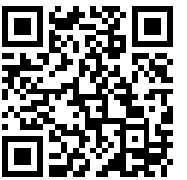
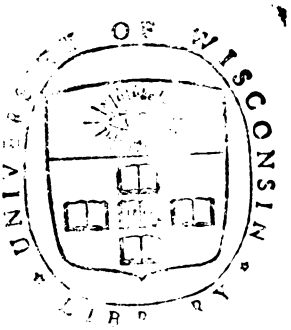
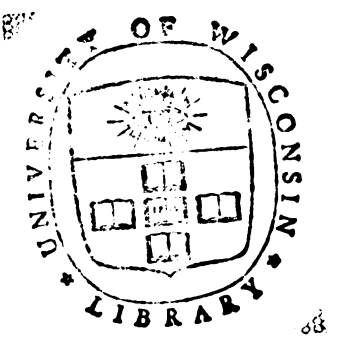






THE MORSE AMERICAN TELEGRAPH

Prof. Henry's attack

and

Prof. Morse's defence . . .

(New York?)

1855

SHAFFNER'S

TELEGRAPH COMPANION,

DEVOTED TO THE SCIENCE AND ART OF THE

MORSE AMERICAN TELEGRAPH.

VOL. II. JANUARY, 1855. No. 1.

Art. I.—THE MORSE AMERICAN TELEGRAPH.

PROF. HENRY'S ATTACK, AND PROF. MORSE'S DEFENCE—CLAIMS OF THE FORMER TO DISCOVERIES AND INVENTIONS BEARING ON THE TELEGRAPH CRITICALLY EXAMINED AND REFUTED BY PROF. MORSE—HISTORY OF EARLY TELEGRAPHS—PROF. MORSE'S CLAIMS AS FIRST INVENTOR ESTABLISHED.

On our return from Europe, in December last, we addressed a letter to Professor Morse, at Poughkeepsie, on the subject of any claims of Professor Henry to the invention of the American Electro-magnetic Telegraph; and this, in consequence of our having found, in our recent intercourse with Professor Wheatstone, in England, and others, that such supposed claims, whether real or pretended, exercised an unfavorable influence on Professor Morse's character for probity and honor. He was spoken of by Mr. Wheatstone particularly, rather as a *lucky* robber of that which was due to Professor Henry; and even in the minds of those who had seen the decisions of our courts in favor of Morse, and the deposition of Professor Henry in the case, there was still a feeling of doubt whether, after all, Mr. Morse had not had the art shrewdly to steer clear of *legal* fraud, and so been lucky enough to escape an adverse decision, on mere legal or technical grounds, while in truth he had actually defrauded Henry both out of the credit and the profit of the invention. Convinced that such an impression was a gross injustice to Professor Morse, and knowing, before we left home, that he had been engaged in preparing a review of that subject, we promised our scientific friends abroad to procure from Pro-

fessor Morse, on our return home, if possible, such a statement from him as would clear up the matter. Consequently, deeming it but right that Professor Morse should know these facts, and act his pleasure in furnishing us with his exposé, we addressed to him the following letter:—

NEW-YORK, *Dec. 27th*, 1854.

TO PROFESSOR S. F. B. MORSE.

MY DEAR SIR:—It affords me great pleasure to inform you that your system of the Electric Telegraph is fast spreading over the earth. Not only is it in general use in America, but, during the past summer, I had the pleasure of witnessing it in successful operation in various parts of Europe. In fact, it is the *only* system in *general* use on the continent. The needle system and yours are at present used between France and England, but throughout all Germany, Belgium, Prussia, Austria, Denmark, Sweden, and Russia, the Morse system is the only one in successful use.

While I was thus pleased to see your Telegraph so popular throughout the Eastern Continent, I was pained to hear a few, in high scientific position, asserting that Professor Joseph Henry, of America, was the true inventor of your system; for, say they, "you obtained all you knew of the invention from his writings and in conversations with him." This erroneous statement has, doubtless, been circulated through Professor Henry's deposition in Europe by those who are jealous of the superiority of your system over others with whom it interferes, and in which they are interested. But it is true, nevertheless, that many persons have really been led to believe that it is to Professor Joseph Henry that the world is indebted for your Telegraph, and that you had in some way wronged him out of it.

Of course, so far as I knew, I endeavored to correct this unjust statement, but the following, among others, was quoted to me from the *London Mechanics' Magazine* as evidence of the fact.

"We cannot but feel that, in relation to the practical development and discovery of the principles embraced in the Electro-Magnetic Telegraph of Professor Morse, our country is *more indebted to Professor Joseph Henry than any other living man*, and he has *neither received the public credit nor honor, which are justly his due*, much less any remuneration for his invaluable discoveries. He was the first man in the world who moved machinery by an Electro-magnet; and he is the inventor of the '*Electro-magnet*' to do this—and without this Morse's Telegraph would yet be in oblivion."

It will be seen that the above extract, based on Henry's de-

position, is taken from the Scientific American, and thus has had weight in Europe.

There is also an extract from Donald Mann's Telegraph Magazine, (now discontinued,) and based on the same ground, which, so far, implicates you in some ungenerous, if not dishonorable, conduct towards Professor Henry, as to require some explanation, but you having left it unexplained, the impression is strengthened abroad that there is something of the kind which you fear to meet. The following is the extract:

"One writer points to one great defect in Vail's book—a defect which, considering, especially, that Mr. Vail is a partner of Professor Morse, appears in a peculiarly unfortunate light, namely, *the total omission* to narrate in proper manner the *extraordinary electric discoveries of our countryman Professor Henry*, on which the Morse system is essentially founded, though we will not readily believe that this *inexcusable omission* could have been sanctioned by Professor Morse."

Although the writer of the above is reluctant to believe this "*inexcusable omission*" was sanctioned by you, yet without some explanation on your part, you must see that the impression must be left on the minds of those who are strangers to your character, that there is something wrapt up in this mysterious innuendo.

If Professor Henry has been knowing to these statements, I do not see how, as a just man, he could have ever allowed them to pass current without immediately correcting them, but he has never done so, I believe. You will know.

It is due to your country, to your friends, your family, and yourself, to correct an error which has been and is still used to the injury both of your fame and personal character. Silence on your part has hitherto given authority for its continued circulation.

I hope, therefore, you will no longer withhold from the world any facts you may possess in the case, and that you will forward to me, at your earliest convenience, a full and detailed statement relative to any participation Professor Henry may have had in the early invention and introduction of your Telegraph.

I desire the facts for publication in my Magazine, and hope you will find it convenient to give this your earliest consideration.

Very truly yours,
TAL. P. SHAFFNER.

Professor Morse replied as follows:

POUGHKEEPSIE, *Dec. 29th*, 1854.

DEAR SIR:—Yours of the 27th instant I have received. It has given me mingled pleasure and pain. It cannot but be gratifying to me to have corroborated, by your own personal observation during your recent extensive intercourse in Europe, with the most illustrious persons in rank and in science, the fact which I had previously learned, to some extent, from other sources, to wit:—the fact of the popularity, and almost universal extension, of the American Telegraph system throughout the Eastern Continent. I was not, however, aware that it had been introduced to so great an extent as you tell me, in the Northern parts of Europe.

The painful part of your letter acquaints me with a fact, of which I was, indeed, not aware, to wit: that the *absolutely groundless pretensions* to any participation in my invention, set up by, or for Professor Joseph Henry, on the ground of his deposition in the Telegraph suits, had produced, and is still producing, impressions unfavorable to my personal character in Europe.

I knew, indeed, that in various publications, got up under the patronage and to serve the private purposes of those who were pirating my invention, I and my claims were subjected to every sort of misrepresentation and disparagement for effect at home.

While saddened at this ungenerous conduct, on the part of my own countrymen, from whom I was not prepared to expect it, I preferred to pass by these injurious imputations in the main, in silence, leaving their refutation to the less prompt, but not less sure, influence, as I supposed, of the judicial decisions in my favor, not dreaming at the time, that these *home* perversions of the truth, got up by pirate speculators to justify their subterfuge, might have an adverse effect upon scientific minds *abroad*. I knew that opposing counsel, too, had gone to the full length of their proverbial license "to make the worse appear the better side," and had made bold assertions, and sophistical arguments, and assumptions of facts unwarranted by the evidence; but all these I regarded as the usual concomitants of legal warfare, and so bore them as patiently as possible and in silence, awaiting the decision of the cool, dispassionate, discriminating minds of the final arbiters of the strife, conscious that the truth would eventually make its way to the surface, however deeply buried beneath the rubbish of error.

Nevertheless, finding, even at that time, to my surprise and mortification, so great stress laid, by opposing counsel, upon Professor Henry's deposition in the case, I prepared a *defence*

(more than two years ago) against the injurious imputations attempted to be drawn from that deposition, mainly to arm myself with the proper explanations, in case their absence should be made the occasion of any real misgivings among my friends as to the soundness of my own claims, so as to affect my standing with those of my own countrymen, whose good opinion I valued. I considered, also, that it behooved me (as being conversant *alone with many of the facts*, and *possessing the evidence in my own hands*) to vindicate the truth of history, distorted and falsified by the sophistry of counsel. I felt a repugnance, however, to appear, even in self-defence, lest my motives might be mistaken and misrepresented while the decision of the Supreme Court of the United States was in abeyance, and after that decision was given, in my favor, I was disposed to let this judgment of the court, and time, work a gradual cure in neutralizing all the fallacies of my opponents, those professedly drawn from Professor Henry's testimony among the rest, without imposing upon myself the disagreeable necessity of a public exposure of its individual fallaciousness.

But if I have been deceived (and it seems I have been) in regard to the power and extent of the misrepresentation, and it has travelled *abroad* to my injury beyond the influence of the judicial decisions in the United States, or if these decisions have failed to meet the more subtle *moral exigencies* of the case, I have no other course than to comply with your request. I, therefore, send you what I had prepared, which you are at liberty to publish entire, or if its voluminousness startles you, as much as you may think proper. If, for the most part, the subject-matter is, of necessity, personal, you will yet find with it some important facts illustrative of the history of the Telegraph.

I remain, dear Sir,

Respectfully, your obedient servant,

SAMUEL F. B. MORSE.

TAL. P. SHAFFNER, Esq.

THE ELECTRO-MAGNETIC TELEGRAPH.

A DEFENCE

AGAINST THE INJURIOUS DEDUCTIONS DRAWN FROM THE

DEPOSITION OF PROF. JOSEPH HENRY,

(IN THE SEVERAL TELEGRAPH SUITS,)

WITH A CRITICAL REVIEW OF SAID DEPOSITION, AND AN EXAMINATION OF PROF. HENRY'S ALLEGED DISCOVERIES, BEARING UPON THE ELECTRO-MAGNETIC TELEGRAPH.

BY SAMUEL F. B. MORSE, LL.D.,
PROFESSOR IN THE NEW-YORK CITY UNIVERSITY, &c., &c., &c.

INTRODUCTION.

THE deposition of Prof. Joseph Henry (substantially the same in each of four Telegraph suits) has been erected by the ingenuity, and I must add, by the sophistry of the counsel of my opponents, into an apparently formidable barrier to my claims to originality in the invention of the Electro-Magnetic Telegraph.

While the facts he professes truthfully to state have been extravagantly dilated, and his position in relation to discoveries, alleged to have a bearing upon the Electro-Magnetic Telegraph, immoderately exaggerated; yet the fault of this extravagance lies not altogether at their door. Prof. Henry has himself furnished, by the ambiguity, as well as incorrectness of his narration, much of the specious material for their argument.

His deposition has been made to bear, not only on the legal points involved in my invention; but it contains imputations

against my personal character. For *insinuations* that I had derived scientific information from him, not only without *voluntarily* giving him credit; but that I had even persisted in *withholding* it after remonstrance on his part, and a promise on my part to do so; *insinuations* of the untruth of my declaration of the fact that "I had not a knowledge of his 'researches' at the time my invention had been demonstrated;" *insinuations* that my invention of the Receiving Magnet and combined circuits were surreptitiously taken from him. These surely have such a bearing on personal character, as not only to justify, but to demand from me an exposure of their utter want of foundation.

It behooves me, from motives of self-respect, no less than from regard to the opinion of a large body of scientific friends, to free my skirts from any such imputation of unfairness, as the tone and spirit of Prof. Henry's deposition inevitably leaves on the mind of the reader; a tone and spirit, which I am constrained to believe, has biased the after testimony of other witnesses, scientific men, enlisting for him their sympathies, and exciting prejudice in an equal degree against me; so coloring their evidence as to raise the suspicion, (at least in my own mind,) that under the influence of a mistaken impression, that I had been guilty of wrong towards Prof. Henry, they considered that I ought to suffer in kind by a summary application of the *lex talionis.*

Granting all credit for the *generous sentiment* which lies at the foundation of such action, however strongly I may protest against its *rash* application to me, I yet do not refuse to submit to any infliction which their dispassionate judgment may award, after an impartial examination of the merits of the case, being confident, from the ennobling nature of their pursuits, that a *wrong done to me*, will excite these same generous sympathies *for me*, as promptly as they have been excited *against me*, when they believed the *wrong was committed by me.*

It becomes, moreover, not less a duty to the cause of Historic truth, (a duty from which the position of my assailant no less than my own, will not permit me to shrink,) to expose as I shall be able to do, the utter *non-reliability* of Prof. Henry's testimony.

1st. I certainly shall show that I have not only manifested every disposition to give due credit to Prof. Henry, but under the hasty impression, (generated by personal respect, and friendliness towards him,) that he deserved credit for discoveries in science bearing upon the Telegraph, I did actually give him a degree of credit, not only beyond what he had received at that time from the scientific world, but a degree of credit to which *subsequent research* has proved him *not to be entitled.*

2d. I shall show that I am not indebted to him for any discovery in science, bearing on the Telegraph, and that all discoveries of principles having this bearing, were made not by Prof. Henry, but by others, and prior to any experiments of Prof. Henry in the science of Electro-Magnetism.

3d. I shall further show that the claim set up for Prof. Henry, (and with some plausibility, as if sanctioned by him in his deposition,) to the invention of an important part of my Telegraph system, has no validity in fact; and, construing his language to make him consistent with himself, is at variance with his positive disclaimers, and without support in his testimony.

4th. I shall further show that his complaint against me for an alleged injustice to him, in the work of another, (Mr. Alfred Vail,) has not only no foundation as against me; but that it has none as against Mr. Vail, and none whatever, in fact, against any one.

For more convenient reference, and also that the reader may have before him both sides of the matter in controversy, to examine at his leisure, I have given, in an Appendix, Prof. Henry's deposition in full as it appears in the case before the Supreme Court of the United States; and also Prof. Henry's paper, published in the XIX. Vol. of Silliman's Journal of Science; and in the body of my defence, I have also given some extracts from his cross-examinations upon the same deposition in other cases.

No one who reads Prof. Henry's testimony can fail to perceive that he has given it under the influence of some unhappy feeling, occasioned by real or supposed *wrong* to himself, a wrong not ambiguously charged upon me. This feeling manifests itself in the bringing into his deposition matters irrele-

vant, which have no legitimate bearing on the legal points of the several cases, except as they tend to disparage me in the opinion of the Courts, and of the public, as truthful, honorable and frank; and this he has done, I am sorry to say, but as I shall certainly prove, *not in ignorance of facts which make his statements incorrect.* I am aware that this is a serious charge, and no one can feel a deeper repugnance than myself at the necessity imposed upon me, for making and sustaining such a charge against a gentleman of Prof. Henry's high scientific and social position; a gentleman for whom I had cherished so profound a respect, as a deservedly eminent philosopher, and confidence, and even affection, as a friend.

That I have been slow to complain of the injurious character of his testimony; that I have so long allowed, almost entirely uncontradicted, its distortions to have all their legal weight against me in four separate trials, without public exposure, and for a space of four years of time; will at least show, I humbly contend, my reluctance to appear opposed to him, even when self-defence is combined with the defence of the interests of a large body of assignees.

But there is another reason which has had its weight with me. Nassau Hall, though not my Alma Mater, has had associated with it in my mind, from my earliest recollections, one of the deepest, and most sacred ancestral memories—memories which have given Princeton College, and all that is connected with it, a hold on my affections, scarcely less strong than that which attaches them to my own Alma Mater, the time-honored Yale. It was from a venerated ancestor who once presided over the College at Princeton, that I derive my own patronymic; from him whose Christian character was in such strong contrast to that of the infidel Hume, as to be chosen to illustrate most favorably the opposite effects of Infidelity and Christian faith upon the dying hour. And when, therefore, I reflected that Prof. Henry was, at the time of my acquaintance with him, and for so long a period connected as a valuable and efficient officer with this same institution, there are some who can understand the feelings which have hitherto restrained me. I have borne and forborne; but to forbearance there is a limit, for there is a paramount duty to see that the truth of History be

not violated, especially since the Telegraph has now become so largely a part of the history of science, and is directly associated with the progress of the world.

Painful, therefore, as is the task imposed upon me, I cannot shrink from it, but shall endeavor so to perform it, as rather to parry the blows that have been aimed at me, than to inflict any in return. If what I say shall wound, it shall be from the severity of the simple truth itself, rather than from the manner of setting it forth.

I am aware that Prof. Henry commences his deposition with a profession of reluctance to give his testimony; stating, indeed, clearly, that it was "not voluntarily" given, but "on legal summons and in submission to law;" manifesting thereby, as it appears to me, an unnecessary fastidiousness, if he was conscious of being about to give only the facts of history; for surely there is no one who could justly reflect upon him for giving facts; least of all was there required from him any special delicacy towards me in this particular; for I have no motive for desiring that he should conceal anything he knew. On the contrary, one of the most objectionable features of his deposition, in the part in which he makes *complaint*, is the concealment of important facts which essentially change its whole character. His citation to give evidence under oath, in a court of justice, ought undoubtedly to have made him specially cautious, both in the matter to which he deposes, and in the manner of propounding it, lest, by any means, his mode of stating truths or suppressing them, should give just ground for an untrue impression.

The subject-matter of the deposition of Prof. Henry contains:—

1st. Complaints against me of injustice towards him, in not acknowledging my indebtedness to him in elaborating my system of Telegraphs.

2d. Portions of scientific history misstated, so as to give an untrue impression.

3d. Claims to scientific *discoveries*, bearing essentially on my invention of the Telegraph.

4th. Claims, ambiguously expressed, to *invention* of parts of my Telegraph apparatus.

Under the *first* head, I shall have occasion to expose his incorrectness in giving controlling dates, his errors of memory, and his insinuations of untruthfulness in my statements.

Under the *second* head, his looseness and inaccuracy of historical narration.

Under the *third* head, the illusiveness of his claims to *discoveries* bearing on my invention of the Telegraph; and

Under the *fourth* head, the invalidity of any seeming claims to *invention*, on his part, of any portion of my Telegraph apparatus.

These fallacies and errors, interspersed throughout his whole deposition, will require me necessarily to occupy considerable space in exposing them; and in separating the true from the false, I am compelled to be thorough in the exposition of all that is essential, even at the risk of being thought prolix.

From the way in which Prof. Henry alludes to the intercourse between us, and especially from the erroneous conclusions of my having derived indispensable aid in my labors from him, which have been suggested in consequence, I am compelled to narrate, with some minuteness of detail, *the circumstances* which led to our acquaintance; to give the *time* of that acquaintance; and also the *nature* and *extent* of our intercourse.

To begin with our *first acquaintance.*—My acquaintance with Prof. Henry commenced in the month of May, 1839, under the following circumstances:—In the spring of 1838 I visited Europe with my invention, and resided abroad, principally in Paris, till March, 1839, returning home in April of the same year. While absent, my friend and colleague at the New-York City University, Dr. Gale, who had charge of my apparatus in my absence, loaned to Prof. Henry a reel or spool containing five miles of my telegraph wire, with which Prof. Henry made some interesting scientific experiments at Princeton. The results of these were reported by him in a paper read before the American Philosophical Society, November 2d, 1838, and published early in 1839, under the title of "Contributions to Electricity and Magnetism." On my return from Europe, I found awaiting me a copy of Prof. Henry's "Contributions," directed to "Prof. Morse, with the respects of the author." I had returned from Europe in the expectation of proceeding

within five or six weeks to Russia, under a contract with a Russian government agent in Paris—the Baron Meyendorff—to establish the Telegraph in that country. Dr. Gale, my confidential scientific friend, had sailed for New-Orleans on the very day of my return. I could not therefore have my usual consultations with him, for I was naturally anxious to review and revise all the scientific facts that lay at the foundation of my invention, to make assurance doubly sure, before risking in a foreign country either my own or my country's reputation by possible failure. In this conjuncture I wrote the following letter to Prof. Henry, to whom I had never been introduced, availing myself of his politeness in sending me his "Contributions" as a sufficient introduction:—

NEW-YORK, *April 24th*, 1839.

MY DEAR SIR:—On my return, a few days since, from Europe, I found directed to me, through your politeness, a copy of your valuable "Contributions," for which I beg you to accept my warmest thanks. The various cares consequent upon so long an absence from home, and which have demanded my more immediate attention, have prevented me from more than a cursory perusal of its interesting contents; yet I perceive many things of great interest to me in my Telegraphic enterprise. I was glad to learn, by a letter received in Paris, from Dr. Gale, that a spool of 5 miles of my wire was loaned to you, and I perceive that you have already made some interesting experiments with it. In the absence of Dr. Gale, who has gone south, I feel a great desire to consult some scientific gentleman on points of importance bearing upon my Telegraph which I am about to establish in Russia, being under an engagement with the Russian Government Agent in Paris to return to Europe for that purpose in a few weeks. I should be exceedingly happy to see you, and am tempted to break away from my absorbing engagements here to find you at Princeton. In case I should be able to visit Princeton for a few days, a week or two hence, how should I find you engaged? I should come as a learner, and could bring no "contributions" to your stock of experiments of any value, nor any means of furthering your experiments, except, perhaps, the loan of an additional 5 miles of wire which it may be desirable for you to have.

I have many questions to ask, but should be happy in your reply to this letter of an answer to this general one. Have you met with any facts in your experiments, thus far, that would lead you to think that my mode of Telegraphic communication

will prove impracticable? So far as I have consulted the savans of Paris, they have suggested no insurmountable difficulties. I have, however, quite as much confidence in your judgment, from your valuable experience, as in that of any one I have met abroad. I think that you have pursued an original course of experiment, and discovered facts of more value to me than any that have been published abroad.

I will not trouble you at this time with my questions until I know your engagements. Accompanying this is a copy of a report, made by the Academy of Industry, of Paris, on my Telegraph, which I beg you to accept.

Believe me, dear Sir,
With the highest respect,
Your most obedient servant,
SAMUEL F. B. MORSE.

To PROFESSOR JOSEPH HENRY, *Princeton.*

To this letter I received the following reply:—

PRINCETON, *May 6th*, 1839.

DEAR SIR:—Your favor of the 24th ult. came to Princeton during my absence, which will account for the long delay of my answer. I am pleased to learn that you fully sanction the loan which I obtained from Dr. Gale, of your wire, and I shall be happy if any of the results are found to have a practical bearing on the Electrical Telegraph.

It will give me much pleasure to see you in Princeton after this week; my engagements will not then interfere with our communications on the subject of electricity. During this week I shall be almost constantly engaged with a friend in some scientific labors which we are prosecuting together.

I am acquainted with no fact which would lead me to suppose that the project of the Electro-Magnetic Telegraph is impracticable; on the contrary, I believe that science is now ripe for the application, and that there are no difficulties in the way, but such as ingenuity and enterprise may obviate. But what form of the apparatus, or what application of the power will prove best, can, I believe, be only determined by careful experiment. I can say, however, that so far as I am acquainted with the minutiæ of your plan, I see no practical difficulty in the way of its application for comparatively short distances; but if the length of the wire between the stations be great, I think that some other modification will be found necessary, in order to develop a sufficient power at the farther end of the line. I shall, however, be happy to converse freely with you on these points when we meet. In the meatime I remain,

With much respect, yours, &c.,
To PROFESSOR MORSE. JOSEPH HENRY.

Thus was prepared my first personal acquaintance with Prof. Henry. A few days after the receipt of this letter, I visited him, having prepared beforehand a few questions, the better to economize his time. The following is a copy of the original paper, (which I preserved,) with the answers of Prof. Henry, so far as they were given, put down by me in pencil at the time:—

Questions prepared to ask Prof. Henry, and shown him in my visit May, 1839, and his answers, on reading them to him.

1st. Have you any reason to think that magnetism cannot be induced in soft iron, at the distance of 100 miles or more, by a single impulse, or from a single battery apparatus? * "No."

2d. Suppose that a horse-shoe magnet of soft iron, of a given size, receive its maximum of magnetism by a given number of coils around it, of wire, or of ribbon, and by a given sized battery, or number of batteries, at a given distance from the battery, does a succession of magnets introduced into the circuit diminish the magnetism in each? "No."

3d. Have you ascertained the law which regulates the proportion of quantity and intensity from the voltaic battery, necessary to overcome the resistance of the wire in long distances, in inducing magnetism in soft iron? "Ohm has determined it."

4th. Is it quantity or intensity which has most effect in inducing magnetism in soft iron? "Quantity with short, intensity with long wires."

According to the best of my recollection, I left New-York a few days after the receipt of his letter, in the morning of the day, and arrived at Princeton about noon, passed the afternoon and night with Prof. Henry, and returned to New-York the next morning. This was the first intercourse, either personal, or by correspondence, I ever had with him; and pausing a moment at this point, it may be well to inquire, how much of information I could have received from him *personally* or by *correspondence*, in the elaborating of my Telegraph as it then existed?

* That is, from any generator of the galvanic current, whether a voltaic, inductive, or thermo-electric apparatus.

From the autumn of the year 1829 till the autumn of the year 1832, I was in Europe, principally in Italy. Prof. Henry's paper in Silliman's Journal was published in 1831,* (in the interval of my absence from America.) I conceived the Telegraph on board the ship in 1832, while on my return home, *essentially* as it now exists. It was operated in my rooms before numerous persons, my pupils, and others, in 1835; it was exhibited to a large audience of a thousand or more persons, through ten miles of wire, in the New-York City University, in the autumn of 1837; to a committee of the Franklin Institute in Philadelphia, in January, 1838; to Congress and the Cabinet at Washington, for three months, in the early part of that year; to the Academy of Sciences, and thousands of visitors in Paris, in the autumn of 1838; to Members of the Royal Society, of both Houses of Parliament, and the Lords of the Admiralty at Lord Lincoln's,† in London, in the month of March, 1839; *and after all this*, I first became acquainted, either personally, or by correspondence, with Prof. Henry.

Here then is a controlling date, (1839,) proved to be most incorrectly deposed to by Prof. Henry (as 1837). It is a date which controls the question of fact, whether I could have derived from him any information directly by *personal interview*, or *correspondence* with him, leaving but two other sources from which aid from him could be derived by me in the elaboration of my Telegraph.

There are but three sources whence I could derive scientific information from Professor Henry.

1st. *Directly* by personal interview, or by correspondence.

2d. *Indirectly* through others.

3d. From his published works.

The first, I think, is effectually disposed of. The other sources will be examined in the sequel.

While on the subject of erroneous dates given in the deposi-

* Before examining this paper, let me for a moment grant, for argument's sake, that it is (what some would claim for it) suggestive of my Electro-Magnetic Telegraph, what are the *probabilities* that it could reach me in Italy or France, where I was then residing, so as to influence my mind in the conception and construction of the Telegraph on my return voyage in 1832? The probabilities, it will be seen, are certainly very small. The *fact* is, it did not come to my knowledge until *five years after* my return, (in 1837.)

† The present Duke of Newcastle.

tion, I proceed to notice others, illustrative of the incapacity of Prof. Henry, from defective memory, to depose correctly where dates are essential to elicit the truth.

In connection with his declaration that, in 1837, he became acquainted with me in New-York, (instead of 1839 in Princeton,) he says that he "*gave me a certificate in the form of a letter.*" I give the *certificate letter* thus referred to, *and its date*, referring the reader to his deposition, answer to 4th interrogatory, for his narration on this point.

"PRINCETON COLLEGE, *Feb. 24th*, 1842.

"MY DEAR SIR:—I am pleased to learn that you have again petitioned Congress, in reference to your Telegraph, and I most sincerely hope you will succeed in convincing our Representatives of the importance of the invention. In this you may, perhaps, find some difficulty, since, in the minds of many, the Electro-Magnetic Telegraph is associated with the various chimerical projects constantly presented to the public, and particularly with the schemes, so popular a year or two ago, for the application of electricity as a moving power in the arts. I have asserted from the first, that all attempts of this kind are premature, and made without a proper knowledge of scientific principles. The case is, however, entirely different in regard to the Electro-Magnetic Telegraph. *Science is now fully ripe for this application*, and I have not the least doubt, if proper means be afforded, of the perfect success of the invention.

"The idea of transmitting intelligence to a distance, by means of electrical action, has been suggested by various persons, from the time of Franklin to the present; but until within the last few years, or since the principal discoveries in electro-magnetism, all attempts to reduce it to practice were necessarily unsuccessful. The mere suggestion, however, of a scheme of this kind, is a matter for which little credit can be claimed, since it is one which would naturally arise in the mind of almost any person familiar with the phenomena of electricity; but the bringing it forward at the proper moment, when the developments of science are able to furnish the means of certain success, and the devising a plan for carrying it into practical operation, are the grounds of a just claim to scientific reputation as well as to public patronage.

"About the same time with yourself, Prof. Wheatstone, of London, and Dr. Steinheil, of Germany, proposed plans of the Electro-Magnetic Telegraph; but these differ as much from yours as the nature of the common principle would well permit; and unless some essential improvements have lately been made

in these European plans, I should prefer the one invented by yourself.

"With my best wishes for your success, I remain, with much esteem, Yours truly,

(Signed) "JOSEPH HENRY.

"PROFESSOR MORSE."

It will be perceived that Prof. Henry has misrecollected the time and circumstances of our first acquaintance, making the date 1837, in New-York, instead of 1839, in Princeton, and he leaves the impression that the time of giving me his "certificate" letter was in 1837, instead of 1842, the true date, as the letter itself shows.

But while on this topic I may as well fix certain other dates which have a bearing on Prof. Henry's qualifications for a deponent. Prof. Gale, my colleague, left the University for the South in the spring of 1839. Prof. Draper had been appointed his successor in the Chair of Chemistry in September, 1838; but, being unable at once to enter upon the duties of his office, Prof. James C. Fisher received a temporary appointment in January or February of 1839. On my return from Europe, in April, 1839, I found Mr. Fisher in the University, and then made my first acquaintance with him. I had never seen or known him previously. The bill before Congress making an appropriation to test the practicability and utility of my Telegraph, passed on the 3d of March, 1843. My scientific assistants, in preparing the Telegraph, could not well have been appointed *previous* to the passage of this bill. Prof. Gale, Prof. Fisher, and Mr. Vail received their appointment from me, as my assistants, March 27, 1843; and they were confirmed by the Secretary of the Treasury March 28, 1843. The records of the Treasury Department will verify these dates. Let Prof. Henry's narrative now be compared with these facts. He says:—"I learned in 1837, or thereabouts, that Prof. Gale and Dr. Fisher were the scientific assistants of Mr. Morse in preparing the Telegraph. Mr. Vail was also employed, but I know not in what capacity, and I am not personally acquainted with him." Is not here further proof of Professor Henry's *incorrectness in giving dates*, and *looseness and inaccuracy of historical narration?*

As intimately connected with this misrecollection of dates, I must expose another of a more serious character, since it involves a charge against me of deriving from him advice against subterranean conductors, and the adoption of the plan of putting my conductors on posts at his suggestion and recommendation.

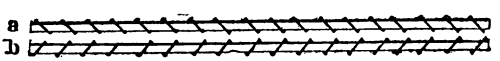
To show which of the two plans of disposing of the conductors was occupying Prof. Henry's mind in April, 1843 (the one in tubes beneath the ground, or the one on posts through the air), I give the following letter from Prof. Henry to Prof. Fisher, who was then my assistant, and who had written to Prof. Henry in relation to the conductors:—

"PRINCETON, *April 17th*, 1843.

"DEAR SIR:—A friend of mine in Trenton has a machine for winding wire, of which he promised to give a description. I will write to you on the subject, and send you a copy of his answer.

"The greatest practical difficulty you will have to contend with, I should think, will be the insulation of the wires. Twine is a partial conductor, and by making the surface sufficiently extended, lateral transmission will take place to some extent. The loss, however, on this account can only be determined by direct experiment with extended wire. It will probably increase with an increasing ratio: first, on account of the greater surface of contact; and secondly, because electricity of greater tension will be required to send the current through the longer wire.

"In order to diminish the number of points of contact, it might perhaps be well to wrap around each wire, besides its continuous covering, an extra strand of coarse twine, with the several turns at a distance from each other. Thus—



a and b representing the two wires, the contact, instead of being continuous, will only exist at the points where the outer strands of twine touch each other.

"Galvanic electricity has never been made to project itself through a stratum of air of the ordinary density, so as to exhibit a spark, although the experiment has been tried with a battery of several hundred plates, and with the poles approached within the $\frac{1}{1000}$th part of an inch. I should therefore conclude that it would be of little importance to fill the space between the wires with cement, provided the metal can be as well secured from contact by less expensive means.

"I shall probably visit New-York some time during the present college vacation, and shall not fail to accept the kind invitation of Prof. Morse to visit your establishment, and perhaps make some experiments with your long wires.

"Respectfully yours, &c.,

(Signed,) "JOSEPH HENRY.

"D. FISHER, *Professor, &c., &c.*"

The plan of preparing my conductors for the Telegraph, at the date of the preceding letter, it will be perceived, was that of inclosing them in *tubes of lead, to be interred in the earth;* and it will be observed that Prof. Henry does not in this letter (where it would be in place, if he had had any doubts of the success of conductors thus laid) suggest anything in regard to putting the conductors on posts.

It will not be out of place here to explain the reason of my adopting at that time the mode of *interring* the conductors, in preference to the present mode on posts, both of which modes (see Vail's book, p. 71) are proposed in my letter to the Secretary of the Treasury of September 27th, 1837, long previous to my acquaintance with Prof. Henry. Both plans had occupied much of my thought previous to the passage of the bill in 1843, for the government experimental essay. In the early attempts to establish the Telegraph in England, as well as here, it was a matter of doubt, and to be determined only by experience, which of the two modes would prove the best; and it may be remarked in passing, that at this day, in Russia and in other European countries, the mode of interring the wires is extensively adopted.

At the time the Telegraph bill passed, in March, 1843, there had been about thirteen miles of Telegraph conductors, for Prof. Wheatstone's Telegraph system in England, put into tubes and interred in the earth; and there was no hint publicly given that that mode was not perfectly successful. I did not feel, therefore, at liberty to expend the public moneys in useless experiments, on a part which seemed to be already settled by Prof. Wheatstone from his adopting that part, and, so far as I could learn, with success. Hence, I fixed upon this mode as one settled; and it was prosecuted until the winter of 1843–44. It was abandoned, among other reasons, in consequence of as-

certaining that in the process of inserting the wire into the pipe, (which was at the moment of forming the pipe from the lead at melting heat,) the insulating covering of the wires had become charred at various and numerous points of the line, to such an extent that greater delay and expense would be necessary to repair the damages than to put the wire upon posts. In this state of affairs, I recurred to my other mode proposed, to wit, *putting my conductors on posts*, (the mode now in use;) and in a letter dated Washington, February 2d, 1844, I asked the permission of the Secretary of the Treasury to try the mode upon posts, as far as Beltsville, twelve miles from Washington, towards Baltimore, which was accorded; and sealed proposals for seven hundred posts were advertised for by me, in the *Madisonian* and in the *National Intelligencer* of February 6th, 1844.

On February 7th, 1844, I went to Baltimore, on my way to New-York. At Baltimore I found awaiting me at the Depôt the following:—

Letter from Prof. Henry, dated Princeton, Jan. 24th, 1844, post marked Jan. 26th, directed "Professor Morse, Electrical Telegraph Depot, Baltimore."

"PRINCETON, *Jan. 24th*, 1844.

"MY DEAR SIR:—I am anxious to hear from you, in reference to the Telegraph, and I have intended to write to you on the subject for a month past, but extra college duties have occupied all my thoughts and all my time, since the beginning of the present term. During the last vacation I occupied myself as usual with my investigations in electricity, and among other results, I arrived at one which I think may have an important bearing on the success of the Telegraph. It is this: while a current of electricity is passing through a wire, one part of the conductor is constantly plus to any other part which succeeds it, the difference in the degree of the electrical state constantly increasing as the distance of the two points is greater. The maximum difference is, therefore, at the two ends; and when the two extremities of a long wire are brought into near approximation, there is a great tendency in the electricity to cut across from the one to the other. This tendency is not due, as has been supposed, merely to the great resistance of the long wire, and the cross-cut offering a less resisting channel; but to the fact of the one part being positive, and the other negative,

and the consequent great attraction of the electricity in the one part for the unsaturated matter in the other.

"According to this view, the insulation which would be sufficient to stop a current when applied to separate two consecutive portions of the same wire which had been divided, would be entirely insufficient to prevent the cutting across. I consider the fact I have stated as one of considerable importance in a theoretical point of view, and I do not wish it to be made public before the publication of my next paper in the Transactions of the American Philosophical Society.

"On reading your letter, on the subject of the Telegraph, in the newspapers, I was struck with the idea that you had probably met with the very difficulty my researches have led me to anticipate. If this is the case, and your insulation is not found sufficient, you have no cause to blame yourself, since the previous state of knowledge on the subject of electricity could not lead you to suspect such a condition of things.

"With much respect,

"Yours truly,

(Signed) JOSEPH HENRY.

"PROFESSOR MORSE."

From this letter it will be perceived that Prof. Henry had his mind occupied at that date with the *conductors in tubes*, predicting "cross-cut," &c., and no suggestion is given of *conductors upon posts*. From the commencement of the letter it will also be seen that for at least *a month before* its date, he could not have suggested to me to put my *conductors on posts*, as he had not written me before on the subject.

Immediately on receipt of this letter, and whilst waiting at Baltimore over night, I hastily wrote the following reply:—

"BALTIMORE, *Feb. 7th*, 1844.

"MY DEAR SIR:—You must think it strange that I have not answered your letter of the 24th ult. before this; but I have this moment received it, in passing through this place on my way to New-York, which I trust will be a sufficient apology for my apparent neglect. I have read your letter with much interest, and it has determined me to make you a visit on my return from New-York, which will be the beginning of the week, perhaps on Tuesday morning, 13th inst. If anything in New-York should prevent me from being with you then, I will drop you a line on Monday morning. Several questions occur to me in this *hurried* moment, (as I leave in the morning and carry this

with me to Princeton, on my way to New-York,) suggested by your results.

"1st. How does the result you have arrived at affect the experiments made on my 80 reels of wire of 160 miles?*

"2d. What distance apart must the wires of a circuit be to prevent the cross-cut? Will *any* insulation in a tube prevent it?

"3d. If my conductors are placed on poles, suspended in the air, will there then be any danger of cross-cut?

"4th. How is it that Wheatstone (who has extended in tubes his conductors for 20 miles or more) has not discovered this difficulty of cross-cut?

"I found the difficulty † which you apprehend, in the insulation of my wires; but this I will explain when I have the pleasure of seeing you.

"In the meantime believe me,

"With sincere respect,

"Your most obedient servant,

"SAMUEL F. B. MORSE.

"*To* PROF. JOSEPH HENRY, *Princeton.*"

With these facts substantiated by letters and dates, I will ask attention to the following passage in Prof. Henry's deposition. In speaking of his experiments, in *repeating Steinheil's earth-circuit*, he says, "The exact date of these experiments I am unable to give without reference to my notes. They were previous, however, to the unsuccessful attempt of Mr. Morse to transmit currents of electricity through wires, buried in the earth, between Washington and Baltimore, and before he attempted to use the earth as a part of the circuit. Previous to this time, and after the above-mentioned experiments, Mr Morse visited me at Princeton, *to consult me on the arrangement of his conductors.* During this visit we conversed freely on the subject of insulation and conduction of wire. *I urged him to put his wires on poles*, and stated to him my experiments and their results."

How stands the matter, then, in regard to his suggestion, or "*urging me to put my wires on poles*"? It is in evidence, and in public documents, that this mode "on poles," was one of the modes

* No cross-cut was noticed in this distance of 160 miles, a circumstance which militated against the theory deduced by Professor Henry, from his experiments.

† That is, "cross-cut;" but this was owing, as I afterwards explained to him, from the injury to the insulation by charring, which produced conduction.

I proposed to the Secretary of the Treasury as early as Sept., 1837, at least a year and a half before my acquaintance with Prof. Henry. Nor could I have been influenced by any correspondence, or conversation with Prof. Henry, in the visit to which he alludes, (and which was, as is shown, about the 13th February, 1844,) to try the mode on poles, after having abandoned the mode of *burial in the earth.* I had not only determined on trying this mode on poles, before the visit referred to by Prof. Henry; but it is seen that I had requested leave, February 2d, 1844, of the Secretary of the Treasury, to adopt that mode, as one long since proposed by me; and I had actually advertised, with the sanction of the Secretary, for the delivery of the posts, *a week before* I made the visit, and had the conversation, in which he says, "I *urged him to put his wires on poles.*"

How is it in regard to *Steinheil's earth-circuit*, which Prof. Henry intimates, I adopted in consequence of its being made known and recommended by him to me? On this it will be only necessary to state that the memoir of that distinguished and able savan, Prof. Steinheil, in which he first announces publicly his "*earth-circuit,*" was read in my presence and hearing, to the Academy of Sciences, in Paris, at their Session, September 10th, 1838, (the same Session in which I explained to that body my Telegraph, and showed it in action;) and this memoir was in my possession the week after in the Comptes Rendus of that Session, published side by side with the description of my own Telegraph, and, therefore, was necessarily known to me long before it could have been known to Prof. Henry. My adoption of Steinheil's earth-circuit (which I need not say I never pretended to claim as my discovery) was not in consequence of any conversation or recommendation from Prof. Henry.

This visit to Prof. Henry was the *second*, and *last* I ever made him at Princeton.

I think I have conclusively shown that I could have derived from Prof. Henry no aid, in *personal interview*, or *by correspondence*, in the invention of my Telegraph. How is it in regard to the second source of information, to wit: *Indirectly through others?*

It is strongly hinted by Prof. Henry, [answer to 4th interroga-

tory,] that, since Dr. Gale, who was my early associate, obtained his knowledge of electro-magnetism from him, I therefore, *indirectly*, through Dr. Gale, obtained his scientific discoveries bearing on the Telegraph. Plausible as this may seem, facts disprove it utterly.

The condition of my Telegraph, previous to the time of my acquaintance with Prof. Henry, in brief, was this. In 1832, I had conceived and planned it in its leading features as a Recording Telegraph. In 1835, it was completely embodied as such, in a practicable form, essentially as it now exists. It operated to mark characters upon a strip or ribbon of paper; moved by clock-work, by means of electro-magnetism excited in an electro-magnet by a galvanic battery. Thus, much had been accomplished even before my acquaintance with my friend and colleague, Dr. Gale. Dr. Gale became acquainted for the first time with the Telegraph when it was in this condition, "*in January*, 1836."* To this fact he testifies in his deposition. He also testifies†—"It was in perfect order when I first saw it." And again‡—"Said Morse could have successfully operated his Telegraph, and did so operate it, prior to my communicating to him any scientific intelligence." Whatever of scientific intelligence, therefore, bearing upon the Telegraph, had ever been communicated to Dr. Gale, by Prof. Henry, either personally, by correspondence, or through his "published researches," could not possibly have been available to me through Dr. Gale, as Prof. Henry intimates, at the time my Telegraph was in actual operation in its first practical embodiment.§

It may be here remarked, that, whatever improvement Prof. Henry claims to have made at this time in the *magnet*, I found the electro-magnet which I use, substantially in its present form in public use, as I had found the galvanic battery and various mechanical elements; knew its principles and action in 1827, as I shall prove; and applied it in combination and with additions, to accomplish my purpose, in the same way as I applied other agencies, without going into the records of its paternity. It was known only as *the electro-magnet*,‖ and I never called it mine.

* S. Court Record, p. 713. † Philadel. Case, Comp. Ev., p. 442. ‡ Ibid, p. 441.
§ See note on last line of answer to 4th interrogatory of Henry's Deposition.
‖ The instruments called *Morse's Register Magnet* and *Morse's Receiving Mag*

When, at a later period, it was stated by certain scientific genealogists who professed to have searched the record, that Prof. Henry had improved the magnet, I neither manifested any disbelief of the fact, nor reluctance to concede to him any credit he might deserve on that account. Nay, more (as I shall show before I close), I awarded to him, under a mistaken apprehension of the real nature of his improvement, more than subsequent research proves him to be entitled to. Besides, what motive could I have in depriving him of any such credit, if the electro-magnet I used was his invention, since I never claimed its invention for myself? It was no sacrifice on my part to give the electro-magnet, if he invented it, to Henry, in preference to Arago or to Sturgeon. I have, indeed, applied the electro-magnet (but not his electro-magnet,) in combination with other discoveries and inventions of others, which have at various periods been given to the public as materials for combination to create inventions. Whatever Prof. Henry's discoveries may have been, he gave them to the public for that purpose; and granting, therefore, that he has made important discoveries, surely it is a singular cause of complaint on his part that the public make use of them.

But granting for the moment, that any discoveries claimed for Professor Henry, in the articles in Silliman's Journal, have in reality an important bearing on the invention of the Electro-Magnetic Telegraph, which I do not concede, these are based on the discoveries and inventions of others, some of which have a much more comprehensive application than any he assumes to have discovered; some, indeed, holding and including all that is claimed for him, and all that he claims as his own, within them, and without which those he would claim could not have existed. For example, where would have been Henry's assumed discoveries, if Volta had not made practical for him the discovery of Galvani, by the invention of the pile, the first

net, are names given to instruments with magnets in combination with other mechanical devices, fitted up by me to accomplish respectively a particular purpose, to wit, *registering* and *receiving*. The compound word designates a combination, in which the magnet is, indeed, an important part; but registering and receiving are the *purposes* severally of the combination, and give the qualifying name to the combination, as pen-lever designates a combination; the lever being an important part, but the pen in this case designating the peculiar purpose of the combination.

intensity battery; and Cruikshank had not furnished his improvement upon Volta, in the battery known by his name, the battery with which Henry made his experiments; or Oersted had not proclaimed for him the birth of Electro-Magnetism, the science in which Henry was experimenting; or Schweigger had not furnished him with the principle of his multiplier; or Arago had not given him the original Electro-Magnet, or Sturgeon his improvement upon it; or Ohm previously determined for him the law of propulsion of the galvanic current? And when, by the aid of the previous discoveries and inventions of so many distinguished philosophers before him, he had added what he claims to be a discovery on his own part, did the Electro-Magnetic Telegraph then stand revealed to the world, the practical, useful instrument we see it to be? Was nothing more to be done? Can Prof. Henry say, "Go read my papers of 1831, and there you will find the Electro-Magnetic Telegraphs of Morse, or of Wheatstone, a telegraph ready to do all that these respectively accomplished, the former in 1835, and the latter in 1837? No. There was no such thing. The Telegraph lay long after 1831, as it had lain before, an inoperative, lifeless, disjointed thing: it had a language to learn; it needed a tongue to talk, and fingers to write; it needed some one yet to conceive a whole; to collect the scattered materials dispersed through the records of years of the laborious research of eminent men, in different sciences and arts. These were to be brought together, to be digested and arranged for a specific purpose, before the Telegraph could be born. Without this gathering, without this application, this combination of parts known, to the production of a new art, and of course to a new process in the arts, there would have been no Electro-Magnetic Recording Telegraph. These various principles were not brought together by Prof. Henry, and applied for this or any other purpose in the arts. His discovery (supposing still that there is such a discovery) was but one in the combination with many other discoveries and inventions quite as necessary as his own to make up the present practical commercial Telegraph. Nor was his alleged discovery the only one remaining to be made, to effect the end; for its *present economical condition* is due to my inventions, and to the subsequent improvements of Professors

Daniel and Grove in the Voltaic battery. But who did bring all these parts together, and by one combination create one consistent operative whole? If Prof. Henry did it not himself, he may yet be permitted to say who did.

It was "he" (whoever he may be) "who brought the idea forward at the proper moment when the developments of science were able to furnish the means of certain success, and devised a plan for carrying it into practical operation." These acts, he says, are "the grounds of a just claim to scientific reputation as well as to public patronage." [Professor Henry's letter to Professor Morse of February 24th, 1842.]

In the argument under this head, I have assumed, thus far, that Professor Henry had actually made discoveries having an important bearing on the Telegraph; and that, even under this aspect of the case, I have shown that I could neither have been acquainted with them *directly* through personal intercourse or correspondence with him, or *indirectly* through Dr. Gale, as he distinctly intimates in his deposition.

The third, and only remaining source whence I could have derived any such information of his alleged discoveries, must be from his *published works*. To these, his "published researches," he constantly refers in the deposition, and these I now proceed to examine.

PROFESSOR HENRY'S "PUBLISHED RESEARCHES."

To ascertain what Professor Henry had discovered, bearing on the Telegraph, previous to my invention, I will now examine his "published researches." The paper in the Albany Transactions (1830), and the two papers in Silliman's Journal of 1831, Vols. XIX. and XX., are the only legitimate documents of his having any bearing on the subject; they are all that was known publicly of his experiments previous to my invention; all that he had then published. The year 1835, not 1849, is the proper stand-point for examining Professor Henry's claims to discoveries bearing on the Telegraph. His paper in Volume XIX. is the one principally relied on as containing the alleged discoveries. It gives a detail of certain experiments to solve a certain problem, to wit: How to obtain "*great magnetic power with a small galvanic element.*" This purpose is declared in the

title of the paper, and is the declared object and result of *twenty-eight* out of the thirty experiments there recorded. He succeeded in his object. By winding his magnet with numerous short coils of conductors, instead of one long wire, as had been usual previously, and uniting the similar poles of each coil into one conductor, uniting the two coil poles at the battery poles, he made what he now styles his *Quantity Magnet*, and produced a greater magnetic power than had anywhere been attained previously in the Electro-magnet. For this success, he gained deserved credit at home and abroad. The term "Quantity Magnet," however, it should be observed, is of very recent origin, as well as the term "Intensity Magnet." The latter is a new name given to the common Electro-Magnet. They are terms first used by Professor Henry, and first by him in the evidence given in his deposition of 1849. He employs them in 1849 in describing his experiments of 1831, to distinguish two different modes of using the helix or coil of the Electro-magnet, the one with a long continuous wire in connection with the common intensity battery, and the other with numerous short wires in connection with a quantity battery. The magnet thus called Intensity Magnet by Professor Henry, is the invention originally of M. Arago, who first used the helix of one continuous long wire round a soft iron bar. Mr. Sturgeon, in 1825, improved upon the Electro-magnet by a longer coil, an improvement deduced as the result of the previous discoveries of Oersted, Schweigger, and Arago. Oersted in 1820 had discovered the magnetic effect of a current of voltaic electricity in a straight conducting wire. This was the embryo of the science of Electro-Magnetism, as Galvani's discovery was the embryo of the science of Galvanism.*

Schweigger improved upon the *primary element* of Oersted, just as Volta had done upon his own *primary element* of a single pair, and demonstrated that the magnetic effect of the current was increased by repeating the primary element, and hence resulted his celebrated *multiplier*, increasing the intensity of mag

* The condition of the simple instrumentality of Oersted's discovery, as he left it, is very analogous (in its relations to its after applications) to the single voltaic element, in its relations to the galvanic or voltaic battery. That is, it was the primary element for producing electro-magnetic effects, as the latter was in producing electrical effects.

netism as the repetiton of the galvanic or voltaic pairs increases the intensity of galvanism. Arago, reflecting on the multiplier of Schweigger, constructed the helix of the electro-magnet, and first demonstrated the practicability of magnetizing soft iron, and also steel bars within it; and then Mr. Sturgeon, applying the helix of Arago, on the principle of Schweigger, to the soft iron bar, adding *more convolutions* to the helix, bending the iron bar into the horse-shoe form, to give it the advantage of the proximity of the two poles as in the permanent horse-shoe steel magnet, produced the first form of the now *common electro-magnet.*

This was the condition of the Electro-Magnet when Prof. James Freeman Dana, in public lectures in New-York, before the New-York Athenæum, in 1827, exhibited to his audience all the principles of the present common electro-magnet, that which Prof. Henry now calls the "intensity magnet." He showed experimentally the magnetic effect of the galvanic current.

1st. In a *single straight* conjunctive wire.

2d. In a conjunctive wire bent into the *form of a ring.*

3d. In a *single spiral*, or helix around a piece of soft iron, bent into a horse-shoe form, (Sturgeon's improvement.)

4th. He showed the increased magnetic effect of the helix when formed with a *volute spiral*, the present mode of forming the helices of the electro-magnet, that is, with the wire superposed or wound upon itself.

In another part of this defence, I have made a large extract from Prof. Dana's manuscript lectures, (in evidence before the Court,) in which the above experiments are given in detail, with their rationale.

From that date to the present, there has been no improvement in the *electro-magnet proper*, having a bearing on the Electric Telegraph. The helices of the magnet employed in my Telegraph, from its inception to the present time, are made precisely on the principle demonstrated, and in the mode indicated, by Prof. Dana, in 1827.

Prof. Henry, subsequently, in 1829, made an improvement in the lifting power of the electro-magnet, which he gave to the

world in 1831. To this improvement he gave, in 1849, the name of *quantity magnet.*

The mode of forming this magnet, and the details of his experiments with it, are the main topic of his paper of 1831, and are in accordance with the proposed object of it, as standing out upon its title; in accordance with the object, as stated in the closing remarks of his paper, in which he says, "the principle object in these experiments was to produce the *greatest* magnetic effect with the *smallest* quantity of galvanism;" and in accordance with the fact that the quantity magnet is illustrated by the *only diagram* of the paper. It is the success of his experiments with this *quantity* magnet, as I shall show, that created any interest among the scientific world, so far as I have been able to discover. But this *quantity* magnet has no bearing whatever on the Telegraph, and has never been used, so far as I can learn, by any one as an instrumentality in the Telegraph; certainly not by me, and is wholly *unnecessary* for the purpose.

The only fact proclaimed in that paper, bearing on the Telegraph, is not that a *powerful magnet*, nor indeed any magnet at all, could be made at a distance. It "relates solely to the *propulsion of a current through a long wire*, if not with an *increased* effect," at least with "*but slightly diminished effect.*"

In one of his experiments "to determine to what extent *the coil* could be applied, in developing magnetism in soft iron, and also to ascertain the most proper length of the wires to be used," his attention was drawn to the fact, not new, that the Cruikshank's battery, of 25 pairs, propelled the current more effectively through a long wire, than did a single galvanic element, or pair. Hence he drew this inference: "the fact that the magnetic action of a current from a trough is *at least* not sensibly diminished by passing through a long wire, is directly applicable to Mr. Barlow's project of forming an Electro-magnetic Telegraph." This is the inference bearing upon a Telegraph, and the *only remark* of his bearing upon a Telegraph, published by him *in this paper, or anywhere else*, previous, at least, to the year 1838.

But what was "Barlow's project of an Electro-magnetic Telegraph?" He had none. Prof. Henry states, in his deposition, that he was in error on that point. Barlow, in reference to a

suggestion that an instantaneous Telegraph might be established, was induced to make some experiments, to ascertain if the only difficulty which he apprehended to its realization, existed or not. His language is, "It had been suggested that an instantaneous Telegraph might be established, by means of conducting wires and compasses," (that is, magnetic needles.) * * "*There was only one question which could render the result doubtful*, and this was, *Is there any diminution of effect by lengthening the conducting wires?* It had been said that the electric fluid from a common electrical battery had been transmitted through a wire, four miles in length, *without any sensible diminution of effect, and to every appearance instantaneously;* and *if this should be found to be the case with the galvanic circuit*, then no question could be entertained of the practicability and the utility of the suggestion adverted to." Barlow adds, "I was, therefore, induced to make the trial; but I found such a sensible diminution, with only 200 feet of wire, as at once to convince me of the impracticability of the scheme."

The *only question* (so believed to be by Barlow) respecting the practicability of establishing an Electric Telegraph, was thus solved by him, adversely to its practicability. This only question is clearly and succinctly — *Can galvanic electricity, like common electricity, be propelled through a long wire, so as to produce no sensible diminution of its effects at a distance?* If it can, the Electric Telegraph is practicable; if it cannot, it is not practicable. This is the problem as presented to the minds both of Prof. Barlow and Prof. Henry.

Prof. Barlow supposed that his experiments had clearly solved the problem *negatively*, because of the observed "diminution of effect," even in a distance of a few hundred feet. Prof. Henry as truly supposed that his incidental experiments as clearly solved the problem *affirmatively*, because he observed that the "magnetic action of a current from a trough is, *at least*, not sensibly diminished by passing through a long wire."

Two important questions here meet us for solution:

First. Did the practicability of the Telegraph depend on the *affirmative* solution of this problem?

Second. Did Prof. Henry solve it *affirmatively?*

In proof that the practicability of the Telegraph did not de-

pend on the affirmative solution of the problem, I have only to refer to the notorious fact in the experience of all Electric Telegraphs, that there is a "*sensible diminution of effect*" proportionate to the length of the conductors, whatever form of battery is employed in generating the current. The inference of Mr. Barlow, therefore, is not correct, as to the *controlling nature* of the fact to be ascertained. It was not the "only question" to be solved, and he was equally incorrect in the general inference which he drew of the "impracticability of the scheme."

Second. Did Prof. Henry solve Barlow's problem affirmatively? It is on this assumption, indeed, that is based the validity of his claims to having removed the difficulties, propounded by Barlow, to the practicability of the Electric Telegraph. In one part of his remarks, he directly maintains the affirmative. He says, "The fact that the magnetic action of a current from a trough is, *at least*, not sensibly diminished, by passing through a long wire, is directly applicable to Mr. Barlow's project of forming an Electro-magnetic Telegraph." But in another part of his remarks, while hypothetically hinting that the effect of a current from a trough may be even "*increased*," in consequence of passing through a long wire, he at the same time suggests a "*slightly diminished effect*;" he says, "but that the effect of a current from a trough, *if not increased*, is but *slightly diminished* in passing through a long wire, is certain."

Which, then, is the real inference that he would draw? If he infers an *increase* of effect *in passing through a long wire*, it is not correct in its applicability to Telegraphs. If he infers that a trough current is, at least, "*not sensibly diminished by passing through a long wire*," it is equally incorrect, as experience shows, since it is notoriously and sensibly diminished in a long circuit. But if he infers that it is but "*slightly diminished in passing through a long wire*," then his inference is measurably correct; but he is then in the predicament that his inference does not meet, nor solve, the main, and, indeed, only point raised by Barlow, which is distinctly, whether there be "*any* diminution of effect by lengthening the conducting wires."

The utmost that can be granted to Prof. Henry, then, under the most favorable aspect of his claims, on the score of his "published researches," is, that he suggested the use of a trough

or intensity battery as a *probable* means of *partially* solving the difficulties in the way of an Electric Telegraph; but, as the question then stood, he did not solve Barlow's difficulty, nor by any means remove other obstacles, quite as indispensable to be removed, before the Telegraph could be a practical invention. But even these *probable* means of *partially* solving the difficulty are not original with Prof. Henry.

There was published in the Transactions of the Bavarian Academy of Sciences, at Munich, in the year 1812, more than twelve years before Barlow's paper was published, and eighteen years before Prof. Henry's experiments, a paper by Samuel Thomas Soëmmerring, from which I make the following extract:—

"In order to exhibit, at least, by a convincing experiment to my own eye, that the difference between communicating through a wire, the length of *two feet* and *two thousand feet*, was not observable, (though reason tells us there must be a difference,) I caused a glass cylinder to be wound round with 2,248 Bavarian feet of wire, *through which was conveyed the current of the voltaic pile*, so as to reach from the pile to the alphabet in the water reservoir, and serve as an example. The evolution of the gas, through this considerable length of wire, appeared to begin as quickly as if the effect had only to traverse *two feet*."

This experiment of Soëmmerring's was not made, indeed, directly for the purpose of showing whether the current of the voltaic pile, as that of common electricity had been, *could be conveyed through a long wire*, nor whether the *magnetic effects* could be thus transmitted; but both were demonstrated by the experiment, as any reader can see, and the great generic fact stands out clearly that *a current from an intensity battery can be propelled*, and produce effects at a distance, and with no more *sensible diminution* of effect than in Henry's subsequent experiment; *none* were noticed, or, at least, *none* are recorded. The well-known indications of a galvanic current, are *evolution of heat; magnetism; chemical decomposition; physiological effects;* and the *spark*. In this case *chemical decomposition* was chosen by Soëmmerring, by which to demonstrate the presence of the current, and the current was shown to be present, with "no

observable difference of effect" between communicating through a wire of the length of *two feet*, and *two thousand feet.*

Suppose, now, that previous to 1831, having my apparatus arranged, as it was arranged in 1835, I was in doubt whether, after all, I could propel galvanic electricity effectively to a distance; and that in searching for some fact, in the Records of Science, to dispel this doubt, I had met with this notice of Soëmmerring's experiments, published in 1812. Is it not perfectly clear that, so far as the propulsion of the galvanic current to a distance is concerned, it would have been more satisfactory than Henry's subsequent experiments, inasmuch as it was propelled through *double the distance*, and could I have failed to notice, too, that the common *voltaic pile*,* an *intensity battery* was used for that purpose? And, further, knowing the usual effects of the galvanic current, and learning that one of these effects was sensibly present at a distance, was the step, a very wide one to the inference, that the galvanic current being present at a distance by one of its effects, the other concomitant effects of the same current were present also?

In the Telegraph case, Smith *vs.* Downing, the testimony of that distinguished savan, Professor Hare, was taken on the opposite side (that of the defendants), and he testifies to having made an experiment which is almost identical with that of Soëmmerring's. He says, "By recurrence to the American Journal of Science, volume VIII., (1825,) it will be seen that an experiment was made agreeable to which *a galvanic discharge appeared to take place as instantaneously through 700 feet of wire as through seven inches.*" But his remarks upon this experiment are worthy of special notice: he adds, "*It follows as a consequence inevitably* in the eye of any one practically acquainted with electro-magnetism, that *an iron bar included in a coiled portion of the wire*, and thus *forming an electro-magnet, might have been magnetized and demagnetized* as readily as in the illustrations, before my class, alluded to."

So in the experiment of Soëmmerring in 1812, can any one doubt if he had introduced an iron bar into the coiled portion

* In the elaborate work of Dr. Schellen, der Electro-magnetische Telegraph, p. 54, the *voltaic pile* used in Soëmmerring's apparatus, is represented, and is the Cruikshank's battery, of 14 plates.

within the glass cylinder, around which was 2,248 feet of wire, that Soëmmerring instead of Oersted would have been the discoverer of Electro-magnetism, and also of the electro-magnet instead of Arago. All the other conditions were there precisely, *except, alone, the iron bar within the helix*, and after the discoveries of Oersted, of Schweigger, of Ampère, and Arago, in 1820, nothing remained to be discovered after this latter date to complete what Henry calls the *intensity magnet.* Everything was ready for the invention of the intensity electro-magnet in 1812, except, only, the simple placing of the iron bar within Soëmmerring's helix. But in 1820 *Arago finished the invention by actually placing such a bar within the helix*, and thus gave birth to the *Electro-magnet.* What then remained for Henry or any one else to do, so far as the *intensity electro-magnet* is concerned? Arago was unquestionably its inventor.

But will it be said that Soëmmerring merely proved, by his experiments, that a current could be transmitted by an intensity battery some two or three thousand feet, and it could only be inferred, plausibly indeed, that it might be propelled further, for he does not give the data by which it could be determined how far the current could be propelled. And then there might be a limit so near as in effect to be an insuperable obstacle to a Telegraph? This is true, and it is a point regarding the practicality of the Telegraph that was yet to be solved before perfect confidence in the results of it as a commercial enterprise could be inspired. But Professor Henry has not solved this difficulty any farther than Soëmmering; indeed, not quite so far. His experiments furnished him with a length of wire of 1,060 feet, (about half the extent of that which Soëmmerring had used;) he applied to this no "new arrangement of battery of his own," as Mr. Chase, counsel for opponents,* has erroneously declared, but the well-known ordinary Cruikshank's battery, the battery almost coeval with the birth of galvanism itself, the same in kind precisely that was used by Soëmmerring 18 years before him; and Professor Henry noticed this result, to wit: that the magnetic effects of the galvanic current were produced in a greater degree at a distance when the Cruikshank's battery was used, than when a single element or single pair was used.

* In his argument before the Supreme Court, Dec. 1852.

Barlow used a single element battery, and drew a false conclusion (except so far as it bore upon the single element battery), opposed to the previous prevailing scientific opinion, which was grounded on the use of the intensity battery. Or will it be still urged that Soëmmerring did not produce "*mechanical effects* at a distance" (a phrase much insisted on in the deposition), and that it was Henry who first demonstrated these effects by his experiments in passing a current through a long wire with an intensity battery? Although the inference, that there was power adequate to the production of "mechanical effects" at a distance, is unavoidable, from this experiment of Soëmmerring, after the discovery of Oersted, and especially after the invention of the multiplier, by Schweigger, yet it was not left to inference alone, after the year 1820.

M. Ampère, in his memoir, presented to the Academy of Sciences the 2d Oct., 1820,* says:—

"On doit conclure de ces observations que les tensions électrique des extrémités de la pile ne sont pour rien dans les phénomènes dont nous nous occupons, car il n'y a certainement pas de tension dans le reste du circuit; ce qui est encore confirmé par *la possibilité de faire mouvoir l'aiguille aimantée a une grande distance de la pile*, au moyen d'un conducteur très long, dont le milieu se recourbe dans la direction du méridien magnetique, au-dessus, et au-dessous de l'aiguille. Cette expérience m'a été indiquée par le savant illustre (Laplace†) auquel les sciences physico-mathematiques doivent surtout les grands progrès qu'elles ont faits de nos jours: *elle a parfaitement réussi.* . . . D'après le succès de cette expérience, on pourrait, au moyen d'autant de fils conducteurs et d'aiguilles aimantées qu'il y a de lettres, et en plaçant chaque lettre sur une aiguille différente établir, à l'aide d'une pile placée loin de ces aiguilles," &c., &c.

Here were "mechanical effects" produced at a distance by Ampère in 1820, who unquestionably used the intensity battery. The needle was deflected, which is a *mechanical effect;* and the Abbé Moigno, in remarking on this passage from Ampère, says that one might conclude that the Electric Telegraph was practicable in 1820—"si l'on avait pu parer dés lors à deux

* Annales de Physique et de Chimie, t. XV., p. 72.
† This gives the first suggestion of a *needle* telegraph to Laplace.

inconvénients très graves, l'action irrégulière des piles, et surtout la décroissance rapide de leur *intensité*."

The rapid decrease of the *intensity*, in the intensity battery then in universal use, was an obstacle to the practicability of the Telegraph; it was a defect common to the pile, the "couronne des tasses," and the Cruikshank's battery; nor was this defect of "rapid decrease of intensity" remedied, until Daniel's & Grove's improvements, in the invention of the *sustaining* battery. There is not a particle of advance on this experiment of Ampère, of 1820, so far as producing "*mechanical effects*" *at a distance* is concerned, in Henry's experiments of 1831.

In the light of these facts and experiments, in which an *intensity battery* had been successfully used to propel a current of galvanic electricity effectively to produce mechanical effects at a distance, Barlow seems rather to have proved that a single pair, or *quantity* battery, *would not answer* for telegraphic purposes, while Soëmmerring, as well as Ampère, had shown, by actual experiment, that an intensity battery would answer to propel a current through a long wire; and Ampère, that it would produce *mechanical effects* at a distance. Prof. Henry used only means which Soëmmerring and Ampère had used before him; and the utmost that can be awarded to him, therefore, on this point, is the restoring the *status quo* of the previous opinion disturbed by the opinion of Prof. Barlow.

One word in regard to *the application of the Cruikshank's battery*, to propel the current through a long wire. Such application was an event inevitable, especially after Soëmmerring's or Ampère's experiment; it would, indeed, have been more surprising, if this intensity battery had not been applied, than that it was so applied. For there were, at that time, only two kinds of galvanic batteries in use, to wit: the "*single element*," "*single pair*," or *quantity battery;* and the battery of many pairs, called the *Voltaic pile*, the *couronne des tasses* and the *Cruikshank's battery*, the *intensity* battery, and this last, among the first and most common intensity batteries in use. I say, then, the application of the *intensity* battery to the Telegraph was an act inevitable. No one, for example, could repeat Soëmmerring's, or Ampère's, experiments, with a view to ascertain how far they

had gone in solving the problem of an Electric Telegraph, or to ascertain how he might improve upon their plans, without *using the Cruikshank's*, or an *intensity battery*. It could not be avoided; for the repeating of their experiments could not be influenced by any conclusions to which Barlow had subsequently arrived, from the use of a *single pair* battery.

The obstacle remaining in the way of the establishment of the Telegraph, was not removed either by Soëmmerring, by Ampère, or by Henry. Even the result which Prof. Henry announces *as* "*certain*," proves to be not correct, as experience shows. There *is* "*a sensible diminution*" in the magnetic action of a current in passing through a long wire, if not sensible in the trifling distance of 1,060 feet, from which he drew his inference; yet experience abundantly proves that, through telegraphic conductors of great length, there is a sensible diminution of effect as the length of wire increases; and although both Soëmmerring, Ampère and Henry, (the two former quite as much as the latter,) in their use of the Cruikshank's intensity battery, suggest the kind of battery best to be used for telegraphic purposes, yet neither of them suggests the remotest hint of any method to overcome the most formidable obstacle that remained foreshadowed, by observing even the *least diminution* of effect as the length of wire increases. A slight diminution observable in a mile, points inevitably to a limit beyond, where, at length, no effect can be produced; and this is fatal to a Telegraph, unless further discovery be made of some mode of overcoming the difficulty. It is not pretended that Prof. Henry has given the slightest hint of any means of overcoming this obstacle in any of his "published researches," nor is there made any claim for him to that effect, until the publication of his deposition of 1849.

In view of these facts, how is it that such exorbitant claims to discoveries in science, bearing upon the Telegraph, have been set up by legal counsel in behalf of Prof. Henry, mainly, as it would seem, for the purpose of disparaging my claims? The process has been one of mystification.

Two things, totally distinct, have been confounded in the judgment formed of Henry's paper in Silliman's Journal. That paper does announce a great improvement in the construction of an electro-magnet for a specific purpose, to wit: for the

attainment "of *great magnetic power* from a *small galvanic element*;" and hence, without inquiring whether such improvement has any bearing upon the Electro-magnetic Telegraph, the conclusion has been hastily adopted, that because Prof. Henry has made an improvement in the *lifting power of the magnet*, therefore he is entitled to the credit of improvement in the Telegraph, and this apparently on the sole ground, that he incidentally refers to a proposed (or what he presumed was a proposed) Electro-magnetic Telegraph, by Barlow. Henry's improvement in the magnet was not made with reference to the Telegraph, is of no use in it, as I have said, and, indeed, has no applicability to it. The reference which he makes to the Telegraph in his "published researches" relates to *the propulsion of a galvanic current by an intensity battery to a distance*, not for the purpose of making an electro-magnet of any kind at a distance, but only for the purpose of deflecting a needle.

It may be incidentally remarked, that the professed object of Henry, in his experiments, is precisely the *reverse* of my object in my experiments. Prof. Henry's object was to produce "*great magnetic power*, with a *small galvanic element*." My object was to produce a magnetic power, however *small*, at a distance, by a galvanic element arrangement, however *large*.

One of the difficulties that seemed most formidable from the outset, that, indeed, which weighed most heavily upon me, arose from observing that, *notwithstanding I had used a Cruikshank's battery*, (*the intensity battery*,) *to propel a galvanic current with an apparently trifling diminution of effect*, through a distance of a few hundred feet, yet the fact that there was *any diminution* as distance increased was suggestive of a limit somewhere, and not far off, where the effect must cease altogether. This difficulty I had indeed remedied by a repeating apparatus, (the receiving magnet, and combined circuits,) so far that the uncertain, or rather unascertained limit of effective motion at a distance being reached, I had the remedy prepared, of a second link, to repeat the effect to a similar distance further on; but here a new obstacle arose, bearing not so much on the *practicability* as the *practicality* of the Telegraph, from the still unascertained limit of this single link, suggesting numerous repeating stations, and, consequently, increased expense to such a degree,

it might be, as still to render the Telegraph, not from any scientific objection, so much as from an economical one, impractical.

In my conception and invention, certainly, as early as March, in 1837, (see extracts from Gale's deposition,) of my receiving magnet and combined circuits, no reader of anything that Prof. Henry had then published will say that there was any hint, by him, of any such contrivance. How does anything proposed in the paper of Henry, of 1831, apply to the removal of the obstacles I have just mentioned to an Electro-magnetic Telegraph? The *deflecting needle* Telegraph was the only Electro-magnetic Telegraph then known or dreamt of, and this is conclusive that Prof. Henry, addressing the conscious knowledge of the scientific mind, at that date, could have had no other kind of Telegraph, in his own mind; certainly no subsequently invented one, unless distinctly suggested by him at the time, and this it is not pretended that he has done. He did not suggest any other; his allusion is only to the one then projected, or rather supposed by him to have been projected by Barlow, a *needle* Telegraph, not to a kind not then in existence. How, then, I say, does anything that Henry proposed, in 1831, apply to the removal of the obstacles I have mentioned as standing in the way of a practical Electro-magnetic Telegraph? There is in his paper an interesting experiment (experiment 7), in which we learn that a Cruikshank's battery, of 25 pairs, was found to be more effective in "producing greater magnetic effects on soft iron after traversing more than one-fifth of a mile of intervening wire, than when it passes only through the wire surrounding the magnet." Then follows Prof. Henry's observation on this discovery, "that the effect of a current from a trough, (that is, an intensity battery,) if not increased, is but slightly diminished in passing through a long wire, is certain." And then, again, "the fact that the magnetic action of a current from a trough is, *at least*, not sensibly diminished, by passing through a long wire, is directly applicable to Mr. Barlow's project of forming an Electro-magnetic Telegraph." All this is very well, so far as it goes; but it goes no farther, as I have shown, than others had gone before him. Neither their discoveries, nor Prof. Henry's, go the whole length of solving the difficulties.

As the experiment left Henry's hands, was there nothing further to be discovered and applied to insure the success of the Telegraph? There is a general direction that a battery of many pairs, an intensity battery, is best for long wires. This, however, is the inevitable inference also from Soëmmerring's experiment, 18 years, and Ampère's, 11 years before. But there is no law discovered, no ratio observed between the number of plates in the battery, the length of conductors, and the effect produced, so as to direct to the distance apart at which repeating stations must be placed. The experiment of Soëmmerring, in 1812, and Ampère, in 1820, as I have observed, are as satisfactory to me in solving any difficulty of that kind, as the experiment of Henry.

But setting this aside for the moment, and allowing to Henry's alleged discovery all the force of originality on this point, to what does it amount? Twenty-five pairs produced a decidedly powerful and encouraging effect through 1,060 feet, or one-fifth of a mile. The inference is a fair one, that to a certain extent the same effect, or one but "*slightly diminished*," can be produced at a greater distance; but with how many pairs; and to how much greater distance? and, especially, with *what diminished effect?* What is the amount of the loss by "*retardation of the velocity of the current*," by increased distance? and then, specially, what is the *remedy* for this loss?

How does Prof. Henry resolve these difficulties? He asks an important question, in the attempt to account for the fact he had noticed; but he leaves it unanswered, and it remains rather to embarrass than to clear up the mind of the inquirer. "May it not be a fact," says Prof. Henry, "that the galvanic fluid, in order to produce the greatest magnetic effects, should move with a *small velocity*, and that in passing through one-fifth of a mile, *its velocity is so retarded* as to produce a greater magnetic action?" Now the *retardation of velocity* in one-fifth of a mile, suggests a *greater* retardation in a mile, and a final *cessation* at some unknown distance beyond. Thus the very cause of magnetic effect which he hypothetically proposes, to wit, "*retardation of velocity*," substitutes a new difficulty, quite as formidable as the one he professes to have removed. I repeat, therefore, there is

nothing in Henry's paper that provides against this exigency, nor hints at any mode of overcoming the difficulty.

I come now to show that this difficulty, consequent on the fact of a "*slightly diminished current*," was overcome by me.

In view directly and distinctly of such a foreshadowed limit to the effective action, even of the most powerful voltaic battery, at some unknown distance, my mind was engaged, as early certainly as 1836, in devising the means of removing this apprehended obstacle to the Telegraph. Hence resulted, first, the *devising* of the plan of *combined circuits* in *successive links*, connected by receiving magnets, which plan was then freely conversed upon with Dr. Gale before its actual invention (or the actual embodiment of the idea); and second, to demonstrate the action experimentally, it was then invented, or put into apparatus, as early as the first week in March, 1837, by embodying the device in an actual combined circuit and receiving magnet. Dr. Gale's testimony (at page 176 Supreme Court Record) is conclusive on this point. This was the mode I invented to overcome the difficulty suggested by a diminution of effect as the length of the conductors increased; and no research, though diligently and perseveringly made by my opponents, has yet discovered a prior date to this invention.

I thus had at command the means of repeating an effect at a distance. But there still remained another problem to solve, bearing rather upon the economy or practicality of the enterprise, than upon its actual *practicability*. This may be stated thus. Given the length of a conductor forming a single link through which the desired effect can be produced, the receiving magnet and combined circuits furnish the means of repeating that effect in successive combined links of the same length, to any distance, even around the globe.

But *the length of that single link* was yet to be ascertained. How long may that link be? Can the current be made effective for a single link of one, two, ten, twenty, one hundred miles? For the number of repeating stations in a distance of thousands of miles must be governed by a solution of this question. What facts in science had been discovered bearing on this question? Two laws opposed to each other, bearing upon the conductibility of the current, were announced, the one by Barlow, in 1824,

the other by Ohm, in 1827. I will use Professor Page's clear statements of these two laws of conduction. Barlow's law was, "*That the conductibility was inversely proportionate to the square of the lengths, and directly as the diameters of the wires, (or as the square roots of their sections.*")

The other, and the true law, as expressed by Dr. Page, is, "*the resistance by bodies to the conduction of electricity, is directly as their lengths, and inversely as the areas of their cross-sections.*"

"This law," says Dr. Page, "was proved many years since by Davy, Pouillet, Becquerel, Christie, Ohm, Fechner, and others." Thus, Barlow's law was proved unsound by what is now known as Ohm's law. Priority in the discovery of this law has been contested among some of these distinguished savans, a question not pertinent to settle in the present disquisition, as it is sufficient for my purpose to know that the law, by whomsoever discovered, was established and published at least two years before Professor Henry made his experiments, and four years before he had published them. All the facts of science, therefore, necessary to be established to make the telegraph practicable, up to the single point of determining the practical length of the first link of the great chain of the combined circuits which I had invented, had been developed and established and published previous to Henry's experiments, and consequently, his results, whatever they may be, were not indispensable to my success in my invention.

Who, then, determined this remaining point? It was not Prof. Henry. Recurring to the previous mode of telegraphing, and the necessity that the *repeating stations* should be in telescopic sight of each other, it is perceived that in a line of telegraphs of 1,000 miles in extent, (assuming that ten miles is a practicable distance with the telescope,) there must be one hundred stations.* Hence I felt an anxiety, while inventing the new Telegraph, certainly not to exceed the number of stations of the old mode. No one, as I have said, had determined by actual experiment the distance to which a current could be effectively propelled through a single circuit, by one electro-

* The average distance in France was less than three miles between the stations; so that in a distance of 1,474 miles, there were no less than 519 stations.

motor; no one had thus determined the length of the first link of a chain of circuits. Should this be no more than ten miles, my Telegraph was then practicable; but should it be proved that twenty, fifty, one hundred miles, or more, were a practicable length for a single link, its still greater triumph over the old modes would be manifest, and would insure its practical success.

I think no one can read my narrations at various times previous to 1844, published in the Congressional Documents, without perceiving that this point was one which seriously occupied my mind. It was one not necessary, as I have said, to prove the *practicability* of the Electric Telegraph; for if only a mile was the limit of that link, it was *practicable;* but that the Telegraph be *practical*, it was a preliminary point necessary to be resolved, since it bears directly on the Telegraph as a practical or commercial enterprise.

In my letter to the Hon. C. G. Ferris, one of the House Committee of Commerce, dated December 6th, 1842, and printed in Vail's book, at page 89, I have thus written:—"I have, nevertheless, been able to resolve all the doubts that lingered in my own mind, in regard to the perfect practicability of establishing my telegraphic system to any extent on the globe. I say 'doubts that lingered in my own mind.' The principal, and, indeed, the only one of a scientific character which at all troubled me, I will state, and the manner in which it has been resolved.

"At an early stage of my experiments, I found that the magnetic power produced in an electro-magnet by a single galvanic pair, diminished rapidly as the length of the conductors increased. Ordinary reasoning on this fact would lead to a conclusion fatal to the whole invention, since at a great distance I could not operate at all, or, in order to operate, I should be compelled to make use of a battery of such size as would render the whole plan in effect impracticable. I was, indeed, aware that by multiplying the pairs in the battery—that is, increasing the intensity, or its propulsive power—certain effects could be produced at great distances, such as the decomposition of water, a visible spark, and the deflection of the magnetic needle. But as magnetic effects, except in the latter case, had not, to my knowledge been made the subject of careful experiment, and as these va

rious effects of electrical action seemed in some respects to be obedient to different laws, I did not feel entirely assured that magnetism could be produced by a multiplication of pairs sufficiently powerful at a great distance to effect my purpose. From a series of experiments which I made, in conjunction with Prof. Fisher, during the last summer, [1842,] upon 33 miles of wire, the interesting fact so favorable to my telegraphic system was fully verified, that, *while the distance increased in an arithmetical ratio, an addition to the series of galvanic pairs of plates increased the magnetic power in a geometric ratio.* Fifty pairs of plates were used, as a constant power. Two miles of conductors at a time, from two to thirty-three, were successively added to the distance. The weight upheld by the magnet from the magnetism produced by fifty pairs, gradually diminished up to the distance of ten miles; after which, *the addition of miles of wire up to thirty-three miles,* (the extent to which we were able to try it,) *caused no further diminution of power.* The weight then sustained was a constant quantity. The practical deduction from these experiments is the fact that, with a very small battery, all the effects I desire, and at any distance, can be produced. In the experiments alluded to, the fifty pairs did not occupy a space of more than eight cubic inches, and they comprised but 50 square inches of active surface.

"The practicability of establishing my telegraphic system is thus relieved from all scientific objections."

At a later period, on the 8th of August, 1843, having one hundred and sixty miles of wire prepared, I repeated the experiments of 1842 in a modified form, and a publication of the results were made in Silliman's Journal of Science, Sept., 1843, with a diagram of the curves projected from both these series of experiments, accompanied by a paper on the law of the conducting power of wires, by my distinguished and learned friend, Prof. John W. Draper, of the New-York City University. This paper, with the diagrams, are given by Mr. Vail, in his book, at pages 53 to 56.

Thus, it will be perceived, that in 1842, seven years before Prof. Henry gave his deposition, and also in 1843, six years before, these experiments of mine, and these results, were published in Congressional documents, and in a Scientific Journal.

They were experiments to determine whether the first link of a chain of circuits was within a *practical* distance; and they did satisfactorily determine that that link was of such a length (more than 33 miles in the one case, and 160 miles in the other,) as to make my Recording Telegraph not only *practicable*, but *practical*.

Were not those experiments within the sphere of electrical science? and was not the deduction from them a scientific deduction? Was not this deduction an original discovery in electrical science, applicable to the invention of the Telegraph? Was it not definitely, that the length of the circuit composing the first link of a telegraph line was so great, that the Telegraph was proved *practical*, as it before had been proved *practicable?* If so, how is it that Prof. Henry (who could scarcely have been ignorant of my experiments, since I sent him, at the time, the printed Congressional document in which they were published,) could say, in his deposition, "I am not aware that Mr. Morse ever made a single original discovery in electricity, magnetism, or electro-magnetism, applicable to the invention of the Telegraph?"

I have already commented at large on the paper in Silliman's Journal of Science, Vol. XIX., 1831, which is principally referred to by Prof. Henry, as containing his discoveries bearing on a Telegraph; and that it may be critically examined, I have embodied it in full in the Appendix. Yet, lest a vague impression may exist that there are other publications of his which contain discoveries pertinent to the case, I will briefly review them all.

The *first* of these "published researches" on the subject of Electro-magnetism, is to be found in the "Transactions of the Albany Institute," Vol. I., Art. IV., page 22, occupying three pages, and read Oct. 10th, 1827, but collected, with other papers of the Society, and published in 1830. It is entitled, "*On some Modifications of the* ELECTRO-MAGNETIC APPARATUS, *by* JOSEPH HENRY." The sole object of this paper is to announce, as its title indicates, some modifications of the Electro-magnetic apparatus already in use, as improvements, or additions, to some constructed by Sturgeon and others, for class illustration of experiments for "deflecting the magnetic needle," "for showing

De la Rives ring," the action of "two conjunctive wires upon each other," and "the dipping needle." Prof. Henry was desirous of forming, as he says, "a set of instruments, on a large scale, that would illustrate all the facts belonging to the science, *with the least expense of galvanism*,"* and he accomplishes his object by applying Schweigger's multiplier to the deflecting of the needle, and also of a wire charged with a galvanic current, to supply the defects of Mr. Sturgeon's apparatus. This is all that is pretended in this paper, and I presume it will not be asserted that there is anything contained in it bearing upon the Electro-magnetic Telegraph.

It may be here noticed, that in his deposition, Prof. Henry, after speaking of the above publication, states that in 1828 he commenced "the investigation of the laws of development of magnetism in soft iron;" that "the first idea that occurred with reference to increasing the power of the electro-magnet, was that of using a longer wire than had before been employed;" he states, also, that he "exhibited to the Albany Institute in March, 1829, electro-magnets of this kind, which possessed magnetic power superior to that of any before known." But these experiments and results, it will be noted, were not deemed by him of sufficient importance to be published in the Transactions: not by any means of so much importance as his "modifications of electro-magnetic apparatus," published in the Transactions in 1830; and the announcement of their ever having been made, was not even published until 1831. If the credit of first making experiments with "*longer wires* than had been previously employed in constructing the coils or helices of the *electro-magnet*," is that which Prof. Henry would here claim, it appears to belong to him; but this was simply an *enlargement*, by addition of wire, of the *volute spiral helix*, which Professor Dana had two years before publicly and experimentally explained in his lectures. The only novelty of Prof. Henry's was the *adding* to the *length of the wire*. The wire of the helix was wound in a *volute spiral*, as Prof. Dana had wound it two years before. There was no new principle discovered; it was

* It cannot but be noticed that a *leading object* of Prof. Henry, not only in his experiments detailed in this paper, but also in the longer paper of January, 1831, in Silliman's Journal, is *economy of galvanism*.

but the mechanical and obvious application of an already known principle, and in accordance with the previous suggestion of at least one other philosopher.

But if there were a question whether Prof. Dana or Prof. Henry had priority in thus improving the electro-magnet, it is a point quite immaterial in its bearing on my telegraph invention. The magnet thus formed was in common public use, as I have before said, when I applied it, and combined it with other apparatus, to the production of my peculiar purpose. I never claimed as my invention the electro-magnet of Arago, nor any of its improvements. It is a matter altogether outside of my claim to the invention of the Electro-Magnetic Telegraph.

The *second* of Prof. Henry's "published researches," is the one just alluded to in the introduction, and published in the Appendix. It occupies eight pages in Silliman's Journal. I have already examined it at length, and I therefore dismiss it here with the single remark, that all the allusion to a Telegraph contained even in this paper, is comprised in *three and a half lines!* to wit:—

"The fact, that the magnetic action of a current from a trough is, *at least*, not sensibly diminished by passing through a long wire, is directly applicable to Mr. Barlow's project of forming an electro-magnetic telegraph."

The *third* and last of his "published researches" previous to 1838, is to be found in the American Journal of Science, Vol. XX., July, 1831. It is entitled, "Art. XVII." "On a reciprocating motion produced by magnetic attraction and repulsion, by Joseph Henry." This paper need not detain us long. No reader of it will be able to find any allusion to a Telegraph in an instrument, which Prof. Henry himself considered "not of much importance," and which he designates as "only a philosophical toy." He suggests, indeed, this general thought, to wit:—"Although in the progress of discovery and invention, it is not impossible that the same principle, or some modification of it, on a more extended scale, may hereafter be applied to some useful purpose."

No sentence could be worded in more general terms, none more full of diluting and qualifying phrases; "not impossible" that some future application of this principle, or something like

it—"some modification of it"—may be made and applied to some not now even conceived purpose, but this not till further discovery and invention!

In regard to this "philosophical toy," it can hardly be a very extraordinary step, after the previous discovery by others of a new mechanical force, the (*Electro-magnetic,*) to make the application of it to the most obvious of the mechanical powers, to wit: the *lever.* Is it claimed for Professor Henry that here he first applied electro-magnetic power to move a lever? If so, I have no wish to deprive him of the credit of it. I never claimed it for myself. Granting it to be his, his invention begins and ends in that single act, so far as this paper shows any use of it, or any suggestion of a use, he himself being the judge. The peculiarity of my use of the electro-magnet is, not that I move a lever* with it, but that the new power (which is not Professor Henry's discovery) is of a nature to be *commanded and nicely regulated at a distance* to carry out my original idea—*a Recording Telegraph*—and in a way in which no other power whatever had been previously commanded for the same purpose.

These three papers comprise the entire "Published Researches" of Professor Henry, up to the time of the planning of my invention of the Combined Circuits and Receiving-magnet, in 1836, and its embodiment in March, 1837. There is no evidence of any published or existing invention (of Professor Henry's, or of any person whatever) of a similar character, or for a similar purpose, previous to this date (March, 1837.) The "*Published Researches,*" then, of Professor Henry, amount altogether to twelve pages, octavo, of a magazine, containing *three and a half lines of allusion to a Telegraph!* and on this slight basis is built the claim for Professor Henry, which is to deprive me of my claim to originality in my invention.

Here this point might be permitted to rest, but that Professor Henry's supposed claims to the invention of the Combined Circuits and Receiving-magnet (claims, if advanced at all, *first* advanced in 1849 in his deposition) have been the basis of assertions by opposite counsel, adverse to my right; and, there-

* The *lever* is not essential; it may be dispensed with.

fore, it is proper to give the deposition on this point a thorough examination.

From some parts of this deposition, notwithstanding his absolute silence on the subject in any of his "published researches," it has been not only inferred, but positively asserted, that Professor Henry had actually provided a remedy for this evil, of a slightly diminished current—and that, too, by a mode which, it is said, is, in principle, if not identically, like mine—and that he had even explained such a process to his pupils.

Previous to my invention, also, he says that he was shown, in London, by Professor Wheatstone, a different device for a similar purpose, in April, 1837, thus interposing two claimants, to wit: Professor Henry and Professor Wheatstone, as disputing my priority in this essential part of my telegraphic invention. I will thoroughly expose the baseless grounds of these pretensions.

First, then, what are the grounds upon which this part of the invention is claimed for Professor Henry?

The records of all Professor Henry's published researches, up to the time of my first intercourse with him, either personal or by correspondence, in May, 1839, will be consulted in vain to find a single hint to this effect. Had he any method of overcoming the difficulty when he wrote me his letter of May 6, 1839, (the first I ever received from him, and before I had any personal acquaintance with him,) when answering the general question which I had put to him in mine of April 24th, 1839?

I say in that letter, "Have you met with any facts in your experiments thus far, that would lead you to think that my mode of telegraphic communication will prove impracticable?" His answer to this question will surely indicate whether he had any plan to overcome any real or apprehended difficulty. What is his reply? "So far as I am acquainted with the minutiæ of your plan, I see no practical difficulty in the way of its application for *comparatively short distances;* but if the length of the wire between the stations be great, I think that some *other modification will be found necessary in order to develope a sufficient power at the other end of the line.*" Now, if Prof. Henry were conscious of having a plan at that time "*to develope a sufficient power at the other end of the line,*" would not here have been the place for some allusion to it? would he not have said something

to this effect:—this difficulty, however, I have long since overcome; I have a modification, which I have been in the habit of explaining for many years to my various classes, therefore, use my secondary circuit and receiving magnet, and the difficulty is obviated, you will then have the modification necessary for your purpose.—Whatever reasons may be assigned why he gave no hint of the kind, it is clear that in May, 1839, he had nothing like my receiving magnet and secondary circuit; yet at this very time, these parts of my Telegraph had been specified by me at the Patent Office, examined, and passed, and the patent ordered to be issued more than a year before, to wit: May 1st, 1838.

But there is another view of this matter. Giving to Prof. Henry the most favorable construction of his claims to discoveries bearing on the Telegraph; they may be stated thus, to wit: that an intensity battery propels the galvanic current to a distance, either with an "*increased effect*" as distance increases, or with an effect "*not sensibly diminished* by passing through a long wire." Now taking it for granted that he believed (and no one will doubt it) that these professed discoveries were correct in the results which he announces; that an intensity battery actually does, (what it is now well ascertained it does not do,) to wit: propel a current with an "*increased*" magnetic effect at a long distance, or that it propels a current at a distance with a magnetic effect "*not sensibly diminished*," (which is equally erroneous,) these results, however incorrect they have since proved to be, must at that time have operated on Prof. Henry's mind as *truths*, and have influenced his speculations on their applicability to the Telegraph project of Barlow *as truths*. In this aspect of the matter, it would appear perfectly conclusive, that Prof. Henry could have had no such device as a "receiving magnet or combined circuits," before he went to Europe, in 1837, and for the plain reason, that the alleged discoveries which he had made, or supposed he had made, rendered any such contrivance wholly unnecessary. A single long circuit, and an intensity battery, on the supposition that his results were correct, were to him all-sufficient. The means for attaining the end of a telegraph, if his discoveries were indeed real, were complete without any such devices, and under such circumstances a repetition of the apparatus, so as to form "combined

circuits," and "receiving magnets," would be but an uncalled for and superfluous complication. Invention, it is well known, grows out of a necessity; but here would be an invention without a necessity—an invention for no purpose. The inference is irresistible that Prof. Henry was mistaken in supposing that he had any such uncalled for device, before he went to Europe in 1837. There he saw Mr. Wheatstone's contrivance, which naturally suggested to him the modification of it of which he speaks.

But fortunately for the consistency as well as the integrity of Prof. Henry, he does not make this claim, however strongly it has been made for him by those who seek to deprive me of my claim in this respect. There are, indeed, *ambiguities* of language which have been misconstrued by opposing counsel, and a meaning elicited which seems to have so far satisfied Mr. Gillett, one of the counsel on the part of the plaintiffs in the case before the Supreme Court, as to lead him boldly to assert no less than *five times* in the course of his argument, merely on the uncertain conjecture of Prof. Henry as to a date, that he, Prof. Henry, had actually combined two circuits with a receiving magnet, and shown the invention, thus complete, to his classes of pupils in Princeton, before he went to Europe in 1837.

Whatever ambiguities of language, however, and seeming claim may there be found, his language interpreted in accordance with plain declarations which he has made in the same document, and elsewhere under oath, declarations without the least ambiguity, forbids that he can have intended to claim anything more than *showing to his pupils, as a class experiment, uncombined, a portion only of the single element of my combined circuits;* I repeat it, *showing to his pupils as a class experiment, uncombined, a portion only of the single element of my combined circuits*, a portion, too, not the discovery of Prof. Henry, but in existence as early as the invention of the electro-magnet, by Arago.

This I will clearly demonstrate before I have done.

The more clearly to make this point understood, I will state what is the great object or aim of my Telegraph; how the electro-magnetic power is applied to accomplish this object; and what part my combined circuits and receiving magnet perform in the process. And then examine what is stated in the depo-

sition to be Professor Henry's object, in any alleged device of his having any resemblance in principle or performance to my combined circuits and receiving magnet.

The main object in my telegraphic invention, as is abundantly set forth, is *Telegraphic Recording*, and by means of electro-magnetism. My new alphabet, adapted to the purpose, is to be marked or printed at a distance. This alphabet is formed by breaking into parts, conventionally, a *continuous line*, thus:

and by different combinations of the longer and shorter parts of such line, with the longer and shorter spaces between the broken parts, the various letters are formed. These letters are produced at a distance, by commanding the magnetic power of a galvanic current, induced in an electro-magnet, causing the current to flow, or to cease flowing at certain determinate intervals of time. *Duration*, both in the flow and the cessation of the current, is an indispensable element in the formation of a line, and also of a space, for forming my letters. The flow of the current of galvanism, and consequently the magnetic power, is caused by *closing* the circuit; the cessation of the current, and consequently the cessation of the magnetic power, is caused by *opening* the circuit. The *duration for a line* is marked by the *duration of the closing;* and the *duration for a space* is marked by the *duration of the opening* of the circuit. Two distinct and opposite acts are, therefore, equally necessary in forming every letter, to wit; *closing* and *opening* the circuit. The *closing* as well as the *opening*, and the *opening* as well as the *closing*, are equally and alike necessary to the result. They are indissolubly connected. *No letter* can be formed but by the conjoint acts of *closing* and *opening* the circuit. Hence, to produce the one or the other of these two acts, at pleasure, it is plain that the instruments at a station must be so constructed as to be controlled by the distant operator, enabling him to produce, at any moment, at his option, either of these two acts, to wit, the *closing* or *opening* of the circuit.

The *first part*, or element, of the combination in the combined circuits, consists of a single circuit of conductors, having in it a battery, an electro-magnet with its armature upon a lever, upon

which lever is a point or pen, brought in contact with a regularly moving strip of paper, when the magnet is charged with magnetism, and released from the contact with the paper by a reacting spring, while the magnet is discharged of its magnetism. The play of the armature of this magnet, or the sphere of its motion, is regulated by adjustable stops, so that during the attraction of the magnet, or flow of the current, it shall not come into actual contact with the poles, or face of the magnet; and during its retreat, while the magnet is without attraction, that is, during the cessation of the flow of the current, it shall not recede beyond the attraction of the magnet when again charged. The *opening* and *closing* of the circuit, is commanded by a key, or instrument, in which the broken parts of the conductors are readily united or severed at the will of the operator.

The *second* part of the combination, in the combined circuits, is similar to the first; is indeed a repetition or duplicate of the first, excepting that the registering instrument is dispensed with, and the magnet in the first circuit of the combination becomes a receiving magnet, that is, it receives the regulated impulses from the distant key, to be simultaneously communicated through the second circuit of the combination by *closing* and *opening* this second circuit. The receiving magnet performs, indeed, in this case, the office of a distant key, which is moved immediately by the electro-magnetic power of the receiving magnet, but is commanded by, and obeys, implicitly the action of the key under the hand of the operator, who thus *closes* or *opens* the second circuit of the combination simultaneously, and with the same exactness as he *closes* and *opens* the first.

Supposing, therefore, a limit of effective action in a long line, for directly marking the characters of the alphabet, by a single circuit, to be reached; my combination of circuits enables me (in the language of my patent) "*to extend more effectually the length* of any desired circuit of conductors, and to *perpetuate the power* of the electric or galvanic current equally throughout the same," and I thus successfully accomplish the ultimate end of my Telegraph, to-wit: *the effective recording of letters at a distance.*

I now come to the examination of the *alleged* device of Prof. Henry to accomplish this purpose.

Here is all that he himself says, on this point, in his deposition proper:—

"In February, 1837, I went to Europe; and early in April of that year Prof. Wheatstone of London, in the course of a visit to him at King's College, London, with Professor Bache, now of the coast survey, explained to us his plans of an Electro-magnetic Telegraph; and among other things, exhibited to us his method of bringing into action a second galvanic circuit. This consisted in closing the second circuit by the deflection of a needle, so placed, that the two ends projecting upwards, of the open circuit, would be united by the contact of the end of the needle when deflected, and on opening or breaking of the circuit, so closed by opening the first circuit, and thus interrupting the current, when the needle would resume its ordinary position under the influence of the magnetism of the earth. I informed him that I had devised another method of producing effects somewhat similar. This consisted in opening the circuit of my large quantity magnet at Princeton, when loaded with many hundred pounds weight, by attracting upwards a small piece of moveable wire, with a small intensity magnet, connected with a long wire circuit. When the circuit of the large battery was thus broken by an action from a distance, the weights would fall, and great mechanical effects could thus be produced, such as the ringing of church bells at the distance of a hundred miles or more, an illustration which I had previously given to my class at Princeton. My impression is strong that I had explained the precise process to my class before I went to Europe, but [*testifying now without the opportunity of reference to my notes,*] I cannot speak positively.* I am, however, certain of having mentioned in my lectures every year previously at Princeton, the project of ringing bells at a distance by the use of the electro-magnet, and of having frequently illustrated the principle to my class, by causing in some cases a thousand pounds to fall on the floor, by merely lifting a piece of wire from two cups of mercury closing the circuit.

* In the Philadelphia case, 1851, as well as in the Boston case, 1850, the words in this last paragraph in Italics and brackets are left out, of his depositions, while the rest of the paragraph is retained. The fair inference from this is, that after having had "the opportunity of reference to his notes," he found nothing in them to confirm his impression. And a further inference taken in connection with the proofs of defective recollection already given, is, that he was mistaken in supposing he had explained any such contrivance as a combination of circuits, until after having seen Wheatstone's method in April, 1837, and on his return in the *autumn* of that year.

"The object of Prof. Wheatstone, as I understood it, in bringing into action a second circuit, was to provide a remedy for the diminution of force in a long circuit. My object, in the process described by me, was to bring into operation a large quantity magnet, connected with a quantity battery in a local circuit, by means of a small intensity magnet and an intensity battery at a distance."*

At this point, as serving to clear the ambiguity of his deposition in chief, if its perusal suggests to any one the idea that Prof. Henry had actually combined two circuits, I quote the 23d cross-interrogatory, with Prof. Henry's answer thereto, in the case (French vs. Rogers, Phila. Resp. Evid., pages 180 and 263, printed copy):—

Twenty-third. "If you have answered that you ever had any device of your own for breaking and closing a distant circuit, state and describe fully and particularly how it was made, each and every part of which it was composed, and the arrangement, connection and material of such parts, and who constructed the same, and where it was constructed, and whether it was used; and if yea, then by whom, how long, from what date to what date, how many times, to what extent, and for what purpose? And if you have stated, in this examination, that such device was one circuit of conductors to break and close another, then state how, and by what means they were connected and combined, designating or describing every piece and part of mechanism by which they were connected, and the use of every such part, and how long was each of such circuits?"

To which Prof. Henry answers:—

"To the *twenty-third* cross-interrogatory, on behalf of the plaintiffs, he answers: The *local circuit* devised by myself, and mentioned in my direct examination, was connected with a quantity battery and a large electro-magnet, loaded with several hundred pounds. When the ends of a forked-shaped wire were drawn up from the surface of mercury, contained in two cups,

* This last paragraph reads thus, as amended by Prof. Henry in 1851. "My object in the process *I have described*, was to *cause* a large quantity magnet connected with a quantity battery in a local circuit *to let fall its load* by means of a small intensity magnet, and an intensity battery at a distance."

(French vs. Rogers, Resp. Ev. p. 254.) Phila. case.

This being his emendation, after the lapse of two years for reflection upon the fact as set forth in his first deposition of 1849, may be considered the statement by which he is willing to abide.

(thimbles were used,) the circuit was broken, and the weights fell to the floor." (Let what follows be specially remarked.) "*This part*" (the *local circuit*,) "of the combination and action was actually and repeatedly exhibited in my lectures at Princeton, from 1833 to 1848. I also accompanied the exhibition with the *statement*," (not with a device showing it,) "that the same effect *could be* produced by the action of a battery at a distance, for ringing bells or producing other mechanical effects. The results of my first experiments in causing an electro-magnet to act through a long wire, furnished me with the means of accomplishing this. For this purpose, it was only necessary to attach the forked wire to the armature of a small intensity magnet connected with the long circuit, in which was also an intensity battery. When the current was passed through the long wire, the armature *would be* attracted upwards, the short circuit *would be* broken, and the weight *would* fall. *

"*I do not recollect to have exhibited the last part of this arrangement in my lectures, or remember when I invented it;* but the invention was made and explained to others, before the publication of Mr. Morse relative to his Telegraph."

It will be perceived that this cross-interrogatory is not answered, but for the most evaded. Yet it does dissipate the fog that hung over the essential point. The *combined circuits*, the matter on which light was sought to be elicited by this question, consists of two circuits *combined*, as the very name indicates, and for the accomplishment of a special, yet complex purpose. A portion only of the first part of a combination is described, that is, *one circuit* uncombined, an arrangement which was in existence, and exhibited to me as early as the beginning of the year 1827, in lectures before the New-York Athenæum, by Professor James Freeman Dana, and this before Prof. Henry

* It cannot escape observation that the *verbs* used by Professor Henry, in describing the part of the combination which existed, and which he exhibited to his classes, and the part which did not exist, and which, of course, he did not exhibit, are significant of the existence of the one, and the non-existence of the other. When he speaks of the former he says, "the local circuit, &c., *was* connected:" "when the ends of the wire *were* drawn up, the circuit *was* broken:" and "the weights *fell* to the floor." Here everything is described as a result that had been demonstrated by apparatus in existence. But when he speaks of the latter he says, "the same effects *could be* produced:" "when the current was passed through the long wire, the armature *would be* attracted:" "the short circuit *would be* broken:" and the weight *would* fall." Here everything is *conditional*; it is a *predicted* result of apparatus not in existence.

had any knowledge whatever of the science of electro-magnetism. This portion alone he describes, and states that he repeatedly exhibited it, in his lectures at Princeton, from 1833 to 1848. There is nothing extraordinary in this. It was but the ordinary class experiment, common then and now in the courses of Natural Philosophy, illustrative of the lifting power of the electro-magnet; the only novelty shown by Prof. Henry to his classes being, as was natural, a substitution of his more powerful quantity magnet for the feebler common electro-magnet, shown for the same purpose by Prof. Dana.

The "*other part* of the *combined circuits*," a part essential to be added, before there could be "*combined* circuits," was not exhibited in his lectures; he does not even "remember when he invented it." Questioned, "where it was constructed; whether it was used; then if yea, by whom; how long; how many times; to what extent; and for what purpose?" the general vague, indefinite answer is, "The invention was made and explained to others" (not in his lectures to his classes) "before the publication of Mr. Morse relative to his Telegraph."*

But he is asked, "for what purpose" he made the device. Although we get no definite answer here, we can gather it distinctly from his answers to interrogatories propounded on the same deposition in the Boston case.†

The sixth question is, "Had it not been proposed prior to 1832, to apply electro-magnetism, developed as above stated, to telegraphing?"

Prof. Henry replies, "I cannot say it had been definitely proposed; but I consider my publication, in the nineteenth volume of Silliman's Journal, as stating the applicability of the electro-magnet to telegraphic purposes; and after my call to Princeton, and in my first lecture to the students of that institution, in 1833, I exhibited the experiment of *causing a large weight to fall to the floor*, and stated that this effect *could be* produced at a

* No date is here fixed. The publication referred to cannot be earlier than that of October, 1837, in Silliman's Journal, because he elsewhere says, that this was the first knowledge he had of my Telegraph. So that this fact determines nothing in his favor, or against me as to priority, even if the inventions were in all respects identical. He had, at that date, been in Europe, seen Wheatstone, and returned, before he knew of my invention.

† Smith vs. Downing, pp. 93, 94.

distance, and that bells *might be* rung with it, and other mechanical actions *produced.*"

The purpose, then, of his "device" distinctly is, "to cause a large weight *to fall to the floor;*" "*the short circuit would be broken, and the weight would fall.*" "I frequently illustrated the principle to my class, by *causing in some cases a thousand pounds to fall on the floor.*" "My object in the process I have described was to *to cause a large quantity magnet,* connected with a quantity battery in a local circuit, *to let fall its load.*" This was the definite *purpose* of Professor Henry, and his end was attained, and by what means? "By *opening* the circuit." There is not a word respecting the *closing* of a circuit anywhere in his evidence, and for a very obvious reason—*opening* the circuit was the only act necessary for his purpose. "The short circuit would be *broken,*" (that is, *opened,*) "and *the weight would fall.*" All that he accomplished—all that he desired to accomplish before his class—the entire process was done by *opening the circuit;* if he had then *closed* the circuit again—even if the *actual* circuit, and *ideal* circuit, the two parts of which he speaks, had been actually put together, so as to form combined circuits—the *closing* would not, and could not, restore the *status quo,* and enable him to repeat the operation, without the invention of additional apparatus. Granting, then, for argument sake, that Professor Henry had actually combined his real and his ideal circuit, and made a system of combined circuits, his purpose was not my purpose, nor could my purpose be accomplished by his means; for it is shown clearly, that the *closing* as well as the *opening,* and the *opening* as well as the *closing,* are essential and conjoint operations in the formation of every character that I cause to be made for telegraphic purposes.

Sufficient has probably been said to satisfy any candid mind, that Professor Henry cannot legally or justly lay claim to any such invention or process as I have claimed in the receiving-magnet and combined circuits.

I will only add one word further on this point. The counsel of my opponents, in their zeal for their clients, seem to have been utterly regardless of the awkward position in which they have placed their witness in respect of consistency, by their overstrained efforts to make him the inventor of the receiving-

magnet and combined circuits. Professor Henry, in his evidence, draws this definite and just distinction between the terms *discovery* and *invention*. "The first," he says, "relates to the development of new facts; the second, to the application of these, or other facts, to practical purposes." He puts himself into the first category, priding himself only on being a discoverer of scientific facts, disclaiming explicitly having applied any of these facts to a process. He also says, that in his first lectures, at Princeton, in 1833, "he mentioned the *project* (Barlow's project) of the Electro-magnetic Telegraph, and explained how the electro-magnet might be used to produce mechanical effects at a distance *adequate* to making signals of various kinds; but lest it should be inferred that by thus simply mentioning the project, and explaining the ability of the electro-magnet to produce mechanical effects, the principles he had alluded to had been actually applied by him to a practical purpose, and so had passed from the abstract region of mere *discovery* into the practical and useful one of *invention*—he immediately adds, "I never myself attempted to reduce these principles to practice, or *to apply any of my discoveries to processes in the arts.*" What can be plainer than that Professor Henry intended to be understood that he made no such invention as the combined circuits and receiving-magnet, and intended to guard his language from any such misconstruction. By saying, then, (as Mr. Gillett in his argument boldly asserts) that the receiving-magnet "was invented by Professor Henry, and *used by him*, as he swears, years before Morse claims to have made the invention," he recklessly asserts that which Professor Henry himself repudiates, and leaps the barrier which Professor Henry had placed to shield himself from any such misconstruction; for if he had made such an invention, and "used it," he did "reduce these principles to practice," and did "apply his alleged discoveries to a process in the arts:" the very thing he emphatically disclaims.

But my opponents have two strings to their bow. If Professor Henry did not invent the combined circuits and receiving-magnet, Professor Wheatstone did; and, as if apprehensive that the claim of the former could not be sustained, they bring forward Professor Henry as a witness to show that Pro-

fessor Wheatstone invented such a contrivance before I invented mine. My argument here need not be long. I might take the ground that the contrivance of Professor Wheatstone is not the same as my contrivance, in its parts, its purpose, or results; that it could not accomplish my purpose, while mine would readily accomplish his purpose—and, therefore, granting that he had invented his contrivance before I invented mine, yet that mine being different in parts, and for a different purpose, and producing a different result, there could be no legal interference in our claims to our respective inventions.

It will be sufficient, however, to show that whatever Professor Wheatstone's invention is, mine was invented previously.

I quote from Professor Gale's deposition.

He says—"It was *early* a question between Prof. Morse and myself, where was the limit of the magnetic power to move a lever. I expressed a doubt whether a lever could be moved by this power, at a distance of twenty miles, and my settled conviction was, that it could not be done with sufficient force to mark characters on paper at 100 miles distance. To this Prof. Morse was accustomed to reply—'If I can succeed in working a magnet ten miles, I can go round the globe.' The chief anxiety at this stage of the invention was, to ascertain the utmost limit of distance at which he (Morse) could work or move a lever by magnetic power. He often said to me—'It matters not how delicate the movement may be; if I can obtain it at all, it is all I want.' Prof. Morse often referred to the number of stations which might be required, and which he observed would add to the complication and expense. The said Morse always expressed his confidence of success in propagating magnetic power through any distance of electric conductors which circumstances might render desirable. His plan was thus often explained to me. '*Suppose*,' said Prof. Morse, '*that in experimenting on twenty miles of wire, we should find that the power of magnetism is so feeble that it will but move a lever with certainty but a hair's breadth; that would be insufficient, it may be, to write or print, yet it would be sufficient to close and break another, or a second, circuit twenty miles further; and this second circuit could in the same manner be made to break and close a third circuit twenty miles further; and so on, round the globe.*'"

Here was the invention of the combined circuits, and the means of combination; to wit, the "*lever*," moved by electro-magnetism, to *close* and *break* a *second* circuit used with a receiving magnet, explained to Dr. Gale "early" after his seeing the Telegraph in operation; that is, then, in the early part of the year 1836. But although thus actually described, so that from the description and a knowledge of the purpose a mechanician could easily have made the invention, yet I will not here press this as the time of the invention, but will show the time when the circuits and magnet were *actually made* and experimentally *used.*

Dr. Gale proceeds—"This general statement of the means to be resorted to, now embraced in what is called the '*receiving magnet*,' to render practical writing or printing by Telegraph through long distances, was shown to me more in detail *early in the spring* of *the year* 1837 (eighteen hundred and thirty-seven,) and I am able to approximate the date very nearly from an accident that occurred to me in falling on the ice formed of late snow in the spring of that year. The accident happened on the occasion of removing to Prof. Morse's rooms in the University some pieces of apparatus to prepare a '*receiving magnet*' temporarily." Dr. Gale proceeds to illustrate the invention with a diagram, and adds, "This I depose and say was the plan then and there revealed and shown to me by Prof. Morse, and which, so far as I know, has constituted an essential part of his Electro-magnetic Telegraph from that day till the present time."

If the time testified to had been unaccompanied with any further evidence than that it was—"*early in the spring of the year* 1837"—a doubt might still exist as to the priority of the two inventions, since April 1st, 1837—the time when Wheatstone's plan was shown to Prof. Henry—might come within the meaning of "early in the spring;" but fortunately a circumstance is casually mentioned by Dr. Gale, in his narration, which has enabled me to fix the date conclusively. He says—"I am able to approximate the date very nearly, from an accident that occurred to me *in falling on the ice formed of late snow* in the spring of that year."

Here, then, is an incident which suggests a clue to the exact time; at least, to a time sufficiently remote to show that such an event must have happened before the 1st of April. Supposing

it to be the *latest* ice formed that year, upon which the slipping took place, (which is by no means certain, for it is possible it might have been on earlier ice,) such ice could not be formed unless the temperature of the atmosphere was below 32° of Fahrenheit. Nor could there be ice formed from snow at a freezing temperature, unless there were snow on the ground. There must be a coincidence of the two conditions—*snow* on the ground, and *a temperature to freeze it.* To ascertain this point, I was at much pains to find a record of the temperature and state of the weather in the city of New-York, from February to May of the year 1837; and, after much research, I at length found such a record, which had been regularly kept by a gentleman of the highest intelligence and character, who has for years, without the intermission of a day, made and recorded his observations at 8, A. M., and 3, P. M., and at 10, P. M., daily, at the corner of Prince and Crosby streets, with a northern exposure, and within musket-shot distance of the New-York City University. This record, so far as it bears on the points in question, I here append:—

Daily Record of the Temperature, and State of the Weather, kept at the corner of Prince and Grosby Streets, Northern Exposure, New-York City.

1837. February.	TEMPERATURE. 8 A. M.	3 P. M.	10 P. M.	STATE OF THE WEATHER.
Mon., 20.....	28°	40°	34°	..Somewhat cloudy till 10, A. M.; then northerly, clear and very pleasant for several hours. Evening, cloudy, and after 10, rainy.
Tues., 21.....	32½	37	34	..After a rainy night, a cloudy, disagreeable day; wind moderate. Baltimore harbor clear of ice, and vessels reach Philadelphia without the assistance of steamboats.
Wed., 22.....	30	38	34½	..Somewhat overcast after 10 A. M., but very pleasant; wind light. On Thursday, P. M., a great snow storm at Cleveland, Ohio, Buffalo and Quebec; snow about 2 feet deep, much drifted by a high and very cold wind. At Quebec, mercury 8° below 0, during the day; wind from N. E.
Thurs., 23.....	32½	34	34	..Snow commenced falling at 6, A. M., which, at ½ past 9, A. M., turned to rain, which fell heavily for several hours. After 3, P. M., foggy and misty. A. M., wind fresh.
Fri., 24.....	36	36	31	..Mostly cloudy till noon, then clear, wind fresh.
Sat., 25.....	30	39	31	..Clear and very pleasant; wind light.
Sund., 26.....	30	37	31	..Clear and pleasant; wind moderate; Hudson open to Nyack, about 25 miles.
Mon., 27.....	37½	41	34	..Cloudy, and P. M., rainy; wind light; about ½ past 11, P. M., it commenced snowing.
Tues., 28.....	19	22	17½	..*Snowed* last night about 2 inches, much drifted by a high wind from N. To-day, clear and windy; somewhat overcast and windy, P. M.; a cold day.
March.				
Wed., 1.....	17	27	23	..Somewhat cloudy; wind light; (snow, this day, at Philadelphia, 3 or 4 inches deep,) none here.
Thurs., 2.....	15	23	20	..Mostly clear, A. M.; mostly cloudy, P. M., and in the evening, *light snow*; wind light.
Fri., 3.....	18	26	14	..*Light snow* during the night, about ½ inch deep; after 9, A. M., clear cold morning. (At Quebec, on Monday, a severe snow storm; snow 5 or 6 feet deep, on a level, in the woods.)
Sat., 4.....	10	22	17½	..Clear, calm, and very cold. Good sleighing at Philadelphia, 8 inches deep; and floating ice in the river.
Sund., 5.....	22	32	26	..From ½ past 7 to ½ past 8, A. M., a light fall of *snow*; after 9, mostly clear and pleasant.
Mon., 6.....	23	34	33	..Clear and pleasant. (On Saturday, snow fell 12 inches deep at Savannah, Ga.; deepest snow known for 37 years.)
Tues., 7.....	32	42	34	..Hazy and partly cloudy; very pleasant day.
Wed, 8.....	34	40	40½	..After 9, A. M., rainy, and in the evening, fresh wind.
Thurs., 9.....	40	41	37	..A. M., foggy, and occasional sprinkling of rain; after 4, P. M., rainy.

1837. March.	TEMPERATURE. 8 A. M.	3 P. M.	10 P. M.	STATE OF THE WEATHER.
Frid., 10	34°	41°	36°	Clear and pleasant.
Sat., 11	33	41	34	Clear and very pleasant.
Sun., 12	31	41	36	Somewhat overcast, and P. M., windy.
Mon., 13	37	50	50	Rainy, and P. M., windy; at 11, P. M., a hard rain.
Tues., 14	44	39½	32	Mostly clear and rather windy.
Wed., 15	26	36	30	Cloudy till 9, A. M., then most clear. Freshets to the North and East, by the heavy rains of Sunday and Monday.
Thurs., 16	26	37	30	Clear and pleasant; the ice in the Hudson moved, opposite New-York, on Monday, but remains fast below.
Fri., 17	31	44	36	Clear, and P. M., windy. Hudson open to West Point.
Sat., 18	41	48	40	Cloudy, and P. M., rainy till evening; then windy.
Sun., 19	32	36	26	Clear, cold and windy.
Mon., 20	28	38	32	Mostly cloudy, A. M., and, at 9, a flurry of snow. P. M., clear, windy.
Tues., 21	34	37	34	Mostly cloudy and windy, and after 5, P. M., very rainy.
Wed., 22	36	34½	34	Heavy rain and high wind all night; to-day, very rainy and misty; wind fresh.
Thurs., 23	34	37	36	Rainy; wind moderate; a steamboat, this day, made her way through the ice to Newburg, from New-York.
Fri., 24	36	46	39	Somewhat cloudy, but very pleasant; wind light.
Sat., 25	38	49	39	Clear and very pleasant; freshets in New-Jersey from heavy rain on Tuesday and Wednesday last.
Sund., 26	41	48	39	Clear and very pleasant; Hudson open to Poughkeepsie.
Mon., 27	39	48	38	Cloudy till 10, A. M., and early in the morning some rain; afterwards somewhat hazy, but very pleasant.
Tues., 28	37	49	46	Foggy till 10, A, M.; afterwards hazy and somewhat cloudy, but very pleasant. About 9, P. M., a shower, with some thunder and lightning.
Wed., 29	36	43	30	Clear and cold, rather windy, P. M.; steamboat reached Hudson.
Thurs., 30	28	40	36	Mostly clear till 4, P. M., then overcast.
Fri., 31	37	40	36	Cloudy, and P. M., rainy and misty; this day first steamboat reached Albany.
April. Sat., 1	37	42	35	Rain till 9, A. M.; afterwards clear and pleasant.
Sun., 2	34	45	40½	Clear and pleasant, windy, somewhat cloudy.
Mon., 3	39	51	35	Slight rain early, A. M.; afterwards misty, clear and windy.
Tues., 4	35	33	32	Cloudy, and P. M., flurry of snow; wind light. (Yesterday, at St. Louis, snow 17 inches deep.) No snow whitened the ground in New-York.
Wed., 5	32	42	36	Clear and pleasant.
Thurs., 6	39	49	41	Clear and very pleasant.
Fri, 7	40	48	44	Cloudy, and, after 5, P. M., rainy, with a fresh wind.

1837. April.	TEMPERATURE. 8 A. M.	3 P. M.	10 P. M.	STATE OF THE WEATHER.
Sat., 8	47°	47°	41°	The lowest temperature, after the 5th of April, is on the 23d, to wit: 32½ degrees; not low enough to form ice, and without *snow on the ground*, even if the temperature had been lower. On the 4th and 5th, the thermometer was at 320, but no snow was on the ground.
Sun., 9	40½	47	41	
Mon., 10	40	54	41	
Tues., 11	41½	59	43	
Wed., 12	43	62	46	
Thurs., 13	46	54	43	
Fri., 14	47	53	46	
Sat., 15	45	56	49	
Sun., 16	44	51	49	
Mon., 17	46	55	49	
Tues., 18	43	49	41	
Wed., 19	42	55	43	
Thurs., 20	43	55	43	
Fri., 21	43	50	42	
Sat., 22	40	49	42	
Sun., 23	44	39	32½	
Mon., 24	34	48	41	

On examination of this record of temperature and the weather, it is found that on Sunday, the 5th of March, there was a light fall of snow, with a temperature until Monday noon the 6th, which would keep it from thawing; but that, then, it must have thawed at 34°, and frozen again at night, so that on Tuesday the 7th the two conditions of snow on the ground, and a temperature to form it into ice after a thaw, coincided, and these conditions do not meet again after that date. The temperature is often low enough afterwards to form ice, but the weather has not produced the snow upon the ground coincident with the temperature for that purpose.

On the 20th March for example, there was a flurry of snow, but the temperature could not have formed it into ice even if it were more than a flurry.

No snow lay on the ground after the 6th of March. The conclusion, therefore, appears to me irresistible, that the time of Dr. Gale's accident, in "slipping on ice, formed of late snow," must have been on Tuesday the 7th of March, or if not at that time, it must have been even *earlier* in the season. It could not possibly have been *later*.

Suppose, then, that Prof. Henry was shown by Prof. Wheatstone a device identically like mine on the 1st of April, 1837, my combined circuits and receiving magnet were made and used experimentally *previous* to that date.

But grant that Prof. Wheatstone's device of combined circuits was identical in its construction, purpose, and results with mine

(which is not true); granting, further, that it was invented previous to mine (which, also, is not true); it is perfectly clear that my invention could not be the result of a knowledge of his. This will appear from the fact that December, 1837, is the date of the enrolment of Prof. Wheatstone's patent; and any one conversant with English patent law will readily perceive, that his invention could not have been published previous to that date without nullifying his patent. His imparting his plan to Prof. Henry in April, 1837, was at this risk; and I am not sure that an opponent of his patent might not have used Prof. Henry's testimony on this point in his deposition successfully to annul it.

THE DESTRUCTION OF AN IMPORTANT PART OF THE RECORD BY FIRE.

I come now to notice an important item of testimony bearing on the question, "Did I, or did I not derive from Prof. Henry the scientific knowledge necessary to my purposes in the Telegraph?" This testimony was excluded from any influence in the decision of the Supreme Court, by its mysterious destruction by fire.

It is not generally known, probably, that a portion of the Record, in this case, sent up from the Court below, was thus mysteriously destroyed by fire in the office of the Clerk of the Supreme Court, in the Capitol at Washington, on the — day of April, 1852.

Among the documents thus destroyed was the original sketch-book I had on board the ship *Sully*, in 1832, in which were some of the drawings then and there made of parts of the Telegraph, especially the elaboration of the characters or marks for recording, with the form and proportions of the type, &c., for that purpose. Also a bag containing some of the type, made after the pattern there delineated, and cast in the latter part of the year 1832. Also the original autographic manuscript lectures, 1, 2, 3, and 4, of Prof. James Freeman Dana, as delivered by him before the New-York Athenæum, in 1827. Also the original electro-magnet with which he illustrated the nature of the electro-magnet in these lectures.

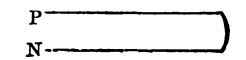
After the announcement of this untoward accident, I presumed these documents, and whatever light they were capable of throwing upon the truth in this case, were irrecoverably lost. But to my surprise and gratification, I have lately discovered [1853] that certified copies of both the sketch-book and Prof. Dana's lectures were taken previous to the transmission of the originals to Washington: The copy of the sketch-book, certified to by John H. Hanna, Clerk Circuit Court, Kentucky District, and the copy of the lectures certified to by George S. Hilliard, Esq., of Boston, Commissioner before whom Mrs. Dana's deposition was taken. I have, therefore, for the purposes of correct evidence, virtually the original documents restored, and shall therefore appeal to these certified copies as to the originals.

I need scarcely remark, that any advantage my cause might have derived from the documents, &c., destroyed, was in consequence of that destruction lost to me. No use was made, or could be made of them, before the Supreme Court. The certified copies were discovered too late to be of service to me there. They may be of some service now.

To show that I do no injustice to Prof. Henry in denying that I derived from him the facts and scientific principles in electro-magnetism on which my Telegraph is based, I quote at some length from these manuscript lectures of Prof. Dana, which, as I have said, *were delivered in public, and in my hearing, in the year* 1827.

In his second lecture, Prof. Dana says, "The effect of the conjunctive wire in impressing the magnetic state is uniform and constant, and we can infer with absolute certainty the kind of magnetism which will be exhibited by either end of a needle, by reference to its position with regard to the wire. We are led to this by our previous knowledge of the positions assumed by a magnetic needle under the influence of the wire. Thus, if the electric current flow from the right hand to the left, and the needle to be magnetized be placed over the wire, the end pointing from us will acquire the austral magnetism, or a north polarity, &c. We have seen that the pole of the magnetic needle, *over* which the positive electricity enters, turns to the east, but the pole *under* which it enters, turns to the west. If,

therefore, a needle be placed between two conjunctive wires situated in the same vertical plane, and transmitting the electric current in opposite directions, it is evident that both will conspire to produce the same effect, which will, consequently, be much more considerable than that produced by either of them alone; but a wire bent in this form, having its two ends connected with the opposite poles of the Voltaic instrument, will evidently have the electric current passing in opposite directions in its upper and lower portions, and, consequently, it will produce on a needle, between them, an effect similar to that produced by the two wires. Wires thus situated produce a more prompt development of magnetism in steel, than a single wire does, because both tend to turn the same kind of magnetism in the same direction, and the opposite magnetisms in opposite directions, and hence we have one method of measuring the action of a battery on steel bars. Again, two parallel wires, having the electric current moving through them, in the same direction, will, evidently, produce a greater effect on a steel bar than either of them alone, for the effect of the whole must be greater than that of a part.



"Where several conjunctive wires are placed together, side by side, the power is apparently diminished in the central wires, and concentrated in the extreme portions; the magnetic state of the latter seems to be augmented by induction or by position.

"When such an assemblage of wires acts on the magnetism of a piece of steel, they decompose it, and each individual wire acts with most force on the magnetism nearest to it. Each conspires in its action to produce the same effect as the others; and hence, in addition to the effects of currents in opposite directions, we have another method of increasing the power of a battery in magnetizing needles. We shall probably render steel strongly magnetic, if we combine these two methods of increasing the effect. *This is effected by forming the conjunctive wire into a spiral around the steel bar to be magnetized;* for, at the opposite extremities of any diameter of this spiral, it is evident that the electric current moves in opposite directions. Suppose the spiral to be placed horizontally, E. and W., the current on its upper part to move from N. to S., it will at its lower part move from

S. to N.; and the spiral thus gives us the combined influence of currents in opposite directions. Moreover, the different coils of the spiral are nearly at right angles with the axis of *the included bar;* and they are parallel to each other. Hence, at any given portion of the bar the effect of many currents passing in the same direction is produced, *and the included bar becomes strongly magnetic; and a spiral placed round a piece of soft iron bent into the form of a horse-shoe magnet, renders it strongly and powerfully magnetic when the electric current is passing through it."* * * * * The opposite sides of a conjunctive wire exhibit the opposite magnetisms; and we have seen that, by placing the wires parallel to each other, and connecting them with a battery so that they may transmit the current in the same direction, that the magnetisms seem to be concentrated in the extreme wires, and that we can thus separate them in a degree from each other. Now, when we consider that the direction of the magnetic power is at right angles to the conjunctive wire, it is evident that in a helix this direction must nearly coincide with that of the axis of the helix, and the one kind of magnetism be found concentrated at one extremity, and the other kind at the opposite end. * * * * Iron filings adhering to dissimilarly electro-magnetic wires, repel each other; and to similarly electro-magnetic wires, attract each other.

"In the course of our reasoning, by which we were led from step to step to the adoption of a spiral or helix in powerfully developing magnetism in bars, we inferred that two or more parallel and similarly electro-magnetic wires acted with greater energy than one, and that the magnetisms were accumulated in the extreme wires by a species of induction between them all. A ribbon of metal substituted for these wires exerts a stronger influence on the needle at its edges than at its sides, for a similar reason. So, also, if *a series of concentric wires* be used, and the electric current sent through them in the same direction, we infer that they will have the power of the corresponding sides of the different rings concentrated and accumulated in their common centre, and will, on the same side of their centre, act as parallel similarly electro-magnetic wires. A *flat spiral or volute*, having two ends connected with the opposite poles of a battery, will correctly represent concentric rings under the condi-

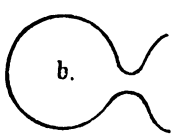
tion we have proposed; and the great quantity of iron filings which such a spiral or volute takes up, and the accumulation of them in the centre, fully evinces the concentration of power there, and the correctness of the reasoning by which we have been led to this modification of the conjunctive wire."

These extracts from Prof. Dana's lectures, demonstrate several important facts.

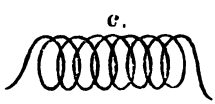
First.—Prof. Henry gives in his deposition the time of his first acquaintance with the science of electro-magnetism; and it will be seen that it was subsequent to the delivery of these lectures of Professor Dana. He says—"I commenced the study of electro-magnetism in 1827." What time in this year does not appear; but, as Prof. Dana's lectures were given in January and February, (and he died in April of that year,) Prof. Henry could scarcely have made much progress in his studies at the time these lectures were delivered; consequently, whatever of information respecting the science of electro-magnetism is contained in these lectures, could not possibly have reached me first through Prof. Henry. They first came to my knowledge through Prof. Dana, whose lectures I attended in 1827.

Second.—I learned from Prof. Dana, in 1827, the elementary facts that lie at the basis of the electro-magnet, to wit:—

a The effect of a *single straight conjunctive wire* in producing magnetism. (Oersted's discovery.)



The effect of a conjunctive wire, *bent into the form of a ring*, for the purpose of increasing the magnetism. (Schweigger's experiment.)



The effect of *a series of these conjunctive wire rings*, forming a *spiral*, for the same purpose of increasing still further the magnetism. (Arago's experiment, at the suggestion of Ampère.)

The effect of a *flat spiral* or *volute*, the conjunctive wire *superposed* upon itself, for still further increasing the magnetism. Schweigger discovered the principle of this modification, and embodied it in his *multiplier*, while Dana applied it to the magnetizing of iron filings in demon-

strating its magnetic power, and suggested it for the electro-magnet.

Third.—I learned from Prof. Dana, in 1827, the rationale of the *electro-magnet*, which latter was exhibited in action. I witnessed the effects of the conjunctive wire in the different forms described by him in his lectures, as a, b, c, d, and exhibited to his audience. The electro-magnet was put in action by an *intensity battery;* it was made to sustain the weight of its armature, when the conjunctive wire was connected with the poles of the battery or the circuit was *closed;* and it was made "to drop its load" upon *opening* the circuit.

These, with many other principles of electro-magnetism, were all illustrated experimentally, to his audience. These being facts, to whom do I owe the first knowledge which I obtained of the science of electro-magnetism, bearing upon the practical development of the Telegraph? Prof. Dana had publicly demonstrated, in my hearing, and to my sight, *all the facts* necessary to be known *respecting the electro-magnet*, so far as it has any relation to the Telegraph, before Prof. Henry had made any experiments whatever in the science of electro-magnetism. Prof. Henry made no subsequent improvement in the electro-magnet, no additions to the *volute modification of the conjunctive wires*, invented by Schweigger, and exhibited by Prof. Dana, that have any bearing whatever on the Telegraph.

The *volute modification* of the helix, to show the concentration of magnetism at its centre, adapted to the electro-magnet, the modification since universally adopted in the construction of the *electro-magnet*, is justly due, I think, to the inventive mind of Prof. James Freeman Dana. Death, in striking him down at the threshold of his fame, not only extinguished a brilliant light in science, one which gave the highest promise of future distinction, but by the suddenness of the stroke, put to peril the just credit due to him for discoveries he had already made. Dana had not only mastered all of the science of *electro-magnetism* then given to the world—a science in which he was an enthusiast; but, standing on the confines that separate the known from the unknown, was, at the time of his decease, preparing for new explorations and new discoveries. I could not mention his name, in this connection, without at least ren-

dering this slight but inadequate homage, to one of the most liberal of men, and amiable of friends, as well as promising philosophers of his age.

It must be obvious, I think, that the question, Who first applied the *volute modification* of the *conjunctive wire* directly to the bar of soft iron? is one altogether immaterial in determining any claim of mine. It lies entirely outside of my invention. The *electro-magnet* and its *armature*, as it exists at this day, was an instrument complete and in common use when I required its aid, and applied it, with adjusting adaptations of my own, to effect my novel purpose; but as an instrument *per se*, it is one to which I have never laid any claim whatever.

The credit, therefore, of the paternity of the electro-magnet is not mine, nor any improvement of it, *per se*, as now employed in the Telegraph. Dana, in 1827, publicly exhibited the electro-magnet, with its spiral conjunctive wire. He also exhibited, at the same time, and directly in the same connection with the electro-magnet, the "flat spiral," or "*volute modification* of the conjunctive wire;" showing its increased power over the single spiral, demonstrating this effect with iron filings, and directly suggesting its application to the soft-iron horse-shoe bar. It surely did not require the actual putting of the iron bar in the place of the iron filings, demonstrably to verify his suggestion.

I have now, I think, satisfactorily shown that I could not have received any aid from Prof. Henry: 1st, *directly* from *personal* interview or *correspondence;* nor, 2d, *indirectly* through others; nor, 3d, from his "*published researches;*" and consequently, the impression that I derived *any* aid from him in the elaboration of my Telegraph, is utterly fallacious.

PROF. HENRY'S COMPLAINT AGAINST ME ON ACCOUNT OF MR. VAIL'S BOOK.

Having disposed of the essential scientific and historical questions at issue in the deposition, there is a *special complaint* of a more personal bearing, preferred by Prof. Henry against me, in connection with Mr. Alfred Vail's work, entitled "The American Electro-magnetic Telegraph," which is made a prominent ground of censure upon me, not only in the deposition

but it seems elsewhere, a complaint which, therefore, demands my attention, and a full exposure.

No correspondence occurred between Prof. Henry and myself from the year 1844 to the year 1846. The Telegraph at this latter period was a successfully established fact, and had become incorporated among the necessary institutions of the country.

It will be perceived, by the correspondence up to this time, that our relations were of the most friendly character. But in the year 1845 an event occurred which suddenly changed their entire aspect. Whether the event alluded to was of a kind justly to disturb these friendly relations, the sequel will show.

Mr. Alfred Vail, the son of Judge Vail, of Morristown, New-Jersey, and brother of the present (1853) Member of Congress, from New-Jersey, the Hon. George Vail, was a student in the New-York City University in 1837. Being a young man of great ingenuity, and having heard of my invention, he was naturally desirous of seeing it as it then existed in my rooms in the University. In the summer of that year he came to my rooms, and I explained it to him, and from that moment to the present, he has taken the deepest interest in the Telegraph. Finding that I was unable to command the means to bring my invention properly before the public, and believing that he could command those means through his father and brother, he expressed this belief to me, and I at once made such an arrangement with him as to secure the pecuniary means and the skill of these gentlemen. It is to their joint liberality, but especially to the attention and skill, and faith in the final success of the enterprise maintained by Mr. Alfred Vail, that is due the success of my endeavors to bring the Telegraph at that time creditably before the public. He was with me, assisting me in its construction, in its first exhibitions to the New-York public, in 1837; to the Franklin Institute, in Philadelphia, and to Congress, in 1838; and from his peculiar experience in all that relates to the invention, was appointed one of my Assistant Superintendents, on the passage of the Telegraph Bill in 1843. On my visit to Europe, in 1838, I learned that electric telegraphs, of various kinds, had been projected and abandoned as impracticable, at various times, from the time of Reiser, 1794. The thought occurred to me to

search the records of science for these various projects as materials to be arranged at some future time, for a History of the Rise and Progress of Electric Telegraphs. With the aid of a scientific friend in Paris, M. Amyot, I made this search at the library of the Academy of Sciences, and made notes of several, to wit: Reizer's, 1794; Dr. Salva's, 1798; Soëmmerring's, 1809; Schilling's, 1833; Steinheil's, 1837; Masson's, 1837; and Amyot's suggestion, 1838. Rough notices of these I brought home. I found no time, however, from the pressure of cares which devolved upon me, to arrange these materials. Mr. Vail manifested a strong desire to publish a work on the Telegraph, and under these circumstances I gave these few materials into his hands, to arrange, to add to, or reject, as he thought proper. It was not my work. Nor did I ever assume any responsibility for its matter, or manner of arrangement. Thus much for the work of Mr. Alfred Vail, on the American Electro-magnetic Telegraph, which was published also at his own expense and risk, and while I was absent in Europe, in 1845.

Soon after my return to the United States, in 1845, I learned casually, in New-York, from some friends, that Prof. Henry had expressed some dissatisfaction towards me, in consequence of the publication of Mr. Vail's book. Of the precise nature of his complaints I had no idea, coming to me as they did, at second hand, and with the vagueness consequent on passing through an indirect channel. My first impression, as well as expression, was, "There is some mistake in this. Prof. Henry and myself are on such terms, that he would not fail to let me know the whole matter directly from himself, if there existed really any serious dissatisfaction." But being about to visit Washington, I took the earliest opportunity on my arrival there, in July, 1846, to question Mr. Vail on the subject, asking him what foundation in his work (which I had not then even examined) there could be for any complaint from Prof. Henry. He seemed wholly at a loss to account for the complaint, which he also had heard. I suggested to him that he ought, as the author of the work, and as an act of courtesy, at least, to write at once to Prof. Henry and have an explanation. With Dr. Gale in company, we sat down in the telegraph office to examine Mr. Vail's work, to discover the cause of the grievance; and as we could only conjecture, we presumed the difficulty was in some omission by Mr.

Vail of some of Prof. Henry's electro-magnetic experiments, when giving the history of electro-magnetism at page 135 of his book. At the moment, Mr. Vail had charge of the telegraphic instrument, and was engaged in receiving from and sending despatches to Baltimore; he complained also of not feeling very well. He turned to me and said to this effect—"I wish you would have the goodness to draft a letter to Professor Henry for me. You know what I would say." To oblige Mr. Vail, as well as to expedite, what I had much at heart, such an explanation as would satisfy Prof. Henry that there had been no intentional slight, either on my own or Mr. Vail's part, I put Mr. Vail's sentiments, which he had just expressed, into letter form, hastily on the spot, and gave it to him to revise. He did so; he approved, and, as he afterwards told me, he copied, signed, and sent it. These are the simple facts in relation to what Prof. Henry says in his deposition of the letter "copied and signed by Mr. Vail, but written by Mr. Morse, as the latter afterwards informed me." I subjoin a copy of the letter deemed so offensive by Prof. Henry as to be treated with contempt. It is as follows:—

WASHINGTON CITY, D. C., *July* (22,) 1846.

DEAR SIR:—I have learned within a few days, from two different sources, that you had made the observation that, in my late work on the Electro-magnetic Telegraph, I had not done you justice. I beg you to believe that if such is the case, it has been wholly undesigned, and I am not now conscious wherein I have offended, whether by act of omission or commission. It has been suggested by Dr. Gale, on looking over the work with the view of discovering the cause, that it might be in that part of it in which I speak of magneto-electricity, page 135, and onward. While engaged on this part of the subject, I not only thought of you, but made many inquiries for your labors, and requested, through a friend, that I might be furnished from yourself with an account of them; but I was so unfortunate as never to hear from you, nor to have found them in print. I am not only willing, but most desirous to have anything on the subject you may be willing to communicate, for I consider you as standing at the head here of this branch of science, and I wish that ample justice may be done to your labors, not only on your own account, but on that of the country which also derives honor from them. I am making preparations for another edition, and be assured I shall feel it to be a great favor to have errors of omission or commission pointed out and corrected. I ought also to

say, in justice to Prof. Morse, to whom I learn you attribute the blame, (on the supposition that he revised my labors,) that my book was published while he was absent in Europe, and that this part was not seen by him until it was in print.

Respectfully, Sir,

Your obedient servant,

ALFRED VAIL.

To PROF. HENRY, *Princeton.*"

The fate of this letter is told by Prof. Henry himself, in his deposition. "To the letter I did not think fit to make any reply." Of this determination, however, I then knew nothing; and as I heard nothing through Mr. Vail of the grievance of Professor Henry from July to October; and then hearing again, through a fresh channel, that this grievance still existed, I determined to know the length and breadth of it, by writing to Prof. Henry myself. Consequently, the following letter was written and sent, and received. It is referred to by Professor Henry in his deposition, when he says—"I afterwards received a letter from Mr. Morse, in his own name, on the same subject; to which I gave a verbal reply in January, 1847, in Washington."

WASHINGTON, *Oct. 17th*, 1846.

MY DEAR SIR:—Some months ago I learned, with much surprise and regret, that some dissatisfaction rested in your mind in regard to me. The person who first informed me was Dr. Gale, and more recently, our mutual friend, Prof. Ellet. He informed me, in substance, that you felt aggrieved, considering that I had not done you justice in my account of the Magnetic Telegraph. I expressed my astonishment, and searched my memory to know how, or where, or when, I could have done you the alleged injustice, conscious, as I was and am, that no man in the country held a higher place, in my respect and affection, than yourself. Dr. Gale suggested it might be in the work of Mr. Vail, my assistant. Prof. Ellet also stated, that you considered me responsible for the statements and conduct of that work. I immediately took up the work, and, eagerly, turned over its pages in search of the injustice, on my part, which, if anywhere, must be found in the Congressional reports, and also to ascertain if injustice had been done anywhere by Mr. Vail. I could only conjecture, and presumed that it might possibly be in the account which Mr. Vail has given of magneto-electricity, page 135, and onward. If here there has been any omission of your name and discoveries, I am confident that a plain statement of the position I hold to Mr. Vail's work will

at least exonerate me from blame; and I am also sure, from the high consideration in which I know that you and your labors are held by Mr. Vail, that no intentional omission was made on his part. In regard to the work in question, I had long desired to have the history of electric telegraphs collected and published, that justice might be done to foreign savans, who had conceived plans of telegraphs, as well as to myself. I had collected accounts of several of the projects devised in Europe, and hoped to have the leisure to put them in form for publication myself, but my other absorbing duties prevented, and Mr. Vail, being desirous of preparing such a work, I gave him my materials, which consisted solely of the plans of different European electric telegraphs, to which materials he made many additions from his own researches, and to him was consigned all the labor, and all the responsibility of their preparation, and the credit and profit which might arise from the work. Just before I left for Europe, in the autumn of 1845, I read over, at Mr. Vail's request, for correction and comparison with the plates, the manuscript of a small portion of this work, consisting only of the plans and descriptions of my own Telegraph. The work was completed and published by Mr. Vail while I was absent, and for his own benefit. You will see by this statement what share I could have had in any omissions of Mr. Vail.

So soon as I returned to Washington in July last, I apprised Mr. Vail of what I had heard in regard to your feelings, and he also expressed his surprise and regret that anything should have occurred, even by accident, which should have given you pain, and I immediately suggested the propriety of his writing to you, to disabuse you of any unfavorable impression his omission may have occasioned in your mind. At his request, I prepared the letter dated about 22nd of July, which he informs me, he copied and sent you. To this letter he says you have not replied; I suppose, nevertheless, that it would exonerate him, and myself certainly, from any blame which a misapprehension of my agency in the work may have produced. On Tuesday last, the day before I left New-York, however, I accidentally fell in with Mr. George Prosch, who had recently (he told me) received your kind hospitalities. Mr. Prosch gave me to understand that you had spoken of me (how lately I don't know) in terms which implied that you still felt much hurt at something I had done or had not done. But I could not learn what it was, nor have I any clue to the cause of your altered feelings towards me. Now, my dear sir, I cannot believe you capable of doing another the injustice to entertain suspicions of him, without giving him some opportunity to exculpate himself. If I have given you any cause to feel aggrieved, I beg you to tell me of it in all frankness, and then if I do not satisfy you that you have had groundless suspicions, you

will have a right to feel aggrieved. If I have done you any wrong, I will make you ample amends. I am certainly unconscious of ever doing you the slightest injustice, in thought, word or deed; on the contrary, I am conscious of entertaining only the most exalted opinion of your genius and labors, and of having on all occasions, where your name was mentioned, or when I had occasion to speak of you, thus expressed my feelings of respect and affection; and in view of all this, I confess that I feel unfeigned mortification to learn, not from one, but from several sources, that you should have any misapprehension on the subject. I can truly say, that if I had heard that my own brother was thus entertaining such unfounded suspicions of me, in regard to himself, I could not feel more surprise or more grief. I trust I need not say more, and that you will let me know from yourself, and not through others, the nature and extent of my supposed offending, so that if I have indeed unwittingly offended, I may make amends; and if I have not, that you may be satisfied that your suspicions are groundless.

With sincere respect,

Your obedient servant,

To Prof. JOSEPH HENRY, *Princeton.* S. F. B. MORSE.

Is it not true, as I stated in the beginning, that Prof. Henry has given his deposition "*not in ignorance of facts which make his statements incorrect?*" He states that he "*presumed the publication of Mr. Vail's work was authorized by me,*" yet he knew that it was not authorized by me. Was he not informed of this fact in this letter of Oct., 1846? It is not for me to explain the cause of his disbelief of my statement in this letter. He knew these facts in 1846; he deposes, in 1849, as if he were in utter ignorance of them.

Besides the "verbal reply" to this letter, which Prof. Henry says he gave in January, 1847, he also wrote the following, a few days after its receipt:—

PRINCETON, *Oct. 23d,* 1846.

MY DEAR SIR:—Your letter of the 17th was received two days ago, but as we are now in the midst of our quarterly examination, and I have nearly 70 examination papers to read and classify within the next five or six days, I shall be obliged to defer my answer in full until next week.

In the mean time, I remain,

Respectfully yours, &c.,

(Signed) JOSEPH HENRY.

S. F. B. MORSE, LL. D.,
Professor, &c.

The written "answer in full" was not given; but Prof. Henry's version of the verbal one in January, 1847, I will now examine.

THE TWO INTERVIEWS—THE INTERVIEW OF 1847.

Among the other matters deposed to on oath by Prof. Henry, he relates the incidents of two interviews, both of which I sought, and held with him, at two several times; one in Dec., 1846, (he says January, 1847,) and the other about the beginning of 1848, in Washington; the first, for an understanding of the nature, and an explanation of the difficulties that had arisen in regard to Mr. Vail's book; and the other, more especially for the purpose of learning, direct from Prof. Henry himself, what were his precise claims to any discovery bearing upon, or seeming to interfere with my claims to any part of my telegraphic invention.

Since Prof. Henry has thought it important to introduce the incidents of these interviews into his testimony, whether they are really important or not, it becomes quite as important that his statements, or rather, I must say, his *misstatements*, (without imputing motive or cause,) should be corrected, lest they unjustly prejudice my position.

After the middle of Dec., 1846, both Prof. Henry and myself being in Washington, I immediately sought him, and the first interview alluded to was held. I then learned, directly from him, that the cause of his dissatisfaction was the omission, by Mr. Vail, in his work entitled "The Electro-magnetic Telegraph," of all other mention of his (Prof. Henry's) labors and discoveries than the fact, that "Prof. Mohl and himself had made powerful electro-magnetic magnets."

From the deep feeling manifested by Prof. Henry on this point, I was under the belief that there must have been some great oversight on the part of Mr. Vail in making such a statement. I then explained my position in regard to Vail's book, stating distinctly what I had previously written him in October, repeating the substance of that letter, and especially stating that if Mr. Vail had omitted mention of his researches, I knew it must be unintentional, for that Mr. Vail had informed me,

that so desirous was he to have some account of Prof. Henry's labors, that he had applied to him through a friend, previous to the publication of his work, to write that part of his work relating to electro-magnetism, for the express purpose of being correct on that point; and that should another edition be published, I had no doubt that Mr. Vail would most gladly embrace the opportunity of doing him justice, and would insert any statement he might wish. The reply to this, on the part of Prof. Henry, I cannot forget, and, I own, it struck me with a sad surprise. He seemed quite excited, and broke out violently against Mr. Vail, with expressions to this effect: "Mr. Vail? I will have nothing to do with Mr. Vail. What right has Mr. Vail to write the history of electro-magnetism? He knows nothing about it." To this I replied, in a tone to mollify his excitement, to this effect: "Why, Prof. Henry, it will not do to say, in our free country, that any man may not write on any subject he pleases;" and finding that Mr. Vail's name only irritated him, I turned the subject by saying to this effect: "Well, Prof. Henry, I will take the earliest opportunity that is afforded me in anything I may publish, to have justice done to your labors; for I do not think that justice has been done you, either in Europe or this country." These were my sentiments at that time. The interview, on the whole, closed pleasantly, so far as I could judge, for I met him several times afterwards, and supposed that, as between him and myself, at least, there was no unpleasant feeling. On my part there never had been any.

Some time during the year 1847, however, I casually heard that Prof. Henry manifested afresh the same feelings of dissatisfaction; and, on inquiry, learned that he had said: "Mr. Vail has published another edition of his work, and has made no change, or addition, in regard to my researches." On learning this, I inquired of Mr. Vail if he had lately published another edition of his work? He replied: "No; a few copies more have been struck off from the stereotype plates, to enable me to pay expenses of publication for which I am in debt, and the year in the title page, 1845, has been changed to 1847, one figure only; that is the only change." "Ah," I observed, "that must be the new grievance of Prof. Henry." I may here

remark, that the contemptuous feeling manifested towards Mr. Vail by Prof. Henry, forbade the idea that he expected, or even wished, on his part, any notice of his researches by Mr. Vail in any new edition, as did also the total neglect of Prof. Henry to reply to Mr. Vail's courteous letter of July, 1846, in which he expressed his desire to have the most authentic means of doing him justice, by a detail of his researches from Prof. Henry himself. Such feeling, on the part of Prof. Henry to Mr. Vail, was certainly not calculated to render the latter over-anxious to trouble himself further on the subject, even if he had made an enlarged and revised edition of his work.

THE INTERVIEW OF 1848.

In January, 1848, there was another interview, sought also by me, with Professor Henry. The immediate occasion of my seeking it was the submitting to me, in manuscript, by the late Professor Sears C. Walker, of the Coast Survey, his report, previous to its publication, containing some remarks on the "*Theory of Morse's Electro-Magnetic Telegraph*," in which the expression "the helix of a soft-iron magnet, prepared after the manner first pointed out by Professor Henry," occurred. When, therefore, Professor Walker submitted to me this report —and I noticed this allusion to "the magnet," as quoted above —I at once observed to Mr. Walker to this effect: "I have now the long-wished for opportunity to do justice, publicly, to Henry's discovery, bearing upon the Telegraph; I should like to see him, however, previously, and learn definitely what he claims to have discovered; I will then prepare a paper, to be appended and published as a note, if you see fit, to your Report." Mr. Walker had also a mistaken notion that the *receiving-magnet* (the instrument which I invented) was Henry's invention. This I disputed, and, to determine this point, I invited him to accompany me to Professor Henry's rooms. The proposition was acceded to by Mr. Walker; we immediately repaired to the Patent Office, where Professor Henry had his office, and meeting with Professor Gale on the way, he also accompanied

us. The interview was held. From all that passed, it was perfectly clear to me that Professor Henry had no such idea as a *combination of circuits*, much less anything like what is known as my receiving-magnet, previous to his visit to Europe in the spring of 1837. Professor Henry's memory of dates, I have shown, is, unfortunately, very defective; and to this I would rather attribute his defective narration, than to any design to misrepresent. When he hinted that he thought that he had the *idea*, before he went abroad, I questioned him closely and particularly on the point, and he hesitated, and expressed doubts whether it was before or after, and said that he did not dare to say positively. His hesitation and whole manner were so convincing to me, at the time, of his not having conceived even the *idea*, much less invented the *thing*, that I well remember saying to Dr. Gale, in reference to it, as we left the room together, to this effect: "There is nothing there, Doctor, which at all interferes with my receiving-magnet invention." Mr. Walker, too, was satisfied that what was called the *receiving-magnet* was not Professor Henry's invention; but he considered that the electro-magnet, which was a part of the combination in its construction, owed its efficiency to what he deemed to be Henry's discovery, to wit: the means of propelling a galvanic current through a long wire, by the use of an intensity battery. On referring to Henry's article in *Silliman's Journal*, I readily conceded that point,* for I then supposed it to be due to him. If I have since changed my opinion, it is because, compelled to make a deeper research, I have found that this was an undisputed fact, from experiment, at least eighteen years before Professor Henry's published experiments in 1831.

As the immediate result of that interview, I drafted the proposed note to Mr. Walker's report on February 1st, 1848, and sent it to him the same day. As its contents have an important bearing upon the character of Professor Henry's deposition, in several vital particulars, I here give it in full.

* Such was the deference which I then bore towards him, and such the confidence I reposed in him, that the simple fact of his making a claim would, at that time, have found me in a state of *heart*, at least, to concede to him anything without question.

WASHINGTON, *January* 31*st*, 1848.

DEAR SIR,—I have perused, with much interest, that part of your manuscript, entitled "*Theory of Morse's Electro-magnetic Telegraph*," which you were so kind as to submit to my examination.

The allusion you make to "the helix of a soft-iron magnet, prepared after the manner first pointed out by Professor Henry," gives me an opportunity, of which I gladly avail myself, to say, that I think justice has not hitherto been done to Professor Henry, either in Europe or this country, for the discovery of a scientific fact, which, in its bearing on telegraphs—whether of the *magnetic needle*, or *electro-magnet* order—is of the greatest importance. To elucidate this, let me briefly state the condition of science, as its discoveries affected the practicability of Electric Telegraphs at the period of Henry's discovery, in 1830. The first plans of telegraphs by electricity were confined, till the year 1800, to *machine electricity*, but from the intractable nature of the agent employed, all of them proved unavailable. Telegraphs by electricity were at that period, fifty years from the first suggestion of the idea by Franklin, abandoned as impracticable. In the year 1800, the chemical effects of Volta's pile revived the idea of Electric Telegraphs by the employment of the *decomposing* effects of Voltaic electricity: and a telegraph, on this scientific fact, received a definite form in the complicated and unavailable plan of Soëmmerring, in 1811. In the year 1819, Oersted's discovery of the *deflection of the magnetic needle*, by the Voltaic current, gave to inventors a new fact in science, to combine with former facts, for the production of an Electric Telegraph. From this discovery of Oersted's, dates the era of needle telegraphs; for Ampère, in the year after, in 1820, suggested the plan of a needle telegraph, of seventy-two conductors and thirty-six needles, one for each letter of the alphabet, and the numerals: but it remained, for at least twelve years, a mere suggestion; and I have not been able to find that one of the most essential *points* requisite to the invention of an Electric Telegraph had, as yet, been ascertained: to wit, *its practicability at a distance.*

Four years after Ampère's suggestion, indeed, in 1824, Prof. Barlow, of the Royal Military Academy at Woolwich, instituted experiments upon the "*magnetic-needle*," through various lengths of wire up to 840 feet, for the purpose of determining this very point, to wit: *the practicability of producing magnetic effects at a distance.* With the Voltaic battery which he employed, he found the magnetizing effects of electricity diminishing

so greatly in that short distance, that he in consequence pronounces "*the idea of an Electric Telegraph to be* CHIMERICAL." If these experiments and results of Barlow are indicative of the state of opinion of the scientific mind of Europe, at that time, which may at least be presumed, from his position—then in 1824 the voice of science in Europe had declared *the idea of an Electric Telegraph to be* CHIMERICAL. I would draw your special attention to the fact, that the experiments of Prof. Barlow had no reference to the *electro-magnet*, as applied to telegraphic purposes. The state of science, indeed, forbids that he could have had any such reference; for it was not until the year after, in 1825, that *Sturgeon*, according to Jacobi, and *Dal Negro*, as quoted by Dr. Schulthess, made the first electro-magnet, by magnetizing soft-iron, *in a horse-shoe form*, by means of copper wires wound around it. In the interval of the years between 1824, when Barlow pronounced the Electric Telegraph chimerical, and 1829, I do not find any suggestions on the subject of Electric Telegraphs. The result of the experiments of Barlow seem to have effectually palsied all efforts in that direction. Experiments in electro-magnetism were now confined to the production of *motive power*, and the attainment of greater *magnetic power* in the electro-magnet. This was the state of electro-magnetism, when, in 1830, Prof. Henry, assisted by Dr. Ten Eyck, while engaged in experiments on "*the application of the principle of the galvanic multiplier to the development of great magnetic power in soft-iron*, made the important discovery that *a battery of intensity* overcame that resistance in a long wire, which Barlow had announced as an insuperable bar to the construction of Electric Telegraphs. Thus was opened the way for fresh efforts in devising a practicable Electric Telegraph; and Baron Schilling, in 1832, and Profs. Gauss and Weber, in 1833, had ample opportunity to learn of Henry's discovery, and avail themselves of it, before they constructed their needle telegraphs.

The fact should not be lost sight of, that up to this date, (1833,) and, indeed, up to the year 1841, there was no European Electro-magnetic Telegraph proposed on any other principle than the *deflection of the needle*. It is at the date, 1832, of Baron Schilling's invention of his Needle Telegraph, (since abandoned as impracticable, from various and obvious causes,) that I conceived my *Electro-magnetic Telegraph*, and first devised an apparatus applying *magnetism produced by electricity*, or the power of the *electro-magnet to imprint characters at a distance*, and these through a *single circuit* of conductors. Here are several substantial, novel, and distinct features of an Electric Telegraph never before suggested by any one. No one had ever before hinted at the employment of the *electro-magnet* for telegraphic purposes; no one had ever before hinted at the *possibility of im-*

printing signs at a distance; no one had ever before hinted at the possibility of even producing intelligible, distinct signs, by Voltaic magnetism at a distance, *through a single circuit of conductors*, much less to *imprint* them at a distance through a *single circuit of conductors.* These were all new features of a Telegraph, and when conceived, I was utterly ignorant that the idea of an Electric Telegraph, of any kind whatever, had been conceived by any other person. I took it for granted that the effects I desired could be produced at a distance, and accordingly in the confidence of this persuasion, I devised and constructed my apparatus for the purpose. I had never even heard or read of Prof. Henry's experiments, nor did I become acquainted with them until after all my apparatus was constructed, and in operation through half a mile of wire, at the New-York City University, in1837. I then learned, for the first time, that an Electric Telegraph of some kind had been *thought of* before I had thought of it; that Barlow had had an idea of an Electric Telegraph, but what that idea was, I had not learned even till within a few weeks past. I also then learned the gratifying fact, that Prof. Henry had actually, two years before my first conception in 1832, in 1830 settled the point of *practicability at a distance*, and had shown that magnetic effects could be produced at a distance, as well in magnetizing the *electro-magnet* as in *deflecting the needle.* While Prof. Henry, therefore, *does not suggest the use of the electro-magnet for telegraphic purposes*, but has direct reference only to *the magnetic needle telegraph*, when he remarks that "his experiments were directly applicable to Mr. Barlow's project of an Electro-magnetic Telegraph," (the *needle telegraph* only being "Barlow's project,") yet his experiments predetermined the practicability of using an electro-magnet also, so soon as I or any one else should propose a plan for its application. While, therefore, I claim to be *the first* to propose the use of *the electro-magnet for telegraphic purposes*, and *the first to construct a Telegraph on the basis of the electro-magnet*, yet to Prof. Henry is unquestionably due the honor of *the discovery of a fact in science*, which proves the practicability of exciting magnetism through a long coil, or at a distance, either *to deflect a needle* or *magnetize soft-iron.*

Let it be borne in mind that all the Electro-magnetic Telegraphs in Europe were *Magnetic Needle Telegraphs*, till the year 1841. My *Electro-magnetic Telegraph* had been publicly exhibited in New-York in 1837, in Paris in 1838, and in London in 1839. It was not till 1837 that Cooke & Wheatstone had invented even their *Needle* Telegraph; and Steinheil in the same year (1837) invented his *Needle* Telegraph; and then, in 1841, for the first time in Europe, is announced a Telegraph using the *electro-magnet*—to wit, "*Wheatstone's Rotating Disc Telegraph*;

and the object of this apparatus is, to *show*, not to *record* signs. It may be further allowed me to say, that, until the government experimental line from Washington to Baltimore had been fully and satisfactorily tested and proved successful, no plan of a Telegraph even proposing *to record permanent characters* by the *electro-magnet*, through *a single conductor*, *at a distance*, had been published in Europe or America.

With great respect, your obedient servant,

SAMUEL F. B. MORSE.

SEARS C. WALKER, Esq.

This note of mine was written, as I have said, immediately after the interview in January, 1848. If it shows errors of fact, these it will be seen are all deferential to Prof. Henry's assumptions, and manifest at least my own disposition to award full justice to him. Others may accuse me of injustice towards them; he cannot, towards himself. Prof. Henry's account of that interview is in his deposition, which was sworn to September, 1849, and, therefore, nearly two years after it was held. Prof. Henry had this note of mine submitted to him, by my request, through Mr. Walker, long before giving that deposition that he might criticise and approve it before it should be published. I wished to have full justice done to him; and promised Mr. Walker that I would incorporate into the statement any suggestions from Prof. Henry which would make it more accurate.

To this end, I wrote the following letter to Mr. Walker:—

WASHINGTON, *February* 1, 1848.

MY DEAR SIR: — I return your manuscript with many thanks. I have prepared a letter which, if consistent with the design of your Report, I should be pleased to see in the Appendix, or as a note. I am having it copied, and will then transmit it to you.

I should be glad to have Prof. Henry see it and approve of it, and criticise it before it is published. Should he make any suggestions that will constitute my statement more accurate, I should be glad to be apprised of them, that I may incorporate them into it. Is the year 1830, or 1829, the proper date of his discovery?

With great respect,

Your most obedient servant,

SAMUEL F. B. MORSE.

SEARS C. WALKER, Esq.

The copy alluded to was finished and sent to Mr. Walker the same day, (Feb. 1, 1848.) After waiting some days, and not hearing anything of the disposal of the note, I wrote again to Mr. Walker the following:—

WASHINGTON, *February* 10, 1848.

DEAR SIR:—I am about making a communication to the French Academy of Sciences, and should be glad to have the decision in regard to the disposal of the note I addressed to you on the subject of Prof. Henry's discovery, as soon as you can make it convenient. I shall be in the city but a few days longer.

Most truly your friend and servant,

SAMUEL F. B. MORSE.

SEARS C. WALKER, Esq.

To this I received from Mr. Walker the following reply:—

WASHINGTON, D. C., *February* 11, 1848.

DEAR SIR:—I hope you will excuse the delay in answering your several esteemed favors. I immediately forwarded my Report and your letters to my principal, Professor Bache, with a request that they should be shown to Prof. Henry. I called yesterday on Prof. Henry to know the result, but did not find him in, otherwise I should have been able to answer you yesterday. I will call again to-day, and will lose no time in communicating the result. I should have answered promptly, had the result depended on my action alone.

Yours, truly and respectfully,

SEARS C. WALKER.

To PROF. SAMUEL F. B. MORSE.

P. S.—I congratulate you on the recent accession to your new dignities. I am quite desirous of seeing the Turkish insignia. Yours, S. C. W.

No communication on the subject, however, was made to me from Prof. Henry, through Mr. Walker, or any one else; no approval, nor criticism, nor suggestions. The Report of Mr. Walker was published in the Coast Survey Report, and without my note appended, or any allusion to it; nor could I learn any reason for the silence maintained by all upon the subject. I had, however, done all that I conceived to be justly required of me under the circumstances. I had given full credit to Prof.

Henry for the discovery which he claimed, and to which I then thought him entitled. I had fulfilled my promise to him, made at the interview of 1846, to wit, that "when I had the opportunity, publicly, to say anything on the subject, I would endeavor to see that justice was done to him." But, presuming from his slight of the opportunity I had afforded him of setting the matter before the public, just as he would wish it to be represented, and that he preferred that nothing should be said or done, I, also, dismissed the subject from my thoughts, until brought up again by the manner in which it is set forth, nearly two years afterwards, in his deposition of 1849.

Now, in the light of these facts, let that part of his deposition, in which he assumes to set forth the matter, be examined. [See Deposition, "My researches, &c, page 109."]

The impression naturally made by this part of Prof. Henry's deposition, whether intended to be made or not, is clearly this: that I was indebted to a knowledge of his experiments, and to a law which he had deduced from them, for success in the early construction of my Telegraph; that this must be so, because Dr. Chilton had stated that he had referred me to Henry's paper in Silliman's Journal, before I made my experiments in the University; that the book published by Mr. Vail, on the Electro-magnetic Telegraph, although professedly edited by Mr. Vail, and copyrighted by him, was, nevertheless, my work, because Mr. Vail was my principal assistant, and was also interested in the telegraph patent, and, therefore, I was responsible for its contents; that this book of Mr. Vail's must contain some grievous wrong to Prof. Henry, worthy of severe reprehension, a just ground for the manifestation of a retaliatory feeling; that when complaint was made to my friends, by Prof. Henry, I wrote a letter to him under the name of Mr. Vail, while the letter was actually my own, and signed by Mr. Vail to keep up the deception, yet, I then had the weakness to confess the trick to him; that in the interview of January, 1847, I acknowledged that injustice had been done him, that is, that I had done him injustice, and promised him reparation, yet that another edition of Mr. Vail's book had been published, and still this grievous wrong remained. Is not this the impression left on the reader's mind?

I will stop at this point, and examine so much of this extraordinary narrative.

In the first place, I have not the slightest recollection of having been referred to Henry's paper by Dr. Chilton. I remember having casually seen at Dr. Chilton's store, in Broadway, in what year I do not recollect, the large magnet of Prof. Henry, like that delineated in Silliman's Journal of 1831. It was the *quantity* magnet, (*now* so called by Henry,) and it is very probable, although I do not recollect it, that Dr. Chilton in showing me its astonishing power, did speak of Henry's paper, in which he describes the manner of constructing it; but from the character of that magnet, so easily comprehended from inspection in a few minutes, with the thing itself embodied before me, I had no particular motive for consulting the published account of it. I needed no written description of a thing understood in a moment from sight, especially, too, as I could not but perceive, at a glance, that it had *no applicability whatever* to the Telegraph. This *quantity* magnet, as thus distinguished by Prof. Henry himself, is not *applicable* to the Telegraph, as I have before observed, is not necessary to it, and never has been applied to it anywhere, so far as I have any knowledge. If, therefore, Dr. Chilton spoke of Henry's paper to me at the time of explaining this *quantity* magnet, which he may well have done, there was nothing in the condition of my invention to induce me to seek for it, or to consult it, or to charge my memory with the fact, that it was mentioned to me. The general impression left on my memory, from seeing that magnet at Dr. Chilton's, is the same that has everywhere else been entertained, to wit, that Prof. Henry had succeeded in his attempts beyond all other previous attempts, in constructing a magnet capable of sustaining an immense weight. That this was the general impression in the scientific world I will presently show. In looking at that magnet, I do not think that, at that time, I could have associated the Telegraph naturally with it at all, any more than if I had been looking at a steam-engine. However this may be, the fact is, that I did not then see nor read Henry's paper, nor did I see or read it till the autumn of 1837.

But what shall I say of the manner and matter of Professor Henry's reference to Vail's book? I have already shown, by

letters and dates, the true position I held in relation to that work. I have shown that, on learning that there was any feeling of dissatisfaction on the part of Professor Henry, on account of its publication, I took the earliest opportunity to disabuse his mind in regard to the character of my relationship to it —and to show, also, that if there had been any error of commission, or omission, on Mr. Vail's part, that it was undoubtedly inadvertent. Notwithstanding all these efforts, however, in correspondence and in two interviews, the grievance seems to be of so flagrant a character, so unpardonable, so incapable of any apology, that it must be dragged into his solemn deposition, some two or three years after, altogether gratuitously, saving only to discolor and complicate the pertinent facts of the case. A grievance so inveterate, that has withstood so many efforts to allay and to remove, must surely be of a most serious character, and deserves being distinctly brought out and examined.

Professor Henry shall state this grievance in his own way: "I was, therefore, much surprised on the publication, in 1845, of a work, purporting to give a history of the Telegraph, and of the principles on which it was founded, by Mr. Vail, then principal assistant of Mr. Morse, and one of the proprietors of his patent, to find all my published researches, relating to the Telegraph, passed over, *with little more than the remark, that Dr. Mohl and myself had made large electro-magnetic magnets.*" Here is "the head and front of my offending;" the cause of the alienation and ill-feeling manifested towards me by Professor Henry ever since 1845, the date of the publication of Mr. Vail's book. I say, "of my offending:" for the offence, such as it is, is charged upon me, and the retaliation is visited upon me; although, I think, it is far from obvious that I am the offending party, notwithstanding the ingenious attempt to show that I stood towards Mr. Vail's book in the somewhat anomalous relationship of "*presumed*" godfather to it. But let that pass. Professor Henry has needlessly committed himself on a mere *presumption*, which proves to be without any foundation—and this, too, when the absolute truth in regard to it could be had for the simple asking. Nay more, he commits himself after this presumption of his *had been shown to be without foundation;* when, at least three years before he gave his deposition, *he knew* that I never had any responsibility whatever for the work.

But it is worth while for a moment to examine *the offence*, by whomsoever committed, and see if it be of a character to create such a perfect revolution in the mind of an ingenious and ingenuous philosopher, to turn it from confidence to suspicion—from friendship to hostility.

Mr. Vail, failing to obtain from Professor Henry himself a more detailed account of his scientific researches than he had found published, but desirous to do ample honor and justice to his scientific labors, found to his hand, in the "Encyclopædia Britannica," the paragraph *complimentary* to Professor Henry which he has quoted in his book, at page 134; it is in these words: "The next step in the progress of discovery, was that of making magnets of extraordinary power by means of a galvanic battery. This seems to have been first accomplished by Professor Mohl, of Utrecht, and Professor Henry, of Princeton, who was able to lift thousands of pounds weight by his apparatus." I say *complimentary*, for it gives him the credit of having made, about the same time with Professor Mohl, of Utrecht, electro-magnets of extraordinary power. That this was the great aim, the main object of Professor Henry, in his experiments—as given in his article in "Silliman's Journal," of 1831, the only great scientific fact developed in any of his researches up to that time—is manifest to any one who will read it. It is the only one deemed by the scientific world of sufficient importance to mention, so far as I have been able to discover—and I have searched with some diligence, so far as leading works have been accessible—and I find that the having developed great magnetic power, and that by a mode of winding the magnet quite original, (the mode exhibited, in the diagram in his article, in his *quantity-magnet*,) is the only fact Professor Henry had published which commanded the attention of the scientific world. For example:

Professor *Daniel*, in his "Introduction to Chemical Philosophy"—and Professor *Noad*, in his "Lectures on Electricity," while they speak quite at length of Henry's experiments on induction, published in his "Contributions," in 1838, say nothing of his experiments on the electro-magnet, in 1831.

Mr. *Sturgeon*, in his "Annals of Electricity," speaking of Prof. Henry, refers only to "his raising the magnetic action of soft-iron," developing "new and inexhaustible sources of force"—

"available as a mechanical agent," contemplating nothing more than an application of the magnet (the *quantity* magnet) to a magnetic engine, giving Henry the credit "for the first working model."

L'Abbe Moigno in treating, expressly, of Electric Telegraphs, with Henry's paper before him, finds nothing there worthy of mention, as bearing on telegraphs, but casually notices that "Sturgeon and Pouillet, Henry in the United States, and Robert, of Manchester, had obtained *astonishing results, in causing electro-magnets to lift many tons weight.*"

De la Rive, in his late elaborate work, "Treatise on Electricity," published this present year (1853), is as silent on the point of stating any other fact of importance as all the rest, and speaks of Henry only in connection with Mohl, Liphaus, and Quetelet, as engaged in researches, "whether it is better that the wire coiled round the two branches of the electro-magnet should be continuous, so as to be traversed successively by the whole of the current, or, if it would be preferable for it, to be divided into one or a greater number of wires, among which the total current should be divided." This latter being Prof. Henry's plan of the *quantity* magnet.

In the *Encyclopædia Britannica*, article *Magnetism*, there is a notice of Prof. Henry's experiments of 1831; but here, as elsewhere, the *great lifting power* of the *quantity* magnet is the sole burden of the writer's notice.

Dr. *Schulthess*, in a lecture on electro-magnetism, delivered to the Philosophical Society at Zurich, Feb., 1833., (see Taylor's Scientific Memoirs, vol. I., p. 534,) speaks also of Henry's experiments thus: "The experiments of Henry and Ten Eyck showed that the *power* of such magnets" (electro-magnets) "might be *greatly augmented*;" and again, "I could not refrain, when speaking of the *powerful electro-magnets* of Henry and Ten Eyck, from asking the question, whether such a *considerable power* as that which is obtained by interrupting the electric current and then restoring it, could not be applied, with advantage, to mechanical science?" In this case, also, it is seen that the *great power* of the quantity magnet of Henry, is that which attracted the attention of this eminent Swiss philosopher.

In vol. XX., 1831, of Silliman's Journal, under the head of

"*Galvano-magnetism*," I find that Henry's experiments, detailed in the January volume, induced *Dr. Hare*, of Philadelphia, and *Prof. Webster*, of Boston, each to repeat his experiments, and to communicate to the Journal their results. The whole burden of each of their statements, is the *increased lifting or sustaining power of the magnet.* No allusion, whatever, is made to any *law of propulsion*, or *adaptation to telegraphic purposes*, as the result of Henry's experiments.

In the same volume of Silliman's Journal appears also the following editorial remarks on a large magnet, made by Prof. Henry for the laboratory of Yale College, in accordance with the very experiments detailed in the preceding volume. Prof. *Silliman* says of Prof. Henry: "He has the honor of having constructed *by far the most powerful magnets* that have ever been known, and his last weighing, armature and all, but 82½ lbs., *sustains over a ton.* It is *eight times more powerful* than any magnet hitherto known in Europe, and between *six and seven times more powerful* than the great magnet in Philadelphia."

Of this tenor are all the allusions or notices in any scientific or other publication that I have met with, of all Prof. Henry's "published researches" up to 1831. Others may have been more fortunate than myself, and may have found notices of a different character; if so, they can be quoted. But that the general impression in the scientific world in regard to Henry's experiments of 1831, was that the great merit of his "published researches" consisted in his "*having made large electro-magetic magnets*," is certain; nor have I found the slightest allusion in any scientific work since 1831, to any discovery of his as having any bearing upon Telegraphs of any kind.

But in addition to all this evidence, that *an increase of the lifting power of the electro-magnet* was the great controlling object of Prof. Henry in his experiments, I cannot omit the testimony of one who has given it contemporaneously with the experiments themselves. This witness is no other than Prof. Henry himself, who, in the very paper in which he details them, gives their result.

After having arrived, in his Exp. 15, to the maximum of power in the magnet with which he was experimenting, to wit, 750 pounds, he says, "The strongest magnet of which we have

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

This, then, being the general impression made by his "published researches" among the scientific at home and abroad, uniformly for some fourteen years, with no attempt, so far as I can learn, on Prof. Henry's part to correct such impression, but on the contrary, corroborated by himself; was it such a crime in Mr. Vail to quote, in 1845, the language of the Encyclopædia Britannica as the current opinion of the day? What more, under all the circumstances, could Mr. Vail have said? What is *his crime*, that he should have said what he did say? But, especially, what is *my crime*, that Mr. Vail said what he did say?

I have now completed the unpleasant duty of defending myself from the ungenerous and untrue imputations educed from Prof. Henry's deposition. This duty has been performed with no unfriendly feeling, and, I trust, I have used no asperity of language, towards Prof. Henry, notwithstanding the just cause he has given me to feel aggrieved.

If I have shown that some discoveries, claimed for him by those selfishly interested in robbing me of my legal rights, do not belong to him; it is because they have been assumed to be his, and have been unnaturally exaggerated to militate against my claims, thus forcing me, critically, to scrutinize their pretensions to originality with him. In this scrutiny, multitudes are far more interested than myself.

For any success which may have attended Prof. Henry's labors to enlarge the boundaries of science, whether in the college laboratory, or in the honored position he holds at the head of the Smithsonian Institute, he deserves and will receive the reward of a just fame. As I gave my feeble influence heartily to his elevation to this post, because, at the time, I believed it eminently alike a place of honor to him, and of advantage to the cause of science, so now I deprecate the painful necessity of being the medium of exposing the weaknesses and errors of such a mind.

* Silliman's Journal of Science, Vol. XIX., p. 405.

Had these errors affected me only, I would gladly have buried them in oblivion, and suffered in silence; but justice to others, quite as much as to myself, and justice to historic truth, has demanded this exposure.

I have shown, as I promised in the outset to show, that any complaints against me, whether from opposing counsel, or from Prof. Henry himself, to the effect that I had not the disposition to do him justice in regard to discoveries, alleged to be his, and to have an essential bearing on the Telegraph, are without any foundation.

I have shown that his "published researches" disclose no new discoveries of any avail in the Telegraph.

I have shown that I did not receive from him, directly or indirectly, any facts whatever previous to the actual demonstration, experimentally, of my telegraphic instruments, nor any facts afterwards, of any service to me in perfecting the invention.

I have shown that in my sincere desire to give him due credit for scientific discoveries, which, from the high consideration in which I held him, I was but too ready to believe were in reality his, on superficial evidence, I had unwittingly given him credit for more than is due to him.

I have shown that the reiterated assertion which has been made by opposing counsel, that "to Prof. Henry is due the invention of the receiving magnet and combined circuits," actually receives no support from Prof. Henry's deposition, and has no foundation in fact.

With this very general summary, I shall, for the present, close my remarks, reserving any minor points for future correction, should I be called upon to defend what I have already advanced.

SAMUEL F. B. MORSE.

LOCUST GROVE, PO'KEEPSIE,

NEW-YORK, *December*, 1853.

APPENDIX.

DEPOSITION OF JOSEPH HENRY.

[*From the Record of the Supreme Court of the United States. With a few Notes, by way of Commentary, by S. F. B. Morse.*]

1. PLEASE state your place of residence and your occupation; also, what attention, if any, you have given to the subjects of electricity, magnetism, and electro-magnetism?

Answer.—I begin this deposition with the express statement that I do not voluntarily give my testimony; but that I appear on legal summons, and in submission to law. I am Secretary to the Smithsonian Institution, established in the City of Washington, where I now reside. The principal direction of the Institution is confided to me. As I do not expect to return to Washington until some time in October, I have been called upon to give my testimony here in Boston. On this account I labor under the disadvantage of being obliged to testify without my notes and papers, which are now in Washington.

I commenced the study of electro-magnetism in 1827;* and since then have, at different times, within the last two and a half years, when I became Secretary of the Smithsonian Institution, made original investigations in this and kindred branches of physical science. I know no person in our country who has paid more attention to the study of the principles of electro-magnetism than myself.

2. Please give a general account of the progress of the science of electro-magnetism, as connected with telegraphic communi-

* It will be noticed that at this date, (the commencement of his studies,) I had already a knowledge of *electro-magnetism*, imparted in attendance on the lectures of Prof. James Freeman Dana, in the beginning of the year 1827.

cation; and of any inventions or discoveries in electro-magnetism applicable to the telegraph made by yourself?

Answer.— I consider an Electro-magnetic Telegraph as one which operates by the combined influence of electricity and magnetism. Prior to the winter of 1819–20, no form of the Electro-magnetic Telegraph was possible. The scientific principles on which it is founded were then unknown. The first fact of electro-magnetism was discovered by Oersted, of Copenhagen, during that winter. It is this: A wire being placed close above, or below, and parallel to a magnetic needle, and a galvanic current being transmitted through the wire, the needle will place itself at right angles to it. This fact was widely published, and the account was everywhere received with interest.

The second fact of importance was discovered independently, and about the same time, by Arago, at Paris, and Davy, at London. It is this: During the transmission of a galvanic current through a wire of copper, or any other metal, the wire exhibits magnetic properties, attracting iron but not copper filings, and having the power of inducing permanent magnetism in steel needles. The next important fact was discovered by Ampère, of Paris, one of the most sagacious and successful cultivators of physical science in the present century. It is this: Two parallel wires through which galvanic currents are passing in the same direction, attract each other; but if the currents pass in opposite directions, they repel each other. On this fact Ampère founded his ingenious theory of magnetism and electro-magnetism. According to this theory, all magnetic phenomena result from the attraction or repulsion of electric currents, supposed to exist in the iron at right angles to the length of the bar; and that all the phenomena of magnetism and electro-magnetism are thus referred to one principle, namely—the action of electrical currents on one another.

Ampère deduced from this theory many interesting results, which were afterwards verified by experiment. He also proposed to the French Academy a plan for the application of electro-magnetism to the transmission of intelligence to a distance. This consisted in deflecting a number of needles at the place of receiving intelligence, by galvanic currents transmitted through long wires. This transmission was to be effected by completing a galvanic circuit. When completed, the needle was deflected. When interrupted, it returned to its ordinary position, under the influence of the attraction of the earth. This project of Ampère was never reduced to practice. All these discoveries and results were prior to 1823.

The next investigations relating to the Magnetic Telegraph were published in 1825. They were by Mr. Barlow, of the Royal Military Academy of Woolwich, England. He found

that there was great diminution in the power of the galvanic current to produce effects with an increase of distance; a diminution so great in a distance of two hundred feet was observed, as to convince him of the impracticability of the scheme of the Electro-magnetic Telegraph. His experiments led him to conclude that the power was inversely as the square root of the length of the wire. The publication of these results put at rest, for a time, all attempts to construct an Electro-magnetic Telegraph.

The next investigation in the order of time, bearing on the Telegraph, were made by Mr. Sturgeon, of England. He bent a piece of iron wire into the form of a horse-shoe, and put loosely around it a coil of copper wire, with wide intervals between the turns or spires, to prevent them touching each other; and through this coil he transmitted a current of galvanism. The iron, under the influence of this current, became magnetic; and thus was produced the first electro-magnetic magnet, sometimes called simply the electro-magnet. An account of this experiment was first published in November, 1825, in the transactions of the Society for the Encouragement of the Arts in England; and was made known in this country through the annals of philosophy, November, 1826.

Nothing further was done pertaining to the telegraph until my own researches in Electro-magnetism, which were commenced in 1828, and continued in 1829, 1830, and subsequently; Barlow's results, as I before observed, had prevented all attempts to construct a Magnetic Telegraph on the plan of Ampère, and our own knowledge of the development of magnetism in soft-iron, as left by Sturgeon, was not such as to be applicable to telegraphic purposes. The electro-magnet of Sturgeon could not be made to act by a current through a long wire, as will be apparent, hereafter, in this deposition.

After repeating the experiments of Oersted, Ampère, and others, and publishing an account, in 1828, of various modifications of electro-magnetic apparatus, I commenced, in that year, the investigation of the laws of the development of magnetism in soft-iron, by means of the electrical current. The first idea that occurred to me, in accordance with the theory of Ampère, with reference to increasing the power of the electro-magnet, was that of using a longer wire than had before been employed. A wire of sixty feet in length, covered with silk, was wound round a whole length of an iron bar, either straight, or in the form of a U, so as to cover its whole length with several thicknesses of the wire.

The results of this arrangement were such as I had anticipated, and electro-magnets of this kind, exhibited to the Albany Institute in March, 1829, possessed magnetic power superior to that of any before known.

The idea afterwards occurred to me, that the quantity of galvanism supplied by a small galvanic battery, might be applied to develop a still greater amount of magnetic power in a large bar of iron. On experiment, I found this idea correct. A battery of two and a half square inches of zinc, developed magnetism, in a large bar, sufficient to lift fourteen pounds.

The next suggestion which occurred to me was that of using a number of wires, of the same length, around the same bar, so as to lessen the resistance which the galvanic current experienced in passing from the zinc to the copper, through the coil. To bring this to the test of experiment, a second wire, equal in length to the first, was wound around the last mentioned magnet, and its ends soldered to the plates of the same battery.

The magnet, with this additional wire, lifted twenty-eight pounds; or, in other words, its power was doubled.

A series of experiments was afterwards made, to determine the resistance to conduction of wires of different lengths and diameters, and the proper lengths and number of wires for producing, with different kinds of galvanic batteries, the maximum of amount of magnetic development, with a given quantity of zinc surface. For this purpose, a bar of sof-tiron, two inches square and twenty inches long, weighing twenty-one pounds, and much larger than any before used, was bent in the form of a horse-shoe. Around this were wound nine strands of copper wire, each sixty feet long, the ends left projecting so that one or more coils could be used at once, either connected with a battery or with each other, thus forming several coils with several battery connections, or one long coil with single battery connections. The greatest effect obtained with this magnet, using a battery of a single pair, with a zinc plate of two-fifths of a square foot of surface, and all the wire arranged as separate coils, was to lift a weight of six hundred and fifty pounds; with a large battery the effect was increased to seven hundred and fifty pounds. In a subsequent series of experiments, not published with the preceding, the same magnet was made to sustain one thousand pounds. When a compound battery was employed, of a number of pairs, it was found that the greatest effect was produced when all the wires were arranged as a single long coil. I subsequently constructed electro-magnets, on the same plan, which supported much greater weights. One of these, now in the cabinet of Princeton, will sustain three thousand six hundred pounds, with a battery occupying about a cubic foot of space. It consists of thirty strands of wire, each about forty feet in length.*

* The natural inference from this narration, from bottom of page 98 to page 101 is a fallacious one. It seems impossible not to infer from it that Prof. Henry, in his experiments from 1828 onward, had the distinct and prominent purpose before his mind, of removing the obstacles to the practicability of an Electro-magnetic Tel-

The above-mentioned experiments exhibit the important fact, that when a galvanic battery of intensity, (that is to say, a battery consisting of a number of pairs,) is employed, the electro-magnet connected with it must be wound with one long wire, in order to produce the greatest effect; and that when a battery of quantity, (that is, one of a single pair,) is employed, the proper form of the magnet connected with it is that in which several shorter wires are wound around the iron. The first of these magnets, which is the one now employed in the long or main circuit of the telegraph, may be called an intensity magnet; and the *second, which is used in the local circuit*, may be denominated the *quantity*.*

The quantity of electricity which can be passed through a long circuit of ordinary-sized wire is, under the most favorable circumstances, exceedingly small; and in order that this may develop magnetism in a bar of iron, it was necessary that it should be made to revolve many times around the iron, that its effects may be multiplied; and this is effected by using a long, single coil. Hence it will be seen, that the electro-magnet of Mr. Sturgeon was not applicable to telegraphic purposes in a long circuit.

Previous to making the last experiments above mentioned, in order to guide myself, I instituted a series of preliminary experiments on the conduction of wires, of different lengths and diameters, with different batteries. In these experiments a galvanometer, or an instrument consisting of a magnetic needle, freely suspended within a coil of wire, was first employed to denote, by the deflection of its needle, the power of the current. The result from a number of experiments, with a battery of a single pair, was the same as that obtained by Barlow—namely, that the power diminished rapidly with the increase of distance.

egraph, by aiming, *first*, to disprove the inference of Barlow, adverse to the project, from having observed the rapid diminution of power in an electric current as distance increased; and *second*, to improve the electro-magnet as left by Sturgeon, for the special purpose of making it applicable to *telegraphic* purposes. All that he had ever published, previous to this deposition, (in which, for the first time, he intimates such an intent,) may be safely challenged for a particle of proof to sustain such an unwarrantable assumption. There is but a *single* allusion, in anything he had ever published, to an Electric Telegraph of any kind; and this is the casual episodical remark in relation to Barlow, in his paper of 1831. Is this ingenuous? What he had *published*, previous to 1835, at least—not what he had *concealed* till 1849—is the only legitimate history for a Court.

* Here is a manifest perversion in the description of my arrangements of magnets, leading to the false impression, that I had adopted the two kinds of magnets which he claims to have perfected; and, to strengthen the impression, names respectively one, the *intensity* magnet, for the long line; and the other, *quantity* magnet, for the short line. The magnet of both lines of my combined circuits, is the common electro-magnet, wound, as suggested by Dana, in 1827. The *quantity* magnet of Henry, the only one possessing novelty, is not necessary in the telegraph, and has never been used in its arrangements, so far as I have learned.

With the same battery, and a larger wire, the diminution was less. The galvanometer was next removed, and a small electro-magnet substituted in its place. With a single battery, the same result was again obtained—a great diminution of lifting power with the increase of distance. After this, the battery of a single pair was removed, and its place supplied by one of intensity, consisting of twenty-five pairs. With this the important fact was observed, that no perceptible diminution of the lifting power took place, when the current was transmitted through an intervening wire, between the battery and the magnet, of upwards of one thousand feet.

This was the first discovery of the fact, that a galvanic current could be transmitted to a great distance with so little a diminution of force as to produce mechanical effects, and of the means by which the transmission could be accomplished.* I saw that the Electric Telegraph was now practicable; and in publishing my experiments and their results, I stated that the fact just mentioned was applicable to Barlow's project of such a telegraph. I had not the paper of Barlow before me, and erred in attributing to him a project of a telegraph, as he only disproved, as he thought, the practicability of one. But the intention† of this statement was to show that I had established the fact, that a mechanical effect‡ could be produced by the galvanic current at a great distance, operating upon a magnet or needle, and that the telegraph was therefore possible. In arriving at these results, and announcing their applicability to the telegraph, I had not in mind any particular form of telegraph, but referred only to the general fact, that it was now demonstrated that a galvanic current could be transmitted to great distances, with sufficient power to produce mechanical effects adequate to the desired object.

The investigations above mentioned were all devised and originated, and the experiments planned, by myself. In con-

* A mistake. [See page 33, Defence.] Soëmmerring had projected a galvanic current to a distance of 2,248 feet, with a force *sufficient* to produce mechanical effects, and used the same electro-motive power as Henry, and this, in 1812, eighteen years before Henry published his experiments. Ampère had, also, actually produced mechanical effects at a distance, in 1820, eleven years before.

† Whatever the *intention* of his statement in his published paper, as here attempted to be explained, the statement itself confines his application to a *needle telegraph*, and *that only*; nor could it have, in his own mind, at the time, any other application, so far as telegraphs were concerned, since the *electro-magnet* had then never been proposed for telegraphic purposes, and Henry does not here propose it, nor even suggest it.

‡ For the perfect securing of "mechanical effects," at a distance, by electro-motive force, all that was necessary to know, after Schweigger had invented his multiplier, but especially after Soëmmerring's and Ampère's experiments, was the law establishing the relation between the *resistance* to be overcome, and the *power* to overcome that resistance; and this law was educed by Professor Ohm, of Nuremberg, in 1827, and is known at this day as "*Ohm's law, or formula.*"

ducting the latter, however, I was assisted by Dr. Philip Ten Eyck, of Albany. An account of the whole* was published in the 19th volume of "Silliman's Journal," in 1831, with the exception of the account of the large magnet afterwards constructed, at Princeton, in 1833, and the experiment mentioned of lifting a thousand pounds with one of my first magnets. While I was engaged in these researches, Professor Mohl, of the University of Utrecht, was pursuing investigations somewhat similar, and succeeded in making powerful electro-magnets, but made no discovery as to the distinction between the two kinds of magnets, or the transmissibility of the galvanic current to a great distance, with power to produce mechanical effects.† In fact, his experiments were but a repetition on a large scale of those of Sturgeon.

After completing the investigations above mentioned, I commenced a series of experiments on another branch of electricity closely connected with this subject. Among other things, I applied the principles above mentioned to the construction of an electro-magnetic machine, which has since excited much attention in reference to the application of electro-magnetism as a motive power in the arts.‡

In 1832 I was called to the chair of Natural Philosophy in the College of New-Jersey, at Princeton; and, in my first course of lectures in that institutio n, in 1833, and in every sub sequent year during my connection with that institution, I mentioned the project of the Electro-magnetic Telegraph, and explained how the electro-magnet might be used to produce mechanical effects at a distance, adequate to making signals of various kinds. I never myself attempted to reduce these principles to practice, or to apply any of my discoveries to processes in the arts. My whole attention, exclusive of my duties to the College, was devoted to original scientific investigations, and I left to others what I considered in a scientific view of subordinate importance, the application of my discoveries to useful purposes in the arts. Besides this, I partook of the feeling

* How far this statement is correct, may readily be ascertained by comparing i with his account of his experiments in his paper, in "Silliman's Journal," which paper is herewith appended.

† Compare this narration with his statement in the last paragraph of his paper in "Silliman's Journal." Here the *magnets*, as applicable to a *telegraph*, are put forward as the principal object of his researches. There the principal object is a magnet of great power, with a small expenditure of galvanism. Not a word respecting the bearing of the magnets upon a *telegraph*.

‡ How is this *electro-magnetic machine* applicable to any telegraph known to Professor Henry, at the time he published his paper describing it? In this deposition there is an attempt to connect them, as if he had my telegraph in his mind at the time. This is all wrong. For Professor Henry's opinion of this, as well as other machines of the same character, see his letter to me of February, 1842, on p. 16, Defence; and for his opinion of its intrinsic value, see the paper itself.

common to men of science, which disinclines them to secure to themselves the advantages of their discoveries by a patent.

In February, 1837, I went to Europe; and early in April of that year, Prof. Wheatstone, of London, in the course of a visit to him in King's College, London, with Prof. Bache, now of the Coast Survey, explained to us his plans of an Electro-magnetic Telegraph; and among other things, exhibited to us his method of bringing into action a second galvanic circuit. This consisted in closing the second circuit by the deflection of a needle, so placed that the two ends projecting upwards, of the open circuit, would be united by the contact of the end of the needle when deflected; and on opening or breaking of the circuit so closed by opening the first circuit, and thus interrupting the current, when the needle would resume its ordinary position under the influence of the magnetism of the earth. I informed him that I had devised another method of producing effects somewhat similar. This consisted in opening the circuit of my large quantity magnet, at Princeton, when loaded with many hundred pounds weight, by attracting upward a small piece of moveable wire with a small intensity magnet, connected with a long wire circuit. When the circuit of the large battery was thus broken by an action from a distance, the weights would fall, and great mechanical effect could thus be produced, such as the ringing of church bells at a distance of a hundred miles or more, an illustration which I had previously given to my class at Princeton. My impression is strong, that I had explained the precise process to my class before I went to Europe; but testifying now without the opportunity of reference to my notes, I cannot speak positively.* I am, however, certain of having mentioned in my lectures every year previously, at Princeton, the project of ringing bells at a distance, by the use of the electro-magnet, and of having frequently illustrated the principle to my class, by causing in some cases a thousand pounds to fall on the floor, by merely lifting a piece of wire from two cups of mercury closing the circuit.

The object of Prof. Wheatstone, as I understood it, in bringing into action a second circuit, was to provide a remedy for the diminution of force in a long circuit. My object in the process described by me, was to bring into operation a large quantity magnet, connected with a quantity battery in a local circuit, by means of a small intensity magnet, and an intensity battery at a distance†.

The only other scientific facts of importance to the practical operation of the telegraph not already mentioned, are the dis-

* See page [55] Defence.
† See remarks on ross examination, p. [57] Defence.

covery by Steinheil, in 1837, in Germany, of the practicability of completing a galvanic circuit, by using the earth for completing the circuit, and the construction of the constant battery in 1836, or about that time, by Prof. Daniel, of King's College, London. I believe that I was the first to repeat the experiments of Steinheil and Daniel in this country. I stretched a wire from my study to my laboratory, through a distance in the air of several hundred yards, and used the earth as a return conductor, with a very minute battery, the negative element of which was a common pin, such as is used in dress, and the positive element the point of a zinc wire immersed in a single drop of acid. With this arrangement, a needle was deflected in my laboratory before my class. I afterwards transmitted currents in various directions, through the college grounds at Princeton. The exact date of these experiments I am unable to give without reference to my notes. They were previous, however, to the unsuccessful attempt of Mr. Morse to transmit currents of electricity through wires buried in the earth, between Washington and Baltimore, and before he attempted to use the earth as a part of the circuit. Previous to this time, and after the abovementioned experiments, Mr. Morse visited me at Princeton, to consult me on the arrangement of his conductors. During this visit, we conversed freely on the subject of insulation and conduction of wires. I urged him to put his wires on poles, and stated to him my experiments and their results.

In the course of the years 1836 and 1837, various plans of more or less merit were devised, and more or less carried into effect, for applying the principles already discovered to the construction of Electro-magnetic Telegraphs, in different parts of the world; but of these I do not undertake to give any particular account. I would say, however, that of these plans, that for which Mr. Morse subsequently obtained a patent was, in my judgment, the best.

3. Please state whether or not you are acquainted with the Electro-magnetic Telegraph, for which S. F. B. Morse obtained a patent in 1846? If you are, please state whether any; and if any, which of the principles or plans which you have described as discovered or announced by yourself or others, are used in the construction or operation of it? Statealso what principles used in the telegraph are, so far as you know, original with Prof. Morse?

Answer.—I am acquainted with the principles and general mode of operation of the telegraph and improvement referred to. The telegraph is based upon the facts discovered by myself,* and others of which I have already given an account.

* It will be perceived that the answer, for the most part, to this inter-

The plan which was first described to me, in the autumn of 1837, by Mr. Morse, or by Prof. Gale,* (who was associated with him in the construction of the telegraph,) was, to employ a single entire circuit of wire, with an intensity battery to excite the current, and an intensity magnet to receive it and produce a mechanical action which would work the recording apparatus. Mr. Morse afterwards employed the intensity battery in a long circuit, and an intensity magnet to receive its current at a distant point, and produce the mechanical effect of closing a secondary circuit. The secondary circuit may be either employed to transmit a second current to a distant point and there close a third circuit, and thus continue the line, or for working a recording apparatus in the secondary circuit; or it may be employed without reference to the continuation of the line, as a short local circuit to work a local magnet. In the first case there must be in the secondary circuit an intensity battery and intensity magnet; in the last case, a quantity magnet and quantity battery are required.†

rogatory, is evasive. He is distinctly asked to point out which of the *principles* or *plans* which he had discovered, are used in the construction of my telegraph. Yet he specifies nothing distinctly as his. He says, in general terms, that it is based on facts, discovered by him. What fact? Is it that of a mode of obtaining the greatest amount of magnetic power, with the smallest amount of galvanism? this is the great fact he announces as his discovery, the only new fact not known before; and with this, as I have said, my telegraph has nothing to do. Does he mean, when describing my plan of employing the usual Cruikshank's battery with an electro-magnet in the circuit, to intimate that this magnet or battery was derived from him? He is in error; Prof. Dana used them in 1827, in connection, and exhibited them to his audience, (to me among the number,) before Prof. Henry had even commenced his studies in electro-magnetism. If he means to intimate that the combination of circuits was his plan, he is in error, as I have shown at length in the Defence at p. [50] and onward. If he means in describing the local circuit arrangement, that the magnet I use is his *quantity* magnet, he is again in error. If he means any other kind of magnet, then it is not his.

* He says, "by Mr. Morse, or by Prof. Gale." If either of us described the telegraph to him at that period, it must have been Prof. Gale; since I have shown that my first acquaintance with him was not till May, 1839.

† Much stress, it will be seen, is laid by Prof. Henry on the terms, '*intensity*' and '*quantity*;' and he has applied these terms to two kinds of magnets: *intensity*, to the usual and common magnet, with a helix of a single long wire; and *quantity*, to a magnet with a helix composed of numerous short wires—this latter being the magnet which he invented to obtain "great magnetic power, at a small expenditure of galvanism." These terms he has borrowed from the two kinds of Voltaic battery—the battery of *intensity*, or one with many elements, plates, or pairs; and the battery of *quantity*, or one with a single element, plate, or pair. It is somewhat difficult to understand what he claims as new in these particulars, as applicable to a telegraph. Both kinds of battery have been known from the very birth of galvanism. The *intensity* forms of the battery were the kinds earliest in general use. Volta's original pile, the first galvanic battery ever made, is an intensity battery. Volta's "chapelet de tasses,"(*a*) or, as it is usually called, "couronne des tasses," is another form of intensity battery; and Cruikshank's improvement on Volta's, made as early as 1804, is also an intensity battery; and the improved batteries of the present day, and those most in use for general pur-

(*a*) Wilkinson's Elements of Galvanism. Vol. II., p. 139.

I heard nothing of the secondary circuit as a part of Mr. Morse's plan, until after his return from Europe, whither he went in 1838.* It was not till long after this that Mr. Morse used the earth as a part of the circuit, in accordance with the discovery of Steinheil.

I am not aware that Mr. Morse ever made a single original discovery, in electricity, magnetism, or electro-magnetism, applicable to the invention of the telegraph. I have always considered his merit to consist in combining and applying the discoveries of others in the invention of a particular instrument and process for telegraphic purposes. I have no means of determining how far this invention is original with himself, or how much is due to those associated with him.

4. Please state when you first became acquainted with Prof. Morse, and what knowledge he possessed of electricity, magnetism, and electro-magnetism; and what information you or others communicated to him, relating to the telegraph? State, also, all you know of the attempts of himself and others associated with him, to construct an Electro-magnetic Telegraph, either from your own observation or from statements made by himself or byothers, in your presence? State, particularly, any conversation, if any, you may have had with him, in reference to your own discoveries, applied to the telegraph?

Answer.—Shortly after my return from Europe, in the autumn of 1837, I learned that Mr. Morse was about to petition Congress for assistance in constructing the Electro-magnetic Tele-

poses in the arts and for scientific investigation, are, for the most part, intensity batteries. The distinctive qualities, too, of *intensity* and *quantity*, in relation to batteries, were among the very earliest discovered and commented on by savans. An *intensity* battery was recognised as having greater *power*, according to the number(*a*) of its plates, and, of course, ability to propel a galvanic current with greater velocity; while a *quantity* battery was perceived to have but a *feeble* power for this purpose. These, therefore, are no recent discoveries. I have used both kinds of battery in the telegraph, from the beginning. The *intensity* battery, on Cruikshank's plan, was first used by me, on a circuit of great length; a battery which may now be used for the telegraph, but with less convenience than Grove's & Daniel's improvements upon it; these latter having the great advantage of *sustaining power*; yet, so far as practicability is concerned, of no superior efficacy in accomplishing the result. The quantity battery is used in my local circuit, because it is sufficient for my purpose; but not the *quantity magnet*. invented and so called by Prof. Henry. I employ, and have always employed, the usual electro-magnet, in one of its earliest forms. Sturgeon's magnet, the kind of magnet first exhibited to me by Prof. Dana, has been tried, and anwers perfectly well, in the local circuit.

* I returned in 1839. So far as this is understood as intimating that I had no such plan as the secondary circuit before I went to Europe, because he was not made acquainted with it, it is sufficient to say the evidence was before the court, that I described the two circuits in combination in 1836, constructed and used them in March, 1837, and specified them at the Patent Office in 1838, *before* I went to Europe.

(*a*) Wilkinson. Vol. II., pp. 360, 149, 153, 157.

graph. Some of my friends in Princeton, knowing what I had done in developing the principles of the telegraph, urged me to make the representations to Congress, which I expressed some thought of doing, namely—that the principles of the Electro-magnetic Telegraph belonged to the science of the world; and that any appropriation which might be made by Congress, should be a premium for the best plan, and the means of testing the same, which the ingenuity of the country might offer. Shortly after this, I visited New-York, and there accidentally made the personal acquaintance of Mr. Morse. He appeared to be an unassuming and prepossessing gentleman, with very little knowledge of the general principles of electricity, magnetism, or electro-magnetism.* He made no claims, in conversation with me, to any scientific discovery, or to anything beyond his particular machine, and process of applying known principles to telegraphic purpos He explained to me his plan of a telegraph with which he had recently made a successful experiment. I thought this plan better than any with which I had been made acquainted in Europe. I became interested in him; and, instead of interfering in his application to Congress, I gave him a certificate in the form of a letter,‡ stating my confidence in the practicability of the Electro-magnetic Telegraph, and my belief that the form proposed by himself was the best which had been published.

Mr. Morse subsequently visited Princeton several times,§ to confer with me on the principles of electricity and magnetism, which might be applicable to the telegraph. I freely gave him any information I possessed.

I learned in 1837, or thereabouts, that Prof. Gale and Dr. Fisher were the scientific assistants of Mr. Morse in preparing the telegraph. Mr. Vail was also employed, but I know not in

* It will be thought that Prof. Henry must have very industriously improved the few moments of this confessedly casual interview, to examine me in the extent of my acquirements in electricity, magnetism, and electro-magnetism. One would suppose he was rather giving to the Faculty of College the result of his examination of a candidate for entering college, than speaking to a court of one, his equal in *academic position*, (I say not—equal in scientific acquirements) This tone savors not a little of pique, and would not have been used by him previous to the publication of Vail's book. But, let it pass. His narration is unfortunately erroneous in dates, as usual. His memory is at fault in making the *first* interview with me in 1837, in New-York, when it has been shown that it was in 1839, two years later, and in Princeton.

† Scientific reputation follows, presumedly, on "scientific discovery." If the former is awarded by a competent judge, to any one, it may be presumed that he deserves it, on account of *scientific discovery*. Prof. Henry, in his letter of February, 1842, awards to me "scientific reputation," as a "*just claim*;" but this was before the fatal year 1845, before Vail's book appeared. *Tempora mutantur*, &c.

‡ The letter of February, 1842, which speaks for itself, written *five years* after this supposititious interview.

§ "Several times!" *twice*, if that is several times; first, in May, 1839, and second, and last, in Feb., 1844.

what capacity, and I am not personally acquainted with him. With Prof. Gale I have been intimately acquainted for several years; he had been a pupil in chemistry of my friend Dr. Torrey, and had studied my papers on electro-magnetism, and, as he informed me, had applied them in the arrangement of the apparatus for the construction of Morse's Telegraph.*

My researches had been given to the world several years before the attempt was made to reduce the Magnetic Telegraph to practice. Mr. Chilton, of New-York, informed me that he had referred Mr. Morse to them previous to his experiments in the New-York University.† I was, therefore, much surprised on the publication, in 1845, of a work purporting to give a history of the telegraph, and of the principles on which it was founded, by Mr. Vail, then principal assistant of Mr. Morse, and one of the proprietors of his patent, to find all my published researches relating to the telegraph passed over with little more than the remark, that Dr. Mohl and myself had made large electro-magnetic magnets. Presuming that this publication was authorized by Mr. Morse, and the proprietors of the telegraph, I complained to some of his friends of the injustice, and after his return from Europe, (for he was absent at the time the book was issued,) I received a letter, copied and signed by Mr. Vail, but written by Mr. Morse, as the latter afterwards informed me, excusing the publication on the ground that he, (Mr. Vail,) was ignorant of what I had done, and asking me for an account of my researches. This letter was addressed to me after the book had been translated into French, and, I believe, published in Paris. To the letter I did not think fit to make any reply. I afterwards received a letter from Mr. Morse, in his own name, on the same subject, to which I gave a verbal reply in January, 1847, in Washington. In this interview, Mr. Morse acknowledged that injustice had been done me, but said that proper reparation would be made. Another issue of the same work was made, bearing date 1847, in which there is no change in the statement relative to my researches.

About the beginning of 1848, Mr. Walker, of the Coast Survey, in a report on the application of the telegraph to the determination of differences of longitude, alluded to my researches. A copy of this was sent to Mr. Morse, which led to an interview between Mr. Walker, Prof. Gale, Mr. Morse, and

* It is intended here to insinuate that if I did not get information *directly* from him or his writings, I got it *indirectly* through Dr. Gale. The fallaciousness of this is shown in the *second* note on the next page.

† As my first experiments in the University in 1835, and onward until 1837, were known to comparatively a small number of persons, my pupils and others, Dr. Chilton could speak only of those that were more publicly shown in 1837, but at which time the telegraph was essentially completed It could have deriv d no aid at this time from anything published by Prof. Henry.

myself. At this meeting, which took place at my office in Washington, Mr. Morse stated that he had not known until reading my paper in January, 1847,* that I had two years before his first conception in 1832, settled the point of practicability of the telegraph, and shown how mechanical effects could be produced at a distance, both in the deflection of a needle and in the action of an electro-magnet; that he did not know, at the time of his experiments in 1837, that there had been any doubts of the action of a current at a distance, and that in the confidence of the persuasion that the effect could be produced, he had devised the proper apparatus by which his telegraph was put in operation. Prof. Gale being then referred to, stated that Mr. Morse had forgotten the precise state of the case; that he, (Mr. Morse,) previous to his, (Dr. Gale's,) connection with him, had not succeeded in producing effects at a distance; that when he was first called in he found Mr. Morse attempting to make an electro-magnet act through a circuit of a few yards of copper wire, suspended around a room in the University of New-York, and that he could not succeed in producing the desired effect, even in *this*, that circuit; that he, (Dr. Gale,) asked him if he had studied Prof. Henry's paper on the subject, and that the answer was "No;" that he then informed Mr. Morse that he would find the principles necessary to success explained in that paper; that instead of the battery of a single element, he should employ one of a number of pairs; and that, in place of the magnet with a short single wire, he should use one with a long coil. Dr. Gale further stated that his apparatus was in the same building, and that having articles of the kind he had mentioned, he procured them, and that with these the action was produced through a circuit of half a mile of wire. To this statement Mr. Morse made no reply. The interview then terminated, and I have since had no further communication with him on the subject.†

* Here dates are misapprehended. It was in 1847, that in passing through Philadelphia, I had access, in the library of the American Philosophical Society, to the Edinburgh Philos. Journal for Jan., 1825, Vol. XII., p. 195, which gives all that I can find respecting "Barlow's project," as Henry styles it. I had endeavored to find this work in the New-York libraries, but in vain; and Henry has confounded the two dates, to wit, the date of 1837, the time of my first knowledge of his paper in Silliman's Journal, and the date of 1847, the time of my first knowledge of the "project of Barlow."

† The whole narration of the incidents of this interview on this page, from line eleven to the bottom, is, from beginning to end, fallacious. The conversation, questions and answers are misrecollected, misrepresented, and untrue. As legal testimony, it was certainly open to exception, and should have been excepted to by legal counsel, but, fortunately, it had no weight with the courts.

Dr. Gale is made by Henry to say, that previous to his (Gale's) connection with me, I had not succeeded in producing effects at a distance, &c., and that I "*had not succeeded, even in a few yards of wire, to produce the desired effect.*" This

5. Please state whether or not you ever constructed any machine, for producing motion, by magnetic attraction and repulsion; if yea, what was it, and what led to the making of it?

Answer. After developing the great magnetic power of the electro-magnet, as already described, the thought occurred to me, that this power might be applied to give motion to a machine. The simplest arrangement which suggested itself to my mind was one already referred to, namely; causing a moveable bar, supported on a horizontal axis, like a scale beam, to be attracted and repelled by two permanent magnets. This could be readily effected by transmitting, through a coil of wire around the suspended bar, a current of galvanism, first in one direction, and then in the opposite direction, the alternations of the current being produced by dipping the ends of wires, projecting from the coil, into cups of mercury, connected with batteries, one on either side. An account of this was published in Silliman's Journal, for 1831, Vol. XX., p. 340. It was the first successful attempt to produce a mechanical motion which might apparently be employed in the arts, as a motive power. This little machine attracted much attention at home and abroad, and various modifications of it were made by myself, and others. I never, however, regarded it as practically applicable in the arts, because of the great expense of producing power by this means, except, perhaps, in particular cases, where expense of power is of little consequence.

6. Please look at the drawings of the Columbian Telegraph, now shown you, marked G. W. B., and N. B. C., and certified by G. S. Hilliard, Commissioner. Describe, generally, the apparatus represented, and its mode of operation, and state in what respects, if any, it differs from the telegraphic apparatus patented by Mr. Morse?

is Prof. Henry's statement of what he says Dr. Gale stated at the interview. Was it Prof. Henry's object to show, that through Dr. Gale, who had a knowledge of his "published researches," these researches were made available to me in my invention? What, then, does Dr. Gale testify when on the witness's stand? When questioned, without any reference to this interview, he says—*First.* "That he became, for the first time, acquainted with my telegraph in January, 1836." *Second.* When questioned, "Was not such machinery," (the telegraph,) "completed when you first saw it, and if not, what remained to be done?" He answers—"*It was in perfect order when I first saw it.*" And again, when asked, "Could said Morse successfully operate his telegraph, *prior to your communicating to him scientific intelligence?* He replies, "Said Morse *could have successfully operated his telegraph,* and DID SO OPERATE IT, PRIOR TO MY COMMUNICATING ANY SCIENTIFIC INTELLIGENCE." A memory defective in an unusual degree, is the most charitable construction for Prof. Henry, that can be put on such manifest aberrations from fact, in attempting to recollect what was said by another. I have evidence that Dr. Gale "*has no recollection*" of making any such remarks at this interview as Prof. Henry has put into his mouth. I certainly have no recollection of any such conversation myself, and I emphatically deny what he reports of the part I took in the conversation.

Answer. I have looked at the drawings, and I find, on examination, that it will be impossible for me to give a definite answer to the question, unless I have more time than is now at my disposal, and the means of examining and comparing the operations of the machines.

7. Please state, if you can, how many original experiments you have made, in the course of your investigation, in electricity, magnetism, and electro-magnetism?

Answer. The experiments I have mentioned, in this deposition, form but a small part of my original investigations. Besides many that I made in Albany, which I have not mentioned, since my removal to Princeton, I have made several thousand on electricity, magnetism, and electro-magnetism; particularly the former, which have more or less bearing on practical applications, of this branch of science, brief minutes of which fill several hundred folio pages. Many of these have not been published in detail. They have cost me years of labor and much expense.

The only reward I ever expected was the consciousness of advancing science, the pleasure of discovering new truths, and the scientific reputation to which these labors would entitle me.

JOSEPH HENRY.

Sworn to before me,
September 7th, 1849.
GEO. S. HILLIARD,
Commissioner.

PROF. HENRY'S PAPER, PUBLISHED IN VOL. XIX. OF SILLIMAN'S JOURNAL OF SCIENCE—JANUARY, 1831.

*On the application of the principle of the Galvanic Multiplier to Electro-magnetic Apparatus, and also to the development of great magnetic power in soft-iron, with a small galvanic element.** By Prof. JOSEPH HENRY, of the Albany Academy.

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

In a paper, published in the Transactions of the Albany Institute, June, 1828, I described some modifications of apparatus, intended to supply this deficiency of Mr. Sturgeon, by introducing the spiral coil, on the principle of the galvanic multiplier of Prof. Schweigger; and this, I think, is applicable in every case where strong magnets cannot be used. The coil is formed by covering copper wire, from $\frac{1}{40}$ to $\frac{1}{20}$ of an inch in diameter, with silk; and in every case which will permit, instead of using a single conducting wire, the effect is multiplied by introducing a coil of this wire, closely turned upon itself. This will be readily understood by an example: thus, in the experiment of Ampère, to show the action of terrestrial magnetism on a galvanic current, instead of using a short single wire, suspended on steel points, 60 feet of wire, covered with silk, are coiled so as to form a ring of about 20 inches in diameter, the several strands of which are bound together by wrapping a narrow silk ribbon around them. The copper and zinc of a pair of small galvanic plates are attached to the ends of the coil, and the whole suspended

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

by a silk fibre, with the galvanic element hanging in a tumbler of diluted acid. After a few oscillations, the apparatus never fails to place itself at right-angles to the magnetic meridian. This article is nothing more than a modification of De la Rive's ring, on a larger scale.

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

A round piece of iron, about $\frac{1}{4}$ of an inch in diameter, was bent into the usual form of a horse-shoe, and instead of loosely coiling around it a few feet of wire, as is usually described, it was tightly wound with 35 feet of wire, covered with silk, so as to form about 400 turns; a pair of small galvanic plates, which could be dipped into a tumbler of diluted acid, was soldered to the ends of the wire, and the whole mounted on a stand. With these small plates, the horse-shoe became much more powerfully magnetic than another of the same size, and wound in the usual manner, by the application of a battery composed of 28 plates of copper and zinc, each 8 inches square. Another convenient form of this apparatus was contrived by winding a straight bar of iron, 9 inches long, with 35 feet of wire, and supporting it horizontally on a small cup of copper, containing a cylinder of zinc; when this cup, which served the double purpose of a stand and the galvanic element, was filled with dilute acid, the bar became a portable electro-magnetic magnet. These articles were exhibited to the Institute in March, 1829.

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

manner that the galvanic current might circulate in the same direction in both, or, in other words, that the two wires might act as one; the effect by this addition was doubled, as the horse-shoe, with the same plates before used, now supported 28 lbs.

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

These experiments conclusively proved that a great development of magnetism could be effected by a very small galvanic element, and also that the power of the coil was materially increased by multiplying the number of wires, without increasing the length of each. The multiplication of the wires increases the power in two ways: first, by conducting a greater quantity of galvanism, and, secondly, by giving it a more proper direction; for since the action of a galvanic current is directly at right-angles to the axis of a magnetic needle, by using several shorter wires, we can wind one on each inch of the length of the bar to be magnetized, so that the magnetism of each inch will be developed by a separate wire; in this way, the action of each particular coil becomes very nearly at right-angles to the axis of the bar, and, consequently, the effect is the greatest possible. This principle is of much greater importance when large bars are used. The advantage of a greater conducting power from using several wires, might, in a less degree, be obtained by substituting for them one large wire of equal sectional area; but, in this case, the obliquity of the spiral would be much greater, and, consequently, the magnetic action less; besides this, the effect appears to depend, in some degree, on the number of turns, which is much increased by using a number of small wires.*

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

A series of experiments were instituted, jointly, by Dr. Philip Ten Eyck and myself. For this purpose, 1,060 feet (a little more than $\frac{1}{5}$ of a mile) of copper wire, of the kind called bell wire, 0·45 $_{1}\frac{4}{0}\frac{5}{0}\frac{}{0}$ of an inch in diameter, were stretched, several times, across the large room of the Academy.

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

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

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

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

The length of the wire forming the galvanometer may be neglected, as it was only eight feet long. This result agrees remarkably with the law discovered by Mr. Ritchie, and published in the last number of the "Journal of the Royal Institution of Great Britain."

Ex. 3. The galvanometer was now removed, and the whole length of the wire attached to the ends of the wire of a small, soft-iron horse-shoe, ¼ of an inch in diameter, and wound with about eight feet of copper wire, with a galvanic current from the plates used in *Exs.* 1 and 2; the magnetism was scarcely observable in the horse-shoe.

Ex. 4. The small plates were removed, and a battery, composed of a piece of zinc plate, 4 inches by 7, surrounded with copper, was substituted; when this was attached immediately to the ends of the 8 feet of wire wound round the horse-shoe, the weight lifted was 4½ lbs.; when the current was passed through the whole length of wire, (1,060 feet,) it lifted about half an ounce.

Ex. 5. The current was passed through half the length of wire (530 feet) with the same battery; it then lifted 2 oz.

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

Ex. 7. The whole length of the wire was attached to a small trough, on Mr. Cruikshank's plan, containing 25 double plates, and presenting exactly the same extent of zinc surface to the action of the acid, as the battery used in the last experiment. The weight lifted in this case was 8 oz.; when the intervening wire was removed, and the trough attached directly to the ends of the wire surrounding the horse-shoe, it lifted only 7 oz. From this experiment, it appears that the current from a galvanic trough is capable of producing greater magnetic effect on soft-iron, after traversing more than ⅕ of a mile of intervening wire, than when it passes only through the wire surrounding the magnet. It is possible that the different states of the trough, with respect to dryness, may have exerted some influence on this remarkable result; but that the effect of a current from a trough, if not increased, is but slightly diminished in passing through a long wire, is certain. A number of other experiments would have been made to verify this, had not our

use of the room been limited, by its being required for public exercises.

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

In order to test, on a large scale, the truth of these preliminary results, a bar of soft-iron, 2 inches square and 20 inches long, was bent into the form of a horse-shoe, 9½ inches high, the sharp edges of the bar were first a little rounded by the hammer: it weighed 21 lbs; a piece of iron, from the same bar, weighing 7 lbs., was filed perfectly flat on one surface, for an armature or lifter; the extremities of the legs of the horse-shoe were also truly ground to the surface of the armature: around this horse-shoe 540 feet of copper bell wire were wound in 9 coils, of 60 feet each; these coils were not continued around the whole length of the bar, but each strand of wire, according to the principle before mentioned, occupied about two inches, and was coiled several times, backward and forward, over itself; the several ends of the wires were left projecting, and all numbered, so that the first and the last end of each strand might be readily distinguished. In this manner, we formed an experimental magnet on a large scale, with which several combinations of wire could be made, by merely uniting the different projecting ends. Thus, if the second end of the first wire be soldered to the first end of the second wire, and so on through all the series, the whole will form a continued coil of one long wire. By soldering different ends, the whole may be formed into a double coil of half the length, or into a triple coil of one-third the length, &c. The horse-shoe was suspended in a strong, octangular wooden frame, 3 feet 9 inches high, and 20 inches

wide; an iron bar was fixed below the magnet, so as to act as a lever of the second order; the different weights supported, were estimated by a sliding weight, in the same manner as with a common steel-yard. See the sketch of the magnet.

In the experiments immediately following, a small single battery was used, consisting of two concentric copper cylinders, with zinc between them; the whole amount of zinc surface exposed to the acid from both sides of the zinc, was $\frac{2}{5}$ of a square foot; the battery required only half a pint of dilute acid for its submersion.

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

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

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

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

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

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

Ex. 14. When all the wires (nine in number) were attached, *the maximum weight lifted was* 650 *lbs.*, and this astonishing result, it must be remembered, was produced by a battery containing only $\frac{2}{5}$ of a square foot of zinc surface, and requiring only half a pint of diluted acid for its submersion.

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

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

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

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

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

Ex.. 17. With six wires, each thirty feet long, attached to the galvanic element; the weight lifted was 375 lbs.

*Ex*d 18. The same wires used in the last experiment, were united so as to form three coils of 60 feet each; the weight supporte was 290 lbs. This result agrees nearly with that of Exp. 11, though the same individual wires were not used; from this it appears, that six short wires are more powerful than three of double the length.

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

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

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

A number of experiments were also made with reference to the best form of the iron to receive magnetism, but no very

satisfactory results were obtained; of these, however, the following are considered as not uninteresting:—

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

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

Ex. 23. A piece of a gun barrel, little less than an inch in diameter, and about 8 inches long, and from $\frac{1}{12}$ to $\frac{1}{15}$ of an inch thick, weighing 8 oz. 3⅔ dr., with the wires and battery as before, lifted 40 lbs.

From the last experiment, it appears that a given quantity of iron, in the form of a hollow cylinder is capable of receiving more magnetism than that of a solid cylinder of less diameter; but it is evident, from Ex. 21, that a solid bar of the same diameter as the gun barrel, and of greater weight, would have lifted more; perhaps the gun barrel was not sufficiently thick for the full development of magnetism, which, according to Barlow's experiments, resides near the surface.* A series of experiments† were separately instituted by Dr. Ten Eyck, in order to determine the maximum development of magnetism in a small quantity of soft-iron; From these the following interesting results were obtained:—

Ex. 1. A horse-shoe of round iron $\frac{36}{100}$ of an inch in diameter, 4 inches long, weighing 2,314 grains, and wound with 23 feet copper wire, diameter $\frac{45}{100}$ of an inch, with a pair of one inch plates, lifted 19 lbs., 5 oz., 6 dwts., 16 grs.; with a pair of 4 inch plates, lifted 25 lbs., 6 oz., 5 dwts.; with the cylindrical element used in Exps. 8, 9, and 10, of former series, it lifted 42 lbs., 6oz., 8 dwts., 8 grs., or 105 times its own weight.

Ex. 2. A horse-shoe of round iron ¼ inch in diameter, 3½ inches in length, weighing 310 grains, and wound with 15 feet copper wire, diameter $\frac{45}{100}$ inch, with a pair of one inch plates, lifted 3 lbs., 11 oz., 7 dwts., 22 grs.; with four inch plates it lifted 5 lbs., 5 oz., 12 dwts., 12 grs.; with the cylindrical element, 8 lbs., 2 oz., 8 dwts., 18 grs., or 152 times its weight.

Ex. 3. A horse-shoe formed of a flat bar $2\frac{1}{10}$ inches long, $\frac{3}{10}$ broad, and $\frac{6}{100}$ thick, weighing 84 grains, and wound with 16 feet of brass wire $\frac{20}{100}$ of an inch in diameter, with a pair of one inch plates, lifted 3 lbs., 2 oz., 3 dwts., 8 grs.; with 4 inch plates, lifted 2 lbs., 10 oz., 2 dwts., 12 grs.; with the

* See Barlow's Essay on Magnetic Attractions, page 50.
† Troy weight is used in these experiments.

cylindrical element, 2 lbs., 10 oz., 13 dwts., 2 grs., or 198 times its own weight.

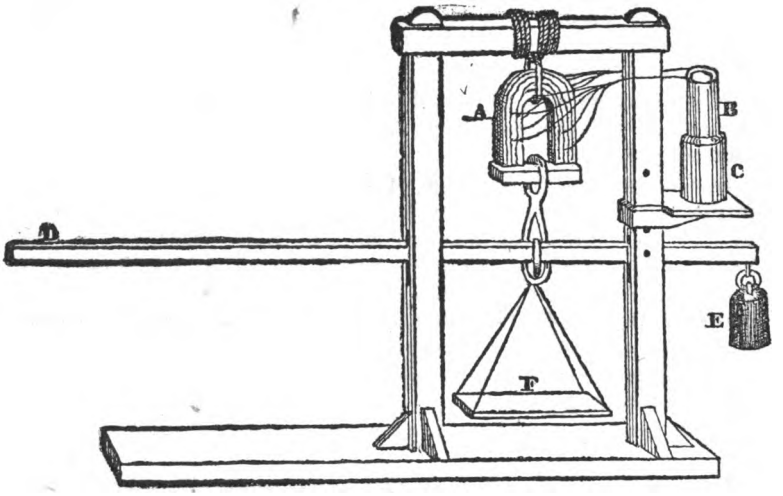
Ex. 4. A horse-shoe of round iron, slightly flattened, one inch in length, diameter (before flattening) $\frac{6}{10}$ inch, weight 6 grs., and wound with 3 feet brass wire, same diameter as that of No. 3, with a pair of one inch plates, lifted 2 oz., 15 dwts., 1 gr.; with four inch plates, lifted 3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

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

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

While engaged in these investigations, the last No. of the Edinburgh Journal of Science was received, containing Prof. Mohl's paper on Electro-Magnetism, some of his results are in a degree similar to those here ascribed; his object, however, was different, it being only to induce strong magnetism on soft-iron with a powerful galvanic battery. The principal object in these experiments was to produce the greatest magnetic effect with the smallest quantity of galvanism. The only effect Prof. Mohl's paper has had over these investigations has been to hasten their publication: the principle on which they were

instituted was known to us nearly two years since, and at that time exhibited to the Albany Institute.



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

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

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ERRATA.

Page 38, line 16, dele "both."
Page 33, line 6th from bottom, for "were" and "are" read "was" and "is."
Page 48, line 15, for "eight" read "nine."
Page 96, date at the bottom, for "1853" read "1854."

Editorial.

PROF. MORSE'S DEFENCE AGAINST PROF. HENRY'S ATTACK.

It is a matter deeply to be regretted, when it becomes necessary for one to defend himself against the unjust aspersions of a fellow-man, but more particularly so in the case given in the present number of the "Telegraph Companion." Although we most heartily concur in the utterance of every word expressed by Prof. Morse, we are, nevertheless, pained to feel, that it is a necessary infliction of a just punishment, and one, too, that is administered with the most moderate feeling, upon the part of Prof. Morse, towards one who has done so much to his injury.

After reading the exposé of Prof. Morse, of what Prof. Henry has done and what he claims to have done, and his incomprehensible course, no one, it occurs to us, can consider the conduct of Prof. Henry as other than evidence of hallucination. Prof. Henry has been regarded as a distinguished man in American science. He has occupied stations of honor in our institutions of learning, and his energy and presumed talents have given him a name of distinction throughout his own land, and have also won for him the admiration of the savans of Europe. The promulgation of the facts, by Prof. Morse, will startle those who have accorded to Prof. Henry unmeasured praise. The question as to what he has really discovered, will naturally arise in the minds of all. It now seems that the whole of his claims to discoveries in science, are to be considered with doubt. Prof. Morse proves his statements so grossly incorrect, that in future no dependence can be placed upon the statistics or assertions put forth by Prof. Henry. The defence of Prof. Morse is sustained by evidence which cannot be questioned, and the tone and words used by him, in repelling such an unjust attack, are certainly marked with as much candor and moderation, as can be desired by the most fastidious reader.

We have not the room in the present number to give a review of the questions under consideration, and so ably set forth by Prof. Morse. In the next number of the "Companion," we may have something to say on the subject.