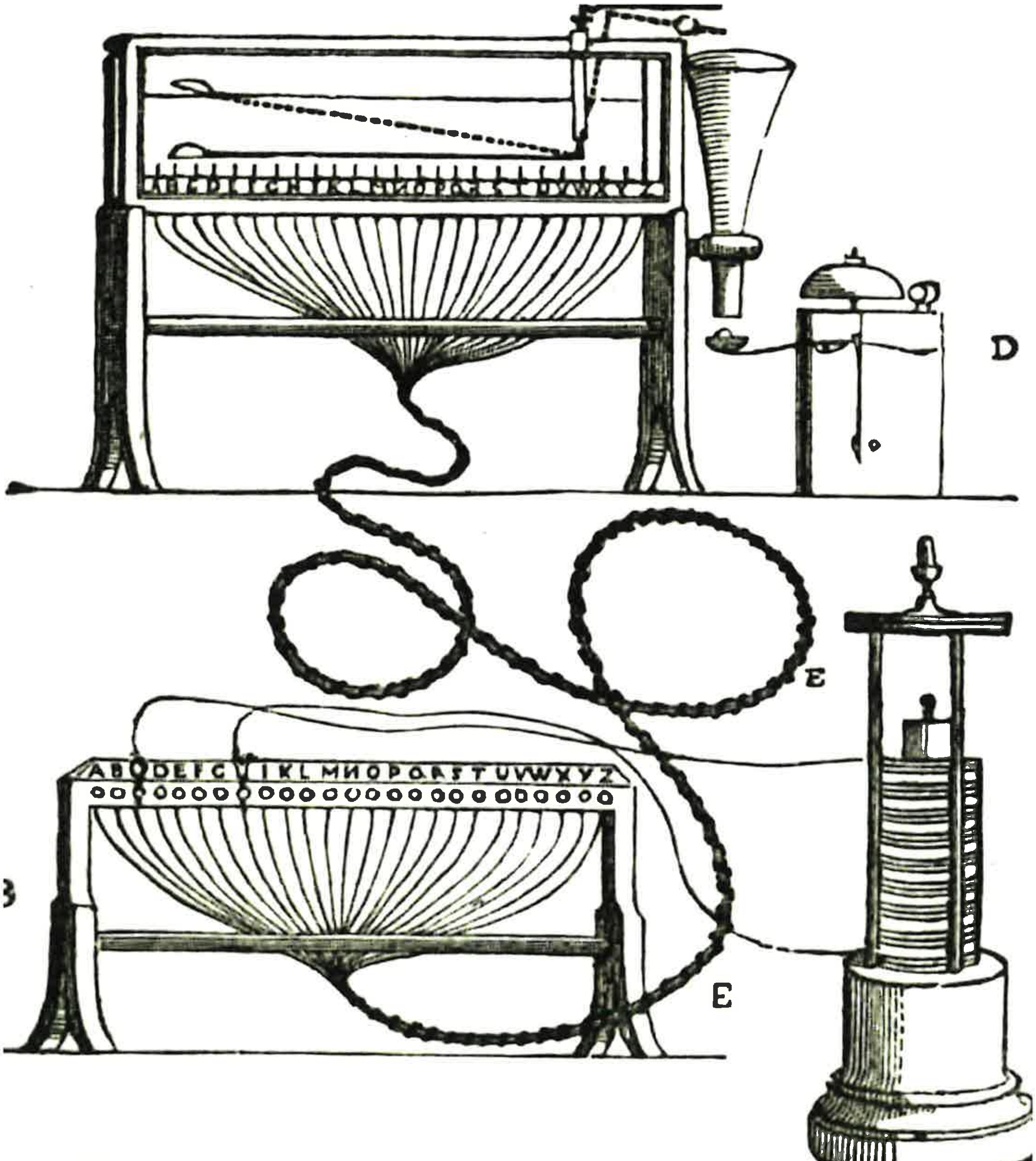

A history of electric telegraphy, to the year 1837

John Joseph Fahie

A HISTORY

OF

ELECTRIC TELEGRAPHY,

TO THE YEAR 1837.

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E. & F. N. Spon; London & New York.

Frontispiece.

See page 129.

A HISTORY

OF

ELECTRIC TELEGRAPHY,

TO THE YEAR 1837.

CHIEFLY COMPILED FROM ORIGINAL SOURCES, AND HITHERTO UNPUBLISHED DOCUMENTS.

BY

J. J. FAHIE,

MEMBER OF THE SOCIETY OF TELEGRAPH-ENGINEERS AND ELECTRICIANS, LONDON; AND OF THE INTERNATIONAL SOCIETY OF ELECTRICIANS, PARIS.

"Their line is gone out through all the earth,
And their words to the end of the world."
Psalms xix. 4.

LONDON:
E. & F. N. SPON, 16, CHARING CROSS.
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1884.

Dedicated

TO

LATIMER CLARK, Esquire,

M.I.C.E., F.R.G.S., F.M.S., PAST PRES. S.T.E. AND E.,

IN ACKNOWLEDGMENT OF MANY KINDNESSES,

BY HIS OBLIGED FRIEND,

THE AUTHOR.

London, *February* 1884.

PREFACE.

PLUTARCH, in the opening sentences of his Life of Demosthenes, says: — "Whosoever shall design to write a history, consisting of materials which must be gathered from observation, and the reading of authors not easy to be had nor writ in his own native language, but many of them foreign and dispersed in other hands: for him it is in the first place and above all things most necessary to reside in some city of good note and fame, addicted to the liberal arts, and populous, where he may have plenty of all sorts of books, and, upon inquiry, may hear and inform himself of such particulars as, having escaped the pens of writers, are yet faithfully preserved in the memories of men; lest otherwise he publish a work deficient in many things, and those such as are necessary to its perfection."

Had we seen this passage a few years ago, the following pages had, probably, never been written, and there would be no need for this preface. The work was begun and brought to a very forward state, not in some city of good note and fame, where plenty of books were to be had, but in what has been rightly called "the confines of the earth—the hot regions of Persia," and under circumstances which, we think, will bear relating.

In our youthful days we contracted two habits, which have been ever since the bane or the solace (we hardly know which to call them) of our existence, *viz.*, a taste for writing, and a taste for scraps. The *Cacoëthes Scribendi* first attacked us, and we can recall letters in the local papers on various topics of local interest, all of which were written early in our teens. When about sixteen years of age we commenced a history of the old castles and churches which abound (in ruins) in and about our native place, the said history being intended to serve also as a guide for tourists who were constantly visiting the neighbourhood. With great industry we got together, in time, some two hundred pages (foolscap) of writing; but the work was never completed. For years we hawked the MS. about, latterly never looking at it, having come to regard it as a standing reproach for time and money misspent; and at last, in a fit of remorse, we gave the papers to the flames in 1875.

Soon after joining the telegraph service, in 1865, our archæological bent took another turn, and we now began to collect books and scraps on electricity, magnetism, and their applications—particularly to telegraphy, and with the same industrious ardour as before. In December 1867, we entered the Persian Gulf Telegraph Department under the Government of

India, where, having a good deal of spare time on our hands, we indulged our habits to the full. In 1871, having amassed a large number of notes, scraps, &c., on submarine telegraphy, we began a work on the history and working of the Persian Gulf cables, of which we had then had over three years' practical experience.

Gradually this developed itself into an ambitious treatise, which we styled "Submarine Telegraphs, their Construction, Submersion, and Maintenance, including their Testing and Practical Working." Of this some three hundred pages (foolscap) are now lying "submerged" in the depths of our trunk, to be, perhaps, "recovered" at some future day—if, haply, they do not share the fate of our History of Ruins!

Unfortunately for us, at least from a book-selling point of view, our old taste for archæology, after lying dormant for years, reasserted itself, and, about six years ago, we found ourselves in the design of writing a history of telegraphy from the time of Adam down to our own! For this we had a pile of notes and paper cuttings—the accumulation of a dozen years, but few books (books are heavy and awkward baggage for one of our necessarily semi-nomadic life). However, with our materials we built up a tolerably fleshy skeleton (if we may so speak), which, on our arrival in England at the close of 1882, after nearly fifteen years' absence, we showed to some friends.

They advised us to fill up the gaps and bring out our book immediately. The first was easy of accomplishment, with the use of the splendid technical libraries of Mr. Latimer Clark and of the Society of Telegraph-Engineers and Electricians, and with an occasional reference to the British Museum; but to find a publisher, *that* was not so easy. Publishers, now as always, fight shy of Dryasdust, and the two or three whom we tried asked us to bring them something new, for, owing to the machinations of us, Electrical Engineers, the world was going at lightning speed, and had no time to look back.

Ultimately we paid a visit to the Editor of *The Electrician*, told him of our discomfiture, showed him our MSS., and repeated an offer that we had made him years before, from Persia, but which he then declined, *viz.*, to publish our articles from week to week in his paper. The Editor did not take long to decide; he would only, however, accept the electrical portion, the non-electric part which deals with fire-, flag-, and semaphore- signalling, acoustic, pneumatic, and hydraulic telegraphs, &c., &c., being, he said, unsuited for his journal. On the principle that half a loaf is better than no bread, we concluded arrangements there and then, and parted with our new-found friend with feelings which time has but intensified.

The present volume is a collection, with very few alterations, of the articles which have regularly appeared in *The Electrician* for the last twelve months. Of these alterations the only ones worth mentioning will be found in our chapters on Mr. Edward Davy; we have made our account of his electro-chemical recording telegraph a little fuller, and have added some new matter lately acquired (1) from recent letters of Mr. Davy himself, (2) from an examination of the private papers of the late Sir William Fothergill Cooke—a privilege for which we are indebted to our kind friend, Mr. Latimer Clark, and (3) from Mr. W. H. Thornthwaite, of London, an old pupil of Edward Davy, whose very interesting reminiscences, we feel sure, will be scanned with pleasure by all our readers.

Now as to the plan of the work. We have divided the history of electricity into three parts, (1) static, or frictional, electricity, (2) dynamic, or galvanic, electricity, and (3) electro-magnetism and magneto-electricity. We have brought our account of each part down to the year 1837, confining ourselves to a notice of such facts and principles only as are employed in the various telegraphic proposals that follow. These, in their turn, are divided into three classes, electrical, galvanic (chemical), and electro-magnetic; and each class, treated chronologically, follows naturally the corresponding part of the history of electricity. The whole is preceded by a full account of what we have called a *foreshadowing of the electric telegraph*, and is followed by an appendix, containing (A) a clear and correct statement of Professor Joseph Henry's little-known connection with electric telegraphy, which is too important to be omitted, but for which we could not conveniently find room in the body of the work, and (B) a few pages supplementary of our chapters on Edward Davy.

In limiting ourselves to the year 1837, we have done so advisedly, for, to attempt even the barest outline of what has been accomplished since then would occupy volumes. Our object has been, as it were, to make a *special* survey of a river from its rise away in some tiny spring to its mouth in the mighty ocean, marking down, as we came along, those of the tributary streams and such other circumstances as specially interested us. Arrived at the mouth, the traveller who wishes for further exploration has only to chose his pilot; for, fortunately, there is no lack of these. We have Highton, Lardner, Sabine, and Culley in England; Shaffner, Prescott, and Reid in America; Moigno, Blavier, and du Moncel in France; Schellen, and Zetzsche in Germany; Saavedra in Spain, and many others in various parts of the world whose names need not be specially mentioned.

As we have in the body of the work given full references for every important statement, it will not be necessary to acknowledge here the sources of our information; indeed it would be simply impossible to do so within the limits of a preface which we feel is already too long. Like Molière, we have taken our materials wherever we could find them, and it is no exaggeration to say that in pursuit of our subject we have laid many hundreds of volumes under tribute; some have given us clues, some have been mines of wealth, others have yielded nothing at all, while, what was worse, a goodly number were of the *ignis fatuus* kind—false accounts, false dates, false references, false everything—which worried us considerably, and over which we lost much precious time.

We gladly, however, take this opportunity of thanking Messrs. Ispolatoff (Russia), D'Amico (Italy), Aylmer (France), Sömmerring (Germany), and Collette (Holland), for their assistance, of which, as they will see, we have made good use in the text. To our friend, Mr. Latimer Clark, our debt is too heavy for liquidation and must remain. He has not only given us the free use of his magnificent library, but has aided and encouraged us with his advice and sympathy, and, in the most generous manner, has placed at our disposal all his private notes. These, we need hardly say, have been of great use to us, and would have been of greater still had we seen them at an earlier stage of our researches.

As we have to return almost immediately to "the confines of the earth," the preparation of the index has been kindly undertaken by our friend, Mr. A. J. Frost, Librarian of the Society of Telegraph-Engineers and Electricians, whose name will be a sufficient guarantee for the accuracy and completeness of the work. In tendering him our cordial thanks for this assistance, we have much pleasure in recording our appreciation of the zeal, ability, and unvarying courtesy with which he performs the duties of his office. His bibliographical knowledge is great and *special*, and has at all times been freely placed at our disposal.

Our book, we hope, will give the *coup de grâce* to many popular errors. Thus, we show that Watson, Franklin, Cavendish, and Volta did not suggest electric telegraphs (pp. 60, 66, and 82); that Galvani was not the first to observe the fundamental phenomenon of what we now call *galvanism* (pp. 175–9); that his experiments in this field were not suggested by a preparation of frog-broth (pp. 180–3); that not Daniell but Dobereiner and Becquerel first employed two-fluid cells with membranous or porous partitions (p. 215); that not Sömmerring but Salvá first proposed a galvanic (chemical) telegraph (p. 220); that not Schilling but Salvá first suggested a submarine cable (p. 105); that Romagnosi did not discover electro-magnetism (p. 257); that not Ritter but Gautherot first described the secondary battery (p. 267); that not Cumming nor Nobili but Ampère first invented the astatic needle (p. 280); that not Seebeck but Dessaignes first discovered thermo-electricity (p. 297); that not Thomson but Gauss and Weber first constructed the mirror galvanometer (p. 319); that the use of the earth circuit in telegraphy was clearly and intelligently suggested by an Englishman long before Steinheil made his *accidental* discovery of it (p. 345); and that not Cooke and Wheatstone, nor Morse, but

Henry in America and Edward Davy in England first applied the principle of the relay—a principle of the utmost importance in telegraphy (pp. 359, 511, and 515).

There may be some amongst our readers who will not thank us for upsetting their belief on these and many other points of lesser importance, and who may even call us bad names, as did Professor Leslie on a former occasion, and *à propos* of somebody's quoting Swammerdam's and Sulzer's experiments (pp. 175 and 178) as suggestive of galvanism. Leslie says:— "Such facts are curious and deserve attention, but every honourable mind must pity or scorn that invidious spirit with which some unhappy jackals hunt after imperfect and neglected anticipations with a view of detracting from the merit of full discovery" (*Ency. Brit.*, 8th edition, vol. i. p. 739). For our part we can honestly say that in drawing up our history we have not been influenced by any such views; our sole object has been to tell the truth, the whole truth, to

> "nothing extenuate,
> Nor set down aught in malice."

It is possible, however, that with the best intentions we may, either by omission or commission, be guilty of some unfairness; and if our readers will only show us wherein we have transgressed, we will be ready to make the *amende* if they will kindly afford us an opportunity—in a second edition.

We began our preface with an apology, we will end it with an appeal. We borrowed the one from Plutarch, Newton shall supply the other. At the close of the preface to his immortal *Principia* he says:—"I earnestly entreat that all may be read with candour, and that my labours may be examined not so much with a view to censure as to supply their defects."

THE AUTHOR.

LONDON, *February* 1884.

CONTENTS.

CHAPTER I.

CHAPTER II.

CHAPTER III.

CHAPTER IV.

CHAPTER V.

CHAPTER VI.

b

CHAPTER VII.

CHAPTER VIII.

CHAPTER IX.

CHAPTER X.

CHAPTER XI.

CHAPTER XII.

CHAPTER XIII.

CHAPTER XIV.

CHAPTER XV.

CHAPTER XVI.

CHAPTER XVII.

A

HISTORY

OF

ELECTRIC TELEGRAPHY

TO THE YEAR 1837.

CHAPTER I.

FORESHADOWING OF THE ELECTRIC TELEGRAPH.

> "Whatever draws me on,
> Or sympathy, or some connatural force,
> Powerful at greatest distance to unite,
> With secret amity, things of like kind,
> By secretest conveyance."
>
> Milton, *Paradise Lost*, x. 246. 1667.

AMONGST the many flights of imagination, by which genius has often anticipated the achievements of her more deliberate and cautious sister, earth-walking reason, none, perhaps, is more striking than the story of the sympathetic needles, which was so prevalent in the sixteenth, seventeenth, and eighteenth centuries, and which so beautifully foreshadowed the invention of the electric telegraph.* This romantic tale had

* "In the dream of the Elector Frederick of Saxony, in 1517, the curious reader may like to discern another dim glimmering, a more shadowy foreshadowing, of the electric telegraph, whose hosts of iron

B

reference to a sort of magnetic telegraph, based on the sympathy which was supposed to exist between needles that had been touched by the same magnet, or loadstone, whereby an intercourse could be maintained between distant friends, since every movement imparted to one needle would immediately induce, by sympathy, similar movements in the other. As a history of telegraphy would be manifestly incomplete without a reference to this fabulous contrivance, we propose to deal with it at some length in the present chapter.

For the first suggestions of the sympathetic needle telegraph we must go back a very long way, probably to the date of the discovery of the magnet's attraction for iron. At any rate, we believe that we have found traces of it in the working of the oracles of pagan Greece and Rome. Thus, we read in Maimbourg's *Histoire de l'Arianisme* (Paris, 1686)* :—

---

and copper 'pens' reach to-day the farthest ends of the earth. In this strange dream Martin Luther appeared writing upon the door of the Palace Chapel at Wittemburg. The pen with which he wrote seemed so long that its feather end reached to Rome, and ran full tilt against the Pope's tiara, which his holiness was at the moment wearing. On seeing the danger, the cardinals and princes of the State ran up to support the tottering crown, and, one after another, tried to break the pen, but tried in vain. It crackled, as if made of iron, and could not be broken. While all were wondering at its strength a loud cry arose, and from the monk's long pen issued a host of others."—*Electricity and the Electric Telegraph*, by Dr. George Wilson, London, 1852, p. 59; or D'Aubigné's *History of the Reformation*, chap. iv. book iii.

* English translation of 1728, by the Rev. W. Webster, chap. vi.

"Whilst Valens [the Roman Emperor] was at Antioch in his third consulship, in the year 370, several pagans of distinction, with the philosophers who were in so great reputation under Julian, not being able to bear that the empire should continue in the hands of the Christians, consulted privately the demons, by the means of conjurations, in order to know the destiny of the emperor, and who should be his successor, persuading themselves that the oracle would name a person who should restore the worship of the gods. For this purpose they made a three-footed stool of laurel in imitation of the tripos at Delphos, upon which having laid a basin of divers metals they placed the twenty-four letters of the alphabet round it; then one of these philosophers, who was a magician, being wrapped up in a large mantle, and his head covered, holding in one hand vervain, and in the other a ring, which hung at the end of a small thread, pronounced some execrable conjurations in order to invoke the devils; at which the three-footed stool turning round, and the ring moving of itself, and turning from one side to the other over the letters, it caused them to fall upon the table, and place themselves near each other, whilst the persons who were present set down the like letters in their table-books, till their answer was delivered in heroic verse, which foretold them that their criminal inquiry would cost them their lives, and that the Furies were waiting for the emperor at Mimas, where he was to die of a horrid kind of death [he was subsequently burnt alive by the Goths]; after which the enchanted ring turning about again over the letters, in order to express the name of him who should succeed the emperor, formed first of all these three characters, TH E O; then having added a D to form THEOD the ring stopped, and was not seen to move any more; at which one of the assistants cried out in a transport of joy, 'We must not doubt any longer of it; Theodorus is the person whom the gods appoint for our emperor.'"

If, as it must be admitted, the *modus operandi* is not here very clear, we can still carry back our subject to the same early date, in citing an experiment on magnetic attractions which was certainly popular in the days of St. Augustine, 354–430.

In his *De Civitate Dei*, which was written about 413, he tells us that, being one day on a visit to a bishop named Severus, he saw him take a magnetic stone and hold it under a silver plate, on which he had thrown a piece of iron, which followed exactly all the movements of the hand in which the loadstone was held. He adds that, at the time of his writing, he had under his eyes a vessel filled with water, placed on a table six inches thick, and containing a needle floating on cork, which he could move from side to side according to the movements of a magnetic stone held under the table.*

Leonardus (Camillus), in his *Speculum Lapidum*,

* Basileæ, 1522, pp. 718–19.

&c., 1502, *verbo* MAGNES, refers to this experiment as one familiar to mariners, and Blasius de Vigenere, in his annotations of Livy, says that a letter might be read through a stone wall three feet thick, by guiding, by means of a loadstone or magnet, the needle of a compass over the letters of the alphabet written in the circumference.*

From such experiments as these the sympathetic telegraph was but a step, involving only the supposition that the same effects might be possible at a greater distance, but when, or by whom, this step was first taken it is now difficult to say. It has been traced back to Baptista Porta, the celebrated Neapolitan philosopher, and in all probability originated with him ; for in the same book in which he announces the conceit he describes the above experiment of St. Augustine, and other "wonders of the magnet"; adding that the impostors of his time abused by these means the credulity of the people, by arranging around a basin of water, on which a magnet floated, certain words to serve as answers to the questions which superstitious persons might put to them on the future.†

* *Les Cinq Premiers Livres de Tite Live*, Paris, 1576, vol. i. col. 1316.

† While it is generally admitted that magnetism has conferred incalculable benefits on mankind (witness only the mariner's compass), we have never yet seen it stated that it has at the same time contributed more to our bamboozlement than any other, we might almost say all, of the physical sciences. With the charlatans in all ages and nations, its mysterious powers have ever been fruitful sources of imposture, sometimes harmless, sometimes not. Thus, from the iron crook of the

He then concludes the 21st chapter with the following words, which, so far as yet discovered, contain the first clear enunciation of the sympathetic needle telegraph:—"Lastly, owing to the convenience afforded by the magnet, persons can converse together through long distances." * In the edition of 1589 he is even more explicit, and says in the preface to the seventh book: "I do not fear that with a long absent friend, even though he be confined by prison walls, we can communicate what we wish by means of two compass needles circumscribed with an alphabet."

The next person who mentions this curious notion was Daniel Schwenter, who wrote under the assumed name of Johannes Hercules de Sunde. In his *Steganologia et Steganographia*, published at Nürnberg in 1600, he says, p. 127:—"Inasmuch as this is a wonderful secret I have hitherto hesitated about divulging it, and for this reason disguised my remarks in the first edition of my book so as only to be under-

---

Greek shepherd Magnes, and the magnetic mountains of the geographer Ptolemy, to the magnetic trains of early railway enthusiasts; from the magnetically protected coffin of Confucius to the magnetically suspended one of Mahomed; from the magnetic powders and potions of the ancients, and the metal discs, rods, and unguents of the old magnetisers, to the magnetic belts of the new—the modern panacea for all the ills that flesh is heir to; from the magnetic telegraphs of the sixteenth century to the Gary and Hosmer perpetual motors of the nineteenth, *et hoc genus omne*; all these impostures are, or were, based entirely on the (supposed) force of magnetic attraction, to which must be added an unconscionable amount of ignorance or credulity.

* *Magiæ Naturalis*, p. 88, Naples, 1558.

stood by learned chemists and physicians. I will now, however, communicate it for the benefit of the lovers of science generally." He then goes on to describe, in true cabalistic fashion, the preparation of

FIG. 1.

De Sunde's dial as given in Schott's *Schola Steganographica.*

the two compasses, the needles of which were to be made diamond-shaped from the same piece of steel and magnetised by the same magnet, or rather, magnets, for there were four: 1, Almagrito; 2, Theamedes; 3, Almaslargont; 4, Calamitro; which imparted south, north, east, and west-turning properties respectively to the needles. The compass-cards were divided off into compartments, each containing four letters of the alphabet, and each letter was indicated by the needle pointing, from one to four times, to the division in which it stood. Thus, the letter C would be indicated by three movements of the needle to the first division of the card. The needles were actuated by bar magnets, or chadids, and attention was called by the ringing of a tiny bell, which was so placed in the way of the needle that at each deflection of the latter it was struck, and so continued to ring until removed by the correspondent.

The next and most widely known relation of the story occurs in the *Prolusiones Academicæ*,* of Famianus Strada, a learned Italian Jesuit, first published at Rome in 1617, and often reprinted since. Although the idea did not originate with Strada (for he seems to attribute it to Cardinal Bembo, who died about 1547), he was certainly, as Sir Thomas Browne quaintly says, "The *æolus* that blew it about," for his *Prolusiones* had long been a favourite classic, while the passage referring to the loadstone has, if we may say so, been continually going the rounds of the newspapers. It is quoted more or less fully in many authors of the seventeenth and eighteenth centuries, famous

* Lib. ii., prol. 6.

amongst whom are Hakewill,* Addison,† Akenside,‡ and "Misographos." §

The references to it in the present century are simply too numerous to mention. The following is the latest English version, which, with the original Latin, appeared in the *Telegraphic Journal*, for November 15, 1875:—

"There is a wonderful kind of magnetic stone to which if you bring in contact several bodies of iron or dial-pins, from thence they will not only derive a force and motion by which they will always try to turn themselves to the bear which shines near the pole, but, also, by a strange method and fashion between each other, as many dial-pins as have touched that stone, you will see them all agree in the same position and motion, so that if, by chance, one of these be observed at Rome, another, although it may be removed a long way off, turns itself in the same direction by a secret law of its nature. Therefore try the experiment, if you desire a friend who is at a distance to know anything to whom no letter could get, take a flat smooth disc, describe round the outside edges of the disc stops, and the first letters of the alphabet, in the order in which boys learn them, and place in the centre, lying horizontally, a dial-pin that has touched the magnet,

* *An Apologie or Declaration of the Power and Providence of God in the Government of the World*, 1630.

† *Spectator*, No. 241, 1711, and *Guardian*, No. 119, 1713.

‡ *The Pleasures of Imagination*, 1744.

§ *The Student; or, the Oxford and Cambridge Miscellany*, 1750.

so that, turned easily from thence, it can touch each separate letter that you desire.

"After the pattern of this one, construct another disc, described with a similar margin, and furnished with a pointer of iron—of iron that has received a motion from the same magnet. Let your friend about to depart carry this disc with him, and let it be agreed beforehand at what time, or on what days, he shall observe whether the dial-pin trembles, or what it marks with the indicator. These things being thus arranged, if you desire to address your friend secretly, whom a part of the earth separates far from you, bring your hand to the disc, take hold of the movable iron, here you observe the letters arranged round the whole margin, with stops of which there is need for words, hither direct the iron, and touch with the point the separate letters, now this one, and now the other, whilst, by turning the iron round again and again throughout these, you may distinctly express all the sentiments of your mind.

"Strange, but true! the friend who is far distant sees the movable iron tremble without the touch of any one, and to traverse, now in one, now in another direction; he stands attentive, and observes the leading of the iron, and follows, by collecting the letters from each direction, with which, being formed into words, he perceives what may be intended, and learns from the iron as his interpreter. Moreover, when he sees the dial-pin stop, he, in his turn, if he thinks of any things to answer, in the same manner by the letters being touched separately writes back to his friend.

"Oh, I wish this mode of writing may become in use, a letter would travel safer and quicker, fearing no plots of robbers and retarding rivers. The prince, with his own hands, might despatch business for himself. We, the race of scribes, escaped from an inky sea, would dedicate the pen to the Shores of Magnet."

The Starry Galileo had his say on the same subject, and, as we may expect, said it well: "You remind me," says he, "of one who offered to sell me a secret art, by which, through the attraction of a certain magnetic needle, it would be possible to converse across a space of two or three thousand miles. And I said to him that I would willingly become the purchaser, provided only that I might first make a trial of the art, and that it would be sufficient for the purpose if I were to place myself in one corner of the room and he in the other. He replied that, in so short a distance the action would be scarcely discernible; whereupon I dismissed the fellow, saying that it was not convenient for me just then to travel into Egypt, or Muscovy, for the purpose of trying the experiment, but that if he chose to go there himself, I would remain in Venice and attend to the rest."*

* *Dialogus de Systemate Mundi*, 1632, p. 88. It is curious that Kepler appears to have believed in the efficacy of the sympathetic telegraph. See Fournier's *Le Vieux-Neuf*, Paris, 1857, vol. i. p. 200.

Cardinal Richelieu's system of espionage was so perfect that he was regarded (and feared) by his contemporaries as a dabbler in "diabolical magic." He was supposed to have possessed either a magic mirror, in which he could see all that went on in the world, or the equally magic magnetic telegraph. *À propos* of this, we find the following passage in the *Letters writ by a Turkish Spy*, a work which has been attributed by the elder Disraeli to John Paul Marana:—"This Cardinal said, on another time, that he kept a great many courtiers, yet he could well enough spare them; that he knew what passed in remote places as soon as what was done near him. He once affirmed he knew in less than two hours that the King of England had signed the warrant for the execution of ———. If this particular be true, this minister must be more than a man. Those who are his most devoted creatures affirm he has in a private place in his closet a certain mathematical figure, in the circumference of which are written all the letters of the alphabet, armed with a dart, which marks the letters, which are also marked by their correspondents; and it appears that this dart ripens by the sympathy of a stone, which those who give and receive his advice keep always at hand, which hath been separated from another which the Cardinal has always by him; and it is affirmed that with such an instrument he gives and receives immediately advices."*

The learned physician, Sir Thomas Browne, has

* Thirteenth letter, dated Paris 1639, vol. i.

some cautiously worded sentences on the mythical telegraph, which are worth quoting. "There is," he says, "another conceit of better notice, and whispered thorow the world with some attention; credulous and vulgar auditors readily believing it, and more judicious and distinctive heads not altogether rejecting it. The conceit is excellent, and, if the effect would follow somewhat divine; whereby we might communicate like spirits, and confer on earth with Menippus in the moon. And this is pretended from the sympathy of two needles, touched with the same loadstone, and placed in the center of two abecedary circles, or rings, with letters described round about them, one friend keeping one, and another the other, and agreeing upon an hour wherein they will communicate. For then, saith tradition, at what distance of place soever, when one needle shall be removed unto any letter, the other by a wonderful sympathy, will move unto the same. But herein I confess my experience can find no truth, for having expressly framed two circles of wood, and, according to the number of the Latine letters, divided each into twenty-three parts, placing therein two stiles, or needles, composed of the same steel, touched with the same loadstone and at the same point. Of these two, whenever I removed the one, although but at the distance of but half a span, the other would stand like Hercules pillars, and, if the earth stand still, have surely no motion at all." *

* *Pseudodoxia Epidemica*, book ii. chap. 3.

The *Scepsis Scientifica* of Joseph Glanvill, published in 1665, and which, by the way, secured his admission to the Royal Society, contains, perhaps, the most remarkable allusion to the then prevalent telegraphic fancy. Glanvill, albeit very superstitious, was an ardent and keen-sighted philosopher, and held the most hopeful views as to the discoveries that would be made in after-times. In the following passages he clearly foretells, amongst other wonders, the discovery and extension of telegraphs :—

"Should those heroes go on as they have happily begun, they'll fill the world with wonders. And I doubt not but posterity will find many things that are now but rumours verified into practical realities. It may be, some ages hence, a voyage to the southern unknown tracts, yea, possibly the moon, will not be more strange than one to America. To them that come after us it may be as ordinary to buy a pair of wings to fly into the remotest regions as now a pair of boots to ride a journey. *And to confer at the distance of the Indies by sympathetic conveyances may be as usual to future times as to us in a literary correspondence.*"—C. xix.

"That men should confer at very distant removes by an extemporary intercourse is a reputed impossibility, yet there are some hints in natural operations that give us probability that 'tis feasible, and may be compast without unwarrantable assistance from dæmoniack correspondence. That a couple of needles equally toucht by the same magnet being set in two dyals exactly proportion'd to each other, and circumscribed by the letters of the alphabet, may affect this magnale hath considerable authorities to avouch it. The manner of it is thus represented. Let the friends that would communicate take each a dyal; and having appointed a time for their sympathetic conference, let one move his impregnate needle to any letter in the alphabet, and its affected fellow will precisely respect the same. So that would I know what my friend would acquaint me with, 'tis but observing the letters that are pointed at by my needle, and in their order transcribing them from their sympathised index as its motion directs: and I may be assured that my friend described the same with his, and that the words on my paper are of his inditing.

"Now, though there will be some ill contrivance in a circumstance of this invention, in that the thus impregnate needles will not move to, but avert from each other (as ingenious Dr. Browne in his *Pseudodoxia Epidemica* hath observed), yet this cannot prejudice the main design of this way of secret conveyance, since 'tis but reading counter to the magnetic informer, and noting the letter which is most distant in the abecedarian circle from that which the needle turns to, and the case is not alter'd. Now, though this desirable effect possibly may not yet answer the expectation of inquisitive experiment, *yet 'tis no despicable item, that by some other such way of magnetick efficiency* *it may hereafter with success be attempted,* when magical history shall be enlarged by riper inspections, and 'tis not unlikely but that present discoveries might be improved to the performance."—C. xxi.

At the end of this chapter we give a list of references, as complete as we could make it, which will be useful to those of our readers who may wish to pursue the subject. It will also be instructive from another point of view, for it illustrates, in a very complete way, what Professor Tyndall has so well called the "menial spirit" of the old philosophers.* Notwithstanding that some of the more enlightened authors endeavoured laboriously to disprove the story, it was, for the most part, blindly and unquestioningly repeated, by one writer after another—credulous and vulgar auditors, as Sir Thomas Browne says, readily believing it, and more judicious and distinctive heads not altogether rejecting it, amongst whom we are tempted to reckon the learned knight himself.

Of those who stoutly and, at an early period, combatted the story, Fathers Cabeus and Kircher deserve

* "The seekers after natural knowledge had forsaken that fountain of living waters, the direct appeal to nature by observation and experiment, and had given themselves up to the remanipulation of the notions of their predecessors. It was a time when thought had become abject, and when the acceptance of mere authority led, as it always does in science, to intellectual death. Natural events, instead of being traced to physical, were referred to moral causes; while an exercise of the phantasy, almost as degrading as the spiritualism of the present day, took the place of scientific speculation."—Tyndall's *Address to the British Association at Belfast*, 1874.

to be mentioned—the one for the excellence, and the other for the vehemence of his observations. Those of the former are particularly remarkable, as containing a hazy definition of the "lines of force" theory—a theory which Faraday has turned to such good account in his *Experimental Researches.* Cabeus, as well as we can understand him, says, in his tenth chapter:—"The action by which compass needles are mutually disturbed is not brought about by sympathy, as some persons imagine, who consider sympathy to be a certain agreement, or conformity, between natures or bodies which may be established without any communication. Magnetic attractions and repulsions are physical actions which take place through the instrumentality of a certain quality, or condition, of the intervening space, and which [quality] extends from the influencing body to the influenced body. I cannot admit any other mode of action in magnetic phenomena; nor have I ever seen in the whole circle of the sciences any instance of sympathy or antipathy [at a distance]. * * *

"That which is diffused as a medium [or, that quality, or condition, of the intervening space] is thin and subtle, and can only be seen in its effects; nor does it affect *all* bodies, only such as are either conformable with the influencing body, in which case the result is a perfecting change [or sympathy = attraction], or non-conformable, in which case the result is a corrupting change [or antipathy = repulsion]. This quality is, I repeat, thin and subtle, and does not sensibly affect *all* intermediate [*i. e.*, neighbouring] bodies, although it may be disseminated through them. It only shows a sensibly good or bad effect according to the natures of the bodies opposed to one another.

"Bodies, therefore, are not moved by sympathy or antipathy, unless it be, as I have said, through the medium of certain essences [forces] which are uniformly diffused. When these reach a body that is suitable, they produce certain changes in it, but do not affect, sensibly, the intervening space, or neighbouring non-kindred bodies. Thus, the sense of smell is not perceived in the hand, nor the sense of hearing in the elbow, because, although these parts are equally immersed in the essences [or forces], they are not suitable, or kindred, in their natures to the odoriferous, or acoustic, vibrations." *

Kircher scouts the notion in no measured terms; after soundly rating the propagators of the fable on their invention of the terms *chadid*, *almagrito*, *theamedes*, *almaslargont*, and *calamitro*—vile jargon, which, he says, was coined in the devil's kitchen—he thus delivers himself:—"I do not recollect to have ever

* *Philosophia Magnetica*, &c., chap. x. *A brief letter from a young Oxonian to one of his late fellow pupils upon the subject of Magnetism*, London, 1697, contains, at page 10, a "draught" which illustrates very well the arrangement of magnetic lines of force, and which differs but little from the graphic representations of the present day. The curious little pamphlet is one of many gems in Mr. Latimer Clark's library.

met anything more stupid and silly than this idiotic conception, in the enunciation of which I find as many lies and impositions as there are words, and a crass ignorance of magnetic phenomena withal. In their craving after something wonderful and unknown they have manufactured a secret by means of barbarous and high-sounding words and by imitating the forms of recondite science, with the result that even they themselves cannot understand their own words." *

Many of the authors, who describe the sympathetic needle (dial) telegraph, speak also of another form, which seems to have been especially believed in by the Rosicrusians and Magnetisers of the last two centuries. It was supposed that a sympathetic alphabet could be marked on the flesh, by means of which people could correspond with each other, and communicate all their ideas with the rapidity of volition, no matter how far asunder. From the arms, or hands, of two persons intending to employ this method of correspondence a piece of flesh was cut, and mutually transplanted while still warm and bleeding. The piece grew to the new arm, but still retained so close a sympathy with its native limb, that the latter was always sensible of any injury done to it. Upon these transplanted pieces of flesh were tattooed the letters of the alphabet, and whenever a communication was to be made it was only necessary to prick with a magnetic needle the letters upon the arm composing

* *Magnes, sive de Arte Magnetica*, book ii. part iv. chap. 5.

the message; for whatever letter the one pricked, the same was instantly pained on the arm of the other.*

List of authors of the sixteenth, seventeenth, and eighteenth, centuries, who either describe the sympathetic needle and sympathetic flesh telegraphs, or make a passing allusion to one or both of them; chiefly compiled from Mr. Latimer Clark's list of books shown at the Paris Electrical Exhibition of 1881, and from the catalogues of the British Museum. As far as possible, only first editions quoted in full:—

1558 PORTA (GIAN B.). *Magiæ Naturalis, &c. Libri IIII.* 8vo. (See page 90. Other editions: Antwerp, 1561, 8vo.; Lugduni, 1561, 16mo.; Venetia, 1560, 8vo.; and 1665, 12mo.; Coloniæ, 1562, 12mo.) Neapoli, 1558.

1570 PARACELSUS (*i.e.*, Bombast Von Hohenheim). *De Secretis naturæ mysteriis*, &c. 8vo. (Speaks only of sympathetic flesh telegraph. Numerous editions in British Museum.) Basileæ, 1570.

1586 VIGENERE (BLAISE DE). *Traicté des Chiffres, ou Secretes Manieres d'Éscrire.* (Quoted in *L'Électricien* of Jan. 15, 1884, p. 95.) Paris, 1586.

1589 PORTA (GIAN B.). *Magiæ Naturalis, &c. Libri XX.* Folio. (See preface to Book VII. for first clear mention of sympathetic needle telegraph. Other editions: Francofurti, 1607, 8vo.; Napoli, 1611,

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* Upon this delusion is founded Edmund About's curious novel, *Le Nez d'un Notaire*, in which he relates the odd results of sympathy between the notary's nose and the arm of the man from whom the flesh was taken. But it is not in novels only, that we read of instances of the marvellous power of sympathy in these enlightened days; witness the story of *The Sympathetic Snail Telegraph* of Messrs. Biat and Benoit, which went the rounds of the newspapers forty years ago, and which the curious—we were going to say sympathetic—reader will find fully described in *Chambers's Edinburgh Journal*, for February 15, 1851.

4to.; Hanoviæ, 1619, 8vo.; Lugduni, 1644 and 1651, 12mo.; London, 1658, 4to.; and Amstelodami, 1664, 12mo.) Neapoli, 1589.

1599 PANCIROLLUS (G.). *Rerum Memorabilium*, &c. 8vo. (See Book II. [Nova Reperta], chap. xi., Notes. This author refers to Scaliger [*Exotericarum exercitationem*, &c., exercit. 131], and Bodin [*Methodus ad facilem Historiarum*, &c., chap. vii.], but they only speak of magnetic sympathy at great distances, without any reference to telegraphy. Other editions: two 8vo., Ambergæ, 1607 and 1612; four Francofurti, 1622, 1629–31, 1646, and 1660; Lyon, 1617; and London, 1715.) Ambergæ, 1599.

1600 DE SUNDE (J. H.) (*i. e.*, Daniel Schwenter). *Steganologia et Steganographia*. 8vo. (See p. 127. Janus Hercules de Sunde is an assumed name. Hiller in the preface to his *Mysterum Artis Steganographicæ* 1682, says that it is a synonym for Daniel Schwenter Noribergense; and again on p. 287, quoting Schwenter, he adds in parenthesis, "is est Hercules de Sunde." Other edition: Nürnberg, 1650, 12mo.) Nürnberg, 1600.

1609 DE BOODT (ANSELMUS B.). *Gemmarum et Lapidum Historia*, &c. 4to. (See Book II. Other editions: Lugduni, 1636, 8vo.; Lyon, 1644, 8vo.; and again Lugduni, 1647, 8vo.) Hanoviæ, 1609.

1610 ARGOLUS (ANDREAS). *Epistola ad Davidem Fabricium Frisium*. (He made what he calls a "Stenographic Compass," and held many agreeable conversations by its means with one of his friends.) In Ephemeridæ Patavii, 1610.

1610 ARLENSIS (PETRUS), of Scudalupis. *Sympathia Septem Metallorum*, &c. 8vo. (See chap. 2. This writer, a noted astrologer and alchemist, was the friend and fellow-citizen of Porta, to whom he seems to attribute the first conception of the sympathetic needle telegraph. His *Sympathia* was first published at Rome, but immediately suppressed in order that its grand secrets might not become known. It next appeared at Madrid in folio. The Paris ed. of 1610 was reissued at Hamburg in 1717.) Parisiis, 1610.

1617 STRADA (FAMIANUS). *Prolusiones Academicæ*, &c. 8vo. (See Lib. II., Prol. VI. Other editions:

Lugduni, 1617, and 1627, sm. 8vo.; Audomari, 1619, 12mo.; Mediolani, 1626, 16mo.; Oxoniæ, 1631, 8vo.; and again Oxoniæ, 1745, 8vo.) Romæ, 1617.

1624 VAN ETTEN (H.), (*i. e.*, Leurechon Jean). *La Récréation Mathématique*, &c. 8vo. (See p. 94. This author is the first to give a drawing of the dial. H. Van Etten was a *nom de plume*. See *Notes and Queries*, 1st series, vol. xi. p. 516. Other editions: Paris, 1626; Lyon, 1627; and three London, 1633, 1653, and 1674. To the two latter is added a work of Oughtred, the editor, whose name is so conspicuous on the title-page, that rapid cataloguers make him the author. Ozanam founded his *Recreations* on Van Etten; Montucla made a new book of Ozanam by large additions; and Hutton did the same by Montucla, so that Hutton's well-known work is at the end of a chain, of which Van Etten's is at the beginning. *Notes and Queries*, 1st series, vol. xi. p. 504.) Pont-à-Mousson, 1624.

1629 CABEUS (NICOLAS). *Philosophia Magnetica*, &c. Folio. (See p. 302.) Coloniæ, 1629.

1630 HAKEWILL (GEORGE). *An Apologie or Declaration of the Power and Providence of God*, &c. Folio. (See p. 285. This is second edition; a first appeared in [?] 1627, and a third in 1635. London and Oxford, 1630.

1630 MYDORGE (CLAUDE). *Examen du livre des Récréations Mathématiques*, &c. 12mo. (See Problem 74, pp. 140–44. This is a critically revised edition of Van Etten. Another edition, Paris, 1638.) Paris, 1630.

1631 KIRCHER (ATHANASIUS). *Ars Magnesia*, &c. 4to. (See pp. 35 and 36.) Herbipoli, 1631.

1632 GALILEO (G.). *Dialogus de Systemate Mundi*, &c. 4to. (See p. 88. Editions innumerable in British Museum catalogue.) Fiorenza, 1632.

1636 SCHWENTER (DANIEL). *Deliciæ Physico-Mathematicæ*. (See p. 346. This work is based on Van Etten's, *supra*. Two other 4to. editions appeared at Nürnberg, 1651–3 and 1677.) Nürnberg, 1636.

1638 FLUDD (ROBERT). *Philosophia Moysaica*, &c. Folio. (See Sec. II., Lib. II., Memb. II., Cap. V., and Sec. II., Lib. III., *passim*. An edition in English appeared in London, 1659.) Goudæ, 1638.

1641 KIRCHER (ATHANASIUS). *Magnes, sive de Arte Magnetica.* Sm. 4to. (See p. 382. Other editions: Coloniæ, 1643, 4to.; and Romæ, 1654, folio.) Romæ, 1641.

1641 WILKINS (JOHN). *Mercury, or the secret and swift messenger, showing how a man with privacy and speed may communicate his thoughts to a friend at any distance.* 12mo. (See p. 147. Another edition in 1694.) London, 1641.

1643 SERVIUS (PETRUS). *Dissertatio de Unguento Armario, Sive De Naturæ Artisque Miraculis.* (See para. 65, p. 68. This work is printed in Rattray's *Theatrum*, &c., *infra.*) Romæ, 1643.

1646 BROWNE (SIR THOMAS). *Pseudodoxia Epidemica, or Enquiries into very many received tenents, and commonly presumed truths.* 4to. (See p. 76. Numerous editions in the British Museum.) London, 1646.

1657 TURNER (ROBT.). *Ars Notoria. The Notary Art of Solomon, showing the cabalistical key of magical operations*, &c. 18mo. (See p. 136.) London, 1657.

1657–9 SCHOTT (GASPAR). *Magia Universalis Naturæ et Artis*, &c. 4 vols. 4to. (See vol. iv. p. 49. Copied from De Sunde and Kircher. Other edition: Bambergæ, 1677, 4to.) Herbipoli, 1657–9.

1661 HENRION (DENIS) and MYDORGE (CLAUDE). *Les Récréations Mathématiques, avec l'examen de ses problèmes*, &c. Premièrement reveu par D. Henrion, depuis par M. Mydorge, Cinquième et dernière ed. 12mo. (See Problem 74, pp. 158–61. This is only a revised edition of Mydorge's *Van Etten*, of 1630.) Paris, 1661.

1661 GLANVILL (J.). *The Vanity of Dogmatising, and an Apology for Philosophy.* 8vo. (See p. 202.) London, 1661.

1662 WESTEN (WYNANT VAN). *Het eerste Deel van de Mathematische Vermaeck*, &c. 8vo. Three parts. (See p. 125, Part I. This is an enlarged Dutch edition of Van Etten's, *supra.*) Arnhem, 1662.

1662 RATTRAY (SYLVESTER). *Theatrum Sympatheticum Auctum, exhibens Varios Authores de Pulvere Sympathetico*, &c. 4to. (See p. 546, see Petrus Servius, *supra.*) Norimbergæ, 1662.

1663 HELVETIUS (J. F.). *Theatridium Herculis Triumphantis*, &c. 8vo. (See pp. 11 and 15.) Haye, 1663.

1665 GLANVILL (JOSEPH). *Scepsis Scientifica; or, Confest Ignorance the Way to Science*, &c. 4to. (See p. 150.) London, 1665.

1665 SCHOTT (GASPAR). *Schola Steganographica*, &c. 4to. (See pp. 258-64. Description from De Sunde's, *supra*, with an elaborate drawing of the dial. Copper-plate title-page bears date 1665, printed title-page dated 1680.) Norimbergæ, 1665.

1676 HEIDEL (W. E.). *Johannis Trithemii, &c., Steganographia que Hucusqu: a nemine intellecta*, &c. 4to. (See p. 358.) Moguntiæ, 1676.

1679 MAXWELL (WILLIAM). *De Medicina Magnetica, &c. Lib. III.* 12mo. (See chaps. 11, 12, and 13.) Francofurti, 1679.

1684 DE LANIS (FRANCISCUS). *Magisterium Naturæ et Artis, Opus Physico-Mathematicum.* 3 vols. (See vol. iii. p. 412.) Brixiæ, 1684-96.

1684 MARANA (G. P.) (or The Turkish Spy). *L'Espion du Grand Seigneur*, &c. 12mo. (See vol. i., 13th letter, dated Paris, 1639. Six other editions in British Museum.) ? Paris, 1684, &c.

1689 BLAGRAVE (JOSEPH). *Astrological Practice of Physick*, &c. 12mo. (See p. 112.) London, 1689.

1689 DE RENNEFORT (SOUCHU). *L'Aiman Mystique.* 12mo. Paris, 1689.

1696 DE VALLEMONT (PIERRE LE LORRAIN). *La Physique Occulte, ou traité de la Baguette Divinatoire*, &c. 12mo. (See p. 32 of Appendix. Other editions: Paris and Amsterdam, 1693, 12mo.; and Amsterdam, 1696, 12mo.) Paris, 1696.

1701-2 LE BRUN (PIERRE). *Histoire Critique des Pratiques Superstitieuses.* 2 vols. 12mo. (See vol. i. p. 294. Other editions: Amsterdam, 1733-36; and Paris, 1750-1.) Rouen, 1701-2.

1711-13 ADDISON (JOSEPH). *The Spectator*, No. 241, for 1711. (See p. 206. See also *The Guardian*, No. 119, for 1713.) London, 1711-13.

1718 DU PETIT ALBERT. *Secrets Merveilleux de la Magie Naturelle et Cabalistique.* (See p. 228. Other editions: Lyon, 1743 and 1762; and Paris, 1815.) Lyon, 1718.

1723 SANTANELLI (F.). *Philosophia Recondita, sive Magicæ Magnetica*, &c. 4to. (See chap. xiv.) Coloniæ, 1723.

1730 BAILEY (NATHAN). *Dictionarium Britannicum*, &c. Folio. See word "Loadstone." Another London edition of 1736.) London, 1730.

1744 AKENSIDE (MARK). *The Pleasures of Imagination.* (See Book III., verses 325-37.) London, 1744.

1750-1 "MISOGRAPHOS." *The Student; or, the Oxford and Cambridge Monthly Miscellany.* 2 vols. (See vol. i. p. 354. A translation of Strada's verses.) Oxford, 1750-1.

1762 DIDEROT. *Memoirs. Correspondance et ouvrages inédits de Diderot.* (See p. 278. Diderot, in his letter to Madame Volland of 28th July, 1762, alludes to Comus [Ledru] and his supposed telegraph.) Paris, 1841.

1769 GUYOT. *Nouvelles Récréations Physiques et Mathématiques.* 4 vols. 8vo. (See vol. i. p. 17. At p. 134 there is a full description, with illustrations, of what was probably Comus's apparatus. Two other Paris editions of 1786 and 1799.) Paris, 1769.

1788 BARTHÉLEMY (JEAN JACQUES). *Voyage du Jeune Anacharsis en Grèce*, &c. 4to. (Quoted in *Journal of the Society of Arts*, May 20, 1859, p. 472: twelve other editions (of which three are English translations) in the British Museum. See also *Correspondance Inédite du Madame du Deffand*, vol. ii. p. 99.) Paris, 1788.

1795 EDGEWORTH (RICHARD LOVELL). *Essay on the Art of Conveying Secret and Swift Intelligence.* Published in the *Transactions of the Royal Irish Academy.* (See vol. vi. p. 125.) Dublin, 1797.

1797 GAMBLE (J.). *An Essay on the Different Modes of Communication by Signals*, &c. 4to. (See p. 57.) London, 1797.

CHAPTER II.

STATIC, OR FRICTIONAL, ELECTRICITY—HISTORY IN RELATION TO TELEGRAPHY.

"Thales call,
He, whose enquiring mind paused musingly
On the mysterious power, to action roused
By amber rubbed. This power (to him) a spirit,
Woke from its slumbers by all-wondrous art."

Oersted's *The Soul in Nature*,
p. 157 of Bohn's edition.

THE science of electricity is a comparatively modern creation, dating only from the commencement of the seventeenth century. It owes nothing, or almost nothing, to antiquity, and, in this respect, forms a remarkable contrast to most of the other branches of human knowledge—notably those of astronomy and mechanics, heat and light. The vast discoveries, says Lardner, which have accumulated respecting this extraordinary agent, by which its connection with, and influence upon, the whole material universe—its relations to the phenomena of organised bodies—the part it plays in the functions of animal and vegetable vitality—its subservience to the uses of man as a mechanical power—its intimate connection with the chemical constitution of material substances—in fine, its application in almost every division of the sciences, and every department of the arts, have been severally demonstrated, are exclusively and peculiarly due to the spirit of modern research, and, in a great degree, to the labours of the present age.*

Yet it is not that, in this case, nature had concealed her secrets with more than her usual coyness, for we find, scattered through the writings of the ancients, many observations on a class of phenomena, which, if rightly examined, must have led to the establishment of electricity as a department of physics.

That amber acquires, by friction, the power of attracting light bodies, such as bits of straw, wood, and dry leaves, is a fact which is probably as old as the discovery of the substance itself. Thales, one of the seven wise men of Greece, described the property six hundred years before Christ, and not as if it were with him a new phenomenon, but rather as a familiar illustration of his philosophical tenets.† Aristotle, Pliny, and other Greek and Roman writers, also record the fact, and even sometimes mention luminous appearances attending the friction.‡ Theophrastus, B.C. 321, on the authority of Diocles, speaks of the *lapis lyncurius*, supposed to be our modern tourma-

* *Manual of Electricity, Magnetism, and Meteorology*, vol. i. p. 2.

† He ascribed to amber some living principle, some soul, which could be roused to action by friction, and, in the spirit of the age, it was declared sacred. For the same reason, the loadstone was venerated, it being supposed to possess an immaterial spirit under the influence of which it attracted iron.—Aristotle, *De Anima*, i. 2.

‡ Pliny, book xxxvii. chap. iii.

line, as possessing the same property as amber, adding that it attracts not only straws and leaves, but copper also, and even iron, if it be in small particles.*

The emission of sparks from the human body, when submitted to friction, had also been noticed, as in the case of Servius Tullius, the sixth King of Rome, whose locks were frequently observed to give off sparks under the operations of the toilette. Eustathius, Bishop of Thessalonica, A.D. 1160, cites another instance in his *Commentarii ad Homeri Iliadem*, that of a certain ancient philosopher, who, occasionally, when changing his dress, emitted sparks, and, sometimes, even entire flames, accompanied by crackling noises. He also mentions the case of Walimer, a Gothic chief, who flourished A.D. 415, who used to give off sparks from his body.†

The Greeks and Romans were not the only people

* *De Lapidibus*, p. 124, Hill's edition.

† In *Iliad*, E, p. 515, Roman ed. We do not notice the frequent allusions in the pages of Cæsar, Livy, Plutarch, and others, to flames at the points of the soldiers' javelins, at the tops of the masts of ships, and, sometimes, even on the heads of the sailors themselves; for all these phenomena, though now known to be of the same nature as those described in the text, were then regarded simply as manifestations of the gods. See a very interesting example of this in Plutarch's *Life of Timolcon*, vol. iii. p. 16, Dacier's edition. For much interesting information on this subject, see Dr. William Falconer's "Observations on the Knowledge of the Ancients respecting Electricity," in vol. iii. *Memoirs of the Literary and Philosophical Society of Manchester*, 1790; also Tomlinson's *The Thunderstorm*, p. 96. In the early ages of the Church, the Popes were often reckoned as magicians, Gregory VII. being held in especial awe, because when he pulled off his gloves fiery sparks issued from them.

of antiquity to whom these phenomena were familiar. Thus, in the Persian language amber is called *Káh-rubá*, or attractor of straw, as the magnet is called *Ahang-rubá*, or attractor of iron. In the old Persian romance, *The Loves of Majnoon and Leila*, the lover says of his adored one, "She was as amber, and I but as straw; she touched me, and I shall ever cling to her." In the writings of Kuopho, a Chinese physicist of the fourth century, we read, "The attraction of a magnet for iron is like that of amber for the smallest grain of mustard seed. It is like a breath of wind, which mysteriously penetrates through both, and communicates itself with the rapidity of an arrow."

Humboldt,* after referring to this interesting fact, tells us how he himself had observed, with astonishment, on the woody banks of the Orinoco, in the sports of the natives, that the excitement of electricity by friction was known to these savage races. Children, he says, may be seen to rub the dry, flat, and shining seeds, or husks, of a trailing plant until they are able to attract threads of cotton and pieces of bamboo cane.

Such phenomena, says Lardner, in the work from which we lately quoted,† attracted little attention, and provoked no scientific research. Vacant wonder was the most exalted sentiment they raised; and they accordingly remained, while centuries rolled away,

* *Cosmos*, London, 1849 ed., vol. i. p. 176.

† Vol. i. p. 4.

barren and isolated facts upon the surface of human knowledge. The vein whence these precious fragments were detached, and which, as we have shown, *cropped out* sufficiently often to challenge the notice of the miner, continued unexplored; and its splendid treasures were reserved to reward the toil and crown the enterprise of modern times.

Without going the length of asserting that electrical phenomena were entirely neglected during the long night of the middle ages, it seems certain that, with the exception of the discovery of the electrical property of jet, little advance was made up to the close of the sixteenth century. Then it was that Dr. Gilbert, of Colchester, for the first time collected the scattered fragments, and, with many valuable observations of his own, shaped them into the nucleus of a new science, to which he gave the name Electricity, from the Greek word ἤλεκτρον, signifying amber. In his great work, *De Magnete*,* published in the year 1600, he described the only three substances known up to his time as susceptible of electrical excitation, and added a variety of others, such as spars, jems, fossils, glasses, and resins, which enjoyed, equally with them, the power of attracting not only light

* This book, although mainly devoted to magnetism, has many pages on electricity; and, besides its intrinsic value, is interesting as containing the first publications on our subject. William Gilbert was a member of the College of Physicians, London, and became Physician in Ordinary to Queen Elizabeth, who, conceiving a high opinion of his learning, allowed him an annual pension to enable him to prosecute his studies. He died in 1603.

bodies, like feathers and straws, but all solid and fluid matter, as metals, stones, water, and oil.

He also observed some of the circumstances which affect the production of electricity, such as the hygrometric state of the atmosphere. Thus, he noticed that when the wind blew from the north and east, and was dry, the body could be excited by a brisk and light friction continued for a few minutes, but that when the wind was from the south and moist, it was difficult, and sometimes impossible, to excite it at all. In order to test the condition of the various substances experimented upon, Gilbert made use of a light needle of any metal, balanced, and turning freely on a pivot, like the magnetic needle, to the extremities of which he presented the bodies after excitation.

Some of Gilbert's deductions were curiously fallacious. In pointing out, for instance, the distinction between magnetic and electric attraction, he affirmed that magnets and iron mutually attracted each other, but that when an electric was excited it alone attracted, the substances attracted remaining inactive. He noticed also, as a special distinction between magnetism and electricity, that the former repelled as well as attracted, whilst the latter only attracted.*

The few references to electricity in the works of Sir Francis Bacon, Nicolas Cabeus, Kenelm Digby, Gassendi, Descartes, Thomas Browne, and others, may be passed over in silence, as they are chiefly

* *De Magnete*, lib. ii. cap. 2–4.

theoretical, and did not contribute in any way to the advancement of the science.*

The celebrated Robert Boyle, to whom some of the other physical sciences owe such great obligations, directed much of his attention to the subject of electricity, and has left us an account of his experiments, in a small work, entitled *Experiments and Notes about the Mechanical Origine or Production of Electricity*, London, 1675. By means of a suspended needle, he discovered that amber retained its attractive virtue after the friction which excited it had ceased; and though smoothness of surface had been regarded as advantageous for excitation, yet he found a diamond, which, in its rough state, exceeded all the polished ones, and all the electrics that he had tried, it having been able to move the needle three minutes after he had ceased to rub it. He found also that heat and "tersion" (*i. e.*, the cleaning or wiping of any body) increased the electrical effect; and that if the attracted body were fixed, and the attracting one movable, their approach would take place all the same, thus disproving one of Gilbert's deductions. To Dr. Gilbert's list of electrics, he added several new ones, as glass of antimony, white sapphire, white amethyst, carnelian, &c.

Like all his predecessors, Boyle (in whom, by the way, the theorising faculty was particularly strong)

* Jacob Böhmen, the Teutonic Theosopher, who lived 1575–1624, and who wrote largely on astrology, philosophy, chemistry, and divinity, has some pages on electricity. See *Notes and Queries*, July 28, 1855, p. 63.

speculated, in his turn, on the cause of electrical phenomena; but it seems that he, as well as they, could find no better explanation than that offered by the Ionic sage, twenty-three centuries before. The supposition was that the excited body threw out a glutinous or unctuous effluvium, which laid hold of small bodies in its path, and, on returning to its source, carried them along with it.* The *Philosophical Transactions* of this period contain some learned disquisitions in support of this (now strange) hypothesis, and even experiments are described which were considered as conclusive of its correctness.†

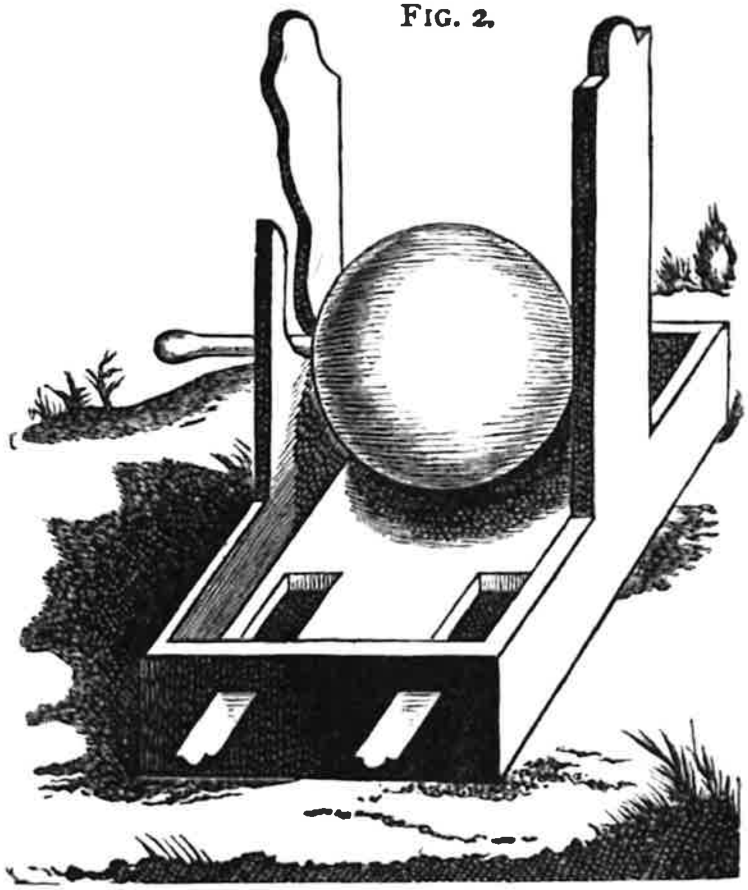
Otto Guericke, burgomaster of Magdeburg, and inventor of the air-pump, was contemporary with Boyle, and to him we owe some most important advances. In 1671, he constructed the first electrical machine, by means of which he was able to produce electricity in far greater quantities than had hitherto been possible from the friction of glass or sulphur rods. With this machine, which consisted of a globe of

* Boyle is sometimes said to have been the first, in modern times, to observe the electric light—an assertion which seems to be based upon his observation, in 1663, of the light which some diamonds gave out, in the dark, after being rubbed. But it is doubtful if this was not an optical rather than an electrical effect, an instance of what may be called latent light, and therefore belonging to the class of phenomena, of which the celebrated Bologna stone, discovered in 1602 by the quondam shoemaker Casiorolus, was the first recorded example, as Balmain's luminous paint is the last. For much interesting information on this subject, see Sir D. Brewster's *Letters on Natural Magic*.

† *Phil. Trans.*, for 1699, vol. xxi. p. 5.

sulphur,* mounted on a revolving axis, and excited by the friction of a cloth held in the hand, he discovered

FIG. 2.



The First Electrical Machine, copied from p. 148 of Otto Guericke's *Experimenta Nova*, &c.

the "hissing noise and gleaming light" which accompany strong electrification.

* Sulphur, it may be remarked, was a favourite electric with early experimenters, as it was imagined that electricity was emitted with the sulphurous effluvium produced by the friction. In the construction of his machine, Guericke, for example, cast the sulphur in a glass globe, which he afterwards broke, so as to expose the sulphur to the action of the rubber, little imagining that the glass globe itself would have answered his purpose just as well.

To him also belongs the discovery of the property of electrical repulsion. He ascertained that a feather, when attracted to an excited electric, was instantly repelled, and was incapable of a second attraction, until it had been touched by the finger or some other body. He also observed that a feather, when thus repelled, always kept the same side towards the excited electric—a fact the correspondence of which with the position of the moon towards the earth, induced him and other philosophers to assume that the revolution of the moon round the earth might be explained on electrical principles. Again, in the observation that a substance becomes electric by being merely brought near to another electrified body, Guericke discovered the fact, though not the principle, of induction.*

Newton, about the same time, published another effect of induction, *viz.:* one side of a glass plate being electrified, the other side will also be electrified, and will attract any light bodies within its influence. Laying upon a table a disc of glass two inches broad, in a brass hoop or ring, so that it might be one-eighth of an inch from the table, and then rubbing it briskly, little pieces of paper, laid upon the table under the glass, moved nimbly to and fro, and twirled about in the air, continuing these motions for a considerable time after he had ceased rubbing. Upon sliding his

* *Experimenta Nova Magdeburgica*, Amstelodami, 1672, lib. iv. cap. 15.

finger over the glass, though he did not agitate it, nor, by consequence, the air beneath, he observed that the papers, as they hung under the glass, would receive some new motion, inclining this way or that, according to the direction of his finger.

The Royal Society had ordered this experiment to be repeated at their meeting of December 16, 1675, and, in order to ensure its success, had obtained a full account of it from its distinguished author. The experiment, however, failed, and the secretary requested the loan of Sir Isaac's apparatus, inquiring, at the same time, whether or not he had guarded against the papers being disturbed by the air which might have somewhere stolen in? In replying, on the 21st of December, Newton advised them to rub the glass "with stuff whose threads may rake its surface, and if that will not do, rub it with the finger ends to and fro, and knock them as often upon the glass." Following these directions, the Society succeeded, on January 31, 1676, when they used a scrubbing brush of short hog's bristles, and the heft of a knife made with whalebone! *

In the 8th and 27th queries at the end of his treatise on Optics, Newton has introduced the subject of electricity in such a manner as to convey some notion of the theoretical views which he had been led to form. He says (8th query):—"A globe of glass about eight or ten inches in diameter being put into a

* See Brewster's *Life of Sir Isaac Newton*, pp. 307-8: or Birch's *History of the Royal Society*, vol. iii. pp. 260-70.

frame where it may be swiftly turned round, its axis will, in turning, shine where it rubs against the palm of one's hand applied to it; and if at the same time a piece of white paper be held at the distance of half an inch from the glass, the electric vapour, which is excited by the friction of the glass against the hand, will, by dashing against the paper, be put into such an agitation as to emit light, and make the paper appear livid like a glow-worm. In rushing out of the glass, it will even sometimes push against the finger so as to be felt." And again, in the 27th query, he says:—"Let him also tell me how an electric body can, by friction, emit an exhalation so rare and subtile, and yet so potent, as by its emission to cause no sensible diminution of the weight of the electric body, and to be expanded through a sphere whose diameter is above two feet, and yet to be able to agitate and carry up leaf copper, or leaf gold, at the distance of above a foot from the electric body." *

Between 1705 and 1711, Hauksbee made many

* These appear to be the only published observations of the great Sir Isaac on electrical matters; but it would seem that, in moments of leisure from weightier business, he bestowed an occasional glance on the infant science. This will be apparent from the following extract from an autograph letter, which Mr. Latimer Clark has lately unearthed, and which will be found in full in *The Electrician* Journal, for April 16, 1881:—"I have been much amused by ye singular φενομενα resulting from bringing of a needle into contact with a piece of amber or resin fricated on silke clothe. Ye flame putteth me in mind of sheet lightning on a small (how very small) scale." Although this letter is dated "London, December 15, 1716," it would seem from the wording that Newton was unaware of similar comparisons instituted several years before, by Hauksbee and Wall.

valuable and interesting observations, of which we must content ourselves with a brief *résumé*, referring our readers for fuller accounts to the original papers in the *Philosophical Transactions*, or to Priestley's excellent *History and Present State of Electricity*, pp. 15–23, 5th ed. In 1705, he showed that light could be produced by passing common air through mercury, contained in a well-exhausted glass receiver. The air, rushing through the mercury, blew it against the sides of the glass, and made it appear like a body of fire, consisting of an abundance of glowing globules. In repeating this experiment with about three pounds of mercury, and making it break into a shower, by dashing it against the crown of another glass vessel, flashes resembling lightning, of a very pale colour, and distinguishable from the rest of the produced light, were thrown off from the crown of the glass in all directions.* Hauksbee likewise showed that considerable light may be produced by agitating mercury in a partially exhausted tube; and that even in the open air numerous flashes of light are discoverable by shaking quicksilver in any glass vessel.

* Electric light *in vacuo* was first observed by Picard in 1675. While carrying a barometer from the Observatory to Porte St. Michel in Paris, he observed light in the vacuous portion. Sebastien and Cassini observed it afterwards in other barometers. John Bernouilli, in 1700, devised a "mercurial phosphorus" by shaking mercury in a tube which had been exhausted by an air-pump. This was handed to the King of Prussia—Frederick I.—who awarded it a medal, of forty ducats' value. The great mathematician wrote a poem in honour of the occasion.—Tyndall's *Notes on Electricity*.

In a subsequent series of experiments on the light produced by the attrition of bodies *in vacuo*, he showed that glass, when thus excited, emitted light in as strange a form as lightning, particularly when he used a rubber that had been previously drenched in spirits of wine. In all these experiments Hauksbee had no notion of the electrical origin of the light, and in saying that it resembled lightning he was only using a simile, without any suspicion of a closer connection.

Like Sir Isaac Newton, Hauksbee employed a glass globe machine, as he thought that this material was capable of more powerful effects. When exhausted of air, and turned briskly, the application of his hand would produce a strong light on the inside; and, by re-admitting the air, light appeared on the outside also. By bringing an exhausted globe near to an excited one, he found that a light was produced in the former, which soon disappeared; but which immediately re-appeared, with great beauty, on a further excitation.

The following experiment must at that time, and indeed for long after, have been considered one of great singularity. Having coated one half of the inside of a glass globe with sealing-wax, which in some places was an eighth of an inch thick, and therefore quite opaque, he exhausted it and put it in motion. On applying his hand, for the purpose of excitation, its outline soon became distinctly visible on the concave surface of the wax, thus making it seem to be transparent, although before excitation it would only just allow the flame of a lighted candle to be seen through it in the dark. The same result was obtained when pitch, or common brimstone, was substituted for the sealing-wax.

Besides light and crackling noises, Hauksbee also noticed that an electrified body was able to produce a sense of pain (the electric shock) in the hand, or face, that touched it—an observation which is also claimed for his friend, Dr. Wall.

This latter philosopher is, however, best known as being the first to suspect the identity of lightning and electricity. The happy thought was suggested to him, as he tells us in a paper read before the Royal Society in 1708, by the sparks and crackling sounds produced by the friction of a large stick of amber against a woollen cloth. "Upon drawing," he says, "the piece of amber swiftly through the woollen cloth, and squeezing it pretty hard with my hand, a prodigious number of little cracklings was heard, every one of which produced a little flash of light; but when the amber was drawn gently and slightly through the cloth, it produced a light, but no crackling. By holding a finger at a little distance from the amber, a crackling is produced, with a great flash of light succeeding it; and what is very surprising, on its eruption it strikes the finger very sensibly, wheresoever applied, with a push or puff like wind. The crackling is full as loud as that of charcoal on fire;

nay, five or six cracklings, or more, according to the quickness of placing the finger, have been produced from one single friction, light always succeeding each of them. Now I doubt not but on using a longer and larger piece of amber, both the cracklings and light would be much greater. This light and crackling seem in some degree to represent thunder and lightning." *

So far, experimenters had worked without any system, and without in the least comprehending the principles on which the effects they produced depended. Highly important as were all their observations, the true foundations of electricity as a science cannot, therefore, be said to have been laid until Stephen Gray, a pensioner of the Charter-house, London, gave to the world that justly celebrated series of experiments which, begun in 1720, only ended with his last breath in 1736.† As from this point the domain widens, we will confine ourselves in the rest of this chapter to noticing only such discoveries of Gray and succeeding philosophers as bear intimately on our subject.

In February 1729, Gray discovered the principle of electric conduction and insulation, and in doing

* Hutton's *Phil. Trans. Abridged*, vol. v. p. 409.

† This remarkable man was (so to speak) dying when his last experiments were made, and, unable to write himself, he dictated an account of them to Dr. Mortimer, the secretary of the Royal Society, the day before his death.—See *Phil. Trans.*, vol. xxxix. p. 400, 1735-36, or Hutton's *Abridgment*, vol. viii. p. 110.

so might almost be said to have invented electric telegraphy, of which it is the very *alpha* and *omega*. This important discovery was made in the following manner:—Wishing to excite in metals, as had already been done in glass, resin, &c., the power of attraction and repulsion, he tried various methods, such as rubbing, heating, and hammering; but all to no end. At last an idea occurred to him that, as a glass tube, when rubbed in the dark, communicated its light freely to bodies, so it might communicate a power of attraction, which, at this time, was considered the only absolute proof of the presence of electricity. In order to test this, he took a glass tube, 3 feet 5 inches long and 1 inch diameter, and filled up the ends with pieces of cork to keep out the dust when the tube was not in use. His first experiment was to ascertain if there was any difference in its power of attraction when the tube was stopped at both ends by the corks, and when left entirely open; but he could perceive no sensible difference. Then holding a feather over against the end of the tube, he found it would fly to the cork, being attracted by it as readily as by the tube itself. He concluded from this that the electric virtue, conferred on the tube by friction, passed spontaneously to the cork.

It then occurred to him * to inquire whether this

* We follow in this and the next three paragraphs Lardner's *Manual of Electricity, Magnetism*, &c., vol. i. pp. 8–9. See also Priestley's *History of Electricity*, pp. 24–39.

transmission of electricity would be made to other substances besides cork. With this view he obtained a deal rod about four inches in length, to one end of which he attached an ivory ball, and inserted the other in the cork, by which the glass tube was stopped. On exciting the tube, he found that the ivory ball attracted and repelled the feather even more vigorously than the cork. He then tried longer rods of deal, and pieces of brass and iron wire, with like results. Finally he attached to one end of the tube a piece of common packthread, and, suspending from its lower end the ivory ball and various other bodies, found that all of them were capable of acquiring the electric state when the tube was excited. Experiments of this kind were made from the balconies of his house and other elevated stations.

With a true philosophic spirit, he now determined to inquire what circumstances attending the *manner* of experimenting produced any real effect upon the results; and, first, whether the *position* or *direction* of the rods, wires, or cords, by which the electricity was transmitted from the excited tube, affected the phenomena. For this purpose he extended a piece of packthread in a horizontal direction, supporting it at different points by other pieces of similar cord, which were attached to nails driven into a wooden beam, and which were, therefore, in a vertical position. To one end of the horizontal cord he attached the ivory ball, and to the other he tied the end of the glass tube. On exciting the tube he found that no electricity was transmitted to the ball, a circumstance which he rightly ascribed to its escape by the vertical cords, the nails supporting them, and the wooden beam.

Soon after this (June 30, 1729), Gray was engaged in repeating his experiments at the house of Mr. Wheeler, who was afterwards associated with him in these investigations, when that gentleman suggested that threads of silk should be used to support the horizontal line of cord, instead of pieces of packthread. It does not appear that this suggestion of Wheeler proceeded from any knowledge, or suspicion, of the electric properties of silk; and still less does it appear that Gray was acquainted with them; for, in assenting to the proposition of his friend, he observed, that "silk might do better than packthread on account of its smallness, as less of the virtue would probably pass off by it than by the thickness of the hempen line which had been previously used."

They accordingly (July 2, 1729) extended a packthread through a distance of about eighty feet in a horizontal direction, supporting it by threads of silk. To one end they attached the ivory ball, and to the other the glass tube. When the latter was excited, the ball immediately became electric, as was manifested by its attracting metallic leaf held near it. Next day, they extended their experiments to lines of packthread still longer, when the silk threads used for its support were found to be too weak, and were broken. Being under the (erroneous) impression that the escape of the electricity was prevented by the fineness of the silk, they now substituted for it thin brass wire, which they expected, being still finer than the silk, would more effectually intercept the electricity; and which, from its nature, would have all the necessary strength. The experiment, however, completely failed. No electricity was conveyed to the ivory ball, the whole having escaped by the brass wire, notwithstanding its fineness. They now saw that the silk threads intercepted the electricity, because they were *silk*, and not because they were *fine*.

Having thus accidentally discovered the property of insulation, they proceeded to investigate its generalisation, and found that it was enjoyed by resin, hair, glass, and some other substances.

In fact, it soon became apparent that in this respect all matter may be said to belong to one of two classes, the one like the packthread and brass wire, favouring the dissipation, or carrying away, of the electric power, and the other like the silk and glass opposing it.*

* Soon after this, in August 1729, Gray discovered that when the electrified tube was brought near to any part of a *non-electric* or conducting body, without touching it, the part most remote from the tube became electrified. He thus fell upon the fact, which afterwards led to the principle of induction. The science, however, was not yet ripe for that great discovery, and Gray, like Otto Guericke before him, and Wilson and Canton after him, continued to apply the principles of induction without the most remote suspicion of the rich mine whose treasures lay beneath his feet, and which it was one of the glories of Franklin to bring to light.

Armed with this knowledge, Gray and Wheeler, in July 1729, had the great satisfaction of being able to transmit the electric power through as much as 765 feet of packthread, supported by loops of silk; and in August 1730, through 886 feet of wire. It is curious to observe that in these experiments, as, indeed, in all others on electrical conduction, we have all the essentials—crude, of course—of a perfect telegraph, the insulated line, the source of electricity in the rubbed glass, the indicating instrument in the down feather, and the earth, or return circuit, the function of which, however, was not then suspected.

While Gray and Wheeler were pursuing their investigations in England, Dufay, of the Academy of Sciences, and Intendant of the Royal Botanic Gardens, was actively engaged in Paris, in a similar manner. The researches of this philosopher, so celebrated as the originator * of the double-fluid theory of electricity, embraced the period between 1733 and 1737. He added largely to the class of bodies called *electrics*, by showing that all substances, except metals, and bodies in the soft or liquid state, might be made electric, by first heating them, and then rubbing them on any kind of cloth; and as regards even these

* He can hardly be called its author—at all events in its present form. For Symmer's claims to this honour, the reader is referred to Priestley's *History of Electricity*, p. 227. The writer of the article Electricity in the *Encyclopædia Britannica*, 7th edition, says, but we know not on what authority, that this important discovery was simultaneously and independently made by Dufay in France and by White in England.

exceptions, he showed that they, and, generally, *all* bodies, solid and liquid, could be electrified, if only the precaution were taken of first placing them on glass stands.

In repeating Gray's experiments with the pack-thread, he perceived that they succeeded better after wetting the line, and, with the aid of this fact, he was able to transmit the electric power along a cord of nearly 1300 feet, which he supported at intervals on glass tubes.

His discovery of the dual character of electricity was, like most of the other capital discoveries hitherto made, entirely due to chance. A piece of gold leaf having been repelled by an excited glass rod, Dufay pursued it with an excited rod of sealing-wax, expecting that the effect would be the same. His astonishment, therefore, was great on seeing the gold leaf fly to the wax, and, on repeating the experiment, the same result invariably followed; the gold leaf, when repelled by glass, was attracted by resin, and, when repelled by resin, was attracted by glass. Hence Dufay concluded that there were two distinct kinds of electricity, and, as one was produced from glass, and the other from resin, he distinguished them by the names *vitreous* and *resinous*.

In repeating Otto Guericke's experiments, Dufay discovered another general law, which enabled him to explain a number of observations that hitherto were obscure and puzzling. This law is, that an electrified body attracts those that are not so, and repels them as soon as they become electric by contact with itself. Thus, gold leaf is first attracted by the excited tube, and acquires an electricity by the contact, in consequence of which it is immediately repelled. Nor is it again attracted while it retains this electric quality; but, if now it chance to light on some other body, it straightway loses its electricity, and is then re-attracted by the tube, which, after having given it a new charge, repels it a second time, and so on, as long as the tube itself retains any electricity.*

The study of electricity was next taken up, in 1737, by Desaguliers, who, though born in France in 1683, early removed to England, and died in London in 1744. Two years before his death he published a *Dissertation Concerning Electricity*,† which is remarkable as being the first book on the subject in the English language. Desaguliers' investigations were mainly concerned with the relative conducting powers of various bodies, but he otherwise did good and useful work, by methodising the information that had already accumulated, and by improving in some

* Priestley's *History of Electricity*, pp. 40–50.

† As a reason for his engaging in this pursuit so late in life, Desaguliers makes the curious assertion that he was debarred from doing so earlier by the peculiar temper of Stephen Gray, who would have abandoned the field entirely if he saw that anything was done in apparent opposition or rivalry to himself.—Brewster's *Edinburgh Encyclopædia*, *verbo* Electricity, p. 415.

It is difficult to reconcile this passage with the following, which we extract from Desaguliers' *Dissertation*, p. 47:—"Indeed, a few electrical experiments, made by Mr. Gray and myself many years ago, important respects the nomenclature. Thus, the labours of Gray, Wheeler, Dufay, and himself, had shown that all matter was divisible into two great classes, these he now proposed to distinguish by the names *Electrics*, or bodies in which electricity could be excited by friction, and *Non-electrics*, or those in which it could not be excited, but which could receive it from an electric. He also first employed the words *Conductor* and *Non-conductor* in the same sense as they are used at the present day.

In the *Philosophical Transactions*, for 1739, vol. xli. p. 209, will be found his experiments on the transmission of electricity, which were made at H.R.H. the Prince of Wales's house at Cliefden, on April 15, 1738. "Having heard that electricity had been carried along a hempen string five or six hundred feet, but having only seen it done when the string was carried backwards and forwards in a room, by silk supporters, Dr. D. wished to try it with a packthread stretched out at full length; for which purpose, having joined a piece

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are mentioned in the first volume of my *Course of Experimental Philosophy*, pp. 17–21."

The following lines by the poet Cawthorn depict the neglect and indigence into which Desaguliers fell in his old age:—

"Can Britain * * * * *
* * permit the weeping muse to tell
How poor neglected Desaguliers fell?
How he, who taught two gracious kings to view
All Boyle ennobled, and all Bacon knew,
Died in a cell, without a friend to save,
Without a guinea, and without a grave?"

The Vanity of Human Enjoyments, v. 147–54.

of catgut to one end of a string, he fastened it to a door; and having also tied another catgut to the other end of the string, he fastened it at the other end of the house. At the places where the packthread was joined to the catgut he left eighteen inches of the thread hanging down, and fastened a *lignum vitæ* handle of a burning-glass to one, while he applied a rubbed tube to the other. He made the electricity run to the *lignum vitæ*, but with some difficulty, which he attributed to the sizing, being an animal substance, that still adhered to the thread as it was new; therefore, he caused the thread to be wet with a sponge from one end to the other, to wash off the size; then was the electricity from the tube communicated very soon and very strongly; for the thread of trial was drawn by the *lignum vitæ* at the distance of a foot.

"Afterwards, having joined more packthread together, he made a string of 420 feet long, which he supported at intervals by pieces of catgut. The string was previously dipped in a pail of water, but great care was taken that the catgut should not be wet. Then he applied the rubbed tube at one end, while an assistant held the thread of trial near the handle at the other, whereupon it was strongly attracted, though the wind was very high, and blowed in the contrary direction to that in which the electricity ran.

"He first tried the experiment with the packthread dry, but then it would not succeed at that distance." *

* Hutton's *Phil. Trans. Abridged*, vol. viii. p. 357.

Up to this time, and until some years later, experiments on the transmission of electricity to a distance excited no attention outside a very narrow circle of scientific men, and even amongst these, they served only to illustrate the two great electrical properties of bodies—conduction and insulation—without evoking the slightest suspicion of their practical value. The whole subject of electricity now, however, began to attract general attention, especially amongst the Germans, and the first consequence was considerable improvement in the power and efficiency of electrical apparatus. About 1741, Professors Hausen, of Leipsic, and Boze, of Wittemburg, revived the use of the glass globe machine, first introduced many years before, by Newton and Hauksbee, but which, after their time, had been supplanted, to the great detriment of the science, by the glass tube and silk rubber of Gray. Boze also added, for the first time, the prime conductor, which consisted of an oblong cylinder of tin or iron. This was at first held in position by a man, who was insulated, by standing on cakes of resin, but it was subsequently suspended by silken cords, and, in order to facilitate the passage of the electricity, a number of linen strings were added, which served the purpose, though very imperfectly, of the metal points now employed. Professor Winkler, of Leipsic, next substituted a fixed woollen cushion in place of the hand for exciting the globe, and lastly, in 1742, Gordon, a Scotch Benedictine monk, and Professor of Natural

Philosophy at Erfurt, substituted a glass cylinder for the globe, and otherwise so increased the power of the machine, that he was able to kill small birds at the end of an iron wire 200 ells (250 yards) long.*

These various improvements were followed, in October 1745, by the discovery of the Leyden Jar. This invention is one of the vexed questions of the science, being claimed, and perhaps with equal justice, for Von Kleist, dean of the Cathedral at Kamin, in Pomerania; for Musschenbröck, the celebrated professor of Leyden; and for Cuneus, a rich burgess of that town. Von Kleist appears to have been first, in point of priority of publication; but his account of the discovery was so obscurely worded, that it was impossible for some time to verify it. The following is an extract from his letter on the subject, which was addressed to Dr. Lieberkuhn, of Berlin, on the 4th November, 1745, and by him communicated to the Berlin Academy:—

"When a nail, or a piece of thick brass wire, is put into a small apothecary's phial and electrified, remarkable effects follow; but the phial must be very dry or warm. I commonly rub it over beforehand with a finger on which I put some pounded chalk. If a little mercury, or a few drops of spirit of wine, be put into it, the experiment succeeds the better. As soon as this phial and nail are removed from the electrifying glass, or the prime conductor to which it has been

* Priestley's *History of Electricity*, pp. 64–67.

exposed is taken away, it throws out a pencil of flame so strong, that with this burning instrument in my hand I have taken above sixty steps in walking about my room. When it is electrified strongly, I can take it into another room, and there fire spirits of wine with it.

"If, whilst it is electrifying, I put my finger, or a piece of gold which I hold in my hand, to the nail, I receive a shock which stuns my arms and shoulders. A tin tube, or a man, placed upon electrics, is electrified much more strongly by this means than in the common way. When I present this phial and nail to a tin tube which I have, fifteen feet long, nothing but experience can make a person believe how strongly it is electrified. Two thin glasses have been broken by the shock. It appears to me very extraordinary that when this phial and nail are in contact with either conducting or non-conducting matter, the strong shock does not follow. I have cemented it to wood, glass, sealing-wax, metal, &c., which I have electrified without any great effect. The human body, therefore, must contribute something to it. This opinion is confirmed by observing that, unless I hold the phial in my hand, I cannot fire spirits of wine with it."

In January 1746, Cuneus made the same discovery, and apparently in the same accidental way. It having been observed by Musschenbröck and his colleagues, Cuneus and Allamand, that electrified bodies speedily lost their virtue, which was supposed to be abstracted by the air itself, and by vapours and effluvia suspended in it, they imagined that if they could surround them with any insulating substance, so as to exclude the contact of the atmosphere, they could communicate a more intense electrical power, and could preserve that power for a longer time.* Water appeared one of the most convenient recipients for the electrical influence, and glass the most effectual and easy insulating envelope. It appeared, therefore, very obvious, that water enclosed in a glass bottle must retain the electricity given to it, and that by such means a greater charge or accumulation of electric force might be obtained than by any expedient before resorted to.

In the first experiments made in conformity with these views, no remarkable results were obtained. But it happened on one occasion that Cuneus held the glass bottle in his right hand, while the water contained in it communicated by a wire with the prime conductor of a powerful machine. When he considered that it had received a sufficient charge, he applied his left hand to the wire to disengage it

* In a paper read before the Royal Society in 1735, Stephen Gray has these curiously prophetic words:—"Though these effects of the fire and explosion of electricity communicated to a metallic rod are at present only minute, it is probable that in time there may be found out *a way to collect a greater quantity of the electric fire*, and consequently to increase the force of that power, which by several of these experiments, if we are permitted to compare small things with great, seems to be of the same nature with that of thunder and lightning."—Priestley, p. 54.

from the conductor. He was instantly struck with a convulsive shock, which filled him with the utmost consternation, and made him let fall the flask. Musschenbröck and others quickly repeated the experiment, and with like results.*

In describing these, in a letter to Réaumur, Musschenbröck said he felt himself struck in the arms, shoulders, and breast, so that he lost his breath, and was two days before he recovered from the effects of the blow, and the terror. He added that he would not repeat the experiment for the whole kingdom of France. Boze, on the other hand, seems to have coveted electrical martyrdom, for he is said to have expressed a wish to die by the shock (the name by which this phenomenon was known), that the account of his death might furnish an article for the Memoirs of the French Academy. Allamand, the associate of Musschenbröck, took the shock from a common beer-glass, and lost the use of his breath for some minutes, and then felt so intense a pain along his right arm, that he feared permanent injury from it. Professor Winkler, on undergoing the experiment for the first time, suffered great convulsions, his blood was agitated, and fearing an ardent fever, he had recourse to cooling medicines. His wife, also, with a courage only equalled by her curiosity, twice subjected herself to the shock, and was so enfeebled thereby that she could hardly walk, and on trying it

* Priestley's *History of Electricity*, pp. 75–8.

again, a week later, it gave her bleeding at the nose.*

An account of these extraordinary effects soon got abroad, and spread over Europe with the rapidity almost of the spark itself. The experiments were repeated everywhere, and excited the wonder of all classes towards what was regarded as "a prodigy of nature and philosophy." Indeed, so popular did they become, that great numbers of *impromptu* electricians wandered over every part of Europe, and enriched themselves by gratifying the universal curiosity at so much per shock.

But as soon as these first feelings of wonder had abated, philosophers set themselves seriously to study the powers of the new machine; and the circumstances which influenced the force of the shock first engaged their attention.

Musschenbröck observed that if the glass were wet on the outer surface the success of the experiment was impaired. Dr. (afterwards Sir William) Watson, apothecary and physician, of London, next proved that, while the force of the shock was increased by diminishing the thickness of the glass, it was independent of the power of the machine by which the glass was charged.

* Priestley, pp. 78–9. It is no doubt to the "uncontrolled use of the imagination in science," that we must, in a great measure, attribute these first effects of an experiment with which electricians are now so familiar, and which every school boy and girl undergo nowadays from motives of curiosity or amusement.

By further repeating and varying the experiment, Watson found that the force of the charge depended on the extent of the external surface of the glass in contact with the hand of the operator. It next occurred to Dr. Bevis that the hand might be efficient merely as a conductor of electricity, and in that case that the object might be more effectually and conveniently attained by coating the exterior of the phial with sheet lead or tin-foil. This expedient was completely successful, and the phial, so far as related to its external surface, assumed its present form.

Another important step in the improvement of the Leyden jar was also due to the suggestion of Dr. Bevis. It appeared that the force of the charge increased with the magnitude of the jar, but not in proportion to the quantity of water it contained. It was conjectured that it might depend on the extent of the surface of glass in contact with water; and that as water was considered to play the part merely of a conductor in the experiment, metal, which was a better conductor, would be at least equally effectual. Three phials were therefore procured and filled to the usual height with shot instead of water. A metallic communication was made between the shot contained in each of them, and the result was a charge of greatly augmented force. This was, in fact, the first electric battery.

Dr. Bevis now saw that the seat of the electric influence was the surface of contact of the metal* and the glass, and rightly inferred that the form of a bottle or jar was not, in any way, connected with the principle of the experiment. He, therefore, took a common pane of glass, and having coated the opposite faces with tin-foil, to within an inch of the edge, obtained as strong a charge from it as from a phial having the same extent of coated surface. Dr. Watson being informed of this, coated large jars, made of thin glass, on the inside and outside with silver leaf, extending nearly to the top of the jars, the effects of which fully corroborated the anticipations of Dr. Bevis, and established the law that the force of the charge was proportional to the extent of coated surface, and to the thinness of the glass.†

Experiments on the transmission and velocity of electricity, to which the new discovery lent a fresh and fascinating interest, were now resumed. Daniel Gralath, early in 1746, was the first to transmit the

* This question was very beautifully settled a year or two later by the celebrated Benjamin Franklin. He charged a jar, and then insulating it, removed the cork and the wire by which the electricity was conveyed from the machine to the inside of the jar. On examining these he found them free from electricity. He next carefully decanted the water from the charged jar into another insulated vessel. On examining this it was also found to be free from electricity. Other water in its natural state was now introduced into the charged jar to replace that which had been decanted, and on placing one hand on the outside coating, and the other in the water, he received the shock as forcibly as if no change had been made in the jar since it was first charged.—Priestley, p. 144.

† Priestley, pp. 82–7.

shock to a distance, which he did by discharging a battery, composed of several jars, through a chain of twenty persons, with outstretched arms. In May 1746, Joseph Franz, at Vienna, discharged a jar through 1500 feet of iron, and, in the following July, Winkler charged, as well as discharged, a battery of three jars through an insulated wire, thirty ells long, and laid along the bank of the river Pleisse, whose waters formed the return half of the circuit.*

The Abbé Nollet, whose name is famous in the annals of this period, had meanwhile taken up the subject in France. He first, April 1746, transmitted the shock of a Leyden jar through a chain of 180 of the Royal Guards at Paris, and soon after performed a grander experiment of the same kind at the Carthusian convent. By means of iron wires stretched between every two of the monks he formed a large circle of 5400 feet, through which he discharged his jars, with the result in every case that, at the moment of discharge, all the persons in the circuit gave a sudden spring, showing that the shock was felt by each at the same instant and to the same degree of intensity.

* Winkler had previously, in 1744, ascertained that the rapidity of an electric discharge was exceedingly great and comparable with the speed of lightning. He also, as the result of his experiments, concluded "that electricity could be transmitted to the ends of the earth, if a conducting body covered, or insulated, with silk be laid so far, it being only necessary to consider that there may be a certain amount of resistance to the transmission."—*Thoughts on the Properties, Operations, and Causes of Electricity*, Leipsic, 1744, pp. 146, 149.

Lemonnier, the younger, also of Paris, employed still longer circuits composed of 2000 toises (12,780 feet) of iron wire laid along the ground, and, although some of the wire dragged upon wet grass, through hedges, and over newly-ploughed fields, the shock was in no way diminished, a fact which was then thought very surprising. In other experiments he made use of two large basins of water in the gardens of the Tuileries. In April 1746, in the court of the Carthusians, he so laid out two parallel wires of 5700 feet each, that all four ends were close together. Between one pair he placed a jar, and grasped the other extremities himself; then on causing the circuit to be completed, he could not distinguish any interval (so short was it) between the spark at the jar, and the shock through his arms.*

Upon receiving an account of these performances from Lemonnier, our own distinguished countryman, Watson, took up the inquiry, and pursued it so successfully as not only to eclipse the achievements of his neighbours, but to gain for himself in after years the credit of being the first to propose an electric telegraph —an idea which, as we shall presently see, is quite erroneous.† Watson's experiments were very numerous, and were carried out on a grand scale, under the auspices of a committee of the Royal Society, con-

* Priestley, pp. 92–5.

† The suggestion has been claimed for Franklin and Cavendish, and with as little reason. It is time that writers on the telegraph ceased to bandy pretensions for which there is no foundation whatever.

sisting of Mr. Folkes, Lord C. Cavendish, Dr. Bevis, and others. As preparing the way surely, though unsuspectedly, for the first suggestions of an electric telegraph, these investigations must ever possess a peculiar interest for telegraphists, and we therefore make no apology for presenting to our readers the following detailed account of them, for which we are indebted to Dr. Priestley's work, pp. 95–102.

Dr. Watson, who wrote a full account* of the labours of the Committee for the Royal Society, begins with observing (which was verified in all their experiments) that the electric shock is not, strictly speaking, conducted in the shortest manner possible, unless the bodies through which it passes conduct equally well. The circuit, he says, is always formed through the best conductors, though the length be ever so great—a most sagacious observation for the man and the time.

The first trials took place on the 14th and 18th July, 1747, on a wire carried from one side of the Thames to the other over old Westminster Bridge. One end of this wire communicated with the interior of a charged Leyden jar, the other was held by a person on the opposite bank of the river, who also held in his other hand an iron rod which he dipped into the water. Near the jar stood another person holding in one hand a wire communicating with the exterior

* *An Account of the Experiments made by some Gentlemen of the Royal Society*, &c., 8vo., London, 1748.

coating of the jar, and in the other an iron rod. On dipping this into the water and thus completing the circuit for the discharge, the shock was instantly felt by both persons, but more strongly by him who stood near to the jar—because, as Watson rightly stated, part of the electricity went from the wire down the moist stonework of the bridge, thereby making several shorter circuits to the jar, but still all passing through the observer who stood near it.

The next attempt was to force the shock through a circuit of two miles at the New River, near London. This was accomplished on the 24th July at two places, at one of which the distance by land was 800 feet, and by water 2000; and at the other, 2800 feet of land and 8000 feet of water.

The disposition of the apparatus was similar to that at Westminster Bridge, and the results were equally satisfactory. On repeating the experiments, however, the rods, instead of being dipped into the water, were merely thrust into the ground about twenty feet from the water's edge. The effect was the same, as it was found that the shock was equally well transmitted. This occasioned a doubt whether in the former case the shock might not have been conveyed through the ground between the two rods, instead of passing through all the windings of the river, and subsequent experiments showed that such was the case. Other experiments followed at the same place, on the 28th July, when for the first time the wire was supported in its whole length by dry sticks, and on the 5th August, at Highbury Barn, when it was found that dry ground conducted the electric virtue quite as well as water.

Finally, on the 14th August at Shooter's Hill, an experiment was made "to try whether the electric shock was perceptible at twice the distance to which it had yet been carried, in ground perfectly dry, and where no water was near; and also to distinguish if possible its velocity as compared with that of sound." The circuit consisted of two miles of wire, and two miles of perfectly dry ground, but one shower of rain having fallen in the previous five weeks. The wire from the inner coating of the jar was 6732 feet long, and was supported all the way upon baked sticks, and that which communicated with the outer coating was similarly insulated, and was 3868 feet long. The observers placed at the ends of these wires, two miles apart, were provided with stop watches with which to note the moment that they felt the shock. The result of a series of careful observations was that "as far as could be distinguished the time in which the electric matter performed its circuit might have been instantaneous."

Not satisfied, apparently, with this result, the inquiry was resumed in the following year, when a series of trials was performed after the manner of Lemonnier's Carthusian experiment of 1746. On the 5th August, 1748, a circuit of two miles was formed at Shooter's

Hill by several turnings of wire in the same field. The middle of this wire was led into the same room as the Leyden jar, and there Watson placed himself in the centre of the line, taking in each hand the ends of the wire, and noting the spark with his eye while he felt the shock in his arms. Under these circumstances the jar was discharged several times, but in no instance could the observer distinguish the slightest interval between the moments at which the spark was seen and the shock felt; whereupon it was decided that the time occupied by the passage of electricity along 6138 feet of wire was altogether inappreciable.

In 1748 Benjamin Franklin performed his celebrated experiments across the Schuylkill at Philadelphia, and De Luc some months later (1749) across the Lake of Geneva. Franklin thus playfully refers to his experiments at the end of a letter to his friend and correspondent, Peter Collinson, of London, dated Philadelphia, 1748:—

"Chagrined a little that we have hitherto been able to produce nothing in this way of use to mankind, and the hot weather coming on, when electrical experiments are not so agreeable, 'tis proposed to put an end to them for this season, somewhat humorously, in a party of pleasure on the banks of the Skuylkil. Spirits at the same time are to be fired by a spark sent from side to side through the river, *without any other conductor than the water*—an experiment which we some time since performed, to the amazement of many. A turkey is to be killed for our dinner by the electrical shock, and roasted by the electrical jack, before a fire kindled by the electrified bottle, when the healths of all the famous electricians in England, Holland, France, and Germany are to be drank in electrified bumpers, under the discharge of guns from the electrical battery." *

As the words that we have italicised in this extract are apt to mislead, and indeed have misled, some writers into supposing that Franklin here describes an experiment akin to that of telegraphing without wires, from which so much was expected forty years ago, we quote the following details from vol. i. p. 202, of *Franklin's Complete Works*, London, 1806:—"Two iron rods, about three feet long, were planted just within the margin of the river, on the opposite sides. A thick piece of wire, with a small round knob at its end, was fixed on the top of one of the rods, bending downwards, so as to deliver commodiously the spark upon the surface of the spirit. A small wire, fastened by one end to the handle of the spoon containing the spirit, was carried across the river, and supported in the air by

* "An electric battery, famous because it was once owned and operated by Benjamin Franklin, and other distinguished scientific men, has been in constant use at Dartmouth College for years, and is now employed almost daily for class-room experiments in physics. It was at one time in the hands of the celebrated Dr. Priestley, the discoverer of hydrogen."—American newspaper. Another interesting relic—Faraday's first electrical machine—is still in vigorous action at the Royal Institution, and was used by Dr. Gladstone to illustrate his Christmas Lectures in 1874-5.

the rope commonly used to hold by, in drawing ferry-boats over. The other end of this wire was tied round the coating of the bottle, which, being charged, the spark was delivered from the hook to the top of the rod standing in the water on that side. At the same instant the rod on the other side delivered a spark to the spoon and fired the spirit, the electric fire returning to the coating of the bottle, through the handle of the spoon, and the supported wire connected with them." The experiment was, therefore, precisely the same as that of Watson across the Thames, the only difference being in the words used to describe it. In the one case the discharge is said to go out by the water and return by the wire, and in the other to go out by the wire and return by the water.

Notwithstanding the singular suggestiveness of all these experiments, no one up to this time appears to have entertained the faintest suspicion of their applicability to telegraphic purposes; or, indeed, to any useful purpose whatever. Thus Watson, in a letter to the Royal Society, says :—"If it should be asked to what useful purposes the effects of electricity can be applied, it may be answered that we are not yet so far advanced in these discoveries as to render them conducive to the service of mankind," but, he adds, "future philosophers may deduce from them uses extremely beneficial to society in general." This was in 1746, and with reference to his then recent ignition of spirits by the spark; but even after his brilliant experiments in the following years, of which we have just given an account, he does not appear to have formed any more hopeful view. We also find the great Franklin, who was always in search of the practical in science, positively expressing his disappointment in the letter just quoted at being unable to find any useful application of electricity.*

* His suggestion of the lightning-conductor was not made until towards the end of July 1750. For this he was indebted to an experiment of his friend, Thomas Hopkinson. This philosopher electrified a small iron ball, to which he fixed a needle, in the hope that from the point, as from a focus, he would draw a stronger spark. Greatly surprised at finding that, instead of increasing the spark, the point dissipated it altogether, he mentioned his failure to Franklin. On repeating the experiment, the latter ascertained, not only that the ball could not be electrified when a needle was fastened to it, but that, when the needle was removed and the ball charged, the charge was silently and speedily withdrawn, when a point connected with the earth was presented to it. Reflecting on this, Franklin conceived the idea that pointed rods of iron fixed in the air might draw down the lightning without noise or danger.—*Franklin's Complete Works*, vol. i. p. 172, London, 1806.

CHAPTER III.

TELEGRAPHS BASED ON STATIC, OR FRICTIONAL, ELECTRICITY.

"Canst thou send lightnings, that they may go, and say unto thee, Here we are?"—*Job* xxxviii. 35.

1753.—*C. M.'s Telegraph.*

THE first distinct proposal to employ electricity for the transmission of intelligence, of which we have any record, is that contained in a letter printed in the number of the *Scots' Magazine*, Edinburgh, for February 17, 1753. As this is one of the most interesting documents to be found in the whole history of telegraphy, we will quote it *in extenso* for the benefit of our readers :—

To the Author of the *Scots' Magazine.*

"Renfrew, Feb. 1, 1753.

"Sir,—It is well known to all who are conversant in electrical experiments, that the electric power may be propagated along a small wire, from one place to another, without being sensibly abated by the length of its progress. Let, then, a set of wires, equal in number to the letters of the alphabet, be extended horizontally between two given places, parallel to one another, and each of them about an inch distant from that next to it. At every twenty yards' end, let them be fixed in glass, or jeweller's cement, to some firm body, both to prevent them from touching the earth, or any other non-electric, and from breaking by their own gravity. Let the electric gun-barrel be placed at right angles with the extremities of the wires, and about an inch below them. Also let the wires be fixed in a solid piece of glass, at six inches from the end; and let that part of them which reaches from the glass to the machine have sufficient spring and stiffness to recover its situation after having been brought in contact with the barrel. Close by the supporting glass, let a ball be suspended from every wire; and about a sixth or an eighth of an inch below the balls, place the letters of the alphabet, marked on bits of paper, or any other substance that may be light enough to rise to the electrified ball; and at the same time let it be so contrived, that each of them may re-assume its proper place when dropt.*

"All things constructed as above, and the minute previously fixed, I begin the conversation with my distant friend in this manner. Having set the electrical machine a-going as in ordinary experiments, suppose I am to pronounce the word *Sir*; with a piece of glass, or any other *electric per se*, I strike the wire S,

* It will be observed that in this and most other systems based upon common, or frictional, electricity, the authors constantly, although often unknowingly, used the earth circuit.

so as to bring it in contact with the barrel, then *i*, then *r*, all in the same way; and my correspondent, almost in the same instant, observes these several characters rise in order to the electrified balls at his end of the wires. Thus I spell away as long as I think fit; and my correspondent, for the sake of memory, writes the characters as they rise, and may join and read them afterwards as often as he inclines. Upon a signal given, or from choice, I stop the machine; and, taking up the pen in my turn, I write down whatever my friend at the other end strikes out.

"If anybody should think this way tiresome, let him, instead of the balls, suspend a range of bells from the roof, equal in number to the letters of the alphabet; gradually decreasing in size from the bell A to Z; and from the horizontal wires, let there be another set reaching to the several bells; one, *viz.*, from the horizontal wire A to the bell A, another from the horizontal wire B to the bell B, &c. Then let him who begins the discourse bring the wires in contact with the barrel, as before; and the electrical spark, breaking on bells of different size, will inform his correspondent by the sound what wires have been touched. And thus, by some practice, they may come to understand the language of the chimes in whole words, without being put to the trouble of noting down every letter.

"The same thing may be otherwise effected. Let the balls be suspended over the characters as before, but instead of bringing the ends of the horizontal wires in contact with the barrel, let a second set reach from the electrified cake, so as to be in contact with the horizontal ones; and let it be so contrived, at the same time, that any of them may be removed from its corresponding horizontal by the slightest touch, and may bring itself again into contact when left at liberty. This may be done by the help of a small spring and slider, or twenty other methods, which the least ingenuity will discover. In this way the characters will always adhere to the balls, excepting when any one of the secondaries is removed from contact with its horizontal; and then the letter at the other end of the horizontal will immediately drop from its ball. But I mention this only by way of variety.

"Some may, perhaps, think that although the electric fire has not been observed to diminish sensibly in its progress through any length of wire that has been tried hitherto, yet as that has never exceeded some thirty, or forty, yards, it may be reasonably supposed that in a far greater length it would be remarkably diminished, and probably would be entirely drained off in a few miles by the surrounding air. To prevent the objection, and save longer argument, lay over the wires from one end to the other with a thin coat of jeweller's cement. This may be done for a trifle of additional expense, and, as it is an *electric per se*, will effectually secure any part of the fire from mixing with the atmosphere.—I am, &c., "C. M."

From the concluding paragraph it is evident that the writer was not acquainted with Watson's experiments, as detailed in our last chapter, else he would not have suggested insulating the wires, from end to end, with jeweller's cement, and, probably, not even have noticed the objection at all. His suggestions of reading by sound of differently-toned bells, and of keeping his wires charged with electricity, and indicating the signals by discharge, are very ingenious, and deserve to be remembered to his credit in these days of their realisation. The former plan is familiar to us in Bright's Acoustic, or Bell, telegraph of 1855, while the latter was, as we shall presently see, employed by Ronalds in 1816, and is realised to perfection in the method now used in signalling through all long cables.

Unfortunately, little, or nothing, is known of C. M. An inquiry as to his identity was first started by "Inquirendo," Glasgow, in *Notes and Queries*, for October 15, 1853; then by George Blair, also of Glasgow, in the Glasgow *Reformers' Gazette*, for November 1853, in which he, for the first time, republished C. M.'s letter; and, lastly, by Sir David Brewster, in the Glasgow *Commonwealth*, for January 21, 1854. Nothing, however, came of the inquiry for a long time, and all hopes of solving the question were abandoned, when, on December 8, 1858, the following letter appeared:—

"To the Editor of the *Commonwealth.*

"145, Great Eastern Road.

"Sir,—I have not heard that a name has yet been proposed for the C. M. that wrote to the *Scots' Magazine* last century from Renfrew, giving some hints about the electric telegraph.

"I send you what follows, as I think it gives some probability to C. M. being Charles Marshall.

"In our house was a copy of Knox's *History of the Reformation*, published in Paisley, in 1791. My uncle James's name is in the list of subscribers in Renfrew. Anent this my mother spoke as follows :—'There was a very clever man living in Paisley at that time, that had formerly lived in Renfrew. He asked my uncle, as they were acquainted, to canvass for subscribers in Renfrew. The said clever man could light a room with coal reek, and make lightning speak and write upon the wall,' &c.

"That this was the C. M. of the electric telegraph there can, I think, be no doubt.

"Now, it is probable that the man that solicited my uncle to canvass for subscribers subscribed himself; and in Well Meadow, Paisley, I find the name Charles Marshall, and this is the only name in the list of 1000 names that answers the initials C. M. My list, however, is not complete for Glasgow.

"Peradventure some one belonging to Paisley may have somewhat to say of Charles Marshall.

"ALEX. DICK."

To this letter were appended the following remarks by Sir David Brewster, to whom the editor appears to have submitted it prior to publication:—"That Charles Marshall might have been the inventor, had we known nothing more than that he was a resident in Renfrew about the time when the letter was sent to the *Scots' Magazine*, was very probable; but when we add to this probability the fact that Charles Marshall was a clever man, and that he was known as a person who could make lightning speak and write upon the wall, and who could also light a room with coal reek (smoke), we can hardly doubt that he was the C. M. who invented the electric telegraph, and that he is entitled to the additional honour of having first invented and used gas from coal."*

Commenting on this correspondence, in *Notes and Queries*, for July 14, 1860, George Blair says:—"That the Charles Marshall who resided at Well Meadows, Paisley, in 1791, was not the C. M. of the *Scots' Magazine*, and, therefore, not the inventor of the electric telegraph, I succeeded in ascertaining positively about a year ago, on the highest possible authority. Through the kindness of a venerable friend in Paisley, I traced out the fact that a Charles Marshall, who once resided in the Well Meadows, had come from Aberdeen; and that a son of his, a clergyman, was still living. Discovering the address of this gentleman, I applied

* These letters, copied from the *Commonwealth*, are reprinted in the *Engineer*, for Dec. 24, 1858, p. 484.

to him for information; and he states in his reply that he had no doubt his father was the Charles Marshall who appears in Mr. Dick's list; but that he could not be the C. M. of the *Scots' Magazine.*

* * * * * *

"At the time when C. M.'s letter was first disinterred, the most diligent search was made by the schoolmaster of Renfrew, who is also session-clerk, not only in the records of the kirk-session, but also among the old people of the parish, without a shadow of success; and, strange as it may appear, the name of C. M. remains at the present moment as great a mystery as that of Junius."

Whether Sir David Brewster was aware of these fresh facts we cannot say, but certain it is that, in October 1859, he accepted the evidence in favour of C. M. being a Charles Morrison, with as much warmth, and, we fear, as much haste, as he had done that for Charles Marshall in the previous December. At p. 207 of *The Home Life of Sir David Brewster* (Edinburgh, 1869), Mrs. Gordon says:—"After a good deal of correspondence on the subject, Sir David Brewster gave up all hope of discovering the name of the inventor, and it was not until 1859 that he had the great pleasure of solving the mystery in the following manner:—He received from Mr. Loudon, of Port Glasgow, a letter, dated 31st October, 1859, stating that, while reading the article in the *North British*

Review, his attention was arrested by the letter of C. M., and having mentioned the fact to Mr. Forman, a friend then living with him, he told him that he could solve the mystery regarding these initials. Mr. Forman recollects distinctly having read a letter, dated 1750, and addressed by his grandfather, a farmer, near Stirling, to Miss Margaret Winsgate, residing at Craigengilt, near Denny (to whom he was subsequently married), referring to a gentleman in Renfrew of the name of Charles Morrison, who transmitted messages along wires by means of electricity, and who was a native of Greenock, and bred a surgeon. Mr. Forman also states that he was connected with the tobacco trade in Glasgow, that he was regarded by the people in Renfrew as a sort of wizard, and that he was obliged, or found it convenient, to leave Renfrew and settle in Virginia, where he died. Mr. Forman also recollects reading a letter in the handwriting of Charles Morrison, addressed to Mr. Forman, his grandfather, and dated 25th September, 1752, giving an account of his experiments, and stating that he had sent an account of them to Sir Hans Sloane, the President of the Royal Society of London, who had encouraged him to perfect his experiments, and to whom he had promised to publish an account of what he had done. In this letter Mr. Morrison stated that, as he was likely to be ridiculed by many of his acquaintances, he would publish his paper in the *Scots' Magazine* only with his initials."

How far this statement may be credited we will not undertake to say; we would, however, just point out that Sir Hans Sloane resigned the presidentship of the Royal Society in 1741, and lived in strict retirement at Chelsea until his death, which occurred on January 11, 1752, at the advanced age of ninety-two years. It is not likely, therefore, that he would have received, or written, any letters of the above-mentioned nature in the last days of his life. At any rate, a careful search through his papers, which we have instituted in the British. Museum and the Royal Society, has failed to discover any.

1767.—*Bozolus's Telegraph.*

Joseph Bozolus, a Jesuit and lecturer on natural philosophy in the College at Rome, was the next to suggest an electric telegraph, and one in which the spark was the active principle. This must have been some time anterior to 1767, as we find it familiarly described in a Latin poem,* published in that year.

His proposition was to lay underground two (? insulated) wires between the communicating stations, which may be any distance apart. At both stations the ends of the wires were to be brought close together, without touching, so as to facilitate the passage of a spark. When, under these circumstances, at one end, the inner coating of a charged plate, or jar, was con-

* *Electricorum*, by Josephus Marianus Parthenius (*i. e.*, G. M. Mazzolari), libri vi., 8vo., Romæ, 1767.

nected to one wire, and the outer coating to the other, the discharge would take place through the wires, and manifest itself, at the break, at the distant end, in the form of a spark. An alphabet of such sparks, Bozolus says, could be arranged with a friend without any difficulty, and a means of communication be thus contrived, which, as tolerably easy, he leaves to each one's judgment to devise and settle in detail.

Bozolus appears to have been a man of varied acquirements. As a sort of diversion from more serious studies, he undertook an Italian translation of the *Iliad* and *Odyssey* of Homer, which Mazzolari, himself no mean poet, praises very highly.

As the *Electricorum* is very scarce, and, therefore, not easily accessible, we present our readers with a faithful transcript of the verses descriptive of the telegraph, which we have extracted from a copy of the work in the British Museum.

"Quid dicam, extrema pendentis parte catenæ,
Qui palmam objecit, confestim flamma reluxit,
Tenviaque arguto strepuerunt sibila vento?
Et qui continuos secum prius ordine longo
Disposuit globulos; tum flammam excivit, et ignem
A primo insinuans sollers traduxit ad imum?
Atque hic arte quidem multa omniginæque Minervæ
Instructus studiis vitro impiger instat, et usque
Extundit visenda novis spectacula formis.
Quid? quod et elicitas vario discrimine flammas
Nunquam tentatos idem detorquet ad usus;
Insuetisque notis absentem affatur amicum.
Quippe duo a nexa in longum deducta catena
Aenea fila trahit; spatium distantia amici
Definit certum, verum, quo lumina fallat

Spectantum, et miram quo callidus occulat artem,
Fila solo condit penitus defossa sub imo,
Sic tamen; ut capita emergant tum denique; signa
Conscius opperiens condicta ubi servat amicus.
Ipse autem interea vitri revolubilis orbem
De more exagitans fluctum derivat; et inde,
Qua duo se extrema respectant aenea parte,
Attactum citra et præscripto limite, fila;
Composito tot scintillas educit, ad usum
Quot talem elicitis opus est; quæ singula nempe
Designent elementa; quibus in verba coactis
Sensa animi pateant, certa et sententia constet.
Atque his indiciis, fidaque interprete flamma
Absens absentem dictis compellat amicum."

Lib. i. pp. 32–35.

1773.—*Odier's Telegraph.*

The idea of an electric telegraph appears next to have occurred to Louis Odier, a distinguished physician of Geneva, who thus wrote, in 1773, to a lady of his acquaintance:—

"I shall amuse you, perhaps, in telling you that I have in my head certain experiments by which to enter into conversation with the emperor of Mogol, or of China, the English, the French, or any other people of Europe, in a way that, without inconveniencing yourself, you may intercommunicate all that you wish, at a distance of four or five thousand leagues in less than half an hour! Will that suffice you for glory? There is nothing more real. Whatever be the course of those experiments, they must necessarily lead to some grand discovery; but I have not the courage to undertake them this winter. What gave me the idea was a word which I heard spoken casually the other day at Sir John Pringle's table, where I had the pleasure of dining with Franklin, Priestley, and other great geniuses." *

Although, according to Professor Maunoir, Odier was about this time devoting much attention to electricity, we do not find that he ever attempted to carry out his telegraphic idea.

1777.—*Volta's (so-called) Telegraph.*

At p. 243, vol. i., of *The Journal of the Society of Telegraph Engineers*, we find the following letter :—

"To the Secretary of the Society of Telegraph Engineers.

"Battle, Sussex, July 4th, 1872.

"Sir,—I have not met with any statement in English histories, or other English treatises, on the Electric Telegraph, relative to *Volta's* proposed Electric Telegraph.

"Professor L. Magrini, member of a committee appointed to examine and report upon Volta's library, manuscripts, and instruments, published a paper in the *Atti del Reale Istituto Lombardo*, vol. ii., entitled, *Notizie, Biografiche e Scientifiche su Alessandro Volta.*

* *Chambers's Papers for the People*, 1851, Art. *Electric Communications*, p. 6. Also Dodd's *Railways, Steamers, and Telegraphs*, London and Edinburgh, 1867, p. 226. Odier took out his degrees at Edinburgh, where he might well have read, or heard, of C. M.'s letter in the *Scots' Magazine* of 1753.

This paper was read at various times, in 1861, at the said institute. It contains a paragraph of which the following is a literal translation :—

"'An autograph manuscript, dated Como, 15th April, 1777, which is suspected (and the suspicion was confirmed by one of the sons of Volta) to have been addressed to Professor Barletti, contains various experiments on his pistols, and the singular proposition, very remarkable for that time, of transmitting signals by means of ordinary electricity. Besides the figure, there are particulars conducive to its practical application.

"'This letter is of the greatest interest for the history of the science, inasmuch as it indicates the first bold and certain step in the invention and institution of the electric telegraph.'

"Although our Charles Marshall, of Renfrew, in 1753, and others, forestalled this proposition, it is interesting, as proving that the *in re electrica Princeps* believed in the efficiency of frictional electricity for the purpose.

"I am, Sir, your obedient servant,

"FRANCIS RONALDS.

"G. E. Preece, Esq."

Now, although, as Ronalds says, and as we here see, Volta was not the first to propose an electric telegraph, still we were delighted to learn, on such apparently good authority, that the great Italian philosopher had turned his mind to telegraphy at all, and we eagerly sought for some particulars of his plan. After much trouble we succeeded in getting a copy of the letter referred to by Professor Magrini, and great was our disappointment to find that it contained nothing more than the *suggestion* of an experiment which was *carried out* (though on a lesser scale) thirty years before by Lemonnier, Watson, Franklin, De Luc, and others. In order that the reader may be able to form his own opinion on this point, we give Volta's original letter, as well as a translation, which we have made from the French of César Cantu, the distinguished Italian historian.* Volta says:—

"Quante belle idee di sperienze sorprendenti mi van ribollendo in testa, eseguibili con questo stratagemma di mandare la scintilla elettrica a far lo sbaro della pistola a qualsivoglia distanza e in qualsivoglia direzione e posizione! Invece del colombino che va ad appiccar l'incendio alla macchina di fuochi artificiali, io vi mandero du qualunque sito anche non diretto la scintilla elettrica, che col mezzo della pistola aggiustata al sito della pianta artifiziale, vi metterà fuoco. Sentite. Io non so a quanti miglia un fil di ferro, tirato sul suolo dei campi o della strada, che in fine si ripiegasse indietro, o incontrasse un canal d'acqua di ritorno, condurebbe giusta il sentier segnato la scintilla commovente. Ma prevegga che un lunghissimo

* See *Le Correspondant*, a French scientific periodical, for August 1867, p. 1059, also *Les Mondes*, for December 5, 1867, p. 561.

viaggio, dè tratti di terra molto bagnati, o delle acque scorrenti stabili rebbero troppo presso una communicazione e quioi devierebbe il corso del fuoco elettrico, spiccato dall uncino della caraffa per ricondursi al fondo. Ma se il fil di ferro fosse sostenuto alto da terra da pali di legno qua e là piantati, ex. gr., da Como fine a Milane; e quivi interrotto solamente dalla una pistola, continuasse e venisse in fine a pescare nel canale naviglio, continua col mio lago di Como; non credo impossibile de far lo sbaro della pistola a Milano con una buona boccia di Leyden, da me scaricata in Como."

"The more I reflect, the more I see the beautiful experiments that can be made by means of the spark in exploding the electric pistol at any distance. An iron wire, stretched along the fields, or roads, for I know not how many miles, could conduct the spark. As, however, in long distances moist earth and water-courses would be encountered, which would draw off the electric fire, the wire may be supported on posts placed at regular intervals, say from Como to Milan. At the latter place its continuity would be interrupted only by my electric pistol, from which it would pass into the canal, which communicates with my lake at Como. In this case I do not believe it impossible to explode my pistol at Milan when I discharge a powerful Leyden jar at Como" [through the wire].

"According to this document," says Cantu, "it is incontestable that Volta had in mind an electric telegraph, half a century before those [alluding to Ampère] who have been proclaimed its inventors. The basis of this astonishing discovery lies in the possibility of transmitting to a great distance the electric virtue, and there causing it to manifest itself in signs. Now this is what Volta had clearly perceived, and, further, he indicated a plan, which is to-day universal, of insulating the conducting wire on posts." * Perceiving a fact, or principle, and applying it, are two very different things. Gray, Dufay, Watson, and all those who made experiments on the transmission of electricity long before Volta, perceived the same fact as he did, and, like him, missed its application. To say then, as Professor Magrini does, that Volta's letter indicates the first bold and certain step in the invention and institution of the electric telegraph is to

* *Le Correspondant*, p. 1060. In the course of a somewhat effusive letter on Italy's claim to the discovery of the electric telegraph, Cantu relates the following interesting particulars. The apartments which Volta occupied at Como, were, for a time, preserved in the state in which he left them at his death (March 5, 1827). There one could see his books, papers, machines, even his tobacco pouch, spectacles, decorations, and cane ; in short, everything that becomes a sacred relic when death has removed him who used it. Amongst the pieces of apparatus, were all those which he had himself invented, including the first pile, and that which he took to Paris, in 1801, when invited by Napoleon to repeat his experiments before the Institute.

In consequence of the pecuniary embarrassments of Volta's sons, these precious relics were in danger of being dispersed, when the Academy of Sciences of Lombardy stepped in, and, while it assisted the sons, honoured the father. The whole collection was purchased for 100,000 livres, and lodged in a chamber of the palace of Brera at Milan, where, under the appellation of *Cimeli di Volta*, it is preserved with reverent care.

assign to it a meaning which it was never, we believe, intended to convey; and we are the more confirmed in this opinion by the fact that, although Volta lived to the year 1827, and must have heard of the numerous telegraphic proposals made up to that time, he never claimed to have done anything in that way himself.

1782.—*Anonymous Telegraph.*

The next proposal, which is an exceedingly interesting one, is contained in an anonymous letter to the *Journal de Paris*, No. 150, for May 30, 1782, a translation of which we append:—

"To the Authors of the Journal.

"A way of establishing a communication between two very distant places has been proposed to me, and those of your readers who care for this kind of scientific amusement will not, perhaps, be angry with me for telling them what it is.

"Let there be two gilt iron wires put underground in separate wooden tubes filled in with resin, and let each wire terminate in a knob. Between one pair of knobs, connect a letter formed of metallic [tin-foil] strips after the fashion of those electrical toys, called 'spangled panes'; if, now, at the other end we touch the inside of a Leyden jar to one knob, and the outside to the other, so as to discharge the jar through the wires, the letter will be at the same instant illuminated.

"Thus, with twenty-four such pairs, one could quickly spell all that was desired, it being only requisite to have a sufficient number of charged Leyden jars always ready.

"As it would not be necessary to make the letters very luminous, a slight indication being sufficient, complete darkness would not be required for the perception of the characters, and feebly charged jars would, therefore, suffice, which would greatly facilitate matters. The letters may even be suppressed, and then there would be one instrument common to the twenty-four systems (pairs) of wires [*sic*].

"These means could be simplified by having only five pairs of wires, and attaching a character, or letter, to each of their combinations, 1 1°, 2 2°, * * 5 5°; 1 1°, and 2 2°, 1 1°, and 3 3°; * * 1 1°, 2 2°, and 3 3°; and so on, which would make thirty-one characters; six pairs of wires would, in the same way, yield sixty-three, and thus one could arrive at a sort of tachygraphy, or fast writing, one character (or signal) sufficing for a whole word, or phrase, as may be previously agreed upon. There would be some difficulty, however, in discharging at exactly the same instant several (separate) jars through as many separate pairs. One might also use successive combinations of these pairs, 2 to 2, 3 to 3, and so on, in which way five pairs would give 125 signals, and six 216, which would be very fast writing indeed.

"The wooden tubes might, very probably, be unnecessary ; but in view of accidents, such as fractures, it would always be safer to employ them.

"One could use *simple* electricity [*i. e.*, direct from the machine], and so greatly simplify the apparatus, but as the superficial area of a great length of wire, even when a very fine one was used, would be considerable, this plan would necessitate very powerful machines. In either method, however, the object could easily be obtained by using very large *electrophoroi.*

"It would be necessary to give each correspondent a means of notifying that he wished to communicate, to prevent constant watching and cross signalling. For this an electric bell would suffice, and by agreeing beforehand that one stroke shall mean 'I will call you up in 15 minutes,' two strokes 'I am all attention,' &c., all confusion would be avoided.

"As this letter is only intended for those who amuse themselves with physics, they can easily supply for themselves all the details that I have omitted.

"I have the honour to be, &c."

This letter is copied, almost *verbatim*, in *Le Mercure de France*, for June 8, 1782, and is also embodied in a letter, dated June 5, 1782,* where the writer

* In Metra's *Correspondance Secrète*, &c., Londres, 1788, vol. xiii. p. 84. Mr. Aylmer, to whom we are indebted for the copy of this letter which appeared in *Le Mercure de France*, tells us that the Comte du Moncel attributes it to Le Sage, but we shall presently see reasons for doubting this.

prefaces it with the following remarks: "We have Linguet once more installed in the career in which his labours have been so disagreeably interrupted. His project of an easy communication between two very distant places appears to be only the dream of some pleasant trifler. It is, however, not new, and would only imperfectly accomplish its object; but still there may be some good in it."

In these remarks Metra somewhat mixes his facts. There is no more authority for the statement that Linguet was the writer than that he, at this time, was engaged on experiments on some kind of a *luminous* telegraph, which he planned while a prisoner in the Bastille, and in exchange for which he is popularly, though erroneously, supposed to have received his liberty. On the other hand, we have positive proof that he was not the writer, firstly, in the opening sentence of the letter itself, and secondly, in the following passage from his *Mémoires sur la Bastille:*— "I will one day make known my ideas on this subject [of signalling by means of light]. The invention will certainly admit of being greatly improved, as I have no doubt it will be. I am persuaded that in time it will become the most useful instrument of commerce, and all correspondence of that kind; just as electricity will be the most powerful agent of medicine; and as the fire-pump will be the principle of all mechanic processes which require, or are to communicate, great force" (Note 13).

1782.—*Le Sage's Telegraph.*

On seeing these accounts, George Louis Le Sage,* a *savant* of French extraction, residing at Geneva, published a method somewhat similar to C. M.'s, in a letter, dated June 22, 1782, and addressed to his friend, M. Prévost, at Berlin.

He writes: "I am going to entertain you with one of my old discoveries, which I see has just been found out by others, at least, up to a certain point. It is a ready and swift method of correspondence between two distant places by means of electricity, which occurred to me thirty, or thirty-five, years ago, and which I then reduced to a simple system, far more practicable than the form with which the new inventor has endowed it.

"I have often spoken of it to one or two persons,† but I see no reason for supposing that the new inventor has drawn his ideas from these conversations. The thing is so natural that, to discover it, it is only necessary that one should be in search of some means of very rapid correspondence; and people have, on

* "Upon the present venerable and learned M. le Sage of Geneva devolved, in a great measure, the education of Lord Mahon, who is frequently heard to mention the name of his preceptor with considerable respect. He even goes so far as to pronounce M. le Sage the most learned man in Europe." *Vide* Life of Earl Stanhope, in *Public Characters of* 1800–1801, London, 1801, p. 88.

† In *Le Journal des Sçavans*, 4to., Paris, 1782 (for Sept., p. 637), this extract is prefaced thus:—"Il y a trente ans qu'il en parla, et une personne à qui il en fit part, offre de l'attester; mais ceux qui connoissent la sagacité et la candeur de ce digne citoyen, ne formeront à cet ègard aucun doute."

occasion, turned their minds to this subject * * * *, as, for example, Mr. Linguet.

"But it is time to tell you briefly in what my plan consisted. One can imagine a subterranean tube, of glazed earthenware, the inside of which is divided, at every fathom's length, by diaphragms, or partitions, of glazed earthenware, or of glass, pierced by twenty-four holes, so as to give passage to as many brass wires, which could in this way be supported and kept apart. At each of the extremities of this tube, the twenty-four wires are arranged horizontally, like the keys of a harpsichord, each wire having suspended above it a letter of the alphabet, while immediately underneath, on a table, are pieces of gold leaf, or other bodies that can be as easily attracted, and are, at the same time, easily visible.

"He, who wishes to signal anything, shall touch the ends of the wires with an excited glass tube, according to the order of the letters composing the words; while his correspondent writes down the characters under which he sees the little gold leaves play. The other details are easily supplied."

Le Sage had an idea of offering his invention to Frederick the Great, and drew up an introductory note as follows:—

"To the King of Prussia.

"Sire,—My little fortune is not only sufficient for all my wants, but even for all my tastes—except one, *viz.*, that of contributing to the wants and tastes of others; and this desire all the monarchs of the world, united, could not enable me to fully satisfy. It is not, then, to a patron who can give much, that I take the liberty of dedicating the following discovery, but to a patron who can do much with it, and who can judge for himself of its utility without having to refer it to his advisers." *

Whether he ever carried out this idea or not is difficult to say, but it is certain that his plan was never practically tried, and, like so many of its class, was soon forgotten.

1787.—*Lomond's Telegraph.*

The next plan that we have to notice was a decided improvement, and had an actual existence, though on a very small scale. Seeing, no doubt, the difficulty and expense of using many wires, Lomond of Paris reduced, at one sweep, the number to one, and thus produced a really serviceable telegraph. Arthur Young, the diligent writer on natural and industrial resources, saw this apparatus in action during his first visit to Paris, and thus describes it in his journal, under date October 16, 1787:—

* See *Notice de la vie et des écrits de George-Louis Le Sage de Genève*, &c., par Pierre Prévost, 8vo., Genève, 1805, pp. 176–7. All writers on the Electric Telegraph, copying Moigno (*Traité de Télégraphie Électrique*, Paris, 1849 and 1852), say that Le Sage actually established his telegraph at Geneva in 1774—an assertion for which we have not been able to find any authority.

"In the evening to M. Lomond, a very ingenious and inventive mechanic, who has made an improvement of the jenny for spinning cotton; common machines are said to make too hard a thread for certain fabrics, but this forms it loose and spongy. In electricity he has made a remarkable discovery. You write two or three words on a paper; he takes it with him into a room and turns a machine enclosed in a cylindrical case, at the top of which is an electrometer, a small fine pith-ball*; a wire connects with a similar cylinder and electrometer in a distant apartment, and his wife, by remarking the corresponding motions of the ball, writes down the words they indicate, from which it appears that he has formed an alphabet of motions. As the length of the wire makes no difference in the effect, a correspondence might be carried on at any distance; within and without a besieged town for instance, or for a purpose much more worthy, and a thousand times more harmless—between two lovers prohibited, or prevented, from any better connection. Whatever the use may

* Soon after the discovery of the Leyden jar the necessity of some sufficient indicator of the presence and power of electricity began to be felt, and after some clumsy attempts at an electrometer by Gralath, Ellicott, and others, the Abbé Nollet adopted the simple expedient of suspending two threads, which when electrified would separate by their mutual repulsion. Waitz hung little leaden pellets from the threads for greater steadiness, and Canton, in 1753, improved upon this by substituting two pith balls suspended in contact by fine wires—a contrivance which is used to this day. The electrometer mentioned in the text was of the kind known as the quadrant electrometer, introduced by Henley in 1772.

be, the invention is beautiful. Mons. Lomond has made many other curious machines, all the entire work of his own hands. Mechanical invention seems to be in him a natural propensity."*

As in all systems where the signals were indicated by electroscopes, or electrometers, their action would continue so long as the charge communicated to the wires lasted, and, as during this time it would not be possible to make another signal, the authors must in some way have discharged the wires after every signal, so as to allow the balls, gold leaves, or other indicators, to resume their normal position. This they might have done, either by touching the wires with the finger after the signal had been noted, or by making the indicators themselves strike against some body that would convey their charges to earth. But, probably, there was no need for any such stratagem, as the insulation of the wires would be so imperfect, and the speed of signalling so slow, that the inconvenience would not have been felt.

1790.—*Chappe's Telegraph.*

Most of our readers have, doubtless, heard of Claude Chappe's Semaphore, or Optico-mechanical Telegraph, which, in one form or another (for, like all successful inventions, it had many imitators), did such good service in the first half of this century. Few, however,

* *Travels during the years* 1787, 1788, *and* 1789, *&c., in the Kingdom of France*, Dublin, 1793, vol. i. p. 135.

are aware that, before deciding on this form of instrument, he essayed the employment of electricity for telegraphic purposes.

Reserving a full account of Claude Chappe's life and works for its proper place in our General History of Telegraphy, which we hope soon to publish, we need only concern ourselves here with a brief reference to his early experiments with electricity.

In 1790, he conceived the idea of a telegraph. He first employed two clocks, marking seconds, in combination with sound signals, which were produced by striking on that homely utensil, a stewpan (*casserole*). Round the seconds dials were marked off equal spaces corresponding to the numerals 1 to 9, and the cipher 0. The clocks being so regulated that the second hands moved in unison, pointing to the same figures at the same instant, it is clear that, in order to indicate any particular figure, Chappe had only to strike the stewpan the moment the hand of his dial entered the space occupied by that figure; his correspondent, hearing the sound, must necessarily note the same symbol; and so, successive figures, or groups of figures, answering to words and phrases in a vocabulary, could be indicated with great ease and rapidity.

But as sound travels so comparatively slowly, it would in long distances lag behind, and indicate, it may be, only an A, or B, when an E, or G, was intended. Under these circumstances it was but natural that Chappe should bethink himself of electricity, of which he was a diligent student, and on which he had just communicated a series of papers to the *Journal de Physique* (which, by the way, obtained his election as a member of the Philomathic Society).

He erected insulated wires for a certain distance,* and arranged that the discharge of a Leyden jar should indicate the precise moment for noting the position of the hands; but while he was thus removing one difficulty he found himself introducing another, *viz.*, one of electrical insulation. The more he extended his wires, the greater, of course, his difficulty became, until in despair he abandoned the use of electricity, and took to that of optico-mechanics.

In the actual state of telegraphy this circumstance becomes an interesting one, for Chappe held in his hands a power which was destined soon, under another form, to demolish the grand structure on which he was about to spend so much time and labour. Happily, perhaps, he did not live to experience this mortification, for he died January 23, 1805, at the early age of forty-two.

1790.—*Réveroni-Saint-Cyr's Telegraph.*

This gallant officer is said to have proposed in this year an electric telegraph for announcing the result of the lottery drawings, so as to frustrate the knaveries

* Gerspach's *Histoire Administrative de la Télégraphie Aérienne en France*, Paris, 1861, p. 7.

of certain individuals; but, apparently, details are wanting.*

1794.—*Reusser's Telegraph.*

The next proposal of which we have to speak, and which, in comparison with Lomond's, or Chappe's, was a very clumsy one, is thus described by its author:†—

"I have lately contrived a species of electric letter post, by means of which a letter may be sent in one moment to a great distance. I sit at home before my electric machine, and I dictate to some one, on the other side of the street, an entire letter, which he himself writes down. On an ordinary table is fixed, in an upright position, a square board to which a glass plate is fastened. On this plate are glued little squares of tin-foil, cut after the fashion of luminous panes, and each standing for a letter of the alphabet. From one side of these little squares extend long wires, enclosed in glass tubes, which go, underground, to the place whither the despatch is to be transmitted. The distant ends are there connected to tin-foil strips similar in all respects to the first, and, like them, each marked by a letter of the alphabet; the free ends of all the strips are connected to one return-wire, which goes to the transmitting table. If, now, one touches the outer coating of a Leyden jar with the return-wire,

* Etenaud's *La Télégraphie Électrique*, &c., Montpellier, 1872, vol. i. p. 27.

† Voigt's *Magazin für das Neueste aus der Physik*, vol. ix. part i. p. 183.

and connects the inner coating with the free end of that piece of tin-foil which corresponds to the letter required to be indicated, sparks will be produced, as well at the near, as at the distant tin-foil, and the correspondent there watching will write down the letter."

Reusser concludes: "Will the execution of this plan, on a large scale, ever take place? That is not the question. It is possible, though it would cost a good deal, but post horses from St. Petersburg to Lisbon are also very expensive. At any rate, whenever the idea is realised I will claim a recompense."

The editor, Johann Heinrich Voigt, appends to the above communication the suggestion of an alarum, which is usually credited to Reusser himself. Voigt says: "Mr. Reusser ought to have proposed to add to his arrangement a flask of some detonating gas, which one could explode by means of the electric spark, and so attract the attention of the distant correspondent to his tin-foil squares."

In comparing the accounts of Reusser's telegraph usually given with our own, many inaccuracies will be observed. Thus, most writers affirm that each piece of tin-foil was cut into the form of a letter of the alphabet, which, on the passage of the spark, became luminous, as in the French telegraph of 1782, or in that of Salvá, which will presently be described. The German text does not admit of this interpretation, for, if such were the case, it would have been unnecessary to affix letters to the squares of tin-foil. Neither is there any authority for the statement that thirty-six circuits for letters and numerals were proposed, which, according to some writers, were entirely metallic, and, therefore, consisted of seventy-two wires, while others assert that there were only thirty-six wires, and that the earth was employed to complete the circuits. Again, it is always said that Reusser, or rather Voigt, was the first to propose an alarum, whereas we have seen that this was done, twelve years before, by the anonymous correspondent of the *Journal de Paris*, 1782.

1794–5.—*Böckmann's, Lullin's, and Cavallo's Telegraphs.*

Böckmann, Lullin, and Cavallo, all about this time, proposed various modifications of Reusser's plan, all requiring but one or two wires, and differing only in their methods of combining the sparks and intervals into a code. Böckmann's, which is a mere suggestion, is to be found at p. 17 of his *Versuch über Telegraphie und Telegraphen*, published at Carlsruhe in 1794;* Lullin's we have not been able to trace further back than Reid's *The Telegraph in America*, New York, 1879, p. 69; while Cavallo's is described, at length, in his *Complete Treatise on Electricity*, &c.,† from which we condense the following account:—

"The attempts recently made," says Cavallo, "to convey intelligence from one place to another at a great distance, with the utmost quickness, have in-

* Also Zetzsche's *Geschichte der Elektrischen Telegraphie*, p. 32.
† Fourth edition, London, 1795, vol. iii. pp. 285–96.

duced me to publish the following experiments, which I made some years ago, and of which I should not have taken any further notice, had it not been for the above-mentioned circumstance, which shows that they may possibly be of use for that and other purposes."

The object for which those experiments were performed was to fire gunpowder, or other combustible matter, from a great distance, by means of electricity. At first a circuit was made with a very long brass wire, the two ends of which returned to the same place, whilst the middle was at a great distance. At this (middle) point an interruption was made, in which a cartridge of gunpowder, mixed with steel filings, was placed. Then, by applying a charged Leyden phial to the two extremities of the wire (in the usual way) the cartridge was fired.

It proving very troublesome to keep the wires from touching, the experiment was tried with one wire only. A brass wire, one-fiftieth of an inch diameter, and two hundred feet long, was laid on the ground, and one end was inserted in the cartridge of gunpowder and steel filings. Another piece of the same wire had, likewise, one end inserted in the cartridge, whilst the other was thrust into the ground. The distant end of the wire was then connected to the inner coating of a charged jar, while the outer coating was touched with a ground wire. That the discharge took place as before, was proved by the powder being sometimes fired.

Phosphorus and other combustible substances were next tried, but nothing was found to succeed so well as a mixture of inflammable and common air, confined in specially prepared flasks.

Having made this discovery, Cavallo next directed his attention to the best means of insulating the communicating wire, and at last so contrived that it might be laid indifferently on wet or dry ground, or even through water.

"A piece of annealed copper or brass wire," he says, "being stretched from one side of a room to the other, heat it by means of a flame of a candle, or of a red-hot piece of iron, and, as you proceed, rub a lump of pitch over the part just heated. When the wire has been thus covered, a slip of linen rag must be put round it, which can be easily made to adhere, and over this rag another coat of melted pitch must be laid with a brush. This second layer must be covered with a slip of woollen cloth, which must be fastened by means of a needle and thread. Lastly, the cloth must be covered with a thick coat of oil paint. In this manner many pieces of wire, each of about twenty or thirty feet in length, may be prepared, which may afterwards be joined together, so as to form one continued metallic communication; but care must be taken to secure the places where the pieces are joined, which is most readily done by wrapping a piece of oil-silk over the painted cloth, and binding it with thread. When a long wire has been thus made out of the various short pieces, let one end of it be formed into a ring, and to the other adapt a small brass ball.

"Through the wires so prepared the flask of inflammable air was always exploded, and whenever the discharge was passed through a flask of common air a spark was seen, and by sending a number of such sparks at different intervals of time according to a settled plan, any sort of intelligence might be conveyed instantaneously from the place in which the operator stands to the other place in which the flask is situated." *

"With respect to the greatest distance to which such communication might be extended," concludes Cavallo, "I can only say that I never tried the experiment with a wire of communication longer than about two hundred and fifty feet; but from the results of those experiments, and from the analogy of other facts, I am led to believe that the above-mentioned sort of communication might be extended to two or three miles, and probably to a much greater distance."

1795-8.—*Salvá's Telegraph.*

Of all the pioneers of the electric telegraph in the last century, Don Francisco Salvá, of Barcelona,†

* Moigno (*Télégraphie Électrique*, p. 61) says Cavallo proposed to express signals by the explosion, by the spark, of such substances as gunpowder, phosphorus, phosphuretted hydrogen, &c., but this is an error.

† Don Francisco Salvá y Campillo was born at Barcelona, July 12, 1751. After graduating, with honours, in the universities of his native place, of Valencia, of Huesca, and of Tolosa, he travelled in Italy, France, and other parts of the Continent, and made the acquaintance

deserves the most honourable mention, as well for the extent and completeness of his designs, as for the zeal and intelligence with which he carried them out. His proposals are described with great clearness in a memoir which he read before the Academy of Sciences, Barcelona, December 16, 1795, and from which we cannot do better than make some extracts :*—

"If," he says, "there were a wire from this city to Mataro, and another from Mataro back, and a man were there to take hold of the ends, we might, with a Leyden jar, give him a shock from this end, and so advise him of any matter previously agreed on, such as a friend's death. But this is not enough, as, if electricity is to be of any use in telegraphy, it must be capable of communicating every kind of information whatsoever; it must, in a word, be able to speak. This is happily of no great difficulty.

"With twenty-two letters, or even with eighteen, we can express, with sufficient precision, every word in the language, and thus, with forty-four wires from Mataro to Barcelona, twenty-two men there, each to take hold of a pair of wires, and twenty-two charged Leyden jars here, we could speak with Mataro, each man there representing a letter of the alphabet, and giving

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of many of its learned men, including Le Sage, Reusser, and other well-known electricians. Besides being an able electrician, Salvá was a distinguished physician, and ardently promoted the cause of vaccination in Spain. He died February 13, 1828. See Saavedra's Biography in the *Revista de Telegrafos* for 1876.

* Translated from Saavedra's *Tratado de Telegrafia*, 2nd ed., pp. 11[illegible] [illegible]l. i.

notice when he felt the shock. Let us suppose that those receive shocks who represent the letters p, e, d, r, o, we shall then have transmitted the word Pedro. All this is within the limits of possibility; but let us see if it cannot be simplified.*

"It is not necessary to keep twenty-two men at Mataro, nor twenty-two Leyden jars at Barcelona, if we fix the ends of each pair of the wires in such a way that one or two men may be able to discriminate the signals. In this way six or eight jars at each end would suffice for intercommunication, for, of course,

* Zetzsche (*Geschichte der Elektrischen Telegraphie*, p. 21) says no attempt had been made to construct a telegraph with the physiological effects of static electricity for its basis. Salvá's is an early example; here is another, though of a negative kind. The Rev. J. Gamble, in his excellent treatise on Semaphoric Telegraphs, says, in reviewing the different modes of communication that had been proposed up to his time:—

"Full as many, if not greater, objections will probably operate against every contrivance where electricity shall be used as the vehicle of information. The velocity with which this fluid passes, where the conductors are tolerably perfect, and also that it may be made to pass through water to a very great distance, when it forms part of the circuit, are properties which appear to have given rise to the idea of using it as a means of correspondence. I have never [?even] heard it mentioned, that an alarm may be given to a very great distance, by firing a pistol charged with inflammable air, which explodes by the smallest spark of electricity; but the further communication could only be maintained by a certain number of shocks being the preconcerted signal of each letter, and requires that the man who receives the intelligence should remain constantly in the circuit of the electric fluid. The whole success of the experiment would likewise depend on an apparatus liable to an infinite number of accidents, scarce in the power of human foresight to guard against."—*Essay on the Different Modes of Communication by Signals*, London, 1797, p. 73. We shall meet with other examples further on.

Mataro can as easily speak with Barcelona, as Barcelona with Mataro.

"It appears, however, little short of impossible to erect and maintain so many wires, for, even with the loftiest and most inaccessible supports, boys would manage to injure them; but as it is not necessary to keep them very far apart, they can be rolled together in one strong cable, and placed at a great height.* In the first trials made with a cable of this kind I covered each wire with paper, coated with pitch, or some other idio-electric substance, then, tying them together, I bound the whole with more paper, which effectually prevented any lateral escape of the electricity. In practice the wire cable could be laid in subterranean tubes, which, for greater insulation, should be covered with one or two coats of resin."

In selecting Barcelona and Mataro, distant about thirteen miles, Salvá did not imply that this was the limit at which his telegraph would be practicable; on the contrary, he thought it very probable that the distance at which the electric discharge would be effective was proportional to the number of jars, and, therefore, that with a large battery telegraphic communication may be established between Barcelona and Madrid, and even between places one hundred, or more, leagues apart.

After showing the superiority of an electric telegraph over the optical (semaphore) system then in

* As is done in London at the present day.

use, he lays special stress on the advantages of the former as regards communication between places separated by the sea, and adds :—

"In no place can the electric telegraph [wires] be better deposited. It is not impossible to construct, or protect, the cables with their twenty-two [pairs of] wires, so as to render them impervious to the water. At the bottom of the sea their bed would be ready made for them, and it would be an extraordinary casualty indeed that should disturb them. * * *

"In 1747, Watson, Bevis, and others, in England, showed how the water of the Thames may be made to form part of the circuit of a Leyden jar, and this makes us consider whether it would not suffice for our telegraph to lay a cable of twenty-two wires only across the sea, and to use the water of the latter in place of the twenty-two return wires." *

In the experiments with which Salvá illustrated his paper, he indicated the letters in a way which, by some strange mistake, has always been ascribed to Reusser. The seventeen essential letters of the

* Because Baron Schilling, of St. Petersburg, used a "subaqueous galvanic conducting cord" across the river Neva in 1812, and, in 1837, proposed to unite Cronstadt with the capital by means of a submarine cable, he has been called the Father of submarine telegraphy (Hamel's *Historical Account*, &c., pp. 16 and 67, of W. F. Cooke's reprint). But Salvá was, as we here see, at least seventeen years before him with the suggestion, and to Salvá therefore ought to belong the honour which has hitherto been accorded to the Russian philosopher. As we shall see in a future chapter, this is not the only case in which honours justly due to Salvá are unjustly heaped on another.

alphabet (for he omitted those little used, or whose power could be represented by others) were cut out of parallel strips of tin-foil, pasted on bits of glass, after the fashion of spangled panes, and to the ends of each piece of tin-foil were attached the extremities of the corresponding pair of wires. All the wires were bound up in two cables, which were prepared in the way before described, the out-going wires being collected in one cable, and the return wires in the other.

To indicate a letter, A, for example, it was only necessary to take the ends of the corresponding pair of wires, and connect one end with the outer, and the other with the inner coating of a charged jar. Immediately on thus completing the circuit, the observer, at the other end of the cable, heard the noise of the spark, and saw it illuminate the letter A, in its passage across the breaks in the tin-foil.*

From 1796 to 1799 Salvá resided at Madrid, having been invited by the Academy of Sciences of that capital to engage in some experiments of great public interest. There he had the *entrée* of all the *salons*, and was courted by everybody of consideration—amongst the rest, by the Infante Don Antonio, who appears to have assisted him in perfecting his tele-

* "The late Dr. Balcells, professor in the Industrial School of Barcelona, whose acquaintance I made towards the latter years of his long life, and who, in his turn, had known the celebrated physicist, Salvá, has often assured me that the apparatus just described was tried by its inventor from the Academy of Sciences to the Fort of Atarazanas, across the Ramblas, a distance of about a kilometre."—Saavedra, *Tratado de Telegrafia*, 2nd ed., vol. i. p. 122.

graph. The favourite Godoy, Prince of Peace, was another good friend, to whom Salvá was indebted for an introduction to the King, Charles IV., as we learn from the following paragraph in the *Gaceta de Madrid*, November 29, 1796:*—"The Prince of Peace, who testifies the most laudable zeal for the progress of the sciences, understanding that Dr. Francisco Salvá had read at the Academy of Sciences, at Barcelona, a memoir on the application of electricity to the telegraph, and presented at the same time an electrical telegraph of his own invention, requested to examine the apparatus himself. Satisfied with the exactness and celerity with which communications may be made by its means, he introduced the doctor to the King of Spain. The Prince of Peace afterwards, in the presence of their Majesties and the whole court, made some communications with this telegraph, completely to their satisfaction. The Infante Don Antonio proposes to have one of them of the most complete construction, which shall possess power sufficient to communicate between the greatest distances, by land

* First translated into English in *The Monthly Magazine*, for February 1797, p. 148. Also noticed in Voigt's *Magazin*, for 1798, vol. xi. part iv. p. 61. As a curiosity of bookmaking, we may observe that, in every account of Salvá's telegraph that we have seen, the extracts from the Madrid *Gaceta* and Voigt's *Magazin* are given as if they referred to two entirely different affairs, the latter being usually rendered as follows:—Voigt's *Magazin*, in reference to these experiments, announced two years afterwards that Don Antonio *constructed* a telegraph upon a very grand scale, and to a very great extent. *It also states that the same young Prince was informed at night, by means of this telegraph, of news that highly interested him!* See Highton's *Electric Telegraph: its History and Progress*, London, 1852, p. 43, as a case in point.

or sea. With this view, His Highness has ordered the construction of an electrical machine, the cylinder of which is to be more than forty inches in diameter. He intends, as soon as it is finished, to undertake a series of curious and useful experiments, in conjunction with Dr. Salvá. This is an employment worthy of a great prince. An account of the results will be given to the public in due course."

Notwithstanding this promise, the subject is not again referred to in any succeeding number of the *Gaceta;* but according to Dr. Balcells, the friend of Salvá, a modification of his telegraph which required only one wire was actually constructed in 1798 between Madrid and Aranjuez, a distance of about twenty-six miles. At p. 14 of Gauss and Weber's *Resultate*, &c., for 1837, there is a note of Humboldt's in which he refers to this line, but credits it to Bétancourt, a French engineer. This is clearly a mistake, into which the great traveller might have been led by the probable fact that an engineer of that name was employed to superintend the work—a supposition which is likely enough seeing the greatness of the undertaking.

Dr. Balcells, whose evidence as just quoted should be conclusive on this point, says, further, that the remains of Salvá's telegraph, which, at first, were destined for Don Antonio's museum, were presented, in 1824, to the College of Pharmacy of San Fernando, of which he (Balcells) was then the Adjutant.*

* Saavedra, vol. i. p. 124.

CHAPTER IV.

TELEGRAPHS BASED ON STATIC, OR FRICTIONAL, ELECTRICITY (*continued*).

1802.—*Alexandre's Telegraph.*

TWENTY-FIVE years ago, in the course of a research amongst the imperial archives at Paris, M. Edouard Gerspach, of the French Telegraph Administration, discovered some documents which, in our eyes, are of exceeding value, as establishing for La Belle France the honour of the invention of the first step-by-step, or A.B.C., telegraph. These papers were embodied by M. Gerspach in a memoir for the *Annales Télégraphiques* for March-April, 1859, pp. 188-99, to which we are indebted for much of what follows in this article.

Jean Alexandre was born at Paris, the natural son, it is said, of Jean-Jacques Rousseau. He had the education of a mechanic, some say of a physician, but his actual career was truly a faithful image of the troublous times in which he lived. In 1787 he was at Poitiers, following the trade of gilder, and, as he had a fine voice, he sang in the churches, which added somewhat to his slender emoluments. But soon the revolution came to Poitiers, and swept away the *clientèle* of the poor gilder and carver. He went to Paris, and there maintained himself for a while by singing in the choir of St. Sulpice; but the revolutionary tide followed him, and closed the doors of St. Sulpice, as of all the other churches, leaving Alexandre high and dry again, without the means of subsistence.

Feeling there was nothing else to be done, he now took to politics, and, after the manner of the times, soon found himself president of a section of the Luxembourg (club), and, later on, a deputy of the Convention. This latter honour, however, his simple manners made him decline. But greater still were yet in store, and, as he was preparing to return to his workshop at Poitiers, the Government sent him thither, but with the exalted rank of Commissary-General of War. Later on, he was promoted to be chief of the military division of Lyons, where he had to organise an army of 80,000 men. With the title of Chief Agent of the Army of the West, he next went to Angers, where, from the forty-two departments that were under his orders, he had to raise another army of 200,000 men. With all this greatness, he still was not happy; he yearned for a quiet life—a feeling which seems to have grown daily stronger with him, until, at last becoming irresistible, he quitted honours and politics, and returned to his home at Poitiers, as poor as he had left it—a fact, by the way, which speaks volumes for the integrity of his character.

Here we find him, in 1802, producing his *télégraphe intime*, or secret telegraph. He wrote to Chaptal, Minister of the Interior, acquainting him briefly with the discovery, and asking assistance to enable him to go to Paris, and exhibit his machine to the Government. The Minister asked (and naturally), in the first place, for full particulars and plans of the apparatus, but Alexandre declined to divulge his secret, and addressed himself next to Cochon, Prefect of Vienne, offering to make an experiment before him. The Prefect, agreeably impressed with the conversation of the inventor, whose quick and vigorous imagination he found to contrast singularly with the simplicity of his demeanour, granted his request, and accordingly, on the 13th Brumaire, year X (early in 1802), he went, accompanied by the chief engineer of the department, to Alexandre's house. The experiments were crowned with unhoped-for success, and the Prefect drew up a report for the minister, Chaptal, of which the following is the substance :—

"We were conducted into a room on the ground floor, in the centre of which we found a box nearly 1·5 metre high, and about 30 centimetres broad and deep. This box was surmounted by a dial, around which were traced all the letters of the alphabet. A well-poised needle, or pointer, travelled round the circle at the will of a distant and invisible agent, and stopped over such letters as composed the words that he wished to communicate. The completion of each word and phrase was indicated by an entire revolution of the pointer, which, in its normal state of rest, always occupied a certain determined position [corresponding, no doubt, to our zero].

"A correspondence was established between the [distant] agent and ourselves, and the success was all that we could desire. The dial repeated exactly all the phrases that we had dictated, and the [distant] agent added some from himself, which we had no difficulty in understanding. On asking why the second box was situated in an upper story, about 15 metres distant, instead of being placed on the same level as the first, the inventor replied that it was to show that difference of level had no effect on its action, and that the conductors could in every case go up and down, and adapt themselves to the inequalities of the ground.

"We understood, without, however, his distinctly saying so, that the author derives his power (*usage*) from some fluid, either electric or magnetic. He told us that, in the course of experiment, he had met with a strange matter, or power (of which, until then, he had been ignorant) which, he was almost tempted to believe, is generally diffused, and forms, in some sort, the soul of the universe; that he had discovered the means of utilising the effects of this power, so as to make them conduce to the success of his machine;

and that he was certain of being able to propagate them with the celerity of light, and to any distance that may be required."

In concluding this report on the invention, which the Prefect characterised as a work of genius, he urged that Alexandre should be called to Paris, at the expense of the State, in order that he may repeat his experiments under the eyes of the Government. The minister, Chaptal, did not, however, regard the discovery at all so favourably, evidently imagining it to be a telegraph of the Chappe, or semaphore, kind, and wrote to the inventor's agent, declining to have anything to do with him. Such a rebuff would have acted as a *quietus* to ordinary people; but inventors are proverbially a tenacious race. Alexandre was an inventor, and, firm in his convictions, he quitted Poitiers, and, in hopes of better fortune, betook himself to Tours.

There, at his invitation, General Pommereul, Prefect of the Department of the Indre and Loire, and the mayor and officers of the city of Tours, assembled at his house to assist at a public trial of the apparatus. As before, one of the machines was on the ground floor, and the other on the first story, separated from the lower room by an antechamber and a small court. The Prefect dictated the phrase, "Genius knows no limits," which was transmitted to the distant end, and thence returned with all the success imaginable. The next phrase, "There are no longer miracles," was repeated with the same result, and many others followed, in which all the words were reproduced by the machines, letter for letter, with the greatest exactness.*

All these experiments, conclusive as they were, had, nevertheless, little effect in advancing Alexandre's interests; they drew on him the commendations of the multitude, made his name known, but contributed nothing towards the attainment of his end, which was Paris, and the patronage of the First Consul, to whom only would he confide his secret. Having no money for the further prosecution of his plans, he now entered into partnership with a M. Beauvais, who was to supply all sums necessary, and to receive in return a quarter of the profits of the enterprise, Alexandre keeping to himself the secret of his invention until he had netted 60,000 francs by its exploitation, after which it was to become joint property. No sooner were these terms concluded than Beauvais, provided with the official accounts of the experiments at Poitiers and Tours, addressed himself to Napoleon, and solicited the honour of a trial in his apartments, and in his presence alone. Napoleon, perhaps smelling gunpowder, declined the meeting, but referred the papers to Delambre, the illustrious academician and

* The *English Chronicle* newspaper of June 19–22, 1802, has a short account of these experiments, concluding as follows:—"The art or mechanism by which this is effected is unknown, but the inventor says that he can extend it to the distance of four or five leagues, even though a river should be interposed." There is a copy, probably unique, in Mr. Latimer Clark's library.

astronomer, who, some weeks later, returned a report, of which the following is a free translation :—

"*Report of Citizen Delambre on the Secret Telegraph of Citizen Alexandre, submitted to the First Consul by Citizen Beauvais.*

"Paris, 10 Fructidor, an X.

"The papers which the First Consul has caused me to examine do not contain sufficient details to enable me to form an opinion, nor, after the two interviews that I have had with Citizen Beauvais, am I able to do more than offer the merest conjectures on the advantages and disadvantages of the Secret Telegraph.

"Citizen Beauvais knows the secret of Citizen Alexandre, but he has promised to impart it to no one but the First Consul himself. This circumstance must make any report from me valueless, for how can one judge of a machine which one has neither seen nor understands?

"All that we know is that this telegraph is composed of two similar boxes, each having a dial, round whose face are marked all the letters of the alphabet. By means of a winch, or handle, the pointer of one dial is moved to any desired letter or letters, and, at the same instant, the pointer of the other dial repeats the same movements, and in exactly the same order. When these two boxes are placed in two separate apartments, two persons can write and reply without seeing each other, and without being seen, and in such a way that no one can doubt the correspondence,

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which, moreover, can be carried on at any time, as neither night nor fogs can intercept the transmission.

"By means of this telegraph the governor of a besieged place could carry on a secret and continuous correspondence with a person four or five leagues distant, or even at any distance, and communication can be established between the two boxes as readily as one can hang a bell (*qu'on poserait un mouvement de sonnette*).

"The inventor carried out two experiments with his machines at Poitiers and at Tours, in presence of the prefects and mayors of the respective places, and the official reports of these functionaries attest that the results were completely successful. Now, the inventor and his associate ask, either that the First Consul will be pleased to permit of one of the boxes being placed in his apartment, and the other in that of the Consul Cambacérès, so as to give to their experiments all the *éclat* and authenticity possible; or, that he will accord an audience of ten minutes to Citizen Beauvais, who will then communicate to him the secret (of the telegraph), which is so simple that the bare description will be equivalent to a practical demonstration. They add that the idea is so natural as to leave little room to fear that it will ever occur to any *savant* [*sic*]. It is said, however, that Citizen Montgolfier divined it, after some hours' reflection, on a description of the apparatus which was given to him.

"After this statement, which is the substance of -versations with Citizen Beauvais, a very few remarks must suffice. If, as one would be inclined to believe from the comparison with bell-hanging, the means employed comprised wheels, levers, and such like,* the invention would not be very surprising, and one could easily imagine the practical difficulties that would be encountered as soon as it was attempted to employ it over distances of several leagues.

"If, on the contrary, as the official report from Poitiers seems to show, the means of communication is a fluid (*i. e.*, a natural force), the inventor deserves much more credit for having discovered how to utilise it so as to produce, at any distance, effects so regular and so unfailing. But then, one may demand, what guarantee have we for these effects? Neither the experiments at Poitiers, nor those at Tours, in which the distance was only a few metres, supply it. No more would the proposed experiment between the chambers of the First and Second Consuls. So long as the motive power remains a secret, one can never vouch for more than what one sees, and it will be entirely wrong to conclude, from the success of an experiment on a small scale, that like results will be obtained over more considerable distances. If the effect is only attainable at a distance of some few metres, the machine ought to be sent to the scientific toy shops.

"If Citizen Beauvais, who offers to defray the expenses of an experiment, had proposed to carry it out in presence of commissioners appointed for the

* Forming, in fact, a kind of mechanical telegraph like the railway semaphores of to-day.

purpose, there could be no objection to granting his request; for, although an experiment on a small scale would not be very conclusive, still it would enable us to see what might be hoped from a trial of a grander and more expensive kind. But Citizen Beauvais, without expressly declining a commission, desires, in the first place, to secure the testimony and approbation of the First Consul. It only remains, then, for the First Consul to say whether, in view of the little chance of success attaching to an invention so little proved, and announced as so marvellous, he will spare a few moments for the examination of a discovery of an artist, who is described as one as full of genius as he is devoid of scientific learning and of fortune.

"He makes a secret of his discovery, and I ought to judge it with severity, and according to the laws of probability; but the limits of the probable are not those of the possible, and Citizen Alexandre must be sure of his facts, since he offers to expose all to the First Consul. It, therefore, only remains for me to hope that the First Consul will grant him an audience, and that, in the sequel, he will have reason to welcome the inventor, and recompense worthily the author.

"DELAMBRE."

With this most interesting document ends the story of the *Secret Telegraph*. In 1806 Alexandre was at Bordeaux, taking out a patent for a machine for filtering the water of the Garonne for supplying the city; but, although the authorities seem to have afforded him every facility towards the accomplishment of his scheme, it was never carried out, through want of money. We next hear of him in 1831, when he submitted to the King, Louis Philippe, a project for steering balloons. He died soon after at Angoulême, leaving a widow, who died in 1833, at Poitiers, in extreme want.

Such is the sad story, as told by M. Gerspach, of one who must be regarded as a veritable pioneer in electric telegraphy; for, although Alexandre chose to surround his invention with an air of mystery, and preserved only too faithfully the secret of its action, we believe that he had, in effect, constructed a telegraph of the A, B, C, sort, with static electricity as his motive power.

Some writers, however, regard his apparatus, like that of Comus, as only another instance of the sympathetic needle telegraph, and seek to explain its action somewhat after the manner figured and described by Guyot.* But there seems to us to be two very good reasons against this theory: first, the impossibility of carrying out any such deception in the apartments of the two consuls; and second, the character of

* *Nouvelles Récréations Physiques et Mathématiques*, Paris, 1769, vol. i. p. 134. M. Aug. Guerout is the latest exponent of this theory. See *La Lumière Électrique*, for March 3, 1883.

Napoleon, who, as all the world knows, was not a man to be trifled with.

The suspicion of Delambre, that it partook of the nature of a mechanical telegraph, we consider equally disproved by the words of the *procès-verbal* from Poitiers. "He told us that, in the course of experiment, he had met with a strange matter, or power (of which, until then, he had been ignorant), which, he was almost tempted to believe, is generally diffused, and forms in some sort the soul of the universe; that he had discovered the means of utilising the effects of this power, so as to make them conduce to the success of his machine; and that he was certain of being able to propagate them with the celerity of light, and to any distance that may be required." Surely a mechanician would not speak thus of a combination of ropes, wheels, and pulleys. Although, once upon a time, Archimedes glorified the power of the lever, when he said that by its means he could move the world, no Archimedes of our day would be so extravagant as to call the same power, mighty as it is, the soul of the universe.

On the other hand, the language just quoted would apply very well to electricity. Thales called it a spirit, Otto Guericke thought it controlled the revolution of the moon round the earth, and Stephen Gray that of the planets round the sun; Franklin showed its identity with lightning; John Wesley regarded it as an universal healer; and Galvani had just con-founded it with life. Well, then, might Alexandre be excused for calling it the soul of the universe.

Again, let us recollect that while he was still a young man the invention of the Chappe semaphore, and its wonderful performances, were the theme of daily conversation; and that rival plans were being frequently started—some, semaphores more or less like Chappe's, and for night as well as day service; some, based on the properties of acoustics, as those of Gauthey and Count Rumford; and some again, as we have seen in these pages, on those of electricity.* What more natural, then, than that Alexandre, a clever mechanician, and a man of a quick and vigorous imagination, should invent an electric telegraph.

Now, let us regard the apparatus as described by M. Cochon, in connection with the half admission that electricity was its basis, and that it was operated by a winch, or handle, as mentioned by Delambre. Do not this handle, the box, the dial on the top, and the conductor recall the telegraph of Lomond, which was the wonder of Paris in 1787, and which has been already described in these pages. The dial of Alexandre, it is true, is an immense improvement on the

* We may here refer to a remark of Amyot's, for which we have not been able to find room before, to the effect that, somewhere about 1798, Henry Monton Berton, the distinguished French composer, conceived the idea of an electric telegraph (*Note historique sur le Télégraphe Électrique*, in the *Comptes Rendus*, for July 9, 1838). This *note* is reprinted *in extenso* in Julia de Fontenelle's *Manuel de l'Électricité*, but in neither case are any details given.

pith-ball indicator of Lomond, but that (the dial), too, had its prototype in the synchronous clockwork dial with which Chappe essayed an electric telegraph in 1790, and which, no doubt, was equally well known as the machine of Lomond. Indeed, the inference to us seems irresistible, that Alexandre took Lomond's and Chappe's contrivances as his basis, and built upon them his own improvements.

The only point that remains for consideration is, how did the working (? revolving) of the handle actuate the pointers? The explanation to our mind is not far to seek. Given an electrical machine inside the box, and a train of wheels behind the dial, and in gear with the pointer, and it would be easy for a clever mechanician to make the repulsion of a sort of pith-ball electrometer (acting also as a pawl) against a discharging surface, and its subsequent collapse, give motion of a step-by-step character to the wheels, and, through them, to the pointer. The prime conductors of both machines would, under our supposition, be connected by a wire (probably concealed from view), and thus the movements of one pointer would be synchronous with those of the other.

Some writers, as Cézanne * and Berio,† think it likely that Alexandre used the electricity of the pile, then newly discovered by Volta; but the use of a handle is as fatal to such an assumption, as it is favourable to that of an electrical machine being the *primum mobile.*

* *Le Cable Transatlantique*, Paris, 1867, p. 32.

† *Ephemerides of the Lecture Society*, Genoa, 1872, p. 645.

1806–14.—*Ralph Wedgwood's Telegraph.*

The next proposal of a telegraph based, presumably, on static, or frictional, electricity, is due to a member of the Wedgwood family. Ralph Wedgwood was born in 1766, and was brought up by his father at Etruria, where he received much valuable aid in chemistry, &c., from his distinguished relative Josiah. He afterwards carried on business, as a potter, under the style of "Wedgwood and Co.," at the Hill Works, Burslem; but was ruined through losses during the war. After a short and unsatisfactory partnership with Messrs. Tomlinson and Co., of Ferrybridge, Yorkshire, he removed to Bransford, near Worcester, where he issued prospectuses for teaching chemistry at schools. Thence, in 1803, he moved to London, travelling in a carriage of his own constructing, which he described as "a long coach to get out behind, and on grass-hopper springs, now used by all the mails."

He appears to have early shown a genius for inventing, and while yet at Bransford had perfected many useful contrivances—amongst them, a "Pen-napolygraph," for writing with a number of pens attached to one handle; and a "Pocket-secretary," since better known as the "Manifold-writer." On coming to London he found that the first-mentioned apparatus had already been invented by another person, but the second, proving to be new, he patented as "an apparatus for producing duplicates of writing."

In 1806, he established himself at Charing Cross, and soon after turned his attention to the construction of an electric telegraph, the first suggestions of which he seems to have obtained from his father.* In 1814, having perfected his plans, he submitted them to Lord Castlereagh, at the Admiralty; and after a proper interval his son, Ralph, waited on his lordship to learn his views with regard to the new invention. He was dismissed with the assurance that "the war being at an end, and money scarce, the old system [of shutter-semaphores] was sufficient for the country."

These chilling words appear to have been stereotyped, ready for use, for, as we shall see in the course of our history, they were the identical missiles with which a wearied and, perhaps, worried bureaucracy repulsed other telegraph inventors, as Sharpe and Ronalds, Porter and Alexander,† and goodness knows how many others besides. They certainly were the death of Wedgwood's telegraph, for he dropped it in disgust, leaving on record only a few words as to its uses and advantages—precisely such as we find them to-day. These show such an appreciation of the value of the electric telegraph, that we feel certain his

* According to Llewellynn Jewitt, whose *Life of Josiah Wedgwood*, &c., London, 1865, we follow in this volume; see chap. ix. pp. 178–81. See also Jewitt's *Ceramic Art in Great Britain*, London, 1878, vol. i. pp. 489–92.

† The writer of the article "Fifty Years' Progress" in *The Times*, January 5, 1875, says that Alexander could not hear the word "telegraph" without a shudder!

own invention was of no mean order, and we must ever regret, therefore, that he has left us nothing as to its construction or mode of action. His remarks are contained in a pamphlet,* dated May 29, 1815; and as they are all that we have on our subject we shall quote them entire:—

"A modification of the stylographic principle proposed for the adoption of Parliament, in lieu of telegraphs, *viz.*:—

"The Fulguri-Polygraph, which admits of writing in several distant places at one and the same time, and by the agency of two persons only.

"This invention is founded on the capacity of electricity to produce motion in the act of acquiring an equilibrium; which motion, by the aid of machinery, is made to distribute matter at the extremities of any given course. And the matter so distributed being variously modified in correspondence with the letters of the alphabet, and communicable in rapid succession at the will of the operator, it is obvious that writing at immense distances hereby becomes practicable; and, further, as lines of communication can be multiplied from any given point, and those lines affected by one

* Entitled *An Address to the Public on the Advantages of a proposed introduction of the Stylographic Principle of writing into general use; and also of an improved species of Telegraphy, calculated for the use of the Public, as well as for the Government.* It will be found at the end of his *Book of Remembrance*, which was published in London, 1814. Wedgwood was an exceedingly reticent man, and, it is feared, carried with him to the grave other scientific secrets, as well as that of the telegraph. He died at Chelsea in 1837.

and the same application of the electric matter, it is evident from hence also that fac-similes of a despatch, written, as for instance, in London, may, with facility, be written also in Plymouth, Dover, Hull, Leith, Liverpool, and Bristol, or any other place, by the same person, and by one and the same act. Whilst this invention proposes to remove the usual imperfections and impediments of telegraphs, it gives the rapidity of lightning to correspondence *when* and *wherever* we wish, and renders *null* the *principal disadvantages* of *distance* to *correspondents*.

"Independent of the advantages which this invention offers to Government, it is also susceptible of much utility to the public at large, inasmuch as the offices which might be constructed for the purposes of this invention might be let to individuals by the hour, for private uses, by which means the machinery might be at all times fully occupied; and the private uses which could thus be made of this invention might be applied towards refunding the expenses of the institution and also for increasing the revenue. Innumerable are the instances wherein such an invention may be beneficially applied in this country, more especially at a time when her distinguished situation in the political, commercial, and moral world, has made her the central point of nations and the great bond of their union. To the seat of her Government, therefore, it must be highly desirable to effect the *most speedy and certain communication* from every quarter of the world, whilst it would at any moment there concentrate instantaneous intelligence of the situation of each and every principal part of the nation, as well as of each and every branch of its various departments."

In communicating the above extract to *The Commercial Magazine*, for December 1846 (pp. 257-60), Mr. W. R. Wedgwood thus urges the claims of his father to a share in the discovery of the electric telegraph:—"It may be asked, why did not Mr. Ralph Wedgwood carry his invention into practical application? The answer is very obvious. Railways were not then in existence, and the connecting medium required an uninterrupted course such as railways alone afford. Such an invention also required the assistance either of Government or a powerful company, the scheme being too gigantic for an individual to work. The inventor, then, it will be perceived, did all that was possible to bring the discovery into practical use; for, in the first instance, he offered it to the Government, who refused it; and, as it was for the benefit of the nation, he then made public his scheme of an electric telegraph in the manner quoted from his pamphlet."

1816.—*Ronalds' Telegraph.*

This ingenious contrivance belongs to the synchronous class of telegraphs, of which we have already seen two other examples, *viz.*, those of Chappe, 1790, and Alexandre, 1802. It is, in fact, only the realisation of Chappe's idea. Sir Francis (then Mr.) Ronalds took up the subject of telegraphy in 1816, and pursued it very ardently for some years, until, like Wedgwood, disgusted with the apathetic conduct of the Government, he dropped the matter, and, more in sorrow than in anger, took leave of a science which, as he says, was up to that time a favourite source of amusement. Fortunately for the science, he returned to his old love in later years, and, dying August 8, 1873, left us a grand legacy in the Ronalds' Library.*

In 1823, he published a thin octavo volume, entitled *Descriptions of an Electrical Telegraph, and of some other Electrical Apparatus;* and, in 1871, the original work having become very scarce, he issued a reprint of the part relating to his telegraph. From this, in accordance with our rule of consulting, when possible, original sources, we extract the following account.

* A magnificent collection of books on electricity, magnetism, and their applications. The catalogue compiled by Sir Francis is a monument of the concentrated and well-directed labour of its indefatigable author. It has been ably edited by Mr. A. J. Frost, and published at an almost nominal price by the Society of Telegraph-Engineers and Electricians. No student of electricity should be without it. A short, alas! too short, biography of Sir Francis by the editor is prefixed to the catalogue, to which we refer our readers for much interesting information. We would here correct an error—the only one, we believe—into which the biographer has fallen. On p. xv. he says, "Wheatstone, then a boy of about 15, was present at many of the principal experiments at Hammersmith." Wheatstone was born at Gloucester in 1802, where he lived until the year 1823, when he came up to London, and opened business as a maker of musical instruments. It seems impossible to us that a poor lad of 14, as Wheatstone was in 1816, could have been present at Ronalds' experiments, even supposing that he was not then living far away in Gloucester.

The drawings with which our subject is illustrated have been reduced on stone from the original copper-plates which were engraved from Ronalds' own sketches, in 1823.

Ronalds begins by saying :—"Some German and American savans first projected galvanic, or voltaic, telegraphs, by the decomposition of water, &c. But the other [or static] form of the fluid appeared to me to afford the most accurate and practicable means of conveying intelligence ; and, in the summer of 1816, I *amused* myself by wasting, I fear, a great deal of time, and no small expenditure, in trying to prove, by experiments on a much more extensive scale than had hitherto been adopted, the validity of a project of this kind."

These experiments were carried out on a lawn, or grass-plot, adjoining his residence at Hammersmith, and as, of course, it was impossible to lay out in a straight line a great length of wire in such a situation, he had recourse to the following expedient: Two strong frames of wood (see Frontispiece) were erected at a distance of twenty yards from each other, and to each were fixed nineteen horizontal bars. To each of the latter, and at a few inches apart, were attached thirty-seven hooks, from which depended as many silken loops. Through these loops was passed a small iron wire, which, going from one frame to the other, and making its inflections at the points of support, formed one continuous length of more than eight miles.

When a Canton's pith-ball electrometer was connected with each extremity of this wire, and it was charged by a Leyden jar, the balls of both electrometers appeared to diverge at exactly the same moment; and when the wire was discharged, by being touched with the hand, they both collapsed as suddenly and, as it appeared, as simultaneously. When any person took a shock through the whole length of wire, and the shock was compelled to pass also through two insulated inflammable air pistols, one connected with each end of the wire, the shock and explosion seemed to occur at the same instant.

When the spark was passed through two gas pistols, and any one closed his eyes, it was impossible for him to distinguish more than one explosion, although both pistols were, of course, fired. Sometimes one, and sometimes both pistols were feebly charged with gas, but nobody, whose back was turned, could tell from the report, except by mere chance, whether one or both were exploded.

Thus, then, three of the senses, *viz.*, sight, feeling, and hearing, seemed to receive absolute conviction of the instantaneous transmission of electric signs through the pistols, the eight miles of wire, and the body of the experimenter (pp. 4 and 5).

Accepting these experiments as conclusive of the practicability of an electric telegraph, Ronalds next sought out the best means of establishing a communication between any two distant points; and, after trying a number of ways, at last adopted the following,

Fig. 2.

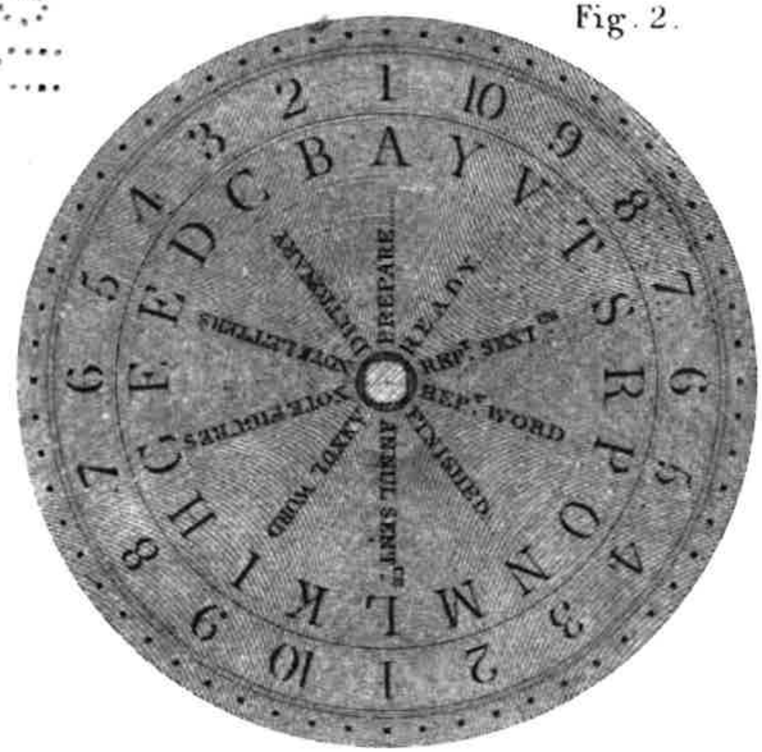


Fig. 3.

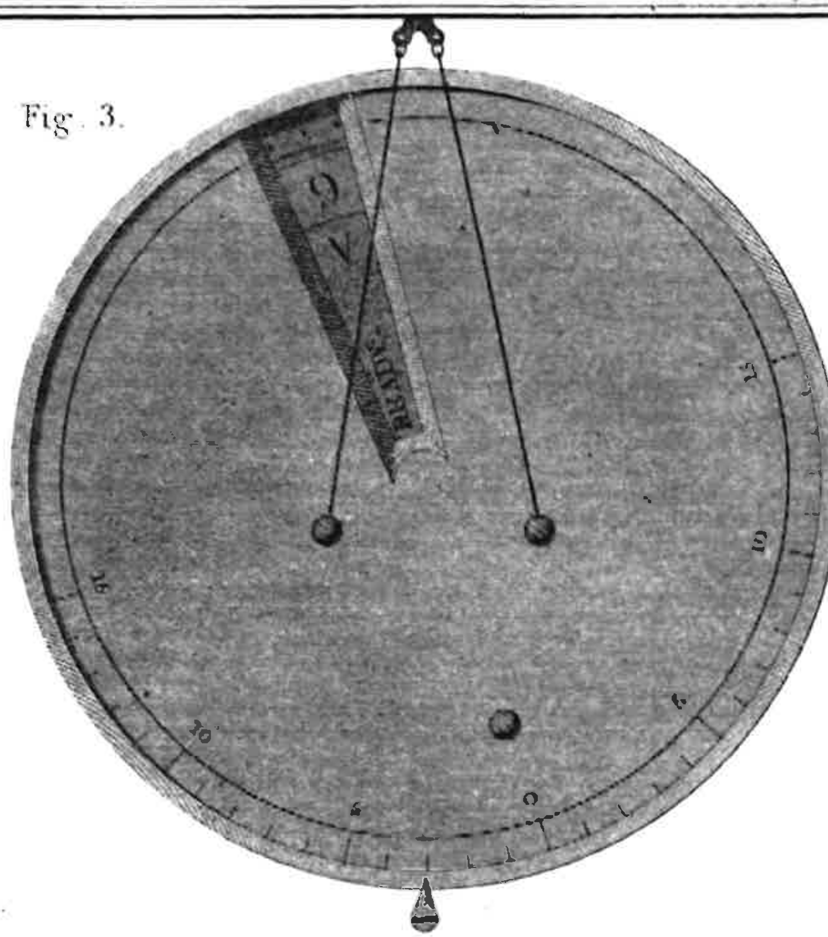


Fig 1.

Sprague & Co. Photo-litho. London

E. & F. N. Spon; London & New York.

as being the most convenient. A trench was dug in the garden, 525 feet in length, and 4 feet deep. In it was laid a trough of wood, two inches square, and well lined, inside and out, with pitch. In the trough thick glass tubes were placed, through which, finally, the wire (of brass and copper) was drawn. The trough was then covered with strips of wood, previously smeared with hot pitch, and, after painting with the same material the joints so as to make assurance doubly sure, the trench was filled in with earth. Plate I., Fig. 1, represents a section of this trough, tube, and wire. It will be seen that the different lengths of tube A, B, C, did not touch, but that at each joint, or rather interval, other short tubes, or ferrules, D, E, were placed, of just sufficient diameter to admit the ends of the long ones, together with a little *soft* wax. This arrangement effectually excluded any moisture, and yet left the parts free to expand and contract with variations of temperature.

The apparatus for indicating the signals, and its *modus operandi*, are thus described :—A light, circular brass plate, Fig. 2, divided into twenty equal parts, was fixed upon the seconds' arbor of a clock which beat dead seconds. Each division bore a figure, a letter, and a preparatory sign. The figures were divided into two series, from 1 to 10, and the letters were arranged alphabetically, leaving out J, Q, U, W, X, and Z, as of little use. In front of this disc was fixed another brass plate, Fig. 3, capable of being occasionally revolved by the hand round its centre. This plate had an aperture of such dimensions, that, whilst the first disc, Fig. 2, was carried round by the motion of the clock, only one set of letter, figure, and preparatory sign could be seen, as shown in the figure. In front of this pair of plates was suspended a Canton's pith-ball electrometer, B, Fig. 3, from an insulated wire, C, which communicated with a cylindrical machine, D, of only six inches diameter, on one side; and with the line wire, E, insulated and buried in the way just described, on the other.

Another electrical machine and clock, furnished with an electrometer and plates, being connected to the other end of the line in precisely the same way, it is easy to see, that when the wire was charged by the machine at *either* end, the balls of the electrometers at *both* ends diverged; and that when the wire was discharged at either station, both pairs of balls collapsed at the same time. Whenever, therefore, the wire was discharged at the moment that a given letter, figure, and sign of one clock appeared in view through the aperture, the same letter, figure, and sign appeared also in view at the other clock; and thus, by discharging the line at one end, and by noting down whatever appeared in view at every collapse of the pith-balls at the other, any required words could be spelt. By the use of a telegraphic dictionary, the construction of which is explained at pages 8 and 9 of Ronalds' little treatise, words, and even whole sentences, could be intimated by only three discharges, which could be effected, in the shortest time in nine seconds, and in the longest in ninety seconds, making a mean of fifty-four seconds.

Whenever it was necessary to distinguish the preparatory signs from the figures and letters, a higher charge than usual was given to the wire, which made the pith-balls diverge more widely; and it was always understood that the first sign, *viz.*, *prepare*, was intended when that word, the letter A, and the figure 1, were in view at the sender's clock. Should, therefore, the receiver's clock not exhibit the same sign, in consequence of its having gained, or lost, some seconds, he noted the difference, and turned his outer plate, Fig. 3, through as many spaces, either to the right or left, as the occasion required, the sender all the while repeating the signal, *prepare*. As soon as the receiver had adjusted his apparatus, he intimated the fact by discharging the wire at the moment when the word *ready* appeared through the opening. In order to indicate when letters were meant, when plain figures, and when code figures referring to words and sentences in the dictionary, suitable preparatory signs were made beforehand, as *note letters*, *note figures*, and *dictionary*. Other preparatory signs of frequent use were marked on the dials, and were designated in the same manner whenever required.

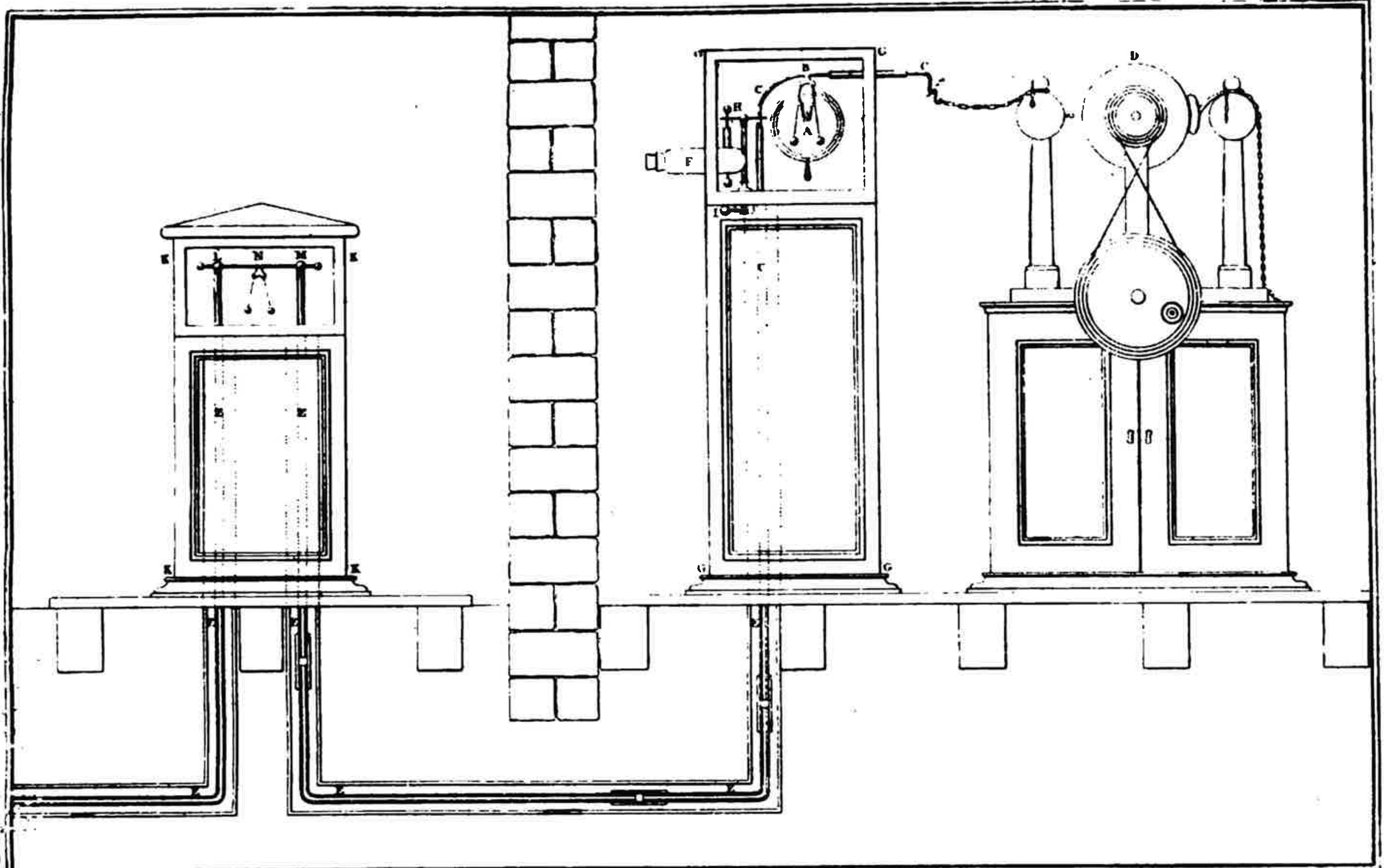
The gas pistol F, Plate II., which passed through the side of the clock-case G, was furnished with a kind of discharging-rod, H, by means of which a spark might pass through and explode it when the sender made the sign *prepare*. This obviated the necessity of constant watching on the part of the attendant, which was found so irksome in the semaphores of those days. By a slight turn of the handle, I, the attendant could break the connection between the line wire and the pistol, and so put his apparatus into a condition to "receive."

Midway between the ends of the wire was placed the contrivance, K, K, by which its continuity could be broken at pleasure, for the purpose of ascertaining (in case any accident had happened to injure the insulation of the buried wire) which half had sustained the injury, or if both had. It is seen that the two sides of the wire were led up into the case, and terminated in two clasps, L, and M, which were connected by the metal cross piece, N, carrying a pair of pith-balls. By detaching this wire from the clasp L, whilst it still remained in contact with M, or *vice versâ*, it could at once be seen which half of the wire did not allow the balls to diverge, and, consequently, which half was damaged, or if both were.

One of the stations of this miniature telegraph was in a room over a stable, and the other in a tool-house at the end of the garden, the connecting wire being laid under the gravel walk.*

* After a lapse of nearly fifty years, a portion of this line was dug up, in 1862, in the way described in Frost's *Biography*, p. xviii. Some years later the specimen came into the possession of Mr. Latimer Clark, by whom it, as well as the original dial apparatus, was exhibited at

Plate II, to face p. 134



Sprague & Co Photo-litho London

E. & F. N. Spon, London & New York.

Having made a large number of experiments with this line, and having thoroughly proved the practicability of his invention, Ronalds decided upon bringing it to the notice of the Government. This he did on the 11th of July, 1816, in a letter addressed to Lord Melville, the First Lord of the Admiralty, as follows:—

"Upper Mall, Hammersmith,
July 11, 1816.

"Mr. Ronalds presents his respectful compliments to Lord Melville, and takes the liberty of soliciting his lordship's attention to a mode of conveying telegraphic intelligence with great rapidity, accuracy, and certainty, in all states of the atmosphere, either at night or in the day, and at small expense, which has occurred to him whilst pursuing some electrical experiments. Having been at some pains to ascertain the *practicability* of the scheme, it appears to Mr. Ronalds, and to a few gentlemen by whom it has been examined, to possess several important advantages over any species of telegraph hitherto invented, and he would be much gratified by an opportunity of demonstrating those advantages to Lord Melville by an experiment which he has no doubt would be deemed decisive, if it should be perfectly agreeable and consistent with his lordship's engagements to

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the Special Loan Collection of Scientific Apparatus, South Kensington Museum, 1876, and at the late Electrical Exhibitions in Paris and London (Crystal Palace). They were also shown in the British Section of the Vienna Exhibition, last year.

honour Mr. Ronalds with a call; or he would be very happy to explain more particularly the nature of the contrivance if Lord Melville could conveniently oblige him by appointing an interview."

Lord Melville was obliging enough, in reply to this communication, to request his private secretary, Mr. Hay, to see Ronalds on the subject, but before an interview could be arranged, and while the nature of the invention was yet a secret, except to Lord Henniker, Dr. Rees, Mr. Brand, and a few particular friends, an intimation was received from Mr. Barrow, the Secretary of the Admiralty, to the effect that telegraphs of any kind were then wholly unnecessary, and that no other than the one then in use (the old Semaphores of Murray and Popham) would be adopted. This much-quoted and now historic *communique* ran as follows: *—

"Admiralty Office, 5th August.

"Mr. Barrow presents his compliments to Mr. Ronalds, and acquaints him, with reference to his note of the 3rd inst., that telegraphs of any kind are now wholly unnecessary, and that no other than the one now in use will be adopted."

In reference to this correspondence Ronalds says:— "I felt very little disappointment, and not a shadow

* The originals of these important documents, with many other valuable papers, relating to this subject, may be consulted in the Ronalds' Library. See p. 439 of the Catalogue.

of resentment, on the occasion, because every one knows that telegraphs have long been great bores at the Admiralty. Should they *again* become *necessary*, however, perhaps electricity and electricians may be indulged by his lordship and Mr. Barrow with an opportunity of *proving* what they are capable of in this way. I claim no indulgence for mere chimeras and chimera framers, and I hope to escape the fate of being ranked in that unenviable class" (p. 24).

Ronalds will always occupy a high position in the history of the telegraph, not only on account of the excellence and completeness of his invention, but also for the ardour with which he pursued his experiments, and endeavoured to bring them to the notice of his countrymen.* Had he worked in the days of railways and joint-stock enterprise, there can be no doubt that his energy and skill would have triumphed over every obstacle, and he would have stood forth as the practical introducer of electric telegraphs; but he was a generation too soon, the world was not yet ready for him.

His little *brochure* of 1823 is the first work ever published on the subject of electric telegraphy, and is

* It might have been with a knowledge of Ronalds' telegraphic experiments that Andrew Crosse, in 1816, uttered the prophecy, with which his biographer opens the story of his life:—"I prophesy that by means of the electric agency we shall be enabled to communicate our thoughts instantaneously with the uttermost ends of the earth."—*Memorials Scientific and Literary of Andrew Crosse, the Electrician*, London, 1857.

so marvellously complete, that it might almost serve as a text-book for students at the present day. In it he proposes the establishment of telegraph offices throughout the kingdom, and points out some of the benefits which the Government and public would derive from their existence. "Why," he asks, "has no ***serious trial*** yet been made of the qualifications of so ***diligent*** a courier? And if he should ***be*** proved competent to the task, why should not our kings hold councils at Brighton with their ministers in London? Why should not our Government govern at Portsmouth almost as promptly as in Downing-street? Why should our defaulters escape by default of our foggy climate? and, since our piteous inamorati are not all Alphei, why should they add to the torments of absence those dilatory tormentors, pens, ink, paper, and posts? Let us have ***electrical conversazione offices***, communicating with each other all over the kingdom *if we can.*" *

It would hardly be possible at the present day to describe more accurately the progress of electric telegraphy than in these characteristic sentences. We have "electrical conversazione offices" all over the kingdom. The wires which connect Balmoral, Windsor,

* Pp. 2, 3. It is curious to note the similarity of ideas on this subject that occurs in the extract, which we have given, on page 126, from Ralph Wedgwood's pamphlet of 1815. The two trains of thought are perfectly independent, for we believe that Ronalds knew nothing of Wedgwood's invention—a conclusion to which we are led by the absence of the latter's name from the Ronalds' catalogue.

and Osborne with Downing-street, enable Her Majesty to "hold councils with her Ministers in London" at any moment; while the extensive system of Admiralty and War Office telegraphs enables the Government to "govern at Portsmouth [and many places besides] as promptly as in Downing-street." One of the very first results of the earliest telegraph was the capture of Tawell, the Quaker murderer; and the curious ramification of police telegraphy in London, if not an absolute protection against our "foggy climate," is, at least, a terror to those who might otherwise elude the grasp of the law. As for our "piteous inamorati," it is perfectly well known that they use the wires as freely as most people, and that "love telegrams" are gradually taking the place of "love letters."

His underground wire was a fair specimen of what exists at the present day. We use iron, or earthenware, pipes in lieu of his wooden trough; but we are not very far in advance here, for he points out by way of anticipating possible objections to his plan, that "cast-iron troughs might be rendered as tight as gas-pipes," should it be deemed desirable to employ them. He did not recommend his glass-tube insulators to the exclusion of all other methods, as, for example, that of Cavallo, by means of pitch and cloth. "No person," says he, "of competent experience in these matters will doubt, that either of them, or several other plans that might be chosen, would be efficient. But since accident and decay compose the lot of all inanimate as well as animated nature, let two or more sets of troughs, tubes, and wires be laid down; so that, whilst one may be undergoing repair, the others may be ready for use" (p. 16).

On the general question of conservancy he says:— "To protect the wire from mischievously disposed persons, let the tubes be buried six feet below the surface of the middle of high roads, and let each tube take a different route to arrive at the same place. Could any number of rogues, then, open trenches six feet deep, in two or more public high roads or streets, and get through two or more strong cast-iron troughs, in a less space of time than forty minutes? for we shall presently see that they would be detected before the expiration of that time. *If they could*, render their difficulties greater by cutting the trench deeper: and should they still succeed in breaking the communication by these means, hang them if you can catch them, damn them if you cannot, and mend it immediately in both cases. Should mischievous devils from the subterranean regions (*viz.*, the cellars) attack my wire, condemn the *houses* belonging thereunto, which cannot *easily* escape detection by running away" (p. 17).

Ronalds, however, proposed to rely upon other means than Lynch law in maintaining his communications, and here, again, the telegraph engineers of the present day have followed out his ideas almost to the letter. He proposed to keep his wire constantly charged with electricity, then to have certain proving stations (like that described on page 134) at frequent intervals along the line; and a staff of persons who would constantly watch the provers, and set out the moment that any indication of an interruption was given. Suitable situations for such proving stations he conceived to be "post-offices in towns and villages, turnpike gates, and the like."

"To put a simple case: we will imagine twenty proving stations established between London and Brighton, or any distance of fifty miles, only four persons employed (but not exclusively) to keep watch over them, and each watchman to have charge of five provers. It is evident that (were he to dwell at the centre one of the five), in order to examine the two on each side of it, he would have to ride only four miles and eight-tenths, which journey even our two-penny post-boy can perform in something less than forty minutes; and he would discover that the defect rested somewhere between two of the provers, a distance of two miles and four-tenths. Let him report his discovery accordingly to the engineer, who may open the trench and the trough at mid-distance of this two miles and four-tenths, make an experiment upon the wire itself similar to that of the provers, and, when he has discovered which half is defective, operate upon that half in the same way. Thus proceeding continually, he must arrive, after ten bisections, within about three yards of the defect" (pp. 19, 20).

Now, what are these innumerable "flush-boxes" which are to be found everywhere in the streets of London and other large cities but "provers" of our underground telegraphic system? Most people are familiar with the snake-like coils of telegraph wires, which are every now and then laid bare in those curious apertures in the pavement, and the little clock-face, with a single handle, which is the invariable companion of the workman engaged in the hole. He is simply "proving" a wire which has been found faulty. Then, again, as regards overhead wires, what are the "linemen" stationed at certain intervals along the route of a trunk line but the "provers" of the section which it is their duty to traverse from time to time, working on either side of their station, precisely as Ronalds would have worked his "sorry little two-penny post cove"?

We must not omit to mention that Ronalds clearly foresaw by the sheer force of reasoning the phenomenon of retardation of signals in buried wires, such as we find it to-day.* At p. 5 of his *brochure* he says:—

"I do not contend, nor even admit, that an *instantaneous discharge*, through a wire of *unlimited* extent, would occur in *all* cases" (p. 5). And again, on

* Zetzsche tries to combat this assertion at p. 38 of his *Geschichte der Elektrischen Telegraphie*, Berlin, 1867.

p. 12:—"That objection, which has seemed to most of those with whom I have conversed on the subject the least obvious, appears to me the most important, therefore I begin with *it*; *viz.*, the probability that the electrical compensation, which would take place in a wire enclosed in glass tubes of many miles in length (the wire acting, as it were, like the interior coating to a battery) *might* amount to the *retention* of a charge, or, at least, might destroy the *suddenness* of a discharge, or, in other words, it might arrive at such a degree as to retain the charge with more or less force, even although the wire were brought into contact with the earth."

Referring to the difficulty that had been urged of keeping the wire charged with electricity, Ronalds says, on p. 21:—"As to sufficiency (I have no dread of the charge of vanity in borrowing a boast from the great mechanic), give me *materiel* enough, and I will electrify the world. The Harlem machine would probably *in time* electrify, sufficiently for our purpose, a wire circumscribing the half of England: but we want to *save* time; therefore let us have a small steam engine, to work a sufficient number of plates to charge batteries, or reservoirs, of such capacity as will charge the wire *as suddenly* as it may be discharged when the telegraph is at work; and when it is *not* at work, let the machine be still kept in gentle motion, to supply the loss of electricity by default of insulation; which default, perhaps, could not be avoided, because (be the atmosphere ever so dry, and the glass insulators ever so perfect) conductors are, I believe, robbed of their electricity by the same three processes by which Sir Humphrey Davy and Mr. Leslie say that bodies are robbed of their sensible heat, *viz.*, by radiation, by conduction, and by the motion of the particles of air."

While freely admitting that electro-magnetism was much better adapted to the purposes of telegraphy, Ronalds maintained to the last the practicability of his own plans. In a letter to Mr. Latimer Clark, dated Battle, 9th Dec., 1866, he thus writes:—"Had the necessary steps been taken in 1816 to provide a tensional electric telegraph for Government and general purposes, such an instrument might have been constructed and usefully employed, and might have been greatly improved* after the so-called Oersted discovery. * * *

"Do we not all know that an electrophorus (of glass or resin) will remain charged, even when both opposed metallic surfaces are in conducting communication, and can *you* not believe that my difficulty

* In the way, for example, suggested in the following extract from Mr. (afterwards Sir) W. F. Cooke's letter to Ronalds, of 11th December, 1866:—"I have often thought what a fortunate thing it would have been if I had known of your labours in 1837. The letters of the alphabet, three letters in a row, might have been distinguished on your clocks by a movement of a needle to the left [for, say, the outer letter], the middle letter by a flourish of the needle right and left, and the inner letter by a movement of the needle to the right." See Ronalds' MSS.

of discharging my wire was greater than that of preserving a charge? * * *

"I could always supply as much electricity as might be wanted for any length of my telegraphic wire, and it did *not fail*, as many very respectable witnesses well know. I did not (properly speaking) discharge the wire so much as to cause the electrometer [balls] to collapse, the threads merely vibrated sufficiently to designate a sign when the wire was touched by a rapid stroke." *

* Extracted by kind permission of Mr. Latimer Clark. See also his letter of January 3, 1867, to Mr. (afterwards Sir) W. F. Cooke; and his comments on a letter in *The Reader* of January 5, 1867; both preserved in the *Ronalds' MSS. on the Electric Telegraph*.

CHAPTER V.

TELEGRAPHS BASED ON STATIC, OR FRICTIONAL, ELECTRICITY (*continued*).

1824.—*Egerton Smith's Telegraph.*

IN *The Kaleidoscope, or Literary and Scientific Mirror* * we find a paragraph in which the editor, Mr. Egerton Smith, suggests a telegraph, which resembles that of the Anonymous Frenchman, 1782, or that of Salvá, 1795, in the mode of indicating the signals; and that of Le Sage, 1782, in the mode of insulating the conducting wires. This paragraph, which was kindly pointed out to us by Mr. Latimer Clark, runs as follows:—

"Amongst the numerous, pleasing, and ingenious, philosophical recreations exhibited by Mr. Charles, at the Theatre of Magic, is the following beautiful electrical experiment:—Mr. Charles presents to any of the company a musical tablet, containing [the names of] twenty-four popular tunes; any lady or gentleman then privately selects one tune, which is marked with

* Liverpool, October 19, 1824, p. 133.

a silver bodkin. The book, or tablet, is closed without having been seen by Mr. Charles. It is then placed near the stage on a music-stand which communicates with another stand stationed in the orchestra above, at the very extremity of the room, at least thirty yards from the former. On this other stand is fixed a musical tablet corresponding with that below. The connection between the two music-books is made by means of twenty-four stationary wires, being the number of the tunes in each book. The musicians are directed to keep their eyes fixed upon the tablet in the orchestra, until, at Mr. Charles's command, an electrical shock passes from the lower to the upper music-book, illuminating the tune which had been secretly selected. The musicians, at this strange signal, forthwith proceed to play this illuminated air, to the great astonishment of the audience.

"There can be no doubt that most rapid telegraphs might be constructed on this principle, especially to convey intelligence in the night. We will imagine a case which is perfectly practicable, although the trouble and expense attending the project would outbalance all its advantages.

"If by means of pipes underground a communication were formed between Liverpool and London, and throughout the length of this tube twenty-four metal wires [were] stretched [and] supported at intervals by non-conducting substances, one of each of the wires communicating with a letter of the alphabet, formed of metal [foil], stationed at each extremity: If this were done, and it is quite practicable, we have little doubt that an express might be sent from Liverpool to London, and *vice versâ*, in a minute, or perhaps less. It would be necessary to have good chronometers, in order that the parties might be on the look-out at the precise time, or nearly so. The communication on this plan would be letter by letter; the person sending the message would merely have to touch the metallic letters in succession with the electric fluid, which would instantly pass along the wire to the other extremity where it would illuminate the corresponding letter. The communication would thus be made as fast as the operator could impart the shock."

1825.—"*Moderator's*" *Telegraph*.

The following proposal of what may be called a physiological telegraph* is extracted from the London *Mechanics' Magazine*, for June 11, 1825, p. 148:—

"Electric Telegraphs.

"Sir,—There is, I think, in one of the numbers of the *Spectator*, dated about a hundred years ago, a passage tending to ridicule some projector of that day, who had proposed to 'turn smoke into light and light into glory.' This early idea of gas-light, to which it seems plainly to refer, was received as an idle dream,

* See note p. 103, *supra*.

and is only preserved to us, like straws in amber, by the wit and satire of Addison, or Steele. We are to learn, therefore, not too hastily to reject even those hints which are not immediately clear to us.

"Under protection of this remark I venture to propose to you that a telegraphic communication may be held, at whatever distance, without a moment's loss of time in transmission, and equally applicable by day or night, by means of the electric shock.

"An experiment of this kind has been tried on a chain of conductors of three miles in extent, and the shock returned without any perceptible time spent in its going round; and may not the same principle be applicable for 100 or 10,000 miles? Let the conductors be laid down under the centre of the post-roads, imbedded in rosin, or any other the best non-conductor, in pipes of stoneware. The electric shock may be so disposed as to ignite gunpowder; but if this is not sufficient to rouse up a drowsy officer on the night-watch, let the first shock pass through his elbows, then he will be quite awake to attend to the second; and by a series of gradations in the strength and number of shocks, and the interval between each, every variety of signal may be made quite intelligible, without exposure to the public eye, as in the usual telegraph, and without any obstruction from darkness, fogs, &c. It was mentioned before that electricity will fire gunpowder—that is known; we may imagine, therefore, that on any worthy occasion, preparations having been made for the expected event, as the birth of a Royal heir, a monarch might at one moment, with his own hand, discharge the guns of all the batteries of the land in which he reigns, and receive the congratulations of a whole people by the like return.

"I am, &c.,

"MODERATOR." *

* To the same class belonged the electro-physiological telegraph proposed by Vorsselmann de Heer, and exhibited by him at a meeting of the Physical Society of Deventer, on January 31, 1839. In this system the correspondent received the signals at his fingers' ends, by placing them upon the ten keys of a finger-board, which, by means of separate line wires, communicated with corresponding keys at the distant station. The signals were indicated by sending an induction current through two of the wires, and the shocks were observed—(*a*) in one finger of the right and one of the left hand, or (*b*) in two fingers of the right hand, or (*c*) in two fingers of the left hand. The (*a*) shocks represented the letters of the alphabet, the (*b*) shocks, the ten numerals, and the (*c*) shocks, ten code, or conventional signs. See Vorsselmann de Heer's *Théorie de la Télégraphie Électrique*, &c., Deventer, 1839, and Moigno's *Traité de Télégraphie Électrique*, Paris, 1852, pp. 90 and 364. Reading by shocks, taken on the tongue, or fingers, has long been practised as a make-shift by inspectors and line-men all over the world. Varley mentioned it in the discussion which followed the reading of the late Sir William Siemens' paper before the Society of Arts, April 23, 1858.

Quite recently (April 1878), yet another form of physiological telegraph has been submitted by M. Mongenot to the French Academy, in which the transmitter and receiver are the same, and consist of two ivory plates carrying the disconnected ends of the two line wires. The sender places this contrivance between his lips, and sends the message by talking, or by closing the circuit by his lips according to a code of signals. The receiver, holding the receiving apparatus similarly, interprets the message by the sensation he feels. This plan was, evidently, suggested by Sulzer's experiment of 1767. See his *Nouvelle Théorie des Plaisirs*, p. 155; or p. 178, *infra*.

1825.—*R. H.'s Telegraph.*

In reference to the letter which we have just given from the *Mechanics' Magazine*, another correspondent "R. H.," wrote as follows in the number of the journal for June 25, 1825:—

"The present telegraphic communication is effected by means of six shifting boards, in a manner with which your readers are doubtless conversant. Now, if it be practicable to lay down one wire, it will be equally practicable to lay down six; and the cost of the wire would be nearly all the difference in the expense. Let the wires terminate in a dark room. On one wall let there be the figures 1, 2, 3, 4, 5, 6, prepared in *tin-foil*, according to the method practised by electricians, in forming what are called *luminous modes and figures.* Bring the six wires in contact with the six figures separately. With this contrivance, all the signals may be performed, as at present with six shifting boards. A shake of the arm, as 'Moderator' suggests, may call the watch to his duty; and he could name the signals as they appear, to his assistant, as is the present custom in the established telegraphs. His assistant must, of course, be separated from the dark room by a slight partition, that should be proof against light, but not against the full hearing of the human voice." *

* A further communication on the subject was promised but never made. In the hope of finding some clue to the writers of these letters, we have carefully looked through several succeeding volumes of the *Mechanics' Magazine*, but without success.

1825.—*Porter's Telegraph.*

We copy the following letter from *The Morning Herald*, of September 23, 1837 :—

"The Electric Telegraph.

"16, Somers Place, New Road, St. Pancras,
Sept. 16.

"Mr. Editor,—It now appears that the above application of the electric power is likely to be brought forward for the most useful purposes.

"At Munich, as stated by the *New Wurtsburg Gazette* of the 30th of June, the inhabitants were somewhat astonished by seeing, on the roofs of the loftiest houses, several men employed in passing iron wires, which extended from the towers of the church of Notre Dame to the observatory of Bogenhausen, and back to the church, intended to exemplify a project (so they call it) of Professor Steinheil, for the conveyance of intelligence by means of electric magnetism, whereby they conjecture that, in two seconds, communication may be conveyed from Lisbon to St. Petersburg. It further states there are other candidates beside the above-named Professor in the field, and a little time will decide whether Scotland, France, or Germany, is to carry off the honours for this disputed, and, if practicable, most valuable invention. If, Mr. Editor, you give place in your columns to the above and what follows, I think it will show that not a Scotchman, a Frenchman, or a German, but an Englishman, has the claim.

"On the 8th August, 1825, I requested the Lords

Commissioners of the British Admiralty to afford me an opportunity for bringing under their consideration a method of instantaneous communication with the out-ports, which neither foggy weather nor the darkness of night would obstruct. The next day I received the following answer :—

"'Admiralty Office, August 9, 1825.

"'Sir,—In reply to your letter of the 8th inst., I am commanded by my Lords Commissioners of the Admiralty to acquaint you that you may attend here any morning respecting your method of instantaneous communication with the out-ports either in foggy weather or at night.

"'I am, Sir, your obedient servant,
(Signed) "'J. W. CROKER.

"'To Mr. S. Porter.'

"I attended the board, and proposed to their Lordships that electrical machines should be kept ready for use at the Admiralty Office and at each out-port, and that a conducting chain, or wire, of brass, or copper, secured in tubes of glass, be carried under the surface of the most frequented roads, so that any malicious attempt to interrupt the communication would soon be observed by travellers. What, Mr. Editor, under such circumstances, can prevent the electric impulse from proceeding with the utmost velocity to its destination? The Lords of the Admiralty asked me whether I had prepared a code of signals? I answered no, but referred them to writings on the subject by Dr. Franklin, which show that more by the power of electricity can be given than by a telegraph of wood.

"The Germans are wrong in using iron wire, a metal most subject to corrosion, particularly when exposed to the changes of the atmosphere. I ask them two questions. How will they carry this wire from Lisbon to St. Petersburg, where lofty buildings on the line are rarely to be found? and how will they secure a poor bird from destruction, which, perching upon the decayed wire, may break it, and, together with a despatch from Lisbon, go into oblivion? The invention has been tried successfully on the London and Birmingham railroad, the conductors being enclosed in hemp, or wood. However, this will not do; both are of a perishable nature; both will absorb damp, and every part of the apparatus employed in electricity should be kept dry. Let the experiment be made with glass to protect the conductor, and it will be found durable; and, as to its effect, I feel confident that if such a method of communication had been prepared from Ramsgate to the Admiralty Office, and continued from thence to Windsor Castle, our most excellent Queen would have been apprised of the arrival of her illustrious relations, the King and Queen of the Belgians, before the last salute gun was fired.

"I am, Sir, your respectful servant,

"SAMUEL PORTER."

1826–7.—*Dyar's Telegraph.*

About this time Harrison Gray Dyar, of New York, constructed a telegraph which was of an entirely different character to any of those hitherto described, as it depended for its action on the power of the spark to effect chemical decompositions. This property of electricity was first observed about the middle of the last century, and, had chemical science attained then to a sufficiently advanced state, it could not have failed to lead to the discovery of electro-chemistry.*

Besides being an *electro-chemical* telegraph (although not the first), Dyar's invention had the great merit of being a (in fact, the first) *recording* telegraph, and a fairly perfect one to boot, and, had he only used

* Beccaria, by the electric spark, decomposed the sulphuret of mercury, and recovered the metals, in some instances, from their oxides. Watson found that an electric discharge passing through fine wire rendered it incandescent, and that it was even fused and burned. Canton, repeating these experiments with brass wire, found that, after the fusion by electricity, drops of copper only were found, the zinc having apparently evaporated. Beccaria observed that when the electric spark was transmitted through water, bubbles of gas rose from the liquid, the nature, or origin, of which he was unable to determine. Had he suspected that water was not what it was then supposed to be, a simple elementary substance, the discovery of its composition could scarcely have eluded his sagacity. Franklin found that the frequent application of the electric spark had eaten away iron; on which Priestley observed that it must be the effect of some acid, and suggested the inquiry whether electricity might not probably *redden vegetable blues?* Priestley also observed that, in transmitting electricity through a copper chain, a black dust was left on the paper which supported the chain at the points where the links touched it; and, on examining this dust, he found it to contain copper.—Lardner's *Electricity, Magnetism, and Meteorology*, vol. i. pp. 78–9.

voltaic, instead of static, electricity, the problem of electric telegraphy might have been solved in 1827. And, thus, with a start of several years, there can be little doubt that electro-chemical telegraphs would have made a better stand than they afterwards did in the struggle for existence; although, perhaps, there can be as little doubt that, in obedience to the inexorable law of the survival of the fittest, they must have eventually yielded to the more practicable electro-magnetic forms of Cooke and Morse.

In connection with one of the many telegraph suits in which Morse was long engaged in America, Dyar gave the following account of his early project, in a letter to Dr. Bell, of Charlestown, dated Paris, March 8, 1848:—

"Since reading your letter, and when searching for some papers in reference to my connection with this subject, I found a letter of introduction, dated the day before my departure from America, in February 1831, from an old and good friend, Charles Walker, to his brother-in-law, S. F. B. Morse, artist, at that time in Europe. At the sight of this letter, it occurred to me that this Mr. Morse might be the same person as Mr. Morse of the electric telegraph, which I found to be the case. The fact of the patentee of this telegraph, which is so identical with my own, being the brother-in-law of, and living with, my friend and legal counsel, Charles Walker, at the time of, and subsequent to, my experiments on the electric telegraph in 1826 and

1827, has changed my opinion as to my remaining passive, and allowing another to enjoy the honour of a discovery, which, by priority, is clearly due to me, and which, presumptively, is only a continuation [resumption] of my plans, without any material invention [improvement] on the part of another.

* * * * * *

"I invented a plan of a telegraph, which should be independent of day, or night, or weather, which should extend from town to town, or city to city, without any intermediary agency, by means of an insulated wire, suspended on poles, and through which I intended to send strokes of electricity, in such a manner as that the diverse distances of time separating the divers sparks should represent the different letters of the alphabet, and stops between the words, &c. This absolute, or this relative, difference of time between the several sparks I intended to take off from an electric machine by a little mechanical contrivance, regulated by a pendulum; while the sparks themselves were intended to be recorded upon a moving, or revolving, sheet of moistened litmus paper, which, by the formation of nitric acid by the spark in its passage through the paper, would leave [show] a red spot for each spark. These so-produced red spots, with their relative interspaces, were, as I have said, taken as an equivalent for the letters of the alphabet, &c., or for other signs intended to be transmitted, whereby a correspondence could be kept up through one wire of any length, either in one direction, or back and forwards, simultaneously or successively. In addition to this use of electricity I considered that I had, if wanted, an auxiliary resource in the power of sending impulses along the same wire, properly suspended, somewhat like the action of a common bell-wire in a house.

"Now you will perceive that this plan is like that known as Morse's telegraph, with the exception that his is inferior to mine, inasmuch as he and others now make use of electro-magnetism, in place of the simple spark, which requires that they should, in order to get dots, or marks, upon paper, make use of mechanical motions, which require time; whereas my dots were produced by chemical action of the spark itself, and would be, for that reason, transmitted and recorded with any required velocity.

* * * * * *

"In order to carry out my invention I associated myself with a Mr. Brown, of Providence, who gave me certain sums of money to become my partner. We employed a Mr. Connel, of New York, to aid in getting the capital wanted to carry the wires to Philadelphia. This we considered as accomplished; but, before beginning on the long wire, it was decided that we should try some miles of it on Long Island. Accordingly I obtained some fine card wire, intending to run it several times around the Old Union Race-course. We put up this wire at different lengths, in curves and straight lines, by suspending it [with glass insulators] from stake to stake, and tree to tree, until we concluded that our experiments justified our undertaking to carry it from New York to Philadelphia. At this moment our agent brought a suit, or summons, against me for 20,000 dollars, for agencies and services, which I found was done to extort a concession of a share of the whole project.

"I appeared before Judge Irving, who, on hearing my statement, dismissed the suit as groundless. A few days after this, our patent agent (for, being no longer able to keep our invention a secret, we had applied for a patent) came to Mr. Brown and myself and stated that Mr. Connel had obtained a writ against us, under a charge of conspiracy for carrying on secret communication from city to city, and advised us to leave New York until he could settle the affair for us. As you may suppose, this happening just after the notorious bank-conspiracy trials, we were frightened beyond measure, and the same night slipped off to Providence. There I remained some time, and did not return to New York for many months, and then with much fear of a suit. This is the circumstance which put an end [to our project], killing effectually all desire to engage further on such a dangerous enterprise. I think that, on my return to New York, I consulted Charles Walker, who thought that, however groundless such a charge might be, it might give me infinite trouble to stand a suit. From all this the very name of electric telegraph has given me pain whenever I have heard it mentioned, until I received your last letter, stimulating me to come out with my claims; and even now I cannot overcome the painful association of ideas which the name excites."

To this very interesting statement, Dr. Bell has added the following corroborative testimony:—"I was engaged with Harrison Gray Dyar for many months in 1828. We often conversed upon the subject of his having invented an electric telegraph, and I recollect seeing in his apartment a quantity of iron wire which he had procured for the construction of his telegraph. I recollect his saying he had suspended some of this wire at an elevation around the race-course at Long Island, to a length which satisfied him that there were no practical difficulties in carrying it from New York to Philadelphia, which, he stated, was his intention. I recollect suggesting doubts whether the wire would bear the necessary straightening up between the posts, and his reply, that the trial on Long Island had proved to him that there was no difficulty to be apprehended in this direction. My impression, derived from his conversation, was that the electric spark was to be sent from one end of the wire to the other, where it was to leave its mark upon some chemically prepared paper."*

* In these extracts we have followed *History, Theory, and Practice of the Electric Telegraph*, by George B. Prescott, Boston, 1860, pp. 427–30; *Historical Sketch of the Electric Telegraph*, &c., by Alexander Jones, New York, 1852, pp. 35–7; and *The Telegrapher*, New York, vol. i. pp. 48 and 163.

In 1831 Dyar came to Europe on business connected with some of his mechanical inventions, and resided principally at Paris until 1858, when he returned to the United States for good. His connection with telegraphy is somehow little known to the present generation, although, in 1826–7, he was widely known, at least in America, for his electrical researches. It is satisfactory to learn that his pursuits in other departments of science brought him an ample fortune, which was largely augmented by real estate investments in the city of New York. Dyar was born at Boston, Mass., in 1805, and died at Rhinebeck, N.Y., on the 31st January, 1875.

1828.—*Tribouillet de St. Amand's Telegraph.*

In this year* Victor Tribouillet de St. Amand proposed a single line telegraph between Paris and Brussels. The conducting wire was to be varnished with shellac, wound with silk, coated with resin, and enclosed in lengths of glass tubing carefully luted with resin; the whole being substantially wrapped and water-proofed, and, finally, buried some feet deep in the earth.

Nothing is known for certain of the signalling arrangements, and it is even doubtful to what class the invention belongs; as, while a strong voltaic battery was the source of electricity, the receiving

* According to *Journal des Travaux de l'Acad. de l'Industrie Française*, Mar. 1839, p. 43.

instrument was to be an electroscope, or electrometer. Vail,* Prescott,† and American writers, generally, evidently regard it as belonging to the electro-magnetic form; while Zetzsche ‡ and Guerout § class it amongst those based on static electricity.

The author appears to have provided no particular form of alphabet, or code, leaving it to each person to devise his own out of the motions of which the electroscope was susceptible.

1830.—*Recy's Telegraph.*

In a *brochure* of 35 pages, entitled *Télétatodydaxie, ou Télégraphie Électrique*, Hubert Recy describes a crude system of syllabic telegraphy. Although his little book was not published in Paris until 1838, we gather from the text that his plans were laid as early as 1830. At p. 34 he writes:—"I had a thought of offering [my teletatodydax] to civilisation, a thought fixed and durable, because, notwithstanding some respectable opinions, I believe it useful to man in seasonable times; but I did not wish to make it known in 1830 and during the stormy years that followed."

His telegraphic language is composed of (*a*) four initial vowels, (*b*) fifteen diphthongs, and (*c*) six

* *American Electro-Magnetic Telegraph*, 1845, p. 135.

† *History, Theory, and Practice of the Electric Telegraph*, 1860, p. 394.

‡ *Geschichte der Elektrischen Telegraphie*, 1877, sec. 6, para. 11.

§ *La Lumière Électrique*, March 3, 1883, p. 263.

monosyllables, all of which, with their various combinations, are figured in tables at pp. 5 and 6 of his pamphlet.

The line wires were to be of iron, enveloped in wax-cloth, then well tarred and enclosed in a leaden tube to preserve them from moisture, and so prevent the diminution of the force of the electric spark. They might be placed at some feet underground along the high roads like water pipes, and those parts destined for submersion in water, across the sea, for example, to England, should be prepared with the greatest care, so as to entirely exclude the moisture.*

In certain cases, he says, the metals of the railway could be used as lines of communication for the conveyance of the electric spark, and nothing would be easier than to put them into a condition to fulfil this important function, each rail representing a line.

At the sending station were the electrical machines for producing the sparks, and electrometers, one on each wire, for indicating the passage and strength of the same. At the receiving station the lines terminated in needles, or points, which dipped into little cups containing some inflammable substance like alcohol, or even hydrogen gas. A sufficient number of these cups was always at hand, ready

* At p. 25 he repeats:—"To communicate with England, Algeria, and other places, it would suffice to enclose the iron wire in an impermeable cloth, well tarred, and covered with sheet lead. In this way the electricity would operate with as much freedom as in subterranean lines, to which rivers would be no obstacles."

charged, to take the place of those exploded in the course of correspondence.

The line wires, which were bound together side by side, were marked, the one, say the right, with the *units*, or vowels, and the other, the left, with the *fives*, or diphthongs. As a general rule in teletatodydaxy that vowel termination which aids most in the expression and comprehension of a word, or phrase, is é (*é fermé*); for example—" Méhémet-Ali, vice-roi d'Égypte, fait travailler à la découverte des mines de Syrie " might be transmitted thus—Méhémété élé, vécé ré d'Égépété, fé térévéléré é lé dékévérété dé méné dé Séré, in pronouncing which rapidly, and without dwelling too much on the é, the ear would easily comprehend the sense.*

After showing, pp. 10 to 14, how this sentence should be transmitted, the author says that every conceivable communication could be made in the same way, each syllable being expressible, according to his tables, by vowels alone, or by vowels and diphthongs combined. One class of vowel, or *units*, was represented by one spark in the right line, a second class by two sparks, a third by three, and a fourth by four. Each diphthong (and monosyllable), or *five*,

* Recurring to this subject, the author says, on p. 16, Suppose the phrase to be pronounced by a stranger, you listen, and, as in a discourse one single sentence, when well understood, enables one to gather the sense of the whole, so in this case one single word well understood aids to a comprehension of the whole sentence, usage and practice will do the rest.

was similarly represented by one, two, three, or four, sparks in the left wire, either alone, or immediately followed by one, two, three, or four, sparks in the right wire, according as the syllable was in the first, second, third, or fourth, class of *fives*, and in the second, third, fourth, or fifth place of the class. Thus, Ba, which is in the first place of the first class of *fives*, would be represented by one spark in the left wire; while Pa, which is in the third place of the second class, would be indicated by two sparks in the left wire for the class, followed by three sparks in the right for the place.

In case it would be impossible to establish two wires, on account of the expense, or from any other cause, the author shows how one wire would suffice, the signalling requiring, in this case, only a little more time, and a little more attention. The vowels would be transmitted as before, but the diphthongs and monosyllables would be expressed by two sparks in rapid succession *te te*, the interval between being much less than that between the vowels; and, for greater clearness, the end of each word would be notified by the signal A, which would be neither the end of the last word, nor the commencement of the following.

If desired, each letter, or character, of the teletatodydaxical tables could represent some conventional phrase, or the sparks could stand for figures which would belong to words and phrases in a dictionary, or code.

The author concludes a rhapsody on the uses which the great Napoleon would have made of teletatodydaxy had it been then discovered * in words, which, in these days of their realisation, deserve to be remembered :—" If, in the time of Napoleon, gas-lighting had been as general as it is now, and some one had told him : 'by means of the teletatodydax you can, in less than a second, light all the lamps of the capital at the same time and as one lamp'; or, as everything sublunary has disadvantages as well as advantages, 'you cannot guard yourself against the malefactors who would sow infernal machines under your feet, would fire your ships, arsenals, powder-magazines, and monuments'—enemies all the more difficult to discover, since they can perpetrate their crimes from afar by means of the wire; would Napoleon have shut his eyes and ears to these facts? No, such advantages and disadvantages combined would certainly have fixed his attention, and, not being able to annihilate a power of which he would wish only himself to know the force, he would so control it as to draw for himself all the advantages, and, at the same time, prevent others from putting it to wrongful ends" (p. 34).

1837.—*Du Jardin's Telegraph.*

Du Jardin, of Lille, whose fast-speed type-writer was used, for a short time, in 1866, on the late

* *We* think he would have made short work of it.

Electric and International Telegraph Company's lines, was occupied with the telegraph as far back as 1837.

His first ideas on the subject were, in that year, communicated to the Paris Academy of Sciences; but, except the bare title of the paper in the *Compte Rendu*, for July 10, 1837, nothing appears to have been published. We learn, however, from Professor Magrini* that he proposed to erect a single wire between the Tuileries and the Arc de l'Etoile, and to employ an electric machine and a sensitive electroscope for the signalling apparatus.

If none of the contrivances that we have described in the foregoing pages ever passed the stage of experiment, it is because they, one and all, laboured under two heavy disadvantages—the one, that they were in advance of the age, and the other, the intractable nature of the force employed, rendering its transmission to any distance impossible in the open air, and exceedingly difficult through buried wires.

Of course, if no other form of electricity had been discovered, some of these inventions—notably those of Alexandre, Ronalds, and Dyar—could be improved, so that we should have at this day electric telegraphs, not so simple, nor with so many resources as those at present in use, but yet instruments that would fulfil the grand object of communicating at a distance with lightning speed. Many practical difficulties

* *Telegrafo Elettro-Magnetico*, Venezia, 1838, p. 23.

would, however, remain, which, even with our present extended knowledge, we could not entirely obviate, and which would, therefore, have hindered their complete success.

If, then, none of their authors, though through no fault of his own, deserves the title of inventor of a really practicable and commercially successful telegraph, we must, at least, give one and all the credit of having fully appreciated its importance, and of having dedicated their energies to the accomplishment of the task they set themselves in the face of many difficulties and disappointments.*

* Since 1837 the following telegraphs have been proposed in which static electricity was to be employed:—By the Rev. H. Highton, in 1844 (Patent No. 10,257 of 10th July); by Isham Baggs, in 1856 (Patent No. 1775 of 25th July)—a most interesting document, which will repay perusal in these days of multiplex and fast-speed apparatus; by C. F. Varley, in 1860 (Patent No. 206 of 27th January); and by Wenckebach, a Dutch electrician, in 1873 (*Journal Télégraphique de Berne*, for March 25, 1873).

CHAPTER VI.

DYNAMIC ELECTRICITY—HISTORY IN RELATION TO TELEGRAPHY.

> "The hooked torpedo, with instinctive force,
> Calls all his magic from its secret source;
> Quick through the slender line and polished wand
> It darts, and tingles in the offending hand."
>
> Pennant's *Oppian*.

THE discoveries of the Italian philosophers, Galvani and Volta, at the close of the last century, marked a new era in the history of telegraphy, by furnishing a form of electricity as tractable and copious, as that derived from friction was volatile and small.

Before entering into this subject it may be well to say a few words on the early history of what has been called *Animal Electricity*—a force which is identical with, and whose early manifestations in certain fishes led up to, Galvanism.

Although this power is now known to exist in many fishes, and even in some of the lower animals,*

* With the aid of a microscope sparks have been seen to issue from the *annelides* and *infusoria*, and the luminosity of the glow-worm and other shining insects is thought to be due to the same cause. Margrave describes an insect, a native of Brazil, which, on being touched, gives a very perceptible shock; and specimens of the *Sepia* and *Polypi* have also been observed to do the same.—Kirby and Spence's *Introduction to Entomology*, London, 1856, 7th ed., p. 56.

the torpedo was the only instance known to the ancients.*

Aristotle says :—"This fish hides itself in the sand, or mud, and catches those that swim over it by benumbing them, of which some persons have been eye-witnesses. The same fish has also the power of benumbing men."† Pliny writes :—"From a considerable distance even, and if only touched with the end of a spear, or staff, this fish has the property of benumbing the most vigorous arm, and of rivetting the feet of the runner, however swift he may be in the race."‡ Plutarch declares that the torpedo affects fishermen through the drag net, and that, were water to be poured on a living one, the person pouring it would be affected, the sensation being communicated through the water to the hand. Claudian and Galen have much to the same effect, and Oppian is even more explicit, for he describes the organs by which the fish exerts its extraordinary power. "It is," he says, "attributable to two organs of a radiated texture, which are situated one on each side of the fish."§

The ancients knew something also of what we

* The name of the torpedo in the Arabian language is *ra'ad*, which means *lightning*.

† *History of Animals*, ix. 37.

‡ *Natural History*, xxxii. 2.

§ Lib. ii. v. 62. In the *Phil. Trans.*, for 1773, p. 481, the celebrated Hunter published the anatomical structure of the torpedo, showing the position of the electric organs. In a fish eighteen inches long it was found that the number of columns composing each organ amounted to 470.

would now call *Medical Electricity*. Thus, we read that Dioscorides, the physician of Anthony and Cleopatra, used to cure inveterate headaches by applying a live torpedo to the head; * and that (as related by Scribonius Largus †) Anthero, a freedman of Tiberius, was cured of the gout by the same means. The patient in such cases had to stand on the sea-shore with a live torpedo under foot, until not only the feet but the legs as far as the knees became numb.

The *Gymnotus electricus* was first made known in Europe in 1671 by Richer, one of a party sent out by the French Academy for astronomical observations at Cayenne. The accounts which he brought home of its *shocking* powers were, however, received with much scepticism, and it was not until towards the middle of the last century that the observations of Condamine, Fermin, Bancroft, and others had fully established their credibility.

The gymnotus, which inhabits the warmer regions of Africa and South America, delivers far stronger shocks than the torpedo, the strokes of the larger ones being, according to Bancroft, instantly fatal. When one of average dimensions is touched with one hand a smart shock is felt in the hand and forearm; and when both are applied it affects the whole frame, striking, apparently, to the very heart. Thus, Humboldt mentions that, treading upon an ordinary

* Lib. ii., Art. Torpedo.

† *De Compositione Medicamentorum Medica*, cap. i. and xli.

specimen, he experienced a more dreadful shock than he ever received from a Leyden jar, and that he felt severe pain in his knees, and other parts of his body, which continued for several hours. According to Bryant, a discharge sometimes occasions such strong cramps of the muscles which grasp the fish that they cannot let it go.*

On the river Old Calabar, the electrical properties of the gymnotus are used by the natives to cure their sick children; a small specimen of the fish is put into a dish containing water, and the child is made to play with it, or the child is put into a tub of water and the fish put in beside it.

Of the remaining electrical fishes, the Silurus, introduced by Adanson, in 1751, is an inhabitant of the Nile and Senegal; the Trichiurus inhabits the Indian Seas; and the Tetraodon is found near the Canary Islands and along the American coast.

Although Redi, 1678, Kempfer, 1702, and others had made many and accurate observations on the torpedo, the electrical nature of the phenomena exhibited by this and the other fishes that we have named was not known, nor even suspected, up to the middle of the last century. The idea first occurred to Professor Musschenbröck of Leyden in reference to the torpedo, and nearly at the same time (1751), Adanson formed a similar notion regarding the

* *Transactions of the American Society*, vol. ii. See also *Mechanics' Magazine*, for August 6, 1825.

Silurus; but it was not till the years 1772–4 that the fact was clearly established by the experiments of Walsh, S'Gravesande, Hunter, Ingenhousz, and others.*

Walsh, in transmitting to Benjamin Franklin, then in London, the results of his researches for communication to the Royal Society, says:—"It is with peculiar interest that I make to you my first communication, that the effect of the torpedo appears to be absolutely electrical," and he concludes, after going fully over the details, "He, who predicted and showed that electricity wings the formidable bolt of the atmosphere, will hear with attention that in the deep it speeds a humbler bolt, silent and invisible; he, who analysed the electric phial, will hear with pleasure that its laws prevail in *animated* phials; he, who by reason became an electrician, will hear with reverence of an instinctive electrician gifted at its birth with a wonderful apparatus, and with skill to use it.†

It is singular that, while the examination of the torpedo was going on in Europe, similar investigations were taking place in America with respect to the gymnotus. These were made in Philadelphia and Charleston by Drs. Williamson and Garden, and the same conclusions, grounded on the same *data*, were arrived at. These are thus summed up by their authors:—"As the fluid discharged by the eel affects the same parts that are affected by the electric fluid;

* *Phil. Trans.*, 1773 and 1775. † Ibid., 1773, pp. 461–72.

as it excites sensations perfectly similar; as it kills and stuns animals in the same manner; as it is conveyed by the same bodies which convey the electric fluid, and refuses to be conveyed by others that refuse to convey the electric fluid, it must itself be the electric fluid, and the shock given by the eel must be the electric shock." *

Though these early experiments thus led to the strong presumption that this peculiar animal power was precisely of the same nature with common electricity, yet they were very far from affording that absolute demonstration which alone satisfies the requirements of modern science; and, hence, naturalists have ever been on the watch to seize every opportunity which could supply additional evidence. The science of electricity, likewise, has since those days been prosecuted with the greatest success, and the phenomena of the respective subjects have mutually thrown light upon each other. As regards electricity, there are now a number of palpable effects which are considered as demonstrative of its presence and operation, chief amongst which are the shock, the electric spark, heat, magnetic virtue, and chemical agency. These positive proofs of the operation of electricity were soon desiderated in connection with the animals we have named, and one after another, by the ingenuity of experimenters, have been at last obtained.†

Now to resume our subject. In the hundred years

* *Phil. Trans.*, 1775, pp. 94 and 102.

† Faraday's *Exper. Researches*, series iii. and xv.

preceding the discoveries of Galvani and Volta, we find record of many observations of a character closely resembling the fundamental ones, which, in their hands, led to the grand discovery of dynamic electricity. Thus, in 1671, Richter noticed that the gymnotus was able to produce by its shocks a sort of sympathetic quivering in dead fishes lying around it. In 1678, Swammerdam, in some experiments before his friend and patron, the Grand Duke of Tuscany, produced convulsions in the muscle of a frog, by holding it against a brass ring from which it hung by a silver wire—an experiment which, as we shall presently see, exactly resembles that by which Galvani became so famous more than a hundred years later.

This celebrated experiment is thus described in Swammerdam's *Biblia Naturæ*, vol. ii. p. 839:—"Let there be a cylindrical glass tube, in the interior of which is placed a muscle, whence proceeds a nerve that has been enveloped in its course with a small silver wire, so as to give us the power of raising it without pressing it too much, or wounding it. This wire is made to pass through a ring bored in the extremity of a small copper support and soldered to a sort of piston, or partition; but the little silver wire is so arranged that, on passing between the glass and the piston, the nerve may be drawn by the hand and so touch the copper. The muscle is immediately seen to contract."

Du Verney, in 1700, made a similar observation, and Caldani, 1757, described what he called "the revival of frogs by electric discharges." Du Verney's experiment is thus described:—"M. Du Verney showed a frog just dead, which, in taking the nerves of the belly that go to the thighs and legs, and irritating them a little with a scalpel, trembled and suffered a sort of convulsion. Afterwards he cut the nerves, and, holding them a little stretched with his hand, he made them tremble again by the same motion of the scalpel." *

The experiments described in the following extract from the *Philosophical Transactions*, for 1732, are of an exactly similar kind. We copy from a paper headed "Experiments to prove the existence of a fluid in the nerves," by Alexander Stuart, M.D.:—

"The existence of a fluid in the nerves (commonly called the *animal spirits*) has been doubted of by many; and, notwithstanding experiments made by ligatures upon the nerves, &c., continues to be controverted by some. This induced me to make the following experiments, which I hope may help to set that doctrine, which is of so much consequence in the animal economy and practice of physic, in a clearer light than I think it has hitherto appeared in.

"*Experiment I.*—I suspended a frog by the fore-legs in a frame leaving the inferior parts loose; then,

* Martyn and Chambers' *The Phil. Hist. and Mems. of the Royal Acad. of Sciences at Paris*, London, 1742, vol. i. p. 187. Du Verney was a celebrated anatomist, for whom the use of vaccine as early as 1705 is claimed with a great show of reason. See Fournier's *Le Vieux-Neuf*, Paris, 1859, vol. ii. p. 385.

the head being cut off with a pair of scissors, I made a slight push perpendicularly downwards, upon the uppermost extremity of the *medulla spinalis*, in the upper vertebra, with the button-end of the probe, filed flat and smooth for that purpose; by which all the inferior parts were instantaneously brought into the fullest and strongest contraction; and this I repeated several times, on the same frog, with equal success, intermitting a few seconds of time between the pushes, which, if repeated too quick, made the contractions much slighter.

"*Experiment II.*—With the same flat button-end of the probe, I pushed slightly towards the brain in the head, upon that end of the *medulla oblongata* appearing in the occipital hole of the skull; upon which the eyes were convulsed. This also I repeated several times on the same head with the same effect.

"These two experiments show that the brain and nerves contribute to muscular motion, and that to a very high degree."*

In their results these experiments were precisely the same as those with which the name of Galvani is associated. Nor was the mode of operating very different, even in the use of only one kind of metal. In Galvani's experiments, excitation was produced by contact, or communication, of nerves and muscles. In Stuart's the convulsions were produced by exciting the spinal marrow.

* Vol. xxxvii. p. 327.

Sulzer, in his *Nouvelle Théorie des Plaisirs*, published at Berlin in 1767, described the peculiar taste occasioned by pieces of silver and lead in contact with each other and with the tongue. He, however, had no suspicion of the electrical nature of this effect, but thought it "not improbable that, by the combination of the two metals, a solution of either of them may have taken place, in consequence of which the dissolved particles penetrate into the tongue; or we may conjecture that the combination of these metals occasions a trembling motion in their respective particles, which, exciting the nerves of the tongue, causes that peculiar sensation."*

The next person to whom chance afforded an opportunity of making the discovery of galvanism, but who let it pass with as little profit as Sulzer and his predecessors had done, was Domenico Cotugno, professor of anatomy at Naples. His observations are contained in the following letter,† dated Naples, October 2, 1784, and addressed to the Chevalier Vivenzio:—

"Sir,—The observation which I mentioned some days ago, when we were discoursing together of the electrical animals upon which I said that I believed the mouse to be one of the number, is the following:—

"Towards the latter end of March I was sitting with

* Note to text on p. 155. The date of this experiment is variously stated as 1752, and 1760. See note under Sulzer in *Ronalds' Catalogue*.

† Extracted from Cavallo's *Complete Treatise on Electricity*, 4th ed., London, 1795, vol. iii. p. 6.

a table before me; and observing something to move about my foot, which drew my attention, looking towards the floor, I saw a small domestic mouse, which, as its coat indicated, must have been very young. As the little animal could not move very quick, I easily laid hold of it by the skin of the back, and turned it upside down; then with a small knife that laid by me, I intended to dissect it. When I first made the incision into the epigastric region, the mouse was situated between the thumb and first finger of my left hand, and its tail was got between the two last fingers. I had hardly cut through part of the skin of that region, when the mouse vibrated its tail between the fingers, and was so violently agitated against the third finger, that, to my great astonishment, I felt a shock through my left arm as far as the neck, attended with an internal tremor, a painful sensation in the muscles of the arm, and such giddiness of the head, that, being affrighted, I dropped the mouse.

The stupor of the arm lasted upwards of a quarter of an hour, nor could I afterwards think of the accident without emotion. I had no idea that such an animal was electrical; but in this I had the positive proof of experience." *

* Volta, in telling this story in after years, used to say that Cotugno was a pupil of Galvani, and that it was his drawing his master's attention to the phenomenon that put Galvani on the trail of his great discovery.—Robertson's *Mémoires Récréatifs Scientifiques et Anecdotiques*, Paris, 1840, vol. i. p. 233.

Galvani's great discovery is popularly supposed to have resulted from an accidental observation on frogs made in 1790; but as early, at least, as 1780, he was engaged, as we learn from Gherardi, his biographer, in experiments on the muscular contractions of these animals under the influence of electricity.*

One day in that year (November 6), while preparing "in the usual manner" a frog in the vicinity of an electrical machine with which some friends were amusing themselves, he observed the animal's body to be suddenly convulsed. Astonished at this phenomenon, and supposing that it might be owing to his having wounded the nerve, Galvani pricked it with the point of his knife to assure himself whether or not this was the case, but no convulsion ensued. He again touched the nerve with his knife, and, directing a spark to be taken at the same time from the machine, had the pleasure of seeing the contortions renewed. Upon a third trial the animal's body remained motionless, but observing that he held the knife by its ivory handle, he grasped the metal, and immediately the convulsions took place each time that a spark appeared.†

* From two papers in the *Bolognese Transactions*, one, *On the Muscular Movement of Frogs*, dated April 22, 1773; and the other, *On the Action of Opium on the Nerves of Frogs*, dated January 20, 1774, it is evident that Galvani's acquaintance with frogs was long anterior even to the year 1780. We follow mainly, in our account of Galvani's researches, Professor Forbes' Dissertation (Sixth), chap. vii., in the *Encyclopædia Britannica*, 8th ed.

† These experiments are similar to, and are explained by, the

After a number of similar experiments with the machine, Galvani resolved to try the effect of atmospheric electricity, and with this object erected a lightning conductor on the roof of his house to which he attached metallic rods leading into his laboratory. These he connected with the nerves of frogs and other animals, and fastened to their legs wires which reached to the ground. As was anticipated, the animals were greatly convulsed whenever lightning appeared, and even when any storm-cloud passed over the apparatus. These experiments were continued in 1781 and 1782, and were afterwards embodied in a paper (not published) *On the Nervous Force and its Relation to Electricity*. In 1786, Galvani resumed the inquiry with the aid of his nephew, Camillo, and it was in the course of these studies that certain facts were observed which led immediately to the discovery of galvanism.

One day (the 20th) in September 1786, Camillo Galvani had prepared some frogs for experiment, and

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phenomenon of the *lateral shock*, or *return stroke*, first observed by Wilson, of Dublin, in 1746, but first explained by Lord Mahon in 1779. In Galvani's experiment the frog, while it merely lay on the table, so being insulated, had its electricities separated by induction at every turn of the machine, and on the passage of every spark their reunion took place, but with so small effect that it escaped notice. When, however, the animal was placed in connection with the ground, through the knife and body of the professor, one of the separated electricities freely escaped, thus rendering a greater inductive charge possible, and raising the *return stroke* to a sufficient strength to convulse the dead limbs. It is but fair to add that Galvani himself suggested this explanation some years later.

had hung them, by an iron hook, from the top of an iron rail of the balcony outside Galvani's laboratory to be ready for use. Soon he noticed that when, by accident, a frog was pressed, or blown, against the rail, the legs contracted as they were wont to do when excited by the electricity of the machine, or of the atmosphere. Surprised at this effect where there was apparently no exciting cause, he called his uncle to witness it, but Galvani dismissed it on the easy assumption that the movements were connected with some unseen changes in the electrical state of the atmosphere. He soon, however, found that this was not the case, and, after varying in many ways the circumstances in which the frogs were placed, at length discovered that the convulsions were the result of the simultaneous contact of the iron with the nerves and muscles, and that the effect was increased by using a combination of different metals—such as iron and silver, or iron and copper.

Galvani, who was an anatomist first and an electrician afterwards, accounted for these effects by supposing that in the animal economy there exists a natural source of electricity; that at the junction of the nerves and muscles this electricity is decomposed, the positive fluid going to the nerve, and the negative to the muscle; that these are, therefore, analogous to the internal and external coatings of a charged Leyden jar; that the metallic connection made between the nerve and the muscle serves as a conductor for these opposite electricities; and that, on establishing the connection, the same discharge takes place as in the Leyden experiment. Galvani's researches were not made public until the year 1791, when they were embodied in his celebrated paper printed in the Bolognese *Transactions* of that year.

It will be evident from this account, which is based upon the researches of Gherardi, Galvani's biographer, supported by original documents, how absurd is the popular story, first invented by Alibert in his *Éloges historiques de Galvani* (Paris, 1802), and constantly repeated since, that "this immortal discovery arose, in the most immediate and direct way, from a slight cold with which Madame Galvani was attacked in 1790, and for which her physician prescribed the use of frog-broth." As if frog-broth were usually prepared in the laboratory!

Luigi Galvani was born at Bologna on the 9th of September, 1737, and died there December 4, 1798. From his youth he was remarkable for the ardour with which he prosecuted his studies in anatomy and physiology, and at the early age of twenty-five he was appointed professor of these sciences in the University of his native place.

The closing years of his life form a sad contrast to those of his great contemporary, Volta, who died, in 1827, covered with honours.* At the moment when

* Alessandro Volta was born at Como, February 19, 1745. Soon after his discovery of the pile, in 1801, he was invited to Paris, and

Galvani was immortalising his name, he was obliged to undergo the most cruel blows of destiny; for he lost his dearly loved wife, Lucia Galeazzi, and, a short time afterwards, had the misfortune to be ordered by the Cisalpine Republic to take an oath which was entirely opposed to his political and religious convictions. He did not hesitate a moment, but promptly refused, and permitted himself to be stripped of his position and titles. Reduced nearly to poverty, he retired to his brother's house, and soon fell into a state of lethargy from which he could be aroused, neither by medicine, nor by the decree of the government, which, out of respect for his celebrity, reinstated him in his position as professor of anatomy in the University of Bologna. The great physicist died without having again occupied the chair which he had rendered so illustrious.

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was honoured with the presence of the First Consul while repeating his experiments before the Institute. Bonaparte conferred upon him the orders of the Legion of Honour, and of the Iron Crown, and he was afterwards nominated a count, and senator of the kingdom of Italy. At the formation of the Italian Institute, a meeting was held, at which Bonaparte presided, for the purpose of nominating the principal members. When they were considering whether or not they should draw up a list of the members in an alphabetical order, Bonaparte wrote at the head of a sheet of paper the name of Volta, and, delivering it to the secretary, said, "Do as you please at present, provided that name is the first." At his death, on March 5, 1827, his fellow-citizens struck a medal, and erected a monument to his memory; and a niche in the façade of the public schools of Como, which had been left empty for him between the busts of Pliny and Giovio, natives of the town, was fi[illegible] his bust. See note on p. 84.

In 1879, the city of Bologna erected a statue in his honour, from the chisel of Adalbert Cincetti, the eminent Roman sculptor. It represents him at the moment when the muscles of the frog are revealing to him the effects of electricity on the animal organism.

Galvani's theory fascinated for a time the physiologists. The phenomena of animal life had hitherto been ascribed to an hypothetical agent, called the *nervous fluid*, which now the new discovery had consigned to oblivion. Electricity was, henceforth, the great vital force, by which the decrees of the understanding, and the dictates of the will, were conveyed from the organs of the brain to the obedient members of the body.

CHAPTER VII.

DYNAMIC ELECTRICITY—HISTORY IN RELATION TO TELEGRAPHY (*continued*).

ALEXANDER Volta, then Professor of Physics at Pavia, and already well-known for his researches in electricity, had naturally his attention directed, in common with other philosophers, to the Bolognese experiments, and, although at first he warmly espoused Galvani's opinions, his superior sagacity soon enabled him to detect their want of basis. He first ascertained that the contractions of the frog ensued on simply touching, with the extremities of the metallic arc, two points of the same nervous filament; he next found that it was possible with the metallic arc to produce, either the sensation of light, or that of taste, by applying it to the nerves of the eye and tongue respectively.* In short, he ended by showing that the exciting cause was nothing more nor less than ordinary electricity, produced by the *contact* of the two metals, the convulsion of the frog being simply due

* These observations were independently made in England about the same time (1793); the one by Fowler, and the other by Professor Robison, of Edinburgh.

to the passage of the electricity so developed along the nerves and muscles.*

The first analogy which Volta produced in support of his *theory of contact* was derived from the well-known experiment of Sulzer, which we have just described in these pages. From that it is seen that if two pieces of dissimilar metal, such as lead and silver, be placed one above, and the other below, the tongue, no particular effect will be perceived so long as they are not in contact with each other; but if their outer edges be brought together, a peculiar taste will be felt. If the metals be applied in one order, the taste will be acidulous. If the order be inverted, it will be alkaline. Now, if the tongue be applied to the conductor of a common electrical machine, an acidulous, or alkaline, taste will be perceived, according as the conductor is electrified positively, or negatively. Volta contended, therefore, that the identity of the cause should be inferred from the identity of the effects; that, as positive electricity produced an acid savour, and negative electricity an alkaline, on the conductor of

* Volta first broached his contact theory in two letters, in French, to Cavallo, dated September 13 and October 25, 1792. See *Phil. Trans.*, 1793, pp. 10–44; also chaps. x. and xiii. vol. i. of Robertson's *Mémoires Récréatifs*, Paris, 1840, for much interesting information on the early history of galvanism. Robertson was a celebrated aeronaut, a friend of Volta, and one of the founders of the Galvanic Society of Paris early in the present century. Dubois Reymond, in his *Untersuchungen über thierische Elektricität*, Berlin, 1848, gives a good account of this celebrated dispute, from a physiologist's point of view. See pp. 3–19 of the English translation, edited by Dr. Bence Jones, London, 1852.

the machine, so the same effects on the organs of taste produced by the metals ought to be ascribed to the same cause.

In August 1796, Volta arranged an experiment which, by eliminating the physiological element, afforded, as he thought, a direct and unequivocal proof of the correctness of his hypothesis. He took two discs, one of copper and the other of zinc, and, by means of their insulating handles, carefully brought them into contact and suddenly separated them without friction; then, on presenting them to a delicate condensing electroscope, the usual indications of electricity were obtained, the zinc being found to be feebly charged with positive, and the copper with negative, electricity.

Of the numerous philosophers in every part of Europe who took part in the discussions, and varied and repeated the experiments connected with these questions, one to whom attention is more especially due was Fabroni, who, in the year 1792, communicated his researches to the Florentine Academy. In this paper is found the first suggestion of the chemical origin of galvanic electricity.

Fabroni supposed that, in the experiments of Galvani and Volta, a chemical change was made by the contact of one of the metals with the liquid matter always found on the parts of the animal body; and that the immediate cause of the convulsions was not, as supposed by Galvani, due to animal electricity, nor, as assumed by Volta, to a current of electricity emanating from the surface of contact of the two metals, but to the decomposition of the fluid upon the animal substance, and the transition of oxygen from a state of combination with it to combination with the metal. The electricity produced in the experiments Fabroni ascribed entirely to these chemical changes, it being then known that chemical processes were generally attended with sensible signs of electricity.*

Galvani's theory was soon rejected on all hands, but a bitter war raged for a long time between the partisans of the contact theory of Volta and of the chemical theory of Fabroni. Now, however, it is generally conceded that both contact between dissimilar substances and chemical action are necessary to produce the effect. "Perhaps," says Fleeming Jenkin, in his excellent little text-book, "it is strictly accurate to say that difference of potential is produced by contact, and that the current which is maintained by it is produced by chemical action." †

In pursuing his inquiries on galvanic electricity, Volta felt the necessity of collecting it in much greater quantities than could be obtained from the combination of a single pair of copper and zinc plates as above described, and he, therefore, sought for some

* *Journal de Physique*, xlix. p. 348.

† This theory was, we believe, first propounded in England by Sir Humphry Davy in 1806. See Lardner's *Electricity, Magnetism, and Meteorology*, vol. i. p. 164.

means by which he could combine, and, as it were, superpose two or more currents, and thus multiply the effect. With this object he conceived the idea of placing alternately, one over the other, an equal number of discs of copper and zinc; but he found that the effect produced was no greater than that of a single pair, and for reasons which he was not slow to perceive. With such an arrangement as that described, there would proceed, according to Volta's own theory, from the first surface a negative downward and a positive upward current, from the second a positive downward and a negative upward, and from the third a negative downward and a positive upward, and so on. The downward currents would be alternately positive and negative, and the same would be the case with the upward currents; and since the surfaces of contact were equal, all the *intermediate* currents would neutralise each other, and the effect of the pile would simply be that of the two extreme discs.

Volta, therefore, saw the necessity of adopting some expedient by which all the currents in the same direction should be of the same kind, and, if this could be accomplished, it was easy to see that the resulting currents, negative at the bottom and positive at the top, would be as many times more intense as there were surfaces of contact. To effect this it was necessary to destroy the galvanic action at all those surfaces from which descending positive and ascending negative currents would proceed; but while this was being done it was also essential that the progress of the descending negative and ascending positive currents should still be uninterrupted. The interposition of any substance, which would have no sensible galvanic action on either of the metals between each disc of copper and the disc of zinc immediately below it, would attain one of these ends, but in order to allow the free progress of the remaining currents in each direction, such substance should be a sufficiently good conductor of electricity. Volta selected, as the fittest means of fulfilling these conditions, discs of wet cloth, which would give rise to no galvanic action, while their moisture would endow them with sufficient conducting power.

Although the principle of the pile was thus evolved as early as the middle of 1796, Volta does not appear to have actually constructed the instrument with which his name has become so imperishably associated until some three years later, and it was not until the 20th March, 1800, that he published a description of it in a letter to Sir Joseph Banks, President of the Royal Society. In this letter he thus writes:—"I took some dozens of discs of copper, brass, or better, of silver, one inch in diameter (coins for instance), and an equal number of plates of tin, or, what is much better, of zinc. I prepared also a sufficient number of discs of card-board, leather, or some other spongy matter capable of imbibing and retaining water, or, what is much better, brine. I placed on a table a disc of silver, and on it a disc of zinc, then one of the moist discs; then another disc of silver, followed by one of zinc, and one of card-board. I continued to form of these several stages a column as high as could sustain itself without falling."

Fig. 3.

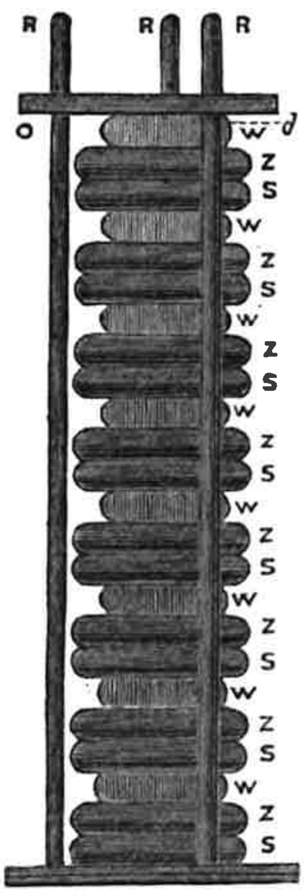


Fig. 3 represents one of the earliest forms of the pile. It consists of discs of silver, S, zinc, Z, and some bibulous substance soaked in brine, W. The rods R, R, R, are of glass, or baked wood, and with the piece O, which slides freely up and down, serve to keep the discs in position.

The invention of the pile had been scarcely more than hinted at, when the compound nature of water was discovered by its means. The first four pages only of the letter of Volta to Sir Joseph Banks were despatched on the 20th of March, 1800; and as these were not produced in public till the receipt of the remainder, the letter was not read at the Royal Society, or published, until the 26th of June following. This first portion, in which was described, generally, the formation of the pile, was, however, shown in the latter end of April to some scientific men, and, among others, to Sir Anthony (then Mr.) Carlisle, who was engaged at the time in certain physiological inquiries. Mr. W. Nicholson, the conductor of the scientific journal known as *Nicholson's Journal*, and Carlisle constructed a pile of seventeen silver half-crown pieces alternated with equal discs of copper and cloth soaked in a weak solution of common salt, with which, on the 30th of April, they commenced their experiments.

It happened that a drop of water was used to make good the contact of the conducting wire with a plate to which the electricity was to be transmitted; Carlisle observed a disengagement of gas in this water, and Nicholson recognised the odour of hydrogen proceeding from it. In order to observe this effect with more advantage, a small glass tube, open at both ends, was stopped at one end by a cork, and being then filled with water was similarly stopped at the other end. Through both corks pieces of brass wire were inserted, the points of which were adjusted at a distance of an inch and three-quarters asunder in the water. When these wires were put in communication with the opposite ends of the pile, bubbles of gas were evolved from the point of the negative wire, and the end of the positive wire became tarnished. The gas evolved appeared on examination to be hydrogen, and the tarnish was found to proceed from the oxydation of the positive wire. Thus was inaugurated on the

2nd of May, 1800, a new line of research, the limits of which, even now, it is impossible to foresee.*

Nicholson observed that the same process of the decomposition of water was carried on in the body of the pile, as between the two ends of the wire, the side of the zinc next the fluid being covered with oxide in two, or three, days, and the apparatus then ceasing to act. He also observed that the common salt, which had been dissolved in the water, was precipitated, for, gradually, an efflorescence of soda appeared round the margin of the discs.

Nicholson made the further important observation that, by employing discs of considerably more extensive surface, no greater effect was produced in the decomposition of water, or in the strength of the shock; whence he concluded that the repetition of the series is of more consequence to these actions than the enlargement of the surface.†

Cruickshank, of Woolwich, confirmed the observations of Nicholson respecting the appearance of sparks and the decomposition of water. This last pheno-

* Nicholson's *Journal of Natural Philosophy*, for 1800, vol. iv. p. 179. For the composition and decomposition of water by the electric spark, see Lord Brougham's paper in the *Mechanics' Magazine*, for November 9, 1839.

† In some experiments on the combustion of metals, Fourcroy, Thénard, and Vauquelin made the same observation in connection with the trough form of the pile. They found that the energy of the shock and the power of decomposition were not increased by the size of the plates, but by the number of the repetitions; while the same extent of surface, arranged in the form of a few large plates, readily consumed metallic leaves.—*Annales de Chimie*, xxxix. 103.

menon he varied in different ways. By employing silver terminals, or *electrodes*, and passing the current through water tinged with litmus, he found that the wire connected with the *zinc* end of the pile imparted a red tinge to the fluid contiguous to it; and that, by using water tinged with Brazil wood, the wire connected with the *silver* end of the pile produced a deeper shade of colour in the surrounding fluid; whence it appeared that an acid was formed in the former, and an alkali in the latter, case.

He next tried the effects of the wires on solutions of acetate of lead, sulphate of copper, and nitrate of silver, with the result that, in each case, the metallic base was deposited at the negative, and the acid at the positive pole. In the latter case he observes, "the metal shot into fine needles, like crystals articulated, or jointed, to each other, as in the *Arbor Dianæ*." Muriate of ammonia and nitrate of magnesia were next decomposed, the acid, as before, going to the positive, and the alkali to the negative, pole.

These experiments were made as early as June 1800; and in the September following, Cruickshank published a second memoir, in which he directed his attention to the nature of the gases emitted at the electrodes; to the effects of different kinds of electrodes; and to the influence of the fluid medium. The following are the most important of his conclusions:—

(1). From the wire connected with the silver, or copper, end of the pile, whatever be its composition, if it terminate in water, the gas evolved is, chiefly, hydrogen; if it terminate in a metallic solution, the metal is reduced and is deposited upon the wire.

(2). When the wire connected with the zinc end is composed of a non-oxydable metal, nearly pure oxygen is set free; when of an oxydable metal, it is partly oxydated, and partly dissolved, and only a small quantity of oxygen is set free.

(3). When fluids contain no oxygen they appear to be incapable of transmitting the voltaic current; while, on the contrary, it would seem that it may be transmitted by every one which contains this element.*

These views were confirmed by some experiments that were performed, about the same time, by Colonel Haldane. He found that the pile ceased to act when immersed in water, or when placed in the vacuum of an air-pump; that it acted more powerfully in oxygen gas than when confined in an equal bulk of atmospheric air,† and that azote had the same effect as a vacuum. These circumstances led him to conceive that its action depended essentially upon the consumption of oxygen, which it derives from the atmosphere. Haldane also made some experiments on the

* Nicholson's *Journal*, iv. pp. 187 and 254.

† Biot and Cuvier observed the converse of this. When the pile was enclosed in a limited quantity of air, they found that, after some time, the air was sensibly deoxydated.—*Annales de Chimie*, xxxix. 242.

series of metals which are the best adapted for producing the voltaic effects, and the relative power which they possess in this respect.*

While these investigations were proceeding, Ritter, afterwards so distinguished for his experimental researches, but then young and unknown, made various experiments at Jena on the effects of the pile; and, apparently without knowing what had been done in England, discovered its property of decomposing water and saline compounds, and of collecting oxygen and the acids at the positive, and hydrogen and the bases at the negative, pole. He also showed that the decomposing power in the case of water could be transmitted through sulphuric acid, the oxygen being evolved from a portion of water on one side of the acid, while the hydrogen was produced from another separate portion on the other side of it.†

When the chemical powers of the pile became known in England, Sir Humphry (then Mr.) Davy was commencing those labours in chemical science which subsequently surrounded his name with so much lustre, and have left traces of his genius in the history of scientific discovery, which must remain as long as the knowledge of the laws of nature is valued by mankind. The circumstance attending the decompositions effected between the poles of the pile which caused the greatest surprise was the production of one element of the compound at one pole, and the

* Nicholson's *Journal*, iv. pp. 247, 313. † Ibid., iv. 511.

other element at the other pole, without any discoverable transfer of either of the disengaged elements between the wires. If the decomposition was conceived to take place at the positive wire, the constituent appearing at the negative wire must be presumed to travel through the fluid in the separated state from the positive to the negative point; and if it was conceived to take place at the negative wire, a similar transfer must be imagined in the opposite direction.

Thus, if water be decomposed, and the decomposition be conceived to take place at the positive wire where the oxygen is visibly evolved, the hydrogen from which that oxygen is separated must be supposed to travel through the water to the negative wire, and only to become visible when it meets the point of that wire; and if, on the other hand, the decomposition be imagined to take place at the negative wire where the hydrogen is visibly evolved, the oxygen must be supposed to pass invisibly through the water to the point of the positive wire, and there become visible. But what appeared still more unaccountable was, that in the experiment of Ritter it would seem that one, or other, of the elements of the water must have passed through the intervening sulphuric acid. So impossible did such an invisible transfer appear to Ritter, that at that time he regarded his experiment as proving that one portion of the water acted on was wholly converted into oxygen, and the other portion into hydrogen.

This point was the first to attract the attention of Davy,* and it occurred to him to try if decomposition could be produced in quantities of water contained in separate vessels united by a conducting substance, placing the positive wire in one vessel and the negative in the other. For this purpose, the positive and negative wires were immersed in two separate glasses of pure water. So long as the glasses remained unconnected, no effect was produced; but when Davy put a finger of the right hand in one glass, and of the left hand in the other, decomposition was immediately manifested. The same experiment was afterwards repeated, making the communication between the two glasses by a chain of three persons. If any substance passed between the wires in these cases, it must have been transmitted through the bodies of the persons forming the line of communication between the glasses.

The use of the living animal body as a line of communication being inconvenient where experiments of long continuance were desired, Davy substituted fresh muscular fibre, the conducting power of which, though inferior to that of the living animal, was sufficient. When the two glasses were connected by this substance, decomposition went on as before, but more slowly.

To ascertain whether metallic communication

* In our account of Davy's researches we follow mainly Lardner's *Electricity, Magnetism, and Meteorology*, vol. i. pp. 119–29, which we have carefully collated with Davy's own memoir in the *Phil. Trans.*, for 1801.

between the liquid decomposed and the pile was essential, he now placed lines of muscular fibre between the ends of the pile and the glasses of water respectively, and at the same time connected the two glasses with each other by means of a metallic wire. He was surprised to find oxygen evolved in the *negative*, and hydrogen in the *positive*, glass, contrary to what had occurred when the pile was connected with the glasses by wires. In none of these cases did he observe the disengagement of gas, either from the muscular fibre, or from the living hand immersed in the water.

In October 1800, after many experiments on the chemical effects of the pile, Davy commenced an investigation of the relation which its power had to the chemical action of the liquid conductor on the more oxydable of its metallic elements. He showed that at common temperatures zinc connected with silver suffers no oxydation in water which is well purged of air and free from acids; and that, with such water as a liquid conductor, the pile is incapable of evolving any quantity of electricity which can be rendered sensible, either by the shock, or by the decomposition of water; but that, if the water hold in combination oxygen or acid, then oxydation of the zinc takes place, and electricity is sensibly evolved. In fine, he concluded that the power of the pile appeared to be, in great measure, proportional to the power of the liquid between the plates to oxydate the zinc.

To ascertain whether a liquid solution, capable of conducting the electric current between the positive and negative wires of a voltaic pile, but not capable of producing any chemical action on its metallic elements, would, when used between its plates, evolve electricity, Davy constructed a pile in which the liquid was a solution of sulphuret of strontia.* Twenty-five pairs of silver and zinc plates, alternated with cloths moistened in this solution, produced no sensible action, though the moment the sides of the pile were moistened with nitrous acid, the ends gave shocks as powerful as those of a similar pile constructed in the usual manner.

The inventor of the pile maintained that, among the metals, those which held the extreme places in the scale of electromotive power were silver and zinc; and that, consequently, these metals, paired in a pile, would be more powerful, *cæteris paribus*, than any other. But as he had shown that pure charcoal was a good conductor of the electric current, and that the electromotive virtue seemed also to depend on the different conducting powers of the metallic elements, it was consistent with analogy that charcoal, combined with another substance of different conducting power, would produce voltaic action. Dr. Wells † was

* When the current from an active pile was transmitted through this liquid, the shock was as sensible as if the communication had been made through water.

† *Phil. Trans.*, 1795.

the first to demonstrate this by showing that a combination of charcoal and zinc produced sensible convulsions in the frog; and, subsequently, Davy constructed a pile, consisting of a series of eight glasses, with small pieces of well-burned charcoal and zinc, using a solution of red sulphate of iron as the liquid conductor. This series gave sensible shocks, and rapidly decomposed water. Compared with an equal and similar series of silver and zinc, its effects were much stronger.

In considering the various arrangements and combinations in which voltaic action had been manifested, Davy observed, as a common character, that one of the two metallic elements was oxydated, and the other not. Did the production of the electric current, then, depend merely on the presence of two metallic surfaces, one undergoing oxydation, separated by a conductor of electricity? and, if so, might not a voltaic arrangement be made by one metal only, if its opposite surfaces were placed in contact with two different liquids, one of which would oxydate it, and the other transmit electricity without producing oxydation? To reduce these questions to the test of experiment with a single metallic plate would have been easy; but in constructing a series, or pile, the two liquids, the oxydating and the non-oxydating, must be in contact, and subject to intermixture. To overcome this difficulty, different expedients were resorted to, with more or less success; but the most convenient and effectual method of attaining the desired end was that suggested to Davy by Count Rumford.

Let an oblong trough be formed as a substitute for the pile; and let grooves be made in it such as to allow of the insertion of a number of plates, by which the trough may be divided into a series of water-tight cells. Let plates of the metal of which the apparatus is to be constructed be made to fit these grooves; and let as many plates of glass, or other non-conducting material, of the same form and magnitude, be provided. Let the metallic plates be inserted in alternate grooves of the trough, and the glass plates in the intermediate grooves, so as to divide the trough into a succession of cells, each having on one side metal, and on the other, glass. Let the alternate cells be filled with the oxydating liquid, and the intermediate cells with the liquid which conducts without oxydating. Let slips of moistened cloth be hung over the edge of each of the glass plates, so that their ends shall dip into the liquids in the adjacent cells. This cloth, or rather the liquid it imbibes, will conduct the electric current from cell to cell, without permitting the intermixture of the liquids.

In the first arrangements made on this principle, the most oxydable metals, such as zinc, tin, and some others, were tried. The oxydating liquid was dilute nitric acid, and the other plain water. In a combination consisting of twenty such pairs sensible but weak effects were produced on the organs of sense, and water was decomposed slowly by wires from the extremities. The wire from the end towards which the oxydating surfaces were directed evolved hydrogen, and the other oxygen.

To determine whether the evolution of the electric current was dependent on the production of *oxydation* only, or would attend *other chemical effects* producible by the action of substances in solution upon metal, the oxydating liquid was now replaced by solutions of the sulphurets, and metallic plates were selected on which these solutions would exert a chemical action. Silver, copper, and lead were tried in this way, solution of sulphuret of potash and pure water being the liquids employed. A series of eight metallic plates produced sensible effects. Copper was the most active of the metals tried, and lead the least so. In these cases, the terminal wires effected, in the usual manner, the decomposition of water, the wire from which hydrogen was evolved being that which was connected with the end of the series to which the surface of the metal not chemically acted on was presented.

It will be observed that in this case the direction of the electric current relatively to the surfaces of the metallic plates was the reverse of the former, for when oxydation was produced, the oxydating sides of the plates looked towards the *negative* end of the series. Comparing these two effects, Davy was led by analogy to suspect that if one set of cells was filled with an oxydating solution, while the other set was filled with a solution of sulphuret, or any other which would produce a like chemical action, the combined effects of the currents proceeding from the two distinct chemical processes would be obtained. This was accordingly tried, and the results were as foreseen. A series, consisting of three plates of copper, or silver, arranged in this way, produced sensible effects; and twelve or thirteen decomposed water rapidly.

As it appeared from former experiments that charcoal possessed, as a voltaic element, the same properties as the metals, the next step in this course of experiments was, naturally, to try whether a voltaic arrangement could not be constructed without any metallic element, by substituting charcoal for the metallic plates in the series above described. This was accomplished by means of an arrangement in the form of the *couronne des tasses*. Pieces of charcoal, made from very dense wood, were formed into arcs; and the liquids were arranged in alternate glasses The charcoal arcs were placed so as to have one end immersed in each liquid, the intermediate glasses being connected by slips of bibulous paper. When the liquids were dilute acid and water, a series consisting of twenty pieces of charcoal gave sensible shocks, and decomposed water. This arrangement also acted, and with increased effect, when the liquids were sulphuric acid and solution of sulphuret of potash.

Soon after the discovery of the pile, Dr. Wollaston turned his attention to the subject, and in the *Philosophical Transactions*, for 1801 (p. 427), records his observations, which are marked by his accustomed sagacity and penetration. He observed, like Davy, that the energy of the pile seemed to be in proportion to the tendency which one of the metals had to be acted upon by the interposed fluid. If, he says, a plate of zinc and a plate of silver be immersed in dilute sulphuric acid, and kept asunder, the silver is not affected, but the zinc begins to decompose the water, and to evolve hydrogen. If the plates be now placed in contact, the silver discharges hydrogen, and the zinc continues, as before, to be dissolved. From these and other analogous facts, he concludes, that whenever a metal is dissolved by an acid, electricity is disengaged.*

Davy's experiments have shown that in all voltaic combinations only one of the metallic elements is attacked by the liquid; but this condition, although desirable, is not essential to the production of electricity. It is sufficient if the chemical action of the liquid upon one of the metals be greater than upon the other; for then the two metals may be considered to give rise to two separate currents, of which the one proceeding from the metal most attacked is the stronger, the current perceived being the difference

* He extends this principle to the action of the electrical machine, which, he conceives, has its power increased by applying to the cushion an amalgam, into the composition of which enters an easily oxydable metal. Clearly the zinc used by Wollaston was very impure, for, as we now know, *pure* zinc is unaffected so long as it is not joined to the silver.

between the two. If the currents were absolutely equal, a condition, however, practically impossible to realise, we must assume that no electrical effects would be produced.

As a voltaic current, then, is produced whenever two metals are placed in metallic contact in a liquid which acts more powerfully upon one than upon the other, it is easy to see that there must be a great choice in the mode of producing such currents. The following is a list of the principal metals, arranged in what is called an *electromotive series*, and from which any two being taken and placed in contact in, say, dilute sulphuric acid, that metal highest in the list is the one that will suffer oxydation. This is called the *electropositive* metal in contradistinction to its fellow, which is denominated *electronegative*:—

Zinc	Nickel	Silver
Cadmium	Bismuth	Gold
Tin	Antimony	Platinum
Lead	Copper	Graphite
Iron	Mercury	

It will be seen that the electrical deportment of any metal depends upon the metal with which it is associated. Iron, for instance, is electronegative towards zinc, but electropositive towards copper; while copper, in its turn, is electronegative towards iron and zinc, but electropositive towards silver, platinum, or graphite.

The force resulting from the contact of two metals, in a liquid is called the *electromotive force*, and, as may be supposed, is greater in proportion to the distance of the two metals from one another in the above list. Thus, the electromotive force between zinc and platinum is greater than that between zinc and iron, or between zinc and copper. Indeed the law, as established by Poggendorff, is, that the electromotive force between any two metals is equal to the sum of the electromotive forces between all the intervening metals.*

The electromotive force is influenced by the condition of the metal; rolled zinc, for example, is negative towards cast zinc. It also depends on the *degree of concentration* of the liquid; thus, in dilute nitric acid zinc is positive towards tin, and mercury positive towards lead; while in concentrated nitric acid, the reverse is the case, mercury and zinc being respectively electronegative towards lead and tin.

The *nature* of the liquid is also of influence, as is seen from the change in the relative position of the metals in the following lists:—

Caustic Potass.	Sulphide of Potassium.
Zinc	Zinc
Tin	Copper
Cadmium	Cadmium
Antimony	Tin
Lead	Silver
Bismuth	Antimony
Iron	Lead
Copper	Bismuth
Nickel	Nickel
Silver	Iron

In short, anything that affects the energy of the chemical action on the positive plate, or the resultant

* Ganot's *Elementary Treatise on Physics*, London, 1881, p. 707.

actions on the two plates, affects to a like degree the electromotive force of the combination.

Of the theories proposed to explain chemical decomposition by the voltaic apparatus, that of Grotthus was the earliest and most plausible. To simplify the view of this theory, we shall take as an example of its application the decomposition of water. Each molecule of water being composed of a molecule of oxygen and a molecule of hydrogen, their natural electricities are in equilibrium when not exposed to any disturbing force, each possessing equal quantities of the positive and negative fluids. The electricity of the positive wire acting on the natural electricities of the contiguous molecule of water, attracts the negative and repels the positive fluid. It is further assumed in this theory, that oxygen has a natural attraction for negative, and hydrogen for positive electricity; therefore the positive wire in attracting the negative fluid of the contiguous molecule of water, and repelling its positive fluid, attracts its constituent molecule of oxygen, and repels its molecule of hydrogen. The particle of water, therefore, places itself with its oxygen next the positive wire, and its hydrogen on the opposite side.

The positive electricity of the first particle of water thus accumulated on its hydrogen molecule, produces the same action on the succeeding molecule of water as the wire did upon the first molecule; and a similar arrangement of the second molecule of water is the result. This second molecule acts in like manner on the third, and so on. All the particles of water between the positive and negative wires thus assume a polar arrangement, and have their natural electricities decomposed; the negative poles and oxygen molecules looking towards the positive wire, and the positive poles and hydrogen molecules looking towards the negative wire. The electro-positive wire now separates the oxygen molecule of the contiguous particle of water from its hydrogen molecule, neutralises its negative electricity, and either dismisses it (the oxygen) in the gaseous form, or combines with it, according to the degree of its affinity for the metal of the wire. The hydrogen molecule thus liberated effects in like manner the decomposition of the second particle of water, combining with its oxygen, and thus again forming water, and liberating hydrogen. The latter acts in the same manner on the next particle of water, and so on..

Thus, a series of decompositions and recompositions is supposed to be carried on through the fluid, until the process reaches the particle of water contiguous to the negative wire. The molecule of hydrogen there disengaged gives up its positive electricity (by which an equal portion of negative electricity proceeding from the wire is neutralised), and then escapes in the gaseous form. It is equally compatible with this theory to suppose the series of decompositions and recompositions to commence at the negative and terminate at the positive wire, or to commence simultaneously at both, and terminate at an intermediate point by the union of the last molecule of oxygen disengaged in the one series with the last molecule of hydrogen disengaged in the other.

Grotthus illustrated this ingenious hypothesis by comparing the supposed phenomena with the mechanical effects produced when a number of elastic balls —ivory balls for example—are suspended, so that their centres shall be in the same straight line, and their surfaces mutually touch, and either of the extreme balls of the series is raised and let fall against the adjacent one. The effect is propagated through the series, and although action and reaction are suffered by each ball, and each is instrumental in transmitting the effect, no visible change takes place in any ball except the last, which alone recoils in consequence of the impact.*

The investigations of which the pile became the instrument now began to assume an importance which rendered it necessary to give it greater power, either by increasing its height, or by enlarging the surfaces of the plates. In either case, inconveniences were encountered which imposed a practical limit to the increase of its power. When the number, or magnitude, of the discs was considerable, the incumbent pressure discharged the liquid from the intermediate

* Lardner's *Electricity, Magnetism, and Meteorology*, vol. i. pp. 135–37; also *Phil. Mag.*, for 1806, vol. xxv. p. 334.

card-board, so that the energy of the pile gradually diminished from the first, and ultimately ceased altogether.* It had then to be taken to pieces, the metals cleaned, and the card-board re-soaked in the solution, every time it was required.

Volta himself, seeing these inconveniences, proposed an arrangement which he called *la couronne des tasses*, and which consisted of a circle, or row, of small cups containing a solution of salt. In each cup were placed a small plate, or bar, of zinc, and another of silver, not touching, the zinc of the first cup being connected metallically to the silver of the second, the zinc of this to the silver of the third, and so on. The silver rod of the first cup and the zinc rod of the last formed the poles of the apparatus. Twenty such combinations were able to decompose water, and thirty gave a distinct shock to the moistened hands.

A still more convenient form was that known as *Cruickshank's Battery*, which was introduced in 1800, within a few weeks of the announcement of Volta's discovery. It consisted of a number of pairs of zinc and copper plates soldered together and cemented into grooves in an oblong trough of wood, the spaces between each being filled with the exciting liquid. On this plan was constructed the great battery of

* To prevent this, Ritter turned up the edges of the lower discs so as to retain the liquid. His piles were thus able to preserve their powers for a fortnight. See the *Phil. Mag.*, vol. xxiii. p. 51.

600 pairs given to the Polytechnic School of Paris by Napoleon I., and with which Gay Lussac and Thénard made their experiments in 1808.*

Dr. Babington's arrangement was a great improvement upon this form. The plates of copper and zinc, four inches square, were united in pairs by soldering at one point only. The trough in which they were immersed was made of porcelain, and divided into ten, or twelve, equal compartments. The plates were attached to a strip of wood, well baked and varnished, and so arranged that each pair should enclose a partition between them when let down into the trough. By this means the whole set could be lifted at once into, or out of, the little cells, and thus, while the exciting fluid remained in the trough, the action of the battery could be suspended at pleasure, and the plates, when corroded, could be easily replaced.

A battery of 2000 pairs, with a surface of 128,000 square inches, was made on this plan for the Royal

* An amusing story anent this battery is told in Dr. Paris's *Life of Sir Humphry Davy*:—When Napoleon heard of the decomposition of the alkalies by an English philosopher, he angrily questioned the *savans* of the Paris Institute why the discovery had not been made in France. The excuse alleged was the want of a battery of sufficient power. He immediately commanded one to be made, and when completed he went to see it. With his usual impetuosity, the Emperor seized the terminal wires, and, before he could be checked by the attendant, applied them to his tongue. His Imperial Majesty was rendered nearly senseless by the shock, and as soon as he recovered from its effects he walked out of the laboratory with as much composure as he could assume, not requiring further experiments to test the power of the battery, nor did he ever afterwards allude to the subject.—Vol. ii. p. 24.

Institution of London, with which Davy and Faraday performed those long-continued and brilliant series of experiments, for which the Royal Institution will ever be celebrated.* Children in 1809, Wollaston in 1815, Berzelius in 1818, and others, proposed various modifications of the trough battery, all having for their object increase of power, with more cleanliness and less waste.

But all these arrangements of two metals in one fluid, constituting what are now called *single-fluid batteries*, had one great defect: their power, variable from the first, rapidly declined, and, sooner or later, ceased altogether.

This defect was due to two causes, first, the decrease in the chemical action owing to the neutralisation of the sulphuric acid by its combination with the zinc;

* "When the whole series was put into action, platina, quartz, sapphire, magnesia, and lime, were all rapidly fused; while diamond, charcoal, and plumbago, in small portions disappeared, and seemed to be completely evaporated. A singularly beautiful effect was produced by placing pieces of charcoal at the two ends of the wires; when they were brought within the thirtieth, or fortieth, part of an inch of each other, a bright spark was produced, above half the volume of the charcoal, which was rather more than an inch long, and the points became ignited to whiteness. By withdrawing them from each other, a constant discharge took place through the heated air, in a space equal to at least four inches, producing a most brilliant arc of light."—Bostock's *History of Galvanism*, p. 95. This refers to Davy's experiments of 1809, when he for the first time produced a *continuous* arc of light, but long before this the electric light, *as a spark*, had been obtained from charcoal points, as by Davy himself (Nicholson's *Journal*, Oct. 1800, p. 150), by Moyes (*Phil. Mag.*, vol. ix. p. 219), and by Robertson, to whom we referred on p. 187 (*Journal de Paris*, Mar. 12, 1802). See *The Electrician*, vol. xi. p. 162.

second, *polarisation* of the negative, or copper, plate, giving rise to *secondary currents*. These are currents which are produced in the battery in a contrary direction to the principal one, and which destroy it, either totally, or partially. In a couple of zinc, copper, and sulphuric acid diluted with water, for example, when the circuit is closed sulphate of zinc is formed which dissolves in the liquid, and at the same time hydrogen gas, in what is called its *nascent* state, is gradually deposited on the copper.

Now, it has been found that hydrogen deposited in this manner on metallic surfaces acts far more energetically than ordinary hydrogen. In virtue, therefore, of this increased action it gradually reduces some of the sulphate of zinc dispersed in the liquid, causing a layer of metallic zinc to be formed on the surface of the copper plate; hence, instead of having two different metals, copper and zinc, we have two metals becoming gradually less different, and, consequently, in the connecting wire there are two currents in opposite directions tending to become equal, and so to neutralise one another. When the copper plate is entirely covered with zinc the action of the couple ceases, for the condition essential to this action no longer exists, *viz.*, two dissimilar metals.

Becquerel, of Brussels, was the first to recognise these causes of the inconstancy of the voltaic battery, and, in 1829, he devised the first double-fluid arrangement, which, while it prevented polarisation, maintained the supply of acid around the positive plate, thus removing both sources of weakness at once.

It was composed of two small glass vessels, one of which contained concentrated nitric acid, and the other a solution of caustic potash, also concentrated. The two vessels communicated with each other by means of a bent glass tube, filled with fine clay, moistened with a solution of sea-salt. In the vessel which contained the alkali was immersed a plate of gold, and in the other a plate of platinum. By connecting the two through a galvanometer a constant and tolerably energetic current was perceived, resulting from the reaction of the acid on the sea-salt and potash.*

In 1830, Wach constructed double-fluid batteries on this plan, using animal bladders as the separating medium.

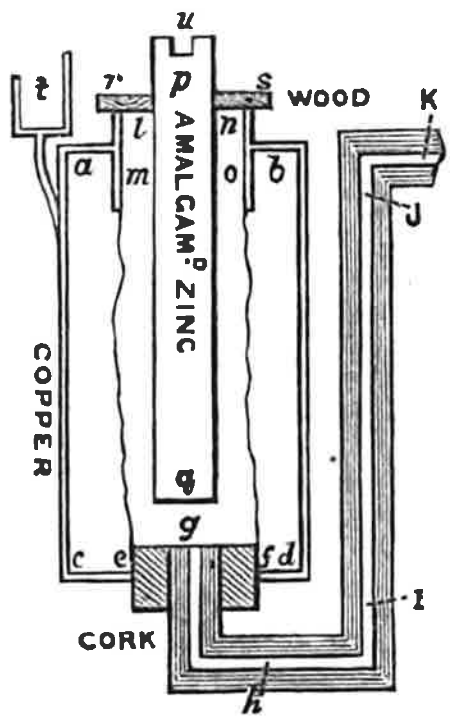
Professor Daniell, of King's College, London, is commonly supposed, in England at all events, to have been the first to make a double-fluid battery; but although he was not the first, as we see, his independent researches and his beautiful memoir on the subject, in the *Philosophical Transactions*, for 1836, were the means of bringing the improvement into general notice, and hence, no doubt, the popular belief.†

* *Comptes Rendus*, for 1837, No. 2.

† For much valuable information on this question, as between Daniell and Becquerel, see Sturgeon's *Annals of Electricity*, vol. ix. pp. 534–49. Zetzsche, at p. 45 of his *Geschichte der Elektrischen Telegraphie*, says that Dobereiner, Privy Councillor at Jena, had, as far back as 1821, constructed such a battery as the constant form of Daniell. See also Dove's *Über Elektricität*, Berlin, 1848, p. 24.

Double-fluid batteries are so simply constructed nowadays that our readers will probably be surprised to learn the pains that Daniell was at in contriving his. Fig. 4 represents a section of one of his original cells; *a*, *b*, *c*, *d*, is a cylinder of copper, six inches high and three and a half inches wide; it is open at the top *a*, *b*, but closed at the bottom, except for a collar *e*, *f*, one inch and a half wide, intended for the reception of a cork into which a glass syphon tube, *g*, *h*, *i*, *j*, *k*, is fitted water-tight. On the top *a*, *b*, a copper collar, corresponding with the one at the bottom, rests by two horizontal arms. Previously to fixing the cork, a membranous tube, formed of part of the gullet of an ox, is drawn through the lower collar *e*, *f*, and fastened with twine to the upper, *l*, *m*, *n*, *o*, and, when tightly fixed by the cork plug below, forms an internal cavity to the cell, communicating with the syphon tube, so that, when filled with any liquid to the level, *m*, *o*, any addition causes an overflow at the aperture *k*.

FIG. 4.



The objects which Daniell proposed to himself in constructing this cell were (1) the removal of the oxide of zinc as fast as formed, and (2) the absorption of the hydrogen evolved upon the copper, without the precipitation thereon of any substance that could impair its action.

The first he effected by suspending the zinc rod, which he took care to be amalgamated,* in the interior membranous cell, into which fresh acidulated water was allowed slowly to drop (from a funnel suspended over it, whose aperture was adjusted to this purpose), whilst the heavier solution of the oxide was withdrawn from the bottom at an equal rate by the syphon tube. The second object was attained by charging the exterior space surrounding the membrane, with a saturated solution of sulphate of copper. When the circuit was completed the current passed freely, no hydrogen was observed to collect on the negative plate, but, instead, a beautiful pink coating of pure copper was deposited upon it, and thus perpetually renewed its surface.

Although this cell was much more steady and permanent in its action than one of the ordinary single-

* The first mention of amalgamated zinc in voltaic arrangements occurs in Sir H. Davy's Bakerian lecture, for 1826, in which he simply remarked that "zinc in amalgamation with mercury is positive with respect to pure zinc" (*Phil. Trans.*, 1826, part iii.), without any allusion to the probable beneficial employment of it in the general construction of batteries. Kemp of Edinburgh was the first to employ amalgamated zinc and copper in the regular construction of batteries. See his paper in Jameson's *New Edinburgh Philosophical Journal*, for Dec. 1828.

fluid construction, it still showed a gradual but very slow decline, which Daniell traced to the weakening of the saline solution by the precipitation of its copper and consequent decline of its conducting power. To obviate this defect some crystals of sulphate of copper were suspended in muslin bags, which just dipped below the surface of the solution in the copper cylinder, and which, gradually dissolving as the precipitation proceeded, kept the solution in a state of saturation. This expedient fairly answered the purpose, and its author was delighted to find that "the current was now perfectly steady for six hours together."

Such, in brief, is the evolution of the far-famed Daniell cell. Its further development we need not pursue in these pages, as all the later forms, as well as many other kinds of double-fluid, or so-called *constant*, batteries, are to be found in all the text-books on electricity.

CHAPTER VIII.

TELEGRAPHS (CHEMICAL) BASED ON DYNAMIC ELECTRICITY.

"Awhile forbear,
* * * * *
Nor scorn man's efforts at a natural growth,
Which in some distant age may hope to find
Maturity, if not perfection."

Household Words, June 14, 1851.

1800–4.—*Salvá's Telegraph.*

It is generally supposed that Sömmerring was the first to employ the electricity of the pile for telegraphic purposes; but M. Saavedra * has shown that this honour belongs to his distinguished countryman, Don Francisco Salvá, whose name has already occurred in our pages (pp. 101–8).

At a meeting of the Academy of Sciences of Barcelona, held on the 14th May, 1800, Salvá read a paper, entitled *Galvanism and its application to Telegraphy*, in which, after an elaborate dissertation on the phenomena and theories of the new science, he proceeds to consider its application to telegraphy.

* *Tratado de Telegrafia*, Barcelona, 1880, vol. i. pp. 331–35. In our account of Salvá we translate, literally, from this excellent treatise.

He relates the experiments made for this purpose at his residence with line wires, some 310 metres long, stretched across the terrace and garden, and fastened at the ends to varnished glass insulators, and through which he distinctly perceived the convulsions of the frog, notwithstanding the distance. The fact that the contractions sometimes took place without closing the circuit led to the discovery, that, on account of the wire being uncovered, its extension permitted its taking electricity from the atmosphere, so as to act upon the frog. The conducting wires, adds Salvá, can act by means of galvanism alone, as he demonstrated by insulating his small line.* He expressed the conviction that he could obtain a telegraphic communication at a much greater distance.

The memoir does not enter into details as to this new telegraphic proposal, limiting itself to saying that it could be made by a process analogous to that described at the meeting of December 16, 1795,† with the advantages of greater durability and cheapness as compared with the old plan.

Salvá employed, as his motive power, the electricity produced by a great number of frogs.

This illustrious Spanish physician was therefore the first person who attempted to apply electricity dynamically for the purpose of telegraphing. "It is," says Saavedra, "not without reason I must con-

* These passages are obscure in the original.

† See p. 101, *ante*.

fess, notwithstanding my cosmopolitan opinions on scientific questions, that the Catalans hold Salvá to be the inventor of electric telegraphy. With documents as authentic as those which I have seen with my own eyes, in the very handwriting of this distinguished professor (which documents are at this present moment to be found in the library of the Academy of Sciences of Barcelona), it is impossible for any author to henceforth deny, even if others did precede Salvá in telegraphic experiments with static electricity, that no one preceded him in the application of the docile electro-dynamic fluid to distant communications."

On the 22nd February, 1804, when the invention of the voltaic pile had hardly begun to be known in Europe (for in that period there were no telegraphs or railroads), Don Francisco Salvá Campillo read before the Academy of Sciences at Barcelona another paper, called *The Second Treatise on Galvanism applied to Telegraphy*. He therein enumerates the difficulties which optic telegraphy presents in actual practice, and shows its inadequacy to the amount of work required, and its unproductiveness to the State, on account of the great expense attending its erection and maintenance. He relates, referring to two personal friends as witnesses, that Napoleon I., in the midst of a Session of the National Institute of Paris, declared that he had often received news by express sooner than by the optic telegraph, which, he says, is not to be wondered at, considering the fogs and other impediments peculiar to that system.

Salvá says in this paper, that when he read the previous one in 1800, he had not heard of the instrument invented by Volta, called *Volta's Column*, which is not strange, considering its so recent invention, and that it was not made public at all until the middle of the year 1800, when it was published in Nicholson's *Journal*. This, says Salvá, yields more fluid than the electric machine, and could be well applied to telegraphy, as the force can be obtained more simply and steadily than in the static form. He describes what had been done by scientific men towards improving its form; he observes that the force of the shock is in proportion to the number of pairs, but not to the extent of surface in contact, and relates the experiments made to demonstrate this; he proposes to avoid the excessive height of the pile (the well-known objection to which is the great weight of the upper discs pressing on those below), by forming a battery of several piles united; he complains of its being so difficult to clean, and concludes with his belief in the eventual obtainment of piles in a much greater state of perfection.

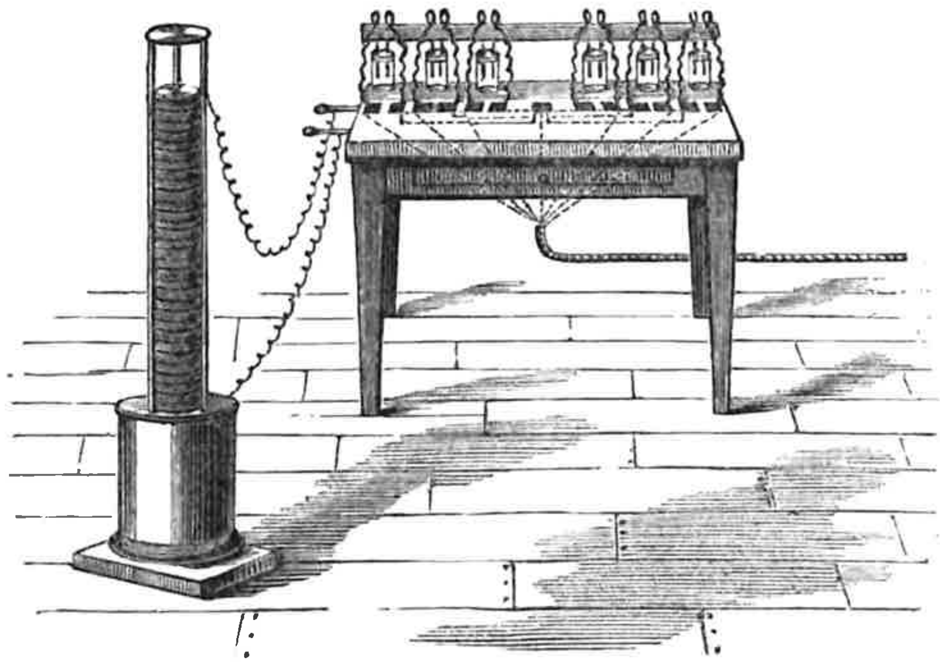
As to the means of indicating the signals, Salvá shows some hesitation, since, although he alludes to the contractions of frogs as adequate to the effect, he manifests an inclination for employing the decomposition of water.

For this last he gives sufficient explanation as to the system that could be adopted. It would suffice for the ends of each pair of wires to be inserted through a cork into a glass tube containing water. As the wire that communicates with the zinc plate of the pile is covered with bubbles of hydrogen gas, and the other is oxydated, these actions would economise, in the diversity of their effects, one half the conductors, since in applying the wires in a certain way to the poles of the voltaic pile, the letter A, for example, could be had, and, effecting the contact in a contrary way, the letter B could be indicated. Six wires would thus be enough for a telegraph, which would greatly reduce the expense and simplify the installation. After reading this paper the author proceeded to the experiments necessary towards perfectly understanding the above statements.

The apparatus constructed by Salvá on this occasion has not been preserved, but from the description of it in the memoir M. Saavedra thinks that it took a form similar to that shown in Fig. 5. On a table was arranged a number of flasks of water, one flask serving for two letters, or signals. Into each dipped two metallic rods, one of which was connected to a corresponding line wire, and the other to a return wire, of which there was only one, common to all. The different line wires and the return wire were similarly connected at the distant end.

When it was desired to transmit a signal, all the line wire rods at the sending end were removed from the flasks, or raised so as to be clear of the water, then one pole, say the positive, of a pile was touched to the return wire, and the other pole, the negative, to the wire corresponding to the letter desired to be signalled. Immediately this was done the water in the flask, into which the distant ends of the wires

FIG. 5.



Salvá's Telegraph.

dipped, began to be decomposed, bubbles of oxygen gas being given off at one rod, and bubbles of hydrogen at the other. By reversing the poles of the pile at the sending end the bubbles of oxygen and hydrogen changed places, thus making it possible for one wire to serve for two signals, for since the bubbles of hydrogen (being the more numerous) were taken to represent the signals, their evolution at the line wire might stand for the letter A, and at the return wire for B, and so on. When the communication was ended the rods were let down into the water, and the distant correspondent proceeded in the same manner to transmit his reply.

These notable and interesting memoirs, says Saavedra, have, ever since they were read, slept the sleep of the innocent on the shelves of the modest Scientific Academy of Catalonia; no one took the trouble to publish them at the time—a thing not strange in those days when their transcendent value was not appreciated, and when scientific journals were few in number, and little given to investigation; but it was unpardonable to neglect their publication subsequently, when the glorious realisation of public telegraphy excited general enthusiasm, and all the civilised nations made every effort to allege—through their numerous scientific and literary publications—the part each had taken in the great discovery. If, therefore, neither the author, nor any one else in Barcelona, or even in Spain, took the trouble to publish these trials of an electric telegraph, is it to be wondered at that foreign authors do not mention them, attributing to Sömmerring and to Coxe of 1809 and 1816 the first application of voltaic electricity to telegraphy? Is it strange that this should be the case when even the few Spanish writers who pay any attention to these matters, repeat the same words in chorus, as though the unfortunate country of Cervantes and Balmes were not also the birthplace of Blasco de Garay and of Salvá?

In this connection we would ask our readers to peruse again our account of Salvá's earlier experiments, which will be found at pp. 101–8, *ante*.

We join with M. Saavedra in the hope that his distinguished countryman will in future be better known and appreciated for his early and valuable contributions to the art of telegraphy.

1809–12.—*Sömmerring's Telegraph.*

Sömmerring's telegraph was based on the same principle as Salvá's, and was not very dissimilar in detail. There is an interesting account of this contrivance in the *Journal of the Society of Arts*,* for 1859, contributed by Dr. Hamel, of St. Petersburg. According to this indefatigable writer, the war between France and Austria, in 1809, gave rise to Sömmerring's discovery. On the 9th April in that year the Austrian troops crossed the river Inn, and on the 16th occupied Munich, whence King Maximilian had fled on hearing of their approach. The Emperor Napoleon, having speedy intelligence of this move by Chappe's sema-

* Vol. vii. pp. 595–99 and 605–10, *Historical Account of the Introduction of the Galvanic and Electro-Magnetic Telegraph into England*. Republished in pamphlet form, in Nov. 1859, by W. F. Cooke, with comments. See also *Der Elektrische Telegraph als Deutsche Erfindung S. T. Von Sömmerring's aus dessen Tagebüchern nachgewiesen*, 21 pp., published at Frankfort in 1863, by Sömmerring's only son.

phore, hastened away with some troops, and, so rapid and unexpected were his movements, that in less than a week the Austrians were obliged to retire, and on the 25th Maximilian re-entered his capital.

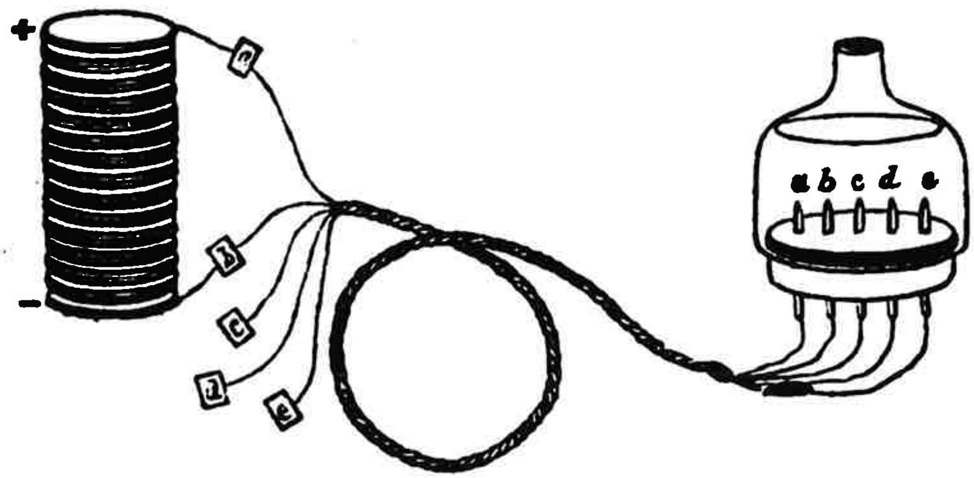
This event in which Chappe's semaphore played so important a part caused much attention to be directed to the subject of telegraphy, and on the 5th July following we find the Bavarian minister, Montgelas, requesting his friend, Dr. Sömmerring, to bring the subject before the Academy of Sciences (of Munich), of which he was a distinguished member.*

Sömmerring at once gave the matter his attention, and soon it occurred to him to try whether the visible evolution of gases from the decomposition of water by the voltaic current might not answer the purpose. He worked at this idea incessantly, and, before three days had elapsed, had constructed his first apparatus, shown in Fig. 6. He took five wires of silver, or copper, and, insulating each with a thick coating of sealing-wax, bound the whole up into a cable. These wires, at one end, terminated in five pins which penetrated a glass vessel containing acidulated water; and, at the other, were capable of being put in connection with the poles of a pile of fifteen pairs of zinc discs, and Brabant thalers, separated by felt soaked in hydrochloric acid. By touching any two of the wires to the poles of the pile he was able to produce, at their distant ends, a disengagement of gases, and

* Hamel, Cooke's reprint, pp. 5–7.

thereby indicate any of the five letters *a, b, c, d, e.* Having thus shown the feasibility of his project, he set himself to perfect his apparatus, and worked at it with such a will that by the 6th of August it was completed. He wrote in his diary on that day:—"I have tried the entirely finished apparatus which completely answers my expectations. It works quickly through

FIG. 6.



wires of 362 Prussian feet." Two days later he worked it through 1000 feet, and then through 2000 feet, the wire in each case being wound round a glass cylinder for greater compactness.*

As there is great diversity amongst writers on the telegraph not only as regards the date of this invention, but as to the number of wires used, and other details of its construction, we translate the following

* Hamel, pp. 7, 8. On the 4th February, 1812, he worked through 4000 feet, and on the 15th March following through as much as 10,000 feet.

description from the author's own paper, which was read before the Munich Academy of Sciences, on the 29th August, 1809, on which occasion the telegraph was exhibited in action:—

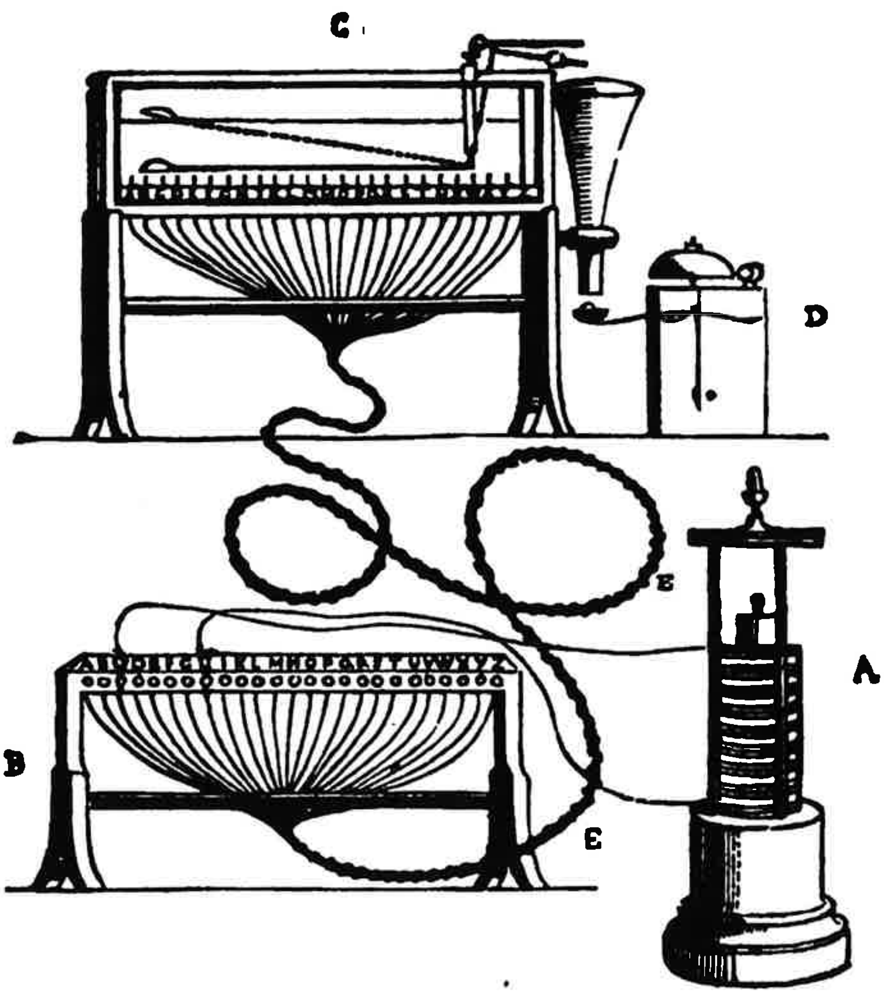
"In the bottom of a glass reservoir of water, 170 mm. long, 25 mm. broad, and 65 mm. high, of which C, in Fig. 7 is a sectional view, are thirty-five gold points, or pins, passing up through the bottom of the trough, and corresponding with the twenty-five letters of the German alphabet and the ten numerals. The thirty-five pins are each connected to as many insulated copper wires, E, E,* which, extending to the distant station, are there soldered to thirty-five brass terminals arranged on a wooden bar B. Through the front end of each of these terminals there is a small hole for the reception of brass pegs, one of which is attached to the wire coming from the positive pole, and the other to that from the negative pole of the voltaic pile A. Each of the thirty-five terminals corresponds, through its wire, with a pin in the distant reservoir, and is lettered accordingly.

"When thus arranged, the two pegs from the pile are taken, one in each hand, and, two terminals being selected, are pushed into the holes. The communication is now established and gas is evolved at the corresponding pins in the distant reservoir, hydrogen

* In September 1811, Sömmerring reduced the number of wires to twenty-seven, of which twenty-five were for the letters, one for the stop, and one for the note of interrogation, or repetition.—Hamel, p. 19.

at the pin in connection with the positive pole of the pile, and oxygen at the other. In this way every letter and numeral may be indicated at pleasure, and,

FIG. 7.



if the following rules be observed, one can communicate as much as, if not more than, is possible by the common (semaphore) telegraph.

"*First Rule.*—As the hydrogen gas evolved is greater in quantity than the oxygen, therefore those letters which the former gas represents are more easily distinguished than those of the latter, and must be so noted. For example, in the words containing *ak*, *ad*, *em*, *ie*, we indicate the letters *a*, *a*, *e*, *i*, by the hydrogen; *k*, *d*, *m*, *e*, on the other hand, by the oxygen.*

"*Second Rule.*—To telegraph two letters of the same name, we must use a unit, unless they are separated by the syllable. For example, the word *anna* may be telegraphed without the unit, as the syllable *an* is first indicated and then *na*. The word *nanni*, on the contrary, cannot be telegraphed without the use of the unit, because *na* is first telegraphed, and then comes *nn*, which cannot be indicated in the same vessel. It would, however, be possible to telegraph even three, or more, letters at the same time by increasing the number of wires from twenty-five to fifty, but this would very much augment the cost of construction and the care of attendance.

"*Third Rule.*—To indicate the conclusion of a word, the unit 1 must be used with the last single letter, being made to follow the letter. It must also be prefixed to the letter commencing a word when that letter follows a word of *two letters* only. For example: *Sie lebt* must be represented *Si*, *e*l, *le*, *bt*; and *Er lebt*

* This plan of sending the current down one wire and making it return by any other was employed by Cooke and Wheatstone in their five-needle telegraph of 1837. It proved to be a little complicated in the present instance, and Sömmerring subsequently adopted the practice of signalling only one letter at a time, the oxygen signal being neglected.

must be represented *Er*, 1*l*, *eb*, *t*l. Instead of using the unit, another signal may be introduced, the cross †, to indicate the separation of syllables.

"Suppose now the decomposing table is situated in one city, and the peg arrangement in another, connected by thirty-five wires. Then the operator, with his voltaic pile and pegs at one station, may communicate intelligence to the observer of the gas at the decomposing table of the other station.

"The metallic plates, or terminals, with which the wires are connected have conical-shaped holes in their ends; and the pegs attached to the two wires of the voltaic pile are likewise of a conical shape, so that, when they are put in the holes, there may be a close fit, preventing oxydation and ensuring a good contact, as it is well known that slight oxydation of the parts in contact will interrupt the communication. The peg arrangement might be so contrived as to use permanent keys, which for the thirty-five plates would require seventy pins. The first key might be for hydrogen A; the second key for oxygen A; the third key for hydrogen B; the fourth key for oxygen B, and so on.

"The preparation and management of the voltaic pile is so well known that little need be said, except that it should be of that durability as to last more than a month. It should not be of very broad surfaces, as I have proved that six of my usual pairs (each one consisting of a Brabant dollar, felt moistened with a saturated solution of common salt, and a disc of zinc, weighing 52 grains) would evolve more gas than five pairs of the great battery of our Academy.

"As to the cost of construction, this model, which I have had the honour to exhibit to the Royal Academy, cost 30 florins. One line, consisting of thirty-five wires, laid in glass, or earthen, pipes, each wire insulated with silk, and measuring 22,827 Prussian feet, or a German mile, might be made for less than 2000 florins, as appears from the cost of my short one."

On the 23rd August, 1810, Sömmerring perfected his apparatus by adding a contrivance D, Fig. 7, for attracting attention at the distant station. He made the gas, rising in small bubbles from two contiguous pins in the water, collect under a sort of inverted glass spoon at the end of a long lever, which, rising, made a second lever bent in the opposite direction on the same axle descend and throw off a little perforated leaden ball, resting lightly on it, and which, falling on an escapement, set the clockwork of an ordinary alarum, D, in motion. This arrangement, simple as it is, gave Sömmerring much trouble. He writes in his diary, "If the principal part of the telegraph gave me no trouble, and demanded no alteration, but was ready in a few days, this secondary object, the alarum, cost me a great deal of reflection, and many useless trials with wheelwork, till, at last, I hit upon this very simple arrangement." The existence of this alarum is not generally known, and, probably, because it is not represented on the two plates which accompanied the description of the telegraph in the Memoirs of the Munich Academy, which plates, as Dr. Hamel has shown, were engraved before the alarum was invented.*

Sömmerring's wires, which were of brass, or copper, were insulated with a coating of gum-lac, then wrapped round with silk thread, and united into a cable, which was also bound with thread, and covered with gum-lac, or with a ribbon, soaked in that material. The cable was then wound on reels. In practice there was no appreciable retardation in the action of the apparatus through the greatest length of wire, the evolution of the gas appearing to begin as quickly as if the current had only to traverse two feet. The only effect of distance (*i. e.*, resistance) was to reduce the quantity of the gases evolved in a given time.†

This telegraph, complex and unpractical as it was, was earnestly prosecuted by its author for several years, and received a large share of attention, princes, statesmen, and philosophers thronging to his lodgings to witness its performances. He had a complete apparatus connected up in his house, which worked through insulated wires carried round on the outside, and which he always took great delight in showing to

* Hamel, pp. 13, 14.

† In connection with this evolution Sömmerring noticed a curious phenomenon. Whenever the gases were evolved at two neighbouring gold points, as A, and B, the hydrogen bubbles always ascended vertically, but those of the oxygen inclined towards the hydrogen.

his visitors. At the suggestion of one of the most constant and intimate of these, Baron Schilling (of whom more hereafter), Sömmerring, on the 6th June, 1811, tried the action of his telegraph when the conducting wires were cut and the ends separated by an interval of water in wooden tubs. The result was that the signals appeared just as well as if the wires had not been cut, but they ceased as soon as the water in the two tubs was connected by a wire, the current then returning by this shorter road. On the two following days he performed with his friend some other experiments, first across a canal off the river Isar, and then along the river itself, in which he showed that the water and earth might be used instead of a return wire, an experiment which was probably suggested by the similar one of Aldini, in 1802, across the harbour of Calais.*

Besides his own apparatus, Sömmerring had prepared other models for exhibition in France, Austria, Russia, Switzerland, and England. With the latter, which, by the way, was never forwarded, "fearing difficulties at the custom-house," he sent a description in English, in which he expressed the hope that "Sir Humphry Davy would receive it favourably, perhaps improve it, and further its application in Great Britain.†

The instruments designed for exhibition in Paris were entrusted to Baron Larrey (an old friend and

* Hamel, pp. 15–17. † Ibid., p. 33.

correspondent of Sömmerring, and then Inspector-General of the Army Medical Department under the Empire), who happened to be passing through Munich on his way home from the battle-fields of Aspern, Esslingen, and Deutsch Wagram. This was on the 4th November, 1809, and, immediately after Larrey's departure, Sömmerring drew up an account of his telegraph in French, under the title *Mémoire sur le Télégraphe*, which, on the 12th November, he forwarded to his friend at Paris, accompanied by a private note as follows :—

"I have the honour to enclose a memoir, which, with the model that you have kindly taken charge of, will, I hope, explain matters clearly and briefly. I am anxious to know what reception His Imperial Majesty will accord to my ideas. The memoir, as you will see, describes the principal results of some experiments as varied as I could make them; and I hope that they will interest many members of the Institute, for independently of the great interest that attaches to them they are not wanting in novelty."

Replying on the 10th December, 1809, Larrey says :—"His Majesty, being prevented by press of business from occupying himself just now with scientific matters, has informed me that he will inspect your apparatus later on. Meanwhile I have decided to present it to the first class of the Institute, which [on the 5th December] received it with interest, and appointed a commission to report upon it."

Writing again, on the 28th December, to Baron de Kobell, one of the Bavarian Secretaries of State, Larrey says:—"I have the honour to send you the little case for Doctor Sömmerring of which I have already spoken to you. Will you kindly send it on to him by the first safe means that you can find. I hope that the contents will please him.

"I shall take care to inform him of the nature of the report of the Institute upon his telegraphic machine as soon as it appears; but in this there will probably be some delay, as the academicians who have charge of the matter are greatly occupied at present on some pressing Government affairs."

The commission consisted of Biot, Carnot, Charles, and Monge, but, for some unaccountable reason (at least, Biot, in after years, could recollect none), no report was ever made, and full eighteen months later (May 12, 1811) the instruments were sent back to Munich.

Writing to Sömmerring, on April 19, 1810, Larrey says:—

"My dear and respected friend,—Three months ago I sent to Mr. de Kobell, for transmission to you, a small case containing some remarkably diseased bones, and some brief notices upon them; your long silence makes me fear that this case has not reached you. Will you, my dear Doctor, kindly enlighten me on this point?

"I also informed you that the Institute had appointed a commission to report upon your telegraph, but certain persons, no doubt moved by jealousy, do not regard the discovery with the interest that it ought to inspire, and the report is, consequently, not yet made.

"Wishing to meet your desires, at least in part, I have inserted a notice of your instrument in the current number of the *Bulletin de la Société Médicale d'Émulation*, after having submitted it to the Society. If you have not received the Journal I will send you a copy."

In this paper, *On the Origin and Structure of the Encephalic Nerves*, Larrey briefly introduces the telegraph, and then goes on to speak with much detail of the analogy which its many wires offered to the single fibres of the nervous system, an analogy which Sömmerring himself had pointed out in his French memoir, as well as in the original account of his telegraph in the *Transactions* of the Munich Academy.* Larrey's article was republished twenty years later (in November 1829), in his *Clinique Chirurgicale;* † but, in both publications, it was placed in the midst of

* The same analogy between the nerves of the body and the telegraphic system of the world has since been frequently noticed. See Fechner's *Lehrbuch des Galvanismus*, Leipsic, 1829, p. 269; *Mechanics' Magazine*, for 1837–8, p. 262; and *Notes and Queries*, for August 27, 1870, p. 173.

† Vol. i. p. 361.

pathological and surgical subjects, where one would never look for an invention for telegraphic purposes.

On the 30th July, 1810, Sömmerring replied in the following curious letter:—

"I have read with great pleasure, sir, you dissertation on my telegraph. Have you received my memoir, which I posted on the 12th November; and have you kindly communicated it to the Institute?

"The old conducting wires are somewhat damaged, and as it was entirely to avoid delay that I did not renew them before despatching the apparatus, I would be glad if they could be replaced by new wires of the sort used in harpsichords, covered with silk thread, as the material of which these are composed is more durable than the old copper wires. Had I imagined, sir, that you would have taken such an interest in my invention as to charge yourself with its transport to Paris, I would certainly not have omitted, beforehand, to effect the necessary changes, which, without counting the time, require only a little care. I am very much afraid that, besides the fragility of the copper wires, the rough usage to which they have been subjected in the course of experiment may have rubbed off in places the silk, and so may cause intermediate contacts of the metal, whence must result a derangement of the whole system.

"Allow me, then, to beg of you not to show the instrument to the Prince de Neufchâtel, or even to His Majesty the Emperor, until the above-mentioned repairs have been effected, either by myself, or, if its return would take too long, by some competent mechanic in Paris."

Regarding the model of the telegraph which Sömmerring delivered to Count Jeroslas Potocki, a colonel of Russian Engineers, for exhibition at Vienna and St. Petersburg, the following letter has been preserved by Sömmerring's family:*—

"Baaden, near Vienna, July 5, 1811.

"Sir,—I hasten to inform you that, on my return to Vienna, their Majesties, the Emperor and Empress, signified their desire to see the electric telegraph—an invention which does honour to human genius. On the first of the current month I had the pleasure to show your telegraph to their Majesties, and they were enchanted. His Majesty was so pleased that he expressed his desire to have a telegraph from Laxenburg to Vienna (a distance of about nine miles). He did not omit to ask me to whom we are indebted for so ingenious an invention. He knows you by reputation, and says that you are one of the first anatomists living. In fact, I can assure you that their Majesties, and the Archdukes, who were also present, were enchanted.

"Professor Jacquin, of Vienna, wishes to come to

* We are indebted for this and the preceding extracts to Mr. Karl Sömmerring, of Frankfort, a grandson of the distinguished physicist of whom we are writing.

see me at Baaden, with the view of inspecting the telegraph.

"In fine, your invention has had the greatest success, and I do not doubt for an instant that, especially in Russia, it will be carried out on a grand scale. I shall not fail to acquaint you with the reception that it may there meet with. Meanwhile I pray you accept the assurance of the highest sentiments with which I am, sir, your very humble and very obedient servant,

"JEROSLAS POTOCKI."

The apparatus which Sömmerring sent to his son, Wilhelm, at Geneva, where he was then studying, is still preserved by the family of the latter at Frankfort. It was exhibited at Vienna in 1873, at London (South Kensington Museum) in 1876, and at the Paris Exhibition of 1881, at all of which places it elicited, as may be supposed, the liveliest interest.

Sömmerring, who was a distinguished anatomist and physiologist, was born at Thorn, West Prussia, on January 28, 1755, and died at Frankfort, on 2nd March, 1830. He was elected a member of the Munich Academy of Sciences in 1805, was made Knight of the Order of St. Anne of Russia in 1818, and, in 1819, was elected an honorary member of the Imperial Academy of Sciences of St. Petersburg. Quite recently, we believe, a monument has been erected to his memory in the city of Frankfort, where he passed the last ten years of his interesting life.

Dr. Hamel, in his *Historical Account*, &c., pays a very just tribute to his worth, with which we will close this account of his telegraph:—"When one studies the life and the labours of Sömmerring, it is impossible not to feel the highest esteem for him, as a man and as a philosopher. Not vanity, not eagerness of gain, but pure love of science and the wish to be useful were the motives of his incessant activity. Nor was Sömmerring too sanguine in his expectation with regard to the application of his invention. He expressed the hope that it might serve to telegraph from Munich to Augsburg, nay, from one end of the kingdom to the other, without intermediate stations" (pp. 34, 35).

1811.—*Schweigger's Telegraph.*

In preparing an account of Sömmerring's telegraph for insertion in his *Journal für Chemie und Physik*,* Schweigger of Nürenberg, and later of Halle—the same who afterwards invented the galvanometer—was struck with the insuperable difficulty there would be in dealing practically with so many wires, and in his paper he suggested a plan which required only two wires, and two piles of different strengths, so that at one time the weaker may be used, and at another time the stronger, or even both combined. In this way the quantity of gas produced in a given time at the distant station would be varied, a small quantity

* Vol. ii. p. 240, for 1811.

denoting one letter, and a larger another. Again, by varying (1) the duration of the evolutions, and (2) the intervals between, other letters might be indicated; and thus, by the combination of these primary elements of quantity and time, all the letters of the alphabet could be expressed through two wires instead of thirty-seven. In ignorance of Sömmerring's alarum, Schweigger suggested the firing of Volta's gas-pistols by Leyden jars as a means of drawing attention.*

1813.—*Sharpe's Telegraph.*

All that we know of this invention is contained in the following paragraph, which we copy from the *Repertory of Arts*, 2nd series, June 1816, p. 23:—

"*On the Electrical Telegraph.* Communicated by Mr. J. R. Sharpe, of Doe Hill, near Alfreton.—In the *Repertory of Arts*, vol. xxiv., 2nd series, p. 188, is an account of an electric telegraph by M. Soemmering. This account I did not see till a few weeks ago. Without the slightest wish to throw a doubt over the originality of M. Soemmering's invention, I beg leave to mention that an experiment, showing the advantages to be obtained from the application of the certain and rapid motion of the electric principle

* He also described a sort of manifold short-hand, or sign-printer, like that patented by Wheatstone in 1841; but this had nothing to do with electricity, and was mentioned *par parenthèse*.

through an extensive voltaic circuit to the purposes of the ordinary telegraph, was exhibited by me before the Right Hon. the Lords of the Admiralty, in the beginning of February 1813."

My Lords are said to have approved the design, but passed it over with the remark that "As the war was over, and money scarce, they could not carry it into effect." *

A nephew of the inventor, writing in 1861,† says, in reference to the above announcement, that Mr. Sharpe "conveyed signals a distance of seven miles under water." In the hope of getting further information we addressed ourselves to this gentleman, but, unfortunately, he could add nothing to the above-mentioned facts.

Mr. J. R. Sharpe was bred in London as a solicitor, but early left the profession, and retired to Doe Hill, which he built in 1801. He was always of a studious turn, and even in advanced age amused himself with mathematical problems. He died November 11, 1859, aged eighty-four years.

It was probably anent Sharpe's proposals that the following squibs were written, which we extract from *The Satirist*, September and October 1813.

"On the report that it is in contemplation to substitute an electrical mode of communication with the

* *Saturday Review*, August 21, 1858, p. 190.

† *A Treatise on the Construction and Submersion of Deep-Sea Electric Telegraph Cables*, by Benjamin Sharpe, London, 1861, p. 16.

outposts (by means of wires laid underground) for the existing telegraphic system :—

"Our telegraphs, just as they are, let us keep,
They forward good news from afar,
And still may send better—that Boney's asleep,
And ended oppression and war.

Electrical telegraphs all must deplore,
Their service would merely be mocking ;
Unfit to afford us intelligence more
Than such as would really be *shocking!*
"TAM GLEN."

"On the Proposed Electrical Telegraph, October 1813 :—

" When a victory we gain
(As we've oft done in Spain)
It is usual to load well with powder,
And discharge 'midst a crowd
All the Park guns so loud,
And the guns of the Tower, which are louder.

But the guns of the Tower,
And the Park guns want power
To proclaim as they ought what we pride in ;
So when now we succeed
It is wisely decreed
To announce it from the *batteries of Leyden.*"

1816.—*Coxe's Telegraph.*

In February 1816, Dr. J. Redman Coxe, professor of chemistry at Philadelphia, published some suggestions for an electro-chemical telegraph on the same principle as those already described. His views are given in the following letter, which we have extracted from Thomson's *Annals of Philosophy*, vol. vii. pp. 162–3, headed "Use of Galvanism as a Telegraph":—

"I observe in one of the volumes of your *Annals of Philosophy* a proposition to employ galvanism as a solvent for the urinary calculus, which has been very properly, I think, opposed by Mr. Armiger. I merely notice this, as it gives me the opportunity of saying that a similar idea was maintained in a thesis three years ago by a graduate of the University of Pennsylvania.

"I have, however, contemplated this important agent as a probable means of establishing telegraphic communication with as much rapidity, and, perhaps, less expense than any hitherto employed. I do not know how far experiment has determined galvanic action to be communicable by means of wires, but there is no reason to suppose it confined as to limits, certainly not as to time. Now, by means of apparatus fixed at certain distances, as telegraphic stations, by tubes for the decomposition of water and of metallic salts, &c., regularly arranged, such a key might be adopted as would be requisite to communicate words, sentences, or figures, from one station to another, and so on to the end of the line. I will take another opportunity to enlarge upon this, as I think it might serve many useful purposes; but, like all others, it requires time to mature. As it takes up little room, and may be fixed in private, it might in many cases, of besieged towns, &c., convey useful intelligence with scarcely a chance of detection by the enemy. However fanciful in speculation, I have no doubt that sooner or later it will be rendered useful in practice.

"I have thus, my dear sir, ventured to encroach upon your time with some crude ideas that may serve perhaps to elicit some useful experiments at the hands of others. When we consider what wonderful results have arisen from the first trifling experiments of the junction of a small piece of silver and zinc in so short a period, what may not be expected from the further extension of galvanic electricity? I have no doubt of its being the chiefest agent in the hands of nature in the mighty changes that occur around us. If metals are compound bodies, which I doubt not, will not this active principle combine their constituents in numerous places so as to explain their metallic formation; and if such constituents are in themselves aeriform, may not galvanism reasonably tend to explain the existence of metals in situations in which their specific gravities certainly do not entitle us to look for them?"

Dr. Coxe does not appear to have ever reduced his ideas to practice, but the large faith which he expresses in the capabilities of galvanism deserves to be remembered to his credit. Indeed there can be no doubt that, if electrical science had made no further advances, the early projects that we have been describing in this chapter would have gradually developed themselves into practical electro-chemical telegraphs, such as were afterwards proposed by E. Davy in 1838, by Smith in 1843, by Bain in 1846, and by Morse in 1849.* But the grand discovery of electro-magnetism was at hand, and soon turned the tide of invention into quite another channel.

* Besides all these inventions, other electro-chemical telegraphs have been proposed by Bakewell, Caselli, Bonelli, D'Arlincourt, Sawyer, and others. All are dependent on a fact which, as we have shown in our seventh chapter (p. 195), was first observed by Cruickshank in 1800, very soon after the announcement of the voltaic pile, *viz.*, the power of electricity to discolour litmus paper.

CHAPTER IX.

ELECTRO-MAGNETISM AND MAGNETO-ELECTRICITY —HISTORY IN RELATION TO TELEGRAPHY.

"Around the magnet, Faraday
Is sure that Volta's lightnings play,
But how to draw them from the wire?
He took a lesson from the heart—
'Tis when we meet—'tis when we part
Breaks forth the electric fire."

Impromptu, by Herbert Mayo,
in *Blackwood's Magazine.*

FROM an early period in the history of electricity philosophers began to point out strong resemblances between the phenomena which it exhibits and those of magnetism. In both sciences there existed two forces of opposite kind, capable, when separate, of acting with great energy, and being, when combined, perfectly neutralised and exhibiting no signs of activity; there was the same attraction and repulsion between the two magnetisms as between the two electricities, and according to the same law of inverse squares; the action of free electricity on a neighbouring body was not unlike that which a magnet exercises upon iron; and, lastly, the distribution of the two forces in the one seemed to differ little from that of the two forces in the other.

These analogies were powerfully supported by several facts. Thus, as early as 1630 Gassendi observed that magnetism was communicated to ferruginous bodies by lightning; the compass needles of ships were known to have their poles weakened, and even reversed, by a similar cause—a fact first recorded by English navigators in 1675; and, in 1750, Professor Wargentin remarked that delicately-suspended magnets were affected by the aurora borealis.*

With such analogies, and supported by such remarkable facts as these, the suspicion was but natural that the two sciences were allied in some close and intimate way, and accordingly we find that, about the middle of the last century, the discovery of this relation became a favourite pursuit.

Swedenborg was the first to boldly express his views on this subject in his *Principia Rerum Naturalium* (Dresden, 1734), in which he argued a close relationship between electricity and magnetism on the ground of their both being polar forces.

In 1748, Beraut, professor of mathematics in the College of Lyons, published at Bordeaux a thin volume of 38 pages,† which is probably the first dis-

* *Encys. Brit. and Metropol.*, articles Electricity and Magnetism. A similar observation to that of Wargentin was made by Halley, and afterwards more accurately by Dalton, both of whom likewise found that the beams of the aurora were always parallel to the magnetic meridian.—*Trans. Cambridge Phil. Soc.*, vol. i.

† *Dissertation sur le rapport qui se trouve entre la Cause des effets de l'Aiman, et celles des Phénomènes de l'Électricité.*

tinct treatise on its subject, and which also goes to show that a true connection exists; that, in fact, it is the same force only differently disposed, which produces both electric and magnetic phenomena.

In studying the points of analogy between lightning and electricity, the great Franklin remarked that the latter, like the former, had the power not merely of destroying the magnetism of a needle, but of completely reversing its polarity. By discharging four large Leyden jars through a common sewing needle, he was able to impart to it such a degree of magnetism that, when floated on water, it placed itself in the plane of the magnetic meridian. When the discharge was sent through a steel wire perpendicular to the horizon it was permanently magnetised, with its lower end a North, and its upper end a South pole; and, on reversing the position of the wire and again transmitting through it the discharge, the polarity was either destroyed, or entirely reversed. Franklin also found that the polarity of the loadstone could be destroyed in a similar manner.*

Dalibard, about the same time, imagined that he had proved that the electric discharge gives a northern polarity to that point of a steel bar at which it enters, and a southern polarity to that at which it makes its exit, while Wilcke, for his part, was equally satisfied that an invariable connection existed between negative electricity and northern polarity.

* Priestley's *History of Electricity*, London, 1767, p. 178.

From a review of all these, and other observations by himself made between 1753 and 1758, Beccaria came to the conclusion that the polarity of a needle magnetised by electricity was invariably determined by the direction in which the electric discharge was made to pass through it; and as a consequence he assumed the polarity acquired by ferruginous bodies which had been struck by lightning as a test of the kind of electricity with which the thunder-cloud was charged.

Applying this criterion to the earth itself, Beccaria conjectured that terrestrial magnetism was, like that of the needle magnetised by Franklin and Dalibard, the mere effect of permanent currents of natural electricity, established and maintained upon its surface by various physical causes; that, as a violent current, like that which attends the exhibition of lightning, produces instantaneous and powerful magnetism in substances capable of receiving that quality, so may a more gentle, regular, and constant circulation of the electric fluid upon the earth impress the same virtue on all such bodies as are capable of it. "Of such fluid, thus ever present," observes Beccaria, "I think that some portion is constantly passing through all bodies situate on the earth, especially those which are metallic and ferruginous; *and I imagine it must be those currents which impress on fire-irons, and other similar things, the power which they are known to acquire of directing* *themselves according to the magnetic meridian when they are properly balanced.*" *

Diderot, one of the editors of the celebrated "Encyclopædia," and whom the *Revue des deux Mondes* † calls a "Darwinist a century before Darwin," was also, as early as 1762, a firm believer in the identity of electricity and magnetism, and has left in his writings some arguments in support of this hypothesis.

In his essay *On the Interpretation of Nature* he says:—"There is great reason for supposing that magnetism and electricity depend on the same causes. Why may not these be the rotation of the earth, and the energy of the substances composing it, combined with the action of the moon? The ebb and flow of the tides, currents, winds, light, motion of the free particles of the globe, perhaps even of the entire crust round its nucleus, produce, in an infinite number of ways, continual friction. The effect of these causes, acting as they do sensibly and unceasingly, must be, at the end of ages, very considerable. The nucleus or kernel of the earth is a mass of glass, its surface is covered only with remains of glass—sands and vitrifiable substances. Glass is, of all bodies, the one that yields most electricity on being rubbed. Why may not the sum total of terrestrial

* Ampère's theory of electro-magnetism, and likewise his view of terrestrial magnetism, are here distinctly foreshadowed by this most acute and accurate observer. For a full account of Beccaria's researches, see Priestley's *History of Électricity*, London, 1767, pp. 340–352.

† For December 1, 1879, p. 567.

electricity be the result of all these frictions, either at the external surface of the earth, or at that of its internal kernel?

"From this general cause it is presumable that we can deduce, by experiments, a particular cause which shall establish between two grand phenomena, *viz.*, the position of the aurora borealis and the direction of the magnetic needle, a connection similar to that which is proved to exist between magnetism and electricity by the fact that we can magnetise a needle without a magnet and by means only of electricity.

"These notions may be either true or false. They have no existence so far but in my imagination. It is for experience to give them solidity, and it is for the physicist to discover wherein the phenomena differ, or how to establish their identity." *

In the year 1774, the following question was proposed by the Electoral Academy of Bavaria as the subject of a prize essay:—"Is there a real and physical analogy between electric and magnetic forces, and, if such analogy exist, in what manner do these forces act upon the animal body?" The essays received on that occasion were collected and published ten years later by Professor Van Swinden, of Franeker—the winner of one of the prizes.† Some of

* The physicist has been true to the trust. See *Collection Complète des Œuvres Philosophiques, Littéraires, et Dramatiques de Diderot*, 8vo., 5 vols., Londres, 1773, vol. ii. p. 28.

† *Recueil de Mémoires sur l'Analogie de l'Electricité et du Magnétisme*, &c., 3 vols., 8vo., La Haye, 1784.

the essayists, and amongst them Van Swinden, maintained that "the similarity was but apparent, and did not constitute a real physical resemblance;" while, on the other hand, Professors Steiglehner and Hubner contended that "so close an analogy as that exhibited by the two sciences indicated a single agency acting under different circumstances." *

In this unsettled state the subject remained for many years until the discovery of galvanism and the invention of the voltaic pile, which, by furnishing the philosopher with the means of maintaining a continuous current of electricity in large quantity, enabled him to study its effects under the most favourable circumstances.

Early in the present century philosophers thought they saw an analogy between magnetism and galvanism in a phenomenon, which we find thus referred to in Lehot's *Observations sur le Galvanisme et le Magnétisme:* †—"It has long been known that the two wires which terminate a pile attract one another, and, after contact, adhere like two magnets. This attraction between the two wires, one of which receives, and the other loses, the galvanic fluid, differs essentially from electrical attraction, as Ritter observed, since it is not followed by a repulsion after contact, but continues as long as the chain is closed" (note on p. 4).‡

* Noad's *Manual of Electricity*, p. 641.

† Paris, *circa*, 1806, 8vo., 8 pp.

‡ This discovery appears to have been made independently, and about the same time, by Gautherot, in 1801 (*Philosophical Magazine*,

In the same spirit of inquiry Desormes and Hachette, in 1805, tried to ascertain the direction which a voltaic pile, whose poles were not joined, would take when freely suspended horizontally. The pile, "composed of 1480 thin plates of copper tinned with zinc, of the diameter of a five-franc piece," was placed upon a boat, which floated on the water of a large vat; but it assumed no determinate direction, although "a magnetised steel bar, of a weight nearly equal to that of the pile, and placed like it upon the boat, would turn, after some oscillations, into the magnetic meridian." *

The honour of the discovery of the much-sought-for connection between electricity and magnetism has often, in the last fifty years, been claimed for Romagnosi, an Italian writer who is justly esteemed for his works on history, law, and political philosophy. Govi,† however, in 1869, showed in the clearest manner possible the absurdity of this claim; but, notwithstanding, it has been again put forward, and this time by no

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for 1828, vol. iv. p. 458); by Laplace; and by Biot (*Journal de Physique et de Chimie*, &c., for 1801, vol. liii. p. 266). The latter made the further very acute observation that, if the wires be attached to plates of metal, and these plates be approached by their *edges*, they will attract one another; while if approached by their *faces* no action whatever takes place.

For other interesting experiments of this kind, see Nicholson's *Journal*, for 1804, vol. vii. p. 304.

* *Philosophical Magazine*, for 1821, vol. lvii. p. 43.

† *Romagnosi e l'Elettro-Magnetismo*, Turin, 1869.

less an authority than Dr. Tommasi, of Paris, in a recent number of *Cosmos les Mondes* (June 30, 1883).

Dr. Tommasi, in republishing Romagnosi's experiment, asks the following questions, which he submitted, in particular, to the managing committee of the (late) Vienna Exhibition, in the hope that they might have been brought before electricians :—

"Is it to Oersted, or to Romagnosi, that we should ascribe the merit of having first observed the deviation of the magnetic needle by the action of the galvanic current?

"Had Oersted any knowledge of the experiment of Romagnosi when he published his discovery of electro-magnetism ? *

"Have any other *savants* taken part in this discovery ?"

Now, we should have thought that after the admirable *exposé* of Govi, to which we have just referred, no electrician would seriously put to himself these questions. But it appears that our Paris *confrère* does so, although, if he had only read carefully the facts on which he bases them, he would perceive that they have no relation whatever to electro-magnetic action, but are simply effects of ordinary electrical attraction and repulsion brought about by the static charge which is always accumulated at the poles of a strong voltaic *pile*—the form of battery used in Romagnosi's

* Dr. Hamel, for one, thought he had, and tries to prove it in his *Historical Account*, &c., of 1859 (pp. 37–9 of W. F. Cooke's reprint).

experiments, and which, as is well known, exhibits this phenomenon in a far more exalted degree than the ordinary cell arrangement.

We cannot establish better the correctness of our conclusions than by quoting in full the recital of Romagnosi's experiment, as it originally appeared in the *Gazzetta di Trento*, of August 3, 1802:*—

"Article on Galvanism.

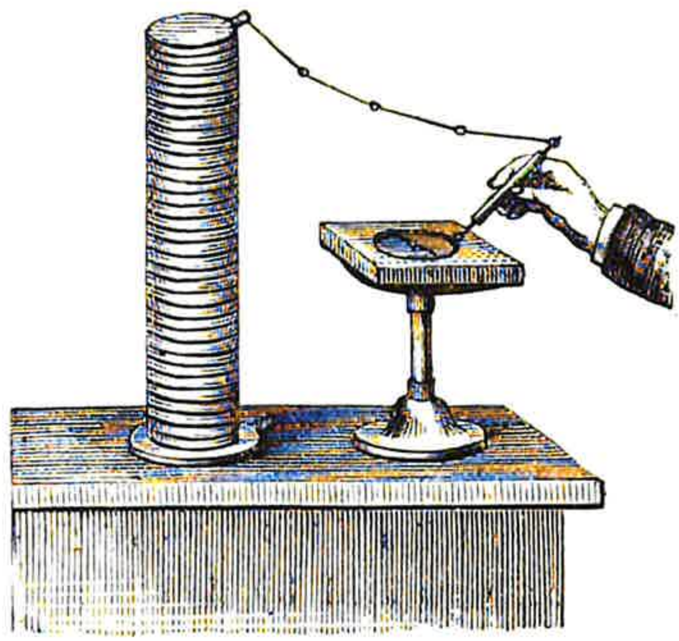
"The Counsellor, Gian Domenico de Romagnosi, of this city, known to the republic of letters by his learned productions, hastens to communicate to the physicists of Europe an experiment showing the action of the galvanic fluid on magnetism.

"Having constructed a voltaic pile, of thin discs of copper and zinc, separated by flannel soaked in a solution of sal-ammoniac, he attached to one of the poles one end of a silver chain, the other end of which passed through a short glass tube, and terminated in a silver knob. This being done, he took an ordinary compass-box, placed it on a glass stand, removed its glass cover, and touched one end of the needle with the silver knob, which he took care to hold by its glass envelope. After a few seconds' contact, the needle was observed to take up a new position, where it remained, even after the removal of the knob. A fresh application of the knob caused a still further

* Our translation is made from the reprint at p. 8 of Govi's *Romagnosi e l'Elettro-Magnetismo*.

deflection of the needle, which was always observed to remain in the position to which it was last deflected, as if its polarity were altogether destroyed.

FIG. 8.



Romagnosi's Experiment, according to Govi.

"In order to restore this polarity, Romagnosi took the compass-box between his fingers and thumbs, and held it steadily for some seconds. The needle then returned to its original position, not all at once, but little by little, advancing like the minute or seconds hand of a clock.

"These experiments were made in the month of May, and repeated in the presence of a few spectators, when the effect was obtained without trouble and at a very sensible distance."

Here it will be seen that Romagnosi uses only *one* pole of the pile, and *never speaks of the circuit being closed*—facts which show that his experiment has no resemblance to that of Oersted.

The effects which he describes are, moreover, easily explainable on another hypothesis. The compass needle, we may imagine, received a charge of static electricity by contact with the charged pole of the pile. Being insulated, it could not part with this charge, and, consequently, as soon as it had attained the same potential as the voltaic pole, mutual repulsion ensued. As the needle belonged to "an ordinary compass-box," we may assume it was neither strongly magnetised, nor delicately suspended. Friction at the point of support, then, might more than counterbalance the directive force of the earth, and so the needle would always remain in the position to which it had been last repelled.

The "restoration of polarity," or the bringing back of the needle to the magnetic meridian, by merely holding the compass-box steadily between the fingers and thumbs, although savouring of legerdemain, was really due to a "simple turn of the wrist." Romagnosi may have imagined that he held the compass-box *steadily*, but there can be no doubt that his hands suffered a slight and imperceptible tremor, which, aided by the directive force of terrestrial magnetism, sufficed to shake the needle into a north and south position.

Another, and to us convincing, argument against the supposition that Romagnosi had any share in the discovery of electro-magnetism is that he himself never claimed any, although he lived down to the year 1835, or fifteen years after the announcement of the Danish philosopher.*

To the same category belongs the contrivance of Schweigger, which is described in Gehlen's *Journal für die Chemie und Physik*, for 1808 (pp. 206–8), and on the strength of which a recent writer † says that the celebrated inventor of the galvanometer ought also to be considered as the discoverer of electro-magnetism. There is no ground whatever for this statement. Schweigger's paper, which is headed *On the Employment of the Magnetic Force for Measuring the Electrical*, simply describes an electroscope for indicating the attraction and repulsion of ordinary, or frictional, electricity, and which he used as a substitute for the torsion electrometer of Coulomb. It consisted of a magnetic needle, armed at each end with a brass knob, and mounted on a pivot, as in an ordinary compass.

In fact, these experiments of Romagnosi and Schweigger are but modifications of one which dates back to the very earliest days in the history of electricity, and upon which, we have no doubt, Milner, in 1783, constructed the electrometer now known as

* For some very interesting experiments of this kind, see Van Mons' *Journal de Chimie*, for Jan. 1803, p. 52; also Nicholson's *Journal*, vol. vii. p. 304.

† In the *Journal für Math. und Physik*, Berlin, 1873, p. 609.

Peltier's. In our second chapter (p. 31) we have said, when speaking of Gilbert:—"In order to test the condition of the various substances experimented upon, Gilbert made use of a light needle of any metal, balanced, and turning freely on a pivot, like the magnetic needle, to the extremities of which he presented the bodies after excitation." Romagnosi and Schweigger have done no more than this—hardly as much, for Gilbert's contrivance was a valuable instrument of research, while those of the later philosophers were barren of results.

Other instances of this phenomenon, contributed by Robins and Kinnersley respectively, occur in the *Philosophical Transactions*, for 1746 and 1763; and a recent example, which is described in the *American Polytechnic Review*, for 1881, is considered so puzzling that "it is given for what it is worth" in the scientific paper in which we find it:*—

"An American surveyor, who had been taking some delicate bearings, was puzzled to find that the magnetic needle did not give the same bearing twice, and he observed that it never *quite* settled. This could not be explained as due to metallic articles in the dress, or pockets, of the observer; and an examination of the magnifying glass used in reading the needle was made. The magnifier was similar to those now universally used to read the verniers and needle-bearings

* An exactly similar case is recorded at p. 280, vol. xxi., of *The Quarterly Journal of Science and the Arts* (Royal Institution), for 1826.

of field instruments, having a black vulcanite frame, highly polished, and in this, it is stated, the whole cause of the trouble lay. It was found that this frame was peculiarly liable to become electrified, that the slightest friction, even the mere carrying in the pocket, was sufficient to charge it, and that, when thus electrified, if brought near the needle of a compass, it had almost the effect of a loadstone in drawing it from its true settling place. On discarding this magnifier and using an ordinary glass lens without a frame, no further trouble was found in the field work done with the compass. This must be taken for what it is worth."

As little value attaches to the observation of Mojon which we find recorded by Aldini, and which seems to us but a repetition of Franklin's experiment (before mentioned, p. 252), with this difference, that a voltaic battery was used instead of one of Leyden jars. Aldini says:—"The following experiment has been quite recently communicated to me by its author Mojon:—

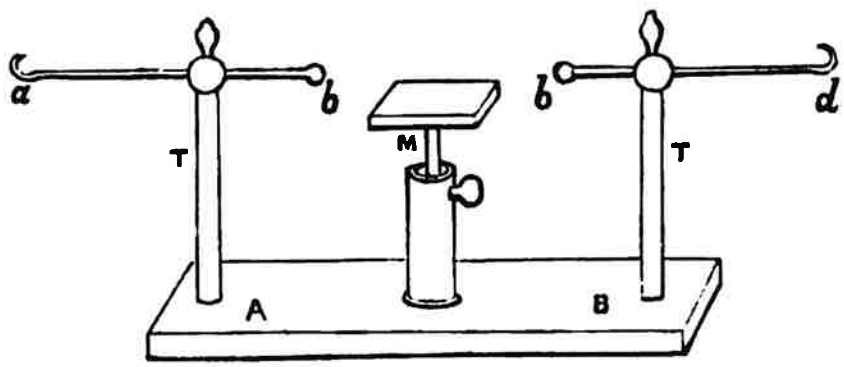
"Having placed horizontally sewing-needles, very fine, and two inches long, he put the two extremities in communication with the two poles of a battery of one hundred cups, and on withdrawing the needles, at the end of twenty days, he found them a little oxidised, but at the same time endowed with a very sensible magnetic polarity. This new property of galvanism has been verified by other observers, and lately by

Romanesi, who has found that galvanism is able to deflect a magnetic needle." *

At p. 120 of his *Manuel du Galvanisme* (Paris, 1805), Joseph Izarn describes Mojon's experiment, and appends an illustration, which shows most conclusively that it had no reference to electro-magnetism. His words are :—

"Apparatus for observing the action of galvanism on the polarity of a magnetised needle :—

FIG. 9.



Mojon's Experiment, according to Izarn.

"*Preparation*. Arrange the horizontal rods *a b*, *b d* (Fig. 9) so that they may approach the magnetic bar shown between them, in place of the knobs *b b*, screw on little pincers which take hold of the magnetic bar, and attach one pole of a pile to *a*, and the other to *d*, thus completing the voltaic circuit through the length of the magnet.

* *Essai Théorique et Expérimental sur le Galvanisme*, Paris, 1804, vol. i. p. 339.

"*Effects.* According to the observations of Romagnosi the magnet experiences a declination, and according to those of Mojon needles not previously magnetised acquire by this means a sort of magnetic polarity." *

In a paper read before the Royal Academy of Munich, in May 1805, Ritter, a Bavarian philosopher, advanced some curious speculations, which, although always quoted, as suggestive of electro-magnetism, are really as wide of the mark as the experiments of Romagnosi, Schweigger, and Mojon. We find them thus described in the *Philosophical Magazine*, for 1806:†—

"The pile with which M. Ritter commonly performs his experiments consists of 100 pairs of plates of metal, two inches in diameter; the pieces of zinc have

* Mr. Sabine appears to have studied Izarn, yet he writes thus, at p. 23 of his *History and Progress of the Electric Telegraph*, 2nd edit., London, 1869:—"After explaining the way to prepare the apparatus, *which consists simply in putting a freely suspended magnet needle parallel and close to a straight metallic conductor* through which a galvanic current is circulating, he describes the effects in the following words," &c. The words that we have italicised are altogether misleading.

† Vol. xxiii. p. 51. "An ingenious and extraordinary man, from whom much might have been expected, had nature permitted the continuance of his scrutiny into her secret operations. A premature death deprived the world of one whose constitutional singularity of opinion, ardency of research, and originality of invention, rendered him at once systematic in eccentricity, inexhaustible in discovery, and ingenious even in error."—Donovan's *Essay on the Origin, Progress, and Present State of Galvanism*, Dublin, 1816, p. 107.

Johann Wilhelm Ritter was born December 16, 1776, and died at Munich, January 23, 1810.

a rim to prevent the liquid pressed out from flowing away, and the apparatus is insulated by several plates of glass.

"As he resides at present near Jena I have not had an opportunity of seeing experiments with his great battery of 2000 pieces, or with his battery of 50 pieces, each thirty-six inches square, the action of which continues very perceptible for a fortnight. Neither have I seen his experiments with the new battery of his invention, consisting of a single metal, and which he calls *the charging pile.**

"I have, however, seen him galvanise a louis d'or. He places it between two pieces of pasteboard thoroughly wetted, and keeps it six or eight minutes in the circuit of the pile. Thus it becomes charged, though not immediately in contact with the conducting wires. If applied to the recently bared crural nerves of a frog the usual contractions ensue. I put a louis d'or thus galvanised into my pocket, and Ritter told me, some minutes after, that I might discover it from the rest by trying them in succession upon the frog. I made the trial, and actually distinguished, among several others, one in which only the exciting quality was evident.

"The charge is retained in proportion to the time that the coin has been in the circuit of the pile. Thus,

* The charging pile, or, as we now call it, the secondary battery, was first described by Gautherot in 1801. See Izarn's *Manuel du Galvanisme*, Paris, 1804, p. 250; also *Phil. Mag.*, for 1806, vol. xxiv. p. 185.

of three different coins, which Ritter charged in my presence, none lost its charge under five minutes.

"A metal thus retaining the galvanic charge, though touched by the hand and other metals, shows that this communication of galvanic virtue has more affinity with magnetism than with electricity, and assigns to the galvanic fluid an intermediate rank between the two.

"Ritter can, in the way I have just described, charge at once any number of pieces. It is only necessary that the two extreme pieces of the number communicate with the pile through the intervention of wet pasteboards. It is with metallic discs charged in this manner, and placed upon one another, with pieces of wet pasteboard alternately interposed, that he constructs his charging pile, which ought, in remembrance of its inventor, to be called the *Ritterian pile*. The construction of this pile shows that each metal galvanised in this way acquires polarity, as the needle does when touched with a magnet.*

* * * * * *

"After showing me his experiments on the different contractibility of various muscles, Ritter made me

* We may here dispose of a paragraph which has hitherto puzzled a good many writers, who have supposed it to refer to some kind of magneto-electric machine. It occurs in *The Monthly Magazine*, for April 1802, p. 268, and reads as follows :—

"Galvanism is at present a subject of occupation of all the German philosophers and chemists. At Vienna an important discovery has been announced—an artificial magnet—employed instead of Volta's observe that the piece of gold galvanised by communication with the pile exerts at once the action of two metals, or of one voltaic couple, and that the face, which in the voltaic circuit was next the negative pole, became positive; and the face towards the positive pole, negative.

"Having discovered a way to galvanise metals, as iron is rendered magnetic, and having found that the galvanised metals always exhibit two poles as the magnetised needle does, Ritter suspended a galvanised gold needle on a pivot, and perceived that it had a certain dip and variation, or deflection, and that the angle of deviation was always the same in all his experiments. It differed, however, from that of the magnetic needle, and it was the positive pole that always dipped." *

Ritter also observed that a needle composed of silver and zinc arranged itself in the magnetic meridian, and was slightly attracted and repelled by the poles of a magnet; and, again, that a metallic wire through which a current had been passed took up of itself a N.E. and S.W. direction.

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pile, decomposes water equally well as that pile, or the electrical machine; whence it has been concluded that the electric, galvanic, and magnetic fluids are the same." Clearly the artificial magnet here mentioned can be none other than Ritter's secondary pile. One thing is certain, it cannot be a magneto-electric machine, for magneto-electricity was not known in 1802.

* C. Bernoulli, in *Van Mons' Journal*, vol. vi. See further on this subject in *Phil. Mag.*, vol. xxv. pp. 368–9.