsatisfactory state.

It should be recollected that while the exterior materials

*[Re-printed in Silliman's American Journal of Science, July, 1856; vol. XXII, pp. 30–38. Also in the Smithsonian Report for 1856; pp. 303–310.]

of a building are to be exposed for centuries, the conclusions desired are to be drawn from results produced in the course of a few weeks. Besides this, in the present state of science we do not know all the actions to which the materials are subjected in nature, nor can we fully estimate the amount of those which are known.

The solvent power of water, which attacks even glass must in time produce an appreciable effect on the most solid material, particularly where it contains, as the water of the atmosphere always does, carbonic acid in solution. The attrition of siliceous dusts, when blown against a building, or washed down its sides by rain, is evidently operative in wearing away the surface, though the evanescent portion removed at each time may not be indicated by the nicest balance. An examination of the basin which formerly received the water from the fountain at the western entrance of the Capitol, now deposited in the Patent Office, will convince any one of the great amount of action produced principally by water charged with carbonic acid. Again, every flash of lightning not only generates nitric acid, (which in solution in the rain acts on the marble,) but also by its inductive effects at a distance produces chemical changes along the moist wall, which are at the present time beyond our means of estimating. Also the constant variations of temperature from day to day, and even from hour to hour, give rise to molecular motions which must affect the durability of the material of a building. Recent observations on the pendulum have shown that the Bunker Hill Monument is scarcely for a moment in a state of rest, but is constantly warping and bending under the influence of the ever varying temperature of its different sides.

Moreover, as soon as the polished surface of a building is made rough from any of the causes aforementioned, the seeds of minute lichens and mosses, which are constantly floating in the atmosphere, make it a place of repose, and from the growth and decay of the microscopic plants which spring from these, dis-coloration is produced, and disintegration assisted.

But perhaps the greatest source of dilapidation in a climate like ours, is that of the alternations of freezing and thawing which take place during the winter season; but though this effect must be comparatively large, yet, in good marble, it requires the accumulated results of a number of years, in order definitely to estimate its amount.

From a due consideration of all the facts, the commission is convinced that the only entirely reliable means of ascertaining the comparative capability of marble to resist the weather, is to study the actual effects of the atmosphere upon it as exhibited in buildings which for years have been exposed to these influences. Unfortunately however, in this country, but few opportunities for applying this test are to be found. It is true some analogous information may be derived from the examination of the exposed surfaces of marble in their out-crops at the quarry; but in this case the length of time they have been exposed, and the changes of actions to which they may have been subjected during perhaps long geological periods, are unknown; and since different quarries may not have been exposed to the same action they do not always afford definite data for accurate comparative estimates of durability, except where different specimens occur in the same quarry.

As we have said before, the art of testing the quality of stone for building purposes is at present in a very imperfect state; the object is to imitate the operations of nature, and at the same time to hasten the effect by increasing the energy of the action, and after all, the result may be deemed but as approximative, or to a considerable degree—merely probable.

About twenty years ago an ingenious process was devised by M. Brard, which consists in saturating the stone to be tested with a solution of the sulphate of soda. In drying this salt crystallizes and expands, thus producing an exfoliation of surface which is supposed to imitate the effect of frost. Though this process has been much relied on, and generally employed, recent investigations made by Dr. Owen lead us to doubt its perfect analogy with that of the operations of nature. He found that the results produced by the actual

exposure to freezing and thawing in the air during a portion of winter, in the case of the more porous stones, produced very different results from those obtained by the use of the salt. It appears from his experiments that the action of the latter is chemical as well as mechanical.

The commission, in consideration of this, has attempted to produce results on the stone by freezing and thawing by means of artificial cold and heat. This process is however laborious; each specimen must be inclosed in a separate box fitted with a cover, and the amount of exfoliation produced is so slight that in good marble the operation requires to be repeated many times before satisfactorily comparable results can be obtained. In prosecuting this part of the inquiries, unforeseen difficulties have occured in ascertaining precisely the amount of the disintegration, and it has been found that the results are liable to be vitiated by circumstances which were not foreseen at the commencement.

It would seem at first sight, (and the commission when it undertook the investigation held the opinion,) that but little difficulty would be found in ascertaining the strength of the various specimens of marbles. In this however it was in error. The first difficulty which occurred was to procure the proper instrument for the purpose. On examining the account of that used by Rennie, and described in the Transactions of the Royal Society of London, the commission found that its construction involved too much friction to allow of definite comparative results. Friction itself has to be overcome as well as the resistance to compression, and since it increases in proportion to the pressure, the stronger stones would appear relatively to withstand too great a compressing force.

The commission first examined a hydraulic press which had previously been employed in experiments of this kind, for the use of the Government, but found that it was liable to the same objection as that of the machine of Rennie. The commission was however extremely fortunate in obtaining subsequently, through the politeness of Commodore Ballard, commandant of the Navy Yard, the use of an admirable in-

strument devised by Major Wade, late of the United States Army, and constructed, under his direction, for the purpose of testing the strength of gun metals. This instrument consists of a compound lever, the several fulcra of which are knife-edges opposed to hardened steel surfaces. The commission verified the delicacy and accuracy of the indications of this instrument by actual weighing, and found, in accordance with the description of Major Wade, the equilibrium was produced by *one* pound in opposition to *two hundred*. In the use of this instrument the commission was much indebted to the experience and scientific knowledge of Lieutenant J. A. Dahlgren, of the Navy Yard, and to the liberality with which all the appliances of that important public establishment were put at its disposal.

Specimens of the different samples of marble were prepared in the form of cubes of one inch and a half in dimension, and consequently exhibiting a base of two and a quarter square inches. These were dressed by ordinary workmen with the use of a square, and the opposite sides made as nearly parallel as possible by being ground by hand on a flat surface. They were then placed between two thick steel plates, and in order to insure an equality of pressure, independent of any want of perfect parallelism and flatness on the two opposite surfaces, a thin plate of lead was interposed above and below between the stone and the plates of steel. This was in accordance with a plan adopted by Rennie, and the one which appears to have been used by most—if not all—of the subsequent experimenters, in researches of this kind. Some doubt however was expressed as to the action of interposed lead, which induced a series of experiments to settle this question, when the remarkable fact was discovered that the yielding and approximately equable pressure of the lead caused the stone to give way at about half the pressure it would sustain without such an interposition. For example, one of the cubes, precisely similar to another which withstood a pressure of upwards of 60,000 pounds when placed in immediate contact with the steel plates, gave way at about 30,000 with lead interposed. This interesting fact was verified in a series of

experiments embracing samples of nearly all the marbles under trial, and in no case did an exception occur to vary the result.

The explanation of this remarkable phenomenon—now that it is known, is not difficult. The stone tends to give way by bulging out in the centre of each of its four perpendicular faces, and to form two pyramidal figures with their apices opposed to each other at the centre of the cube, and their bases against the steel plates. In the case where rigid equable pressure is employed, as in that of the thick steel plate, all parts must give way together. But in that of a *yielding* equable pressure, as in the case of interposed lead, the stone first gives way along the outer lines or those of least resistance, and the remaining pressure must be sustained by the central portion around the vertical axis of the cube.

After this important fact was clearly determined, lead and all other interposed substances were discarded, and a method devised by which the upper and lower surfaces of the cube could be ground into perfect parallelism. This consists in the use of a rectangular iron frame into which a row of six of the specimens could be fastened by a screw at the end. The upper and lower surfaces of this iron frame were wrought into perfect parallelism by the operation of a planing machine. The stones being fastened into this, with a small portion of the upper and lower parts projecting, the whole were ground down to a flat surface until the iron and the face of the cubes were thus brought into a continuous plane. The frame was then turned over and the opposite surfaces ground in like manner. Care was of course taken that the surfaces thus reduced to perfect parallelism, in order to receive the action of the machine, were parallel to the natural beds of the stone.

All the specimens tested were subjected to this process, and in their exposure to pressure were found to give concordant results. The crushing force exhibited was therefore much greater than that heretofore given for the same material.

The commission also determined the specific gravities of the different samples submitted to its examination, and also the quantity of water which each absorbs.

The commission considers these determinations, and particularly that of the resistance to crushing,—tests of much importance, as indicating the cohesive force of the particles of the stone, and its capacity to resist most of the influences before mentioned.

The amount of water absorbed may be regarded as a measure of the antagonistic force to cohesion, which tends, in the expansion of freezing, to disintegrate the surface. In considering however the indication of this test, care must be taken to make the comparison between marbles of nearly the same texture, because a coarsely crystallized stone may apparently absorb a small quantity of water, while in reality the cement which unites the crystals of the same stone may absorb a much larger quantity. That this may be so was clearly established in the experiments with the coarsely crystallized marbles examined by the commission. When these were submitted to a liquid which slightly tinged the stone, the coloration was more intense around the margin of each crystal, indicating a greater amount of absorption in these portions of the surface.

The marble chosen for the Capitol is a dolomite, or in other words, is composed of carbonate of lime and magnesia in nearly atomic proportions. It was analyzed by Dr. John Torrey, of New York, and Dr. Frederick A. Genth, of Philadelphia. According to the analysis of the former it consists, in hundredth parts, of—

Carbonate of lime	54·621
Carbonate of magnesia	43·932
Carbonate of protoxide of iron	·365
Carbonate of protoxide of manganese	(a trace)
Mica	·472
Water and loss	·610

The marble is obtained from a quarry in the south-easterly part of the town of Lee, in the State of Massachusetts, and belongs to the great deposit of primitive limestone which abounds in that part of the district. It is generally white with occasional blue veins. The structure is fine-grained. Under the microscope it exhibits fine crystals of colorless mica.

and occasionally also small particles of bisulphuret of iron. Its specific gravity is 2·8620; its weight 178·87 lbs. per cubic foot; it absorbs ·103 parts of an ounce per cubic inch, and its porosity is great in proportion to its power of resistance to pressure. It sustains 23,917 lbs. to the square inch. It not only absorbs water by capillary attraction, but, in common with other marbles, suffers the diffusion of gases to take place through its substance. Dr. Torrey found that hydrogen and other gases, separated from each other by slices of the mineral, diffuse themselves with considerable rapidity through the partition.

This marble, soon after the workmen commenced placing it in the walls, exhibited a discoloration of a brownish hue, no trace of which appeared so long as the blocks remained exposed to the air in the stone-cutter's yard. Various suggestions and experiments were made in regard to the cause of this remarkable phenomenon, and it was finally concluded that it was due to the previous absorption by the marble—of water holding in solution a small portion of organic matter, together with the absorption of another portion of water from the mortar.

To illustrate the process, let us suppose a fine capillary tube, the lower end of it immersed in water, and of which the internal diameter is sufficiently small to allow the liquid to rise to the top, and be exposed to the atmosphere, evaporation will take place at the upper surface of the column, a new portion of water will be drawn up to supply the loss; and if this process be continued any material which may be dissolved in the water, or mechanically mixed with it, will be found deposited at the upper orifice of the tube, or at the point of evaporation.

If however the lower portion of the tube be not furnished with a supply of water, the evaporation at the top will not take place, and the deposition of foreign matter will not be exhibited, even though the tube itself may be filled with water impregnated with impurities. The pores of the stones, so long as the blocks remain in the yard, are in the condition of the tube not supplied at its lower end with water, and

consequently no current takes place through them, and the amount of evaporation is comparatively small; but when the same blocks are placed in the wall of the building the absorbed water from the mortar at the interior surface gives the supply of the liquid necessary to carry the coloring material to the exterior surface, and deposit it at the outer orifices of the pores.

The cause of the phenomenon being known, a remedy was readily suggested, which consisted in covering the surface of the stone to be embedded in mortar with a coating of asphaltum. This expedient has apparently proved successful. The discoloration is gradually disappearing and in time will probably be entirely imperceptible.

This marble, with many other specimens, was submitted to the freezing process fifty times in succession. It generally remained in the freezing mixture for twenty-four hours, but sometimes was frozen twice in the same day. The quantity of material lost was ·00315 part of an ounce. On these data Captain M. C. Meigs has founded an interesting calculation, which consists in determining the depth to which the exfoliation extended below the surface as the effect of its having been frozen fifty times. He found this to be very nearly the ten thousandth part of an inch. Now, if we allow the alterations of freezing and thawing in a year on an average to be fifty times each, which in this latitude would be a liberal one, it would require ten thousand years for the surface of the marble to be exfoliated to the depth of one inch. This fact may be interesting to the geologist as well as to the builder.

Quite a number of different varieties of marble were experimented upon. A full statement of the result of each will be given in the reports of the committees.

On molecular Cohesion.

At the meeting of the Association at Cleveland I made a communication on the subject of *Cohesion*.* The paper how-

* [Proceedings of the American Association for the Advancement of Science, July, 1853; vol. VII, p. 270. Only the title published.]

ever was presented at the last hour; the facts were not fully stated and have never been published. I will therefore briefly occupy your time in presenting some of the facts I then intended to communicate, and which I have since verified by further experiments and observations.

In a series of experiments made some ten years ago I showed that the attraction of the particles for each other—of a substance in a liquid form—was as great as that of the same substance in a solid form.* Consequently, the distinction between liquidity and solidity does not consist in a difference in the attractive power occasioned directly by the repulsion of heat; but it depends upon the perfect mobility of the atoms,—the loss of adhesion, or *lateral* cohesion. We may explain this by assuming an incipient crystallization of atoms into molecules, and consider the first effect of heat as that of breaking down these crystals and permitting each atom to move freely around each other. When this crystalline arrangement is perfect, and no lateral motion is allowed in the atoms, the body may be denominated perfectly rigid. We have approximately an example of this in cast-steel, in which no slipping takes place of the parts on each other, or no material elongation of the mass; and when a rupture is produced by a tensile force a rod of this material is broken with a transverse fracture of the same size as that of the original section of the bar. In this case every atom is separated at once from the other, and the breaking weight may be considered as a measure of the aggregate attraction of cohesion among the whole transverse series of the atoms of the metal.

The effect however is quite different when we attempt to pull apart a rod of lead. The atoms or molecules slip upon each other. The rod is increased in length and diminished in thickness until a separation is produced. Instead of lead we may use still softer materials, such as wax, putty, &c., until at length we arrive at a substance in a liquid form. This will stand at the lower extremity of the scale, and between extreme rigidity on the one hand, and extreme liquid-

*[Proceedings Am. Philosophical Society. See *ante*, page 217.]

ity on the other, we may find a series of substances gradually shading from one extremity to the other.

According to the views I have presented the difference in the tenacity in steel and lead does not consist in the attractive *cohesion* of the atoms, but in their capability of slipping upon each other. From this view it follows that the form of the material ought to have some effect upon its tenacity, and also that the strength of the article should depend in some degree upon the process to which it has been subjected.

For example, I have found that softer substances in which the outer atoms have freedom of motion, while the inner ones by the pressure of those exterior are more confined, break unequally; the inner fibres (if I may so call the rows of atoms) give way first and entirely separate, while the exterior fibres show but little indications of a change of this kind.

If a cylindrical rod of lead three quarters of an inch in diameter be turned down on a lathe in one part to about half an inch, and then be gradually broken by a force exerted in the direction of its length, it will exhibit a cylindrical hollow along its axis of half an inch in length, and at least a tenth of an inch in diameter. With substances of greater rigidity this effect is less apparent, but it exists even in iron, and the interior fibres of a rod of this metal may be entirely separated, while the outer surface presents no appearance of change.

From this it would appear, that metals should never be elongated by mere stretching, but in all cases by the process of wire-drawing, or rolling. A wire or bar must always be weakened by a force which permanently increases its length —without at the same time compressing it.

Another effect of the lateral motion of the atoms of a soft heavy body, when acted upon by a percussive force with a hammer of small dimensions in comparison with the mass of metal, (for example, if a large shaft of iron be hammered with an ordinary sledge,) is a tendency to expand the surface so as to make it separate from the middle portions. The interior of the mass by its own inertia becomes as it were an anvil, between which and the hammer, the exterior portions

are stretched longitudinally and transversely. I here exhibit to the Association a piece of iron—originally from a square bar four feet long, which has been so hammered as to produce a perforation of the whole length entirely through the axis. The bar can be seen through as if it were the tube of a telescope.

This fact appears to me to be of great importance in a practical point of view, and may be connected with many of the lamentable accidents which have occured in the breaking of the axles of locomotive engines. These, in all cases, ought to be formed by *rolling* and not with the hammer.

The whole subject of the molecular constitution of matter offers a rich field for investigation, and isolated facts which are familiar to almost every one, when attentively studied, will yield results alike interesting to abstract science and to practical art.

ON THE EFFECT OF MINGLING RADIATING SUBSTANCES WITH COMBUSTIBLE MATERIALS.

(Proceedings American Association Adv. of Science, vol. IX, pp. 112–116.)

August 17, 1855.

I beg leave to call the attention of the Association for a few moments to a paper published by our distinguished countryman, Count Rumford, in 1802, in the first volume of the Journal of the Royal Institution of Great Britain, page 28, entitled "Observations relative to the Means of Increasing the Quantities of Heat obtained in the Combustion of Fuel."

"It is a fact," says Count Rumford, "which has long been known, that clay and several other incombustible substances, when mixed with sea-coal in certain proportions, cause the latter to give out more heat in its combustion than it can be made to produce when it is burnt pure or unmixed."

"It has been ascertained that when the sides and back of an open chimney fire-place, in which coals are burnt, are

composed of fire-brick and heated red hot, they throw off into the room more heat than the burning coals themselves."

"The fuel therefore," says Count Rumford, "should be disposed or placed so as to heat the back and sides of the grate, which must always be constructed of fire-brick and never of iron."

The vertical stratum of coal should be as thin as is consistent with perfect combustion, for a large mass of coal in the grate arrests the rays which proceed from the back and sides of the grate, and prevents their coming into the room. The grate or fire-place itself may be so contrived as to produce a proper degree of radiation, but when this is not the case, Rumford advises that the bottom of the grate be covered with a single layer of balls of fire-brick, each perfectly globular and two and a half or two and three quarters inches in diameter. "On this layer of balls fire is to be kindled, and in filling the grate more balls are to be added with the coals, care being taken to mix the coals and balls well together in due proportions. If this is done the fire will not only be very beautiful, but will send off a much greater quantity of radiant heat into the room than without them." Rumford also declares that these balls cause the cinders to be almost entirely consumed. "The same effect is said to be produced by the mixture of coals and clay when the fuel is burnt in a a close fire-place, such as an iron stove; and it is the custom in the Netherlands to mix moistened clay with the coals before they are introduced into a stove of this form."

Count Rumford gives no account, in the paper I have cited, of experiments by which the fact of the greater radiation from the balls was tested.

In reading his paper some years since the idea occurred to me that this experiment would be worthy of repetition, with the more manageable and delicate appliances which science has of late years furnished for the use of the investigator. For this purpose I employed the thermo-electrical apparatus of Melloni, furnished with a tube like a telescope to circumscribe the field of radiation, and the result confirmed the statement of Rumford that more heat was radiated from

pieces of fire-brick mingled with the coal than from the combustible itself. The effect however would probably have been greater with bituminous coal. The arrangement for experimenting with coal in a fire-place was very imperfect, and I had recourse to the heat produced by the flame of a spirit lamp, and also of a jet of hydrogen. A flame of this kind was placed before the thermo-pile at such a distance that the needle of the galvanometer stood at 15°; the end of the platinum wire coiled into a spiral form was then introduced into the flame, and an instant increase of the radiation of heat was observed, the galvanometer advancing to 27°.

It has long been well known that the introduction of a platinum wire into a pale flame of this character greatly increases the radiation of light, and from this experiment it is evident that the radiation of heat is increased in a like degree. After this a number of different substances were employed, such as glass, carbonate of lime, sulphate of lime, stone coal, fire clay, &c. The greatest effect appeared to be produced with pieces of carbonate of lime. The exact order however could not be determined without procuring a series of balls of the same diameter of these different substances. The most striking effect was produced at the very top of the flame, placing the platinum wire in the heated though almost non-luminous air, immediately above the highest point of combustion.

We cannot suppose in these experiments that the absolute amount of heat produced by the combustion of a given quantity of fuel is increased. The most probable conjecture is that the heat of combination is converted into radiant heat, and that the flame itself is cooled in proportion as the radiation is increased. In order to bring this idea to the test of experiment, a slip of mica, one-fifth of an inch in breadth, was introduced vertically into the lower part of the flame, while the platinum wire occupied the space just above the top. The slip of mica was placed with its flat side vertically so as not to affect by its radiation the heat of the wire. With this arrangement the radiation of heat from the platinum

was diminished. A corresponding diminution was also produced in the amount of radiant light given off, and this was readily perceptible to the sight. This effect was not due to the cooling of the flame by the conduction of the mica, since it is almost a non-conductor of heat, and this property was exhibited by the fact that the luminosity of the mica was confined to that part which was at the surface of the flame on either side.

It appears therefore from these experiments, that the introduction of a solid of great radiating power into a mass of materials in a state of combustion, increases the amount of heat thrown into the space around, without increasing the absolute quantity produced by combustion, the increase of radiant heat being at the expense of the heat of combination. To give a practical illustration of the condition of the matter, if a given quantity of fuel is employed in evaporating water, by combustion under a kettle, the useful effect would be diminished by inserting in the flame beneath or amid the combustible a better radiating substance than itself, while in the case of a fire to warm a room the effect would be directly opposite; a greater amount of heat would be thrown into room, and less of the heat of combustion would be carried up the chimney with the escaping gas. Or to give another example. If over a coal fire a boiling pot be suspended, and a roasting oven before it, the introduction of a radiating material would increase the effect on the latter at the expense of that on the former.

Count Rumford has elsewhere shown that flame is a bad conductor of heat, and in stoves and boilers heated by flame it is therefore necessary that the draft be made to impinge with considerable force upon projecting portions of the metal in order that the greatest amount of heat may be absorbed.

If a column of heated air moves rapidly through a perpendicular stove-pipe, but a comparatively small portion of the heat will be absorbed by the metal and radiated into space around. A cylindrical stratum of non-conducting air in contact with the metal will be comparatively at rest, and through this the moving column of heated air will rapidly

ascend, without communicating its heat to the metal. If however in this case the current of air be obstructed, and the cylindrical motion deranged by partially closing a damper, the heat immediately around the point of obstruction will be greatly increased. With a proper arrangement of parts I have known a dark stove-pipe immediately to become red opposite and above the damper by the partial closing of this valve. It is probable that heat might be economized in certain cases by introducing radiating materials in flues. It should however be recollected that the draft would be impeded by the introduction of foreign materials: 1st. On account of a direct obstruction; and, 2nd. Because of the diminished temperature.

It is frequently stated in works on chemistry that the heating power of the flame of the compound blowpipe is very great, while its illuminating power is quite small. The truth is however that the radiation of heat from its flame is only commensurate with its radiation of light, and that what tends to increase the one will also increase the other.

The radiation from heated—though non-luminous air, would, from these views, appear to be small, though from meteorological considerations they would seem to be considerable.

That a solid substance increases the radiation of the heat of a flame is an interesting fact in connection with the nature of heat itself. It would seem to show that the vibrations of gross matter are necessary to give sufficient intensity of impulse to produce the phenomena of ordinary radiant heat. Also, since the light is much increased by the same process we would infer that by means of the solid the vibrations constituting heat are actually converted into those which produce the phenomena of light. The whole subject is worthy of further investigation, both in a practical and abstract scientific point of view.

ACCOUNT OF EXPERIMENTS ON THE ALLEGED SPONTANEOUS SEPARATION OF ALCOHOL AND WATER.

(Proceedings American Association Adv. of Science, vol. IX, pp. 140–144.)

August 20, 1855.

At the last meeting of the American Association a notice was given of a new process for procuring alcohol, for which a patent had been granted. The weak spirit, left to itself in a vessel of great height, was said to separate spontaneously into a strong alcohol, which rose to the top of the column, and into a weaker spirit which was found at the bottom.

For the following statement and remarks relative to granting the patent I am indebted to Dr. Leonard Gale, one of the principal examiners of the Patent Office:

"When the alleged invention was presented much doubt was expressed as to the working of the plan, and the author was requested to answer the following questions to satisfy the office on the subject:

"'Have you employed this device for purifying alcohol or whiskey? If so, please state what kind, what size, and what proportioned apparatus you have used on a working scale, and what results you have obtained.'

"To this the applicant replies—

"'I have used this device as a mode of separating alcohol from whiskey for several months. The column was of wrought iron about one hundred feet high, and twelve inches in diameter. It was elevated from the cellar through and above the building; the whiskey was forced in from the upper room of the building through an iron pipe leading over the top of the column, and down the inside about fifty feet. This sized column will, I find, separate about two hundred gallons of alcohol from the water in the space of twelve hours. The larger the diameter the more rapid the process of separation.'

"It had been stated by the party in correspondence that he had been led to the trial of the experiment by noticing

that the liquor in the upper part of a tall standing cask was thought to be stronger than that drawn out near the bottom."

This statement would seem to receive some countenance from the following remarks on the same subject in Gmelin's Treatise on Chemistry, vol. I, p. 112, English edition:

"Similarly brandy kept in casks is said to contain a greater proportion of spirit in the upper, and of water in the lower part. Here again the question may be raised whether the cask may have been filled with successive portions of different strengths which may have disposed themselves in layers one above another."

"As to the propriety," says Dr. Gale, "of granting or refusing a patent, on the evidence before the office, in consideration of the oath of the inventor, the want of means in the office to satisfactorily verify or disprove the experiment, and lastly, the subsequent statement of the inventor that he had verified the experiment by several months' work on a practical scale, these facts were regarded as good ground for issuing the patent. If the party should be found to have made a false statement, and so committed a fraud on the Patent Office, these acts were his own, and for which he must be held responsible."

If the result said to be obtained were true, it would follow that the affinity of bodies for each other would be modified by pressure. Though from theoretical considerations, it might not be thought impossible that the attraction of two substances for each other might be increased by an increase of pressure, yet there is no antecedent probability that the attraction would be diminished under this influence. But as an account of this invention had been widely circulated in the newspapers, its author had received from the Patent Office the right to vend the privilege of its use, and the public were exposed to be defrauded in the purchase of that which was worthless, it seemed desirable to settle the question as to the truth of the principle by direct experiment, irrespective of theoretical considerations, and on a scale of sufficient magnitude to leave no doubt as to the result.

With this view, in behalf of the Smithsonian Institution,

I accepted the proffered co-operation of Professor George C. Schæffer, of the Patent Office, and directed the putting up of the necessary apparatus in one of the towers of the Smithsonian building. The determination of the density of the liquid, and the details of the experiments, were intrusted to Professor Schaeffer, to whom I am also indebted for the following account of the process employed and the results obtained.

As the successful experiment was said to have been made with a column of liquid nearly one hundred feet high, and as the pressure of such a column was given as the cause of the separation of the water or alcohol from the mixture, the repetition of the experiment should be on a corresponding scale.

The great tower of the Institution building was already fitted for experiments requiring like conveniences. A well, or series of openings giving a height of over one hundred feet, passing through several stories was the place selected. A series of stout iron tubes of about an inch and a half in internal diameter formed the column, the total length of which was one hundred and six feet. Four stop-cocks were provided, one at bottom, one about four feet from the top, and the other two to divide the interval equally or nearly so.

The liquor used was common rye whiskey of 44 per cent. at 60° Fahr., and of 44 on the United States Revenue hydrometer, one of which was used in testing the liquor.

The experiment commenced on the 18th of November, 1854; a leak occurring caused the trial to be limited to the lower thirty feet, after the lapse of a few hours. On the 20th the tube was refilled, and after testing at intervals of a few days, the loss was supplied, the whole apparatus, with each cock and the top sealed up, was left to itself until December 14th, when it was again tried at each cock. With a slightly diminished quantity, about one hundred feet in height, the whole again stood until the 18th of April, 1855, when the tests were again made.

Fortunately for the result, the original liquor had been repeatedly tested at different temperatures; the contents of

every vessel used to contain it having been tried at each of the several fillings of the tube, which were made on the first days of the experiment, when a leak required its discharge for the purpose of tightening the joints. A portion of the original liquid which had been set aside was also tried at the end of the experiment, and at different temperatures.

The readings of the hydrometer were made with as much accuracy as possible under the circumstances, some of them being taken late at night and exposed in the open tower to a violent wind. No pains were spared to test the liquid under every variety of circumstances. At first the windows of the tower were open, but for the last two or three months they were closed. Fifty-four readings were made; nineteen of which were from the original liquid, and the remainder on that drawn from the different cocks. The result may be stated, as follows:

On plotting the readings of indication and temperature they all follow nearly in the same line, the deviations of those taken from the original fluid being quite as great as those taken from either the bottom or top, even after the lapse of months. Or in other words, within the limits of error (the extreme being but a portion of a degree of the hydrometer), there is not the slightest indication of any difference of density between the original liquor and that from the top or bottom of the column after the lapse of hours, days, weeks, or months. The fluid at the bottom of the tube, it must be remembered, was for five months exposed to the pressure of a column of fluid at least one hundred feet high. This pressure however is much within that at which inferior champagne bottles are burst, and if pressure alone could produce such an effect, wine of that kind should have long ere this given instances of it.

As the fact has been taken for granted, and chemists of repute have made use of it, there seems good ground for thus formally refuting an error which, at first sight, would not appear worthy of being dignified by so much notice.

REMARKS ON THE UNITED STATES LIGHT-HOUSE SERVICE.

(Report of the United States Light-House Board for 1873, pp. 3–7.)

October 14, 1873.

No part of the executive branch of the Government includes more diversified duties or involves greater responsibilities than the Light-House Establishment.

The character of the aids which any nation furnishes the mariner in approaching and leaving its shores, marks in a conspicuous degree, its advance in civilization. Whatever tends to facilitate navigation, or to lessen its dangers, serves to increase commerce, and hence is of importance not only to the dwellers on the seaboard, but to the inhabitants of every part of the country. Whoever has surplus products of industry to dispose of has a pecuniary interest in the improvement of commerce.

Every shipwreck which occurs enhances the cost of transportation, and therefore affects the interests of the producer. But it is not alone in view of its economical effects that the light-house system is to be regarded. It is a life-preserving establishment, founded on the principles of Christian benevolence. None can appreciate so well the value of a proper system of this kind as he who has been exposed for weeks and perhaps months to the perils of the ocean, and is approaching in the darkness of night a lee shore. He looks then, with anxious gaze, for the friendly light which is to point the way amid treacherous rocks and sunken shoals to a haven of safety. Or it may be in mid-day, when observations cannot be had, the sun and coast being hid by dense fogs, such as imperil navigation on our northern and western coasts. He then listens with breathless silence for the sound of the fog-trumpet which shall insure his position and give him the desired direction of his course.

With that entire confidence which is inspired by a perfect light-house system the alternatives of life and death, of riches and poverty, are daily hazarded; and therefore it is of the

first importance that the signals, whether of light or sound which indicate the direction of the course, and the beacons which mark the channel, shall be of the most improved character, and that they be under the charge of intelligent, efficient, and trustworthy attendants. But above all, one maxim should ever be observed, namely perfect regularity of exhibition of every signal from night to night and from year to year. A light for example which has been regularly visible from a tower, (it may be for years,) cannot be suffered to fail for a single night, or even for a single hour, without danger of casualties of the most serious character. A failure of such a light to send forth its expected ray is as it were a breach of a solemn promise, which may allure the confiding mariner to a disastrous ship-wreck, or to an untimely death.

In view of these facts our Government early established a light-house system, which though simple and inexpensive at first, has since been extended and improved to meet the wants of an increasing commerce and the unrivalled resources of the country. It has been maintained with an enlightened liberality which indicates a just appreciation of its importance.

The magnitude of the light-house system of the United States may be inferred from the following facts: First, the immense extent of the coast which from the St. Croix River on the boundary of Maine, to the mouth of the Rio Grande in the Gulf of Mexico, includes a distance of over 5000 miles; on the Pacific coast a length of about 1,500 miles; on the great northern lakes about 3,000 miles, and on inland rivers about 700 miles, making a total of more than 10,000 miles. Secondly, the magnitude of the system is exhibited by the fact that nearly every square foot of the margin of the sea throughout the whole extent of 5,000 miles along the Atlantic and Gulf coast is more or less illuminated by light-house rays, the mariner rarely losing sight of one light until he has gained another. Thirdly, the same fact is illustrated by the number of signals now in actual existence as exhibited in the following table:

Total Signals for the Entire Establishment.

Light-houses and lighted beacons	591
Light-houses and lighted beacons finished and lighted during the year ending July 1, 1873	29
Light-ships	21
Fog-signals, operated by steam or hot-air engines	35
Day or unlighted beacons	363
Buoys in position	2,838

To carry on so extended a system necessarily requires a carefully devised organization, based upon the history of all that has been recorded in regard to the subject, and a series of efficient officers and trained assistants.

The duties which belong to the light-house system involve the most varied knowledge and practical skill, a thorough acquaintance with the wants of commerce, engineering abilities of high order, with scientific acquirements, which shall appreciate the value of every new discovery that may find an application in the improvement of signals, and the ability to make or direct such investigations as may from time to time be found desirable. To insure these requisites the organization of the light-house system includes: First, a Light-House Board, consisting of two officers of the Navy, two engineer officers of the Army, and two scientific civilians, with the addition of an officer of the Navy and an engineer officer of the Army as secretaries, who are also members of the Board: Secondly, it also includes twelve inspectors from the Army or Navy, and as many engineer officers from the Army, who have united charge of the twelve districts into which the coast is divided.

The Light-House Board, having charge of the supervision of the whole system, is divided into five committees, to each of which special duties are assigned. These committees are on finance, engineering, floating aids, lighting, and experiments. It is the duty of each member of the Board to render himself intimately acquainted with the details of the business intrusted to his care, as well as to keep himself informed, as far as possible, of the condition of the general system. For this purpose, as well as that of insuring the proper working of the establishment in the several districts, it is advisable that

he should make, from time to time, inspections of light-houses at various points on the coast. The inspector of each district is required to visit, at stated intervals, each light-house within his jurisdiction after completion by the engineers, to correct any delinquencies on the part of the keepers, and to supply oil and other materials necessary to the efficient maintenance of the signals, and finally to inform the engineer as to any repairs which may be required. The district engineers, as well as the engineer officers of the board, find full employment for all the theoretical knowledge and practical skill they possess in the surveys of new sites, making studies for the construction of new permanent aids to navigation (many of them on submarine sites in exposed positions), in planning and rearing the towers, and in fitting up the lenticular apparatus. - - - -

It has been thought that the light-house system is of a practical character, and therefore does not require the aid of high science. But in regard to this, it may be observed that the present system of light-house apparatus, now in use in every part of the civilized world, was invented and introduced into practice in its minutest details by a man of abstract science, the celebrated Fresnel, who shared with Young, of England, the invention of the undulatory theory of light, and its application to all the phenomena of optics.

The light apparatus introduced by the Board as a substitute for that previously in use is principally that of the French system. But the Board has from the first been alive to the introduction of improvements and has carefully considered every suggestion and tested every invention which gave promise of greater economy or efficiency. Instead of sperm-oil, which was first employed, it has introduced, at one-third of the cost, lard-oil, and with this, a required modification of the lamps, particularly those of the larger kind, in order that the oil may be burned at a higher temperature, especially in the northern portions of the United States.

But the greatest improvement which has been introduced is that relative to fog-signals,—indispensable aids to navigation, especially on the northeastern, and western portions of

our coast. At first these signals were principally confined to bells, weighing in some cases from 2,000 to 2,500 pounds. These were rung by winding up a weight which in its descent gave motion to a hammer striking the bell. In regard to this signal, an improvement has been introduced, by which an expenditure of about one-tenth of the power produces an equal effect. Bells are still used in cases where the signal is required to be heard only at a comparatively small distance, but in most cases much more powerful instruments are required, such as are founded on what is called resonance, in which the air itself is the resounding body as well as the conductor of sound. These instruments are of three kinds: first, the ordinary locomotive whistle, much enlarged in size and somewhat modified in form, and blown by steam from a high-pressure tubular boiler; second, the reed-trumpet actuated by air condensed in a reservoir by the power of a caloric engine; third, the syren-trumpet, operated by steam from a boiler sustaining a pressure of from 50 to 70 pounds per square inch. The sound from these instruments is many times more powerful than that from the largest bells. - - - -

The Light-House Board, during the past year, desirous of acquainting itself minutely with any improvements which of late years may have been introduced into the light-house service in Europe, obtained the sanction of the honorable the Secretary of the Treasury to commission Major George H. Elliot, of the Corps of Engineers of the Army and engineer-secretary of the Board, to visit Europe and report upon everything which he might observe relative to light-house apparatus and the management of light-house systems. He has lately returned, after having gathered information which will prove of importance in its application in our country, as is evident from his preliminary report.

Major Elliot was everywhere received with marked cordiality, and every facility was given him to inspect the various coasts and systems of administration, of which full information was furnished him, together with the drawings and models necessary for a perfect acquaintance with the latest improvements which have been adopted in Great Bri-

tain and on the continent. The special thanks of the Board are due to His Royal Highness the Duke of Edinburgh, the master; to Sir Frederick Arrow, the deputy master; and the elder brethren of Trinity House, for the warmth of their reception and the marked distinction they conferred upon him as the representive of the Board; and to M. Leonce Reynaud, inspector-general of bridges and roads, and director of the French light-house service, for his efforts to make the visit of Major Elliot profitable to his country and agreeable to himself.

RESEARCHES IN SOUND, IN RELATION TO FOG-SIGNALLING.

(Report of the United States Light-House Board for 1874, pp. 83–87.)

INTRODUCTION.

Fog.—Among the impediments to navigation none perhaps is more to be dreaded than that which arises from fogs; and consequently the nature of this impediment and the means which may be devised for obviating it are objects of great interest to the mariner. Fogs are in all cases produced when cold air is mingled with warm air saturated with moisture. In this case the invisible vapor of the warmer air is condensed by the cold into minute particles of liquid water, which by their immense number and multiplicity of reflecting surfaces obstruct the rays of light in the same way that a piece of transparent glass when pounded becomes almost entirely opaque and is seen by reflection as a white mass. So greatly does a dense fog obstruct light that the most intense artificial illumination, such as that produced by the combustion of magnesium, by the burning of oxygen and hydrogen in contact with lime, and that produced between the charcoal points of a powerful electrical apparatus are entirely obscured at comparatively short distances. Even the light of the sun, which is far more intense than that of any artificial illumination, is so diminished by a single mile of dense fog that the luminary itself becomes invisible. Recourse must therefore be had to some other means than that of light to enable the mariner to recognize his position on approaching the coast when the land is obscured by fog.

The only means at present known for obviating the difficulty is that of employing powerful sounding instruments which may be heard at a sufficient distance through the fog to give timely warning of impending danger. Investigations therefore as to the nature of sound and its applications to fog-signals become an important object to those in charge of aids to navigation. Such investigations are of special im-

portance in connection with the light-house service of the United States. The north-eastern coast of the United States on the Atlantic, and the entire western coast on the Pacific, included in our territory, are subject—especially during the summer months, to dense fogs which greatly impede navigation as well as endanger life and property.

The origin of the fogs on our coast is readily explained by reference to a few simple principles of physical geography. In the Atlantic ocean there exists a current of warm water proceeding from the Gulf of Mexico between Cuba and Florida which flows along our coast to the latitude of about 35°, and turning gradually to the eastward crosses the Atlantic and impinges against the coast of northern Europe. Throughout its entire course, on account of the immense capacity of water for heat, the temperature of the stream is greater that that of the ocean on either side. In addition to this stream the Atlantic ocean is traversed by another current of an entirely opposite character, one of cold water, which coming from the arctic regions down Davis's Strait, is thrown by the rotation of the earth, against our coast, passing between it and the Gulf-stream, and sinking under the latter as it approaches the southern extremity of the United States. These conditions are those most favorable to the production of fogs, since whenever the warm air, surcharged with moisture, is blown from the Gulf-stream in the Atlantic—over the arctic current along the coast, and mingles with the cold air of the latter, a precipitation of its vapor takes place in the form of fog. Hence, especially in summer, when the wind in the eastern part of the United States is from a south-easterly direction, fogs prevail. As we proceed southerly along the coast, the fog-producing winds take a more easterly direction.

A somewhat similar circulation in the Pacific ocean produces fogs on the western coast of the United States. In this ocean a current of warm water, starting from the equatorial regions, passes along the shores of China and Japan, and following the general trend of the coast, turns eastward and continues along our shore. The northern part of this cur-

rent being warmer than the ocean through which it passes, tends to produce dense fogs in the region of the Aleutian Islands and the coast of Alaska. As this current descends along the American coast into lower latitudes it gradually loses its warmth, and soon assumes the character—in regard to the water through which it passes, of a comparatively colder stream; and to this cause we would attribute the prevalence of fogs on the coast of Oregon and California, which are most prevalent during the spring and early summer, with wind from the north-west and west.

From what has been said, it is evident that the fogs in the Aleutian Islands occur chiefly in summer, when south-westerly winds prevail and mingle the moist air from the warm current with the colder air of the more northerly latitude. In winter, the wind being from the north chiefly, the moist air is driven in an opposite direction, and dense fogs therefore at this season do not prevail.

In regard to the fogs on the coast of Maine, the following interesting facts were furnished me by the late Dr. William Stimpson, formerly of the Smithsonian Institution, and of the Chicago Academy of Sciences, who had much experience as to the weather during his dredging for marine specimens of natural history in the region of Grand Manan Island, at the entrance of the Bay of Fundy.

"So sharply marked," says Dr. Stimpson, "is the difference of temperature of the warm water from the Gulf-stream and that of the polar current, that in sailing in some cases only a few lengths of a ship the temperature of the water will change from 70° to 50°. The fog frequently comes rolling in with the speed of a race-horse; in some cases while dredging, happening to turn my eyes to the south, a bank of fog has been seen approaching with such rapidity that there was scarcely time in which to take compass-bearing of some object on shore by which to steer, before I would be entirely shut in, perhaps for days together." He also mentions the fact that it frequently happened during a warm day, while a dense fog existed some distance from the shore, close in to the latter there would be a space entirely clear; this was

probably due to the reflection and radiation of the heat from the land, which converted the watery particles into invisible vapor.

Dr. Stimpson has also noticed another phenomenon of some interest. "When a dense fog, coming in regularly from the sea, reaches the land, it gradually rises in the atmosphere and forms a heavy, dark cloud, which is frequently precipitated in rain." This rising of fog is not due, according to the doctor, to a surface-wind from the west pressing under it and buoying it upward, since the wind at the time is from the ocean. It is probably due to the greater heat of the land causing an upward current, which when once started, by its inertia carries the cloud up to a region of lower temperature, and hence the precipitation. The height of the fog along the coast is not usually very great, and can be frequently overlooked from the mast-head. The deception as to size and distance of objects as seen in a fog is also a remarkable phenomenon when observed for the first time. A piece of floating wood at a little distance is magnified into a large object, and after much experience the doctor was not able to overcome the delusion. It is said that the sailors in the Bay of Fundy prefer of two evils a fog that remains constant in density to one that is variable, although the variation may be toward a greater degree of lightness, on account of the varying intensity producing a varied and erroneous impression of the size and distance of the objects seen through it. It is also his impression that sound can be heard as well during fog as in clear weather, although there is a delusion even in this, since the source of sound when seen, appears at a greater distance than in a clear atmosphere, and hence the sound itself would appear to be magnified.

Fogs also exist on the Mississippi, especially on the lower portion of the river. They are of two classes, those which result from the cooling of the earth, particularly during the summer in clear nights, with wind probably from a northerly direction, followed by a gentle, warm wind from the south surcharged with moisture, and the other induced by the water of the river, which coming from melting snow of

northern regions, is colder than the air in the vicinity. The air over the river being thus cooled below the temperature of a gentle wind from the south, the moisture of the latter is precipitated. This fog, which occurs in the last of winter, during the spring, and beginning of summer, is very dense, but is confined entirely to the atmosphere above the river, while the other class of fog exists over the land as well.

Fog-signals.—The importance of fog-signals as aids to navigation, especially on the north-eastern portion of our coast, the shore of which is exceedingly bold and to the approach of which the sounding-line gives no sure indication, has been from the first an object of special attention.

At the beginning of the operations of the Light-house Board, such instruments were employed for producing sound as had been used in other countries; these consisted of gongs, bells, guns, horns, &c. The bells were actuated by clock machinery, which was wound up from time to time and struck at intervals of regular sequence by which their position might be identified. The machinery however by which these bells was struck was of a rude character and exceedingly wasteful of power, the weight continuing to descend during the whole period of operation, including the successive intervals of silence. This defect was remedied by the invention of Mr. Stevens, who introduced an escapement arrangement, similar to that of a clock, which is kept in motion by a small weight, a larger one being brought into operation only during the instant of striking.

Bell-buoys were also introduced at various points. These consisted of a bell supported on a water-tight vessel and rung by the oscillation of the waves. But all contrivances of this kind have been found to be untrustworthy; the sound which they emit is of comparatively feeble character, can be heard at but a small distance, and is frequently inefficient during a fog which occurs in calm weather. Besides this, automatic fog-signals are liable to be interfered with by ice in northern positions, and in all sections—to derangement at times when no substitute can be put in their place, as can be in the cases of the bells rung by machinery under the immediate con-

trol of keepers. A signal which is liable to be interrupted in its warnings is worse than no signal, since its absence may give confidence of safety in the midst of danger, and thus prevent the necessary caution which would otherwise be employed.

Guns have been employed on the United States coast, first under the direction of General Bates, engineer of the twelfth district, at Point Bonita, San Francisco Bay, California. The gun at this station consisted of a 24-pounder, furnished by the War Department. The necessary arrangements being made, by the construction of a powder-house, and laying of a platform, and employment of a gunner,—notice to mariners was given that after the 8th of August, 1856, a signal-gun would be fired every hour and half-hour, night and day, during foggy or thick weather. The first year, with the exception of eighty-eight foggy days, omitted for want of powder, 1390 rounds were fired. These consumed 5560 pounds of powder, at a cost of $1,487, pay of gunner and incidentals excluded. The following year the discharges were 1582, or about one-eleventh of the number of hours and half-hours of the whole time. The fog-gun was found to answer a useful purpose; vessels by the help of it alone having come into the harbor during a fog at night, as well as in the day, that otherwise could not possibly have entered. This signal was continued until it was superseded by a bell-boat. A gun was also used at West Quoddy Head, near the extreme eastern part of Maine. It consisted of a short piece or carronade, 5 feet long, with a bore of 5¼ inches, charged with four pounds of blasting powder. The powder was made up in cartridges and kept in chests in the work-house. The gun was only fired on foggy days, when the steamboat running between Boston and Saint John, New Brunswick, was approaching the light-house from the former place. In going in the other direction the signal was not so much required, because in the former case (of approach) the vessel had been for some time out of sight of land, and consequently its position could not be so well known. The firing was commenced with the hearing of the steamer's whistle as she

was approaching, and as the wind during the fog at this place is generally from the south, the steamer could be heard five or six miles. The firing was continued as frequently as the gun could be loaded until the steamer answered by a signal of three puffs of its whistle. The number of discharges was from one to six, the latter exhausting a keg of powder valued at $8. The keeper of the light-house acted as gunner without compensation other than his salary. The cost of powder was paid by the steamboat company. The report of the gun was heard from two to six miles.

This signal has been abandoned,—because of the danger attending its use—the length of the intervals between the successive explosions—and the brief duration of the sound, which renders it difficult to determine with accuracy its direction.

The lamented General Hartman Bache, of the Light-House Board, adopted a very ingenious plan for an automatic fog-signal which consisted in taking advantage of a conical opening in the coast, generally designated a blow-hole. On the apex of this hole he erected a chimney which terminated in a tube surmounted by a locomotive whistle. By this arrangement a loud sound was produced as often as a wave entered the mouth of the indentation. The penetrating power of the sound from this arrangement would not be great if it depended merely on the hydrostatic pressure of the wave, since this, under favorable circumstances, would not be more than that of a column of water 20 feet high, giving a pressure, of about 10 pounds to the square inch. The effect however of the percussion might add considerably to this, though the latter would be confined in effect to a single instant. In regard to the practical result from this arrangement, which was continued in operation for several years, it was found not to obviate the necessity of producing sounds of greater power. It is however founded on an ingenious idea, and may be susceptible of application in other cases.

Experiments by Professor J. H. Alexander, in 1855.

The Light-House Board was not content with the employ-

ment alone of the fog-signals in ordinary use, but directed a series of experiments in order to improve this branch of its service. For this purpose the board employed Prof. J. H. Alexander, of Baltimore, who made a report on the subject, which was published among the documents. The investigations of Professor Alexander related especially to the use of the locomotive steam-whistle as a fog-signal, and in his report he details the results of a series of experiments in regard to the nature and adjustment of the whistle, the quantity of steam necessary to actuate it, with suggestions as to its general economy and management. He found, what has since been fully shown, that the power of the sound depends upon the pressure of the steam in the boiler, and the pitch upon the distance between the circular orifice through which the steam issues, and the edge of the bell. He appears however to be under an erroneous impression that the sound is produced by the vibrations of the metal of the goblet or bell, while in fact this latter portion of the apparatus is a resounding cavity, which as I have shown in subsequent experiments, may be constructed of wood as well as of brass, in order to produce the same effect. Prof. Alexander also mentions the effect of the wind in diminishing the penetrating power of sound when in an adverse direction, either directly or approximately. He also recommends the adoption of an automatic pump to supply the boilers with water, and also to open and shut the valves at the proper intervals for blowing the whistle. He states that the location of a sound can be determined more precisely in the case of loud, high sounds than in that of feebler or lower ones. I am not prepared to concur with him on this point, in view of experiments of my own. In all cases however loud sounds are more desirable than feebler ones, in order that they may be heard at a greater distance above the noise of the surf and that of the wind as it passes through the spars and rigging of vessels.

The board at this time however was not prepared to adopt these suggestions, and an unsuccessful attempt to use a steam-boiler, rendered abortive by the incapacity of the

keeper to give it proper attendance, discouraged for a time efforts in this line.

Previous to the investigations of Prof. Alexander at the expense of the Light-House Board, Mr. Daboll, of New London had for several years been experimenting on his own account with reference to a fog-signal. His plan consisted in employing a reed trumpet, constructed after the manner of a clarionet, and sounded by means of air condensed in a reservoir, the condensation being produced by horse-power operating through suitable machinery. Although the sound of this was more penetrating than that of bells, still the expense and inconvenience of the maintenance of a horse, together with the cost of machinery, prevented its adoption. Mr. Daboll however after this presented to the board a modification of his invention, in which a hot-air engine of Ericsson's patent was substituted as the motive power, instead of the horse; and the writer of this report, as chairman of the committee on experiments in behalf of the board, examined this invention and reported in favor of its adoption. The other members of the committee made an unfavorable report, on the ground that fog-signals were of little importance, since the mariner should know his place by the character of his soundings in all places where accurate surveys had been made, or should not venture near the coast until the fog was dissipated. The board however established Daboll trumpets at different stations which have been in constant use up to the present time.

PART I.—INVESTIGATIONS FROM 1865 TO 1872.

(Report of the United States Light-House Board for 1874, pp. 87–107.)

Experiments near New Haven, in 1865.

The subject of sound, in connection with fog-signals, still continued to occupy the attention of the board, and a series of investigations was made in October, 1865, at the light-house near New Haven, under the direction of the writer of this report, in connection with Commodore, now Admiral L.

M. Powell, inspector, and Mr. Lederle, acting engineer of the third district.

The principal object was to compare the sound of bells, of steam-whistles, and other instruments, and the effect of reflectors, and also the operation of different hot-air engines. For this purpose the committee was furnished with two small sailing vessels. As these were very imperfectly applicable, since they could not be moved without wind, the writer of the report devised an instrument denominated an "artificial ear," by which the relative penetrating power of different sounding bodies could be determined and expressed in numbers by the removal of the observer to a comparatively short distance from the point of origin of the sound. This instrument consisted of a conical horn made of ordinary tinned sheet iron, the axis of which was about 4 feet in length, the diameter of the larger end 9 inches, and tapering gradually to $1\frac{3}{4}$ of an inch at the smaller end. The axis of this horn was bent at the smaller end in a gentle curve, until the plane of the section of the smaller end was at right angles to the transverse section of the larger end, so that when the axis of the trumpet was held horizontally and the larger section vertically, then the section of the smaller end would be horizontal. Across the smaller end a thin membrane of gold-beater's skin was slightly stretched and secured by a thread. On this membrane fine sand was strewn. To protect the latter from disturbance by the wind it was surrounded by a cylinder of glass cut from a lamp-chimney, the upper end of which was covered with a plate of glass; and in the improved condition of the instrument, with a magnifying lens with which to observe more minutely the motions of the sand. To use this instrument in comparing the relative penetrating power of sound from different sources, as for example from two bells, the axis being held horizontal, the mouth was turned toward one of the bells, and the effect causing agitation of the sand was noted. The instrument was then removed to a station a little farther from the bell, and the effect again noted, the distance being increased step by step until no motion in the sand could be observed

through the lens. This distance being measured in feet or yards gave the number indicating the penetrating power of the instrument under trial. The same experiment was immediately repeated under the same conditions of temperature, air, wind, &c., with the other sounding apparatus, and the relative number of yards indicating the distance taken as the penetrating powers of the two instruments. It should be observed in the use of this instrument, that it is intended merely to concentrate the rays of sound and not to act as a resounding cavity; since in that case the sound—in unison with the resounding note, would produce an effect at a greater distance than one in discord.

The indications of this instrument were compared with the results obtained by the ear in the use of the two vessels, and in all cases were in exact accordance; and it was accordingly used in the following investigations, and has been found of great service in all subsequent experiments on the penetration of sound.

The only precaution in using it is that the membrane shall not be of such tension as to vibrate in unison with a single sound or its octaves; or in other words that the instrument must be so adjusted by varying the length of the axis or the tension of the membrane that it shall be in discordance with the sounds to be measured, and only act as a condenser of the sonorous waves.

The first experiments made were with regard to the influence of reflectors. For this purpose a concave wooden reflector had been prepared, consisting of the segment of a sphere of 16 feet radius and covered with plaster, exposing a surface of 64 square feet. In the focus of this, by means of a temporary railway, a bell or whistle could be readily introduced or withdrawn. The centre of the mouth of the bell was placed in the horizontal axis of the reflector. This arrangement being completed, the sound of the bell with and without the reflector behind it was alternately observed. Within the distance of about 500 yards the effect was evidently increased, as indicated by the motion of the sand on the membrane, but beyond this, the difference was less and

less perceptible, and at the limit of audibility, the addition of the reflector appeared to us entirely imperceptible. This result was corroborated by subsequent experiments in which a whistle was heard nearly as well in the rear of a reflector as before it. It would appear from these results that while feeble sounds at small distances are reflected as rays of light are, waves of powerful sound spread laterally, and even when projected from the mouth of a trumpet tend at a great distance to embrace the whole circle of the horizon.

Upon this and all the subsequent experiments, as it will appear, the principle of reflection as a means of re-inforcing sound is but slightly applicable to fog-signals. It is evident however that the effect will be somewhat increased by augmenting the size of the reflector and by more completely inclosing the source of sound in a conical or pyramidal reflector.

Another series of experiments was made to ascertain whether the penetration of the sound was greater in the direction of the axis of the bells, or at right angles to the axis; or in other words, whether the sound was louder in front of the mouth of a bell or of its rim. The result of this experiment was considered of importance, since in one of the light-houses a bell has been placed with the plane of its mouth at right angles to the horizon, instead of being placed as usual parallel to the same. The effect on the sound in these two positions was similar to that produced by the bell with a reflector, the noise at a short distance being greater with the mouth toward the observer than when the rim was in the plane of the ear. At a considerable distance however, the difference between the two sounds was imperceptible. In practice therefore it is of very little importance whether the axis of the bell is perpendicular or parallel to the horizon.

The first fog-signal examined in this series of experiments was a double whistle, improperly called a steam-gong, designed principally for a fire-alarm and for signals for the commencement of working-hours in large manufacturing establishments. It consisted of two bells of the ordinary steam-whistle on the same hollow axis, mouth to mouth,

with a flat, hollow cylinder between them, through the upper and lower surfaces of which the circular sheets of steam issue, the vibration of which produces the sound. In the instrument under examination, the upper bell was 20 inches in length of axis, and 12 inches in diameter, and the lower whistle was of the same diameter, with a length of axis of 14 inches. The note of the shorter bell was a fifth above that of the longer. This arrangement gave a melodious sound, unlike that of the ordinary locomotive-whistle, and on that account had a peculiar merit. The sound was also very loud, and according to testimony—had been heard under favorable circumstances more than twenty miles. It required a large quantity of steam however, to give it its full effect, and the only means to obtain an approximate idea as to this quantity was that afforded by observing its action on a boiler of a woolen manufactory near Newport. It was here blown with a pressure of at least 75 pounds. From theoretical considerations however, it might be inferred that its maximum penetrating power would not be greater than that of a single whistle using the same amount of steam, and this theoretical inference was borne out by the subsequent experiments of General James C. Duane. But from the strikingly distinctive character of its tone it has in our opinion an advantage over a single whistle expending an equal quantity of steam.

The fact that the vibration of the metal of the bell had no practical effect on the penetrating power of the sound was proved quite conclusively by winding tightly around each bell, over its whole length, a thick cord, which would effectually stop all vibration. The penetration of the sound produced under this condition was the same as that with the bells free. It is true, the latter produces a difference in the quality of the tone, such as that which is observed in a brass instrument and that of one of wood or ivory. The inventor was not aware that the sound produced was from the resonance of the air within the bell, and not from the metal of the bell itself, and had obtained a patent, not only for the invention of the double whistle, but also for the special compound of metal of which it was composed.

Another apparatus proposed to be used as a fog-signal was presented for examination by the Marine Signal Company, of Wallingford, Conn. It consisted of a curved tube of copper nearly in the form of the letter C, and was supported on an axis passing through the centre of the figure. An ordinary bell-whistle was attached to each extremity of the tube, the instrument being placed in a vertical position and partly filled with water, then made to oscillate on its centre of support. By this means the air was drawn in at one end and forced out through the whistle at the other. The motion being reversed the air was drawn in at the end through which it had just made its exit and forced out through the whistle at the other. By rocking the instrument, either by hand or by the motion of the vessel, a continued sound could be produced. The motive power in the former case was muscular energy, and the experiments which were made at this time, as well as all that have been made subsequently, conclusively prove that the penetrating power of the sound for practical use as a fog-signal depends upon the intensity of the motive energy employed. No instrument operated through levers and pumps by hand-power is sufficient for the purpose.

One of these instruments with two 4-inch whistles gave a sound, (as indicated by the artificial ear,) the power of which was about one-tenth of that of a steam-trumpet. It was supposed however that this instrument would be applicable for light-ships; and that if extended entirely across the vessel and armed with whistles of large size, it would be operated by the rolling of the vessel, and thus serve to give warning in time of thick weather. But as it frequently happens that fog exists during a calm, this invention could not be relied upon to give warning in all cases of danger. Besides this, the ordinary roll of a ship is not sufficient to produce a hydrostatic pressure of more than five or six pounds to the square inch, which is insufficient to give an effective sound. It has however been proposed to increase the power by using quicksilver instead of water; but besides the first cost of this material, and the constant loss by leakage and oxidation, the tendency to affect the health of the crew is an objection

to the introduction of this modification of the apparatus into light-ships.

The other instruments which were subjected to trial were an ordinary steam-whistle and a Daboll trumpet. The bell of the whistle was 6 inches in diameter, 9 inches in height, and received the sheet of steam through an opening of one-thirtieth of an inch in width; was worked by a pressure of condensed air of from 20 to 35 pounds per square inch, and blown once in a minute for about five seconds. The air was condensed by a Roper engine of one-horse power. The penetrating power of the sound was increased by an increase in the pressure of the air, and also the pitch. The tone however of the instrument was lowered by increasing the distance between the orifice through which the circular sheet of air issued at the lower rim of the bell or resounding cavity. To prove conclusively that the bell performs the part of a mere resounding cavity, a wooden one—on a subsequent occasion, was substituted for that of metal without a change in the loudness or the pitch of the sound.

The penetrating power of the whistle was compared with a Daboll trumpet, actuated by an Ericsson engine of about the same power; the reservoir for the condensed air of each machine was furnished with a pressure-guage, and by knowing the capacity of the condensing pumps and the number of strokes required to produce the pressure, the relative amount of power was determined. The result was that the penetrating power of the trumpet was nearly double that of the whistle, and that an equal effect was produced at the same distance by about one-fourth of the power expended in the case of the latter. It must be recollected though that the whistle sends sonorous waves of equal intensity in every direction, while the greatest power of the trumpet is in the direction of its axis. This difference however is lessened on account of the spreading of the sound to which we have before alluded.* The whistle was blown, as we have said, with

*It is worthy of note however that in the case of a sound having primarily an axial direction, the subsequent lateral diffusion must result in enfeebling the whole sphere of expanding sound-waves in a more rapid ratio than the square of the distance.

a pressure of from 20 to 35 pounds, while the trumpet was sounded with a pressure of from 12 to 15 pounds. In the case of the whistle, the pressure in the reservoir may be indefinitely increased with an increase of the penetrating power of the sound produced, while in the case of the trumpet a pressure greater than a given amount entirely stops the blast by preventing the recoil of the vibrating tongue; this being made of steel, in the larger instruments $2\frac{1}{2}$ inches wide and 8 inches long, would receive a pressure of steam, at only 10 pounds to the square inch, of 200 pounds, tending to press it into the opening and to prevent its recoil, this circumstance limits the power of a trumpet of given dimensions. It is well fitted however to operate with a hot-air engine, and is the least expensive in fuel of any of the instruments now employed. The whistle is the simpler and easier of management, although they both require arrangement of machinery in order that they may be operated automatically.

It is a matter of much importance to obtain a hot-air engine of sufficient power, and suitable for working fog-signals of all classes. This will be evident when we consider the difficulty in many cases of obtaining fresh water for producing steam, and the expense of the renewal of the boilers in the use of salt-water, as well as that of the loss of power in frequently blowing out the latter, in addition to the danger of the use of steam by unskillful attendants.

The merits of the two engines however under consideration could not be fully tested by the short trial to which they were subjected during these experiments. The principal objection to the Ericsson engine was the size of the fly-wheel and the weight of the several parts of the machine; the Roper engine was much more compact, and appeared to work with more facility, but from the greater heat imparted to the air the packing was liable to burn out and required to be frequently renewed. Although at first the impression of the committee was in favor of the Roper engine, yet in subsequent trials of actual practice it was found too difficult to be kept in order to be employed for light-house purposes,

and its use has consequently been abandoned; another hot-air engine has been employed by the board, the invention of a Mr. Wilcox, which has also been discontinued for a similar reason. I was assured by the person last named, a very ingenious mechanician, that when the several patents for hot-air engines expired, a much more efficient instrument could be devised by combining the best features of each of those now in use.

For determining the relative penetrating power of these instruments, the use of two vessels had been obtained, with the idea of observing the sound simultaneously in opposite directions.

Unfortunately however, the location which had been chosen for these experiments was of a very unfavorable character in regard to the employment of sailing-vessels and the use of the artificial ear. It was fully open to the ocean only in a southerly direction, navigation up the bay to the north being limited to three and a half miles, while on shore a sufficient unobstructed space could not be obtained for the proper use of the artificial ear. With these obstructions and the necessity of beating against the wind, thereby constantly altering the direction of the vessel, exact comparisons were not possible, yet the observations made were sufficiently definite to warrant certain conclusions from them as to the relative power of the various instruments submitted to examination.

The following is a synopsis of the observations on four different days. Before giving these, it is necessary to observe that at each stroke of the piston of the hot-air engine a loud sound was produced by the blowing off of the hot air from the cylinder, after it has done its work. In the following statement of results the noise thus produced is called the exhaust. On the first day, but one set of observations was made, the vessel's course being nearly in the line of the axis of the trumpet. The order of penetrating power was as follows: 1st, trumpet; 2nd, exhaust; 3d, bell; these instruments being heard respectively at $5\frac{1}{8}$, $3\frac{1}{2}$, and 2 miles. The whistle was not sounded.

On the second day, simultaneous observations were made

from two vessels sailing in nearly opposite directions. The results of the observations made on the vessel sailing in a southerly direction were very irregular. The trumpet was heard at $3\frac{5}{8}$ miles and lost at $4\frac{3}{8}$ miles with the wind slightly in favor of the sound, and heard at $6\frac{1}{4}$ miles with the wind somewhat against the sound; it was heard even at $7\frac{5}{8}$ miles from the masthead, though inaudible from the deck. In all these cases the position of the vessel was nearly in line with the axis of the trumpet.

The whistle and exhaust were heard at $7\frac{1}{4}$ miles with a feeble opposing wind, and lost at $6\frac{1}{4}$ miles when the force of the wind became greater.

The order of penetration in this series of observations was: 1st, trumpet and gong; 2nd, whistle; 3d, exhaust.

In the case of a vessel sailing northward, its course being almost directly against the wind and in the rear of the trumpet, all the sounds were lost at less distances than in the case of the other vessel. The observations showed very clearly the effect of the wind, the bell at a certain distance being heard indistinctly with a strong opposing wind and more and more plainly as the wind died away. The trumpet was heard only as far as the whistle, the vessel being in the rear of it.

On the third day, observations were made from the two vessels, both however sailing to the south. From the vessel sailing at right angles to the direction of the wind the order of penetration was: 1st, trumpet; 2nd, whistle; 3d, exhaust; 4th, bell.

In the case of the other vessel the opposing effect of the wind was greater, and the sounds were heard to a less distance; the order was: 1st, trumpet; 2nd, whistle; 3d, exhaust; 4th, bell; 5th, rocker.

On the fourth day, two trips were made by the same vessel in the course of the day, one being northward and the other southward. In the first case the trumpet was lost at $3\frac{1}{8}$ miles, the vessel being nearly in its rear; in the second case, the wind being almost directly opposed to the sound, the large bell was heard at $1\frac{1}{8}$ miles, and lost at $\frac{7}{8}$ of a mile,

which was probably due to increase of the force of the wind; the trumpet was lost at 3½ miles.

In all these observations, owing to the unfavorable conditions of the locality, and the direction of the wind, we were unable to obtain any satisfactory observations on sound moving with the wind. In all cases the results were obtained from sounds moving nearly against the wind, or at right angles to it. From the results of the whole it appears that the sound was heard farther with a light opposing wind than with a stronger one, and that it was heard farthest of all at right angles to the wind. From this latter fact however it should not be inferred that in this case sound could be heard farther at right angles to the wind than with the wind, but that in this direction the effect of the wind was neutralized. The results also exhibited, in a striking manner, the divergency of sound from the axis of the trumpet, the trumpet being heard in the line of its axis in front at 6 miles and behind at 3 miles, the wind being nearly the same in both cases.

All the observations were repeated on land with the artificial ear as far as the unfavorable condition of the surface would permit. Although the limit, as to distance, at which the sand might be moved was not in most cases observed, yet the relative degree of agitation at a given distance established clearly which was the most powerful instrument, the result giving precisely the same order of penetration of the different instruments as determined by direct audition.

During this series of investigations an interesting fact was discovered, namely, a sound moving against the wind, inaudible to the ear on the deck of the schooner, was heard by ascending to the mast-head. This remarkable fact at first suggested the idea that sound was more readily conveyed by the upper current of air than the lower, and this appeared to be in accordance with the following statement of Captain Keeney, who is commander of one of the light-house vessels, and has been for a long time engaged on the banks of Newfoundland in the occupation of fishing: "When the fishermen in the morning hear the sound of the surf to the leeward,

or from a point toward which the wind is blowing, they take this as an infallible indication that in the course of from one to five hours the wind will change to the opposite direction from which it is blowing at the time." The same statement was made to me by the intelligent keeper of the fog-signal at Block Island. In these cases it would appear that the wind had already changed direction above, and was thus transmitting the sound more in its own direction than that of the wind at the surface of the earth.

Another remarkable fact bearing on this same point is established by the observations of General James C. Duane. At Cape Elizabeth, 9 miles south-easterly from the general's house, at Portland, is a fog-signal consisting of a whistle 10 inches in diameter; at Portland Head, about 4 miles from the same city, in nearly the same direction, is a Daboll trumpet. There can be no doubt, says the general, that those signals can be heard much better during a heavy north-east snow-storm than at any other time. "As the wind increases in force, the sound of the nearer instrument, the trumpet, diminishes, but the whistle becomes more distinct; but I have never known the wind to blow hard enough to prevent the sound of the latter from reaching this city." In this case the sound comes to the city in nearly direct opposition to the course of the wind, and the explanation which suggested itself to me was that during the continuance of the storm, while the wind was blowing from the northeast at the surface, there was a current of equal or greater intensity blowing in an opposite direction above, by which the sound was carried in direct opposition to the direction of the surface current. The existence of such an upper current is in accordance with the hypothesis of the character of a north-east storm, which sometimes rages for several days at a given point on the coast without being felt more than a few miles in the interior, the air continuously flowing in below and going out above. Indeed, in such cases a break in the lower clouds reveals the fact of the existence above of a rapid current in the opposite direction.

The full significance however of this idea did not reveal

itself to me until in searching the bibliography of sound I found an account of the hypothesis of Professor George G. Stokes, in the proceedings of the British Association for 1856,* in which the effect of an upper current in deflecting the wave of sound so as to throw it down upon the ear of the auditor, or directing it upward far above his head, is fully explained. This subject will be referred to in the subsequent parts of the report, in the attempt to explain various abnormal phenomena of sound that have been observed during the series of investigations connected with the Light-House Board.

During these investigations an attempt was made to ascertain the velocity of the wind in an upper stratum as compared with that in the lower. The only important result however was the fact that the velocity of the shadow of a cloud passing over the ground was much greater than that of the air at the surface, the velocity of the latter being determined approximately by running a given distance with such speed that a small flag was at rest along the side of its pole. While this velocity was not perhaps greater than six miles per hour, that of the shadow of the cloud was apparently equal to that of a horse at full speed.

During this and subsequent investigations, inquiries were made in regard to the effect of fog upon sound, it being a subject of considerable importance to ascertain whether waves of sound like the rays of light are absorbed or stifled by fog. On this point however observers disagree. From the very striking analogy which exists in many respects between sound and light, the opinion largely prevails that sound is impeded by fog, although observers who have not been influenced by this analogy have in many instances adopted the opposite opinion that sound is better heard during a fog than in clear weather. For instance, the Rev. Peter Ferguson, of Massachusetts, informs me that from his own observations sound is conveyed farther in a fog than in a clear air. He founds this opinion on observations which he has made on the sound of locomotives of

* Report of British Association, 1856; Abstracts, p. 22.

several railways in passing over bridges at a distance. Unfortunately, the question is a difficult one to settle, since in order to arrive at a true result, the effect of the wind must be carefully eliminated. Captain Keeney, who has previously been mentioned, related the following occurrence, in the first part of which he was led to suppose that fog had a very marked influence in deadening sound, though in a subsequent part he came to an opposite conclusion: He was sailing during a dense fog with a slight wind bearing him toward a light-vessel, the locality of which he expected to find by means of the fog-signal. He kept on his course until he thought himself very near the ship, without hearing the stroke of the bell. He then anchored for the night, and found himself next morning within a short distance of the light-vessel, but still heard no sound, although he was assured when he got to it that the bell had been ringing all night. He then passed on in the same direction in which he had previously sailed, leaving the light-vessel behind, and constantly heard the bell for a distance of several miles, the density of the fog not perceptibly diminishing. In this case it is evident that the deadening of the sound was not due to the fog, but (as we shall hereafter see,) in all probability to the combined action of the upper and the lower currents of air.

On returning to Washington the writer took advantage of the occurence of a fog to make an experiment as to the penetration of the sound of a small bell rung by clock-work, the apparatus being the part of a moderator-lamp intended to give warning to the keepers when the supply of oil ceased. The result of the experiment was contrary to the supposition of absorption of the sound by the fog, but the change in the condition of the atmosphere as to temperature and the motion of the air before the experiment could be repeated in clear weather rendered the result not entirely satisfactory.

Experiments at Sandy Hook in 1867.

The next series of experiments was made from October 10 to October 18, 1867, under the direction of the writer of this

report, in connection with General O. M. Poe, engineer-secretary of the Light-House Board, Commodore (now Admiral) Augustus L. Case, then inspector of the third light-house district, and Mr. Lederle, acting engineer of the same district.

The principal object of these investigations was to compare different instruments and to ascertain the improvements which had been made in them since the date of the last investigations, especially the examination of a new fog-signal called the siren, and the comparison of it with the Daboll trumpet, although other investigations were made relative to the general subject of sound in relation to fog-signals. The locality chosen was Sandy Hook, a narrow peninsula projecting northward about five miles into the middle of the Lower Bay of New York, (and almost at right angles to its coast,) having a width of about half a mile. Near the northern point on the east shore a temporary building was erected for the shelter of the engines and other instruments.

The comparisons in regard to penetrating power were made by the use of the artificial ear heretofore described, by carrying this off a measured distance until the sand ceased to move. This operation was much facilitated by previous surveys by members of the engineer corps, who had staked off a straight line parallel with the shore, and accurately divided it into equal distances of 100 feet.

On account of the character of the deep and loose sand, walking along this distance was exceedingly difficult, and to obviate this, a carriage with broad wheels drawn by two horses was employed. An awning over this vehicle protected the observer from the sun, and enabled him without fatigue and at his ease to note the agitations of sand on the drum of the artificial ear, the mouth of which was directed from the rear of the carriage toward the sounding instrument.

For these and other facilities we were indebted to General Andrew A. Humphreys, chief of the Engineer Bureau, who gave orders to the officer in charge of the military works at Sandy Hook to afford us every aid in his power in carrying on the investigation.

The instruments employed were—

1st. A first-class Daboll trumpet (the patent for which, since the death of Mr. Daboll, is owned by Mr. James A. Robinson,) operated by an Ericsson hot-air engine. It carried a steel reed 10 inches long, $2\frac{3}{4}$ inches wide, and $\frac{1}{2}$ inch in thickness at the vibrating end, but increasing gradually to an inch at the larger extremity. This was attached to a large vertical trumpet curved at the upper end into a horizontal direction and furnished with an automatic arrangement for producing an oscillation of the instrument of about 60° in the arc of the horizon. Its entire length, including the curvature, was 17 feet. It was $3\frac{1}{2}$ inches at the smaller end and had a flaring mouth 38 inches in diameter. The engine had a cylinder 32 inches in diameter, with an air-chamber of $4\frac{1}{2}$ feet in diameter and 6 feet long, and was able to furnish continually a five-second blast every minute at a pressure of from 15 to 30 pounds.

2d. A siren, originally invented by Cagniard de Latour, and well known to the physicist as a means of comparing sounds, and measuring the number of vibrations in different musical notes. Under the direction of the Light-House Board, Mr. Brown, of New York, had made a series of experiments on this instrument in reference to its adoption as a fog-signal, and these experiments have been eminently successful. The instrument as it now exists differs in two essential particulars from the original invention of Latour: 1st, it is connected with a trumpet in which it supplies the place of the reed in producing the agitation of the air necessary to the generation of the sound; and 2d, the revolving disk, which opens and shuts the orifices producing the blasts, is driven not by the blast itself impinging on oblique openings, as in the original instrument, but by a small engine connected with the feed-pump of the boiler.

The general character of the instrument may be understood from the following description: Suppose a drum of short axis, into one head of which is inserted a steam-pipe connected with a locomotive boiler, while the other end has in it a triangular orifice through which the steam is at

brief intervals allowed to escape. Immediately before this head, and in close contact with it, is a revolving disk in which are eight orifices. By this arrangement, at every complete revolution of the disk, the orifice in the head of the drum is opened and shut eight times in succession, thus producing a rapid series of impulses of steam against the air into the smaller orifice of the trumpet placed immediately in front of the revolving disk. These impulses are of such energy and rapidity as to produce a sound unrivalled in intensity and penetrating power by that of any other instrument yet devised.

The siren was operated by an upright cylindrical tubular boiler, with a pressure of from 50 to 100 pounds on the square inch. This form of boiler was subsequently replaced by an ordinary horizontal locomotive-boiler with a small engine attached for feeding it, and for rotating the disk, the latter being effected by means of a band passing over pulleys of suitable relative dimensions.

3d. A steam-whistle 8 inches in diameter. Through some mis-understanding a series of whistles of different diameters was not furnished as was intended.

The first experiments to be noted were those in regard to a comparison of the penetrating power of the siren and the whistle; the fitting up of the Daboll trumpet not having been completed. The principal object of this however, was to test again the truthfulness of the indications of the artificial ear in comparison with those of the natural ear.

An experiment was made both by means of the artificial ear on land, and by actually going off on the ocean in a steamer until the sounds became inaudible to the natural ear. By the latter method the two sounds ceased to be heard at the distances of six and twelve and a half miles, respectively. The indications of the artificial ear gave a similar result, the distance at which the sand ceased to move in one case being double that of the other. In both cases the conditions of wind and weather were apparently the same. In the case of the steamer the distance was estimated by noting the interval of time between the flash of steam and the perception of the sound.

Comparison of the Daboll trumpet and the siren.—The pressure of the hot air in the reservoir of the hot-air engine of the trumpet was about 20 pounds, and that of the steam in the boiler of the siren about 75 pounds. These pressures are however not considered of importance in these experiments, since the object was not so much to determine the relative amount of motive power employed, as the amount of penetrating energy produced by these two instruments, each being one of the first of its class.

1. At distance 50, the trumpet produced a decided motion of the sand, while the siren gave a similar result at distance 58. The two observations being made within ten minutes of each other, it may be assumed that the condition of the wind was the same in the two cases, and hence the numbers above given may be taken as the relative penetrating powers of the two instruments.

2. Another series of experiments was instituted to determine whether a high or a low note gave the greater penetration. For this purpose the siren was sounded with different velocities of rotation of the perforated disk, the pressure of steam remaining at 90 pounds per square inch. The effect upon the artificial ear in causing greater or less agitation of sand was taken as the indication of the penetrating power of the different tones. The number of revolutions of the disk in a given time was determined by a counting apparatus, consisting of a train of wheels and a series of dials showing tens, hundreds, and thousands of revolutions; this was temporarily attached to the projecting end of the spindle of the revolving disk by pushing the projecting axis of the instrument into a hole in the end of the spindle.

From the whole of this series of experiments it appeared that a revolution which gave 400 impulses in a second was the best with the siren when furnished with a trumpet. On reflection however it was concluded that this result might not be entirely due to the pitch, but in part to the perfect unison of that number of impulses of the siren with the natural tone of the trumpet. To obviate this complication a series of experiments was next day made on the penetra-

tion of different pitches with the siren alone, the trumpet being removed. The result was as follows:

The siren was sounded at five different pitches, the artificial ear being at such a distance as to be near the limit of disturbance by the sound. In this condition the lowest pitch gave no motion of sand. A little higher, slight motion of sand. Still higher, considerable motion of sand; and with a higher pitch again, no motion of sand. The best result obtained was with a revolution which gave 360 impulses in a second.

3. An attempt was made to determine the most effective pitch or tone of the steam whistle. It was started with what appeared to be the fundamental note of the bell, which gave slight motion of sand; a higher tone a better motion; still higher, sand briskly agitated; next, several tones lower, no motion; higher, no motion; still higher, no motion. The variation in the tone was made by altering the distance between the bell and the orifice through which the steam was ejected.

The result of this experiment indicated nothing of a definite character other than that with a given pressure there is a maximum effect produced when the vibrations of the sheet of air issuing from the circular orifice are in unison with the natural vibrations from the cavity of the bell, a condition which can be determined in any case only by actual experiment. In practice, Mr. Brown was enabled to produce the best effect by regulating the velocity until the trumpet gave the greatest penetrating power, as indicated by an artificial ear of little sensibility; which latter was adopted in order that it might be employed for determining the relative power while the observer was but a few yards from the machine. These experiments have been made in an apartment of less than 80 feet in length, in which the sounding apparatus was placed at one end, and the artificial ear at the other, substituting fine shot in the place of sand.

The experiments with the siren however, indicate the fact that neither the highest nor the lowest pitch of an instrument gives the greatest penetrating power, but one of a medium character.

Another element of importance in the construction of these instruments is the volume of sound. To illustrate this it may be mentioned that a harpsichord-wire stretched between two strings of India rubber when made to vibrate by means of a fiddle-bow, gives scarcely any appreciable sound. We attribute this to the want of quantity in the aerial wave; for if the same wire be stretched over a sounding-board having a wide area the effect will be a comparatively loud sound but of less duration with a given impulse. It was therefore suggested that the width of the reed in the Daboll trumpet, the form and size of the hole in the disk of the siren, and the circumference of the vibrating sheet of air issuing from the circular orifice of the whistle, would affect the power of the sound. The only means of testing this suggestion is by using reeds of different widths, sirens with disks of different-shaped openings, and whistles of different diameters. In conformity with this view Mr. Brown has made a series of empirical experiments with openings of different forms, which have greatly improved the operation of the siren, while Mr. Wilcox has experimented on several forms of reeds, of which the following is the result:

The best reed obtained was $2\frac{1}{4}$ inches wide, 8 inches long in the vibrating part, $\frac{5}{8}$ inch thick at the butt, and $\frac{1}{4}$ inch thick at the free end. This sounded at a pressure of from 20 to 30 pounds. The thinner reeds gave a sound at a less pressure, from 5 to 10 pounds, the thicker at from 20 to 30 pounds. A reed $8\frac{1}{2}$ inches long in the vibrating part, 1 inch thick at the butt, $\frac{3}{4}$ inch thick at the end, and 3 inches wide, did not begin to sound until a pressure of 80 pounds was reached, when it gave a sound of a dull character. Another reed of the same width, $\frac{5}{8}$ inch thick at the butt, and $\frac{7}{16}$ inch at the end, and same length, gave a sound at 75 pounds pressure, but still dull and of little penetrating power. These reeds were evidently too heavy in proportion to their elasticity. These were made without the addition of a trumpet, and therefore to produce the best result when used with a trumpet the latter must be increased or diminished in length until its natural vibrations are in harmony with those of the former, as will be

seen hereafter. General James C. Duane has also made experiments on whistles of different diameters, of which the result will be given.

Another consideration in regard to the same matter is that of the amplitude of the oscillations of the tongue or steel reed in its excursion in producing the sound; the time of oscillation (that is the pitch) remaining the same, the amplitude will depend upon the elasticity of the reed, the power to surmount which will again depend upon the pressure of steam in the boiler, and hence we might infer that an increase of pressure in the boiler with an increase of the elasticity of the reed, everything else being the same, would produce an increase in penetrating power. From the general analogy of mechanical effects produced by motive power we may denote the effect upon the ear by the expression mv^2, in which m expresses the mass or quantity of air in motion, and v the velocity of the particles in vibration.

If this be the expression for the effect upon the ear, it is evident that in case of a very high note the amplitude of the vibration must be so small that the effect would approximate that of a continued pressure rather than that of distinct alternations of pressure, giving a vibrating motion to the drum of the ear.

4. Experiments were next made to determine the penetrating power in the case of the siren under different pressures of steam in the boiler. The experiments commenced with a pressure of 100 pounds. The pressure at each blast was noted by two observers, and to compare these pressures with the indications of the sand, the time of the blasts was also noted.

The following are the results:

Pressure.	Relative distances at which the sand ceased to move.
100	61
90	59
80	58
70	57
60	57
50	56
40	55
30	53
20	51

From this series of experiments it appears that a diminution of pressure is attended with a comparatively small diminution in the penetrating power of the siren.

In regard to this unexpected result,—of great practical importance, the following appears to be the explanation. It is a well-known principle in aerial mechanics that the velocity of the efflux of air from an orifice in a reservoir does not increase with an increase of condensation, when the spouting is into a vacuum. This is evident when we reflect that the weight or density of the air moving out is increased in proportion to the elasticity or pressure; that is, the increase in the propelling force is proportional to the increase in the weight to be moved, hence the velocity must remain the same.

In the foregoing experiments with high pressures—large in proportion to the resistance of the air, the velocity of efflux should therefore be but little increased with the increase of pressure, and inasmuch as the velocity is the most important factor in the expression mv^2, which indicates the effect on the tympanum, the penetrating power of the sound should be in accordance with the above experimental results.

A similar result cannot be expected with the use of the whistle, or the trumpet, since in the former, the stiffness of the aerial reed depends upon its density, which will be in proportion to the pressure in the boiler; and in the case of the latter—on the one hand, no sound can be produced unless the pressure be sufficient to overcome the resistance of the reed, and on the other, the sound must cease when the pressure is so great as to prevent the recoil of the reed.

5. An experiment was made to determine the effect of a small whistle inserted into the side of a trumpet near the small end. The whistle being sounded before and after it was placed in the trumpet, the result was as follows: The penetrating powers were in the ratio of 40:51, while the tone was considerably modified. From this experiment it appears that a whistle may be used to actuate a trumpet or to exercise the functions of a reed. In order however to get the best results, it would be necessary that the trumpet

and whistle should be in unison, but it may be doubted whether an increase of effect, with a given amount of power, would result from using such an arrangement; it might nevertheless be of advantage in certain cases to direct the sound of a locomotive whistle in a definite direction, and to use a smaller whistle, especially in cities in which the locomotive passes through long streets; perhaps in this case the sound might be less disagreeable than that of the naked whistle, which sends its sound-waves laterally with as much force as in the direction of the motion of the engine.

6. General Poe called attention to the sound produced by the paddle-wheels of a steamer in the offing—at a distance estimated at four and a half miles. The sound was quite distinct when the ears were brought near the surface of the beach.

In this connection he stated that he had heard the approach of a small steamer on the northern lakes when its hull was still below the horizon, and was even enabled to designate the particular vessel from among others by the peculiarity of the sound.

The sound in the case of the steamer is made at the surface of the water, and it might be worth the trouble to try experiments as to the transmission of sound under this condition, and the collection of it by means of ear-trumpets, the mouths of which are near the water, the sound being conveyed through tubes to the ears of the pilot. In order however to determine in this case the direction of the source of sound, two trumpets would be necessary, one connected with each ear, since we judge of the direction of a sound by its simultaneous effects on the two auditory nerves. This suggestion, as well as many others which have occurred in the course of these researches, is worthy of special investigation.

7. A series of experiments was made to compare trumpets of different materials and forms, having the same length and transverse areas, all blown at a pressure of 9½ pounds.

The following table gives the results:

No.	Material of trumpet.	Cross-section.	Relative distances at which the sand ceased to move.
1	Wood.	Square.	13
2	Brass.	Circular.	23
3	Cast iron.	Circular.	24
4	Wood.	Circular.	30

From these experiments it would appear that the material or elasticity of the trumpet had little or no effect on the penetrating power of the sound, although the shape appeared to have some effect, the pyramidal trumpet, or one with square cross-section (No. 1) giving a less result than the conical ones of the same sectional area. A comparison was made between a long straight trumpet and one of the same length curved at its upper end, which gave the same penetrating power with the same pressure. It is probable that a thin metallic trumpet would give greater lateral divergency to the sound, and also a slightly different tone.

8. The effect of a hopper-formed reflector was next tried with the whistle, the axis of which was about 5 feet in length, the mouth 6 feet square, and the small end about 18 inches. When the whistle was sounded at the small end of this reflector, the distance at which the sand ceased to move was 51; the sound of the same whistle without the reflector ceased to move the sand at 40. The ratio of these distances would have been less with a more sensitive instrument at a greater distance on account of the divergency of the rays.

9. In order to determine the diminution of sound by departing from the axis of the trumpet, a series of experiments was made with a rotating trumpet, the axis of which was at first directed along the graduated line of observation, and subsequently deflected from that line a given number of degrees. The following were the results:

Direction of the trumpet.	Relative distances at which the sand ceased to move.
Along the line	26
Deflected 30°	23
Deflected 60°	21
Deflected 90°	18
Deflected 120°	13

These results illustrate very strikingly the tendency of sound to spread on either side of the axis of the trumpet; had the experiments been made with a more sensitive instrument and at a greater distance the effect would have shown a much greater divergency of the sound. It should be observed however that the mouth of the trumpet in this case was 36 inches, which is unusually large.

From the experiments made near New Haven, and also from those at this station, it appears that the actual amount of power to produce sound of a given penetration is absolutely less with a reed trumpet than with a locomotive whistle. This fact probably finds its explanation in the circumstance that in each of these instruments the loudness of the sound is due to the vibration of the air in the interior of the trumpet and in the bell of the whistle, each of these being a resounding cavity; and furthermore that in these cavities the air is put in a state of sustained vibration by the undulations of a tongue, in the one case of metal, in the other of air; and furthermore it requires much more steam to set the air in motion by the tongue of air than by the solid tongue of steel, the former requiring a considerable portion of the motive power to give to the current of which it consists, the proper degree of stiffness (if I may use the word,) to produce the necessary rapidity of oscillation. But whatever may be said in regard to this supposition, it is evident in case reliable hot-air engines cannot be obtained, that the Daboll trumpet may be operated by a steam-engine, although at an increased cost of maintenance, but this increase we think will still not be in proportion to the sound obtained in comparison with the whistle.

Another question which naturally arises, but which has not yet been definitely settled by experiment, is whether both the siren and the whistle would not—equally with the trumpet, give more efficient results when worked by condensed air than by steam.

From hypothetical considerations this would appear to be the case, since the intensity of sound depends upon the density of the medium in which it is produced; and as the steam is considerably lighter than air, and as the cavities of all of these instruments are largely filled with steam, the intensity of sound would on this account seem to be less than if filled with air.

At the conclusion of the experiments at Sandy Hook, the siren was adopted as a fog-signal, in addition to the reed-trumpet and the locomotive-whistle, to be applied to the more important stations; while large bells were retained for points at which fog-signals were required to be heard at but comparatively short distances. These instruments of the first class being adopted, it became of importance to determine—in actual practice, the cost of maintenance, the best method of working them, and any other facts which might have a bearing on their use.

But as investigations of this kind would require much time and peculiar advantages as to location and mechanical appliances, this matter was referred to General J. C. Duane, the engineer in charge of the 1st and 2d light-house districts, who had peculiar facilities near his residence, at Portland, Me.,—in the way of workshops and other conveniences, and who from his established reputation for ingenuity and practical skill in mechanism, was well qualified for the work. The assignment of this duty to General Duane by the Light-House Board was made during my absence in Europe, in 1870, and as my vacation in 1871 was devoted to light-house duty in California, I had no opportunity of conferring with him on the subject until after his experiments were completed. His results are therefore entirely independent of those obtained under my direction, and I give them herewith in his own words, with such comments as they may suggest, and as are necessary to a proper elucidation of the subject.

Experiments at Portland, Me., 1871, *by General James C. Duane.*

The apparatus employed consisted of the first-class siren, a first-class Daboll trumpet, and steam-whistles of various sizes.

The points to be decided were:

1st. The relative power of these machines; *i. e.*, the distances at which they could be heard under various conditions of the atmosphere:

2d. The amount of fuel and water consumed by each:

3d. The attention and skill required in operating them:

4th. Their endurance:

5th. Whether they are sufficiently simple in construction to permit of their being managed and kept in running order by the class of men usually appointed light-house keepers.

In conducting these experiments the following method was pursued:

The signals were sounded at alternate minutes, and their sound compared at distances of two, three, and four miles, and from different directions. On every occasion the quantity of fuel and water consumed per hour by each—was carefully noted, and the condition of each machine examined, both before and after the trial, to ascertain whether any of its parts had sustained injury.

Before giving the results of these experiments some facts should be stated, which will explain the difficulty of determining the power of a fog-signal.

There are six steam fog-whistles on the coast of Maine; these have been frequently heard at a distance of twenty miles, and as frequently cannot be heard at the distance of two miles, and this with no perceptible difference in the state of the atmosphere.

The signal is often heard at a great distance in one direction, while in another it will be scarcely audible at the distance of a mile. This is not the effect of wind, as the signal is frequently heard much farther against the wind than with it. For example, the whistle on Cape Elizabeth can always be distinctly heard in Portland,—a distance of nine miles, during a heavy north-east snow-storm, the wind blowing a gale directly from Portland toward the whistle.

In this sentence, General Duane certainly does not intend to convey the idea that a signal is frequently heard "at a much greater distance against the wind than with it," since this assertion would be at variance with the general experience of mankind; but the word "frequently" applies to the whistle on Cape Elizabeth, which has been already mentioned as a remarkably exceptional case, in which the sound is heard best against the wind during a north-east snow-storm.

The most perplexing difficulty however arises from the fact that the signal often appears to be surrounded by a belt, varying in radius from one to one and a half miles, from which the sound appears to be entirely absent. Thus in moving directly from a station, the sound is audible for the distance of a mile, is then lost for about the same distance, after which it is again distinctly heard for a long time. This action is common to all ear-signals, and has been at times observed at all the stations,—at one of which the signal is situated on a bare rock twenty miles from the main-land, with no surrounding objects to affect the sound.

All attempts to re-enforce the sound by means of reflectors have hitherto been unsuccessful. Upon a large scale, sound on striking a surface does not

appear to be reflected after the manner of light and heat, but to roll along it like a cloud of smoke.

This statement is in a measure in accordance with results which I have previously found in connection with investigations at the light-house near New Haven, in which the conclusion was arrived at, that although rays of feeble sound for a short distance observe the law that the angle of reflection is equal to the angle of incidence—after the manner of light, yet those of powerful sound tend to diverge laterally to such a degree as to render reflectors of comparatively little use.

In view of these circumstances, it will be obvious that it was extremely difficult to determine the extent of the power of the various signals under examination.

It should be remembered that while the sound from the whistle is equally distributed in all directions,* that from the two other signals, both of which are provided with trumpets, is not so distributed.

The difference is apparent near by, but (as we have seen before) on account of the tendency of sound to spread, it is imperceptible at a distance.

In the siren the sound is most distinct in the axis of the trumpet.

In the Daboll trumpet it is usually strongest in a plane perpendicular to this axis.

This is at variance directly with any observation I have myself made.

Relative power.—From the average of a great number of experiments the following result was obtained:

The power of the first-class siren, 12-inch whistle, and first-class Daboll trumpet, may be expressed by the numbers 9, 7, 4.

The extreme limit of sound of the siren was not ascertained. That of the 12-inch whistle is about twenty miles, and of the trumpet twelve.

Consumption of fuel and water.—The siren, when working with a pressure of 72 pounds of steam, consumes about 180 pounds of coal and 126 gallons of water per hour.

The 12-in. whistle, with 55 pounds pressure of steam, consumes 60 pounds of coal and 40 gallons of water per hour.

The Daboll trumpet, with 10 pounds pressure of air in the tank, consumes about 20 pounds of coal per hour.

The relative expenditure of fuel would be: Siren, 9; whistle, 3; trumpet, 1.

---

*The sound of the whistle is equally distributed horizontally. It is however much stronger in the plane containing the lower edge of the bell than on either side of this plane. Thus if the whistle is standing upright,—in the ordinary position, its sound is more distinct in a horizontal plane passing through the whistle, than above or below it.

The siren.—Of the three machines this is the most complicated. It uses steam at a high pressure, and some of its parts move with very great velocity, the siren spindle making from 1,800 to 2,400 revolutions per minute. The boiler must be driven to its full capacity in order to furnish sufficient steam. A large quantity of steam is at intervals suddenly drawn from the boiler, causing a tendency to foam and to eject a considerable amount of water through the trumpet.

The constant attention of the keeper is required to regulate the fire, the supply of water to the boiler, of oil to the journals, &c.

In general terms, it may be stated that the siren requires more skill and attention in its management than either of the other signals.

The Daboll trumpet.—As the caloric engine, which has been hitherto employed to operate this signal, requires little fuel, no water, and is perfectly safe as regards danger from explosion, it would—at the first glance, appear to be the most suitable power that could be applied to fog-signals, and was accordingly at first exclusively adopted for this purpose. It was however found to be so liable to accident, and so difficult to repair, that of late years it has been almost entirely rejected. In the steam-boiler, the furnace is surrounded by water, and it is impossible under ordinary circumstances to heat the metal much above the temperature of the water. The furnace of the caloric engine is surrounded by air, and is therefore liable to be burned out if the fire is not properly regulated.

The working-piston is packed with leather, and as it moves horizontally, with its whole weight resting on the lower side of the cylinder, the packing at its lower edge is soon worn out.

If the engine is allowed to stop with the piston at the furnace-end of the cylinder the leather is destroyed by the heat. The re-packing of a piston is a difficult and expensive operation, requiring more skill than can be expected among the class of men from which light-house keepers are appointed.

Another accident to which these engines are subject arises from a sudden check in the velocity of the piston, caused either by the jamming of the leather packing or the introduction of dirt into the open end of the cylinder, in which case the momentum of the heavy eccentrically-loaded fly-wheel is almost sure to break the main rocker-shaft.

The expense of repairs is considerably increased by the fact that these engines are not now in general use, and when important repairs are required it is usually necessary to send to the manufacturer.

This signal requires much attention. The fires must be carefully regulated to avoid burning out the furnace, the journals thoroughly oiled, and the cylinders well supplied with tallow.

The steam-whistle.—This machine requiring much less steam than the siren in proportion to the size of its boiler, there is not the same necessity for forcing the fire; the pressure of steam required is less, and the point from which it is drawn much higher above the water-level in the boiler, and there is consequently no tendency to foam.

The machinery is simple; the piston pressure very light, producing but little strain on the different parts of the engine, which is therefore not liable to get out of order and requires no more attention than a common stationary engine.

One marked advantage possessed by this signal is that should the engine become disabled, the whistle may still be sounded by working the valve by hand. This is not the case with the two others, where an accident to any part of the machinery renders the signal for the time useless.

It will thus be seen that the siren is the most expensive of the fog-signals as regards maintenance, and that it is adapted only to such stations as are abundantly supplied with water and situated in the vicinity of machine-shops where the necessary repairs can be promptly made.

On the other hand, as it is the most powerful signal, there are certain stations where it should have the preference; as for example Sandy Hook, which from its importance demands the best signal that can be procured, regardless of cost. Such stations should be provided with duplicate apparatus, well supplied with spare parts, to guard against any possibility of accident.

There should be a keeper whose sole business must be to attend the signal, and who should have sufficient mechanical skill to make the ordinary repairs. He should moreover be a licensed engineer.

There will also be required an assistant, who may be one of the light-keepers, to relieve him during the continuance of foggy weather.

The steam-whistle is the simplest in construction, most easily managed and kept in repair, and requires the least attention of all the fog-signals. It is sufficiently powerful for most localities, while its consumption of fuel and water is moderate.

It has been found on this coast that a sufficient quantity of rain-water can be collected to supply the 12-in. whistle at nearly every station. This has been the case for the last two years at Martinicus.

The Daboll trumpet, operated by a caloric engine, should only be employed in exceptional cases, such as at stations where no water can be procured, and where—from the proximity of other signals, it may be necessary to vary the nature of the sound.

The trumpet however may undoubtedly be very much improved by employing steam power for condensing the air. The amount of work required, which is that of compressing 70 cubic feet of air to an average pressure of 8 pounds per inch, would be less than two-horse power. For this purpose the expenditure of fuel and water would be moderate; indeed, the exhaust steam could be condensed and returned to the cistern, should the supply of water be limited.

The siren also is susceptible of improvement, especially as regards simplification.

In the foregoing remarks I think the General has expressed a somewhat undue partiality for the whistle, and somewhat over-estimated the defects of the other instruments. The trumpets with Ericsson engine have not been abandoned, except partially in the two districts under the direction of General Duane, to which he probably intended to confine his statement. They are still in use in the third district, where they are preferred by General Woodruff, who finds no difficulty in keeping them in repair, having employed a skilled machinist who has made these instruments his special study, and who—visiting them from time to time, makes repairs and supplies new parts.

The intermittent action of fog-signals makes it necessary to employ a peculiar form of boiler. The steam used is at a high pressure, and drawn off at intervals; consequently there is a tendency to foam and throw out water with the steam. To obviate this difficulty the form of boiler found by experience to be best adapted to this service is a horizontal tubular boiler (locomotive), with rather more than one-half of the interior space allowed for steam-room. The steam-dome is very large, and is surmounted by a steam pipe 12 ins. in diameter. Both the dome and pipe were formerly made much

smaller, but were gradually enlarged as long as any difficulty with regard to foaming was noticed. The steam is drawn off at a point 10 ins. above the water-level in the boiler. The main points to be observed are to have plenty of steam-room, and to draw the steam from a point high above the water-level. It will be readily perceived that a vertical tubular boiler is entirely unsuited to this work.

It is essential, both as regards economy of fuel and the efficient working of the signal, that the boiler (including the dome and stand-pipe) should be well covered with some good non-conductor of heat. A material (called salamander felting) manufactured in Troy, N. Y., was used on the fog-whistle boiler at House Island during the winter of 1870. There resulted a saving of more than 20 per cent. of fuel over that consumed in the same boiler when uncovered. Where this material cannot be procured, a thick layer of hair felting, covered with canvas, will be found to answer a good purpose.

Various expedients have been proposed with the view of keeping the water in the boilers hot when the signals are not in operation, that the signal may always be ready to sound at a very short notice, and that the water in the boiler and pipes may be prevented from freezing in extremely cold weather. One of these contrivances is "Sutton's circulating water heater." It consists essentially of a small, vertical, tubular boiler, entirely filled with water, and connected with the boiler or tank which contains the water to be heated, by two pipes on different levels. As soon as the water in the heater is warmed, a circulation commences, the hot water flowing through the upper pipe into the boiler, and the cold through the lower pipe from the boiler to the heater. As the furnace in the heater is very small but little fuel is consumed, and nearly the entire heat produced by the combustion is utilized.

The apparatus has been extensively employed in heating the water in tanks designed for filling the steam fire-engine boilers, when the alarm of fire is first given, and appears admirably adapted to this purpose If used in connection with a steam boiler, it should be disconnected before steam is raised in the latter, as from its construction it is not calculated to withstand any considerable pressure.

An arrangement (similar in principle) has been used in the first light-house district, consisting of a small cylinder coal-stove, of the ordinary pattern, around the interior of which, and above the grate, is introduced a single coil of $\frac{3}{4}$ in. pipe. This coil is connected with the boiler by two pipes, one entering near the bottom, the other about 2 feet higher. It has been found that in consequence of the rapid circulation of the water through this coil, and the great capacity of water for heat, nearly all the heat from the fire in the stove is transferred to the water in the boiler. This arrangement possesses the advantage of the $\frac{3}{4}$ in. pipe, being strong enough to stand any pressure that can be used in the boiler, thus rendering it unnecessary to disconnect it at any time.

Experience has however proved that none of these contrivances are essential. It is seldom that an attentive keeper cannot fore see the approach of fog or snow in time to have the apparatus in operation as soon as required, even when obliged to start his fire with cold water in the boiler.

Keepers should be directed to watch the state of the weather carefully, and to light their fires at the first indication of fog or snow-storm. As soon as the water in the boiler is near the boiling point, should the necessity for sounding the signal have not yet arisen, the fire may be banked, and in this state the water may be kept hot for any length of time at a moderate expenditure of fuel. With proper care, no more fuel is required to keep the water at the requisite temperature by means of a banked fire than by any other method, and it is a matter of great importance to avoid complicating fog-signal apparatus by unnecessary appendages.

The same plan should be adopted in extremely cold weather to prevent the water in the boiler from freezing. There should be a small air-cock in the draught-pipe near its junction with the feed-pump, and in cold weather

this should be opened when the pump is not in use, in order to allow the pipe to empty itself.

When the draught-pipe cannot be protected from the cold, and the well is at a considerable distance from the engine, the following expedient has been employed with success: The pipe is enclosed in an India-rubber hose of about double its diameter, and from time to time steam is forced through the space between the hose and draught-pipe by means of a small pipe from the boiler.

Although the laws governing the reflection of light and heat are undoubtedly in a great measure applicable to sound, there are yet so many disturbing influences, such as inflection, refraction, (caused by the varying density of the atmosphere,) &c., interfering with the reflection of the latter, that but little use can be made of this property in directing and condensing the waves of sound issuing from a fog-signal. This fact may be illustrated by an account of some experiments made during the last year.

A whistle being sounded in the focus of a large parabolic reflector, it was very perceptible to an observer in the immediate vicinity that the sound was louder in the front than in the rear of the reflector. As the distance of the observer from the whistle was increased, this disparity rapidly diminished, and at the distance of a few hundred yards, entirely disappeared. The *beam* of sound had been dissipated and the *shadow* had vanished. The effect of a horizontal sounding-board 10 feet square, suspended over the whistle to prevent the escape of sound in a vertical direction, was inappreciable at the distance of a quarter of a mile.

The employment of a trumpet with the whistle was rather more successful. The trumpet was constructed of wood, in the form of the frustum of a square pyramid; the larger base being 10 ft. by 10 ft., the smaller base 2 ft. by 2 ft., and the length 20 ft. The axis was horizontal, and the whistle placed at the smaller end. By this arrangement the increased power of the sound could be perceived at the distance of a mile, the action being similar to that of a speaking-trumpet.

It is probable that some modification of this form of whistle may be advantageously employed in certain localities, but there is however a disadvantage attending the use of a trumpet with fog-signals.

The sound from a trumpet not being uniformly distributed, it is difficult to estimate the distance of the signal, or as the pilots term it "to locate the sound." This has been observed in the siren and Daboll trumpet. The sound from these signals being stronger on one course than any other, may be distinctly heard from a vessel when crossing the axis of the beam of sound, but as its distance from this line increases, the sound appears fainter and more remote, although the vessel may be approaching the signal.

From an attentive observation during three years of the fog-signals on this coast, and from the reports received from captains and pilots of coasting vessels, I am convinced that in some conditions of the atmosphere the most powerful signals will be at times unreliable.

Now it frequently occurs that a signal, which under ordinary circumstances would be audible at the distance of fifteen miles, cannot be heard from a vessel at the distance of a single mile. This is probably due to the reflection mentioned by Humboldt.

The temperature of the air over the land where the fog-signal is located, being very different from that over the sea, the sound—in passing from the former to the latter, undergoes reflection at their surface of contact. The correctness of this view is rendered more probable by the fact that when the sound is thus impeded in the direction of the sea it has been observed to be much stronger inland.

When a vessel approaches a signal in a fog, a difficulty is sometimes experienced in determining the position of the signal by the direction from which the sound appears to proceed, the apparent and true direction being

entirely different. This is undoubtedly due to the refraction of sound passing through media of different density.

Experiments and observation lead to the conclusion that these anomalies in the penetration and direction of sound from fog-signals are to be attributed mainly to the want of uniformity in the surrounding atmosphere, and that snow, rain, fog, and the force and direction of the wind, have much less influence than has generally been supposed.

In the foregoing I differ entirely in opinion from General Duane as to the cause of extinction of powerful sounds being due to the unequal density of the atmosphere. The velocity of sound is not at all affected by barometric pressure, but if the difference in pressure is caused by a difference in heat, or by the expansive power of vapor mingled with the air, a slight degree of obstruction of sounds may be observed. But this effect I think is entirely too minute to produce the results noted by General Duane, while we shall find in the action of the currents of wind above and below a true and sufficient cause.

The experimental whistles were of the following dimensions, viz., 2½ inches, 3 ins., 4 ins., 5 ins., 6 ins., 10 ins., 12 ins., and 18 ins. in diameter. Those of 2½ ins., 3 ins., 5 ins., and 10 ins. were fitted (instead of the ordinary bell) with long cylinders provided with movable pistons, so that the effective length of the bell could be altered at pleasure. The pitch of the blast was found to vary with the length of the bell, and the power of the whistle with its diameter. The ratio of the power to the diameter was not accurately obtained, but it is probable that the extreme range of sound of a whistle is proportional to the square root of its diameter.

This result (that the pitch varies with the length of the bell,) is in conformity with well-established principles of resounding cavities; and that the power should increase with the extent of the aerial reed (the vibrations of which give motion to the resounding air within the cavity,) is also as we have seen, in accordance with hypothetical considerations: but as the density of this stream of steam (and consequently the rapidity of its vibrations) depends upon the pressure of the steam in the boiler, a perfect whistle should have the capability of changing its dimensions, not only in relation to the width of its throat, but also in regard to the pressure of the steam in the reservoir.

The pitch giving the greatest range appears to be at the middle of the scale of sound. It is certain that a good result cannot be obtained from either a very shrill or a bass note. This remark is applicable to all varieties of signal.

The 10-in. and 12-in. whistles are recommended for ordinary use. The 18-in. whistle is more powerful, but the increase of power bears too small a proportion to that of the expenditure of fuel to render its employment generally advisable. The best results were obtained by giving the whistle the following proportions: The diameter of the bell equaling two-thirds of its length, and the set of the bell, *i. e.*, the vertical distance of the lower edge above the cup, the one-third to one-fourth of the diameter for a pressure of 50 to 60 pounds of steam.

A bell (whether operated by hand or by machinery,) cannot be considered an efficient fog-signal on the sea-coast. In calm weather it cannot be heard half the time at a greater distance than one mile, while in rough weather the noise of the surf will drown its sound to seaward altogether.

On approaching a station I have frequently seen the bell rung violently by the keeper, without being able to hear the sound until I had landed.

Nevertheless, all important stations should be provided with bells, as there are occasions when they may serve a useful purpose, but it should be well understood by mariners that they must not expect always to hear the bells as a matter of course.

Bells should not be omitted at stations furnished with steam fog-signals, especially when the latter are not in duplicate, and mariners should be warned that the bell will be sounded when the regular signal is disabled.

It has been observed that a bell rung by hand can be heard farther than when sounded by machinery, and many of the steamboat companies on this coast pay the keepers of bells rung by clock-work to ring them by hand when the boats of their line are expected to pass.

We think the difference in the effect of ringing of bells by hand or by machinery is so slight as to be inappreciable except at a short distance. It is true (as I have before observed,) that the sound is louder when the mouth of the bell is directed toward the hearer than when the edge is so directed, but on account of the spreading of this sound, the effect is lost in a small distance, and indeed in one light-house the bell is permanently placed with the axis of its mouth directed horizontally, and in this position, if the bell were struck interiorly with a hammer, which would give it a larger vibration than if struck exteriorly, I doubt whether any difference would be observed between the two methods of ringing; and if any existed it would probably be in favor of the fixed bell rung by machinery.

On rivers, narrow channels, and lakes, where the difficulty from the noise of the surf does not exist, this species of signal may be used to advantage, as its maintenance requires but a small expenditure of either money or labor, and by a proper arrangement of the machinery the intervals between the strokes of the bell may be so regulated as to avoid the danger of confounding the signals, however near together.

Although a bell may be heard better when sounded by hand than by clock-work, yet in thoroughfares where the signal must be kept in constant operation during the entire continuance of a fog, it would be impracticable to make use of the former method, and recourse must be had to machinery.

In arranging the signal the bell and machinery must be placed as low as possible, as the sound is heard much more plainly on the water when the bell is near its surface, and also as the machinery, when thus situated, is steadier and more readily accessible.

Particulars as to the siren.—The boiler of a second-class apparatus is 12 feet long, 42 inches in diameter, and has 300 feet of heating surface. The dome is 2 feet in diameter and 3 feet high.

The cylinder of the engine is 4 inches in diameter and 6 inches stroke. The prolongation of the piston-rod forms the plunger of the feed-pump. The main shaft carries three pulleys; the larger driving the siren-spindle, the second, the worm and screw-gear, and the third, the governor.

In the worm-gear the wheel makes two revolutions per minute, and is provided with a cam, which acting on a lever opens the valve, admitting steam through the siren-disks. The cam has such a length as to hold the valve open for about seven seconds. A counter-weight closes the valve as soon as the lever is released by the cam.

The siren itself consists of a cylindrical steam-chest, closed at one end by a perforated brass plate. The perforations are twelve in number, equi-distant from each other, and arranged on the circumference of a circle, whose center is in the axis of the cylinder. The other end is closed by a cast-iron head. The heads are connected by a brass pipe, through which the spindle passes.

The perforated head is covered on the exterior by a brass disk, attached to the spindle, having twelve rectangular notches corresponding to the apertures on the former, and so arranged that by its revolution these apertures are simultaneously opened and closed. The spindle is driven by a belt from the large pulley on the main shaft. This shaft makes 180 revolutions per minute; the spindle, 1,620; and as there are 12 apertures in the disks, from each there will issue jets of steam at the rate of 19,440 per minute. The sound produced by these impulses may be rendered more or less acute by increasing or diminishing the velocity of revolution.

The valve and valve-seat are disks similar to those already described, having however four openings instead of twelve. The valve revolves on the brass tube inclosing the siren-spindle, and is worked by a bevel gear. The trumpet is of cast-iron.

The Daboll trumpet.—The apparatus used in the foregoing experiments is a second-class trumpet, operated by an Ericsson caloric-engine. The air-pump is single-acting. Its cylinder is 12 ins. in diameter by 12 in. stroke. The engine makes 40 strokes per minute. There is a screw-thread raised on the main shaft, which acting on a wheel drives a bevel gear, giving motion to a cam-wheel. The latter makes one revolution in two minutes, and is furnished with three equidistant cams. These cams—pressing on the valve-lever, throw the valve open once in forty seconds, admitting the compressed air through the reed-chest into the trumpet.

The quantity of air forced into the tank should be in excess of that needed for the trumpet, the surplus being allowed to escape through a delicate safety-valve. This is necessary to provide against a deficiency in case of leakage, and also to allow the pressure of air to be regulated to accommodate the reed. Each reed requiring a different pressure, it is necessary to alter the pressure of the valve-spring whenever a reed is changed.

The first-class trumpet differs only in size from that described.

The caloric-engine for the first class has a 30 in. cylinder. The air-pump is 16½ ins. by 15 in. stroke.

The steam-whistle.—The boiler of this machine is that of the siren. On the forward part of the boiler, the bed-plate of a small engine is secured by two cast-iron brackets. The cylinder of this engine is 4 ins. by 9 ins. The fly-wheel shaft carries an eccentric, which, acting through a rod and pawl on a ratchet-wheel, gives the required motion to the cam-wheel shaft.

The cam-wheel (which makes one revolution per minute,) is provided with

one or more cams, depending on the number of blasts to be given in a minute; the length of the blast being regulated by that of the cams.

The valve for admitting the steam into the whistle is a balance-valve, the diameters of the two disks being respectively $3\frac{1}{4}$ ins. and $2\frac{3}{4}$ ins., which difference is sufficient to cause the pressure of steam to close the valve tight, without requiring too great a force to open it. The valve is worked by a stem attached to the rocker-shaft at the lower part of the steam-pipe. This shaft passes through a stuffing-box in the steam-pipe, and is provided with a collar, which the pressure of the steam forces against the interior boss on the pipe, thus making the joint steam-tight. The exterior arm on this rocker-shaft (as well as that on the engine,) is perforated in such a manner as to allow the throw of the valve to be adjusted.

In the comments I have made on the report of General Duane, the intention was not in the least to disparage the value of his results, which can scarcely be too highly appreciated; but inasmuch as the true explanation of the phenomena he has observed has an important bearing on the location of fog-signals and on their general application as aids to navigation, and is also of great interest to the physicist, who values every addition to theoretical as well as practical knowledge, I have thought not only that the remarks here offered are necessary, but also that special investigations should be made to ascertain more definitely the conditions under which the abnormal phenomena described by the General occur, and to assign if possible a more definite and efficient cause than those to which he has attributed them.

Much thought has therefore been given to the subject, and since the date of General Duane's report I have embraced every opportunity that occurred for making observations in regard to them. The first step we made toward obtaining a clew to the explanation of the phenomena in question resulted from observations made at New Haven, namely: 1st, the tendency of sound to spread laterally into its shadow; 2d, the fact that a sound is frequently borne by an upper current in an opposite direction to the wind at the surface; and 3d, that a sound moving against a wind is heard better at a higher elevation. The first point to consider is in what manner the wind affects sound. That it is in some way connected with the distance to which sound can be heard is incontestably settled by general observation. At first sight,

the explanation of this might seem to be very simple, namely, that the sound is borne on in the one direction and retarded in the other by the motion of the wind. But this explanation, satisfactory as it might appear, cannot be true. Sound moves at the rate of about 780 miles an hour, and therefore on the above supposition, a wind of 7·8 miles per hour could neither retard nor accelerate its velocity more than one per cent.—an amount inappreciable to ordinary observation; whereas we know that a wind of the velocity mentioned is frequently accompanied with a reduction of the penetrating power of sound—of more than 50 per cent.

The explanation of this phenomenon, as suggested by the hypothesis of Professor Stokes, is founded on the fact that in the case of a deep current of air, the lower stratum—or that next the earth, is more retarded by friction than the one immediately above, and this again than the one above it, and so on. The effect of this diminution of velocity as we descend toward the earth, is—in the case of sound moving with the current, to carry the upper part of the sound-waves more rapidly forward than the lower parts, thus causing their resultant impulse (the sound-beam) to incline toward the earth, or in other words to be thrown down upon the ear of the observer. When the sound is in a contrary direction to the current, an opposite effect is produced; the upper portion of the sound-waves is more retarded than the lower, which advancing more rapidly in consequence, inclines the rays of sound upward and directs them above the head of the observer. To render this more clear, let us recall the nature of a beam of sound, in still air, projected in a horizontal direction. It consists of a series of concentric waves perpendicular to the direction of the beam,—like the palings of a fence. Now if the upper part of the waves has a slightly greater velocity than the lower, the beam will be bent downward in a manner somewhat analogous to that of a ray of light in proceeding from a rarer to a denser medium. The effect of this deformation of the wave will be cumulative from the sound-centre onward, and hence—although the velocity of the wind may have no perceptible effect on the

velocity of the sound, yet this bending of the wave being continuous throughout its entire course, a marked effect must be produced.

A precisely similar effect will be the result, but perhaps in a considerably greater degree, in case an upper current is moving in an opposite direction to the lower, when the latter is adverse to the sound; and in this we have a logical explanation of the phenomenon observed by General Duane, in which a fog-signal is well heard during the occurrence of a north-east snow-storm. Certainly this phenomenon cannot be explained by any peculiarity of the atmosphere as to variability of density, or of the amount of vapor which it may contain.

The first phenomenon of the class mentioned by General Duane which I had the good fortune to witness was in company with Sir Frederick Arrow, and Captain Webb, of the Trinity House, London, in their visit to this country in 1872. At the distance of two or three miles from an island in the harbor of Portland, Maine, on which a fog-signal was placed, the sound which had been distinctly heard on approaching the island was lost for nearly a mile, and slightly regained at a less distance. On examining the position of the fog-signal, which was situated on the farther side of the island from the steamer, we found it placed immediately in front of a large house with rising ground in the rear, which caused a sound-shadow, into which the sound was projected at a distance, (on account of the lateral divergence of the rays,) but not in the immediate vicinity of the island. In the same year, I made an excursion in one of the light-house steamers, with Captain Selfridge, to an island on the coast of Maine, at which abnormal phenomena were said to have been observed; but on this occasion no variation of the sound was noted, except that which was directly attributable to the wind, the signal being heard much farther in one direction than in the opposite.

PART II.—ON SOME ABNORMAL PHENOMENA OF SOUND.

(Bulletin Philosophical Society Washington; vol. II, Appendix, pp. 45–52.)

Read December 11, 1872.

The communication which I propose to make this evening is brought forward at this time especially on account of the presence of Dr. Tyndall, he being connected with the light-house system of Great Britain, while the facts I have to state are connected with the light-house service of the United States, and must therefore be of interest to our distinguished visitor. The facts I have to present form part of a general report to be published by the United States Light-House Board.

The Light-House Board of the United States has from its first establishment aimed not only to furnish our sea-coast with all the aids to navigation that have been suggested by the experience of other countries and to adopt the latest improvements, but also to enrich the light-house service with the results of new investigations, and new devices for the improvement of its efficiency, or in other words to add its share to the advancement of a system which pertains to the wants of the highest civilization.

Among the obstructions to navigation none are more serious, especially on the American coast, than those caused by fogs. Fog (as it is well known) is due to the mingling of warmer air surcharged with moisture—with colder air, and nowhere on the surface of the earth do more favorable conditions exist for producing fogs than on both our Atlantic and Pacific coasts. On the Atlantic the cold stream of water from the polar regions in its passage southward, on account of the rotation of the earth—passes close along our eastern coast from one extremity to the other, and parallel to this but opposite in direction, for a considerable distance is the great current of warm water known as the Gulf-stream. Above the latter the air is constantly surcharged with moisture, and consequently whenever light winds blow from the latter across the former, the vapor is condensed into fog, and since

in summer along our eastern coast the southerly wind prevails, we have during July, August, and September, especially on the coast of Maine, an almost continuous prevalence of fogs so dense that distant vision is entirely obstructed.

On the western coast the great current of the Pacific, after having been cooled in the northern regions, in its passage southward gives rise to cold and warm water in juxtaposition, or in other words a current of the former through the latter, and hence whenever a wind blows across the current of cold water a fog is produced.

From the foregoing statement it is evident that among the aids to navigation, fog-signals are almost as important as light-houses. The application of the science of acoustics to the former, is however far less advanced than is that of optics to the latter. Indeed, attempts have been made to apply lights of superior penetrating power (as the electric and calcium lights) to supersede the imperfect fog-signals in use. When however we consider the fact that the absorptive power of a stratum of cloud, which is but a lighter fog, of not more than a mile or two in thickness, is sufficient to obscure the image of the sun, the intensity of the light of which is far greater than that of any artificial light, it must be evident that optical means are insufficient for obviating the difficulty in question.

The great extent of the portions of the coast of the United States which are subject to fogs—renders the investigation of the subject of fog-signals one of the most important duties of the Light-House Board.

In studying this subject it becomes a question of importance to ascertain whether waves of sound (like those of light) are absorbed or stifled by fog; on this point however observers disagree. While from the striking analogy in many respects between light and sound, the opinion has largely prevailed that sound is impeded by fog, the opposite opinion has been adopted by some observers—that sound is in some instances better heard during a fog than in clear weather. To settle this question definitely the Light-House Board has directed that at two light-houses on the route from Boston

to Saint John, the fog-signal shall be sounded every day on which the steamboats from these ports pass the station, both in clear and foggy weather, the pilots on board these vessels having, for a small gratuity, engaged to note the actual distance of the boat when the sound is first heard on approaching the signal and is last heard on receding from it. The boats above mentioned estimate their distance with considerable precision by the number of revolutions of the paddle-wheel as recorded by the indicator of the engine, and it is hoped by this means to definitely decide the point in question. We think it probable that fog may very slightly diminish the penetrating power of sound, or in other words produce an effect analogous to that on the propagation of light. But when we consider the extreme minuteness of the particles of water constituting the fog as compared with the magnitude of the waves of sound, the analogy does not hold except in so small a degree as to be of no practical importance, or in other words the existence of a fog if a true—is we think a wholly insufficient cause of diminution of sound; a view borne out by the great distance at which our signals are heard during a dense fog.

Another cause of the diminution of the penetrating power of sound (also probably a true one) is the varying density of the atmosphere—from heat and moisture, in long distances. The effect of this however would apparently be to slightly distort the wave of sound rather than to obliterate it. However this may be, we think from all the observations we have made, the effect is small in comparison with another cause, viz., that of the influence of wind. During a residence of several weeks at the sea-shore, the variation in intensity of the sound of the breakers at a distance of about a mile, in no case appeared to be co-incident with the variations of an aneroid barometer or a thermometer, but to be in every instance affected by the direction of the wind.

The variation in the distinctness of the sound of a distant instrument as depending on the direction of the wind is so marked, that we are warranted in considering it the principal cause of the inefficiency in certain cases of the most

powerful fog-signals. The effect of the wind is usually attributed (without due consideration) to the motion of the body of air between the hearer and the sounding instrument; in the case of the wind coming toward him, it is supposed that the velocity of the sound is re-enforced by the motion of the air, and when in the opposite direction that it is retarded in an equal degree. A little reflection however will show that this cannot be the cause of the phenomena in question, since the velocity of sound is so vastly greater than that of any ordinary wind that the latter can only impede the progress of the former by a very small percentage of the whole.

Professor Stokes, of Cambridge University, England, has offered a very ingenious hypothetical explanation of the effect of wind on sound, which we think has an important practical bearing—especially in directing the line of research, and subsequent application of principles.

His explanation rests upon the fact that during the passage of a wind between the observer and the sounding instrument, its velocity will be impeded at the surface of the earth on account of friction and other obstacles; and the velocity of the stratum immediately above will be somewhat reduced by that below, and so on, the retardation being gradually lessened as we ascend through the strata. From this it follows that the sound wave will be deformed, and the direction of its *normal* changed. Suppose for example that the wind is blowing directly from the observer. In this case the retardation of the sound wave will be greater above than below, and the upper part of the wave-front will be thrown backward, so that the axis of the phonic ray will be deflected upward, and over the head of the observer. If on the other hand a deep river of wind (so to speak) is blowing directly toward the observer, the upper part of the front of the wave will be inclined down and toward him, concentrating the sound along the surface of the earth.

The science of acoustics in regard to the phenomena of sound as exhibited in limited spaces has been developed with signal success. The laws of its production, propagation, reflection, and refraction have been determined with

much precision, so that we are enabled in most cases to explain, predict, and control the phenomena exhibited under given conditions. But in the case of loud sounds and those which are propagated to a great distance, (such as are to be be employed as fog-signals,) considerable obscurity still exists. As an illustration of this I may mention the frequent occurrence of apparently abnormal phenomena. General G. K. Warren informs me that at the battle of Seven Pines, in June, 1862, near Richmond, General Johnston, of the Confederate army, was within three miles of the scene of action with a force intended to attack the flank of the Northern forces, and although he listened attentively for the sound of the commencement of the engagement, the battle—which was a severe one lasting about three hours, ended without his having heard a single gun. (See Johnston's report.) Another case of a similar kind occurred to General McClellan at the battle of Gaines' Mills, June 27, 1862, also near Richmond. Although a sharp engagement was progressing within three or four miles for four or five hours, the General and his staff were unaware of its occurrence, and when their attention was called to some feeble sound, they had no idea that it was from anything more than a skirmish of little importance. (See Report of the Committee on the Conduct of the War.) A third and perhaps still more remarkable instance is given in a skirmish between a part of the Second Corps under General Warren and a force of the enemy. In this case the sound of the firing was heard more distinctly at General Meade's headquarters than it was at the headquarters of the Second Corps itself, although the latter was about midway between the former and the point of conflict. The sound appeared so near General Meade's camp that the impression was made that the enemy had advanced between it and General Warren's command. In fact, so many instances occurred of wrong impressions as to direction and distance derived from the sound of guns, that little reliance came to be placed on these indications.

In the report of a series of experiments made under the direction of the Light-House Board by General J. C. Duane, of

the Engineer Corps, is the following remark: "The most perplexing difficulty arises from the fact that the fog-signal often appears to be surrounded by a belt varying in radius from one to one and a half miles, from which the sound appears to be entirely absent. Thus in moving directly from a station the sound is audible for the distance of a mile, is then lost for about the same distance, after which it is again distinctly heard for a long time."

Again, in a series of experiments at which Sir Frederick Arrow and Captain Webb, of the Trinity Board, assisted, it was found that in passing in the rear of the opposite side of an island in front of which a fog-signal was placed the sound entirely disappeared, but by going farther off to the distance of two or three miles, it re-appeared in full force, even with a large island intervening. Again, from the experiments made under the immediate direction of the present chairman of the Light-House Board, with the assistance of Admiral Powell and Mr. Lederle, the light-house engineer, and also from separate experiments made by General Duane, it appears that while a reflector in the focus of which a steam whistle or ordinary bell is placed—re-enforces the sound for a short distance, it produces little or no effect at the distance of two or three miles, and indeed the instrument can be as well heard in still air at the distance of four or five miles in the line of the axis of the reflector whether the ear be placed before or behind it. From these results we would infer that the divergency of sound, or its tendency to spread laterally as it passes from its source, is much greater than has been supposed from experiments on a small scale. The idea we wish to convey by this is that a beam of sound issuing through an orifice, although at first proceeding like a beam of light in parallel rays, soon begins to diverge and spread out into a cone, and at a sufficient distance may include even the entire horizon.

We may mention also in this connection, that from the general fact of the divergence of the rays of sound, the application of reflection as a means of re-enforcing sound must of necessity be in a considerable degree a failure.

By the application of the principle we have stated and the effect of the wind in connection with the peculiarities of the topography of a region and the position of the sounding body, we think that not only may most of the phenomena we have just mentioned be accounted for, but also that other abnormal effects may be anticipated.

In critically examining the position of the sounding body in the experiment we have mentioned, in which Sir Frederick Arrow and Captain Webb assisted, it was found that the signal was placed on the side of a bank with a large house directly in the rear, the roof of which tended to deflect the sound upwards so as to produce in the rear a shadow, but on account of the divergency of the beam this shadow vanished at the distance of a mile and a half or two miles, and at the distance of say three miles the sound of the instrument was distinctly heard. I doubt not that on examination all the cases mentioned by General Duane, with one exception, might be referred to the same principle, the exception being expressed in the following remarkable statement in his report to the Light-House Board: "The fog-signals have frequently been heard at a distance of *twenty* miles, and as frequently cannot be heard at the distance of *two* miles, and with no perceptible difference in the state of the atmosphere. The signal is often heard at a great distance in one direction, while in another it will be scarcely audible at the distance of a mile. For example, the whistle at Cape Elizabeth can always be distinctly heard in Portland,—a distance of nine miles, during a heavy north-east snow-storm, the wind blowing a gale directly from Portland toward the whistle."

This is so abnormal a case, and so contrary to generally received opinion, that I hesitated to have it published under the authority of the board until it could be verified and more thoroughly examined. In the observations that have been made under my immediate supervision the sound has always been heard farther *with* the wind than against it. It would appear therefore from all the observations that the normal effect of the wind when blowing directly against the sound, is to greatly diminish it.

There is however a meteorological condition of the atmosphere during a north-east storm on our coast which appears to me to have a direct bearing on the phenomenon in question. It is this, that while a violent wind is blowing from the north-east into the interior of the country, a wind of equal intensity is blowing in an opposite direction at an elevation of a mile or two. This is shown by the rapid eastwardly motion of the upper clouds as occasionally seen through breaks in the lower.

As a further illustration of this principle I may mention that on one occasion (in 1855) I started on my way to Boston from Albany, in the morning of a clear day, with a westerly wind. The weather continued clear and pleasant until after passing the Connecticut river, and until within fifty miles of Boston. We then encountered a storm of wind and rain which continued until we reached the city. On inquiry I learned that the storm had commenced in Boston the evening before, and although the wind had been blowing violently toward Albany for *twenty* hours it had not reached inwardly more than fifty miles. At this point it met the *west* wind and was turned back above in almost a parallel current. This is the general character of north-east storms along our coast, as shown by Mr. Espy, and is directly applicable to the phenomenon mentioned by General Duane, which must be accepted as a fact, though by no means a general one applicable to all stations. While a violent wind was blowing toward his place of observation from Cape Elizabeth at the surface of the earth, a parallel current of air was doubtless flowing above with equal or greater velocity in the opposite direction. The effect of the latter would be to increase the velocity of the upper part of the wave of sound, and of the former to diminish it; the result of the two being to incline the front of the wave of sound toward the observer, or to throw it down toward the earth, thus rendering the distant signal audible under these conditions when otherwise it could not be heard. I think it is probable that the same principle applies to other cases of the abnormal propagation of sound.

For the production of a sound of sufficient power to serve as a fog-signal, bells, gongs, &c., are too feeble except in special cases where the warning required is to be heard only at a small distance. After much experience the Light-House Board has adopted for first-class signals,—instruments actuated by steam or hot-air engines, and such only as depend upon the principle of resonance, or the enforcement of sound by a series of recurring echoes in resounding cavities.

Of these there are three varieties. First, the steam-whistle, of which the part called the bell is a resounding cavity, the sound it emits having no relation to the material of which it is composed; one of the same form and of equal size of wood producing an effect identical with that from one of metal. Another variety is the fog-trumpet, which consists of a trumpet of wood or metal actuated by a reed like that of a clarionet. The third variety is called the siren trumpet, which consists of a hollow drum, into one head of which is inserted a pipe from a steam-boiler, while in the other head a number of holes are pierced, which are alternately opened and shut by a revolving plate having an equal number of holes through it. This drum is placed at the mouth of a large trumpet. The sound is produced by the series of impulses given to the air by the opening and shutting of the orifices and consequent rushing out at intervals with explosive violence of the steam or condensed air. The instrument, as originally invented by Cagniard de Latour, of France, was used simply in experiments in physics to determine the pitch of sound; but Mr. T. Brown, of New York, after adding a trumpet to it and modifying the openings in the head of the drum and the revolving plate, under the direction of the Light-House Board, perfected it as a fog-signal, and as such it has been found the most powerful ever employed.

In ascertaining the penetrating power of different fog-signals I have used with entire success an instrument of which the following is a description: A trumpet of ordinary tinned iron of about 3 feet in length, and 9 inches in diameter at the larger end, and about 1 inch at the smaller, is gradually bent so that the axis of the smaller part is at

right angles to the axis of the larger end; on the smaller end is soldered a cone of which the larger end is about 2 inches in diameter. Across the mouth of this cone is stretched a piece of gold-beater's skin. When the instrument is used, the opening on the larger end is held before the apparatus to be tested, the plane of the mouth of the trumpet being vertical and the membrane being horizontal; over the membrane is strewed a small quantity of fine sand, which is defended from the agitation of the air by a cylinder of glass, the upper end of which is closed by a lens. When the apparatus under examination is sounded, (being sufficiently near,) the sand is agitated; the testing instrument is then moved farther off step by step until the agitation just ceases; this distance being measured is taken as the relative penetrating power of the sounding apparatus. The same process is repeated with another sounding instrument, and the distance at which the sound ceases to produce an effect on the sand is taken as the measure of the penetrating power of this apparatus, and so on. On comparing the results given by this instrument with those obtained by the ear on going out a sufficient distance the two are found to agree precisely in their indications. The great advantage in using this contrivance is that the relative penetrating power of two fog-signals may be obtained within a distance of a few hunded yards, while to compare them by the ear requires the aid of a steamer and a departure from the origin of sound in some cases of fifteen or twenty miles.

PART III.—INVESTIGATIONS DURING 1873 AND 1874.

(Report of the United States Light-House Board for 1874, pp. 107–117.)

Observations on Sound and Fog-Signals in August, 1873.

Professor Henry, chairman, and Commander John G. Walker, naval secretary, of the Light-House Board, left Portland August 12th, 1873, at 3 o'clock P. M. in the steam-tender *Myrtle*, Captain Foster, for Whitehead light-station, at which place abnormal phenomena of sound had been observed.

Whitehead light-station is on a small island about a mile and a half from the coast of Maine, on the western side of the entrance to Penobscot Bay, and in the direct line of the coasting-steamers and other vessels from the westward,—bound into the Penobscot Bay and river. The light-house and fog-signal are situated on the south-east slope of the island, the surface of which consists almost entirely of rock, the middle being at an elevation of 75 feet above the mean tide-level.

The phenomena which had been observed at this and other stations along the coast consisted of great variation of intensity of sound while approaching and receding from the station. As an example of this we may state the experience of the observers on board the steamer *City of Richmond* on one occasion, during a thick fog at night in 1872. The vessel was approaching Whitehead from the south-westward, when at a distance of about six miles from the station, the fog-signal—which is a 10-inch steam-whistle, was distinctly perceived and continued to be heard with increasing intensity of sound until within about three miles, when the sound suddenly ceased to be heard, and was not perceived again until the vessel approached within a quarter of a mile of the station, although from conclusive evidence furnished by the keeper, it was shown that the signal had been sounding during the whole time. The wind during this time was from the south, or approximately in an opposite direction to the sound. Another fact connected with this occurrence was that the keeper on the island distinctly heard the sound of the

whistle of the steamer, which was blown as soon as the whistle at the station ceased to be heard, in order to call the attention of the keeper to what was supposed to be a neglect of his duty in intermitting the operations of his signal. It should be observed that the sound from the steamer in this case was produced by a 6-inch whistle, while that of the station was from an instrument of the same kind, of 10 inches in diameter; or in other words a lesser sound was heard from the steamer, while a sound of greater volume from the station was unheard in an opposite direction. It is evident that this result could not be due to any mottled condition or want of acoustic transparency of the atmosphere, since this would absorb the sound equally in both directions. The only plausible explanation of this phenomenon is that which refers it to the action of the wind. In the case of the sound from the steamer, the wind was favorable for its transmission, and hence it is not strange that its sound should be heard on the island when the sound from the other instrument could not be heard on the steamer. To explain on the same principle the fact of the hearing of the sound at the distance of six miles, and afterward of losing it at the distance of three miles, we have only to suppose that in the first instance the retarding effect of the wind was small, and that in the second it became much greater on account of a sudden increase in the relative velocity of the current in the upper and lower portions.

After making a critical examination of the island and the position of the machinery, and also in regard to any obstacle which might interfere with the propagation of the sound, the keeper was directed to put the instrument in operation and to continue to sound it for at least two hours, or until the steamer was lost sight of; which direction was complied with. In passing from the island, almost directly against a light wind, the intensity of the sound gradually diminished as a whole—with the increase of distance, but varied in loudness from blast to blast, now louder, then again more feeble, until it finally ceased at a distance of about fifteen miles, as estimated by the intervals between the blasts and the sight of the steam as seen through a spy-glass, and also from points on the Coast-Survey charts.

The result of this investigation clearly showed the power of the apparatus in propagating sound under conditions not entirely favorable, since the wind—though light, was in opposition to the sound.

Cape Elizabeth Light-Station, Maine, August 29, 1873.—The fog-signal at this place is on a prominent headland to which the course of all vessels is directed when bound from the southward into Portland Harbor. It is furnished with two light-houses 919 feet apart and 143 feet above sea-level. The easterly tower is connected with the keeper's dwelling by a wooden-covered way 200 feet long and about 12 feet high; the station is furnished with a 10-inch steam fog-whistle, placed to the southward of the easterly tower at a distance of about 625 feet and about at right angles with the covered way; it therefore has a background, including the covered way, of about 65 feet above the height of the whistle, which was found to reflect a perceptible echo. The whistle was actuated by steam at 55 pounds pressure, consuming from 60 to 65 pounds of anthracite coal per hour. The whistle itself differs from the ordinary locomotive-whistle by having a projecting ledge or rim around the lower part through which the sheet of steam issues to strike against the lower edge of the bell. What effect this projecting ledge or rim may have is not known to the observers. This whistle is provided (for the purpose of concentrating the sound in a given direction) with a hollow truncated pyramid 20 feet long, 10 feet square at the large end, and $2\frac{1}{2}$ feet square at the small end, the axis of the pyramid being placed parallel to the horizon, with the whistle at the smaller end. In order to ascertain the effect of this appendage to the whistle, the simplest plan would have been to note the intensity of sound at various points on a circle of which the whistle should be the centre. This being impracticable on account of the intervention of the land, the observations were confined to points on the three arcs of a circle of about 120°, of which the axis divided the space into 80° and 40°, and a radius of one, two, and three miles. The result of these observations was that starting from the

axis of the trumpet on the east side, the sound grew slightly less loud until the prolongation of the side of the trumpet was reached, when it became comparatively faint and continued so until the line between the whistle and observer was entirely unobstructed by the side of the trumpet, when the sound was apparently as loud as in the prolongation of the axis itself. On the west side of the axis of the trumpet, the sound in a like manner diminished from the axis until the prolongation of the side of the trumpet was reached, when it became feeble, again slightly increased, and then gradually diminished until the line of direction made an angle of about 80° with the axis of the trumpet, when it ceased to be heard at a distance of about one and a half miles. It should be observed however that at this point the line of sight of the observers was obstructed by the side of the trumpet and the smoke-stack of the boiler. The wind was light, at south-southwest, approximately in opposition to the direction of the sound when it ceased to be heard.

We are informed that complaints had previously been made by officers of steamers passing near this point that the sound was here inaudible previous to the introduction of this trumpet; it would therefore follow that it is of no use to increase the effect on the western side of the axis, and is of injury to the sound on the lines of prolongation of its sides. If the sound should cease to be heard at the point mentioned when the trumpet is removed, the only apparent cause of the phenomenon will be the prevailing direction of the wind, which coming from the south-west will be in opposition to the sound of the whistle; but in the case of the present investigation the force of the wind was so small that it scarcely appeared adequate to produce the effect, and this question therefore must be left for further investigation. It may be important to state that in the case where the sound ceased to be heard, it was regained by sailing directly toward the station about one mile, or at half a mile from the station.

After making the foregoing observations as to the intensity of sound in different directions from the station,

the observations were closed by sailing directly along the axis of the trumpet until the sound, which gradually grew fainter as the distance increased, finally ceased to be heard at a distance of about nine miles. In comparing this last result with an instrument of about the same power at Whitehead, which gave a perceptible sound at a distance of fifteen miles, the only apparently variable circumstance was the velocity of the wind,—in both cases adverse to the direction of the sound; but in that of Cape Elizabeth it was of considerably greater force.

During the foregoing experiments, when the vessel was about a mile from the station, steaming directly outward,—in the prolongation of the axis of the instrument, there was heard after each sound of the whistle a distinct echo from the broad, unobstructed ocean, which was attributed at the time, as in other cases, to reflections from the crests and hollows of the waves. A similar phenomenon has since been elsewhere observed, and referred to a reflection from air of a different density. This observation becomes important in regard to the solution of the question as to the abnormal phenomena of sound.

Cape Ann Light-Station, Massachusetts, August 31, 1873.—This is one of the most important stations on the New England coast. It is furnished with two first-order lights and a 12-inch steam whistle, actuated by 60 pounds pressure of steam. The present is the fourth engine which has been erected at this station, in consequence of the complaints either as to the inefficiency of the sound or its failure to be heard in certain directions. It was at first proposed to sail entirely around the island in order to test the intensity of the sound in different directions, but this was found impracticable on account of the shallowness of water on the inland side; the observations were therefore confined to the direction in which complaints had been made as to the deficiency of the signal, namely, in a southerly direction. The result of these observations (the points of which included an arc of 120°) was that the sound was heard with equal intensity except when the direction of the station was to the northward and

eastward of the observers; then in one instance the sound became very indistinct, and in another was entirely lost, both at a distance of about two miles. In these cases the line of sight between the observers and the signal was interrupted—in the first by a small building the gable-end of which was within 10 feet of the whistle, and in the second by the south light-tower, which is within 30 feet of the whistle. In this series of experiments, as in the last, the wind was against the sound; the effect was noted by passing over the arc several times at different distances. The wind was from the southward and westward and very light, and the sound was finally lost at about six miles, and in the direction of the obstructions.

Boston Light-Station, August 31, 1873.—The light-house is situated on a low rocky island, on the north side of the main outer entrance to Boston Harbor, nine miles from the city. It is furnished with three caloric engines, two of the second class and one of the first. The two second-class engines are so arranged as to act separately or together, and in the latter arrangement serve to duplicate the larger engine. At the time the observations were made, the larger engine was about being repaired, and one of the smaller engines with the double air-reservoir was used. The larger engine is used with 12 pounds pressure of air, which falls to 8 pounds in producing the sound. The smaller engine, with the double reservoir, is started with 9 pounds pressure, which falls to 8 pounds. This difference in the pressure of air in the two engines is caused by the larger ratio of the reservoir to the size of the reed. With a greater pressure than 12 pounds to the square inch in the larger engine, and 9 pounds in the smaller, no sound is produced; the reed is unable to act against the pressure, and consequently the orifice remains closed. The trumpet of the larger of the engines is reported to have been heard eighteen miles at sea, which—in consideration of the results obtained at Whitehead, we thought very probable. The time required (from starting fires) to get a good working-pressure, is about half an hour. The amount of coal consumed per hour is 17 pounds.

There is moreover at this station a bell, operated by a Stevens clock, not at present used. It is placed on a high, wooden frame structure, on which one of the ancient bell striking machines was originally erected. The most proper position for the fog-signal is on the ground occupied by this bell-tower, but as this was not removed at the time of the erection of the trumpets, they were placed in such positions as to have the line of sound interrupted to the north-eastward by the bell and light towers. It was therefore thought probable that this was the cause of the deficiency of sound in this direction. To test this, the vessel was caused to traverse the arcs of several concentric circles, in the portion of the horizon where the sound was most required as a signal. The first arc traversed was about one and one-half miles from the signal. The vessel on this crossed the axis where the sound was quite loud, and proceeded northward until the sight of the trumpet was obscured by the before-mentioned towers, when the sound became almost inaudible. The vessel next returned across the axis, on a circle of about three miles radius, with similar results; but after crossing the axis the sound on the southern side continued to be but little diminished in intensity along an arc of two and a half miles, or as far as the land would allow the vessel to go. The vessel was next put upon an arc of which the radius was one and a half miles, and on the south side of the axis, and sailed to the northward until the axis was reached; it was then turned and run for the entrance of the harbor, hugging the southern shore, keeping as far from the signal as possible. Throughout this passage the sound was clear and loud, showing very little—if any diminution of power as the several positions deviated more and more from the direction of the axis, until the vessel was at right angles with the axis, the land not permitting any greater distance. The vessel approached to within three-quarters of a mile of the signal and then continued still farther around, until nearly in the rear of it, the sound still continuing clear and loud. The vessel next proceeded up the harbor, nearly in the line of the axis of the trumpet prolonged in the rear

still continuing to hear the signal distinctly until the keeper, losing sight of the vessel, stopped sounding the instrument. These observations were made under very favorable circumstances, it being nearly calm. What wind did exist was about equally favorable to points on either side of the axis. The inference from these observations is—first, that small objects placed near the source of sound tend to diminish its intensity in the direction of its interruption, and should therefore if possible be removed, or the instrument so placed as to obviate such obstructions; and second, that even with the trumpet, the sound so diverges from the axis as to be efficient even in the rear of the instrument.

Observations on Fog-Signals, in August and September, 1874.

The first of these investigations was made August 25, on board the steamer *Putnam*, at Little Gull Island, with Commodore Stephen D. Trenchard, inspector of lights of the third district, accompanied by Governor Charles R. Ingersoll, of Connecticut, and Captain John H. Upshur, U. S. N.

At this place are two sirens, the one to replace the other in case of an accident. One of the sirens was sounded with the pressure of 50 pounds per square inch. The wind was across the axis of the trumpet, and almost precisely at right angles to it.

The steamer was headed against the wind, on a line at right angles to the axis of the trumpet. The sound in this case also travelled against the wind, which was at an estimated velocity of from 4 to 5 miles per hour. The distance travelled before the sound became inaudible, was estimated by the speed of the steamer, at $3\frac{1}{2}$ miles.

The steamer was next headed in an opposite direction and returned along its previous path, across the mouth of the trumpet of the siren, the sound gradually increasing in strength without any marked irregularity, until the siren was reached, and on leaving this, (the course remaining the same,) the sound gradually diminished in intensity, but with less rapidity than before, until it was finally lost at a distance of $7\frac{1}{2}$ miles. In the latter instance, the movement of

the sound was with the wind. The result of these observations was conformable to that generally obtained from previous ones, namely that the sound is seldom or never heard at the same distance in different directions, and moreover that it is generally heard farther with the wind than against it.

The observations of this day also illustrate the spread of the sound-wave on either side of the axis of the trumpet, a fact which has frequently been noticed in other investigations. It may be well to mention that the siren trumpet at this locality is directed horizontally, with its prolonged axis passing over a space of very rough ground, (immediately in front of the mouth of the trumpet,) the surface of which is principally composed of bowlders: one of these (of very large size) is directly in front of the trumpet, and the idea occurred to me that this rough surface might produce some effect on the transmission of sound to a distance. I observed by strewing sand upon a paper that the former was violently agitated when held near the surface of the large bowlder just mentioned, during the blast of the siren trumpet.

At this station, during the visit of Sir Frederick Arrow, the sound was lost in the direction of the axis of the trumpet at a distance of two miles, and then again regained with distinctness at the light-vessel, a distance of four and one-half miles; this was what I have denominated an abnormal phenomenon, which was due as I think—to a slight variation in the velocity of the lower and upper parts of the current of air, but unfortunately, the demand for the use of the vessel as a light-house tender prevented the attempt to ascertain whether the same phenomenon would be observed a second time, and to further investigate its cause.

Observations September 1, 1874.—The second series of investigations this season was made with General J. G. Barnard, of the Light-House Board, and General I. C. Woodruff, engineer of the third district. We proceeded on this occasion in the steamer *Mistletoe* to Block Island,—one of the outer stations of the Light-House Board, fully exposed (without intervention of land) to the waves and storms of the ocean.

On the southerly side of this island a light-house is about being erected, and a siren station had been established at this locality, and was in full operation.

There are here two sirens attached to one boiler, one to be used in case of an accident to the other. For the sake of experiment they are of slightly different qualities, one with a larger trumpet with a revolving disk of the old pattern, giving a lower tone; the other a smaller trumpet having a revolving disk with openings allowing a much more sudden and full blast of steam, and revolving with greater velocity so as to give a higher pitch. The latter is far the superior instrument, as was evident to us by the sound which it produced, and as had been established by the use of the artificial ear in the manufactory of Mr. Brown. The effect on the unguarded ear was scarcely endurable, and the very earth around appeared to tremble during the blast. The keeper (an intelligent man who has been promoted from the position of assistant keeper at Beaver Tail light to this station) informed us that a fleet of fishing-vessels coming in, distinctly heard it at a distance estimated by their rate of sailing at scarcely less than thirty miles; this was on two separate occasions. The keeper had been directed to note and record the date at which he heard the sound from other signals; he reported that he had frequently heard the fog-signal at Point Judith, a distance of seventeen miles, and that the observer at the latter place frequently heard his signal; but on comparing records, the two sounds had not been heard simultaneously by the two keepers; when a sound was heard from one station, the opposite sound was not heard from the other, illustrating again the general rule that sound is not transmitted simultaneously with equal intensity in opposite directions.

This occasion also furnished very favorable conditions for observing the remarkable phenomenon of the ocean-echo. At the cessation of each blast of the trumpet, (after a slight interval,) a distinct and prolonged echo was returned from the un-obstructed ocean. It is important to observe—in regard to this phenomenon, that the siren is placed near the edge

of a perpendicular cliff, at an elevation of from 75 to 100 feet above the ocean, and furthermore that the direction of the wind formed an angle of about 35° with the axis of the trumpet. Now the loudness of this echo was not the greatest at the siren-house, but increased in intensity until a point was reached several hundred yards from the trumpet, approximately more in accordance with a reflection from the waves. The wind was blowing from the shore with the direction of the sound as it went off from the trumpet, and nearly against it on the return of the echo. I have attributed this phenomenon (which was first observed in 1866 at East Quoddy Head, on the coast of Maine, and since at various stations, at which the trumpet or siren has been used,) to the reflection of the sound from the crests and slopes of the waves, and the observation we have mentioned would appear to favor this hypothesis. In connection with this explanation, I may mention that my attention has been called by General M. C. Meigs, of the United States Army, to an echo from the palings of a fence, and also from a series of indentations across the under side of the arch of one of the aqueduct bridges of the Washington water-works. The fact that the sound was much louder at a point considerably distant from the trumpet was noted by one of the party entirely unacquainted with the hypothesis.

The keeper of this station confirmed (without a leading question) the statement of Captain Keeney, that a feeble sound of a distant object—as the roar of the surf, can frequently be heard against the direction of the wind, and that in this case it always betokens a change in the weather, and is in fact used generally by the fishermen as a prognostic of a change in the direction of the wind, which will in the course of a few hours invariably spring up from an opposite quarter. In such case it is highly probable (as has been stated,) that a change has already taken place in the direction of the upper strata of the air, although from theoretical considerations we might infer that the same result would be produced if the wind were stationary above and moving with a considerable velocity in a direction opposite

to the sound at the surface of the earth, the velocity gradually diminishing as we ascend, for in this case also the inclination of the sound waves would be downward.

Observations September 23, 1874.—The third series of investigations was made in company with Captain John L. Davis and Major Peter C. Hains, both of the Light-House Board, and General I. C. Woodruff, engineer of the third district, and Mr. Brown, patentee of the siren. For the purpose, three light-house tenders were employed, viz: the *Mistletoe*, Captain Keeney; the *Putnam*, Captain Field; and the *Cactus*, Captain Latham.

The place of operation chosen for the first day's series was about 1½ miles from the northern point of Sandy Hook.

From the experience gained by the accumulated observations which had been made, it was concluded that the phenomena of sound in regard to perturbing influences could not be properly studied without simultaneously observing the transmission of sound in opposite directions. It was therefore concluded to employ at least two steamers in making the investigations.

In regard to this point the commission was fortunate in being able to command the use, for a limited period, of the three tenders mentioned above, which happened to be at the time assembled at the light-house depot, Staten Island, and could be spared from their ordinary operations for a few days without detriment to the service. It was also fortunate in selecting for the scene of the investigations an unobstructed position in the lower bay of New York, and perhaps still more fortunate in the season of the year when on account of the heat of the sun—a land and sea breeze which changed their directions at a particular hour of the day, enabled results to be obtained bearing especially on the phenomena to be investigated.

Attention was first given to the character of the several steam-whistles which were intended to be used as the sources of the sound during the series of investigations.

These whistles, which were sounded during the whole of the observations with twenty pounds of steam on each boiler,

gave at first discordant sounds, and were found by their effect upon an artificial ear to be considerably different in penetrating power; they were then adjusted by increasing or diminishing the space between the bell and the lower cylinder, (by turning a screw intended for that purpose on the axis of the bell,) until they produced the same effect upon the sand in the membrane of the artificial ear; but in order to be further insured of the equality of the penetrating power of the several whistles, the three steamers abreast—forming as it were a platoon, were directed to proceed against the wind, sounding all the time in regular succession,—the *Cactus* first, then after an interval of a few seconds, the *Mistletoe*, and then the *Putnam*,—until the stationary observers lost the sound of each. They became inaudible all very nearly at the same moment. The sound of the *Putnam* was thought to be slightly less distinct; it was therefore chosen as a stationary vessel, from which the observations of the sound of the other two were to be made.

The *Putnam* being anchored at the point before mentioned, arrangements were made for sending off the other two vessels in opposite directions, one with and the other against the wind, with instructions to return when the sound became inaudible to those on the stationary vessel, this to be indicated by a flag-signal. It should be mentioned that the velocity of the wind was measured from time to time during the subsequent experiments with one of Robinson's hemispherical cup anemometers, made by Casella, of London. The velocity of the wind as observed by this instrument just before the starting of the vessels, was 6 miles per hour, the instrument being freely exposed on the paddle-boxes of the steamer. A sensitive aneroid barometer marked 30·395 ins. and continued to rise gradually during the day to 30·43 ins.; the temperature was 71° F.

1st trial.—The vessels left at 11:18 A. M., the wind being from the west, Captain Davis taking charge of the sounding of the whistle on the *Cactus*, which proceeded east with the wind, the sound coming to the ear of the observer against the wind; while the sounding on the *Mistletoe* was in charge of Gen-

eral Woodruff, and as the vessel steamed against the wind, the sound came to the observers on the stationary vessel with the wind; the other members of the party remained on the *Putnam*, at anchor at the point before mentioned, off the Hook, Major Hains having charge of the signals. The sound of the first of the vessels was heard faintly at 14 minutes after leaving, but not heard at 16 minutes; we may therefore assume that it became inaudible at 15 minutes. And within a minute of the same time,—by a mistake of the signal, the other ceased to advance, and commenced to come back; the sound from it however was very distinct, while at the same moment the sound from the other was inaudible. On account of the mistake mentioned, the relative distance at which the sounds from the two vessels might have become inaudible cannot be accurately given; but the fact observed, that the sound which came with the wind was much more audible than the other, is in conformity with the generally observed fact that sound is heard farther with the wind than against it. In the meantime the velocity of the wind had sunk to $1\frac{1}{2}$ miles per hour.

2*d trial.*—Next the vessels leaving at 11:55 A. M. changed positions; the *Cactus*, under Captain Davis, steamed west, directly in the direction from which the wind came, while the *Mistletoe*, under General Woodruff, steamed east, directly before the wind. The result of this trial was well marked in all respects; the sound of the *Mistletoe* was lost in 9 minutes, which, from the speed of the steamer, was estimated at about $1\frac{1}{2}$ miles, while the sound of the *Cactus* was heard distinctly for 30 minutes, or at an estimated distance of 5 miles. The wind at the middle of this trial had sunk to 0·42 mile per hour, or nearly to a calm. The result of this trial was somewhat abnormal, for though the wind had sunk nearly to a calm, the sound was still heard three times as far in the direction of the slight wind as against it.

3*d trial.*—After a lapse of an hour and a half a third trial was made; the wind had changed within two points of an exactly opposite direction, blowing (from the indications of the anemometer) at the rate of $10\frac{1}{2}$ miles per hour.

The *Cactus* again steamed in the eye of the wind, which was now however from nearly an opposite point, while the other vessel steamed in an opposite direction. The sound of the *Cactus* was lost (with the wind) at the end of twenty-seven minutes, or at a distance of four and a half miles.

The sound of the *Mistletoe* (moving against a brisk wind then blowing) was lost at the end of thirty minutes, or at a distance of five miles.

This result was entirely unexpected and much surprised every member of the party, since it was confidently expected that an increase in the intensity of the wind of more than ten miles per hour, and a change to the opposite direction, would materially affect the audibility of the sound, and give a large result in favor of the sound that moved in the same direction with the wind; but this was not the case. In the course of all the observations in several years in which investigations have been carried on under the direction of the chairman of the board, this is the only instance in which he had heard a sound at a greater distance against the wind than with it; although (as before stated) a number of cases have been reported by other observers in which (under peculiar conditions of the weather) this phenomenon has been observed.

To briefly recapitulate the results, we have in this case three instances in succession in which a sound was heard farther from the west than from the east, although in the meantime the wind had changed to nearly an opposite direction. Had these results been deduced from the first observations made on the influence of wind on sound, or in other words without previous experience, the conclusion would have been definitely reached that something else than wind affected the conveyance of sound, and this conclusion would have been correct if the suggestion had been confined to the wind at the surface; but from previous observations and theoretical conclusions, the observed phenomena are readily accounted for by supposing that during the whole time of observation the wind was blowing from the west in the higher part of the aerial current, and that the calm and

opposing wind observed were confined to the region near the surface. An unsuccessful attempt to test this hypothesis, was made by means of a balloon of tissue-paper, constructed by Major Hains, but which was unfortunately burned in the attempt to inflate it with heated air.

The remainder of this day was devoted to observations on the sound of the siren at the light-house at Sandy Hook. For this purpose the *Cactus*, under Captain Davis, was directed to steam in the eye of the wind, while the *Mistletoe*, under General Woodruff, steamed before the wind, and the *Putnam* steamed at right angles to the wind. Unfortunately, on account of the diminution of light at the closing in of the day, nothing could be observed. The only result obtained was that one of the duplicate sirens was heard more distinctly than the other, namely the one with the higher note.

Experiments September 24, 1874.—The place chosen for the observations of this day was still farther out on the ocean, at the Sandy Hook light-vessel, 6 miles from the nearest point of land. The pressure of the atmosphere was a little greater than the day before, being 30·52; the temperature about the same, 72° Fahr., wind light, from a westerly direction, as on the previous day, with a force (as indicated by the anemometer,) of 1·2 miles per hour. Having been provided with a number of India-rubber toy balloons, the two vessels were sent off in opposite directions,—leaving at 10:40 A. M., the *Mistletoe* toward the west, against the wind, the *Cactus* toward the east, with the wind. A change was also made in observing the sound. In these observations the sound was noted at each vessel from the other, the speed of the steamers being the same; the distance between them when the *Mistletoe* lost the sound of the *Cactus* was two miles, while the *Cactus* continued to hear the *Mistletoe's* sound (coming with the wind) until they were four miles apart. Simultaneously with this observation a balloon was let off from the *Putnam* at the light-vessel, which in its ascent moved continuously obliquely upward in a line slightly curving toward the horizon, in the direction of the

wind at the surface, as far is it could be followed with the eye, indicating a wind in the same direction in the several strata through which it passed, but of a greater velocity in the upper strata.

Second trial.—The vessels now changed places, the *Cactus* steaming west, the *Mistletoe* east, the wind having entirely ceased at the surface of the earth. In this case the *Cactus* lost the sound of the *Mistletoe* when the vessels were two miles apart, while the *Mistletoe* continued to hear the sound of the *Cactus* until they were three miles apart. A balloon let off ascended vertically until it attained an elevation of about one thousand feet, when turning east it followed the direction of the previous one. The sound in this case from the east was heard three miles, and that from the west was heard two miles, while in the preceding observations the distances were as 2 to 1; the only changing element (as far as could be observed) was that of the wind at the surface, which had somewhat diminished.

Third trial 12:45 P. M.—The wind previous to this trial had changed its direction 10 points or about 112½° round through the south, and (as indicated by the anemometer) had a velocity of 4·8 miles per hour. In this case the *Cactus*, going against the wind, lost the *Mistletoe's* sound coming to her against the wind, when the vessels were 1 mile apart, while the *Mistletoe* heard the *Cactus*, the sound coming to her with the wind, when the vessels were 1⅞ miles apart. The several balloons set off at this time were carried by the surface wind westwardly until nearly lost to sight, when they were observed to turn east, following the direction of the wind that prevailed below in the earlier observations. The results of the whole series of observations are extremely interesting. In all the experiments the difference in the audibility of the sound in different directions was very marked, and indeed it rarely happens that the sound is equal in two directions, although from the hypothesis adopted, this may be possible, since according to it both the upper and lower currents have an influence upon the audibility of sound in certain directions.

In the first trial, (of September 23,) the motion of the air being in the same direction both below and above, but probably more rapid above than below on account of resistance, the upper part of the sound-wave would move more rapidly than the lower, and the wave would be deflected downward, and therefore the sound (as usual) heard farther with the wind than against it. In the third experiment of the same day, in which the wind changed to an almost opposite direction, if the wind remained the same above,—as we have reason to suppose it did from the observations on the balloons on the second day, the sound should be heard still farther in the same direction—or against the wind at the surface, since in this case the sound-wave being more retarded near the surface would be tipped over more above and the sound thus be thrown down.

The observations of the next day (Sept. 24) are also in conformity with the same hypothesis, the change in the wind being probably due to the heating of the land—as the day advanced, beyond the temperature of the water, and thus producing a current from the latter to the former, while the wind observed in the morning from the west was the land-wind due to the cooling of the latter.

In the morning the wind was blowing from the west both in the higher strata and at the surface of the earth, and in this condition the sound was heard farther with the wind than against it.

The wind at the surface about mid-day gradually ceased, and shortly afterward sprang up from an easterly direction; in this condition the sound, (with the wind at the surface) was heard at a greater distance. This is also in strict conformity with the theory of a change in the form of the sound-wave, as in the latter case, the lower portion would be retarded, while the upper portion of the wave would be carried forward with the same velocity, and hence the sound would be thrown down on the ear of the observer. To explain the result of the third trial of the second day, we have only to suppose that the influence of the upper current was less than that of the lower. The conditions for these observations

were unusually favorable, the weather continuing the same during the two days, and the change of the wind also taking place at nearly the same hour.

The fact thus established seems entirely incompatible with the supposition that the diminution in the sound is principally caused by a want of homogeneity in the constitution of the atmosphere, since this would operate to absorb sound equally in both directions.

In May, 1873, Professor Tyndall commenced a series of investigations on the subject of the transmission of sound, under the auspices of the Trinity House, of England, in which whistles, trumpets, guns, and a siren were used; the last-named instrument having been lent by the Light-House Board of the United States to the Trinity House for the purpose of the experiments in question. The results of these investigations were in most respects similar to those which we had previously obtained. In regard to the efficiency of the instruments, the same order was determined which has been given in this report, namely the siren, the trumpet, and the whistle. Professor Tyndall's opinion as to the efficiency of the siren may be gathered from the following remarks. Speaking of the obstruction of sound in its application as a fog-signal, he says, "There is but one solution of this difficulty, which is to make the source of sound so powerful as to be able to endure loss and still retain sufficient residue for transmission. Of all the instruments hitherto examined by us the siren comes nearest to the fulfillment of this condition, and its establishment on our coasts will in my opinion prove an incalculable boon to the mariner." Professor Tyndall arrived at the conclusions which the information we had collected tended to establish, that the existence of fog however dense does not materially interfere with the propagation of sound, and also that sound is generally heard farther with the wind than against it, although the variation of the intensity of the sound is not in all cases in proportion to the velocity of the wind. The result of his investigations in regard to the pitch of sound was also similar to those we have given; and indeed all the facts which

he has stated are (with a single exception as to the direction of the echo) in strict accordance with what we have repeatedly observed. We regret to say however, that we cannot subscribe to the conclusions which he draws from his experiments as to the cause of the retardation of sound, that it is due to a flocculent condition of the atmosphere, caused by the intermingling with it of invisible aqueous vapor.

That a flocculent condition of the atmosphere, due to the varying density produced by the mingling of aqueous vapor, is a true cause of obstruction in the transmission of sound is a fact borne out by deduction from the principles of wave-motion, as well as by the experiments of the distinguished physicist of the Royal Institution of Great Britain; but from all the observations we have made on this subject, we are far from thinking that this is the efficient cause of the phenomena under consideration. A fatal objection we think to the truth of the hypothesis Professor Tyndall has advanced, is that the obstruction to the sound—whatever may be its nature, is not the same in different directions. We think we are warranted in asserting that in the cases of acoustic opacity which he has described, if he had simultaneously made observations in an opposite direction, he would have come to a different conclusion. That a flocculent condition of the atmosphere should slightly obstruct the sound is not difficult to conceive; but that it should obstruct the ray in one direction and not in an opposite, or in a greater degree in one direction than in another, the stratum of air being the same in both cases, is at variance with any fact in nature with which we are acquainted. We would hesitate to speak so decidedly against the conclusions of Professor Tyndall,—for whose clearness of conception of physical principles, skill in manipulation, and power of logical deduction, we entertain the highest appreciation, were the facts which have been obtained in our investigations of a less explicit character.

While the phenomena in question are incompatible with the assumption of a flocculent atmosphere as a cause, they are in strict accordance with the hypothesis of the refraction

of the waves of sound—due to a difference in velocity in the upper and lower portions of the currents of air. We do not say however that the transmission of sound in the atmosphere is fully investigated, or that the abnormal phenomena which are said to have been observed in connection with fog-signal stations have been fully explained. So far from this, we freely admit that we are as yet in ignorance as to how the hypothesis we have adopted is applicable to the critical explanation of the obstruction to sound in the abnormal cases mentioned by General Duane. We feel however considerable confidence in its power to afford a rational explanation of these phenomena when the conditions under which they exist shall have been accurately determined.

We are further confirmed in our conclusion by the publication of an interesting paper in the Proceedings of the Royal Society, by Professor Osborne Reynolds, of Owens College, Manchester, intended to show that sound is not absorbed by the condition of the atmosphere, but refracted in a manner analogous to the hypothesis which has been adopted in the preceding report.

Much further investigation is required to enable us to fully understand the effects of winds on the obstruction of sound, and to determine the measure of the effect of variations of density in the air due to inequality of heat and moisture. But such investigations can be made only under peculiar conditions of weather and in favorable localities, with the aid of a number of steamers, and a series of observers, by whom the transmissibility of sound may be simultaneously observed in different directions. The position which we were so fortunate to obtain in our experiments in the lower bay of New York at the season of the prevalence of land and sea breezes was exceptionally favorable for the study of the action of wind upon sound. It is the intention of the Light-House Board to continue observations in regard to this matter, and to embrace every favorable opportunity for their prosecution under new and varied conditions.

PART IV.—INVESTIGATIONS IN 1875.

(Report of the United States Light-House Board for 1875, pp. 104–126.)

Preliminary Remarks.—In the appendix to the Light-House Report of 1874 I gave an account of a series of investigations relative to fog-signals which had been made at different times under the direction of the chairman of the committee on experiments.

These investigations were not confined to the instruments for producing sound, but included a series of observations on sound itself in its application to the uses of the mariner. In the course of these investigations the following conclusions were early arrived at:

1st. That the rays of a beam of loud sound do not (like those of light) move parallel to each other from the surface of a concave reflector, but constantly diverge laterally on all sides; and although at first they are more intense in the axis of the reflector they finally spread out so as to encompass the whole horizon, thus rendering the use of reflectors to enforce sound—of little value in fog-signals.

2d. That the effect of wind in increasing or diminishing sound is not confined to currents of air at the surface of the earth, but that those of higher strata are also efficient in varying its transmission.

3d. That although sound is generally heard farther with the wind than against it, yet in some instances the reverse is remarkably the case, especially in one locality, in which the sound is frequently heard against a north-east snow-storm more distinctly than when the wind is in an opposite direction. This anomaly was referred to the action of an upper current in an opposite direction to that at the earth, such a current being known to exist in the case of north-east storms on our coast. But in what manner the action of the wind increased or diminished the audibility of sound was a problem not solved. It could not be due, as might be thought at first sight, to the acceleration of the sonorous impulse by the addition of the velocity of the wind to that of sound, on the

one hand, nor to the retardation of the latter by the motion of the wind, on the other. The inadequacy of this explanation must be evident, when we reflect that sound moves at the rate of 750 miles an hour, and therefore a wind of $7\frac{1}{2}$ miles an hour would only increase its velocity one per cent.; whereas the actual increase in audibility produced by a wind of this intensity is in some instances several hundred per cent.

In this state of our knowledge, a suggestion of Professor Stokes, of Cambridge, England, which offered a plausible explanation of the action of the wind, became known to me, and was immediately adopted as a working hypothesis to direct investigations.

This suggestion—the importance of which appears to have escaped general recognition, is founded on the fact that the several strata into which a current of air may be divided do not move with the same velocity. The lower stratum is retarded by friction against the earth and by the various obstacles it meets with, the one immediately above by friction against the lower, and so on; hence the velocity increases from the ground upward; a conclusion established by abundant observation. Now in perfectly still air, a sounding instrument—such as a bell, produces a series of concentric waves perfectly spherical; but in air in motion the difference of velocity above and below disturbs the spherical form of the sound-wave, giving it somewhat the character of an oblique ellipsoid, by tending to flatten it above to the windward, and to increase its convexity above to the leeward; and since the direction of the sound is perpendicular to the sound-wave, when moving against the wind it will be thrown upward above the head of the observer, and in the opposite direction downward toward the earth. A similar effect will be produced, but with some variations and perhaps greater intensity, by a wind above opposite to that at the surface of the earth.

These propositions will be rendered plain by the following illustrations (Figures 1, 2, and 3), for which I am indebted to an article just published in the American Journal of Sci-

ence, by Mr. William B. Taylor, on "Recent Researches in Sound," as the present report is passing through the press.

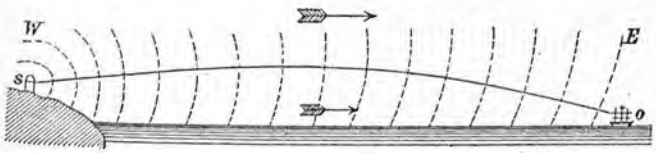


FIG. 1.—Favoring wind.

In these cuts, Figure 1 represents the effect of a favorable wind in depressing the waves of sound; *s* being the signal-station and *o* the point of observation. The wind blowing from *W* to *E*, as the spheroidal faces of the sonorous waves become more pressed forward by the greater velocity of the wind above, (assuming it to be retarded at the surface by friction,) and the direction of the acoustic beam being constantly normal to the wave-surfaces, the lines of direction of the sound will gradually be bent downward and reach the ear of the observer with an accumulated effect at the point *o;* being re-enforced by the lower sound-rays which are reflected from the surface of the water.

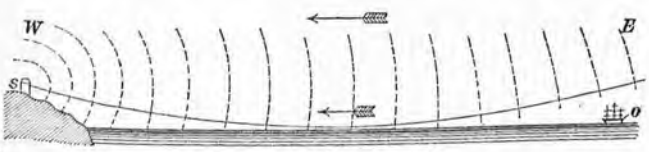


FIG. 2.—Adverse wind.

Figure 2 represents the ordinary effect of an opposing wind here blowing from *E* to *W* against the sound; the wave-faces being more resisted above than below by the swifter wind, (assuming as before a retardation at the surface,) the sound-beams are curved upward, and the lowest ray that in still air would reach the distant observer at *o*, is gradually so tilted up, that it passes above the ear of the listener, leaving him practically in an acoustic shadow; even after due allowance made for the divergence of the sound by dissipation downward.

Figure 3 represents the disturbing effect of two winds, the lower in opposition to the sound, and the upper with it.

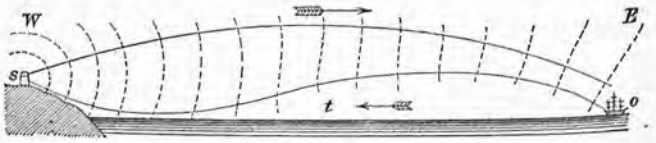


FIG. 3.—Compound wind.

In this case the principal effect will be a depression of the sound-beam, similar to that shown in Figure 1, but more strongly marked, as the difference of motion will be greater as we ascend. Attending this action, says Mr. Taylor, there will probably be some lagging of the lower stratum by reason of the surface-friction, the tendency of which will be to distort the lower part of the sound-waves, giving the lowest sound-beam a reverse or serpentine curvature. Such an effect is represented by the lower line *s t o*, (Figure 3,) the lower ray being at first turned up (by the adverse wind, somewhat as shown in Figure 2) and afterward thrown down by the dominant influence of the higher current of air, rendering the sound less audible at an intermediate point—*t*, than at the more distant station *o*. This hypothetical case of compound refraction offers a plausible explanation of the paradox of a nearer sound being diminished in power by the wind which increases the effect of a more distant one.

In these figures and all the succeeding ones, the direction of the wind is indicated by arrows.

The hypothesis we have adopted (in connection with the fact of the lateral spread of sound) gives a simple explanation of various abnormal phenomena of sound such as have been observed in the previous investigations, and of which the following are examples: First, the audibility of a sound at a distance, and its inaudibility nearer the source of sound; second, the inaudibility of a sound at a given distance in one direction, while a lesser sound is heard at the same distance in an opposite direction; third, the audibility of the sound of an instrument at one time at the distance of several

miles, while at another time the sound of the same instrument cannot be heard at more than a fifth of the same distance; fourth, the circumstance that while the sound is heard generally farther with the wind than against it, in some instances the reverse is the case; fifth, the sudden loss of sound in passing from one locality to another in the same vicinity, the distance from the source of the sound being the same.

The first four of these phenomena find a ready explanation in the hypothesis adopted, by supposing an increase or diminution in the relative velocity of the currents of wind in the upper and lower strata of air. The fifth is explained —either by an irregular twisting of the sound-beam, (as above suggested,) or by the inter-position of an obstacle which casts a sound-shadow—disappearing at a given distance by the convergence of the rays on each side of the obstacle into what would be an optical shadow.

Accounts of these investigations were presented from time to time to the Light-House Board and to the Philosophical Society of Washington in 1872. Subsequently a series of investigations on the same subject was instituted in England by the Elder Brethren of the Trinity House under the direction of their scientific adviser, the eminent physicist, Dr. Tyndall. While in the latter investigations various abnormal phenomena similar in most instances to those we have mentioned were observed, they were referred by Dr. Tyndall to an entirely different cause, viz, to the existence of acoustic clouds, consisting of portions of the atmosphere in a flocculent or mottled condition, due to the unequal distribution of heat and moisture, which absorbing and reflecting the sound, produce an atmosphere of acoustic opacity. While we do not deny the possible existence of such a condition of the atmosphere, we think it insufficient to account for all the phenomena in question, and believe that a more general and efficient cause is that of the *wind*, in accordance with the hypothesis of Professor Stokes.

We regret to differ in opinion from Dr. Tyndall, and have published our dissent from his views in no spirit of captious

criticism or desire to under-value the results he has obtained, some of which are highly important. Our only object in our remarks and in our investigations is the establishment of truth.

The determination of the question as to the cause of the abnormal phenomena of sound we have mentioned, and the discovery of new phenomena—are matters not merely of abstract scientific interest, but of great practical importance, involving the security of life and property, since they include the knowledge necessary to the proper placing of fog-signals and the instruction of mariners in the manner of using them.

The hypothesis we have adopted,—that of the change of direction of sound by the unequal action of the wind upon the sound-waves, is founded on well-established mechanical principles, and offers a ready explanation of facts otherwise inexplicable. It is also a fruitful source from which to deduce new consequences to be verified or dis-proved by direct experiment. It would however ill become the spirit of true science to assert that this hypothesis is sufficient to explain all the facts which may be discovered in regard to sound in its application to fog-signals, or to rest satisfied with the idea that no other expression of a general principle is necessary. An investigation however to be fruitful in results must as a general rule be guided by *a priori* conceptions. Hap-hazard experiments and observations may lead to the discovery of isolated facts, but rarely to the establishment of scientific principles. There is danger however in the use of hypotheses, particularly by those inexperienced in scientific investigations, that the value of certain results may be over-estimated, while to others is assigned less weight than really belongs to them. This tendency must be guarded against. The condition of the experiment must be faithfully narrated, and a scrupulously truthful account of the results given. While we have used the above-mentioned hypothesis in the following investigations as something more than an antecedent probability, we have not excluded observations which may militate against it, and we hold

ourselves ready to admit the application of other principles, or to modify our conception of those we have adopted when new facts are discovered which warrant such changes. But we require positive evidence, and cannot adopt any conclusions which we think are not based upon a logical correlation of facts.

The investigations described in the following account—though simple in their conception, have been difficult and laborious in their execution. To be of the greatest practical value they were required to be made on the ocean, under the conditions in which the results are to be applied to the use of the mariner, and therefore they could only be conducted by means of steam-vessels of sufficient power to withstand the force of rough seas, and at times when these vessels could be spared from other duty. They also required a number of intelligent assistants skilled in observation and faithful in recording results.

Observations in August, 1875, at Block Island.

The party engaged in these investigations consisted of the chairman of the Light-House Board; General I. C. Woodruff, U. S. A., engineer third light-house district; Dr. James C. Welling, president of the Columbian University, Washington, D. C.; Mr. T. Brown, of New York, patentee of the siren; Mr. Edward Woodruff, assistant superintendent of construction; and Captain Keeney, commander of the light-house steamer *Mistletoe*.

We arrived at Block Island on the afternoon of the 4th of August, 1875. This place was chosen as the site of the experiments,—first, on account of its insular position, being as it were in the prolongation of the axis of Long Island, distant fifteen miles from the most easterly part of the latter, and entirely exposed to the winds and waves of the Atlantic Ocean; and secondly, because there are on Block Island two light-houses, one of which is of the first order, and connected with it are two fog-signals, one of them with the latest improvements.

Observations in regard to the Aerial Echo.—This phenomenon has been frequently observed in the researches of the Light-House Board, in case of powerful sounds from the siren and from the fog-trumpet.* It consists of a distinct reflection of sound as if from a point near the horizon in the prolongation of the axis of the trumpet. The question of the origin of this echo has an important bearing—according to Dr. Tyndall, on the explanation of the abnormal phenomena of sound we have mentioned. He refers it to the non-homogeneous condition of portions of the air which reflect back the waves of sound in accordance with the analogy of the reflection of light at the common surface of two media of different densities. We have adopted the provisional hypothesis that it is due to the reflection from the waves and the larger undulations of the surface of the ocean in connection with the divergency of beams of powerful sounds. To bring these hypotheses to the test of a crucial experiment, arrangements were made under the direction of Mr. Brown to change the direction of the axis of one of the sirens from the horizontal to the vertical position.

August 5, 1875.—The first observations were made August 5, with the siren in its usual horizontal position, while the air was so charged with fog as to render the sound of the instrument necessary for the guidance of the mariner, the image of the sun being obscured and the land invisible from the sea. Under these conditions an echo was heard when the pressure of the steam reached 50 pounds per square inch. The reflection in this case (as usual) was from a point in the sea-horizon in the prolongation of the axis of the trumpet. It was not however heard more distinctly when standing near the origin of the sound than

*The same phenomenon is mentioned by Froissart in his account of the embarkation of the expedition of the French and English to the coast of Africa to assist the Genoese against the pirates in 1390. "It was a beautiful sight," says the chronicler, "to view this fleet, with the emblazoned banners of the different lords fluttering in the wind, and to hear the minstrels and other musicians sounding their pipes, clarions, and trumpets, whose sounds were re-echoed back by the sea." (See Illustrations of Froissart by H. N. Humphrey, Plate IV.)

at several hundred feet on either side of it. The interval between the cessation of the original sound and the commencement of the echo was not as marked as in some previous observations, not being more than four or five seconds. The duration of the echo was on the average about eight seconds,—beginning with the time of its first perception and not with the cessation of the sound of the trumpet. General Woodruff and Dr. Welling both noted the peculiar character of the echo, which was that of a series of reflections varying in intensity from a maximum, near the beginning, and gradually dying away. The wind was nearly at right angles to the axis of the trumpet and also to that of the crests of the swell of the ocean, which was rolling in from the effects of a distant commotion. The barometer at 12 M. indicated 30·2 inches; the dry-bulb thermometer 73° F., the wet-bulb 70° F., indicating a remarkable degree of aqueous saturation. During the whole day the air in all the region around Block Island was undoubtedly in a homogeneous condition.

August 6, 1875.—On this day the weather was nearly the same. The fog-signal on the 5th instant was kept in operation for the use of the mariner nineteen hours, and on this day it was blown twenty hours continuously. The barometer marked 30·2 inches; the thermometer 70° F.; the fog not as equally distributed as on the preceding day; the north end of the island (distant four miles) being distinctly visible. The wind was S. W. to S., making an angle of about 60° with the axis of the fog-trumpet. The echo continued to be heard distinctly with a sound varying in intensity, but was not so loud as we have heard it on certain occasions in previous years.

During this and the preceding day workmen were employed under Mr. Brown in inserting a flexible India-rubber tube two inches in diameter between the revolving plate of the siren and the smaller end of the trumpet, so that it might be brought into a vertical position. This work—though apparently simple, was difficult in execution, since it involved the necessity of strong supports for the cast-iron

trumpet, which itself weighed eight hundred pounds, and also a union of the parts of sufficient strength to resist the pressure of the steam at fifty pounds to the square inch.

August 7, 1875.—Wind from the S. S. W.: fog continued. The workmen had not as yet completed the attachment.

August 9, 1875.—Barometer 30·30 inches at 12 M. Dry-bulb thermometer 74° F.; wet bulb 71°·5. Wind S. S. W. Fog dense along the south coast, but light over all the northern portion of the island. The echo was heard all day, not very loud, but distinct. Siren still horizontal, the arrangement for elevating it not having been—at 10 A. M., completed. Experiments were made on the reciprocal sounds of the whistles from two steamers, the results to be given hereafter. At 5 P. M. the adjustment of the flexible tube to the smaller end of the trumpet was finished, which giving an additional length to the instrument of about 5 feet, threw it out of unison with the siren proper. To restore this unison the speed of revolution of the perforated plate was diminished, and after this the trumpet, still being horizontal, was sounded. An echo—similar in character to those which had been observed on the preceding day, and the earlier part of the same day, was produced.

August 10, 1875.—Barometer 30·1 inches. Dry bulb 74°; wet bulb 69° F. Wind W. S. W.; atmosphere hazy. Observations first made with the trumpet horizontal. Echo as that of preceding days, distinct but not very loud, and coming principally from the portion of the horizon in the direction of the axis of the trumpet. The position of the trumpet was then changed, its axis being turned to the zenith in order to make what was thought might be a crucial experiment. When the trumpet was now sounded a much louder echo was produced than that which was heard with the axis of the trumpet horizontal, and it appeared to encircle the whole horizon; but though special attention was directed to the point by all the party present, no reverberation was heard from the zenith. The echo appeared however to be more regular and prolonged from the ocean portion of the horizon than from that of the land.

In this experiment, while there was no reflection from the

zenith in which the sonorous impulse was strongest, there must have been reverberations from the surface of the land and the ocean. This will be evident when we consider the great divergency of sound by which sonorous waves from a vertical trumpet are thrown down to the plane of the horizon on every side, some of which meeting oblique surfaces must be reflected back to the ear of the observer near the source of the sound. This inference will be more evident when it is recollected that the reflected rays of sound diverge as well as those of the original impulse. Hence reflection from the surface of the sea is a true cause of the echo, but whether it be a sufficient one may require further investigation. For this explanation it is not necessary that the sea should be covered with crested waves; a similar effect would take place were the surface perfectly smooth but in the form of long swells, which in places exposed to an open sea are scarcely ever absent. Moreover the increased loudness of the echo is a fact in accordance with the same view.

The observations were repeated with the same effect on succeeding days, until this class of experiments was ended by the bursting of the India-rubber tube. Had a distinct echo been heard from the zenith the result would have been decidedly in favor of the hypothesis of a reflection from the air; but as this was not the case the question still remained undetermined, especially since the atmosphere during these experiments was evidently in a homogeneous condition. We do not agree however in the position taken in the report of the Trinity Board, that on the origin of this echo depends the whole solution of the problem as to the efficient cause of the abnormal phenomena of sound. The ingenious experimental illustrations of the reflection of sound from a flame or heated air establish clearly the possibility of such reflection; but it must be remembered that they were made under exaggerated conditions, the atmosphere being in a state of extreme rarefaction in a limited space, and the sound of a feeble character, while the phenomena in nature are produced with a comparatively small difference of temperature and with powerful sounds.

Experiments as to the Effect of Elevation on Audibility.—For this investigation the first-order light-house at Block Island offered peculiar facilities. It is situated near the edge of a perpendicular bluff 152 feet above the sea. The tower being 52 feet above the base—gives a total height (to the focal plane of the lens) of 204 feet, on the level of which the ear of the observer could be placed.

The first and second experiments of this class were made on the 10th of August, with two light-house steamers—the *Putnam* and the *Mistletoe*, moving simultaneously in opposite directions. The barometer indicated 30·1 inches of atmospheric pressure; the dry-bulb thermometer indicating 74° F., and the wet-bulb 69°. The wind at the time of the experiments was from the west, and of a velocity of seven miles per hour. The vessels started from the point *C*, Fig. 4, opposite the light-house, *A*, about one mile distant, a position as near the shore as it was considered safe to venture. The *Putnam* steamed with the wind, the *Mistletoe* steamed against the wind, each blowing its whistle every half minute. The duration of the sound was noted at the top of the tower and at the level of the sea, Mr. Brown being the observer at the latter station, while the chairman of the Board, with an assistant, observed at the former. On comparing notes, the watches having been previously set to the same time, the following results were found.

First experiment.—The duration of the sound on the tower when coming against the wind was nine minutes, while at the base of the cliff it was heard only one minute. It was afterward found from the records on board of the *Putnam*, the sound of which came against the wind, that this vessel was moving during the experiment at half speed, and hence the duration of the sound on the tower should be considered as 4½ minutes, and the difference in favor of audition on the tower 4 minutes instead of 8, as given by the first record.

Second experiment.—The sound of the *Mistletoe*, coming to the observers with the wind, was heard on the tower during 15 minutes, while it was heard at the base of the cliff during 34 minutes, the difference being 19 minutes in favor of hear-

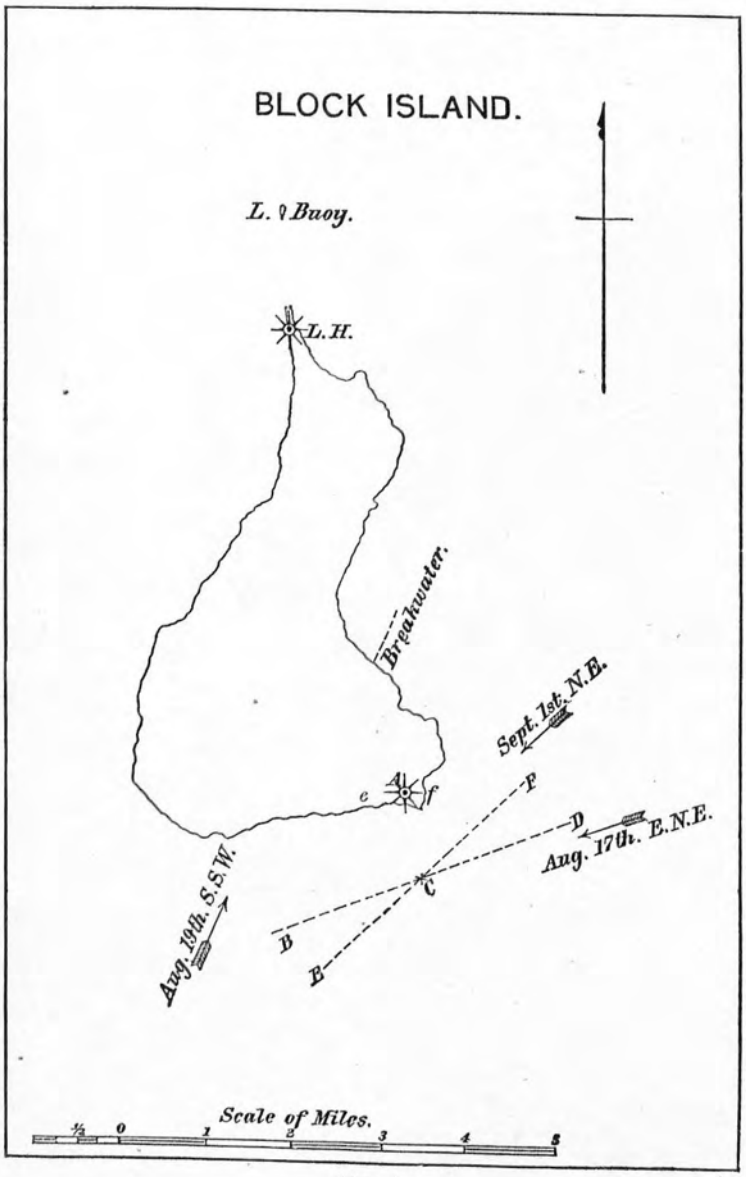


FIG. 4.

ing at the level of the sea. This result—which differs from that of all the other experiments of the same class, deserves special attention.

After making the foregoing experiments of this class, and others on the effect of wind on sound, to be described in the next section, the vessels were called off for other duty, and the investigations were not resumed until August 17, when the following experiments were made:

Third experiment.—The wind was from the E. N. E., at the rate of about five miles per hour at the surface, and a greater velocity at the height of the tower. Barometer, 30·25 ins.; thermometer, 72°.

In this and the subsequent experiments of the same day, but one steamer—the *Mistletoe*—was employed. It started at 10:30 A. M. from the point *C*, Fig. 4, at the foot of the cliff, and steamed W. S. W. along *C B* for about 12 minutes, or a distance of two miles, blowing the whistle every half-minute. To note the duration of the sound, Dr. Welling was stationed at the foot of the cliff, at the level of the sea, while the chairman of the Light-House Board, with an assistant who acted as clerk, was on the upper gallery of the tower, the ears of the latter being almost precisely 200 feet above those of the observer at the foot of the cliff.

The watches having been previously set to the same time, on comparing results it was found that the whistle was heard at the top of the tower for twelve minutes and at the bottom of the cliff for five and one-half minutes, making the difference in favor of audition on the tower six and one-half minutes. In this experiment the sound came to the observers nearly against the wind.

Fourth experiment.—This consisted in directing the *Mistletoe* to proceed in the opposite direction from the same point, along the line *C D*. It started at 11:5 A. M. the breeze being light at the time, and proceeded about two and one-half miles before the sound was lost to the observers. On comparing notes it was found that the sound was heard at the top of the tower during fifteen minutes, and at the level of the sea for eleven minutes, giving a difference in favor of the hearing on the top of the tower of four minutes.

Fifth experiment.—In this, the *Mistletoe* steamed again in the direction with the wind, the sound from its whistle coming

to the ears of the observers against the wind. Starting about 11:45 A. M. and steaming about two miles, the sound was heard on the tower during twelve minutes and at the foot of the cliff during five and one-half minutes, making a difference of six and a half minutes in favor of audition on the tower. Previous to this experiment the wind had veered one point to the west, bringing the direction of the sound to the observers in less direct opposition to the wind than in the last experiment.

Sixth experiment.—In this case the steamer was directed to proceed in the opposite direction, or against the wind, so that the sound of the whistle would reach the ear of the observers in the same direction as that of the wind. It started at 12:19 P. M. and proceeded two and one-sixth miles; the whistle was heard during thirteen minutes on the top of the tower, and at the bottom of the cliff during precisely the same time, the difference between the top of the tower and the bottom of the cliff in this case being nothing.

Seventh experiment.—The vessel having again been called off on other duty the next experiment was made the 1st of September. On this day the wind was north-east; the velocity at the top of the tower was thirteen and a half miles per hour, and at the bottom of the tower eleven miles per hour. The barometer indicated 30·2 inches pressure, the dry bulb 72°, and the wet bulb 67·5°.

The theoretical conditions for exhibiting the effect of height on audition in this experiment were much more favorable than any of the preceding. First, the velocity of the wind was greater; second, the difference between the velocities at top and bottom of the tower was well marked, and the direction of the wind was more favorable for direct opposition to the sound as it came to the ear of the observer. In this case, General Woodruff was the observer at the bottom of the cliff, while the chairman of the Light-House Board and his assistant, with several visitors, were at the top of the tower.

The steamer started at 10:58 A. M. and proceeded during eight minutes, or a mile and one-third, when the sound was

lost at the top of the tower. In this case, though the sound was heard for eight minutes at the top of the tower, and the first five blasts marked on the notes as quite loud, it was not heard at all at the bottom of the cliff, at least a hundred yards nearer the source of the sound.

This result, which interested and surprised a number of intelligent visitors, who were in the tower at the time, strikingly illustrates the effect of elevation on the audibility of sound moving against the wind. The result was so important that it was thought advisable to immediately repeat the experiment under the same conditions.

Eighth experiment.—The *Mistletoe* was again directed to proceed, in the direction of the wind, along the line it had previously traversed. It started at 11:25 A. M., and proceeded during six minutes, or one mile, when the sound was lost at the top of the tower. In this case, the first blast of the whistle was feebly heard at the base of the cliff, but no other, while thirteen blasts were heard at the top of the tower, of which the first six were marked as loud.

That this remarkable effect was not produced by an acoustic cloud or a flocculent atmosphere is evident from the experiment which immediately succeeded.

Ninth experiment.—In this trial, the *Mistletoe* was directed to proceed against the wind, so that the sound of its whistle should come to the ears of the observers with the wind. It started at 11:48 A. M., and proceeded during sixteen minutes, or two and two-thirds miles, when the sound of its whistle was lost to the observers on the top of the tower. In this case the sound of the whistle became audible at the bottom of the cliff as soon as the position of the vessel became such as to bring the sound to the observers approximately with the wind, and continued to be audible during fifteen minutes, or within one minute as long as the sound was heard at the top of the tower.

It may be mentioned as an interesting fact that an assistant who was observing the sound with General Woodruff at the foot of the cliff, when the sound could not be heard at the level of the sea (in the sixth experiment), perceived it

distinctly by ascending the side of the cliff to a height of twenty-five or thirty feet.

All the conditions and results of these experiments are strikingly in conformity with the theory of the refraction of sound which we have previously explained.

The following recapitulation of the results of the foregoing experiments will exhibit their correspondence with the general theory:

Sound heard coming against the wind.

Experiments.	Duration at the top of the tower.	Duration at the base of the cliff.	Difference in favor of audition on the tower.
First	4½ minutes	½ minute	4 minutes.
Third	12 minutes	5½ minutes	6½ minutes.
Fifth	12 minutes	5½ minutes	6½ minutes.
Seventh	8 minutes	Not heard	8 minutes.
Eighth	6 minutes	First blast heard, but no other, ½ minute after starting.	5½ minutes.
	42½ minutes	12 minutes.	30½ minutes.
Average	8·5 minutes	2·4 minutes	6·1 minutes.

Sound heard coming with the wind.

Experiments.	Duration at the top of the tower.	Duration at the base of the cliff.	Difference in favor of audition at base of cliff.
Second	15 minutes	34 minutes	19 minutes.
Fourth	15 minutes	11 minutes	— 4 minutes.
Sixth	13 minutes	13 minutes	0 minutes.
Ninth	16 minutes	15 minutes	— 1 minute.
	59 minutes	73 minutes	14 minutes.
Average	14¾ minutes	18¼ minutes	3½ minutes.

From the first of the foregoing tables it appears that the elevation of the observer has a marked effect on the audition of sound moving against the wind; while from the second, (with one important exception,) it has very little if any effect

on sound moving with the wind. Another experiment relative to the same class of phenomena was made on the 19th of August (see Fig. 4), the wind being S. S. W. Two observers—General Woodruff, and Dr. Welling, starting from the bottom of the cliff immediately below the light-house, went along the beach, the one in the direction *A f*, and the other in direction *A e*. General Woodruff found that the sound of the siren was distinctly heard all the way to the breakwater, and was so loud that it probably could have been heard for several miles in that direction. Dr. Welling—on the contrary, entirely lost the sound within a quarter of a mile of the light-house. This result is readily explained as a case of lateral refraction; the wind was in the direction traversed by General Woodruff, and contrary to that pursued by Dr. Welling. In the one case the wind—retarded by the surface of the cliff, moved with less velocity than it did farther out, and consequently the sound was thrown against the face of the cliff, and on the ear of the observer, and in the other thrown from it, thus leaving as it were a vacuum of sound. The effect in this case was very striking, since the siren was pointed toward the zenith, and the sound in still air could have been heard for miles in every direction.

Investigations as to the Effect of Wind on Audibility.—These observations were made by the aid of two steamers. Captain Walker, naval secretary of the board, having completed a series of inspections in the third district, sent the steamer *Putnam*, under Captain Fields, to aid the *Mistletoe* in the investigations. They were commenced on the 9th of August, at 12 o'clock. The wind was S. S. W. with a velocity of $7\frac{1}{4}$ miles per hour. Barometer, 30·3 inches; thermometer, dry bulb, 74° F.; wet bulb, 71·5° F.

The two steamers started from a buoy near the north end of the island, the one steaming against the wind, and the other with it, each blowing its whistle every minute. The distance travelled by each steamer was estimated by the running time, which from previous observations was found to be ten miles per hour. Each vessel was furnished with a whistle of the same size, of 6 inches diameter, actuated by

the same pressure of 20 pounds of steam, and which by previous comparison—had been found to give sound at this pressure of the same penetrating power. The observations on the *Mistletoe* were made by General Woodruff, and on the *Putnam* by Dr. Welling, each assisted by the officers of the respective vessels. The two steamers proceeded to the buoy, —off the north end of the island, in which position the wind was unobstructed by the land—a low beach. Indeed, the island being entirely destitute of trees, and consisting of a rolling surface, the wind had full sweep over it in every direction.

First experiment.—The *Putnam* went against the wind and the *Mistletoe* in the opposite direction. The *Putnam* lost the sound of the whistle of the *Mistletoe* in two minutes and stopped, but continued to blow the whistle. The *Mistletoe* continued on her course and heard the *Putnam's* whistle for twenty minutes in all. During the first two minutes both vessels were in motion, and therefore the space through which the sound was heard moving against the wind—would be represented by 4, while the space through which the sound was heard moving with the wind—would be represented by $20+2=(22)$, the ratio being $1:5\frac{1}{2}$.

Second experiment.—In this, the *Putnam* went with the wind and the *Mistletoe* in the opposite direction. The *Mistletoe* lost the sound of the *Putnam's* whistle in two minutes. The *Putnam* then stopped and remained at rest, while the *Mistletoe* continued on her course until the *Putnam* lost sound of her whistle, twenty-six minutes later. As both steamers were separating during the first two minutes with equal speed, the distance travelled by the sound heard moving against the wind is represented by 4, while the distance of the sound heard with the wind is represented by $26+2=28$, the ratio being 1:7. It should be mentioned however that the notes in this experiment are defective and somewhat discrepant.

Third experiment.—The *Putnam* went against the wind, the *Mistletoe* in the opposite direction. The *Putnam* lost the sound of the whistle of the *Mistletoe* in two minutes,

while the *Mistletoe* continued to hear the whistle of the *Putnam* ten minutes longer. Owing to a mis-understanding, one of the steamers stopped for two minutes and then resumed its course. As both steamers were separating during the first two minutes with equal speed, the distance of the sound heard moving against the wind is represented by 4, while the sound was heard with the wind through a space denoted by $2 \times 10 + 4 - 2 = 22$, the ratio being $1 : 5\frac{1}{2}$.

Fourth experiment.—The vessels again changed directions, the *Putnam* going with the wind and the *Mistletoe* in the opposite direction. The *Mistletoe* lost the sound in two minutes, and the *Putnam* nine minutes later. As each steamer was moving from the other at the same rate, the distance of the sound heard moving against the wind would be represented by 4, while the distance of the sound moving with the wind would be represented by $9 \times 2 + 4 = 22$, the ratio being again $1 : 5\frac{1}{2}$.

Fifth experiment.—This experiment was made August 10, by the same vessels and same observers: wind W. S. W., of about the same intensity as on previous days; barometer 30·1 ins.; dry bulb 74° F., wet bulb 69°. The *Putnam* steamed against the wind, and the *Mistletoe* in the opposite direction. The *Putnam* lost the sound in two minutes, and the *Mistletoe* nine minutes later. The two vessels moving apart with equal velocity, the space traversed by the sound moving against the wind was represented by 4, while that in the opposite direction was represented by 22 viz., $9 \times 2 + 4 = 22$.

Sixth experiment.—The vessels were next separated in a direction at right angles to the wind, when each lost the sound of the other on an average of six minutes, giving a distance travelled by the sound (while audible) of 12 spaces.

Seventh experiment.—The vessels were next directed along an intermediate course between the direction of the wind and a line at right angles to it with the following results: The *Mistletoe*, against the wind, lost the sound in about two minutes, while the *Putnam* heard the sound seven minutes longer. As in the previous case, the two vessels moving

apart with equal velocity would in two minutes be separated by a space represented by 4, which would indicate the audibility of the sound moving against the wind, and for the same reason the other vessel, hearing the sound seven minutes longer, would have the additional space represented by 14, and adding to this four spaces, we have 18 to represent the audibility of the sound in the direction approximating that of the wind.

The following table exhibits at one view the results of the foregoing experiments, which relate to sound moving against the wind and with the wind, reduced to miles:

Experiment.	Sound with the wind.	Sound against the wind.
	Miles.	*Miles.*
1	3·66	0·66
2	4·66	0·66
3	3·66	0·66
4	3·66	0·66
5	3·66	0·66

These results are in accordance with those of all the direct observations on the effect of wind on sound, which had previously been made by the Light-House Board, with the exception of those at Sandy Hook in September, 1874, as given in the last report, in which the sound was heard from a steamer farther against the wind than in the direction of the wind. This anomaly was explained by the existence of an upper current of air moving in an opposite direction to that at the surface, in accordance with the hypothesis of the refraction of sound.

It will be observed that four of the experiments give exactly the same distances to represent the audibility of sound with and against the wind. This co-incidence was not observed until after the notes were collated for discussion, and (if not accidental) was due to the equal velocity of the wind, and the general conditions of the atmosphere on the two days.

To give a definite idea of these relations, we have plotted

the results obtained on August 10, in Fig. 5, converting the distances into miles, referring them to a common centre, and tracing through the several extremities of the lines representing the distances—a continuous line, which may be designated as the curve of audibility. *C* being the centre to which the sounds are referred, *C A* represents the distance at which the sound was heard against the wind, and *C B*, in the direction of the wind, while *C E* and *C D* represent the distance at right angles to the wind, and *C F* and *C G* the distances respectively with and against the wind on an intermediate course.

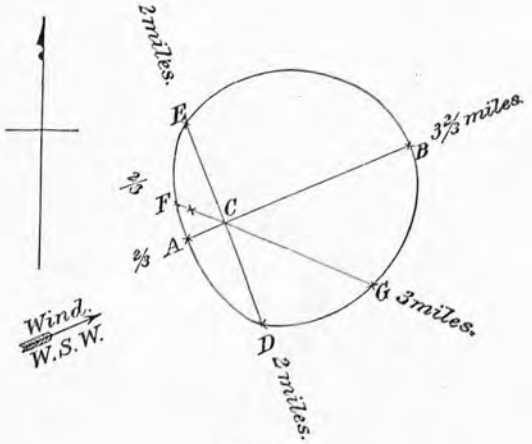


FIG. 5.

The curve which is presented in the foregoing figure may be considered as that which represents the normal limit of audibility during the two days in which the experiments were made. The line *D E* divides the plane of the curve into two unequal portions, *D A F E* and *D G B E*, the former representing the audibility of sound moving against the wind, and the other the audibility of sound moving with the wind.

We can scarcely think that any other condition of the air than that of its motion could produce a result of this kind. It exhibits clearly the fact that sound is not heard as a general rule at right angles to the wind farther than with the

wind, as has been asserted. In this case the ratio of the latter to the former is as 11 to 6, or nearly double.

The investigation of the relation of wind to the penetration of sound was renewed in a series of subsequent experiments, the results of which are to be given in a succeeding part of this report.

It should be observed, in comparing Fig. 5 with the subsequent figures representing the curve of audibility, that the arrow representing the direction of the wind points in the longest direction to the figure, whereas in other figures the pointing is in the opposite direction. The difference arises from the fact that in Fig. 5 the sound is supposed to radiate from the centre, *C*, while in the others the sound converges to the centre as a point of observation. The foregoing diagram and all that follow in this report were plotted by Mr. Edward Woodruff, assistant superintendent of construction of the third light-house district.

Experiments at Little Gull Island, September, 1875.

The next series of experiments made during this season was at Little Gull Island, at the east end of Long Island Sound. This location was chosen on account of its convenience of approach from the harbor of New London, seven miles distant, at which the light-house steamers of the third district usually remain when not engaged in active service, and also because there is a light-house on the island furnished with two sirens of the second order, and an extent of water on every side which would allow the vessels used in the experiments to proceed from the island as a centre to a considerable distance in every direction. The island itself is a small protuberance above the water, merely sufficient in area to support a raised circular platform of about 100 feet in diameter, on which the light-house and other buildings are erected. The following sketch (Fig. 6) will give an idea of the position of Little Gull Island relative to the mainland and the islands in the vicinity.

From this it will be seen that the position was not the most favorable for a stable condition of the atmosphere. As

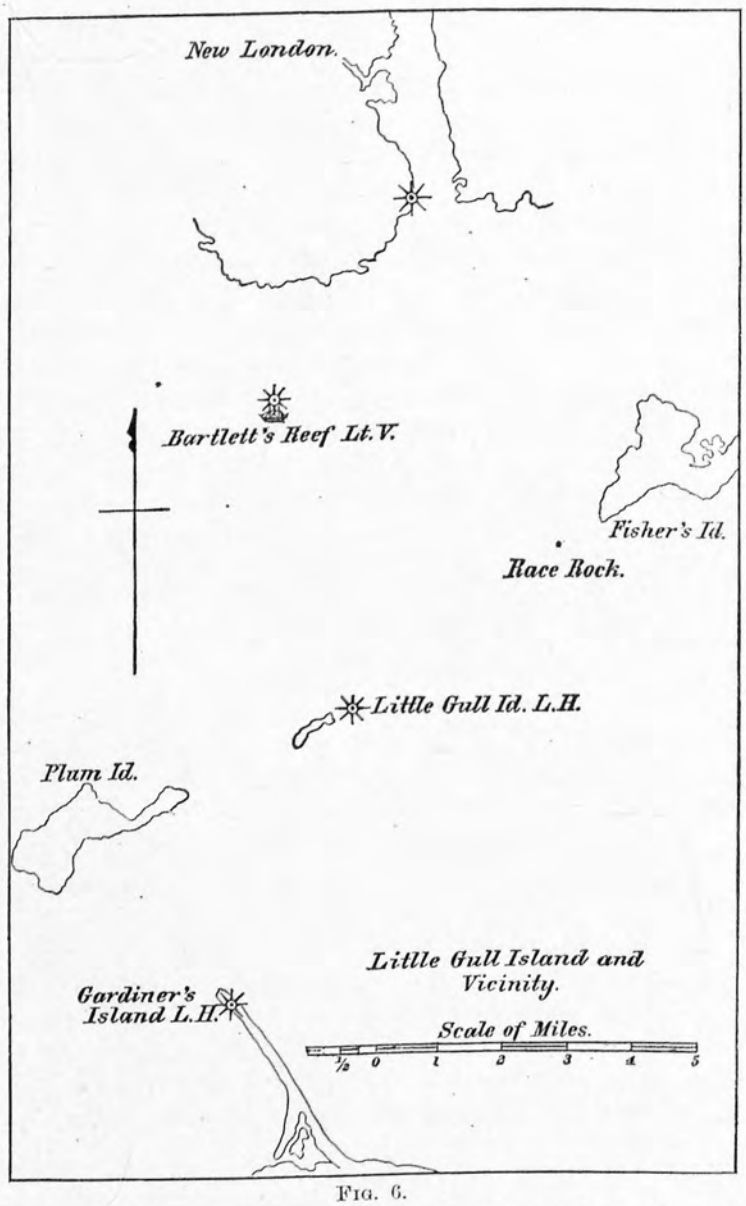


FIG. 6.

the heat of the sun increases during the first part of the day, the temperature of the land rises above that of the sea; and this excess of temperature produces upward currents of air,

disturbing the general flow of wind both at the surface of the sea and at an elevation above. But although the locality was unfavorable for obtaining results tending to exhibit the effects of broad currents of wind flowing in one direction it had the advantage of offering more varied phenomena than could otherwise have been exhibited. Before commencing the experiments, instructions were given to attach a rotating iron neck to the trumpet of one of the sirens, in order that it might be directed to the zenith, while the other siren remained with its axis in a horizontal direction. The observers in these investigations consisted of the chairman of the Board; General Woodruff, engineer of the third district; Mr. Porter Barnard, assistant superintendent of construction; Captain Keeney, and other officers of the *Mistletoe;* with an assistant who acted as one of the observers and recording clerk. The *Mistletoe* was daily employed, though on two occasions the *Cactus*—another of the light-house steamers, rendered assistance.

Observations on the Echo.—The first observations to be mentioned are those relating to the echo; the results however in regard to this are not very satisfactory. The sirens were of the second order, and therefore the echoes produced were not so distinct as those from the larger instrument at Block Island. The echo from the horizontal trumpet was distinct, and in the prolongation of its axis; the interval however between the blast of the siren trumpet and the commencement of the echo was very brief; so short indeed that the ending of the one and the beginning of the other were generally difficult to distinguish. A slight break in the apparatus of the siren produced a continuous hum, which interfered somewhat with the distinct appreciation of the sound of the echo. The keeper thought the weather was not favorable for the production of echoes. He thinks they are heard most distinctly during a perfect calm, which did not occur during the course of these investigations.

The axis of the siren with the movable trumpet being directed to the zenith, strict attention was given by all the observers to any echo which might be produced from it; but in this case, as in that at Block Island, the slight echo

which was heard came from all points of the horizon. On one occasion General Woodruff called attention to a small cloud passing directly over the zenith, from which a few drops of rain fell upon the platform on which the light-house is erected. Advantage was taken of this occurrence to direct strong blasts of the siren toward the cloud, but no perceptible echo was returned. We have failed therefore in this series of investigations to obtain any postive facts in addition to those already known as to the character of the echo. In regard to the hypothesis offered for its explanation, if we found little in its support, we have met with nothing to invalidate it. But whatever may be the cause of the phenomenon, we do not consider it an important factor in explanation of the results we have obtained, since it was too feeble to produce any effect in the way of absorbing any notable part of the original sound. Its importance from Dr. Tyndall's point of view is its apparent support of the hypothesis of a flocculent condition of the atmosphere.

Observations on Effect of Elevation on Audibility.—The next class of experiments at Little Gull Island had relation to the effect of elevation on sound. The conditions here however for arriving at definite results on this point were by no means so favorable as those at Block Island. The height which could be commanded was only that of the tower of the light-house, the gallery of which is 74 feet above the platform upon which the buildings are erected, and 92 feet above the level of the sea,—much less than that at Block Island. Besides this, the variableness of the wind at the surface of the ocean and at heights above was not favorable for the illustration of the point in question.

The theoretical conditions in order that the sound may be heard with greater distinctness at an elevation than below, are (as we have said before,) that the wind be moving with a greater velocity in a given direction at an elevation than at the surface of the earth, and that the difference in the velocities may be against the sound-wave, so that its upper part may be more retarded than the lower. In this case the direction of a beam of sound will be curved upward, leaving as it were a vacuum of sound beneath. The distance of the

origin of sound however must not be too great relatively to the elevation of the observer; otherwise it will pass over his head, as well as over that of the observer at the surface of the earth. In most instances the sound was not continuous, but was interrupted,—heard for a time, then lost; again becoming audible, it was heard until finally lost. Besides this, it was difficult to determine when the sound ceased to be heard, since this depended on the sensibility of the ear and the greater or less attention of the observer at the time of the observation. To obviate these difficulties, as well as the unfavorable condition of too great a distance of the origin of sound from the observer, it was concluded to adopt as the duration of the sound the elapsed time between its beginning and the period when it was first lost.

The observer on the tower was Mr. P. Barnard, while the one below was General Woodruff. From the records of the observations of these gentlemen the following tables are compiled, the first of which indicates the relative duration of sound on the top of the tower and at the bottom,—the sound moving against the wind; the second the same duration, the sound moving with the wind; and the third, the same with the sound at right angles to it.

TABLE 1.—*Sound against the wind.*

Date.	Heard at top of tower.	Heard at foot of tower.
1875.	*min.* *sec.*	*min.* *sec.*
September 2	5 30	4 00
4	4 30	3 30
4	5 30	3 00
4	5 00	4 00
6	7 00	2 15
6	4 00	3 00
7	5 00	2 15
8	6 00	4 00
8	5 30	3 45
8	3 30	2 15
8	3 00	1 15
Mean	4 57	3 01

It appears from Table 1 that without a single exception the duration of the sound was greater at the top of the tower than at the bottom, although the difference in favor of the top of the tower in the several experiments is very variable. These results are in accordance with what was anticipated.

TABLE 2.—*Sound with the wind.*

Date.	Heard at top of tower.	Heard at foot of tower.
1875.	*min.* *sec.*	*min.* *sec.*
September 2	30 00	30 00
3	16 30	18 00
4	21 00	20 30
4	18 00	23 30
6	12 30	12 30
7	6 30	5 30
Mean	17 25	18 20

TABLE 3.—*Sound nearly at right angles to the wind.*

Date.	Heard at top of tower.	Heard at foot of tower.
1875.	*min.* *sec.*	*min.* *sec.*
September 2	6 00	4 00
2	6 45	10 00
2	25 00	23 00
2	16 30	4 00
3	21 00	19 15
3	16 00	14 30
3	23 30	16 45
4	19 30	17 30
6	6 30	5 30
7	5 00	6 45
7	12 00	12 30
8	4 15	3 15
8	9 30	5 00
Mean	13 12	11 00

In the observations recorded in Table 2 the durations of the sound at the bottom and top of the tower are nearly the same, from which we might infer that the elevation of the observer has little effect on the hearing of sound moving with

the wind. Were it not for the result of the first experiment of this class at Block Island, we should not hesitate to adopt this as a general conclusion.

From the general mean of observations given in Table 3, it would appear that the sound moving at right angles to the wind can be heard better at an elevation than at the surface,—a result not anticipated.

Observations on Effect of Wind on Sound.—This series was commenced on the 2d of September. Barometer, 30·3 inches; thermometer, dry bulb, 70·5° F., wet bulb, 67·5°; wind at the surface of sea six miles per hour, and variable: at 3 P. M. the velocity was eight miles at the surface. (See Fig. 7.)

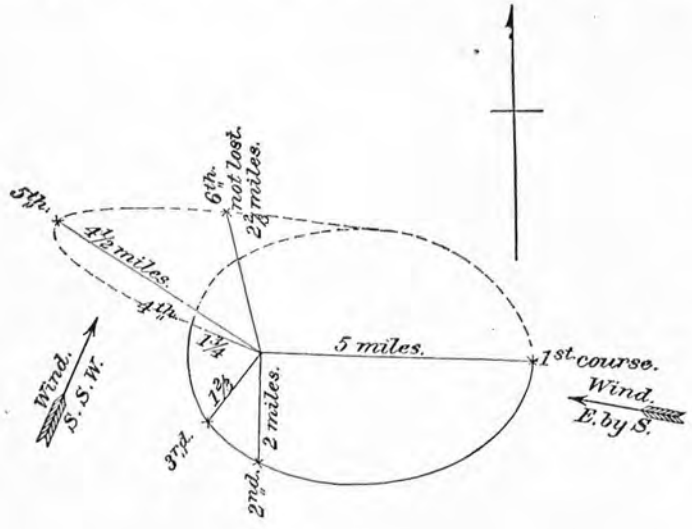


FIG. 7.

The experiments were made by means of the steamer *Mistletoe*, which proceeded from the light-house, as a centre, in different directions, blowing the whistle every half-minute, and returning at a signal, when the sound was lost; the time being noted by different observers, and the distance estimated by the position of the steamer in reference to known objects on the Coast Survey chart, as well as by

angles of azimuth and time of sailing. The steamer was directed to proceed, as indicated in Fig. 7, 1st, against the wind, so that the sound would come to the observers with the wind; 2d, at right angles to the wind; 3d, in an intermediate direction between the last course and the direction of the wind; 4th, approximately with the wind, so that the sound would come to the ears of the observers against the wind; 5th, in an intermediate direction; and, 6th, again at right angles to the wind. It was supposed that by this arrangement a symmetrical curve of sound would be obtained; and we think this would have been the case had the wind remained constant in direction. It did remain nearly the same during the time of describing the first, second, and third courses, and only slightly varied during the fourth; but previous to running the fifth and sixth courses the wind had changed to a direction nearly at right angles to its first course.

As is shown in Fig. 7, the first, second, third, and fourth courses form a normal curve of audition; the fifth and sixth courses however give discordant results, being much longer than a symmetrical curve would indicate, showing a change in the condition of the medium from that which existed during the running of the other courses; this change was evidently that of the wind, which veering (as above stated) through an arc of a little more than 90°, brought it nearly at right angles to the fifth course, and approximately in the direction of the sixth course; the wind also increased its velocity. These changes are sufficient, without other considerations, to give a rational account of the phenomena observed. They both tend to increase the distance at which the sound would be heard.

In these experiments, as in subsequent ones, it is to be regretted that for want of balloons the motion of the air above could not be ascertained, as was done at Sandy Hook in September, 1874. Previous to sailing from the depot at Staten Island attempts had been made to secure a supply of toy balloons, but none could be found at that time in the city of New York. Arrangements were therefore made for procuring a reservoir of condensed hydrogen, by which India-rub-

ber balloons could be inflated at the time they were wanted. Unfortunately this apparatus did not arrive in time to be of much avail in this series of experiments. Besides this, on account of the smallness of the balloons, the ascent was too slow compared to the horizontal motion to indicate the direction of the wind at a considerable elevation above the points of observation. They were however of use in pointing out definitely the direction of the wind and the changes it was undergoing. Moreover, at the time of leaving New York we were able to procure only one anemometer, whereas we ought to have had a number, one for the top of the tower, one for the bottom, and one for each vessel.

Experiments of September 3.—Barometer, 30·02 inches; thermometer, dry bulb, 72·5° F.; wet bulb, 70°; wind from the east, but too slight to move the cups of the anemometer; it soon however sprang up from the opposite direction, in which it continued during the remainder of the day, attaining a velocity of five and a quarter miles per hour.

In these experiments two light-house steamers were employed, the *Mistletoe* and *Cactus*, which enabled us to obtain the results in half the time, and thus to obviate in some degree the effect of any change in the direction of the wind. On this occasion the sound was noted at the light-house as it converged to a centre from the whistle of each vessel, and also simultaneously by each vessel as it diverged from the vertical siren.

We were enabled in this way to produce two curves by a reverse process. These are plotted in Fig. 8, and exhibit a remarkable degree of similarity. The corresponding parts of the two curves, being in each case reversed, exhibit the fact that through the same space in opposite directions the audibility of the sound was similarly increased with the wind and diminished against it. The effect however of the wind in the experiments of this day was less marked than on any in the whole series, and consequently the two curves of audition more nearly approximate circles.

We can see in this result no other effect than that which would be produced from a wind flowing with a uniform but

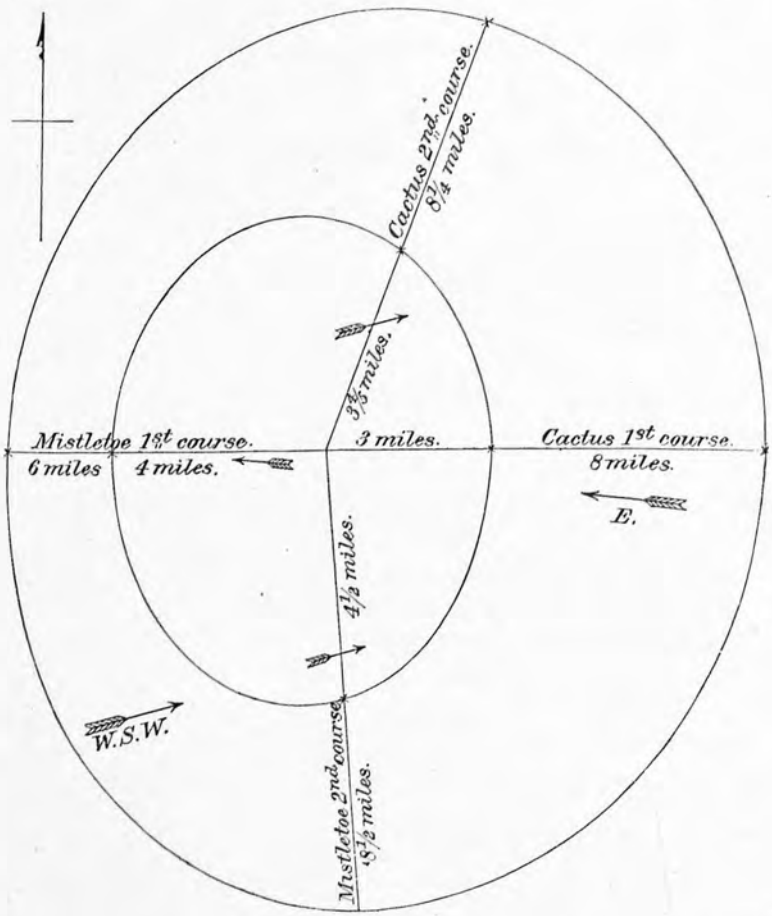


FIG. 8.

slow velocity at the surface, while having a slightly increased velocity above. Had there been no wind, according to this view the two curves would have exhibited two concentric circles.

Experiments of September 4.—Barometer, 29·85 inches, falling; thermometer, dry bulb, 77° F.; wet bulb, 73·25°. Wind south by west, twelve and one-fourth miles per hour at the top of the tower, and nine and one-fourth miles at the bottom; variable.

These experiments were also made with two vessels. The distances and directions are given in Fig. 9. With the exception of the fourth course of the *Cactus* the other courses would form nearly a symmetrical curve, but in this case the sound of the whistle of the *Cactus* was lost at the point *a* at a distance of one mile, and was afterward regained at the point *b*, and continued audible until the steamer reached the point *c*.

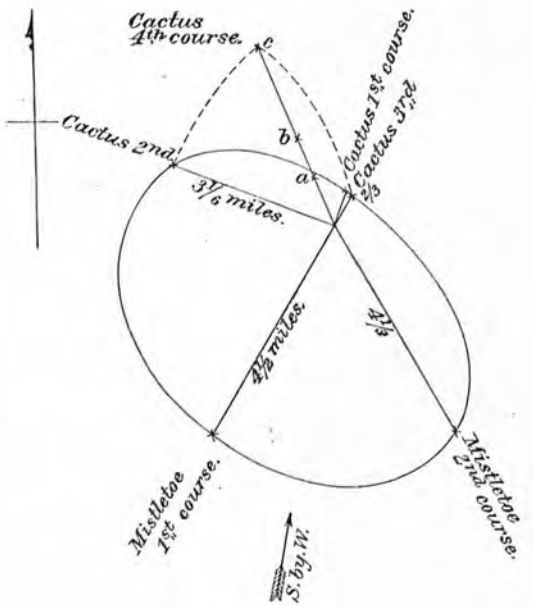


FIG. 9.

This presents one of the abnormal phenomena of sound which might in part be accounted for by the existence of a flocculent cloud between *a* and *b*, but why the sound could be heard so much farther in this direction than in the others is not easy to explain on that hypothesis.

The line *b c* was described after all the lines of Fig. 9 had been completed, and therefore the curve given in the figure correctly represents the boundary of the area of audition while these courses were being run, the point *a* being the termination under that condition of the fourth course of

the *Cactus*. To explain the abnormal line *b c*, we have only to suppose that a change in the velocity of the wind afterward took place by which its opposition to the sound-wave was diminished; this will account for the greater length of the line; the change however did not reach the light-house until after the vessel had passed the point *b*.

As affording evidence in support of this hypothesis, it may be mentioned that on examining the records of the Signal-Service, (of which there is a station at New London, seven miles north of the position at which these observations were made,) it was found that the wind in the morning of that day was south, in the afternoon south-west, and in the evening north-west, and that it was probable as in other cases that the wind had changed above while the part of the course *b c* was being run.

Experiments of September 6.—Barometer, 29·93 inches;

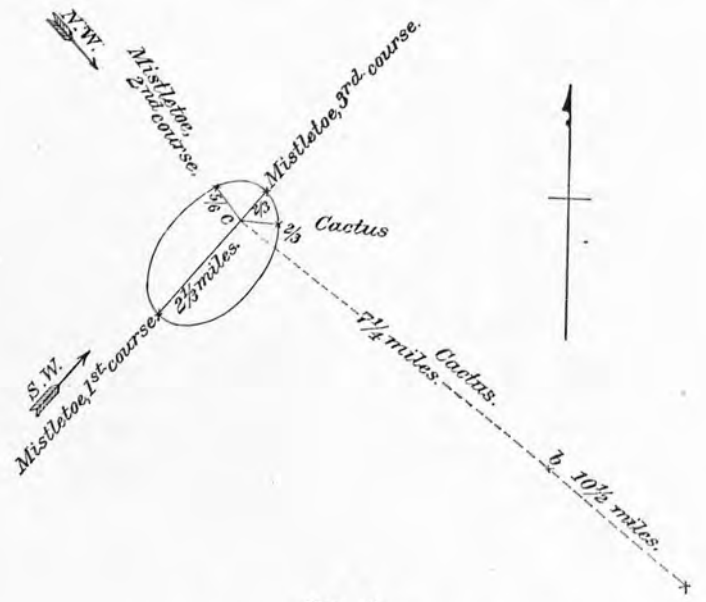


FIG. 10.

thermometer, dry bulb, 74·5° F.; wet bulb, 67°; wind from north-west to south-west, seventeen miles per hour. The wind,

though of higher velocity than on any other occasion, was variable. On this day the experiments were principally made with the *Mistletoe*. The *Cactus* (being obliged to leave on other duty) ran one course a distance of two-thirds of a mile before the sound of her whistle was lost at the light-house. She afterwards steamed off in the direction *c b* (Fig. 10), noting the sound of the siren, which was lost at the point *b*, afterward regained, and heard distinctly ten and one-half miles distant.

During the passage of the first course of the *Mistletoe*, the wind at the surface and above was from south-west, the latter being indicated by a cloud passing the zenith. During the second course the wind was variable, changing its direction about 90°, principally from the north-west; while during the third course the wind was again from the south-west. The long course of the *Cactus* marked on the figure indicates the sound of the siren from the centre outward, as it was heard seven and one-fourth miles, then lost for an interval, and afterward heard again at a distance of three and one-fourth miles farther, making in all ten and one-half miles.

Experiments of September 7.—Barometer, 30·1 inches; ther-

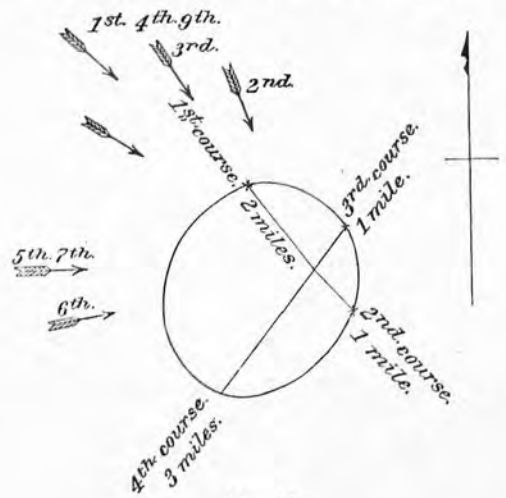


FIG. 11.

mometer, dry bulb, 73° F.; wet bulb, 62°; wind eight miles

per hour at top of tower, and five miles per hour below. The wind was variable, as indicated by the letting-off of balloons, which however did not rise to any great height. The direction of the wind is shown in Fig. 11 by arrows. There is nothing remarkable in the curve of audition of this day. It indicates as usual a greater distance toward the side on which the sound was moving with the wind.

Experiments of September 8.—Barometer, 30·3 inches; thermometer, dry bulb, 70° F., wet bulb, 64·5°: wind, west-south-west, fifteen miles per hour at top of tower, nine miles per hour below. Fig. 12 indicates the curve of audition of the vertical siren as compared with that of the horizontal siren. The steamer first proceeded along the line *C a* nearly in the direction of the axis of the horizontal trumpet. For the distance of the first three miles the horizontal trumpet was the louder. At the point *a*, four miles distant, the two were distinct and very nearly equal. At *b* they were distinct, also very nearly equal, the vertical perhaps a little more distinct. At *c* very nearly equally distinct. At *d* the vertical siren was decidedly more distinct just before entering the optical shadow of the light-house tower and the keeper's dwelling. This shadow continued to the point *e*, which was nearly the extent of the acoustic as well as of the optical shadow, since from *d* to *e* the sound was heard from neither instrument, and the origin of sound was too near to cause much difference between these two shadows. From *f* to *a*, through the point *g*, the two instruments continued to be fully heard—the vertical the more distinct. The effect of the wind in this figure is also very distinctly marked, the longer lines indicating the distance the sound was heard with the wind, and the shorter against

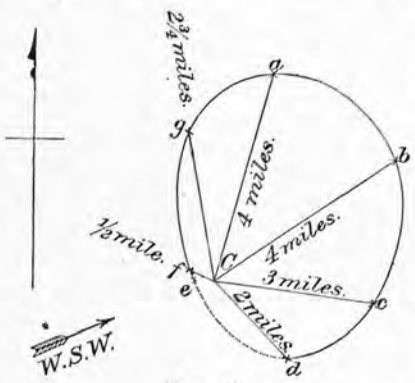


FIG. 12.

it. The curve of this figure is not traced through points at which the sound was absolutely lost, but at which it was heard feebly and with nearly equal distinctness.

Thus far all the facts we have observed, if not in strict conformity with our conception of the hypothesis of Professor Stokes, are at least not incompatible with it. We are now however to direct attention to a fact of much interest, which may not have escaped the attention of the reader; namely the remarkable difference in the area of audition as exhibited in the several figures, all drawn to the same scale. If we compare for example the curve of Fig. 10 with the inner curve of Fig. 8, it might at first sight be inferred that the smallness of the curve in the former case was due to a mottled condition of the atmosphere, which by absorbing the sound—diminished the sphere of audition; but unfortunately for this explanation, it would appear from the observations made by the *Cactus* within the hour of obtaining the data for describing the curve, that the air was then in a remarkably favorable condition for the transmission of sound, since on the south-east course of the steamer (as shown in Fig. 10), the siren was heard ten and a half miles, the ordinary limit of the maximum penetrating power of this instrument—a siren of the second order; while on the 3d of September, the day on which the large curve, Fig. 8, was described, the greatest distance at which the sound of the same instrument could be heard was eight and a half miles.

The only difference in the condition of the air observed during the time of describing the curve of audition given in the figure, and the hearing of the sound by the *Cactus* for ten and a half miles, was a change in the direction (and perhaps in the intensity) of the wind, in the latter case the direction being the same as that of the course of the *Cactus*.

Before therefore admitting any other solution of the question as to the cause of the difference in the area of audition, we must inquire whether it is not possible to refer it to the action of the wind itself.

The most marked difference in the conditions which apparently affected the phenomenon on the days in question

was that of the greater velocity of the wind, both at the surface of the sea and at the top of the tower, and by comparing the several figures in regard to the wind it will be seen that where the condition of the air was nearest that of a calm the larger was the curve of audition, and the nearer the figures approach to a circle, of which the point of origin of sound (or the point of perception) is the centre. From these facts we are inclined to think that sound is not heard as far during a time of high wind in any direction as it is during a perfect calm, and that it is heard farthest with a gentle wind. This conclusion, which was not anticipated at the beginning of these investigations, is we think in strict conformity with the hypothesis adopted. In the case of sound moving against a strong wind, the sonorous waves being thrown up above the ears of the observer, the sphere of audition in that direction is without question greatly diminished; and that it should also be diminished when sound is moving with a strong wind having a greater velocity above than below is not difficult to conceive. In this case the sound-wave will be so thrown down against the earth, and so much of it absorbed, as to weaken the intensity of that part which reaches the ear, while in the case of a feeble wind moving faster above than below, the portion of the wave thrown down from above will only be sufficient to compensate for the smaller loss by friction, and thus the sound may be heard at a greater distance than in still air. But on this point, as well on as others, further experiments are required.

While we consider the wind as the principal agent in producing the abnormal phenomena of sound, we do not by any means regard it as the sole agent. Prof. Osborne Reynolds, of Owens College, Manchester, England, without any knowledge of investigations in America on this subject, instituted a series of experiments on the effect of wind upon sound, and finally adopted precisely the same hypothesis which we have used for generalizing the observed phenomena. He has however, in a very ingenious and important paper presented to the Royal Society in 1874, extended the same principle to the effect of heat in changing the form of the sound-wave, and has shown—both by reasoning and experiment, that the

normal direction of the sound-wave in still air, instead of proceeding horizontally, should be turned upward on account of the greater velocity of sound near the earth, due to the greater heat of the strata in that position than of those above. This principle, which indicates the existence of a true refraction of sound independent of the motion of the medium, is undoubtedly applicable as a modifying influence to the phenomena we have recorded. It produces however only a slight effect in the case we have last mentioned, since the observation on board the *Cactus* shows the condition of the air was that of little acoustic absorption. It would nevertheless favor the hypothesis that sound in perfectly still air of homogeneous density could be heard farther than sound in a moving medium, or in one of unequal temperature. This is also in accordance with the fact repeatedly observed in arctic regions, in which the sound of the human voice is heard at great distances during times of extreme cold. In this case the air is of a uniform temperature above and below, but of diminished elasticity, and should on this account transmit sound with less intensity; and yet the audibility is increased, which is explained by the assumption that its stillness and uniformity of temperature more than compensate for the diminished elasticity. The same may be said with regard to the audibility of sound during a fog, which usually exists during extreme stillness of the air.

Whatever be the cause of the variation in the limit of audition as exhibited in the diagrams, it is less efficient than the ordinary action of the wind in producing the same phenomena. This is evident from the fact that while the ratio of the extreme variation in the limits of audition in the first case is not more than 1 : 3, in the second it is that of 1 : 5.

Moreover, when the effect of the wind on the audition of sound in relation to elevation is considered, we think we are fully warranted in asserting, as we did in our last report, that the wind is a more efficient cause of the variable penetration of sound than the invisible acoustic clouds postulated by Professor Tyndall for the explanation of the phenomena.

The object of these investigations, as stated at the beginning of this report, was to obtain facts which might serve to

establish the true theory of the abnormal phenomena of sound; an object (independent of its scientific interest) of much practical importance in its application to fog-signals. Although the observations were not as perfect as we could wish in many respects, (from want of certain appliances,) they are yet sufficient we think to establish principles of much practical value. For example, if the mariner in approaching a fog-signal—while the wind is blowing against the sound, fails to perceive it on deck, he will probably hear it by ascending to the mast-head; or in case a sound from a given station is constantly obscured in a certain direction, while it is audible in adjacent directions, we may attribute it to a sound-shadow produced by some interposed object. If again the obscuration of sound in a given direction is only observed during a wind moving against the sound, the cause will probably be found in a lateral refraction, due to the retardation of the current of wind against a perpendicular wall or cliff, as in the case observed at Block Island August 19. The subject is indeed one of great complexity, and requires further investigation; but the results thus far obtained may be considered as furnishing the preliminary data on which to found more precise observations. These should be made with the aid of a number of steamers simultaneously employed, each furnished with anemometers and balloons for determining with more accuracy the direction and velocity of the wind.

We hope to renew the investigations during next summer, and in view of this, have directed that in the meantime the light-keepers at Block Island and at Point Judith shall continue to sound their sirens a certain length of time every Monday, noting the direction and velocity of the wind, the temperature and pressure of the air, and the audibility of the sound as it comes reciprocally from each instrument.

It is shown from the results thus far obtained from these reciprocal observations that sound is occasionally heard more distinctly against the wind than in a contrary direction. We think however that these instances are generally followed by a change in the direction of the wind at the surface of the earth

PART V.—INVESTIGATIONS IN 1877.

(Report of the United States Light-House Board for 1877, pp. 61–72.)

On account of the occurrence of the Centennial Exhibition at Philadelphia, which absorbed most of the time of the officers of the Light-House Board not devoted to ordinary light-house service, but few observations were made relative to sound in 1876, and an account of what were made is incorporated in the following report.

Agreeably to previous engagement I visited Portland, Me., to make some further investigation in regard to the abnormal phenomenon of sound noticed in a former report. We left Portland on the afternoon of September 3, 1877, in the steamer *Iris*, which had been fitted up during the year under the direction of the inspector, Commander H. F. Picking, and was in excellent condition, and well adapted to the duty of a light-house tender. The party consisted of General J. C. Duane, engineer of the first district; Commander H. F. Picking, inspector of the first district; Mr. Edward L. Woodruff, assistant engineer of the third district; Mr. Charles Edwards, assistant engineer of the first district, and myself.

Old Anthony Station.—We first examined one of the automatic whistling-buoys invented by Mr. Courtenay, of New York. This was in place and emitting sounds at a station called Old Anthony,—off Cape Elizabeth, about nine miles from Portland. On approaching it at right angles to the direction of the wind, we heard it at the distance of a mile. But the sound did not appear loud even within a few rods. It was however of considerable quantity, being from a locomotive whistle of ten inches in diameter. The instrument is operated by the oscillation of the waves, which at this time were not of sufficient height to move it vertically through a space of more than one foot. It emitted a sound at each oscillation. This invention consists of a large pear-shaped buoy about twelve feet in diameter at the water-surface, floating about twelve feet above the same plane. In the interior of this

buoy is a large tube or hollow cylinder three feet in diameter, extending from the top through the bottom to a depth of about thirty feet below the latter. This tube is open at the bottom, but projects air-tight through the upper part of the buoy, and is closed with a plate having three orifices in it, two for letting in the air into the tube, and one between the others for letting it out to operate the whistle. These orifices are connected with three tubes which extend downward to near the level of the water, where they pass through a diaphragm which divides the cylinder into two parts.

When the buoy rises, the water in the cylinder by its inertia retains its position, and a partial vacuum is formed between the head of the column and the diaphragm, into which the air is drawn through two of the tubes, and when the buoy descends, the escape through the injection tube being prevented by valves, the air is forced out of the inner tube and actuates the whistle.

The mooring-chain, which is sixty fathoms in length, is attached to the cylinder at a point just below the buoy, and is secured to a large stone weighing about six tons. The apparatus rides perpendicularly.

The sound in this instrument is not produced merely by the difference in hydrostatic pressure of the water in the two positions of the buoy, but by the accumulated effect of impulse generated by the motion of the apparatus.

Plans have been devised—but have not yet been perfected, to condense the air in the buoy by the effect of repeated oscillations, until a valve loaded to a definite pressure would open automatically and allow the air to escape. In this way the sound from the accumulated pressure would be produced at intervals to a greater or less extent, and would serve to diversify the character of the sound so as to enable the mariner to distinguish different locations. The invention—even in its present form, is considered a valuable addition to the aids to navigation, has received the unqualified approbation of all navigators on this coast who are acquainted with its operation, and will probably be introduced in all countries where its merits are known. Experience has shown

that it can be permanently moored in deep water, and that vessels can safely approach it within the nearest distance and take perfect departure from it.

The Light-House Board has adopted this buoy as one of its permanent aids to navigation, and will in time introduce it at all points where its presence will be of importance to the navigator. In order to obtain reliable data as to the operations of the automatic buoy, Commander Picking has established a series of observations at all the stations in the neighborhood of the buoys, giving the time of hearing it, the direction of the wind, and the state of the sea; from which it appears that in the month of January, 1877, one of these buoys was heard every day at a station one and one-eighth miles distant; every day but two, at a station two and one-quarter miles distant; fourteen times at a station seven and one-half miles distant, and four times at a station eight and one-half miles distant. It is heard by the pilots of the New York and Boston steamers at distances of from one-fifth to five miles, and has frequently been heard by the inspector of the first light-house district at a distance of nine miles, and even (under the most favorable circumstances) fifteen miles.

We sailed around the buoy and observed the difference in the intensity of sound relatively to the direction of the wind, which was at the time a fresh breeze of from twelve to fifteen miles per hour from the westward;—the greatest intensity being apparently at points forty-five degrees on either side of the axis of the wind. The effect however was not very definitely marked, though the sound on the whole appeared to be greater on the semi-circumference of the circle to the leeward; but the velocity of the wind was so great that the noise produced by it on the rigging of the vessel prevented the effects from being definitely observed.

Experiments have been made with this buoy carrying whistles of different sizes, the result being that a whistle of less than ten inches diameter does not give a sound which can be heard as far as one of the latter size, although when near by, it appears to the ear equally loud.

There is a difference between the quantity of sound and

the loudness. Two sounds may be equally loud when heard near by, yet differ very much in regard to their being heard at a distance, the loudness depending upon the intensity of sound or on the amplitude of vibration of the sounding body, while the quantity of sound depends on the extent of the vibrating surface.

The size of the whistle must be limited by the quantity of air ejected at each oscillation of the buoy. The fact that the ten-inch whistle gives a sound which can be heard farther than one of eight inches, appears to have a bearing on the question of the united effect (the actuating force being the same) of two sounds of the same quantity and pitch, since the sound from several parts of the circumference of the larger whistle may be considered as a union of several sounds of less quantity.

Whitehead Station.—After these observations on the automatic buoy we proceeded along the coast to Whitehead, at the entrance of Penobscot Bay, a distance of sixty miles, which we reached at about twelve o'clock at night, and cast anchor in Seal Harbor, near the Whitehead light-house.

Our first operation next morning was the examination of an automatic fog-bell, invented by Mr. Close, and which has been erected by a special appropriation of Congress. It is very simple in conception, and would do good service in southern latitudes, where it would not be affected by the ice. It consists of an upright shaft thirty-two feet long, fastened to the rock beneath the water and kept in a vertical position by a series of iron rods serving as braces. Around this shaft is a hollow metallic float, having sufficient buoyancy to elevate a vertical rod by the motion of the waves, having at the upper end a rack gearing into a ratchet-wheel. By means of projecting pins on the surface of the wheel, the hammer of the bell is elevated and the bell sounded at each descent of the float. This arrangement is the most simple and efficient of the kind of which we have any knowledge.

The objection to it is its liability to be deranged by the action of ice and the rusting of the parts from exposure to the weather.

Our next operation at this place (the principal object of our excursion) was the examination of the remarkable abnormal phenomenon, which has been frequently observed by the captains of the steamers plying between Boston and New Brunswick, and has also been noticed on two different occasions by officers of the light-house establishment. The phenomenon, as reported by these authorities, consists in hearing the sound distinctly (on approaching the station) at the distance of from six to four miles, then losing it through a space of about three miles, and not hearing it again until within about a quarter of a mile of the instrument, when it becomes suddenly audible in almost full power. This phenomenon is always noticed when the vessel is approaching the signal from the south-west, and the wind is in the same or in a southerly direction, and therefore opposed to the direction of the sound from the station, as is usually the case during a fog. Commander Picking, having frequently received complaints from masters of vessels as to losing the sound at this place, concluded to verify the facts by his own observation. For this purpose, he embraced the opportunity of an inspection-tour in July, 1877, to approach the station from the southwest during a fog. In his own words, he heard the sound distinctly through a space of from six to four miles, then lost it, and could hear nothing until within a quarter of a mile of the station, when the blast of the whistle burst forth in full sound. The wind at this time was from the southward, or against the sound. This cessation in the hearing of the sound could not have been due to the failure of the instrument to emit sound, since its operation is automatic when once started, and in this case the fog so lifted on nearing the station as to admit the observation of the puffs of steam emitted at each blast of the whistle.

On a previous occasion General Duane and Mr. Edwards on approaching the same signal from the south-west heard the sound at about six miles distance, then lost it, and did not again hear it until within about a quarter of a mile. The wind in this instance was also the same as that in the observation of Commander Picking, namely from the southwest.

So well established was the phenomenon, that General Duane attempted to remedy the evil by elevating the duplicate whistle (with which every station is provided) to a height twenty-two feet above the level of the other whistle, by placing it on the upper end of a tube. But this arrangement produced no beneficial effect.

September 4, 1877.—In the morning of September 4, on which we commenced our experiments, the weather was clear, the wind west-southwest, the velocity from ten to twelve miles, remaining nearly constant during the day. Our first object was to verify by direct observation the several features of the phenomenon, and for this purpose we steamed to the southward, or directly to the windward, from the station through the region in which the abnormal phenomena had been noticed. The pressure of the atmosphere, as indicated by an aneroid barometer, was 28·9 inches. The temperature of the air was 67° Fahrenheit; that of the water at various points along our course was 58°, except at two points where the thermometer indicated 57°. This difference was too small to have any perceptible effect on the density of the rapidly moving air which was passing over the surface of the water. As we increased our distance from the signal the sound slightly diminished in loudness until the distance was between a quarter and half a mile, when it suddenly ceased to be heard, and continued inaudible through a distance of about a mile, when it was faintly heard and continued to increase in loudness until we reached the distance of four miles; at this point it was heard with such clearness that the position of the station could be located with facility; but on proceeding farther in the same direction it appeared to diminish gradually except at one point, when a blast, as indicated by the steam issuing from the whistle, was inaudible; but on turning the vessel around the next blast was distinctly heard.

As a second experiment, we retraced the same line back to the station and observed the same phenomena in a reverse order. The sound was heard the loudest at a point four

miles from the station; afterward it diminished and then became inaudible through a space of two miles, and then suddenly burst forth in nearly full intensity at the distance of a quarter of a mile, and continued loud until the station was reached.

As a third experiment, the same line was traversed again, the only difference in the condition of the experiment being that the whistle on the steamer was sounded every minute between the blasts of the signal at the station; and while the observers on the vessel noted the sounds from the latter, those at the station observed the sounds from the former. The same phenomena as described in the previous experiments were witnessed by those on board the vessel, but on receiving the report of the observers at the station it was found that no cessation of the sound from the steamer was observed through the whole distance traversed by the vessel. It should be noted that the whistle at the station is ten inches in diameter, actuated by a pressure of sixty pounds of steam, and that on board the vessel six inches in diameter with twenty-five pounds of steam. It appears from this remarkable result that a feeble sound passes freely through what has been called the region of silence when sent in the direction of the motion of the wind, when a louder sound does not pass in the opposite direction.

As a fourth experiment, the vessel proceeded northward on the side of the station opposite to that before traversed, but in the prolongation of its previous course. The sound from the signal to the observers on the vessel was in this case with the wind, while that from the vessel to the observers at the station, was against the wind. In this experiment no cessation was observed on the vessel in the hearing of the sound from the station; it was heard with varying intensity to the distance of four and a half miles, and could probably have been heard much farther had our progress not been interrupted by land. On returning to the station the observers there reported that after the vessel had left the station, and was scarcely more than a hundred yards distant, not a single blast of its whistle was heard. In this case

the phenomena which had been observed on the southerly side of the station were exhibited in a reverse order on the northerly side.

In what may be considered the fifth experiment, the vessel being at a distance of four miles from the station—on the line traversed in the first two experiments, the sound was slightly heard. The vessel then altered its course so as to steam around the signal, keeping at the same distance until the direction of the station, from the vessel, was nearly at right angles to the direction of the wind; at this point no sound was heard from the station, although it had been slightly heard at points along the curved line traversed in reaching the point mentioned. The vessel then proceeded toward the station in a straight line, but no sound was heard until it approached the latter within a quarter of a mile. The observers at the station however heard the sound from the vessel through the whole distance.

This experiment was made to ascertain the truth of the general impression that at this place the sound is heard better coming at right angles—or across the wind, than in the direction in which it was blowing. The experiment however was found in conformity with the general rule previously established, that the sound was usually heard farthest with the wind, least against the wind, and at an intermediate distance across the wind.

The primary object of these investigations has been to determine the mechanical causes to which the phenomena may be referred, from which new conclusions may be deduced—to be further tested by experiment, and such definite views obtained, as may be of value in the employment of fog-signals for the uses of the mariner.

For this purpose a number of different hypotheses may be provisionally adopted, and each compared with the actual facts observed.

The first hypothesis which had been suggested for the explanation of the phenomena in question was that they are due to some configuration of the land; but on inspecting the Coast-Survey chart of this region it will be seen that the

nearest land consists of a series of broken surfaces not rising above the ocean enough to reflect sound or in any way to produce sound-shadows in the region through which the phenomena are observed. This hypothesis therefore is inadmissible.

Another hypothesis is that of invisible acoustic clouds (as they have been called), or portions of atmosphere existing over the water in the region of silence, which might absorb or variously reflect the sound. That such a condition of a portion of the atmosphere really exists in some cases is a fact which may be inferred from well-established principles of acoustics, as well as from experimental data. They would occur especially in the case of dissolving clouds, which would be accompained by local diminutions of temperature, and also from portions of air which have been abnormally heated by contact with warm earth. But if the phenomena in question were produced by a cloud of this kind, its presence ought to be indicated by carrying through it the usual set of meteorological instruments. This was done in the foregoing experiments, but no change was observed in the indications either of the thermometer or barometer. Unfortunately we had not a hygrometer in our possession, but this observation was less necessary, since from abundant testimony it is established that the same phenomena are exhibited during a dense fog, in which all parts of the atmosphere for miles in extent must be in a homogeneous condition. Furthermore, a local cloud could not continue to exist in a given space for more than an instant while a wind was blowing with a velocity of from ten to twelve miles an hour. Again, this hypothesis fails entirely to explain the fact that this phenomenon is always observed at nearly the same place, especially during a fog, when the wind is in a southerly direction. Finally, it is impossible to conceive of a cloud so arranged as a screen producing a sound-shadow of greater intensity on one side than on the other.

Another hypothesis is that of the refraction of sound due to the action of the wind. It is an inference from well-

established theory, as well as from direct observation, that the sound is refracted by the wind, that it tends to be thrown upward when moving against the wind, and downward with the wind. This result is attributed very properly to the different velocities of the strata, that next the surface being most retarded, those above being less retarded.

The upper part of the front of the wave is thus thrown backward, and the direction of the wave turned upward. In the case of the experiment south of the station, the wind passing over a long line of rough sea was moving less rapidly in its lower stratum than in the higher, and consequently the sound-wave was thrown backward above, and as it issued from the instrument, tended to rise above the head of the observer, and at a certain distance from the origin of the sound—depending upon the difference of velocity above and below, was lost entirely to the observer, and a sound-shadow was thus produced by refraction, which is either surmounted by an undulating course of the sound beam, or is closed in again by the lateral spread of the sound at a given distance.

In the experiment on the other side of the signal, (the vessel proceeding to the north,) the wind coming to the observer on the vessel—had to pass over a rougher surface than that of water, and consequently the difference of velocities above and below, would cause the refraction to be greater; hence the sound from the vessel was almost entirely lost to the observer at the station, while the sound from the station was heard uninterruptedly on the vessel, since it was moving with the wind.

On examining the records of experiments of previous years, I find a number of cases recorded where sounds were heard at a greater distance, while inaudible at a less distance, especially one in connection with the fog-signal at Gull Island, in 1874. In this case the sound, in passing from the signal, was heard distinctly at the distance of about two miles against the wind, then lost for a space of about four and a half miles, and heard again distinctly for a distance of perhaps one mile. At the same station, during the

experiments of 1875, the sound of the whistles of the steamers was heard for a certain distance, then ceased to be heard for a considerable interval, after which it was heard again. Furthermore the pilots of the steamboats from New York to Boston report that the sound of the automatic buoy is found to be intermittent, being heard at a distance, then becoming inaudible, and heard again as the steamer approaches the source of sound.

From all the facts gathered on this subject, I think it highly probable that in all cases in which sound moving against the wind is thrown up above the head of the observer, it tends to descend by the lateral spread of the sound-wave and to reach the earth again at a distance; the conditions however for the actual production of this effect are somewhat special, and will depend upon the amount of the initial refraction and the quantity of the sound-waves. Besides the lateral spread of the sound-wave there are two other causes sufficient (in certain cases) to bring a portion of the sound-waves which have been elevated in the air—back again to the earth: the first is when an upper current of wind is moving in an opposite or approximately opposite direction to that at the surface of the earth, in which case an opposite or downward refraction would take place; and the second is the case in which the surface-wind is terminated above by a stratum of comparatively still air; in this case also, a reverse refraction (but of less amount) would take place, which would tend to bring the sound-wave downward.

We can readily imagine that a solitary island, cooled by the radiation of heat at night, would every morning send a current of cold air in all directions from its centre. In this case, the sound from a whistle placed in the centre of the island would be inaudbile in a space entirely surrounding it, and thus give rise to a condition mentioned by General Duane, in which a fog-signal appeared to be surrounded by a belt of silence.

September 5, 1877.—The next experiment was made on the morning of the 5th, on leaving the station. In this case we proceeded along the direction of the same line in which the

first, second, and third experiments were made on the day before. The wind had changed about four points to the southward. As in the preceding experiments, the sound was lost again at the distance of about one-fourth of a mile, but was not distinctly regained, though some of the observers thought they heard it at a distance of two and one-half miles.

The only perceptible difference in the wind (on the 5th) was that it was a little less rapid, and four points more to the southward.

From a subsequent report of the keepers, the whistle of the vessel was heard continuously as far as the puffs of steam could be observed—a distance of six or seven miles. In this case the sound was moving with the wind. These results therefore are in accordance with those previously obtained.

Monhegan island.—The next experiments were made at Monhegan, an island sixteen miles south-west of Whitehead. On this island there is a Daboll trumpet actuated by a hot-air engine.

We departed from this station in a westerly direction at an angle of 45° to the right of the direction of the wind, and after proceeding about one mile, as estimated by time, we lost the sound of the signal. We then turned at right angles to our former course and proceeded toward the leeward, keeping about the same distance from the signal, when the sound was regained at a point which probably depended upon the direction of the wind and the axis of the trumpet combined. From this point it was heard to a point at the leeward, and thence we retraced our course at about the same distance and proceeded across the axis of the trumpet toward the windward, where the sound was again lost. The only definite result from this experiment was another case of the sound being heard farther to leeward than to the windward.

After this experiment we returned to Portland.

An interesting fact may be mentioned in connection with this station, having a bearing upon the protection of light-houses from lightning. The fog-signal is placed on a small island separated from the large island by a water-space of about one-eighth of a mile. General Duane, desiring to

connect the light-house and fog-signal by an electrical communication, suspended a wire between the two points and attempted to form a ground connection by depositing a plate of metal in the ground on each island, but to his surprise,—though the arrangements were made by a skilled telegrapher, no signal would pass. The two islands being composed of rock and the soil limited in thickness, the conduction was imperfect, and it was only by plunging the plate of metal into the water on each side of the space between the two islands that a signal could be transmitted.

No further experiments on sound were made during this excursion, because the vessel could no longer be spared from more pressing light-house duty in the way of inspection, and transportation of the stated supply of materials to the stations.

On my return to New York, accompanied by Mr. Woodruff, I took the route by the Western railway to the Hudson River at Troy. This line was chosen in order to make some investigations relative to any peculiarities of sound which might be observed in the Hoosac tunnel, through which the railroad passes. For this purpose we spent a day at East Windsor, a village situated near the west end of the tunnel, and were very cordially received by the engineers in charge.

The tunnel is four and three-quarters miles in length, twenty-four feet wide, and twenty feet high to the crown of the arch. It ascends slightly from either end to a point near the centre, where there is a ventilating shaft 1,028 feet high extending to the outer air above. In winter, when the external temperature is less than that within the tunnel, there is a constant current from each end toward the centre, and in the summer, when the temperatures are reversed, there is a current out of the tunnel at either end, except when the external wind is sufficiently strong (especially from the west) to reverse the direction of the current from one half, and direct the stream entirely out of the other entrance. At the time of our visit, there was a gentle current flowing out of both ends.

The only peculiarity of sound which had been observed

(as stated by the engineers,) was that it was greatly stifled immediately after the passage of the locomotive, by the smoke with which the air was filled at the time. So great was this in some cases, that accidents were imminent to the workmen, (who are constantly occupied in the tunnel in lining the crown of the arch with brick,) by the sudden appearance of a locomotive, the approach of which had not been heard.

That the audibility of sound should be diminished by smoke was so contrary to previous conceptions on the subject, (since sound is not practically interrupted by fog, snow, rain, or hail,) I was induced to attribute the effects which had been observed to another cause, and to regard the phenomenon as due to an exaggerated flocculent condition of the air in the tunnel, adopting in this instance the hypothesis advanced by Dr. Tyndall, and so well illustrated by his ingenious experiments. The effect which would be produced in the condition of the air in the tunnel by the passage of a locomotive is indicated by the appearance of the emitted steam extending behind the smoke-stack of a locomotive in rapid progress before the observer at a distance. This consists of a long stream composed of a series of globular masses produced by the successive puffs of steam which are emitted at equal intervals. Allowing the diameter of the driving-wheels to be five feet, then since four puffs are made at each revolution of the wheels, a puff of hot steam would be given out at every four feet travelled by the engine, and these puffs mingling with the air at the ordinary temperature would produce an exaggerated flocculent condition. On our expressing a desire to witness the effect upon sound of the passage of a locomotive through the tunnel, Mr. A. W. Locke, one of the engineers who had charge of the western section, politely offered us the means of experimenting on this point, and also of passing leisurely through the tunnel on a hand-car.

To observe the effect of a locomotive on the sound, we took advantage of the entrance of a freight train impelled by two engines; the extra one being necessary to drive the load up the inclined plane to the middle of the tunnel, where it was de-

tached and returned along the same line, while the train was drawn the remaining distance along the eastern decline by a single engine. In order to make the experiment with regard to sound, the time was accurately noted during which the noise of the entering engines could be distinctly heard, which would give approximately the distance the sound travelled through the flocculent atmosphere produced by the locomotive before becoming inaudible, and again the time was noted from the first hearing of the returning engine until it reached the end of the tunnel. In the meantime the current of air blowing through the tunnel had removed a considerable portion at least—of the flocculent atmosphere, so that the sound in this case came through an atmosphere of comparatively uniform temperature, or one much less flocculent than the other; the result was that the duration of sound in the first case was about a minute, while in the second it was upward of two minutes. The darkness in the tunnel—on account of the smoke, was so profound—immediately after the passage of a locomotive, that with two large torches, charged with mineral oil, the sides of the tunnel at a distance of six feet could scarcely be observed; while in the other half of the tunnel, where no smoke existed, the eastern opening could be observed like a star at the distance of upward of two miles. It was therefore not surprising that the stifling of the sound which was observed should be referred to the smoke as a palpable cause, and that the more efficient one of the varying density or flocculent condition should be disregarded.

The method of determining by experiment the question as to which of these causes was the efficient one did not occur to me until we had left the tunnel, and then the simple expedient suggested itself to me for the purpose of repeating the experiment, that instead of locomotives charged with wood, two locomotives charged with charcoal or coke—which emits no smoke, but only transparent gases, principally carbonic acid,—should be used in an experiment similar to the one just described. This experiment Mr. Locke has kindly promised to perform as soon as it can conveniently be arranged.

The opportunity was embraced while at the mouth of the tunnel to make some observations which might have a bearing upon the phenomena of the aerial echo. For this purpose advantage was taken of a large tool-chest which happened to be placed about twenty or thirty feet within the western mouth of the tunnel. By slamming down violently the cover of this chest, a loud sound of an explosive character was produced, from which a prolonged echo was returned from the interior of the tunnel. This echo was slightly intermittent, suddenly increasing in loudness at intervals for a moment, and again resuming its uniform intensity. This effect was attributed to projecting pieces of rock in that part of the tunnel which had not been lined with brick. An echo was however evidently returned from that portion of which the sides were not projecting, which I would consider an effect of the same cause which produces the aerial echo.

Aerial Echoes.

During the year 1877 (as also in 1876) series of experiments were made on the aerial echo, in which I was assisted in the first series by General Woodruff, engineer of the third light-house district, and in the second series by Edward Woodruff, assistant engineer of the same district. These experiments were made principally at Block Island, though some were also made at Little Gull Island. Especial attention has been given to this phenomenon, (which consists in a distinct echo from the verge of the horizon in the direction of the prolongation of the axis of the trumpet of the siren,) because the study of it has been considered to offer the easiest access to the solution of the question as to the cause of all the abnormal phenomena of sound, and also because it is in itself an object of much scientific interest.

In my previous notice of this phenomenon, in the report of the Light-House Board for 1874, I suggested that it might be due to the reflection from the crests of the waves of the ocean; but as the phenomenon has been observed during all conditions of the surface of the water this explanation is not tenable.

Another hypothesis has been suggested, that it is due to a flocculent condition of the atmosphere, or to an invisible acoustic cloud of a density differing from that of the general atmosphere at the time. To test this hypothesis experimentally, the large trumpet of the siren was gradually elevated from its usual horizontal position to a vertical one. In conception this experiment appears very simple, but on account of the great weight of the trumpet it required the labor of several men for two days to complete the arrangements necessary to the desired end. The trumpet in its vertical position was sounded at intervals for two days, but in no instance was an echo heard from the zenith, but one was in every case produced from the entire horizon. The echo appeared to be somewhat louder from the land portion of the circle of the horizon than from that of the water. On slowly restoring the trumpet to its former direction, the echo gradually increased on the side of the water until the horizontal position was reached, when the echo as usual appeared to proceed from an azimuth of about twenty degrees of the horizon, the middle of which was in the prolongation of the axis of the trumpet. A similar experiment was made with one of the trumpets of the two sirens at Little Gull Island. In this case the trumpet was sounded in a vertical position every day for a week with the same result.

From these experiments it is evident that the phenomenon is in some way connected with the plane of the horizon, and that during the continuance of the experiment of sounding the trumpets while directed towards the zenith, no acoustic cloud capable of producing reflection of sound existed in the atmosphere above them.

Another method of investigating this phenomenon occurred to me, which consisted in observing the effects produced on the ear of the observer by approaching the origin of the echo. For this purpose, during the sounding of the usual interval of twenty seconds of the large trumpet at Block Island, observations were made from a steamer which proceeded from the station into the region of the echo and in the line of the prolongation of the axis of the trumpet, with the following results:

1. As the steamer advanced, and the distance from the trumpet was increased, the loudness of the echo diminished, contrary to the effect of an echo from a plane surface, since in the latter case the echo would have increased in loudness as the reflecting surface was approached, because the whole distance travelled by the sound to and from the reflector would have been lessened. The effect however is in accordance with the supposition that the echo is a multiple sound, the several parts of which proceed from different points at different distances of the space in front of the trumpet, and that as the steamer advances towards the verge of the horizon it leaves behind it a number of the points from which the louder ones proceed, and thus the effect upon the ear is diminished as the distance from the trumpet is increased.

2. The duration of the echo was manifestly increased, in one instance, from five seconds, as heard at the mouth of the trumpet, to twenty seconds. This would also indicate that the echo is a multiple re-action of varying intensities from different points, and that at the place of the steamer, the fainter ones from a greater distance would be heard, which would be inaudible near the trumpet.

3. The arc of the horizon, from which the echo appeared to come, was also increased in some cases to more than three times that subtended by the echo at the place of the trumpet. This fact again indicates that the echo consists of multiple sounds from various points at or near the surface of the sea, the angle which the aggregate of these points subtend necessarily becoming greater as the steamer advances.

But perhaps the most important facts in regard to the echo are those derived from the series of observations on the subject made by Mr. Henry W. Clark, the intelligent keeper of the principal light-house station on Block Island, and by Mr. Joseph Whaley, keeper of the Point Judith light-house. Mr. Clark was furnished with a time-marker to observe the duration of the echo, and both were directed to sound the trumpets every Monday morning for half an hour, noting the temperature, the height of the barometer, the state of

the weather as to clearness or fog, the direction and intensity of the wind, and the surface of the ocean.

From the observations made at these two points, for more than two years at one station and over a year at the other, the echo may be considered as produced constantly under all conditions of weather, even during dense fogs, since at Block Island it was heard 106 times out of 113, and at Point Judith 50 times out of 57, and on the occasions when it was not heard the wind was blowing a gale, making a noise sufficiently loud to drown the sound of the echo. These results would appear to quite effectually disprove the hypothesis that the phenomenon is produced by an acoustic cloud accidentally situated in the prolongation of the axis of the trumpet. An occurrence of such regularity must be due to something more permanent in its effects than a difference in temperature or density of a portion of air—from that of the general atmosphere; since such a condition could not exist in a dense fog embracing all the region in which the phenomenon occurs. Indeed, it is difficult to conceive how the results can be produced, even in a single instance, from a flocculent portion of atmosphere in the prolongation of the axis of the trumpet; since a series of patches of clouds of different temperature and density (if sufficient) would tend to absorb or stifle by repeated reflections—a sound projected into their interior, rather than to transmit it to the ear of the observer.

The question therefore remains to be answered: What is the cause of the aerial echo? As I have stated, it must in some way be connected with the plane of the horizon. The only explanation which suggests itself to me at present, is that the spread of the sound—which fills the whole atmosphere from the zenith to the horizon with sound-waves, may continue its curvilinear direction until it strikes the surface of the water at such an angle and direction as to be reflected back to the ear of the observer. In this case, the echo would be heard from a perfectly flat surface of water; and as different sound-rays would reach the water at different distances and from different azimuths, they would produce the pro-

longed character of the echo, and its angular extent along the horizon.

While we do not advance this hypothesis as a final solution of the question, we shall provisionally adopt it as a means of suggesting further experiments in regard to this perplexing question at another season.

General Conclusions.

From all the experiments which have been made by the Light-House Board in regard to the transmission of sound in free air, and those derived from other observations which can be fully relied upon, the following conclusions may be considered established, subject however to such further modification and extension as subsequent investigation may seem to indicate:

1. The audibility of sound at a distance (the state of the atmosphere being constant) depends upon the character of the sound. The distance through which a sound may be heard is governed by the pitch, the loudness, and the quantity of sound. The pitch or frequency of the impulses in a given time must not be too high, otherwise the amplitude of vibration will be too small to allow a sufficient quantity of air to be put into motion; neither must the pitch be too low, for in this case the motion of the atoms of air in the sound-wave will not be sufficiently rapid to convey the impulse to a great distance. Again the greater the loudness of the sound, (which depends upon the amplitude of the vibrations of the sounding-body,) the greater will be the distance at which it will be heard. And finally, the greater the quantity of sound, (which depends upon the magnitude of the vibrating surface,) the greater will be the distance to which it is audibly transmitted. These results are derived from observations on the siren, the reed-trumpet, and the automatic buoy. The effect of quantity of sound is shown by the fact that in sounding different instruments at the same time, it was found that two sounds apparently of the same loudness were heard at very different distances.

2. The audibility of sound depends upon the state of the

atmosphere. A condition most favorable to the transmission of sound is that of perfect stillness and uniform density and temperature throughout. This is shown by the observations of Parry and other Arctic explorers; although in this case an efficient and co-operating cause is doubtless the downward refraction of sound, due to the greater coldness of the lower strata of air, as first pointed out by Professor Reynolds. Air however is seldom in a state of uniform density, but is pervaded by local currents, due to contact with portions of the earth unequally heated, and from the refractions and reflections to which the sound-wave is subjected in its passage through such a medium it is broken up and lost to the ear at a less distance.

3. But the most efficient cause of difference in audibility is the direct effect produced by the wind. As a general rule, a sound is heard farther when moving with the wind than when moving against it. This effect, which is in conformity with ordinary observation, is not due to an increase of velocity of the sound-wave in one direction and a diminution in the other by the motion of the wind except in an imperceptible degree; for since sound moves at the rate of about seven hundred and fifty miles an hour, a wind of seven miles and a half an hour could increase or diminish the velocity of the sound-wave only one per cent., while the difference of effect observed is in some cases several hundred per cent. The true cause of the remarkable variation in the audibility of sound-beams at a distance—is to be found in the change of their direction. Sound moving with the wind is ordinarily refracted or thrown down toward the earth; while moving against the wind it is ordinarily refracted upward and passes over the head of the observer, so as to be heard at a distance at an elevation of several hundred feet, when inaudible at the surface of the earth.

4. Although as a general rule the sound is heard farther when moving with the wind than when moving against it, yet in some instances sound is heard farthest against the wind; but this phenomenon is shown to be due to a dominant upper wind, blowing at the time in an opposite direc-

tion to that at the surface of the earth. Such winds are not imaginary productions invented to explain the phenomena, but actual existences—established by observation, as in the case of the experiments made at Sandy Hook, in 1874, by means of balloons, and from the actual motion of the the air in the case of north-east storms, as observed at stations on the coast of Maine.

5. Although sound issuing from the mouth of a trumpet is at first concentrated in a given direction, yet it tends to spread so rapidly that at the distance of three or four miles it fills the whole space of air within the circuit of the horizon, and is heard behind the trumpet nearly as well as at an equal distance in front of its mouth. This fact precludes the use of concave reflectors as a means of increasing the intensity of sound in a given direction; for although they do give an increase of sound in the direction of the axis, it is for only a comparatively short distance.

6. It has been established (contrary to what was formerly thought to be the case,) that neither fog, snow, hail, nor rain, materially interferes with the transmission of loud sounds. The siren has been heard at a greater distance during the prevalence of a dense and widely-extended fog than during any other condition of the atmosphere. This may be attributed to the uniform density and stillness of the air at the time.

7. In some cases sound-shadows are produced by projecting portions of land or by buildings situated near the origin of the sound; but these shadows are limited in extent, and are closed in at some distance by the spread of the sound-waves, thus exhibiting the phenomenon of sound being heard at a distance when lost on a nearer approach to the station.

8. It frequently happens on a vessel leaving a station that the sound is suddenly lost at a point in its course, and after remaining inaudible some time is heard again at a greater distance, and is then gradually lost as the distance is increased. This phenomenon is observed only when the sound is moving against the wind, and is therefore attrib-

uted to the upward refraction of the sound-wave, which passes over the head of the observer and continues an upward course until it nearly reaches the upper surface of the current of wind, when the refraction will be reversed and the sound sent downward to the earth; or the effect may be considered as due to a sound-shadow produced by refraction, which is gradually closed in at a distance by the lateral spread of the sound-wave near the earth, in a direction which is not affected by the upward refraction. Another explanation may be found in the probable circumstance of the lower sheet of sound-beams being actually refracted into a serpentine or undulating course, as suggested in the Appendix to the Report of the Light-House Board for 1875.* Such a serpentine course would result from successive layers of unequal velocity in an opposing wind; as being retarded at and near the surface of the earth, attaining its maximum velocity at a height of a few hundred feet, and then being again retarded at greater elevations, by the friction of upper counter currents or of more stationary air. In some cases the phenomenon is due to one or the other of these causes, and in other cases to all combined. That it is not due to the obstructing or screening effects of an abnormal condition of the atmosphere is shown by the fact that a sound transmitted in an opposite direction, through what is called the region of silence, passes without obstruction. It is probable from all the observations, that in all cases of the upward refraction of a sound moving against the wind it tends again to descend to the earth by the natural spread of the sound; though it may generally be so enfeebled by diffusion as well as by distance, as to be inaudible.

9. The existence of a remarkable phenomenon has been established, exhibited in all states of the atmosphere,—during rain, snow, and dense fog, to which has been given the name of aerial echo. It consists of a distinct echo, apparently from a space near the horizon of fifteen or twenty degrees in azimuth, directly in the prolongation of the axis

*[See PART IV; *ante*, p. 450.]

of the trumpet. The loudness of this echo depends upon the loudness and quantity of the original sound, and therefore it is produced with the greatest distinctness by the siren. It cannot be due to the accidental position of a flocculent portion of atmosphere, nor to the direct reflection from the crests of the waves, as was at first supposed, since it is always heard except when the wind is blowing a hurricane.

As a provisional explanation, the hypothesis has been adopted that in the natural spread of the waves of sound some of the rays must take such a curvilinear course as to strike the surface of the water in a vertical direction, and thus be reflected back—by a similar deviation, to the station or location of the origin of the sound.

END OF VOL I.

INDEX TO VOLUME I.

PAGE.

Address at Metropolitan Mechanics' Institute Exhibition ---- 306
Address before American Association for Advancement of Education ---- 325
Æpinus, theory by, of electrical action ---- 301
Ætherial medium necessarily postulated ---- 257, 298
Aerial echoes, general conclusions respecting ---- 504
Aerial echo, observations on ---- 435, 454, 471, 502
Air, refrigeration of, by rarefaction ---- 1
Albany Institute, Transactions of, quoted ---- 1, 2, 3, 8, 37
Alcohol and water, experiments on alleged separation of ---- 360
Alexander, Prof. J. H., experiments by, on fog-whistles ---- 376, 377
Alexander, Prof. Stephen, co-operation of, in observations on magnetic intensity, at Albany, New York ---- 60
Alexander, Prof. Stephen, co-operation of, in observations on solar spots ---- 224
Alloys, diffusion of metals in ---- 229
Alternation in direction of induction currents of successive orders ---- 130
American Journal of Science cited ---- 60, 92, 449
Ampère on electro-magnetism ---- 305
Annals of Philosophy, aurora borealis described in ---- 68
Annals of Philosophy quoted ---- 3, 74
Antinori, Cav., experiment by, on the influence of a spiral conductor ---- 108
Apparatus used in observing magnetic intensity at Albany, New York ---- 60
Apparatus used in testing alleged spontaneous separation of alcohol and water ---- 362
Arago's induction by magnetic rotation ---- 123
Armature of magneto-electrical machine, effect of wire around ---- 117
Arrow, Sir Frederick, witness of abnormal phenomena of sound ---- 415, 421
Atomic constitution of matter postulated ---- 255, 298
Audibility, range of, increased by elevation ---- 458, 463, 472
Audition, continuance of, limited to about one-fifteenth second ---- 296
Aurora, connected with electrical action ---- 287
Aurora, disturbance of earth's magnetism by ---- 58
Aurora, observations on ---- 287
Aurora, observations on the height of ---- 260

Babbage's induction by magnetic rotation ---- 123
Bache, Dr. A. D., on investigations by Ettinghausen, of Vienna, in armature currents in magneto-electric machine ---- 144

PAGE.

Bache, Gen'l Hartman, inventor of automatic fog-whistle 376
Balloons, rubber toy, used to show direction of the upper winds 441, 442
Ball supported by fountain jet, explanation of 254
Barlow, Mr. Peter, experiments by, on electrical conduction through long wires 42, 47
Barnard, Gen'l J. G., co-operation of, in observations on sound 434
Barnard, Mr. T., co-operation of, in observations on sound 473
Bates, Gen'l, first director of gun-signals on California Coast 375
Battery currents and induced currents, difference in action of 128
Battery, galvanic, new arrangement of 80, 81, 82
Battery, galvanic, one element of 82
Battery, galvanic, plan of a convertible 84
Beck, Dr. L. C., assistance of, in electro-magnetic experiments 48
Becquerel's experiments on phosphorescence 204
Becquerel, theory by, of electricity in the thunder cloud 190
Bell-buoys as fog-signals 374
Biot, experiments on phosphorescence 205
Birds killed on telegraph wires 253
Block Island, observations at, on sound 434, 453
Boscovich's theory of the constitution of matter incomplete 256, 297
Boston light-station, observations at, on sound 434
Boussingault's theory of animal power 222
Brard's process of testing building stone with sulphate of soda 346
Brewster, Sir David, observations of, on color-blindness, etc. 233
British Association Report of 1856, cited 390
Brown, Mr. T., improvements by, in the siren as a fog-signal 424
Brown, Mr. T., patentee of the siren, co-operation of, in observations on sound 437, 453
Building materials, mode of testing 344, 347

Cactus steamer, used in observations on sound 437, 477, 483
Calland, observation by, on substituting bundle of wire for solid iron 126
Canton, experiment by, on phosphorescence 204
Cape Ann light-station, observations at, on sound 430
Cape Elizabeth light-station, Maine, observations at, on sound 428
Capillarity in solid metals, influence by their texture 146
Capillarity of metals, observations on 146, 228
Capillary transmission of liquids through solids 146
Case, Commodore Augustus L., co-operation of, in observations on sound 392
Cavendish, on electrical laws 301
Charles, experiment by, as to range of protection by lightning rod 195
Christie, Mr., observations of aurora borealis, in 1831 67
Chronograph for recording velocity of projectiles 212, 215
City of Richmond steamer, observations on, of abnormal phenomena of sound 426, 427

PAGE.
Civilization, a condition of unstable equilibrium ... 327
Clark, Mr. Henry W., observations by, on the aerial echoes ... 504
Clay balls in the fire employed to increase amount of heat ... 289
Close, Mr., inventor of automatic fog-bell ... 490
Cloud, induction from, exerted at great distances ... 199
Coast line United States, length of, requiring maritime lights and signals ... 365
Cohesion as strong in the liquid as in the solid form of matter ... 218, 353
Cohesion of liquids, experiments on ... 217, 218
Coil of copper ribbon, influence of, on self-induction ... 94, 97
Coil of wire when long, electrical effect of ... 112
Coils, influence of, on "intensity" of current ... 113
College of New Jersey, galvanic battery for ... 80
Color-blindness, hereditary predisposition to, noticed ... 238
Color-blindness, hypotheses for explanation of ... 239
Color-blindness, more common among men than women ... 238
Color-blindness, remarks on ... 233
Conductor, spiral, influence of, on galvanic current ... 108
Contributions to Electricity and Magnetism ... 80, 92, 108, 149, 200
Copper handles to conductors, effect of ... 88
Copper ribbon in flat spiral, intense spark from ... 87
Copper ribbon (silk wrapped), self-induction from ... 94
Copper ribbon, two strand, spark from ... 88
Courtenay, Mr., inventor of the whistling-buoy ... 487, 488
Cruickshanks trough, effect of, on induction ... 117
Currents by induction from ordinary electricity ... 105
Current, secondary effect of, upon human body ... 98
Current, self-induction of, effects of ... 106, 131
Currents of successive orders, alternation in direction of ... 130
Currents of various orders of induction from electric spark ... 107

Daboll fog-trumpet, description of ... 393, 397, 412
Daboll trumpet, experimental test of ... 384, 395, 406
Dalton, Dr., his experience in color-blindness ... 236
Daniell's battery employed in experiments ... 150, 153
Davis, Captain John L., co-operation of, in observations on sound ... 437
Davy, Sir H., observations of, on electro-magnetization through interposed plates ... 122, 126
Decomposition of liquid by current from long wire ... 88
Decomposition of liquid by single pair of galvanic plates ... 95
De la Rive's ring, modification of ... 5
Direction of induction currents of several orders, alternation in ... 130
Directions of successive orders of induction dependent on distance ... 137, 138, 143
Discovery and invention different ... 319, 320
Duane, Gen'l James C., co-operation of, in observations on sound ... 487
Duane, Gen'l James C., observations of, on sound ... 389, 404

PAGE.

Dufay's theory of electricity 302
Dumas' theory of animal power 222

Ear, artificial, an instrument for testing intensity of sound 379
Echo, aerial, probably reflection from the ocean surface 505, 510
Echo from the ocean observed at Block Island 435, 436, 454, 502
Echo, limit of distance required to distinguish 296
Education, address on 325
Education a forced condition, requiring exertion 326
Education to follow the order of development of the faculties 335
Edwards, Mr. Charles, co-operation of, in observations on sound 487
Electrical discharge by simultaneous opposite waves 241
Electrical discharge, divellent effects of 142
Electrical discharge, experiments on 240
Electrical fish, shocks from, probably secondary 160
Electrical induction from feeble battery 98
Electrical research, the lateral discharge 101
Electrical spark, most phosphorogenic at its extremities 205
Electricity and Magnetism, contributions to 80, 92, 108, 149, 200
Electricity, currents and sparks of, from magnetism 93
Electricity, free or redundant 101
Electricity, generation of, from steam 189
Electricity in the thunder-cloud, Becquerel's theory of 190
Electricity less dissipated when slowly thrown on wire 102
Electricity of different intensities, battery for producing 80
Electricity, ordinary and galvanic, induction of 108
Electricity, ordinary, induction currents from 105, 106
Electricity, the lateral discharge of 101, 105
Electric lights and calcium lights useless as fog-beacons 417
Electro-dynamic induction, conditions of 108, 111
Electro-magnetic apparatus, on some modifications of 3
Electro-magnetic apparatus, principle of Schweigger's galvanic "multiplier" applied to 37
Electro-magnetic engine 54
Electro-magnet of "intensity" adapted to a telegraph 42
Electro-magnet made for Yale College 50
Elevation, effect of, on range of audibility 458, 463, 472
Elliot, Major George H., commissioned to inspect European light-houses 368
Emmet, Professor, improvement on magneto-electric machines 91
Ericsson hot-air engine used with fog-trumpet 384
Ettinghausen, Professor, of Vienna, investigations on armature currents in magneto-electric machine 144
Evans, the introducer of the high-pressure locomotive 318

Faculties, the successive development of, the key to education 335
Faraday on effect at commencement and ending of current 132

PAGE.
Faraday on influence of spiral conductor ... 108
Faraday on screening effect of interposed plates ... 162
Faraday, reference by, to shock of electrical eel ... 161
Faraday's discovery of electricity from magnetism ... 74
Faraday's experiment on the magnetic polarization of light ... 243
Ferguson, Rev. Peter, observations by, on the effect of fog on sound ... 390
Fire, peculiar action of, on iron nails ... 224
Flame, notice of lecture on ... 2
Flame shown to be a bad conductor of heat ... 358
Fog, an obstruction to navigation ... 370, 416
Fog-bell, automatic, used as a signal at Whitehead Station ... 490
Fog, effect of, on sound disputed ... 390, 417
Fog, effect of, on sound wholly insignificant ... 418
Fog-signals, introduction of ... 374
Fog-signals nearly as important as light-houses ... 417
Fogs on the United States coasts, causes of ... 371
Forbes, James D., electric sparks from magnet ... 79
"Fountain-ball," explanation of ... 254
Fourfold state of matter assumed ... 258
Franklin, both a discoverer and an inventor ... 320
Franklin's theory of electricity ... 302
French writers, theory of thunder storms by ... 103
Fresnel, improvements by, in light-house illumination ... 367
Froissart, notice by, of the ocean echo ... 454

Galvanic battery, new arrangement of ... 80, 81, 82
Galvanic battery, plan of a convertible ... 84
Galvanic currents in adjacent conductors, used to produce motion ... 189
Galvanic "Multiplier" principle applied to electro-magnetic apparatus ... 37
Galvanometer deflection dependent on "quantity" rather than "intensity" of current ... 170
Gautier, M., investigation by, of the effect of solar spots on terrestrial temperature ... 225
Gruelin's Treatise on Chemistry, quoted ... 261
Goddard, Dr., observation of effective length of induction coil ... 116, 117
Guns as fog-signals ... 375

Habits, intellectual and moral, the most important part of education ... 339, 340
Haines, Major Peter C., co-operation of, in observations on sound ... 437
Handles, copper to conductors, effect of ... 88
Hansteen, Prof., magnetic needle from, used for observations on the aurora ... 60
Hansteen, Prof., observations by, on the effect of aurora borealis on the earth's magnetism ... 63

PAGE.

Hare, Dr. Robert, facilities for experiments afforded by ---- 139
Hare, Dr. Robert, theory by, of electrical action ---- 303
Harris, Dr. Snow, observations by, on screening influence of metals ---- 144
Harvey, Mr., observations by, on color-blindness ---- 236
Hauy, theory by, of electrical action ---- 301
Hays, Dr., observations by, on color-blindness ---- 237
Heat, and a thermal telescope for distant radiation ---- 283
Heat, experiments on the radiation of ---- 289
Heat from the moon not satisfactorily determined ---- 284
Heat, interference of ---- 254, 283
Heliostat, on a simple form of ---- 192
Helix of wire, vivid spark from ---- 87
Hereditary pre-disposition to color-blindness noticed ---- 238
Herschel, Sir John, observations by, of induction by magnetic rotation ---- 123
Herschel, Sir William, theory by, of solar spots ---- 224
Hoosac tunnel, New York, observations at, on sound ---- 499
Horsford, Prof. E. N., remarks on paper by ---- 288
Hot-air engine employed with fog-trumpet ---- 384, 385
Human body a complex machine operated like other machines ---- 312
Human soul not properly a motor ---- 312
Humphreys, Gen'l Andrew A., facilities afforded by ---- 392
Hypotheses less useful as explanations, than as directing or suggesting experiments ---- 259
Hypotheses when even erroneous may be useful ---- 298, 300

"Imponderables," on the theory of ---- 297
Induced currents and battery currents, difference in action of ---- 128
Induced currents from ordinary electrical discharge ---- 105, 133
Induction apparently propagated by undulations ---- 302
Induction at a distance of many feet ---- 121, 139, 161, 203
Induction coils with interposed plates ---- 123
Induction currents, alternation of successive orders of ---- 159
Induction currents, directions of, dependent on distance ---- 137, 138, 143
Induction currents, initial and terminal, similar ---- 158
Induction currents of third, fourth, and fifth orders ---- 107, 127, 135
Induction from increasing and from diminishing currents, opposite in sign ---- 170
Induction of current on itself, effects of ---- 106, 131
Induction of electrical cloud exerted at great distances ---- 199, 251
Inductive effects diminished by too great length of coil ---- 116
Inductive effects probable in thunder-storms ---- 140
Inertia not accounted for by the theory of Boscovich ---- 256, 298
Ingersoll, Gov. Charles R., co-operation of, in observations on sound ---- 433
"Intensity" and "quantity" currents from same induction ---- 118
"Intensity" and "quantity" of induced currents compared ---- 113, 115

PAGE.
"Intensity" battery productive of stronger initial induction 152
"Intensity" current induced by one of "quantity" 118, 157
"Intensity" currents of induction from wire helices 151
Interference of heat 283
Iris, steamer, used in observations on sound 487
Iron in axis of flat spiral, effect of 96
Invention different from discovery 319, 320
Ivory, James, on the theory of liquid cohesion and capillarity 218

Joslin, Professor, account of aurora borealis, 19th April, 1831 64

Keeney, Captain, of light-house steamer *Mistletoe*, assistance of, in observations on sound 388, 391, 453

Lateral discharge of electricity increased by self-induction 102
Lateral discharge of free electricity from fine wire 101
Lead having mercury transmitted through it by capillarity 146
Lead, sheet, between building stones a source of weakness 348
Lecture on flame, notice of 2
Lederle, Mr., co-operation of, in observations on sound 379, 392, 421
Lenz, observation on different armature coils of magneto-electric machine 117
Leyden jar, on the phenomena of 293
Liebig's theory of animal power 222
Light, experiment on magnetic polarization of 243
Light-houses, beacons, and signals in the United States 366
Light-house Board Report quoted 426, 447, 487
Light-house Board, United States, organization of 366
Light-house protection from lightning 498, 499
Light-house service of the United States 364
Lightning flash magnetizing needles at the distance of many miles 203
Lightning, personal experience with 196
Lightning, protecting light-houses from 498, 499
Lightning, protection of houses from 231
Lightning, relation of telegraph lines to 244
Lightning-rod, range of protection by 195
Lightning-rods, on the forms of 291
Lightning, some effects of, due to induction 140
Lightning strokes, expansive energy of 232, 290
Little Gull Island, observations at, on sound 433, 469
Liquid cohesion equal to solid cohesion 218, 353
Liquid, decomposition of, by current from long wire 88
Liquid, decomposition of, by single pair of galvanic plates 95
Liquids, experiments on the cohesion of 217, 218
Liquidity the result of mobility, not of diminished cohesion 353, 354
London and Edinburgh Journal of Science quoted 92
Loomis, Prof. Elias, remarks on paper by 291

PAGE

Machines, classification of the functions of ... 314
Magnet (electro-) with long coil, adapted to telegraph ... 42
Magnet (large electro-) made for Yale College ... 50
Magnetic attraction and repulsion, reciprocating motion produced by ... 54
Magnetic intensity at Albany, New York, observations on ... 59
Magnetic polarization of light ... 343
Magnetic power in soft iron, developed with a small galvanic element ... 37
Magnetism and Electricity, contributions to ... 80, 92, 108, 149, 200
Magnetism electrical induction from ... 74, 78, 114
Magnetism of the earth disturbed by aurora borealis ... 58
Magnetism, production of currents and sparks of electricity from ... 73
Magnetizing of needles by lightning many miles distant ... 203
Magneto-electricity, discovery of, by Faraday ... 74, 78, 114
Magneto-electric machine, effect of wire around armature ... 117
Magneto-electric machine, improvement in, by Prof. Ettinghausen 144
Magneto-electric machine, induction currents of several orders from 144
Masson, experiment by, on the influence of a spiral conductor ... 108
Masson's suggestion as to induction shocks from electrical fish ... 160
Matter, four states of, assumed ... 258
Matter, the atomic constitution of ... 255, 298
Matteucci, experiments by, on phosphorescence ... 205
Mechanical arts, on the improvement of ... 306
Mechanics' Institute of Washington, address to ... 306
Memory to be early cultivated and stored ... 335
Mercury transmitted by capillarity through lead ... 146
Metallic roofs, protection of, from lightning ... 231
Metals, capillarity of, observed ... 146, 228
Metals, diffusion of, in alloys ... 229
Metals weakened by hammering ... 355
Metals weakened interiorly by stretching ... 354
Meteorology, etc., application of thermo-galvanometer to ... 215
Mistletoe steamer used in observations on sound ... 434, 437, 453, 458
Molecular cohesion, observations on ... 352
Moll, Prof. G., paper by, on electro-magnetism ... 49
Monhegan Island, observations at, on sound ... 498
Moon, heat from, uncertainty of ... 284
Motor from galvanic currents in conductors ... 189
Motors (natural) origin and classification of ... 220, 221, 311
Myrtle, steam-tender, used in observations on sound ... 426

Nails, iron, peculiar action of fire on ... 224
New Haven, experiments near, on sound and fog-signals, in 1865 ... 378
Newton, Sir Isaac, ring magnet worn by ... 48
Newton's theory of light burdened with supplementary hypotheses 299
Newton's theory of the constitution of matter ... 298
New York, Topographical sketch of ... 8

PAGE.

Ocean-echoes, probably curved sound-beams reflected from the ocean surface 505, 510
Ocean-echo observed at Block Island 435, 454, 502
Oil, stratum of, on connecting mercury, found to increase the electric shock 187
Old Anthony Station, observations at, on whistling-buoys 487, 488
Olmstead, Prof. D., remarks on paper by 290
Organization of Smithsonian Institution, Programme of 263
Oscillation of electrical discharge 201
Owen's investigations on disintegration of building stones 346

Page, Dr. Charles G., experiment by, on stratum of oil over connecting mercury 187
Page, Dr. Charles G., on influence of spiral conductor 108
Patterson, Dr., reference by, to generation of electricity from steam 189
Permeability of transparent solids and liquids to phosphorescence 210, 211
Philosophical Magazine and Annals, quoted 74
Philosophical Society of Washington, Bulletin of, quoted 416
Picking, Commander H. F., co-operation of, in observations on sound 487
Phosphorogenic emanations as derived from electrical spark 205
Phosphorescence effected by polarized electrical light 207
Phosphorescence, experiments on 191, 204
Phosphorescence subject to refraction and dispersion 207
Phosphorescence unequally permeable through different transparent bodies 210, 211
Plateau, Prof., explanation by, of accidental colors 235
Plates, effect of, interposed between electrical induction rings 123
Poe, Gen'l O. M., co-operation of, in observations on sound 392
Poisson, explanation by, of liquid cohesion and capillarity 218
Polarization of light, magnetic 343
Portland, Maine, phenomena of sound observed at 389
Powell, Commodore L. M., co-operation of, in experiments on sound 378, 379, 421
Prevost, observations of, on color-blindness 237
Programme of Organization of the Smithsonian Institution 263
Projectiles (velocity of), a method of determining 212
Putnam, steamer, used in observations on sound 433, 437, 458, 465

"Quantity" and "Intensity" currents from same induction 118
"Quantity" and "Intensity" of induced currents compared 113, 115
"Quantity" battery productive of stronger terminal induction 153
"Quantity" current induced by one of "intensity" 118, 157
"Quantity" currents of induction from ribbon coils 151

Radiation from burning coal increased by mingling with it incombustible substances 289, 355, 357

PAGE.
Radiation of heat, experiments on 289, 355
Radiation (relative) of solar spots, observations on 224
Rarefaction of air, refrigeration by 1
Reciprocating motion produced by magnetic attraction and repulsion 54
Reflectors for projecting sound, trifling effect of 380, 381, 405, 409
Refraction of sound the principle cause of its anomalies .. 445, 446, 449, 507
Refrigeration by rarefaction of air 1
Rennie's apparatus for testing strength of building stones 347, 348
Reversal of magnetic polarity due to oscillatory discharge 202
Reynolds, Prof. Osborne, observations by, on sound noticed 446, 484
Ribbon, copper, in a flat spiral effective in increasing electrical spark 87, 94, 97
Richardson, Dr., observations by, on effect of aurora borealis on magnetic needle 59
Ritchie, Mr., observation by, on law of galvanometer action 41
Robison, theory by, of electrical action 301
Rocker fog-whistle 383
Roper hot-air engine, experiments with, for fog-trumpets 385
Rumford (Count) observations by, on increasing heat of fuel by incombustible substances 289, 355
Sabine, Capt., magnetic needle from, used for observations on the aurora 60
Sandy Hook, observations near, on sound 391, 437
Savary's observations on reversed polarity of magnetized needles .. 144, 200
Saxton, Joseph, observations by, on different armature coils of magneto-electrical machines 117
Schæffer, Prof. George C., co-operation of, in experiments on alleged spontaneous rectification of alcohol 362
Schweigger's galvanic "Multiplier," adaptation of 4, 37
Screening effect of interposed plate, experiments on 163, 165
Secchi, Prof. Angelo, remarks on aurora borealis by 287
Seebeck, observations by, on color-blindness 237
Self-induction on a long wire 79, 87, 141
Selfridge, Captain, witness of abnormal phenomena of sound 415
Shepard, C. U., experiment by, with large electro-magnet made for Yale College 50
Shock, direct and lateral method of receiving 88
Signals and beacons to aid navigation, number of, in the United States 366
Siren and Daboll trumpet compared 395, 406
Siren trumpet as a fog-signal 393, 412, 424
Smithson, James, bequest by, to found a scientific institution commented on 264
Smithsonian Institution, practical operations of 284
Smithsonian Institution, Programme of Organization of 263
Soap-bubble, contractile force of 219

PAGE.
Solar spots, heat radiated from, less than from surrounding parts.. 224, 226
Solar spots, apparatus employed in observing 225
Soul of man not the motive power of his body 312
Sound, abnormal phenomena of, noticed in the cannonading of battle 420
Sound-beams very divergent at a distance 388, 421
Sound, direct and reflected, limit of perception between 295
Sound distinctly heard against the wind indicative of a change in its direction 436
Sound, general conditions of audibility of 506
Sound greatly influenced by reversed upper currents of air 423
Sound, intermittent range of 409, 496, 497
Sound, penetration of, dependent on quantity rather than intensity 490
Sound propagated to great distances along the surface of water ... 400
Sound reflectors of little use for distant propagation 381, 405, 409
Sound, refraction of, by favoring or adverse currents of wind 449, 507
Sound sometimes heard at greater distance against the wind 389, 404
Sound, tendency of, to rise against adverse wind 388
Sound, the range of, influenced by the direction of the wind.. 464, 467, 475
Spark increased from flat spiral of copper ribbon 87, 88
Spark, etc., from long conductor, observations on 87, 103
Spark, intense, on short wire connecting large flat spiral 88
Spiral conductor, increase of self-induction through 89, 92, 95, 97
Spiral conductor, influence of, on galvanic current 108
Spiral, flat, of copper ribbon, strong sparks from 87
Spots (solar), observations on their relative radiation 224
Spurzheim, observations by, on color-blindness 235
Steam, chemical and mechanical effects of 1
Steam, electricity from 189
Steam-pressures, and relative distances of fog-signal sounds 398
Steam-whistle and trumpet compared for penetration 402
Steam-whistle as a fog-signal 382, 394, 406
Stevens, Mr., application by, of clock-escapement for fog-bells 374
Stimpson, Dr. William, observations by, on fog 372, 373
Stokes, Prof. Geo. G., theory by, of the action of wind on sound 390, 414, 419, 448
Sturgeon, Mr. William, improvement by, in electro-magnetic apparatus 3, 7, 37
Sturgeon, Mr. William, observation by, on alternation of successive orders of induction currents 159
Sturgeon, Mr. William, observations by, on different armature coils of magneto-electric machine 117
Sturgeon, Mr. William, observation by, on substituting bundle of wire for solid iron 126
Successive induction currents 127
Successive orders of induction from ordinary electrical discharge.. 135

PAGE.

Taylor, Mr. W. B., illustrations by, of the refraction of sound 449, 450
Telegraph, electro-magnetic, practicable with an "intensity" magnet 42
Telegraph lines, relation of, to lightning 244
Telegraph wires largely affected by induction 251
Temperature and radiation of flame not proportional 358
Ten Eyck, Dr. Philip, co-operation of, in experiments in developing electro-magnetism 40
Ten Eyck, Dr. Philip, independent experiments by, in developing magnetism in soft iron 47
Tertiary induction current, nature of 181
Testing building materials 344
Thermo-galvanometer, application of, to meteorology, etc. 215
Thermo-galvanometer applied to detect distant radiation 283
Thunder-cloud, Becquerel's theory of 190
Thunder-storms, on the effects of 193
Thunder-storms, theory of, by some French writers 103
Topographical sketch of New York 8
Transparent bodies differently permeable to phosphorescence 210, 211
Transverse re-action of galvanism and magnetism, difficult of explanation 305
Trenchard, Commodore Stephen D., co-operation of, in observations on sound 433
Trinity House investigations in sound, by Dr. John Tyndall 444, 451
Trumpet, relative audibility of, in different directions 402
Trumpets of different materials and shapes compared 401
Tuberville, Dr., observations by, on color-blindness 235
Tyndall, Dr. John, hypothesis by, of hygroscopic "flocculence" obstructing sound 445, 451, 485
Tyndall, Dr. John, observations by, on the aerial echo 454, 457
Tyndall, Dr. John, present at meeting of Philosophical Society of Washington 416

Upshur, Captain John H., co-operation of, in observations on sound 433
Ure, Dr., experiment by, with galvanism on body of dead malefactor 160

Van Marum, luminosity of electrified wire observed by 102
Vapor, remarks on the diffusion of 288
Variable range of sound 409
Velocity of projectiles, a method of determining 212
Vessels employed for observing sound in different directions 433, 434, 437, 453, 458, 465, 477, 483
Vision, defective, prevalence of 233
Vitality not a mechanical force 223

Wade, Major, machine devised by, for testing strength of materials 348
Walker, Commander John G., co-operation of, in observations on sound 426

PAGE.
Wartmann, Elie, memoir of, on color-blindness 233, 239
Water surface favorable to the transmission of sound 400
Watkins, Mr., application by, of a spiral conductor in hydro-electricity 108
Watt, James, both a discoverer and an inventor 320
Webb, Captain, witness of abnormal phenomena of sound 415, 421
Welling, Dr. James C., co-operation of, in observations on sound 453, 460, 465
Whaley, Mr. Joseph, observations by, on the aerial echoes 504
Whitehead Light-station, observations at, on sound 426, 490
Wilcox, hot-air engine, experiments with, for fog-trumpets 386
Wind at upper regions observed by toy balloons of rubber 441, 442
Wind, effect of, on range of sound 418, 419, 464, 467, 475
Wind, effect of reversed upper and lower currents on sound 423
Wind stronger in upper regions than near the surface of the earth 390
Wind when adverse causing sound to rise 388
Wire helix, vivid spark from 87
Wires of unequal diameter, effect of self-induction on 94
Wire on armature of magneto-electrical machine, effect of 117
Wire, the length of, in relation to size of battery plates 113
Woodruff, Gen'l I. C., co-operation of, in observations on sound .. 434, 437, 453, 461, 465, 473
Woodruff, Mr. Edward L., co-operation of, in observations on sound 453, 487

Yale College, electro-magnet made for 50
Young, Dr. Thomas, plan by, of a simple heliostat 192
Young, Dr. Thomas, theory by, of liquid cohesion and capillarity 218