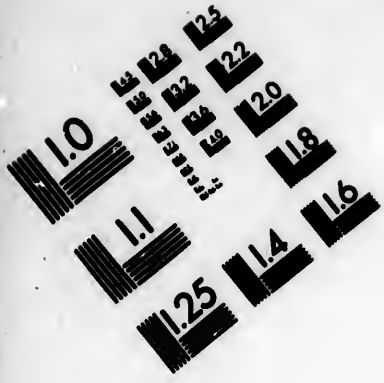

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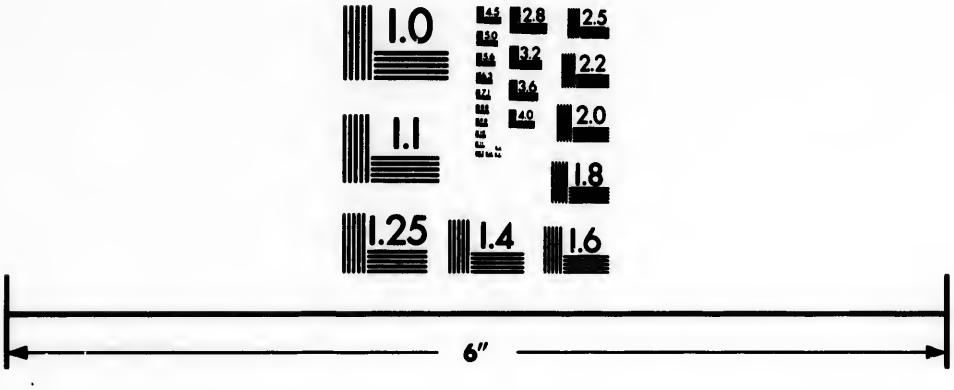

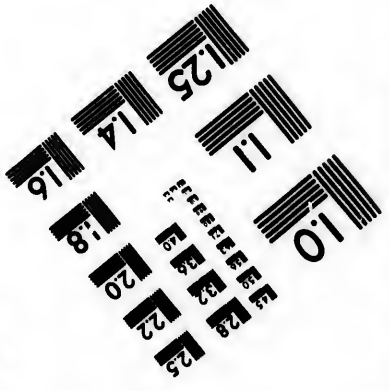

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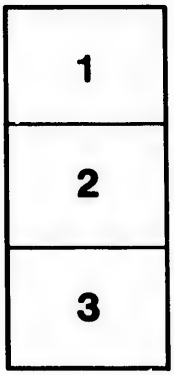

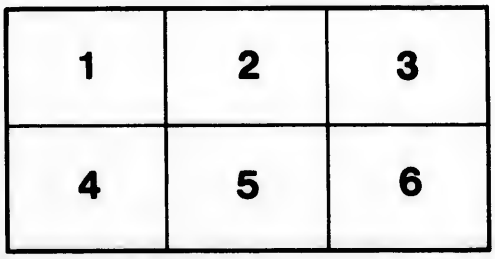

THE

ATLANTIC TELEGRAPH.

THE

RISE, PROGRESS, AND DEVELOPMENT OF ITS ELECTRICAL DEPARTMENT.

BY

EDWARD ORANGE WILDMAN WHITEHOUSE,

ELECTRICIAN-PROJECTOR OF ATLANTIC TELEGRAPH.

LONDON:
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1858.

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RISE, PROGRESS, AND DEVELOPMENT OF ITS ELECTRICAL DEPARTMENT.

THE excitement, consequent upon the unexpectedly successful laying of the Atlantic Cable, and the realisation of electric communication with America—followed as it has been at so early a time by a painfully ominous silence of many days—leads me to believe that a succinct outline of the scientific part of the undertaking would be valued—more especially as there are many facts of importance connected with the Atlantic Telegraph of high interest to the public, which can be elicited only from those whose intimate connection with the enterprise from its very birth has placed them in a position accurately to know, as well as to chronicle such matters.

Dismissing, therefore, entirely, every other feeling than the interests of Science, and more particularly with a view to the progress throughout the world of extended submarine electric intercommunication, I will endeavour to lay before my readers an outline of the glorious enterprise as it has developed itself under the eyes of one of its earliest promoters.

To Professor Morse of America belongs the honour of the first published opinion in 1842, declaring the possibility of

connecting Europe and America by telegraph. The idea was for some time compelled to lie dormant. The first step of actual progress was the discovery of Gutta-percha and its properties as an insulating medium. In 1847 and 1848, the use of underground wire insulated with gutta-percha was proposed and adopted in England by Charles V. Walker, Esq., F.R.S., and in Prussia by Werner Siemens, of Berlin.

The first submarine experiment made in the open sea by use of gutta-percha, was made January 10, 1849, by Charles V. Walker, Esq. He attached two miles of insulated wire, submerged in the sea, to the end of one of the wires of the South Eastern Company's system, at Folkestone, and spoke through it to the Directors in London from the deck of the steamer.

In January 1850, the Messrs. Brett projected and obtained concessions for a line across the Channel; in order to secure which it was necessary to carry a temporary line across at once, and demonstrate its capability before the 1st September of the same year. So small, however, at that time was the public confidence in this undertaking thus begun, that some of the chief contributors to its success have told me that it was not without difficulty they raised among their friends even the very small amount required for this experimental line.

Success, however, crowned their efforts on August 20, 1850, and from that day all doubts as to the possibility of submarine telegraphs were set at rest. A cable of massive construction was laid on September 25, 1851. On the occasion of the only interruption from accident, the vital part of the cable (the insulated conductor) was found, except where broken by violence, to be in as perfect a condition as the day it was laid, the gutta-percha appearing unchanged, and in the highest state of preservation.

Coincident with the use of the gutta-percha covered wires, were observations made by those in charge of various telegraph instruments, that a peculiar form of embarrassment arose in connection with such wires. In the course of his early experience Mr. Siemens had noticed a new and important fact in

connection with the behaviour of such buried wires. I make especial reference to this, because it constitutes the marked distinction between aerial and submerged telegraph wires, and is the origin of the many difficulties with which the latter are compassed. He noticed that, if one pole of a battery was connected with the earth, it was possible to pour a current from the other pole into the near end of a buried wire, although the far end of this wire were retained insulated and the circuit open; and also that the electricity thus poured into the insulated wire could be again drawn out or discharged from either end at pleasure. He investigated this phenomenon; the result at which he arrived is given in a Memoir, presented to the Academy of Sciences at Berlin, on April 15, 1850.

The attention of Professor Faraday was directed to these phenomena by the Electric Telegraph Company. He communicated the results of his inquiries to the Members of the Royal Institution, on January 20, 1854. He confirmed the conclusions at which Siemens had arrived, and also showed that the electric currents which he employed, travelled at the rate of only 750 miles per second along buried wires. He explained how this retardation was due to the Leyden charge, which the current had already been proved to confer upon the wire. In March 1855, he confirmed some results communicated to him by Mr. Latimer Clark, that no increase of velocity was obtained by increasing the intensity of the current by adding to the number of battery cells. Faraday's special work as a philosopher and teacher ended here, when he had in a most masterly manner investigated a new and difficult subject.

The result of these researches, however, by no means ended with the publication of his paper upon the subject. A general opinion prevailed that the facts elicited by him were such as to place an almost insurmountable limit to the more extended spread of submarine telegraphs. The embarrassment to signalling would be such, it was said, as to make it impossible to work any great distance, while the delay or retardation experienced by each wave of electric force would be so great as to make it impossible that any but the shortest lines could work

at such a speed as to be either useful or profitable. Thus the probability of the accomplishment of an Atlantic Telegraph seemed to fade upon the dim horizon, while the chance of any early attempt being made to realise it became altogether hopeless.

It was in March, 1855, and under these circumstances, that I was led to pursue with greater diligence the investigation of the subject which I had commenced in the previous autumn. Already practically conversant with electric research, and with the details of every telegraph in use, their peculiarities and relative advantages, it appeared to me that this problem demanded for its solution a combination of philosophical research of the most extended character, with the most patient and unwearied assiduity in the tentative application to practice of the principles thus elicited. Faraday had enriched the world with new facts, and made known their conditions and laws; but he had left each of us to work out their application to his own purposes and requirements.

Here then was a field upon which I entered with the object of ascertaining, and if true, of demonstrating the possibility of working an Atlantic Telegraph. This object was not taken up lightly, or at hap-hazard, but in full view of those difficulties which at that time were believed by many to be insurmountable; of the almost interminable amount of labour which for years it would necessarily involve; of the very large expenditure which it would require of means drawn from the exercise of an arduous profession; of the injury which the so-called "dabbling in science" would infallibly produce in my professional status and practice; and, lastly, of the great and irretrievable blow which in all these respects would surely follow any ultimate failure or want of success in my undertaking.

From this time every available hour was devoted to the practical and experimental examination of this subject. The observation of new phenomena, I soon found, required the construction of new instruments of research fitted for the especial purpose of their examination. Results of intense interest and of the highest value were thus obtained; some of

which were laid before the British Association at their meetings at Glasgow and Cheltenham in 1855 and 1856, with the special object of exciting an interest in and aiding the cause of Atlantic Telegraphy.

Observations upon the velocity of the electric current under varying circumstances (amounting to many thousand in number) were made upon different lengths of cable in process of manufacture, and the encouraging fact was brought to light that by simply altering the electrical conditions, the transmission of a wave of force through a given distance can be materially accelerated, and in fact that not only can a great economy be attained in the time of transit of each wave, but also that a proportionately greater number of currents can be sent through the same distance in the same time.

The limitations to the working speed of various forms of telegraphs imposed by the electrical conditions of submarine conductors were closely and critically examined, and important results elicited; and here again was made evident the necessity for a specialty in the investigations, as well as in the instruments employed.

The effect of induction, as manifested by the retention of "charge" in the cable, became an object of special research —and a process was arrived at by which the amount of electricity retained and the time of its retention have become the most valuable mode of testing the integrity of the insulation —so perfect, indeed, as to surpass, and almost entirely to supersede, the old mode of testing.

I greatly desired to direct the attention of the scientific world to the importance and the novelty of the results of these researches as they presented themselves, in order that the examination by other minds might severely scrutinise and test their validity and value; but incessant and laborious occupation in the same field prevented my carrying this design into effect. I felt that the opportunities which at that time presented themselves for such research were too valuable to be lost, and that perforce I must postpone publication of the results.

It was at this time that I received from one high in the scientific world, Professor Tyndall, the advice which, coincidently with other considerations, determined the relinquishment of my professional position in Brighton, in the pursuit and investigation of the great problem of Atlantic Telegraphy. He counselled me to give myself wholly to the work, and to make it my own, adding that, as far as he knew, no one else had been working in the same field, at least not in the same manner or with the same success. About the same time Mr. Brett kindly placed at my disposal for research a length of cable, then in process of manufacture for him for the Mediterranean line.

Thus encouraged, the field enlarged before my eye, and ever bearing in view that these researches, to be immediately valuable, must be essentially practical, I endeavoured to grasp and realise every detail as it presented itself, laying up in store for future use the principles thus elicited, and incorporating them into the construction and working of new forms of instruments, several of which I subsequently patented.

Anxious to test these to the utmost under varying and less favourable conditions, as there was at that time no cable of any considerable length laid down, I sought the use of long lengths of insulated subterranean wire, as offering the closest resemblance in function to that which was the special object of research. The system of wires belonging to the Magnetic Telegraph Company seemed to offer the greatest facilities for this purpose. They extend from London, *viâ* Liverpool, Dumfries, Donaghadee, Portpatrick, and Belfast, to Dublin, a distance of 660 miles. Mr. Charles Bright, the engineer of that Company, then stated that the establishment of an Atlantic line of telegraph had long been one of his favourite objects of speculation and of occasional experiment, and he proposed that we should co-operate for the better attainment of our joint object of pursuit. Assenting to this proposition, and to the terms on which it was to be concluded, I entered into an agreement, giving me, at his suggestion, the larger share of anything that might result from our union.

The first joint experiment which we made with my new instruments, designed and constructed especially for such purposes, took place on November 5th, 1855. The whole of this length of subterranean or submarine wire had never before been spoken through by direct action; the messages always having been repeated, by hand, at the several breaks or junctions. The wires were, on this occasion, connected so as to form a single unbroken length of 660 miles, from London to Dublin, which was signalled through both ways, by use of my instruments, with perfect success on the first and only occasion of trial.

Subsequently, in the autumn of 1856, Mr. Cyrus Field took occasion to engage my active co-operation in the great work, admitting me to fair participation therein, and recognising me, by agreement, as one of the original projectors of the Atlantic Telegraph.

The magnitude of the undertaking soon made a division of labour necessary, and while I retained the entire control of electrical matters, Mr. Bright devoted his time to the details of the engineering work.

Additional cables manufactured at Greenwich (Glass & Elliott's) during this year, afforded continual opportunities of extended research, of which the utmost use was made. The arrival of Professor Morse in England enabled me to repeat experiments upon many points for his satisfaction at Greenwich and Blackwall; and I had the advantage of the daily counsel and co-operation of the veteran electrician for many weeks. Cables, containing wires whose aggregate length amounted to nearly 1200 miles, were at our disposal for a considerable time; and no point, I believe, of practical importance escaped close and rigid examination.

The results of these trials were deemed so encouraging, that advantage was taken of the opportunity, and it was thought right to commence the organisation of the Company. To this end an experiment was made on a length of 2000 miles of underground wire belonging to the Magnetic Telegraph Company, and upon a successful result being obtained, it was forthwith incontinently published. This was the second and last

occasion on which I made any joint electrical experiments with Mr. Bright. Of this experiment, upon the issue of which so much was made to depend, I am bound to say that my own habits of research would have led me to repeat it again and again in various forms, before I should have ventured to regard it as a basis of operations in so gigantic an enterprise.

Without (at this distance of time) throwing any doubt upon the facts, I am free to admit that the inferences drawn therefrom were in some respects incautious, and the promises of working speed too sanguine: in which respect more extended experience has convinced me of several sources of error.

The organisation of the Company being now likely to proceed rapidly, I made arrangements for a course of critical and practical researches upon the size and form of conductor most suitable for our purpose. It was obvious that no existing line could afford any criterion; and as the widest difference of opinion existed among electricians upon this point, I did not deem it right to decide the question except by appeal to actual experiment. A conductor about thirty times the sectional area of the present wire had been seriously proposed and thought necessary. Two systems of insulated wires of equal length laid under similar circumstances between the same towns in the north of England, and differing only in the size of the conductors which they contained, seemed to offer themselves for examination. I therefore proposed to seek from the companies to whom those wires belonged, permission to experiment during non-business hours, and to extend my investigations over several weeks.

The rapid and hasty progress of the Company however forbade it; and most reluctantly was I compelled to forego this necessary examination, and to decide immediately upon a conductor which should be certain to answer the purpose, though I had not had the opportunity of satisfying myself that it was the one best fitted for our use.

The form and structure of the conductor and its insulating coatings having been decided upon with all the care which the circumstances allowed, the question of the external form and

structure of the cable remained to be discussed. While, therefore, the engineer was engaged in examining the mechanical fitness, and testing the breaking strain of various specimens of cable of different structures, I had ascertained their specific gravity, and constructed apparatus, and made arrangements for determining the rate of sinking in deep water of several specimens of the best forms of cable proposed, in order that I might be able in this respect to judge of their relative merits.

The Company's operations were now being pressed forward, and I urged most strongly the necessity for the utmost facility to be afforded to the Electrical Department, in order to ascertain from time to time, as the cable increased in length, the fitness of the instruments for their office, and that if alterations should be required, they might be made simultaneously with the growth of the cable. The work was so vast and yet so rapid in its progress, that it would not be possible otherwise to keep pace with it, or to insure the perfect adaptation of means to end.

Duplicate sets of instruments for both stations were to be designed and made under my own eye, and to be perfectly ready in six months. Meanwhile, there did not exist anywhere the opportunity of trying them adequately, except such as the Directors should provide.

Under these circumstances it pained me to find that the contracts for the cable were given out to two manufacturers situate at different parts of England, without any provision being made for the possible use of the entire cable in one unbroken length by myself. Thus, that which I had described as essential to enable me to prepare instruments fit for immediate use was denied me apparently without compunction, and my earnest efforts to insure success frustrated and set aside by those who were unable to appreciate their value.

The testing of the cable during the process of its manufacture presented points of difficulty and new phenomena of great interest. Knowing the importance of this subject, it was my wish to attain the highest degree of perfection in the insulation, an amount of defect or imperfection which might be of no

importance for shorter lines being sufficient to prove absolutely fatal to the project. It became necessary, therefore, to adopt a very high standard, and to make use of a more than usually searching current.

Every mile of cable made came in detail under my own personal examination, and was tested in various ways—for which purpose detectors of special delicacy were constructed. The constant examination of lengths in detail in this manner, enabled me, as mile by mile could be added at pleasure, to make repeated researches into the best means of detecting a fault at various distances. A known faulty piece could be inserted at any distance, and then in a variety of ways, with different instruments, examined and detected. I have records of a great number of trials of this character in which my assistant inserted the fault at a spot unknown to me, in order to test and verify our calculations.

The tact thus derived both by myself and assistant from these repeated experiments has proved to be invaluable, and has already been of the utmost service in the operations of the Company. It has pointed out with confidence the position and nature of an injury immediately upon its occurrence.

During the manufacture of the cable at Greenwich, the opportunity was afforded of observing the effect of temperature upon the insulation in the most marked manner, the change from night to day, and from sunshine to shade, producing a variation in the state of a portion of our cable to a degree which was at first sight alarming. This effect is transient, and ceases when the temperature falls. It is quite unconnected with the occurrence, at a later period, of serious injury to some miles of our cable by the actual melting of the gutta-percha under exposure to the intense heat of the sun. An accurately drawn table, the result of multiplied observations, has been constructed, both as a record for myself and a guide for others who may follow.

During the manufacture of the cable, Professor Thomson drew my attention to certain variations in the conducting power of different specimens of copper wire, amounting in some

instances to as much as 45 or 50 per cent., while yet no proportionate chemical impurity could be detected. From that time every separate hank of wire was tested by myself or my assistant, and all below a certain standard rejected. These, and other points of novelty and of difficulty from time to time presented themselves, and it was necessary that they should be at once met and mastered.

I turn now to some points in the early progress in the engineer's department. In order to determine the best form of cable, great numbers of specimens were made, and of various constructions—some excelling in strength, some in flexibility. They were limited to an average weight of not more than a ton per mile. These were subjected to the most critical trials for strength and flexibility, and the question of their mechanical fitness was, in this respect, I believe, fairly exhausted. Experiments were continued at intervals for several weeks, and the result led to the adoption of the present cable, no trials having been made of the rate of sinking of the various specimens.

The consideration of this vital question having been repeatedly discussed by the Board, had been finally referred by them to a sub-committee of three (of which neither the engineer nor myself formed part), consisting of Mr. Field, Mr. Brett, and Mr. Tupper, and was by them almost immediately decided, upon what appeared to me a very limited and partial examination: flexibility, weight, and strength being apparently the sole points considered: to the exclusion of the questions of specific gravity or the rate of sinking—two points of essential importance in determining the strain upon the cable during the process of paying out.

A specimen cable of light specific gravity and great strength, made after experiments of my own, was rejected by one of the members of this committee in an almost contemptuous manner. —"I would not have it if it were laid," said he.—Nevertheless, I thought it right in a written report to state my own personal conviction of its superiority, with the reasons for my preferring it to the present form.

It is to be regretted that an early promise, incautiously made to the shareholders and to the public, of the intended completion of the line within the year, prevented the determining of many of these points by direct experiment. It had been proposed, and with justice, to make trial in deep water of the relative mechanical advantages of several of the best forms of cable, by paying out and raising again considerable lengths of each. Much information of the highest value would thus have been obtained, both with regard to the best structure of cable, and of the machinery to be employed in its submersion.

The result of the unsuccessful expedition of last year, of the experimental trip, and of the early attempt of the present year, and not less the details of the successful expedition itself, faithfully considered, all lead irresistibly to the conclusion that very great expenditure and grievous discouragement would have been avoided if the course above advocated had been at that time adopted by the Company. It is equally clear that the results of our past experience may lead us on a future occasion to provide a more suitable cable at less cost, requiring a paying-out machinery of a more simple and less ponderous construction, and attended with less hazard in its use.

It devolved upon the engineer at this time to devise the necessary means for the safe submersion of the cable, and it was decided—unwisely and unfortunately as it appeared to me—to make the same machinery fulfil its office for both the light deep-sea part, and the heavy shore-ends of the cable. This almost necessarily insured its unfitness for one or other of these purposes, as in point of flexibility the cables were the very opposite of each other, their size differed considerably, and their relative weights were as eight to one.

The early deep-sea experiment proposed for last year would have set the engineer's mind at rest upon many points of practical importance; but unfortunately time did not admit of this, and meanwhile the work must proceed, and everything be prepared for completion within the prescribed limit. The engineering part of the enterprise has, however, from the first been

brought so prominently before the public, that I need not linger over it, but return again to the special and less known details of progress in my own department.

It was not until a considerable length of cable had been manufactured at Greenwich, that I was enabled to commence trials of a set of experimental instruments made for the special purpose of determining the exact form, size, and structure of induction-coils best suited to our purpose. These trials were resumed from time to time, as the additional length of cable admitted, in order that I might work out the details of the laws by which I should determine the just proportion which each part should bear to others, and the whole to the length of cable, in order to obtain the best possible results. These determinations, involving a very considerable amount of research, were most necessary and important; but they required time, and more of this time than could well be afforded —time, that is, for the cable to grow from length to length, and time again for the alteration and remaking of these experimental instruments as the increased length demanded. Lastly, the actual telegraphic instruments intended for use on the line when laid, could not be put in hand with any certainty of perfect or satisfactory results till all these preliminary stages had been passed through, and a successful trial made upon at least 1000 miles of cable. They ought to have been tried upon 2000; but this had been rendered impossible by the distance which separated the establishments of the two manufacturers engaged upon our cable. It seemed thus likely to be quite as much out of my power to make sure of our instruments by experimental determination, as it had previously been placed beyond my reach to decide strictly and practically upon the size of conductor best fitted for the longest cable which the world would ever require.

Several incidents occurred during the manufacture which caused me the deepest anxiety. I have already alluded to the effect of temperature upon the cable at Greenwich. At Birkenhead the evidence was, if possible, more striking and more embarrassing. The cable was stored in a large room or shed,

under the same roof with the boiler and engine which drove the machinery for its manufacture. The testing of various lengths of cable at this place showed a great depreciation in its condition as compared with the very same lengths previously examined at the Gutta-percha Works in London. This state of things continued during the whole process of the manufacture. Observation satisfied me that the effect of the temperature alone by which it was surrounded was sufficient to account for its anomalous condition. At one time a hobnail drawn from the boot of a workman punctured the gutta-percha and ruined the insulation at the part. At another time a mysterious break in the conductor destroyed our continuity in a most perplexing and capricious manner. Again, there was a day on which the direct rays of the sun were so intense at Greenwich as to cause the gutta-percha in some exposed parts of the cable to soften and actually to exude, showing itself in drops or tears upon the outer surface. Several miles of the cable were thus most seriously injured. I need hardly add that in these and similar cases the injured parts were of course detected and most carefully cut out and removed, the cable being afterwards made as perfect as before. The whole of the steel-covered cable prepared for the purpose of bearing the greater strain expected at the mid-ocean splice was in this way destroyed. It was on consideration decided not to remake it.

The time fixed for the departure of the expedition was now so near at hand that it became necessary to complete our instruments without delay. On trying them through 1200 miles of cable, when that length was finished, shortly before the sailing of the expedition—it was satisfactory to find that there was ample power in the generating apparatus for the whole distance, so far as we could judge from the half-length.

The circumstances already mentioned made it, of course, impossible for me to have my clerks skilled in the manipulation of the instruments. These being necessarily new in their form, and differing in every respect so largely from those previously in use, the most experienced manipulators would have everything to learn afresh. If, therefore, the cable had been laid on

that occasion, we should have had no operators ready to work the instruments.

The trials made in Cork Harbour for the first time, upon the whole length of cable—hurried and imperfect as they were, interrupted too at the turn of tide—proved two things: first, that I had provided ample power; secondly, that the receipt of signals was slower than I had expected, and that consequently parts of the receiving and recording apparatus would require considerable modification. At this distance of time I can hardly trust myself to say how deeply I then felt that the interests and requirements of the electrical part of the undertaking had been subordinated and sacrificed to other and less weighty considerations. The paramount importance of the scientific element in such an enterprise was unrecognised: it remains so to this day. The expedition sailed and failed; its want of success, I believe, admittedly owing to the crude and untried machinery upon which the engineer relied, having proved itself to the most competent judges unequal to the safe submersion of either light or heavy parts of the cable. The opportunity for experiment would have shown wherein it required modification and improvement.

The electrical arrangements for the voyage were scarcely less crude and unprepared than those made mechanically for the submersion of the cable, for reasons already stated. I had expected to be able to make use of our special instruments for speaking during the whole voyage—a mistake as I am free to confess—but one which a previous rehearsal would have immediately made manifest; as it has since proved to be contrary to the very nature of things that our apparatus should do justice to itself in all the varied movements of a vessel in mid-ocean. The constant speaking, too, in the manner proposed, would have rendered the continued careful testing of the cable impossible; the mere interchange of intelligence, courtesies and congratulations, preventing the exercise of that cautious vigilance upon which, indeed, the success of the whole enterprise might absolutely rest.

The loss of the cable in the autumn of last year caused a

temporary paralysis of all our efforts. While, however, it postponed the accomplishment of our wishes, it tended towards insuring the success of our labours at a future time. It had been, in fact, a rehearsal—but made at great cost, attended by a lamentable sacrifice of *prestige*, and, moreover, without those advantages which a premeditated rehearsal would certainly have afforded; not the least of which would have been the confidence given to the public mind, by the obvious careful examination bestowed by the Company upon every part of the great work which they had taken in hand.

The unshipment and storing of our cable at Keyham for the winter promised at length to afford me the opportunity so long sought for and so deeply needed, of thoroughly maturing the details and working of our instruments, in order that on the opening of the line I might be ready for the transmission of intelligence without loss of time. It was not till about six weeks after the accident that I was able to get the first official step taken towards this object, and several weeks more were necessarily spent in transferring and refitting our workshop for repairs and alteration of instruments as required. One fifth, therefore, of the whole time had expired before the work could fairly be begun. In the interim, I was able to get a series of very valuable observations made upon the lost length of cable, one end of which was connected with Valentia Island, while the other had been dropped in the Atlantic. Terrestrial currents, variable in force and in direction, were noted as occupying the cable night and day; these have been tabulated and represented in a series of diagrams intended for presentation to the Royal Society.

The 2150 miles of cable stored at Keyham became now the subject of continued research. One of the points which I was anxious early to examine, was the influence which the coiled state of the cable could exert upon the results of my experiments,—what the amount of that influence might be, whether aiding, embarrassing, or in any way modifying the transmission of signals. The cable might be truly said to represent at that time a gigantic helix, containing 2150 miles of wire; and as it

is well known that the helical form of a conductor exerts in some respects a marked influence upon the current sent through it, it was, I thought, desirable to examine the question closely, to determine what amount of reliance might be placed upon results of trials of instruments necessarily made under such circumstances.

Repeated experiment showed that every wave of electric force on its entrance into the cable was accompanied, or rather preceded, by a smaller wave of opposite polarity, inductively excited in the adjacent helical turns; this small, but opposing antecedent wave, was traceable throughout the whole extent of the cable *pari passu* with the greater; and it was demonstrable that it tended under some circumstances seriously to embarrass the action of the instruments. I therefore felt, and was able to state with confidence, that this source of difficulty would cease upon the laying of the cable; and that, not only on this account, but from other considerations also, I should anticipate a greater ultimate measure of success than we could attain under the existing conditions at Keyham. I referred especially to the low temperature of the ocean depths, calculated largely to benefit the insulation, while at the same time it would augment the transmitting power of the conductor.

Satisfied that results thus obtained would be reliable, and would fall within rather than beyond the mark ultimately attainable, I proceeded to test in the most rigid manner the practical working of the instruments. The staff of manipulating clerks was engaged many hours daily in transmitting despatches; trying and comparing various instruments of my own, various modes of manipulating, and various forms of alphabet; in fact, practically searching for the best in each. Several most important advances in instrumentation and in detail, were obtained in this way, and were engrafted and incorporated with the Company's present system. Other improvements, the results of observations made at a later date, it was not in my power to incorporate, as my request for the additional aid required in the workshop met with a negative response from the Board.

It was my earnest desire at this time to bring before the scientific world some of the more novel and interesting of those phenomena by which I was daily surrounded, and to invite the presence of those specially interested in such matters; assured that by such intercommunication the real welfare of the Company would have been forwarded, while science would have been able both to give and to receive additional impetus. These wishes becoming known at head-quarters, I received a letter forbidding all scientific communication upon this subject, couched in terms so peremptory and oppressive, that I at once felt it necessary to tender my resignation. I was subsequently told that I had entirely misunderstood the purport of this letter, and, being urged to do so, consented to withdraw my resignation, entering at the same time an indignant protest against such attempts at the suppression of thought and intercourse on points of science.

I obtained, shortly afterwards, for several weeks, the kind counsel and aid of Charles Walker, Esq., F.R.S.; conjointly we investigated some matters of high interest, and elicited very important results with reference to the nature and degree of the static charge communicated to the wire under various conditions. We also made very accurate measurements of the proportion of electric force manifested at the distant end of the cable, under various circumstances, as compared with the whole amount of force entering, and the per-centage of loss. It had long been my wish to try some means of shortening the usual mode of telegraphic correspondence, and I had thought it possible to attain this end, either by the use of the phonetic system, or by having recourse to a code arranged on the principle of the Admiralty code, which by a single hoist of several flags can convey an entire sentence. Well aware of the difficulties surrounding this subject, and of its general inapplicability to short lines, I yet thought it worthy of investigation for the special purpose of the Atlantic Telegraph, where every moment saved in transmission would be of importance. I had in the previous year bestowed much attention to this subject, and had a large amount of matter in type and manuscript. The whole

was at that time set aside by order of the Board. Now, however, upon my renewed application, I was allowed to resume it at my own expense; and Mr. Bartholomew, the superintendent of Valentia Station, took the matter most zealously in hand for me, and repeated trials made at that time satisfied me that the foundation then laid admits of being wrought out into a system of great value for all submarine lines of 1000 miles or more, where the process of signalling is comparatively slow. It will require the bestowal of some months further of thought and labour upon the subject, ere it can be laid before the world in a complete form for use.

Some weeks before the first sailing of the expedition in the present year, the chief improvements in instruments—suggested by the experimental practice at Keyham—had been completed and tested repeatedly in every possible way. The final instruments for each station, embodying these improvements, were ordered and put in hand at Henley's, the best manufacturer in London. The certainty, accuracy, and definiteness of the signals received by this method, as well as the ease and correctness of manipulation, surprised and gratified me exceedingly. I could have wished, however, to have added the last improvement, which would have given seventy or eighty per cent. increase in speed, but the want of additional hands in our own workshop at the right time had already made it impossible; it can even now, without difficulty, be appended to the existing instruments at small cost. A transient doubt upon the subject having occasionally arisen in my mind, I thought it right to ascertain by experiment the possibility of rapid interchange of signals at short intervals from either end of the cable alternately, with as little delay as possible. It was gratifying to find that my fears had but little foundation in fact; we could work either way with equal facility, and could readily interchange within a few seconds.

Professor W. Thomson at this time visited Keyham with the object of testing and introducing a new system of telegraphing, about which he was very sanguine, the principle being that the various letters of the alphabet were to be

indicated by corresponding degrees of force exerted by the current and rendered visible upon a small and delicate galvanometer, a single motion sufficing for each letter. A ray of light constituted the index, as is usual in very delicate instruments, and by the movement of this in degrees of deflection, the despatch was to be read off. This system, beautiful in theory, seemed to require much to reduce it to a practical form, if, indeed, it should ever be made available. The nature of the galvanometer, however, and the very small amount of its resistance, allowed it to be left in circuit without interfering with the action of any other instrument then in use, while its delicacy and freedom from inertia or momentum made me think highly of it for other purposes than those for which it was designed by the Professor. I thought I saw more in it as a galvanometer than Professor Thomson had anticipated, but not in his mode of using it. It received the coil currents of my instruments readily, and the letters of our usual alphabet were easily read off upon it and could be either written down or recorded by finger-key upon chemical paper in my usual way. I therefore determined at any future opportunity to give it a full and fair trial under my own eye, and for the purposes to which I have alluded.

The last two or three weeks before the departure of the vessels from Keyham, was necessarily a period of considerable excitement. The directors came down to examine the working of our instruments, with the accuracy and certainty of which I understood they were well satisfied. Professor Thomson's researches and rehearsals were at their height. The vessels left Plymouth for the Bay of Biscay to make an experimental trial in deep water, and returned having learned much, but not perhaps all that such an opportunity might have taught.

The paying-out machinery having been retouched and adjusted in such points as the engineers thought necessary, the expedition set sail for mid-ocean. Of the terrible storm encountered on that occasion by the Agamemnon, the stirring history which appeared in the "Times" must be well in the remembrance of all who read it. The vessels meeting subse-

quently at the rendezvous in mid-ocean, the attempt was made to lay the cable, but without success; the causes of the several accidents on this occasion do not appear to have been clearly ascertained or defined. The first we may regard as having been merely a false start, so small an amount of cable being payed-out: the second remains quite unexplained: while the third seems to have involved the mechanical paradox of a sound cable being suddenly snapped by a strain far below that which it ought to bear, and which by examination afterwards the broken end was found able to bear. Close examination of the broken ends of the wire at the point of fracture revealed nothing, and the paradox, so far as the engineers are concerned, remains unsolved.

Awaiting the arrival of the vessels, the Chairman, Vice-Chairman, Secretary, and several of the Directors spent anxious days at Valentia, till at length came the unwelcome tidings of the failure, and of the Niagara's arrival at Queenstown. The result of this trial was so pre-eminently discouraging, that the warmest friends of the enterprise regarded the sailing of the ships on the next occasion, more in the light of a forlorn hope than of an expedition with any prospect of successful issue. This at least was the tone of feeling which came most frequently before me in conversation with all those in connection with the enterprise—success possible, but almost infinitely improbable.

The electrical arrangements to be made use of this year in the process of paying-out had been reduced to the simplest elements, and the instruments required were of the least complex form. No telegraphing, in the ordinary sense of the term, was to be attempted between the ships, but a code of signals to be used interchangeably every ten minutes by each of the vessels in turn, afforded at once the means of incessant testing, and of conveying, by a change of symbol, information on one or two points connected with the progress of the work. As every ten miles of cable was payed-out, each vessel was to notify the fact to her companion, and to receive her acknowledgment thereof, in token of her receipt of the intelligence. The announcement

of being in shoal water, and of going to land the end, with a very few others, filled up the category of this simple code.

The expedition sailed, bearing with it the good wishes of all, and the sanguine hopes of few, if any, of those connected with the undertaking. That which seemed hardly to be in the engineer's power to accomplish by any effort of skill, was effected for him by other agency than his own, and this in a manner which he, perhaps, would least have anticipated. The machinery contained no special adaptation for *rough* weather; it had been wished to select the finest period of the year, and to take advantage of the calmest weather for the purpose. It is the opinion of one the most competent to judge on board the Agamemnon, that the rough weather on this last occasion saved the cable. This he explained to me as arising from three causes: First, that it made them go faster than they otherwise would have done, and this greatly relieved the strain upon the cable. Secondly, that the load on the breaks being thus rendered unnecessary, it was in great measure removed, and thus a source of great risk greatly lessened. Thirdly, that on the commencement of rough weather, it became obvious that the rise and fall of the stern of the ship alternately hastened and then retarded the movements of the wheels to such an extent, as to make it certain that the inertia of the paying-out sheaves would shortly snap the cable. No compensation apparatus had been provided for this, nor could the steam-engine which had been fitted up on board be made to answer this purpose; but the hands of the sailors, guided and encouraged by their officers, performed this duty, regulating the run of the wheels with the rise and fall of every wave, day and night without intermission, from the commencement of rough weather on the 1st, till the Agamemnon cast anchor at Valentia on the 5th of August.

I have carefully looked over the published Report of the engineer, and can find in it no allusion to this fact; perhaps it escaped his attention: it is the more necessary, then, that it should be brought prominently forward as a matter of the highest importance towards the successful laying of a

future cable. It proves that where engineering skill may fail, unwearied care and attention on the part of others engaged in the work may win success. True, Captain Preedy and the officers of the Agamemnon are not entirely forgotten in the Report, but the acknowledgment of their labours is dealt out with such scant measure that it falls on the ear worse than an empty compliment to those who know how largely, how entirely, the success has thus been earned by the officers and crew of the Agamemnon. Last year the nautical element in the expedition was made light of and disused—this year it seems to have been made use of and ignored.

The arrival of the Agamemnon and Niagara at Valentia and Trinity Bays respectively took place within a few hours of each other. Within a few miles the same length of cable had been expended by each; communication had been maintained, with one or two temporary interruptions, by means of the pre-arranged ship-signals until the time of landing. These interruptions had given rise at the time of their occurrence to the greatest anxiety; but, whatever had been their origin, they had been remedied or passed away, and had ceased to produce any serious inconvenience. The ends of the cable were now landed, the distant one in an almost wilderness at the head of Trinity Bay, our own on Valentia Island. Communication, at first maintained by the ship-signals only, was shortly attempted by use of our special instruments for speaking. Newfoundland answered our coil currents by giving us the pre-arranged landing signal as used on ship-board, and this was repeated frequently during the day. We therefore waited patiently till time should admit of their making the necessary arrangements, continuing, meanwhile, to exchange signals with them, their special instruments for speaking of course not yet being prepared or adjusted.

By midnight on the fourth day after the landing of the end, the speaking instruments at Newfoundland were sufficiently adjusted for them to work. We received from them words at good speed and with perfect accuracy, which were recorded at times by use of my own instruments alone, at other times

received also upon Thomson's galvanometer and recorded in the manner already described at Keyham, and sometimes by both systems simultaneously. Sentences and messages followed, and the facility with which these were received, led me to believe at first that a difficulty which they experienced in reading our signals was to be attributed to the injury, or still imperfect adjustment of the delicate receiving instruments carried out in the Niagara and Gorgon.

The gradually decreasing strength of our current as received at Newfoundland, and reported by them, soon made it evident, however, that slight, but gradually increasing injury to our cable at this end was the cause of this embarrassment. This continued to increase, till they were reduced to reading our signals by barely visible movement of their most sensitive detector—half a degree—while we received more than a hundred times this amount of force from them. Our mode of working was then modified to suit the altered condition of the conductor—voltaic currents of low intensity being employed, and the signals being sent, at their request, much more slowly than before.

Soon they ceased to receive even this feeble trace of communication from us, while yet their signals as received by us were of ample strength. After a time these also began to diminish very considerably in force, and it became evident that the injury was gradually increasing, and would shortly interrupt all communication either way.

The peculiar mode in which this difficulty manifested itself, pointed unmistakeably to the Valentia end as the seat of injury. Our superintendents at Newfoundland themselves suspected it, and twice telegraphed to us to test for it. I had satisfied myself, by most conclusive tests, that it was close at hand.

The shore end, intended for protection of our cable, was lying at Keyham, while our slight and fragile cable, prepared only for the tranquil ocean depths, was exposed to the full swell of the Atlantic on the Irish coast, and to the swaying to and fro with every tide.

I believed, from my testing, and from the gradual mode in

which the injury had manifested itself, that the fault would be found slight in degree, but extended probably over that portion of our fragile cable between high water-mark and the deep and tranquil sea, for which it was alone intended, where it was exposed—and allowed to remain exposed, I cannot but say negligently—almost wantonly—to an amount of strain and attrition for which it was known to be unfitted.

I will not here offer an opinion as to whether it was the duty of the Board or their engineer to have guarded against the occurrence of injury from such well-known causes while the means were at hand provided by themselves: suffice it that up to the fourteenth day, I was left, surrounded by these difficulties, in responsible charge of the station without support.

I therefore took upon myself the *onus* of directing the cable to be raised and examined by Mr. Canning. The weather prevented the completion of this till the day on which I left for town by order of the Directors. On the evening of that day I was informed by telegraph at one of the stations on my journey, that our superintendent had discovered and cut out a faulty part, which by testing had given 45 degrees of loss. And the following morning, on my arrival at the telegraph station at Dublin, I found that the President's message, which had been waiting a long time, was at last coming through the cable.

The submersion of the shore end—always admitted to be necessary—and which ought to have been ready on the arrival of the ships, has been, in my opinion, unnecessarily delayed. It is now again, I hear, postponed, and May is spoken of by some of those immediately connected with the Company as the proper time for recommencing operations. Meantime, no steps that I can ascertain have been taken since I left Valentia to prove the validity or otherwise of my opinion, further than multiplied appeals to a mode of testing demonstrably uncertain in its application to the present purpose.

Of the circumstances attending my resignation of office, the public will become aware through other channels; I will not, therefore, now enter upon them. If the opinion which I had

then formed and expressed as to the nature and seat of the injury should prove to be correct (and I have heard nothing since my resignation which leads me to think otherwise), the Directors of the Atlantic Telegraph Company stand in a remarkable position. Having a success which has startled the whole world laid at their feet, they know not how to use it, but by apathy and incompetence suffer it to elude their grasp, and the grand enterprise of the day to fall into collapse.

P.S.—While penning these lines, I am informed that communication from Valentia Island to the mainland, hitherto established by means of a spare mile-length of the Atlantic deep-sea cable, has been for some days entirely interrupted in consequence of injury occurring within the harbour.

Royal Institution, Albemarle Street,

September 18, 1858.