THE

SCIENTIFIC WORKS

OF

C. WILLIAM SIEMENS, Kt.

F.R.S., D.C.L., LL.D.

CIVIL ENGINEER.

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CIVIL ENGINEER.

A COLLECTION OF

PAPERS AND DISCUSSIONS.

EDITED BY

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THE

SCIENTIFIC PAPERS

OF

SIR WM. SIEMENS, F.R.S.

ELECTRICITY.

ON AN IMPROVED ELECTRIC TELEGRAPH.

By C. W. Siemens.*

A paper on an "Improved Electric Telegraph by Mr. E. W. Siemens" was read and its action illustrated by a pair of working instruments.

Each instrument consists of an electro-magnet between the poles whereof an armature oscillates. Connected with the armature is a lever, which being armed with a spring catch and an arresting pin causes a cogged wheel to revolve through the breadth of one tooth for every oscillation of the armature.

The spindle of the cogged wheel carries on its upper end a hand or pointer which revolves upon a dial plate consisting of a number of keys marked with the letters of the alphabet. An insulating brass piece is fixed upon the oscillating lever, which by striking against two opposite flaps of a second metallic lever moves it a little both ways, which movement is limited by two insulated set screws, the object of which is to break and restore the current alternately by establishing a contact between the second lever and one of the set screws.

The return stroke of the armature is effected by means of an

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* Extract from Minutes of the Society of Arts, May 30, 1849.

B 2

adjustable spiral spring. The current is therefore broken and restored by the apparatus itself. The same operation takes place simultaneously in both or all the instruments included in the circuit, and no movement can take place in any unless the current is restored in all of them. By depressing one of the keys before mentioned, an arm, which is fastened to the cogged wheels is arrested, and consequently the movements of all the instruments must cease, their hands pointing, in all of them, to the same letter on the dial plate.

Each instrument contains an alarum, the construction of which is founded on the same principles as the telegraph.

When the telegraph is in repose the coils of the alarum instrument form, with the earth and line wire, one closed circuit. Before a message can be delivered the arm of every commutator must be turned, whereby the sender of the message excludes his own alarum work from the current but includes his battery and telegraph. His own battery then rings the bell at the other stations, whilst the telegraph at his own station remains inactive, because the alarum work is so arranged that it works with greater facility than the telegraph itself. The receiver of the message on hearing the alarum bell turns the arm of his commutator also, whereby his alarum work is excluded from the circuit, but his telegraph and battery are included in it, and the telegraphs begin to work.

Mr. C. W. Siemens, the brother of the inventor, attended and explained the working and peculiarity of the invention.

The thanks of the meeting were given to Mr. Siemens for his communication.

Mr. C. W. Siemens[*] read a paper on an Improved Electric Telegraph, the invention of his brother, Mr. E. W. Siemens of Berlin, at the meeting of May 30th. The paper was illustrated by a series of diagrams, and a pair of the instruments were exhibited at work.

Without reference to figures, it is impossible to describe satis-

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[*] Extract from Notes of Proceedings of the Society of Arts, Session XCV., 1849.

factorily the beautifully contrived mechanism by which Mr. Siemens's telegraphs work, beyond stating that the telegraph is a self-acting machine, breaking and restoring the current itself; and when put in motion, it continues to work until stopped, by preventing at any time the restoration of the current.

At each end of the line is a dial, with the letters of the alphabet arranged round its face, with pointers like the hands of a clock. These hands revolve contemporaneously at each end of the line; and by pressure on a button opposite to any given letter, the hand stops opposite that letter; whilst at the other station, the hand of the instrument there stops in a similar manner at the same letter. The mode by which this is accomplished is extremely ingenious and accurate.

These telegraphs have been in use for upwards of two years with great success on the German lines. One wire only is required, and this, covered with gutta-percha, is buried in the earth. This plan has answered admirably. By means of this telegraph the Government despatches are sent; and the speeches of the German Parliament at Frankfort have been regularly transmitted to Berlin, and printed the following day at Berlin.

In the discussion of the Papers *

"ON THE ELECTRIC TELEGRAPH; ITS HISTORY, THEORY, AND PRACTICAL APPLICATIONS," by C. C. ADLEY; *and*

"ON THE ELECTRIC TELEGRAPH, AND THE PRINCIPAL IMPROVEMENTS IN ITS CONSTRUCTION," by F. R. WINDOW,

MR. C. W. SIEMENS said, that the arrangements of the instruments and wires which had been executed and adopted by his brother, to a large extent, in Germany and in other countries, differed essentially from other systems.

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* **Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XI. Session 1851–1852, pp. 362–366.**

The instruments he used (which were exhibited on the table) were both of the pointing and the printing classes. The former consisted in the outward arrangement of the dial, around which were placed thirty radial keys, each of which bore a letter of the alphabet, or other sign (the letters which most frequently occurred being repeated at opposite points). A hand on the dial revolved continually when the circuit with the battery was completed, but was stopped opposite any key which was depressed by the operator. A similar instrument was placed at every station, the hands of which were compelled to rotate and to stop in concert, by depression of a key at either station.

When the telegraph was not used, the batteries of all the stations were disconnected from the instruments.

The first step then in sending a message was to move a small handle (whereby the battery of the station was put into circuit), the effect of which was to ring a bell at the nearest station. On hearing the alarm, the officer at that station also moved the handle of the instrument before him, and the dial hands of both rotated simultaneously. The message was thereupon transmitted by simply spelling the words by the depression of the keys on the one side, and by reading the letters indicated by momentary stoppages of the pointer on the other. If the officer at the second station received only a sign denoting a succeeding station, he had to move the handle into a third position, whereby he excluded the instrument at his station, and gave the alarm at the following, and so on. When in another position all the instruments along the line were in circuit, and received the message simultaneously.

The instrument was well adapted for railway service, and for intermediate correspondence generally, because it could be worked at first sight by any uninitiated person. It afforded peculiar facilities for communicating either to one or many stations collectively, and it permitted the ringing of bells, and other signals through one and the same wire, without causing confusion in the transmission of messages.

He might mention as an instance the line from Berlin to Hamburg, where one main wire served for the transmission of messages, and for announcing the progress of every train from station to station, by the ringing of large bells, which were stationed at every crossing, and at such distances apart that the one or the other could be heard at every point of the line. By this arrangement collisions of trains were rendered almost impossible.

Another instance worth notice was the town telegraph of Berlin, which combined all the police and fire stations in such a manner that the alarm of fire was spread in an instant through the district ; so that the director might communicate from the central office, at his pleasure, either to one station only or to a whole group collectively ; all this was accomplished by one circular line of wire.

The printing instrument (or secretary) might or might not be joined to the pointer instrument just described. Its function was to print the messages given and received in common type upon strips of paper, thus giving a duplicated record of the communications at both ends of the line, and avoiding all possibility of error.

It would at first sight appear doubtful whether it would be practically possible to ensure the simultaneous movement of the rotating hands, or pointers, besides working alarums, and printing mechanism, all by the same wire and battery. These doubts must, however, disappear on inquiring into the peculiar principle of action which had been adopted. Unlike other telegraphs, where the communicating instrument was worked by hand, or clockwork, or where the receiving instrument was assisted by such, the instruments in question were purely electrical machines, in which reciprocating motion was produced by the independent action of the instrument in alternately breaking and restoring the galvanic circuit. Between the poles of a horse-shoe electro-magnet, an armature was placed transversely upon a spindle parallel to the shanks of the magnet, so that the attraction of the armature by the electro-magnet produced an angular motion of the former. A spring was attached to the armature, which caused it to fall into its distant position whenever the magnetism ceased, by the break of the circuit. The armature carried with it a lever, to the end of which a ratchet was attached, which moved a ratchet-wheel through the breadth of one tooth for every oscillation of the armature. The spindle of this ratchet-wheel carried the hand or pointer on the dial face, and the number of its teeth corresponded with the number of keys around the dial. The working lever carried, also, two insulated projections with which it struck, towards the end of each stroke, against projecting lugs of a lever below, which, by the slight motion imparted to it, alternately broke and restored the circuit, exactly as the valve-lever of a steam-engine alternately admitted and discharged the steam. The circuit of the line wire was not completed, until both or all the communicating instruments had accomplished their return stroke, and, until then, no fresh motion could take place.

In depressing a key of any one instrument, a projecting lever on the ratchet-wheel was stopped opposite the same ; the oscillating lever was thereby arrested about half way in its return stroke, and was, consequently, prevented from re-establishing the circuit. The pointers on all the instruments stopped at that moment opposite the same letter of the alphabet, and could only resume their motion after the depressed key was released. The internal arrangement of the printing instrument was the same as that of the indicating instruments, with this exception, that instead of the pointer upon the rotating spindle, there was a disc of thin steel plate, divided into thirty segments, upon the extremities of which types representing the letters of the alphabet were soldered.

The instrument contained a second electro-magnet, of comparatively large dimensions, through the coils of which a local battery current passed, each time the working lever of the instrument made an oscillation ; but it had no effect, because the duration of each successive current was insufficient to effect its massive armature. On stopping the instrument (by the depression of a key), the local current of the large magnet continued and caused it to attract its armature. This caused a hammer to strike with considerable force under a letter of the type-wheel, corresponding with the letter on the depressed key, which was forced upwards against an inked cylinder and paper through the breadth of one letter by a ratchet motion, thereby fitting it for the reception of another impression. The alarums were worked on a similar principle.

It had been a matter of surprise how it was possible to produce such powerful effects by galvanic action, without the aid of clockwork, &c., seeing that under ordinary circumstances it hardly sufficed to release a detent, or to deflect a needle. The advantage of the principle of self-interception of current was here made apparent. It enabled them to include the batteries at the various stations into the circuit, so that each battery had only the resistance of a section of the line wire to contend with. Moreover, a small portion only of the active current required to pass the wire at all, the greater portion being what was commonly called "bad circuit," or "derived current" (and which was produced artificially).

An important feature in his brother's system was the use of gutta-percha for coating the underground line-wire, which was first suggested by him in the year 1846, and had since been carried out to the extent of upwards of four thousand English miles.

The advantages of the under-ground system were, that it was not affected by atmospheric influences, such as fog, lightning, aurora borealis, or sudden changes of temperature, which frequently broke the suspended wire; it was also expected to be more durable when properly protected; and lastly, it was beyond the reach of mischievous or riotous persons. The gutta-percha coating was attacked by two enemies, atmospheric air, which gradually converted it by a process of oxidation into a hard and brittle mass, through the crevices of which moisture gained access to the wire, and a species of field rats, which gnawed the wire where it obstructed their mining operations. These two causes combined had destroyed the insulation of several of the early lines, particularly in dry sandy embankments. It was therefore found necessary to protect the gutta-percha by an external coating of lead, which was drawn tight over it through a die. The earlier lines were, moreover, laid only 18 inches below the surface of the ground; but it was found necessary to lay them from 2 feet to 2½ feet deep. The weight and cost of the under-ground line wire per English mile was :—

	£	*s.*	*d.*
Copper wire, 80 lbs. at 1*s.*	4	0	0
Gutta-percha, 68 lbs. at 3*s.*	10	4	0
Lead, 600 lbs. at 3*d*	7	10	0
Total . .	£21	14	0

exclusive of workmanship and delivery. The cost of the trench work differed, of course, with the depth and nature of the soil;

the following might, however, be taken as the average cost per mile of a single wire under-ground telegraph :—

	£	s.	d.
The gutta-percha and lead-coated wire . . .	21	14	0
The trench 2 feet deep, including filling up again .	6	0	0
The instruments and sundries	8	6	0
	£36	0	0

or about £30 when the lead coating was dispensed with, and the gutta-percha was increased to about 100 pounds per mile.

In Germany the trench work was generally taken by contract, at 2½ or 3 silver groschen per 12 feet, which was somewhat less than £6 per mile. Recourse had, in some cases, been had to a species of plough, which was propelled at a walking pace along the line by a locomotive engine, and whereby the cost of ground work was much reduced.

It appeared at first to be a serious difficulty to discover the places of rupture, or of bad insulation, in the under-ground line wire; this had, however, been successfully removed by a simple system. To discover a place of rupture between two stations, a battery was inserted at one station, between the line wire and the earth; an officer then proceeded about midway between the stations and connected a galvanometer with the earth on the one hand and the line wire (which was at intervals accessible by being brought up into testing posts) on the other. If he observed a deflection of the galvanometer needle, the rupture could not be between him and the station with the battery. He therefore proceeded to the next post in the direction of the other station, and so on, until he found upon repeatedly attaching the galvanometer that no deflection took place. The rupture must therefore be situated within the distance between the two testing-posts, which distance was therefore halved and quartered by digging holes, and thus obtaining access to the wire. In the course of an hour the position of the rupture was generally ascertained to within a few yards, which were taken up and a fresh piece of wire soldered to the main wire in two places.

The position of a place of bad insulation was discovered by an application of Ohm's law, without proceeding along the line wire.

A delicate galvanometer was placed with its battery at the end stations, in succession, and the degree of deflection observed. The amount of deflection was inversely as the resistance, and the latter was composed of the resistance of the line wire to the place of leakage, the resistance in the imperfect medium of insulation through which the current escaped into the earth, and finally in the resistance of the earth and battery. The two latter resistances would in both cases be the same, and the difference of deflection was, therefore, solely owing to a difference of resistance in the two sections of line wire, which, by Ohm's law, gave a measure of their respective length.

In the discussion of the Paper

"ON SUBMARINE ELECTRIC TELEGRAPHS,"

By F. R. Window,

Mr. C. W. Siemens* observed, that the subject under discussion involved two principal questions, which should be discussed separately, namely, the mechanical one of insulating, shielding, and submerging the metallic conductor, and the electrical question of transmitting messages through it when laid.

The first question had been treated by the author of the paper, and by most of the previous speakers, purely from a historical point of view; and some erroneous statements had been made, which it was important to correct. The non-conducting property of gutta-percha was discovered, in 1846, by Mr. Werner Siemens, of Berlin. Being appointed a member of a Royal Commission, charged with devising a plan for the establishment of electric telegraphs in Prussia, he proposed, in the spring of 1847, the adoption of underground line wires, coated with gutta-percha. In the autumn of the same year, he completed the first experimental line of twenty miles in length, between Gros Beren and Berlin, which was found

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XVI. Session 1856–1857, pp. 218–220.

to work successfully. Encouraged by this success, the Prussian and other German governments adopted the underground system generally, and in the years 1848 and 1849, about three thousand miles were so laid. In March, 1848, Mr. Werner Siemens submerged in the Bay of Kiel, several miles of copper wire, coated with gutta-percha (by means of the cylinder machine, which he had invented in 1847), for the purpose of establishing an electric communication between the shore and several points in the deep channel, where mines had been laid for warlike purposes; and this was undoubtedly the first attempt ever made to establish submarine communications.

In dealing with long underground line wires, Mr. Werner Siemens became acquainted with the lateral induction, or electric charge of the wire; and having fully investigated this interesting phenomenon, he devised means for counteracting its disturbing influence. In a former discussion at the Institution, upon a paper by Mr. Window on Electric Telegraphs,* read in February, 1852, Mr. Siemens fully described the working of the underground electric telegraph,† and the facts disclosed during that discussion tended powerfully to the introduction of that system into this country.

The question of the retardation of electric waves, in passing through long submarine cables, seemed still to be involved in mystery. Professor Thomson, in his paper read at the meeting of the British Association in 1855,‡ enunciated the theory, that the retardation increased in proportion to the square of the length of the cable; whereas Mr. Whitehouse maintained, that the retardation increased only as the length, an opinion which he substantiated by the results of experiments. Now, Mr. Siemens contended, that Mr. Whitehouse's experiments did not disprove Professor Thomson's theory, but rather corroborated it, if all the circumstances of the experiments in question were taken into account. It had been supposed, that the increasing resistance in long conductors might be overcome by proportionately increasing the

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* Vide Minutes of Proceedings of the Institute of Civil Engineers, Vol. XI. pp. 299–329.

† Ibid. pp. 362–366. Vide *ante*, pp. 5–11.

‡ Vide Report of the Twenty-fifth Meeting of the British Association, Transactions of Sections, p. 21.

electro-motive force; but nature had imposed its limits in this direction, for if the force became excessive, the discharge would no longer pass through the length of the cable, and back through the earth, but would cross the insulating medium in the form of a spark, and disable the entire cable. It had also been proposed to send a considerable number of waves of positive and negative electricity simultaneously through the cable, but Mr. Siemens asserted, that the number of waves that could in that way pass through long cables, without destroying each other, was limited to three or four. He did not believe that it would be possible to send through the projected Atlantic cable, more than one word per minute.

Mr. Siemens had carefully investigated this subject, and had, he thought, discovered means of accelerating the passage of an electric wave through a cable to twice its natural velocity, by simply returning the current through a second insulated wire within the cable, instead of through the earth. The two wires being simultaneously charged, the one with positive and the other with negative electricity, completely neutralized the electric charge of the metallic covering of the wire. Other disturbing causes, such as the magnetization of the surrounding iron covering, which exercised a very considerable retarding effect upon the electric wave, would also be removed. The positive and negative waves in the two enclosed wires, would likewise mutually accelerate each other by voltaic induction. In the experiments of Mr. Whitehouse, the line wires had accidentally been under precisely similar circumstances to those provided by Mr. Siemens; but, judging from the projected Atlantic cable, it did not appear that the advantages obtained had been appreciated, as otherwise the cable would have been constructed on totally different principles.

In the discussion of the Papers

"ON SUBMERGING TELEGRAPHIC CABLES," by J. A. LONGRIDGE, M. Inst. C.E., and C. H. BROOKS ; *and*

"ON THE PRACTICAL OPERATIONS CONNECTED WITH PAYING OUT AND REPAIRING SUBMARINE TELEGRAPH CABLES," by F. C. WEBB, Assoc. Inst. C.E. ;

MR. C. W. SIEMENS * said that he had paid considerable attention to this subject. When assisting at the laying of the Mediterranean cable from Cagliari to Bona, his brother, Mr. Werner Siemens, had devised an apparatus similar to that just described, to regulate the strain on the cable, as it was paid out. The results were very favourable. It not only enabled the brakesman to regulate the strain upon the cable with great nicety, by the deflection of the weighted lever, which rested with its pulley upon the cable, between the brake-wheel and the stern-pulley, but it overcame, to a great extent, the bad effects arising from the pitching of the vessel. When the vessel pitched, the weight rose, and allowed more cable to run out, so that the pulleys of the brake travelled at a more uniform velocity.

With reference to the best form for a submarine cable, he considered that there were several questions involved, which required to be balanced against each other. It had been proved satisfactorily, by the mathematical investigations in the first paper, that a cable of light specific gravity was best suited for laying in great depths. But the cable was composed of several materials. There was the conductor, which, when of copper, had a specific gravity of 11 ; the gutta-percha insulator, nearly equal in weight to sea-water ; and the iron external covering, having a specific gravity of 7. Taking two cables of the same specific gravity : one might have little strength in the covering and a large central core ; while the other might have a small core and great strength in the external covering. In analysing what produced weight in the cable, there came first the conductor, which, for electrical reasons,

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XVII. Session 1857–58, pp. 319–321.

ought to be of the best conducting material, such as copper. The conductor constituted the weight to be carried, and should, therefore, be as light as possible, consistent with the highest conducting power; and, to insure its continuity, it should be relieved from strain by the external coating. He thought that the newly-discovered metal, aluminium, might be used, with advantage, in deep-sea cables, as it was nearly equal in conducting power, and was only one-third the weight of copper. If the proposal to substitute an iron conductor should ever be adopted, it would be found that the retardation by lateral induction, which was the great impediment to the successful working of long submarine communications, would be much increased, and would eventually become practically insurmountable. After the conductor was determined upon, there came the consideration of the insulator, the thickness of which ought to be increased with the length of the cable, in order to keep down the retardation by lateral induction. The insulated conductor, if composed of copper and gutta-percha, was always specifically heavier than water,—and it was the outer covering which must give strength to the fabric. If the outer coating was of soft material, such as caoutchouc or gutta-percha, there was no strength to resist the action of the brake, and the coating would be torn away from the wire within it. Therefore the outer coating should be of hard material, and of great strength, so as to resist the longitudinal strains during the process of submerging, but it should add as little as possible to the weight. He thought that no material fulfilled these conditions so well as soft steel. A thin steel wire covering would produce a cable of the least weight, and capable of suspension in the greatest depth. Nor would it be more expensive than the iron coating, if power of suspension was taken as the basis of the calculation.

ON THE PROGRESS OF THE ELECTRIC TELEGRAPH,

By C. W. Siemens,* C.E.

The growing importance of the electric telegraph, both from a scientific and social point of view, and the circumstance of my connection for a good many years with its practical development, are the apologies I have to make for venturing to occupy the attention of the Society this evening.

The object which I have more particularly in view, is to trace the gradual course of progress of this invention since the time of its first appearance upon the stage, without pretending indeed to establish any new historical facts or to decide upon the relative merits of contending claimants to invention or discovery (although I shall not willingly offend against the right of any one), but with a view to establish more clearly our present position in the scale of progression, and to point out with some degree of certainty the direction in which we should travel in order to realise still greater results, particularly the accomplishment of trans-oceanic communication.

When, little more than a century ago, Franklin, the father of electrical science, ascertained that atmospheric electricity, which manifested itself in the imposing form of thunder and lightning, was identical with frictional electricity, he employed an apparatus comprising an insulated metallic conductor, the electric machine, the earth return circuit, and a receiving instrument, consisting of a pair of cork balls, suspended by silk threads, which, upon being electrified, struck against a pair of signal bells. This apparatus comprised indeed all the elements required for the construction of a modern electric telegraph. Nor was the idea of an electric telegraph new, even in the days of Franklin, for we are informed that as early as the year 1728, a pensioner of the Charter House, named Stephen Gray, made electrical signals through a suspended wire, 765 feet long. Yet a century of unceasing efforts, by men of all civilised nations, including some of the

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* Excerpt Journal of the Society of Arts, Vol. VI. 1857–58, pp. 348–358.

greatest natural philosophers the world ever produced, was required to reduce those elements into available forms for practical purposes.

If we pass over the experiments by Winkler, of Leipzig, in 1746, Watson of London, and Le Monier of Paris, in the year following, as preliminary enquiries into the velocity of the electric current in metallic conductors, we find that the honour of having produced the first electric telegraph is due to Le Sage, of Geneva, who actually constructed in 1774, an experimental line of communication, consisting of 24 suspended line wires, representing the 24 letters of the alphabet respectively. Each wire terminated in a pith ball electrometer, the balls of which separated, upon the wire in question being charged at the other extremity by means of a Leyden jar, denoting the letter intended to be communicated. Lomond of France, perceiving the difficulty and expense attending so many line wires, contrived in 1787, (see Young's Travels in France, 1787) an experimental line of telegraph in his house, consisting of only one line wire connected with a pith ball electrometer at both ends, and he proposed a telegraphic code by repetitions of his only primitive signals. Reisèr, Dr. Salva of Madrid, and many others proposed various modifications of the same apparatus, but it is hardly necessary to add that all of them remained unrewarded by success.

In consequence of so many fruitless attempts, electric telegraphs were already being classed among the chimerical projects of the time, when at the dawn of the present century a new field for invention was opened by the important discoveries of the Italian philosophers, Galvani and Volta.

The voltaic current, unlike the spontaneous discharge of static electricity, could be conducted with comparative facility through long metallic conductors, and was capable of very powerful effects in decomposing water or other substances, which qualities rendered it clearly preferable for telegraphic purposes.

Struck by these views, Soemmering of Munich, constructed in 1808, the first voltaic telegraph consisting of 35 line wires, any two of which could be combined to form the electric circuit and produce a signal at the other extremity by decomposition of water, under any two of 35 inverted glass cups, arranged side by side in an oblong bath of acidulated water. The 35 wires terminated in gold points, under the inverted glass cups (or voltameters) and the rising of the gases of decomposition betrayed to the attentive observer the passage of the current.

The difficulty of dealing with so many wires suggested to the mind of Schweigger the same expedient which Lomond had recourse to with regard to static electricity, that of reducing the number of line wires to a single metallic circuit, and the receiving instrument to a single decomposing cell, having recourse to repetition, and to differences in the duration of succeeding currents, in arranging his telegraphic code.

It seems not improbable that if electrical science had made no further advances, the projects of Soemmering and Schweigger would have gradually expanded into practically working chemical electric telegraphs, such as have been proposed at a much later period by E. Davy, 1838, Morse, 1838, Bain, 1843, and Bakewell in 1848, which last is particularly interesting inasmuch as not mere signals or conventional marks are received by it, but a facsimile of the message, previously written with a solution of shellac upon a metallic surface.

The discovery of Oersted in 1821, which under the hands of Schweigger, Ampère, Arago and Sturgeon, soon expanded into electro-magnetism, turned the tide of invention into quite another direction. Ampère was the first to propose an electro-magnetic needle telegraph consisting of 24 needles, representing each a letter of the alphabet, and 25 line wires, the extra line wire being intended for the metallic return circuit common to all. Ritchie executed, in 1832, a model of Ampère's telegraph, with an essential improvement, to the effect that each needle, by its motion, moved a screen disclosing a letter of the alphabet.

Another version of the same general arrangement was patented by Alexander of Edinburgh, as late as 1837, Fechner of Leipzig, and Schilling von Canstadt of Russia, proposed, in 1832, apparently independently of each other, a single-needle telegraph with deflection of the needle to the right and left; and Fechner was the first to prove, by calculation, the power of the galvanic current to traverse a great length of line wire.

Gauss and Weber, of Goettingen, took up the subject of electric telegraphs at about the same time, but had not proceeded far when their attention was diverted by the great crowning discovery of electrical science, I mean the discovery of induction and of magneto-electric currents by Faraday in 1831.

Gauss and Weber rightly judged the superiority of magneto-electric over voltaic currents for telegraphic purposes, and in applying them they effectually established the first working electric telegraph in 1833, with the arrangements of which I became practically acquainted some years later, when a student at Goettingen.

It consisted of a line wire and return current wire, the former of which was carried upon high posts over the town of Goettingen, extending from the Observatory to the tower of the Public Library, and thence to the new magnetic observatory of Weber, a distance of little more than an English mile. The magneto-electric current was produced by means of a coil containing 3,500 turns, which was situated upon a compound bar magnet, weighing 75 lbs., the coil being at liberty to slide freely to and fro upon the bar. In sliding the coil rapidly from the centre toward the south pole of the magnet and back again, a succession of two opposite currents was produced, which, traversing the line wire circuit, including coils of the receiving instrument, caused a short jerk of the needle, say to the right and back again, whereas the deflection of the needle would be to the left when the exciting coil was moved towards the north pole and back. The amount of motion imparted to the coil determined also the amount of deflection of the needle, and could, by means of a telescope and a scale, be read off in degrees on a reflector attached to the end of the needle. The needle itself weighed 100 lbs., and was suspended from the ceiling of the room by untwisted silk. Notwithstanding the extraordinary weight of the needle (which was the same as used by Gauss to determine the laws of terrestrial magnetism), its motions were beautifully energetic and distinct when viewed through the telescope. Gauss and Weber did not pretend, however, to the construction of a commercially useful electric telegraph, but delegated that task to Steinheil of Munich, who enjoyed already at that time a reputation as a skilled mechanic. Steinheil applied himself vigorously to the task, and produced, in 1837, his needle, printing, and acoustic instruments, which he first tried at Munich through about 5 miles of suspended line wire and shortly afterwards upon the Taunus railway, near Frankfort. In trying whether the rails might not be used as metallic conductors, he rediscovered the conducting power of the earth itself, which, it appears, had been lost sight of since it had first been discovered by Franklin with regard to static electricity, and proved also with regard to voltaic electricity, in 1803, by Erman, Basse, and Aldini.

The first recording instrument, and the telegraphic earth circuit, are discoveries which entitle Steinheil to a high position among the originators of the electric telegraph, although the means he proposed for its execution were too refined for the time, and did not lead on that account to immediate practical results.

At the time when Steinheil was absorbed in his labour, Professor Wheatstone was also engaged upon a series of experiments on the velocity of electricity, with a view to the construction of electric telegraphs, and in June, 1837, he joined Mr. Cooke in a patent for a needle telegraph of five line wires (besides one wire for the return current), and as many needles, which, by an ingenious system of permutations, could be so deflected that any letter of the alphabet was pointed out upon a diamond-shaped board by the convergence of two needles towards it. The line wires were proposed to be coated with insulating material, such as fibrous substances saturated with pitch, and to be drawn into leaden pipes, in order to exclude the moisture of the ground into which they were intended to be laid. An experimental line of telegraph on this principle was established in the same year at the Euston Railway Station, and the results obtained left, it appears from documentary evidence, no doubt upon the mind of the then resident engineer of the London and Birmingham Company, the present Sir Charles Fox, of its ultimate success. That success, however, was not obtained without a struggle against practical difficulties, in the course of which the system underwent important modifications, of which the double needle instrument, such as is still used extensively in this country, and (in 1843) a return to overground line wires, were the results.

To Cooke and Wheatstone is due the credit of having established the first commercially useful lines of electric telegraph, namely, the lines between Paddington and Drayton, commenced in 1838, and between London and Blackwall, commenced in December, 1839, which were soon followed by others.

If viewed from our present position, the needle telegraph cannot be considered an advance, in point of principle, on Gauss and Weber, or Steinheil; it involved in fact a return from magneto-electric to voltaic currents—from a single line wire to several—and from recording of messages to their mere indication; yet, for the time being, when insulation was imperfect, and the important law of Ohm was hardly understood, except by a few natural philosophers, it had the probability of success in its favour, because the duty required from the electric current consisted in deflecting a magnetic needle to a merely appreciable extent, and it was of no great importance to the result whether a more or less considerable proportion of the current was lost through imperfect insulation. The upright weighted needle—the key with dry metallic contacts—and other details, were also of a novel and meritorious character. Why the same system should however be still persisted in at the present day, in this country, when improved systems have been adopted in nearly all other countries, including the British possessions, is a question which, I hope, will receive an answer from those who practically uphold it. It is evident, however, that Wheatstone did not intend to stop there, from his numerous other inventions, which followed each other in rapid succession, and among which his dial and printing instruments—his early applications of magneto-electric currents—the relay—and the first judicious application of electro-magnets, so as to obtain more powerful effects at distant stations, are the most remarkable.

The country of Franklin has not been behindhand in gathering the first-fruits of electrical science. It is said that Morse contemplated the construction of an electric telegraph since the year 1832, although he did not take any overt step till the year 1837, when he lodged a *caveat* in the American Patent Office, which patent was not enrolled till the year 1840. There is no evidence to show that Morse's early ideas had assumed any definite shape until the year 1838, when he deposited an instrument of his construction at the Paris Academy of Sciences. Morse's invention consists chiefly in the substitution of electro-magnets for needles in the construction of a recording instrument, which, in other respects, is similar to Steinheil's. The step was, however, an important one to render the instrument powerful and certain in its action, and, combined with Wheatstone's relay, Morse's recording instrument will, it may be safely affirmed, be used universally for all except local telegraphic communication.

In the year 1845, when the practical utility of electric telegraphs had been demonstrated in England, several continental governments determined upon their establishment. The Belgian, Austrian, and, a few years later, the Sardinian Government, simply adopted the double-needle telegraph. In France, Messrs. De Foy and Breguets fils, contrived a double step-by-step or dial telegraph on Wheatstone's principle, which enabled them to imitate the same code of signals which had been used for the Semaphore telegraph.

In Prussia, a royal commission was appointed to consider and advise upon the system to be adopted, of which commission my brother, Werner Siemens, who had been engaged before with kindred subjects, became the most active member. The commission was in favour of an underground system, and charged Werner Siemens to institute experiments. About this time gutta-percha had become known in this country, and having been struck with its peculiar plasticity, I forwarded my brother a sample, to see whether he could use it for the purpose he had in view. He soon discovered its remarkable insulating properties, and recommended an experiment on a large scale, which, having been sanctioned, he completed a line of from four to five English miles (between Berlin and Grossbeeren) successfully in the summer of 1847. The machine he designed for covering the copper wire with gutta-percha is nearly identical with the cylinder machine still used for the same purpose. In the spring of 1848 a considerable length of gutta-percha coated copper wire was submerged in the harbour of Kiel for military purposes, but it was found that, owing probably to the impurity of the material, the gutta-percha underwent a gradual change, as though it was penetrated by sea-water, to counteract which Werner Siemens proposed, with apparent effect, to mix a small proportion of sulphur with that substance. In the same and following year more than a thousand miles of gutta-percha coated line wire was laid down underground, and proved successful for several years, when it began to fail, for the most part, in consequence of the impure and adulterated condition of the material then supplied. Although the underground line wire has, for the most part, been superseded again by the suspended wire, I venture to assert that we shall eventually return to it for all principal lines, for reasons which I shall enumerate hereafter. The experience gained in this great experiment has been most valuable in paving the way to submarine cables, which, at the present time, occupy so large a share of public attention.

The instruments which Werner Siemens at first proposed, and which are still used extensively on the continent for railway purposes and town service, were dial instruments, involving a peculiar principle, inasmuch as no communicating instrument or any clockwork is employed, but the two or more instruments, connected by the single line wire, break and restore the electric circuit by the action of their own armatures, in a similar way to a steam-engine, which alternately intercepts and restores the communication with the boiler. In arresting the ratchet-wheel of any one of the instruments within the circuit, by depression of a key, bearing a certain letter of the alphabet, the armature of the instrument in question is prevented from restoring the electric circuit, and the hands upon the dials of all the instruments in circuit must stop, pointing all of them to the same letter, until the depressed key is again released. The advantages of this arrangement over previous dial instruments are that the communicating instruments are less liable to fall out of step, and that considerable power of action is obtained, because the batteries of all the intermediate and end stations act in concert, being all included in the general circuit. The dial instrument is in some instances accompanied by a type printing instrument, differing from Wheatstone's and House's arrangements, inasmuch as it is entirely self-acting, the motion of the type wheel, of the paper, and even of the hammer striking the blow upon the type, being effected by electro-magnets instead of clockwork, or an air cylinder, as is the case in House and Brett's arrangement.

Since the time of the first successful introduction of the electric telegraph, a great variety of instruments, insulators, and other appliances, have been proposed, amongst which the chemical recording instruments of Bain and Bakewell, the modifications of Wheatstone's magneto-electric needle, and dial instruments by Henley and Stœhrer, the various combinations of Messrs. Highton, Clark, and Bright, and the more recent productions of Mr. Varley and Mr. Whitehouse, are of undoubted merit in having contributed to the general progress of electric telegraph engineering.

To describe them here would be a task far exceeding the limits of this paper, and I shall proceed at once to point out what, in my opinion, at least, supported by actual experience, are the best means to be adopted, at the present time, for extending the electric telegraph, both on land and across the seas.

The foregoing sketch of the gradual development of the electric telegraph, may serve to show that the particular arrangements adopted to indicate or register the message, or the particular combination of elementary signs, is of secondary importance, but that every essential progress is marked by the discovery of some new means of generating currents of greater dynamic power, or of producing by their means more decided effects at the further extremity of the conductor.

Let us inquire, then, what are the conditions of current generator, current conductor and receiver, best calculated to realise a maximum of palpable effect at great distances.

Inquiry into these questions is of particular interest at the present time, when great efforts are being made to extend telegraphic communication across the Atlantic and Indian oceans, distances far exceeding the length of any land lines yet constructed.

Among the different varieties of electricity hitherto applied to telegraphic purposes, that produced by friction possesses the greatest tension or power to overcome resistance in the conductor. But its discharge is instantaneous, and it is, therefore, ill-suited to produce dynamic effects with time or duration for a factor.

The voltaic current, on the contrary, may be considered as absolutely continuous, and, therefore, as best suited to produce powerful effects, but it is deficient in tension, unless a great number of elements are employed, in which case it becomes expensive and troublesome. A battery of sufficient intensity to convey an effect through the Atlantic cable would have to be composed of at least 500 Daniell's cells, according to ordinary practice, but I apprehend that the internal resistance of such a battery would of itself annihilate its presumed power, and that practically no battery of sufficient power could be thus constructed.

The magneto-electric currents hold an intermediate position between the two just referred to. Their intensity can be increased almost indefinitely, and they are of perceptible duration (the time required to charge an electro-magnet). They may be produced by mechanical agency, on separating a permanent magnet from its armature or surrounding coils, or by means of a voltaic quantity battery and primary coils ; and are, in both instances, by far the cheapest and least variable description of electric currents. The reason why, since the discovery of magneto-electricity in 1831, it has again and again been abandoned in favour of battery currents, may be traced to the imperfect means hitherto known or adopted for its generation or suitable application ; but I hope to prove hereafter that it can be employed at present with perfect success.

Regarding the electric conductor or line-wire, this is either suspended upon poles in the open air, or it is imbedded in gutta-percha, and interred or submerged. Suspended line-wire generally consists of galvanised or painted iron, of from one-eighth to one-fifth of an inch in diameter, and supported, at intervals of from 50 to 60 yards, from posts by means of insulators. The construction of a really efficient insulator has for many years occupied the serious attention of electrical engineers, for upon it chiefly depends the permanent efficiency of the line. A great variety of insulators have been tried, some of which I am enabled, by the kindness of the Electric Telegraph Company, to present to the meeting. According to continental experience, the insulator of Siemens and Halske has been found to combine the desiderata of strength and insulating property in the highest degree. It consists of a cast-iron bracket, assuming the form of an inverted bell, with a cylindrical recess at the bottom. A capsule of porcelain is firmly cemented, by means of sulphur mixed with caput mortuum, into the recess, and into this again a stalk of iron is cemented, which forming a peculiar twisted loop at the end, supports and secures the line-wire. The insulating property depends upon the dryness of an apron-like extension of the porcelain capsule, which, under the protection of the cast-iron bell, is not affected by either rain or dew. Every tenth support is a stretching-post insulator, at which the line-wire is not only supported but held firmly by means of claws, an arrangement which has been found very convenient during the erection of the line-wire, and in case of repairs. An idea of the importance of a good insulator may be formed from the fact, that the cost of finding and repairing a single defect of the line-wire, in a country like Russia, amounts on the average to £30.

We now approach the subject of submerged conductors, which at the present time engrosses the attention of electrical engineers, and also commands a large share of public interest, owing both to the difficulties with which it is surrounded, and the vast importance of the object in view.

Regarding the history of submarine cables, it appears that the first experiments, on a small scale, to submerge an insulated conductor (copper wire coated with cotton thread saturated with pitch and tar) were made at Calcutta, in 1839, by Dr. (now Sir) William O'Shaughnessy.

Professor Wheatstone proposed in the following year to establish a cable between England and France, and prepared very elaborate and well considered plans, which, by his kindness, I am enabled to place before the meeting. The cable Wheatstone proposed contained six separately insulated copper wires which were protected by a strong sheathing of iron, differing, however, from the sheathing now adopted, in being devoid of strength in a longitudinal direction.

Submarine telegraphs must, however, have proved impracticable but for the timely discovery of gutta-percha, and its remarkable insulating properties. It is, therefore, not surprising that the first successful attempts to establish subaqueous conductors were made by Werner Siemens in 1848, in the bay of Kiel, and in crossing the Rhine at Cologne, and other rivers.

The gutta-percha coated copper wire was at first submerged without outer protection, but it was laid by the side of a strong chain to protect it from anchors. In the following year, however, a lead coating was introduced.

The first attempt to establish a subaqueous conductor across the open sea (from Dover to Calais) was made by Wollastone, in 1850. It consisted of a gutta-percha coated copper wire, without external protection, and failed immediately after it had been laid. In the following year Crampton laid a cable between the same places successfully. This cable was sheathed with iron wire, according to Messrs. Newall and Co.'s patent process, which gives great longitudinal strength, and has been generally adopted ever since, except in the instance of the Varna-Balaclava cable (laid by Messrs. Newall and Co. in 1854), which had no sheathing, excepting at the shore ends, and which worked successfully till just before the evacuation of the Crimea by the Allies.

It would be tedious to notice the various successful and unsuccessful attempts which have been made since the year 1837 to establish submarine cables, suffice it to state the general results of the experience obtained, which goes to prove that the difficulty of submerging and working submarine cables is small in shallow and narrow waters, but increases in rapid ratio with the depth and breadth of the ocean to be traversed.

An inquiry into this most interesting subject may be divided into three sufficiently distinct heads, namely, the mechanical problem of constructing and submerging the cable; the electrical condition of the submerged cable; and, lastly, the question of suitable instruments.

The mechanical problem has been discussed lately at great length at the Institution of Civil Engineers, I therefore propose to limit myself to a recital of the principal points of interest which may be considered as established both by theory and in practice.

The cable should be of small specific weight and of great tensile strength, in order that its descent through the water may be retarded by the resisting medium to such a degree that the velocity of maximum acceleration may not exceed one-fourth, or at most one-third, of the velocity of the vessel. This condition of a "balanced cable" being fulfilled, there remains the tendency of the cable to slide down the inclined trough of the water, and it has been proved that this force equals, under all circumstances, the weight of a length of cable (less the weight of water it displaces), reaching from the vessel perpendicularly to the bottom of the sea. The same amount of retarding force must at least be applied to the paying-out brake to prevent great waste of cable, and the cable itself must of course be sufficiently strong to bear this strain without injury to the insulated wire or wires.

Messrs. Longridge and Brooks have been the first to prove, I believe, that currents in the ocean cannot sensibly augment the strain upon a descending cable, nor are they likely to occasion considerable loss.

It has been proposed to increase the floating power of deep-sea cables, by attaching floats at intervals; but it appears to me that such appliances, which depend upon the unerring dexterity of workmen at the moment of danger, and which, moreover, do not relieve the cable from retarding strain at the brake, should be discarded, and the cable be made to possess in itself all the requisite degree of buoyancy and strength. For this purpose the conducting wire or wires should be as light as possible consistent with good conducting power, a combination of properties which seems to point to the newly discovered metal, aluminium, as likely ultimately to supersede copper. The insulating covering of gutta-percha increases the bulk without adding to the weight of the cable, being nearly of the same specific gravity as sea-water, it improves both the mechanical and electrical properties of the cable, and the only limit to its desirable thickness is its expense. The principal weight, and all the available strength of the cable reside in its sheathing, which should be made of a material combining strength with lightness, and also with hardness, to resist the crushing and tearing action of the brake wheel ; and there can be no doubt that steel wire combines these qualities in the highest degree, nor do I think it would be much dearer than iron if power of suspension was taken for the basis of calculation.

It can easily be shown, by the simple rule given above, regarding the strain upon the cable in leaving the vessel, that an iron-sheathed cable cannot, under the most favourable circumstances, be laid in water of more than three miles in depth, without a certainty of rupture taking place, whereas a steel covered cable might be laid with reasonable safety to a depth of five or six miles, which depth is, I believe, rarely exceeded in any ocean.

Respecting the paying-out machinery, I have to notice Messrs. Newall and Co.'s apparatus, consisting of a solid centre, and heavy rings to form a double cone for guiding the cable safely out of the hold, and the brake, which latter should be made as light as possible, to avoid jerks upon the cable, and should indicate the variable strain put upon it, to harmonise its speed with that of the vessel.

In order to ensure continuity of the electric conductor in a cable, a strand of several copper wires is now generally adopted, instead of a single wire, which latter is found to be very liable to break. This simple but useful plan was, I believe, first thought of and acted upon by myself, having ordered some gutta-percha coated strand, for experiment, from the Gutta-percha Company in the spring of 1855, part of which I have laid upon the table.

The electrical condition of the submerged conductor is a subject of the greatest interest, upon which electricians are still divided, and, treated mathematically, involves problems of the highest order, such as only Professor William Thomson and a few others can hope to deal with effectually. The important point is, however, to arrive, first of all, at a clear understanding of the laws of nature upon which those calculations should be based, and those laws when rightly interpreted, are always extremely simple.

The submerged (or underground) line wire may in the first place be considered in the light of a mere conductor, following Ohm's law, which as is well known, is to the effect that the amount of electricity passing in a given time depends upon the sectional area of the conductor, upon the electric force (intensity) of the battery, upon the specific conducting power of the material, and inversely upon the length of the conductor. It is expressed by the following formula:

$P = \frac{E\,a\,c}{l}$, in which P signifies the quantity of electricity passing; E, the electric force of the battery, or its substitute; a, the sectional area of the conductor; c, the specific conducting power; and l, the length of the conductor.

In the next place the cable has to be considered in the light of a Leyden jar of extraordinary length, formed of gutta-percha, with the conductor for an inner, and the sheathing (or moisture) for an outer metallic coating. This Leyden arrangement has to be charged to a certain degree before the electric current can make itself felt at the further extremity, but the supply of electricity being limited at every point by the resistance offered by the conductor, according to Ohm's law, it follows that the entire cable can be charged only in a progressive manner, as though it consisted of a series of Leyden jars charging the one into the other until it reaches the last, which discharges itself through the receiving instrument into the earth. The amount of impediment thus offered to the progress of the electric current depends evidently upon the capacity of the Leyden arrangement, which capacity should be reduced to a minimum for a given size of conductor.

According to Faraday's definition of dialectrics, the electric charge obeys the same simple law, which regulates the dispersion of heat in an imperfect conductor, and which, again, is analogous to Ohm's law regarding electric currents. It follows that the electric charge of a Leyden arrangement is directly proportionate to the lining surfaces—directly to the electric force of the battery (or its substitute) employed, and to the specific inductive capacity of the insulating medium, but inversely proportionate to the thickness of insulating coating, or if expressed by a formula, we have :—

$Q = \frac{E\,S\,k}{d}$ in which Q, expresses the electric charge ; E, the electric force of the battery; S, the metallic surface ; k, the specific inductive capacity ; and d, the thickness of the coating.

This formula is corroborated by a series of very careful experiments by Werner Siemens upon electric cables, and it is of great practical utility if combined with Ohm's formula regarding the conductor.

The following are some of the simple consequences derived from the two formulæ :—

1. The electric force (E) of the battery (and its substitute) has no influence upon the onward velocity of the electric wave, because it increases the value of P and Q equally.

2. The time $\left(t = \frac{Q}{P}\right)$ required to charge a submerged conductor of a given proportion increases in the square ratio of the length (l) of the conductor (in the formula for Q, the factor (S) has to be expressed by l and a) which law was first arrived at by William Thomson in another way, and was communicated by him to the British Association in 1855, but has since been assailed by Whitehouse and other electricians.

3. It is of the first importance to make the conductor of the best conducting material, and the insulating coating of the greatest practical thickness, but of a material with the least specific conductive capacity.

4. Given the materials and the thickness of the insulating coating, the rapidity of progress of the electric wave increases in the simple ratio of the diameter of the conductor ; a proposition differing also from the views of the promoters of the Atlantic

cable, who assert that the maximum result is obtained by a conductor of comparatively small diameter.

The results obtained by means of these formulæ are, however, modified by disturbing causes, which have to be taken into account by the electrical engineer. Among these, the conducting power of the gutta-percha itself is the most important. It appears, from certain experiments made at Birkenhead by Messrs. Newall and Co. upon one-half of the Atlantic cable, that when the entire cable is formed into an electric circuit only about one-third part of the current will follow the wire throughout its length, and the remaining two-thirds will pass through the gutta-percha covering to the earth. The relative amount of leakage through the covering increases in an extraordinarily rapid ratio with an increase of temperature; and it must be deemed a most fortunate circumstance that the temperature of the great oceans is probably not above 40° Fahr. at the bottom, being the temperature of maximum density of water. Messrs. Buff and Beetz have found that glass also becomes conductive of electricity, when but moderately heated; and they attribute the effect to electrolysis, or decomposition of the alkali it contains. In the case of gutta-percha, it arises possibly from the decomposition of water of hydration or of some vegetable constituent of that substance. A careful experimental inquiry into this question, including some other deteriorating effects upon gutta-percha, would be of great practical importance; and it is to be hoped that the Gutta-percha Committee, lately appointed by this Society, will furnish some valuable information.

The effect of leakage through the coating is retardation, in the direct proportion of the surface of the conductor, and the inverse ratio of the thickness of the coating; but the coefficient varies according to the temperature, and quality of the material. There are some other disturbing causes, of comparatively less importance, namely, voltaic induction and magnetisation of the iron sheathing by the line-wire current. The voltaic induction, or tendency of one current to produce a current in the opposite direction in another conductor parallel to itself, is of importance only in the case of compound cables, and may even be turned to advantage if the return current is laid through one of the parallel wires instead of the earth. By the same expedient, magnetisation of the sheathing, which is necessarily a retarding cause, and is, moreover, productive of a disturbing extra current, may be neutralised.

In calculating the time required for an electric current to traverse a cable of given length and proportion, it may be received as an experimental datum to start from, that it reached the distance of 1,000 miles in one second, in a cable consisting of No. 16 copper wire coated with gutta-percha to the thickness of $\frac{5}{16}$ths of its diameter, a proportion most generally adopted. The discharge of the same cable would occupy practically about two seconds, and these times go on increasing in the ratio of the square of the length of the conductor, in as far as the retardation by electric charge is concerned, and in the simple proportion of losses by leakage, voltaic induction, and magnetisation, the result being a mean between the two ratios.

With these facts before us, it would have been impossible to work an electric telegraph across the Atlantic or Indian oceans, with anything approaching a commercial result, and the idea must have been abandoned, but for Faraday's timely discovery that several electric waves may co-exist, following each other in a long cable, whereby the number of impulses to be transmitted in a given time may be greatly increased.

A difficulty experienced in carrying this method of working into effect, is the partial merging of the separate waves into an almost uniform electric charge of the conductor, which causes the receiving instrument to be permanently affected. This difficulty, has, however, been removed by a return to Gauss and Weber's method of working, in sending always two opposite currents in succession, whereby not only the effective value of each wave is doubled, but accumulation of electric charge is entirely prevented, because the two opposite waves, in emerging, destroy each other. This method of working would, however, not be complete without a return also to the same description of current which Gauss and Weber employed. It has, indeed, been shown above, that currents of high electric force do not travel any faster through submerged conductors than feeble currents, but the advantages of the former are that each electric wave represents a larger accumulation of force, and travels consequently to a greater distance before it has so far dispersed as to be no longer capable of producing an effect upon the receiving instrument, and moreover, that the positive and negative impulses are equal in amount.

The success of a long submarine line of electric telegraph depends also in a great measure upon the particular construction of both the communicating and receiving instruments. On this point I am in a position to speak from extensive experience, being connected with an establishment which had to contend at an early period with the difficulties experienced upon long underground lines, which has since carried out extensive systems of telegraphs in Russia and other countries, and has furnished the instruments of most of the continental lines, including those in Turkey, India, and Australia. In addition to this there is the experience of the Black Sea and the Mediterranean lines, which are the longest submarine lines hitherto constructed, with the instrumentation of which I was charged by Messrs. Newall & Co., the successful contractors of those undertakings.

Morse's recording instrument combines, as stated before, many practical advantages which recommend it for universal adoption for all mercantile lines, among which advantages is the facility it offers of forwarding messages at intermediate stations without the intervention of a clerk, in putting on a fresh battery, a system first introduced by Siemens and Halske, and perfected by Steinheil, by which it is made to speak directly between London and the remote parts of Russia.

The real telegraphic receiving instrument is the relay, which has for its duty to establish and break the local circuit of the recording instrument.

An important point in the construction of a delicate relay was the suppression of the armature of the electro-magnet employed (patented by Werner Siemens in 1851) by allowing one of the two upright bars of soft iron composing the horse-shoe electro-magnet to vibrate upon delicate points, and producing rotary motion by the attraction between approximated horizontal arms extending from the same. The application of magneto-electric currents necessitated a corresponding change in this relay; for, however sensitive it might be made, it was necessary that the effect of the line-wire current should be continued till the recording instrument has had time to make a dot or line upon the paper, and the magneto-electric current, being nearly instantaneous, is unsuited for that purpose. This difficulty has been removed by the introduction of permanent magnets, which continue the effect produced by the instantaneous action of the line-wire current, until the opposite effect is produced by the succeeding negative current. The vibrating tongue of the instrument is for this purpose balanced midway between the similar poles of a comparatively powerful permanent magnet, being equally attracted by both but remaining in the proximity of either of them, into the attractive sphere of which it happens to be brought by the instantaneous action of the line-wire current, changing for an instant of time the name of one of the contending poles. A relay on this principle was first exhibited at the Great Exhibition of 1851 by Siemens and Halske.

The relative dimensions of the inductive coils, and of the coils in the relay, (depending upon the length and other conditions of the cable itself) are points which require very careful attention. The common practical rule, that the resistance of the coils must be increased with the increased length of the conductor, is here entirely at fault, for the electric wave, when once formed, is no longer under the influence of its source, but may be compared to the dying wave of the ocean running up a shallow beach, which would have no power to force its way through a long and narrow tube, but is yet capable of delivering a large quantity of water into an open duct. For an analogous reason the coils of the relay must be composed of comparatively short and thick wire. The same rule applies to the inductive coils, which must be composed of thick wire in order to produce a quantitative wave. The Cagliari, Malta, and Corfu line is worked by instruments upon this principle, and the results obtained are very satisfactory, the messages being worked through the entire distance of 700 nautical miles (without making Malta a relay station) with ease, and at a sufficient rate.

This result proves that telegraphic cables not exceeding a thousand miles in length may be worked satisfactorily, and that, consequently, all reasonable doubts may be considered as being removed about the successful operation of a line from London to Calcutta, a result which I sincerely hope to see soon established in fact.

For distances exceeding a thousand miles, the difficulty of sending messages at an efficient rate for commercial purposes remains yet to be solved, for theory and experience combine to prove that the highest rate likely to be attained in working through a distance equal to the intended Atlantic cable, in taking full advantage of the power of waves, will not exceed three, or it may be four, words per minute, unless indeed some new principle of working is yet discovered, whereby a greater result is realised.

There would be one way, indeed, in which the capabilities, not only of long submarine cables, but of electric telegraphs generally, might be greatly increased, which consists in combining a number of insulated line wires into one cable, and working them in metallic couples. This, indeed, is giving up the earth circuit, but, in its stead, we gain the power of several sets of instruments without disturbing interference between the wires by Voltaic induction. Instead of using one of the wires (say the central wire) for the common return circuit, the metallic circuits might be selected by the rule of permutations, which, if carried out, would enable us to connect 6 pairs of instruments by means of 4 wires, 10 pairs by means of 5 wires, and so on. If a cable of 10 wires was laid between two great commercial centres, say between London and Liverpool, as many as 42 pairs of instruments might be used, which might be placed in the counting-houses of great merchants and of their respective agents for their private correspondence, and this step would probably give rise to the more general application of the electric telegraph for private and domestic communication. The instrument that appears to be best suited for such purposes (including railway and town services) is a magneto-electric step-by-step or dial instrument, a specimen of which I have placed before the meeting. This instrument combines the advantages of requiring no battery, with great facility of working, and it contains some novel arrangements, whereby its action is rendered powerful and certain.

Of these instruments, 180 were adopted last year by the Bavarian Government, in lieu of instruments of a similar class that had been used there previously, and it appears, from an official document, that they give great satisfaction. A pair of them is also in use at the War Office and the Horse Guards ; and another pair was taken out by Messrs. Newall & Co., to keep up telegraphic communication between the tender and tug employed in laying the last Mediterranean cables.

My summary of telegraphic novelties would not be complete without a notice of a method of sending messages simultaneously in both directions through one and the same line wire, the joint invention of the Hanoverian telegraph engineer Frischen and my brother. It consists in splitting the current of the battery into two equal parts, of which the one proceeds through the line and the other through an adjustable resistance coil by a short circuit to the earth. Both currents pass in opposite directions round the relay magnet of the communicating station, and neutralise each other in effect, but the portion of current passing along the line wire, produces an effect upon the relay at the receiving station, and *vice versâ,* but if both stations include their batteries at the same time, the current of the line wire will be doubled, and in exercising a preponderating effect upon both relay magnets, will cause both to attract their respective armatures, and establish the printing circuits. By this means, the transmitting power of a single line wire is doubled. This system works satisfactorily between Amsterdam and Rotterdam, and some other places where there is not much interference by intermediate service; but it is, I consider, as yet too refined for general application. The same objection applies to a system of accelerating the speed of transmission of messages by preparing strips of perforated paper which, in passing between a metallic roller and contact finger, break and restore the metallic current with unlimited rapidity,—a system first introduced by Bain years ago. These plans will probably be of great practical utility eventually, when the use of the electric telegraph is more extended.

In conclusion, I have to thank the meeting for their patience in listening to this paper, which far exceeds the limits I had assigned to it. I have to express my special thanks to Professor Wheatstone, Mr. Latimer Clark, Dr. Green, Mr. Edward Bright, and Messrs. Newall & Co., for their liberal aid in furnishing me with models to illustrate the subject.

I wish to draw particular attention to the key and relay arrangements of Mr. C. Varley, used upon the Dutch cable, and the acoustic telegraph, worked by secondary circuit, used by the British Magnetic Telegraph Company, which lack of space has prevented me from describing in the paper. The paper is, I am aware, deficient in many respects; but I shall be satisfied if I have succeeded in showing, by what has been done, what greater results may yet with certainty be accomplished, and if, by inviting discussion, I have contributed to hasten the period when the electric telegraph will no longer be the wonder of the age, but will become the simple and ever-ready agent to extend the range of human intelligence and power upon the earth, fettered no longer by the limits imposed by distance.

In conclusion, Mr. Siemens explained the numerous instruments and diagrams before the meeting, amongst which were the early needle telegraphs, by Cooke & Wheatstone; Professor Wheatstone's dial instrument, and early magneto-electric arrangements; Bain's chemical telegraph, and Henley's double needle telegraph; the instruments in actual use by the Electric and British Telegraph Companies; the arrangement of instruments used in working the Dutch cables, consisting, on the English side, of Mr. Varley's arrangements, and on the Dutch side of Siemens and Halske's recording instruments; the recording instruments worked by induced currents (produced by a Ruhmkorff coil) used on the Mediterranean cables; Siemens and Halske's new step-by-step or dial instruments, and the recording instruments by the same firm which were used upon the East India lines and elsewhere; besides a variety of rotary apparatus, alarums, etc.

DISCUSSION.

The *Chairman* (W. R. Grove, Esq., Q.C., F.R.S.), in inviting discussion, said that perhaps it would be as well that speakers should apply themselves more to general topics, than to the mechanical details of the instruments before them. In looking at the array of apparatus on the table, it was wonderful to think that the whole of these inventions had resulted from the scientific researches of the last half century, which showed how rapid had been the progress of electric science. He thought that important points for discussion were, the best means of insulation, and the best form of battery power. It would be interesting to hear observations upon these two subjects. At present it did not appear that for long lines of telegraphic communication a better insulator than gutta-percha could be found, which combined a great degree of insulation with plasticity, toughness, and strength to resist the ordinary accidents to which telegraphs were subjected. It had been remarked by Professor Faraday that various specimens of gutta-percha differed in conducting power, as also in durability. Doubtless very considerable steps in the improvement of the electric telegraph would be effected if they could with certainty produce gutta-percha of a quality giving it a greater power of insulation. Another important point was what was the best form of power to be used for the transmission of the electric current. That must necessarily differ according to the uses to which the instruments were put. A different power was required for short distances to that which would be suitable for long distances, such as the Atlantic telegraph. One advantage of magneto-electric power, as opposed to that of the battery, was that the apparatus was always ready and only required small mechanical power to work it. It has been found to answer well for short distances, and, with regard to its applicability to long lines, no doubt some opinions would be given that evening. There had of late been many improvements in the means of inducing electricity of high power; for instance, the Ruhmkorff coil, by means of which a great increase in the power of the current had been produced; and thus immense intensity was obtained with a comparatively small battery. It was stated that in order to obtain sufficient intensity to work a length of telegraph such as the Atlantic cable, they would require 500 Daniell's cells, whilst with the Ruhmkorff coil it was probable they would be able to obtain sufficient intensity with a much smaller number. Another important point was the occasional rupture of the copper wire in submarine cables. It was argued that by having the outer iron sheathing of a twisted or spiral form, whilst the wires of the inner core were straight, there was a greater power of stretching in the outer than in the inner wires, and he did not know how far the breakages that had taken place were due to that circumstance. He thought, however, it was very desirable to have the whole cable so constructed that the stretching of the wires, if any, should be uniform, and that one part of the cable should not stretch in a greater degree than the other.

Mr. W. Smith thought that Mr. Siemens was slightly in error upon one or two of the facts he had brought forward. He had stated that the first attempt to establish a subaqueous conductor across the open sea, was made by Wollastone (from Dover to Calais) in 1850; and that in the following year Crampton laid a cable between the same places successfully. This cable, it was added, was sheathed with iron wire, according to Messrs. Newall & Co.'s patent process. He (Mr. Smith) thought there was some mistake here, inasmuch as he was not aware that Messrs. Newall & Co. had any patent for that form of cable. The fact was that in 1847 the first specimens of that form of cable were made by Mr. Brett, who, he believed, patented a system of interoceanic telegraph in the year 1845. Mr. Brett's plan was to coat copper wire with india-rubber—the best insulator then known—and to enclose the wires in a series of iron tubes, united by ball and socket joints. He (Mr. Smith) had no wish to advance any claim to invention in connection with submarine telegraph cables, but he would state that he believed he was the first to communicate to Mr. Brett, in 1847, the idea of protecting the insulated copper wires, forming the core, by a sheathing of iron wire. Mr. Brett adopted the idea, and in the same year some specimens of that form of cable were made for him. That was long prior to the construction of the Dover and Calais cable. The cable to which Mr. Siemens alluded, was manufactured at Wapping, and was only completed, but not commenced, by Messrs. Newall & Co. It was in consequence of some little difference with the contractor, that Messrs. Newall & Co. undertook to complete the cable, which was done with the very machinery which was originally designed for the manufacture of that form of cable.

Mr. Latimer Clark, in reference to the acknowledgment of the labours of Oersted and Ampère in the advancement of electrical science, had been lately struck by a passage in a French work on electricity, published in 1805,* from which it almost appeared

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* "Manuel du Galvanisme," par Giuseppe Izarn, Paris, 1805. The passage is as follows: p. 120, "Appareil pour reconnaître l'action du Galvanisme sur la polarité d'une aiguille aimantée.

"*Préparation.*—Disposez les tiges horizontales *a*, *b*, *d*, de l'appareil, Fig. 53 (a common universal discharger) de manière que les deux boutons se trouvent à une distance un peu moindre que la longeur des aiguilles que vous voudrez southat the influence of an electric current on a magnetic needle, and its effect in magnetising an iron bar, had been noticed and published long prior to the date of Oersted's discovery. Mr. Siemens had erroneously attributed to Professor Faraday, the discovery of the possibility of the co-existence of several waves of electricity in one submerged wire. The phenomenon of the slow transmission of currents through submerged wires, was first noticed by him (Mr. Clark), in April, 1852, in the course of a series of experiments undertaken at the works of the Gutta-percha Company to ascertain how far it would be practicable to work through gutta-percha wires laid underground between London and Liverpool; and, in 1853, a patent was taken out to obviate that effect by surrounding the gutta-percha wire with a coating of asphalt, or some cheap dielectric substance. The Electric Telegraph Company having completed eight underground wires from London to Liverpool, and meeting with much annoyance from the induction, Professor Faraday and Professor Airy were requested to attend at Lothbury, and early in 1854, he (Mr. Clark) exhibited the phenomena of induction, and produced diagrams with three needles on chemically prepared paper, showing, in a very perfect manner, the passage and retardation of the current. These diagrams were afterwards exhibited by Professor Faraday at the Royal Institution, and formed the subject of a lecture there. He (Mr. Clark) had not met with much practical inconvenience from the breakage of the internal copper wire in submerged wires and single submarine cables, and cases of fracture were very unfrequent. In deep submarine cables, where every precaution was requisite, the difficulty had been successfully surmounted by the use of the twisted strand of wires, but as this necessarily occasioned some additional resistance, he did not consider its

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mettre à l'expérience ; et à la place des boutons *b*, *b*, qui sont vissés sur leur tige respective, adaptez aux tiges, ou une petite pince, ou bien un petit ajutage applati.

"*Usage.*—Après avoir placé l'aiguille, de manière que ses deux extrémités soient prises dans les deux petites pinces, établissez une communication de *d* avec une des extrémités d'un électromoteur, et de *a* avec l'extrémité opposée.

"*Effets.* — D'après les observations de Romagnosi, physicien de Trente, l'aiguille déjà aimantée, et que l'on soumet ainsi au courant galvanique, éprouve une déclinaison ; et d'après celles de J. Mojon, savant chimiste de Gènes, les aiguilles non-aimantées acquièrent, par ce moyen, une sorte de polarité magnétique."

universal adoption desirable. With reference to the general use of the double-needle instrument in England, he thought this was not the result of any prejudice, but a consequence of the intrinsic merits of the instrument itself, which were such that when persons had once become familiar with its use, nothing but compulsion would induce them to resort to any other. The Electric Telegraph Company were fully alive to the advantages of the Morse instrument, and had employed it extensively on all their principal commercial circuits for many years, and it was in daily operation on thousands of miles of telegraph in this country. The needle instrument had, however, such advantages over the Morse in simplicity, in rapidity of transmission, and in facility of use, that they had in vain endeavoured to bring the Morse instrument into extensive use on railways. Nothing but the constant use of the two instruments side by side could enable a person to form a correct estimate of their relative value; and he could assure those who were in the habit of condemning the double-needle instrument on purely theoretical considerations, that they were, from imperfect information, falling into a very great error.

Mr. E. Highton said he objected to the statement in the paper—that the change from magneto-electricity to electricity developed directly by a voltaic battery, was a step in a retrograde direction. Every form of magneto-electric machines hitherto used in Great Britain and Ireland had failed; he instanced the instruments of Professor Wheatstone and Mr. Henley—instruments which, he believed, showed inventive talent of the highest order, but they were not commercially comparable with other plans when voltaic electricity was employed. The system of underground wires, as recommended by Mr. Werner Siemens, in Prussia, had proved a fatal failure, and nearly the whole of the capital invested therein by the Government had been lost. He preferred the use of electricity produced by a battery and an electro-magnet, to that produced by a permanent magnet, inasmuch as the one could be increased to any extent, according to the weather, whilst the other could not. He objected to the statement in Mr. Siemens's paper as to Messrs. Newall & Co. being the patentees of the submarine telegraph as now used. The fact was, there was no practical method of making submarine cables published, prior to his own patent of September 21, 1850. He corroborated the statement of the author as to the immense risks that must be incurred in laying submarine cables in great depths of the ocean. He thought that the attention of those connected with the working long lengths of telegraphs should now be directed to a system of codes. He instanced one of his own which contained 800,000,000 times 2,000,000 preconcerted messages, all of which did not occupy one side of half a sheet of foolscap, and each would not occupy more than twelve seconds in transmission. Although Mr. Siemens had stated that by his instrument he could communicate between London and Odessa, there was no proof that this had been done. With respect to insulation, Mr. Highton remarked that this depended very much upon the climate of the country to be passed through. He considered that for England and the west of Ireland, a different kind of insulation was required from that suitable to Italy or India, and such like countries. The telegraphic instruments, batteries, and other apparatus to be employed, ought to be suited to the work to be done, and he believed there was no one telegraphic instrument suitable to all cases throughout the world, but that each particular case required its own special apparatus. With regard to the purification of gutta-percha, which had been alluded to by the chairman, he was happy to say that the Society had appointed a committee to investigate the whole subject, and he hoped that great results would accrue from their investigations. With regard to the breaking of the internal copper wire in submarine cables, he remarked, that in the specification which he made for the British Telegraph Company's cable between Scotland and Ireland, he put in a clause which compelled the contractor for the making of the cable to give double the lay or twist in the copper wires to that of the outside iron wires, and thus prevent all strain from coming upon the copper wires until the iron wires had broken. The submarine cable of the British Telegraph Company had been most successful. Although weighing 180 tons, and containing six wires, of 25 miles in length, it had now been at work for nearly four years, and every wire up to the present moment was perfect, and since its submergence it had not cost the company anything for repairs. With regard to the double-needle system of the original Electric Telegraph Company, he stated his belief that, sooner or later, if they were to compete with their rivals, they must use a one-wire system. Mr. Highton then read an extract from a work published by Mr. Ronalds, in 1823, which showed that the first telegraphic message ever transmitted in Europe was transmitted by an Englishman, in the year 1816, and that Mr. Ronalds then recommended the use of underground wires. Mr. Highton then exhibited and explained the instruments invented by himself, and used by the company with which he was connected, and which, through one wire, transmitted the last parliamentary speech of the Queen from London to Liverpool at the rate of 32 words a minute; and, through the same kind of instrument, with three wires, the speech of the American President, containing upwards of 16,000 words, was telegraphed from Liverpool to London at the rate of upwards of 3,500 words an hour, without a single mistake. He was sure that every one present would join in a vote of thanks to Mr. Siemens for his interesting paper.

Mr. Pearsall regarded the historical record of the electric telegraph, presented to them that evening, as of great value, especially that portion which referred to the experiments of Steinheil. Some years ago, in passing through Bavaria, he (Mr. Pearsall) was charged to ascertain the practical results of Professor Steinheil's researches and experiments, when the Professor stated that he had carried on electro-telegraph communication, without any wire at all, by which he now understood him to mean that he had made use of the rails of the railroad for the line wire, using the earth as the return circuit. With reference to the use of wire rope, he remembered that when the plan of metallic shutters to shop-fronts was first introduced, it was found that great wear and tear was experienced in the friction of the chain by which the shutters were raised and lowered; this had been obviated by the introduction of a rope of twisted wire, sufficiently flexible for the purpose. In the course of the experiments for ascertaining what was a proper material for the purpose, attention was drawn to the means by which the extraordinary flights of ballet aërials on the Italian stage were effected, which was found to be by means of twisted wire rope, and the idea was at once adopted. The machinery then used for the manufacture of wire rope was the same in almost all its details as was now employed in the manufacture of the outer sheathing of submarine telegraphic cables.

Mr. Varley mentioned that his attention had been accidentally directed to the possibility of constructing a telegraph, the signals of which would be communicated by the sense of touch. He had himself been able, by touching the wire whilst an instrument was at work, to interpret the signals by feeling; and he thought possibly this idea might ultimately be practically worked out. Mr. Varley also gave a description of an instrument exhibited by him termed the acoustic telegraph. He begged to ask Mr. Siemens at what rate the Malta cable was worked?

Mr. Siemens replied he believed at the rate of about 12 words per minute, though that very much depended on the skill of the operator.

Mr. Varley added that the experiments with the Atlantic cable had led certain electricians to the conclusion that a small wire conducted more rapidly than a large wire, a conclusion with which he (Mr. Varley) did not agree. If it should be established that the larger wire was the best conductor, he did not apprehend that the expense of a submarine cable would be materially increased by its adoption. The cost of the present Atlantic cable was about £100 per mile, of which sum £60 was due to the outer iron sheathing, and £40 to the copper wire and gutta-percha covering, and of this he thought the gutta-percha cost the larger portion.

Mr. Siemens said, in reply to Mr. Smith, that whatever his or Mr. Brett's merits might be in having first suggested the long spiral iron sheathing of electric cables, there could be no doubt about the fact, as stated in his (Mr. Siemens's) paper, that it was actually constructed according to the process patented by Messrs. Newall & Co. for twisting wire ropes. He felt surprised at Mr. Latimer Clark's assertion, that Oersted, Schweigger, and Ampère, were not the originators of the science of electro-magnetism. The electric charge in undergound line-wire was first observed by his brother, Werner Siemens, and fully described in a memoir, presented to the French Academy in 1849, whereas underground line-wire had not been introduced into this country till 1854. He was glad Mr. Clark acknowledged the superiority of the recording over the needle instrument, but did not feel surprised at his defending the latter, very much on the principle upon which one would defend an absent and dying friend. Mr. Highton had also defended the needle instrument, on account of its comparative simplicity and speed. There might be some degree of force in that argument in regard to this country, where the lines were comparatively short, but a needle telegraph was certainly inadmissible for long and international lines of communication. The defects of the needle telegraph system in this country were, however, sufficiently manifest, from the distortion of names and figures which occurred in almost every message received. Mr. Siemens could not admit Mr. Highton's argument against the application of magneto-electric and induced currents. Their failure in all the early attempts had been admitted in the paper and might be very clearly traced to the short duration of the induced current, which rendered it unfit to exercise any sustained or visible mechanical effect upon the receiving instrument; but he mentioned that, in the construction of the instruments he had placed before the meeting, a new and most important feature had been introduced, that of sustaining the effect produced by an instantaneous current, by means of permanent magnets, the instantaneous line-wire current being only required to disturb for an instant of time the equilibrium between two equal and contending poles. Instruments constructed upon this principle required no adjustment according to the distance and other circumstances, which was another very important point, and there was hardly any limit to be assigned to which the delicacy of the instrument might not be carried. The chief advantage of induced currents for submarine lines consisted, however, in their perfect equality. Respecting the new dial instrument he wished to draw the attention of the meeting to the means adopted to obtain quantitative induced currents by the application of a series of permanent magnets acting in close proximity upon a long rotating keeper of the section of the letter **H**, into the recesses of which the induced wire was coiled, by which arrangement a powerful alarum might even be sounded at a distance of 500 miles, to which distance these instruments worked with absolute certainty. The dead-beat ratchet-motion was also of peculiar construction, which rendered the slip of a tooth impossible even at the highest velocity at which the handle of the instrument could be worked. The mode of receiving messages by touch, which had been mentioned by Mr. Varley, was not new, the same plan having been proposed by Vorsselmann de Heer (see "Pogg. Ann." vol. 46, page 513) in

1839. The most suitable diameter of the conductor, in submarine cables, under given circumstances, might be ascertained without much difficulty from the simple formulæ which he had given, and which he had hoped would have formed a principal point in the discussion.

The Chairman said it was now his pleasing duty to call upon the meeting to join him in a cordial vote of thanks to Mr. Siemens for his very elaborate and valuable paper. He had almost hoped to have heard the battle of magneto-electric and battery power fought over again, as he saw advocates of both systems present. Professor Wheatstone was avowedly in favour of the magneto-electric power, and there had been of late many important improvements in that direction. They had heard that evening one extraordinary communication from Mr. Latimer Clark, which came with great surprise upon all who were acquainted with the normal history of electricity. This statement was, that Oersted was not the first discoverer of electro-magnetism. If a priority of discovery were established on behalf of any other person, it would come with great surprise upon those who had been accustomed to associate that discovery with the name of Oersted since the year 1821. The only scintilla of any prior claim to the discovery was that which was vaguely put forward by Ritter, a man who was no doubt very much underrated in his day. The Chairman concluded by proposing a vote of thanks for the paper which had been read.

A vote of thanks was then passed to Mr. Siemens.

OUTLINE OF THE PRINCIPLES AND PRACTICE INVOLVED IN DEALING WITH THE ELECTRICAL CONDITIONS OF SUBMARINE ELECTRIC TELEGRAPHS,

By WERNER and C. W. SIEMENS.*

THE failures of the more extensive lines of submarine electric telegraphs, which have hitherto been but too frequently experienced, have become manifest almost invariably by a gradual decrease of insulation. In repairing these lines, it has generally been found that the gutta-percha has become disintegrated by the electrolytic action of the currents employed in working the line in places where the thickness of insulating material had been originally considerably below the average, owing to some mechanical injury, or, more frequently, owing to a cavity in the material, forced into by the water, or to an eccentric position of the conductor.

In such places where the insulating covering of gutta-percha has been of uniform and sufficient thickness, no disintegration or partial destruction of the material is observable, even after the line has been worked for many years. The rapidity with which the work of destruction in faulty places proceeds depends entirely upon the intensity and duration of currents employed in working the line. Faults are produced proportionately more rapidly in long lines, owing to the greater resistance of the metallic conductor. Their progress can be retarded in working the lines with *feeble* and *alternating currents*, but it cannot be arrested entirely, and it may be laid down as an axiom that "*so long as any thin places are allowed to remain in the gutta-percha covering of a submarine conductor, so long will their insulation fail by slow degrees.*"

It is, therefore, a matter of first importance to prevent, if possible, all irregularity in the insulating covering. The material employed should be perfectly homogeneous ; it should be put upon

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* **Excerpt Appendix to the Report of the Joint Committee appointed to inquire into the Construction of Submarine Telegraph Cables, London, 1861, pp. 455–458 and 379–382, being a paper read before the British Association for the Advancement of Science in 1860.**

the wire in several coatings, closely adhering to one another; air bubbles should be strictly avoided, and the concentricity of the entire coating be insured by the use of very perfect machinery and strict avoidance of stoppages during the process of covering, to prevent a softening of the several coatings by heat.

Great improvements have of late been effected in the process of covering electric conductors with gutta-percha and intermediate layers of a compound called "Chatterton's mixture," which may be estimated by the fact that the covering of the Rangoon and Singapore cable, now in process of manufacture, insulates fully ten times better than the covering of the Red Sea and India cable did before it was laid.

This marked improvement is due to the greater care taken by the Gutta-Percha Company in the manufacture, under a system of stringent electrical tests, which we are charged by the British Government to apply. The objects of these tests is, in the first place, to ascertain the specific conductivity of each mile of the copper conductor, in order that all below a certain fixed standard may be rejected.

An inquiry into the extraordinary variations in the conductivity of the *copper of commerce* has been made the subject of a careful investigation by Dr. Mathiessen for the British Government, which will probably shortly be published.

In practice we find that the best selected copper employed for telegraphic conductors varies as much as twenty per cent. in its conductivity and that the *purer copper* conducts the best.

The conductivity tests of each mile of an insulated conductor are very necessary, not only to reject the faulty material but also to obtain a complete record of the conductivity of each portion of the cable when completed, without which it is not possible to determine afterwards by galvanic tests and calculations the precise position of a fault.

The more difficult and most important tests are those of the conductivity of the insulating material of *each mile* of insulated conductor, for it is not sufficient to find out any palpable fault or leakage but to appreciate eccentricities, cavities, or other minor defects in the coating, and to reject what falls below the standard of conductivity of the insulating material in its most perfect condition.

It was necessary for the purpose to determine in the first place the specific conductivity of the material which experience has proved to be sufficiently uniform at constant temperatures.

The effect of temperature upon the conductivity of gutta-percha and other insulators has lately been fully investigated by the Scientific Telegraph Committee of the British Government, whose report is however not yet published.

It suffices for our present purpose to state that between the limits of 41° and 80° Fahrenheit we found the conductivity of the insulating covering of the Rangoon and Singapore Cable to increase nearly in the ratio of 1 to 7. The ratio of this enormous increase is, however, by no means constant, and in the absence of very elaborate and reliable experimental results we thought it advisable to test at a uniform temperature of 75° Fahrenheit (20° Cent.) This comparatively high degree of temperature has the advantage that it is seldom exceeded naturally, and that the conductivity being seven times greater at that temperature than at the winter temperature of 41°, the effect of minute faults upon the measuring instrument will also be proportionately exaggerated.

In order to insure uniformity of temperature the coils to be tested are placed for twenty-four hours in tanks containing water regulated to 75°; they are then removed into the testing tank of the same temperature, which is hermetically closed, and hydraulic pressure of at least 600 lb. per square inch applied, in order to force the water into the cavities or fissures that may present themselves. It is a remarkable fact, which is borne out by observation upon cables in process of submersion, that the application of hydrostatic pressure sensibly decreases the conductivity of gutta-percha, which however increases again slightly above the former ratio when the pressure is relieved.

In slightly defective coils the increase of external pressure produces, on the contrary, no increase, or even a decrease of insulating property, and a clue is thus obtained to ascertain otherwise inappreciable defects. The methods usually employed of measuring the conductivity and insulation of conductors in degrees, by simple galvanometer tests, would be insufficient for the purposes here intended.

It was necessary to express the conductivity of both the conductor and the insulating covering by simple numerical expression in units of resistance.

The unit of resistance we have adopted is that of a column of mercury 1 metre in length and of 1 square millimetre sectional area, taken at the freezing point of water. The advantages of this unit have been fully set forth by Mr. Werner Siemens in a treatise published in "Poggendorff's Annalen," vol. 110.

In expressing the degrees of conductivity of both the wire and the insulating medium in definite units of resistance we obtain not only the advantage of a more accurate comparison between the results of different indication, but subsequently when the separate coils are united with a single cable, we have an admirable means of judging its electrical condition if we compare the total resistances of both the conductor and insulating medium with the sum of the resistances previously obtained in testing each coil separately, due allowance being made, of course, for change of temperature.

But the principal advantage derived from this system of measuring consists in the facilities it affords in determining the position of a fault in the cable *while* it is being laid and *after* submersion.

In carrying this system into practice, we construct in the first place coils of definite resistance, which are capable of being combined in such a manner that we can vary the total resistance between the limits of 1 unit and 10,000.

By inserting these alterable resistances into one branch of a Wheatstone's bridge, the resistances of the copper or insulating covering of a cable of considerable length can be ascertained. If, however, it is required to ascertain resistances beyond the limits of the resistance coils, we adopt another arrangement on the principle of the Wheatstone's bridge, which consists in making the two permanent branches of the same also changeable. A, B, C, D, represent the four branches of this arrangement, A, C, and B, D, being in connexion with the galvanometer, A, B, and C, D, the terminals of a battery (Plate 1, Fig. 1).

No current will pass through the instrument when the relation $\frac{A}{B}=\frac{C}{D}$ exists. But as in Wheatstone's arrangement A is always equal to B, the unknown resistance D is equal to the resistance C. A scale containing resistance coils from 1 to 10,000 units would therefore only allow us to ascertain resistances not exceeding these limits, but C and A being each composed of 3 variable coils of 10, 100, and 1,000 units respectively, we are enabled to measure any resistance between 0·01 and one million units with the same degree of accuracy. By means of this arrangement we measure the resistances of copper wire of any length and the insulation resistance of long cables within the limits of correctness of 0·2 per cent.

For the insulation tests of short pieces of cables or of longer cables of better insulating materials, such as india-rubber and Wray's mixture, such method is no longer applicable, because resistance coils of such diversity of dimensions as would be necessary could not be used with sufficient accuracy, chiefly because the greater battery power that would be required would heat the smaller branches of the arrangement, and *thus increasing* the resistance affect the result very considerably.

It was therefore necessary to turn to another method for ascertaining the value in units of the insulation resistances of short pieces of cable, say one knot in length. We employ in such cases a very sensitive sine galvanometer, or if the room permits of it, a Weber's reflecting galvanometer of 40,000 turns, and a magnetic reflector.

By means of an adjusting magnet the sensibility of this instrument can be varied between the limits of 1 and 100.

The astatic condition of the needles of the sine galvanometer being subject to changes, the constant of the instrument should be verified repeatedly while testing.

For the reading of this instrument in degrees we substitute units of resistance by means of the following formula :—

$$R = \frac{\sin \phi^1}{\sin \phi} n \quad . \quad . \quad . \quad . \qquad (I.)$$

in which R is the insulation resistance, ϕ the angle of deflection, ϕ^1 the constant of the instrument, n the number of elements employed.

For the derivation of this formula, see Appendix No. 1.

This method is applicable only for measuring great resistance between certain narrow limits. During the progress of the cable at the sheathing works, the insulation resistance gradually decreases, and the instrument would very soon be too sensitive. It could be made less sensitive, it is true, but in resorting to this it would no longer be possible to appreciate correctly the value of the resistance of the last coil added to the cable.

It was, therefore, necessary to resort to a means of maintaining the original degree of sensitiveness of the measuring instrument, while the total resistance gradually decreases.

For this purpose the coils of the sine galvanometer employed are surrounded by an additional coil of comparatively few turns, through which the current of a constant small battery continually passes.

The insulation current passes through the wire of the instrument, but is counteracted by the current in opposite direction in the outer coils, which is so regulated by means of a resistance coil, that no deflection of the galvanometer needle can be observed.

In adding to the length of the cable, the resistance coil in the outer circuit of the instrument has to be diminished by stopping till the equilibrium of the needle is restored; and the value of the alteration of the resistance coil being known in units, this number has only to be multiplied by the fixed proportion of the relative power of the outer and inner coil upon the needles to produce the correct result.

If W (Plate 1, Fig. 2) represents the resistance of the inner coil, W_1, the resistance coil put into the inner circuit, m, the number of cells of the battery of the inner circuit, w, the resistance of outer coil, w_1, the resistance coil put into the outer circuit, n, the number of cells of the outer battery circuit, and k, the number indicating the proportion of the effect of the outer and inner coil on the needle, we have,

$$\frac{n}{w+w_1}\,k=\frac{m}{W+W_1}\text{; or}$$

$$k=\frac{m\,(w+w_1)}{(W+W_1)\,n}.$$

If instead of W_1 the unknown resistance x of the cable is introduced into the circuit, and the resistance w_1 altered (to V) till the needle is perfectly at zero, when to make the equation quite general

M and N are substituted for m and n, the following equation is established :—

$$\frac{M}{x+W} = \frac{N}{V+w}. k \text{ ; or}$$

$$x = \frac{M\,(V+w)}{N.k} - W$$

or by introducing for k its value from above,

$$x = \frac{M\,n\,(W+W_1)}{N\,m\,(w+w_1)}\,(V+w) - W$$

The chief advantage of this arrangement consists in the unchanged sensibility of the instrument, since the whole strength of the insulation current acts upon the needle, which nevertheless is brought back always to zero.

In measuring the insulation resistance of short cables the resistance of the galvanometer coils W and w may in practice be neglected, and the following more simple formula may be adopted :—

$$x = \frac{M}{N} \cdot \frac{V}{k} \quad . \quad . \quad . \quad . \quad . \quad \text{(II.)}$$

The value of k is independent of the sensibility of the needle, and need only be determined once for all.

The tests are thus reduced to a very simple and easy method.

In order to calculate the insulation resistance of insulated wires from the specific conductivity of the material used, and vice versâ, we employ the following formula :—

$$W = \frac{C.\ \log.\ \frac{R}{r}}{2\,\pi\,l\,\lambda} \quad . \quad . \quad . \quad \text{(III.)}$$

The derivation of this formula has been given by Mr. Werner Siemens in "Poggendorff's Annalen" of 1857, vol. 102, and will be found in Appendix 2 of this paper.

The foregoing methods suffice to ascertain insulation and copper resistances of cables of all lengths and forms ; they do not comprise, however, the test necessary to determine their inductive capacities.

Recent experiments hereafter given prove that the specific inductive capacity of insulating materials is more to be relied upon for permanency than their specific conductivity ; the inductive capacity is, moreover, independent of local defects in the insulating covering, being dependent chiefly upon the general geometrical form of the insulator. In ascertaining, therefore, the inductive capacity of a length of cable, as compared with a standard Leyden jar, and in comparing this result with the total capacity due to the material employed, a means is obtained of ascertaining with great certainty whether the material is disposed throughout its length in equal thickness round the conductor, or whether the wire lies partly eccentric. A knowledge of the inductive capacity of a cable is, moreover, absolutely necessary, in order to determine the position of a break in the conductor when the broken end remains insulated.

According to Faraday's conception, the inductive action is communicated, say from the interior electrified covering of a Leyden jar to the exterior, from atom to atom, through the dialectric. In our case the jar is represented by the cable, the inner covering of which is formed by the surface of the copper wire, the exterior by the water.

The laws which apply to the motion of heat and electricity in conductors are accordingly *directly* applicable to electro-induction, which may be expressed by the conductivity multiplied by a constant varying with the nature of the insulating material.

Starting from this point of view the inductive capacity of any insulated wire will be represented by the formula

$$K = \frac{I\,C\,2\,\pi\,1}{\log.\frac{R}{r}}\,; \qquad . \quad . \quad . \ (\text{IV.})$$

in which the inductive capacity I takes the place of the specific conductivity λ of the previous formula. The unit measure of inductive capacity is assumed to be the capacity of a Leyden jar of two square plates of the unit of measure in area, and placed at unit distance apart.

Professor W. Thomson has obtained the same formula in a direct and most elegant manner, which differed from that of

Mr. Werner Siemens in the value of the constant, proving that he started with another unit. Mr. Werner Siemens's method has been fully developed in "Poggendorff's Annalen," vol. 102.

In dealing with cylindrical jars, or with cables, this formula may be written more simply thus :—

$$K = \frac{I \,.\, C}{\log.\,\frac{R}{r}}.$$

In our experiments the inductive capacity of a Leyden jar is measured by the deflection of a galvanometer needle. If the deflection of the needle is caused by a current of very short duration, the quantity of the electricity passing through the galvanometer is equal to

$$K = \frac{\sin\frac{A}{2}.}{E}$$

In practice it is found to be very difficult to read with sufficient accuracy the sudden deflection of a needle, and we prefer for practical use an instrument which we have placed before the Section, enabling us to obtain a rapid succession of charging or discharging currents which in passing through the galvanometer produce a steady deflection of the needle, capable of being read with great accuracy. The value of these deflections is calculated by means of the following formula :—

If A is the angle through which the sine galvanometer has to be turned to bring the needle to zero, C the number of charges or discharges per second, E the electromotive power of the battery, we have—

$$K = \frac{\sin A^{*}}{E}\, C$$

or if K_1 is the unit capacity of a jar and A the corresponding

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* In this case the amount of charge is represented by a constant deflection, and therefore by sin A, whilst above, where it was given by one swing of the needle, it is equal to sin $\frac{A}{2}$.

angle of readjustment of the instrument, we have (if the number of discharges per second remains the same)—

$$K : K_1 = \sin A \ : \ \sin A_1$$

$$K = \frac{K_1 \sin A}{\sin A_1}$$

By permission of the British Government, we have been enabled to test the Government experimental cables by this method.

The results of these experiments show satisfactorily the accuracy of the methods employed. They also prove that the formula employed in calculating the specific inductive capacities which Professor W. Thomson and Mr. Werner Siemens obtained in entirely different ways, can be relied upon in practice.

The specific *induction* of all gutta-percha covered wires is shown to be nearly the same and to be entirely independent of its specific *conductivity*, while india-rubber and its compounds are far inferior in specific induction to gutta-percha. The specific induction of gutta-percha being taken as a unit, that of india-rubber is equal to 0·7 only, and that of Wray's mixture = 0·8.

We have still to make mention of those methods which have been frequently resorted to of late of ascertaining by means of sensible electrometers the decrease of tension in a heavily charged cable when left to itself.

If E represents the tension of a galvanic battery in communication with the cable, as observed by a sine electrometer, y the remaining tension in the cable after an interval of time t, K the capacity, and w the resistance of the insulator, there will be, according to the law of Ohm, after the interval t a current of discharge $= \frac{y}{w}$, by which the tension is decreased during the time dt by dy. Hence we obtain the equations :—

$$K . dy = \frac{y}{w} dt$$

$$- \frac{dy}{y} = \frac{dt}{Kw}$$

$$C \quad \log. y = \frac{t}{Kw}$$

and since for $\begin{matrix} t = 0, \\ y = E, \end{matrix}$ the integration constant $C = \log. E$

and $$\log. \frac{E}{y} = \frac{t}{Kw}$$

or $$\frac{E}{y} = \epsilon^{\frac{t}{Kw}}$$

and $$y = \frac{E}{\epsilon^{\frac{t}{Kw}}}$$

In a regular cable

$$K = I \frac{2 \pi . l}{\log. \frac{R}{r}} \quad \text{and} \quad w = \frac{\log. \frac{R}{r}}{\lambda . 2 \pi . l}$$

or $$Kw = \frac{I}{\lambda} \quad \text{and therefore} \quad \log. \frac{E}{y} = \frac{t \lambda}{I}$$

and $$\lambda = \frac{I \log. \frac{E}{y}}{t} \quad . \quad . \quad . \quad . \quad . \quad \text{(V.)}$$

and therefore

$$\lambda : I = \log. \frac{E}{y} : t$$

This method is well adapted for ascertaining the specific resistances of insulating materials, and to compare the insulation of two similar cables, even when no instrument capable of exact measurement is at hand. It suffices to observe the times required for the reduction of the original tensions to a given fraction. As the proportion $\frac{E}{y}$ although unknown is in each case the same, it is obvious from the former formula that

$$\frac{t \lambda}{I} = \frac{t_1 \lambda_1}{I}$$

and

$$\frac{\lambda}{\lambda_1} = \frac{t_1}{t}$$

where λ and t represent specific conductivities and times occupied in both experiments.

This result is independent of any eccentricity of the wire in its insulating covering. The method is therefore well adapted for determining the specific resistance of materials, but as it is necessary to ascertain whether the wire is throughout the cable, concentric with the insulator, this method cannot be exclusively used.

Besides this process requires considerable time in testing well insulated cables. Again, another objection to its exclusive use arises from the possibility of slight faults in long cables passing unappreciated, as the loss of tension through such faults will be exceedingly small as compared with the whole charge.

We therefore prefer to determine the loss of tension not by an electrometer, but by measuring the charge a, and after the lapse of one minute the discharge b by the galvanometer needle. We then have the loss of quantity or tension during the minute

$$L = 1 - \frac{b}{a} \quad . \quad . \quad . \quad . \quad \text{(VI.)}$$

In order to associate this formula with the system previously developed, it is only necessary to remark that $\frac{b}{a}$ is equal to $\frac{y}{E}$.

The cable having been tested from the earliest stage of its manufacture (in lengths of one knot) subsequently during the joining and covering of the cable, and finally during the paying out, these tests must strictly control each other, and must consequently be recorded systematically. The chief care during the submersion of the cable should be to detect at once the slightest change in its insulation, in order that the paying-out machinery may be stopped instantly. It sometimes happens, however, that a fault does not appear immediately on submersion. It is therefore necessary, if a fault appears, to calculate its exact place before taking any other steps to remove it. In order to do this effectually, it is necessary to test the cable from both ends, *i.e.*, from the ship and from the land station, as the determination from one side gives only the maximum distance.

In paying out submarine cables, we pursue the following plan of testings :—

A clockwork arrangement at the land station is made to put the cable by rotation to earth, to the poles of a battery, and to insulation.

On board the ship there is constantly a bridge of resistances in connexion with the line. Whilst the electrician keeps the bridge in equilibrium, he is enabled to ascertain alternately the resistances of insulation and continuity.

The attendant of the station likewise observes the two data, and transmits them telegraphically to the ship. If these four tests differ materially they indicate thereby the existence of a fault, the position of which can be calculated from the data obtained. This method of observing the conditions of the cable, although very fatiguing to the electrician employed, has been found to answer perfectly well in paying out the Indian lines. During the paying out of the Aden-Kurrachee section by Messrs. R. S. Newall & Co., we were by this means enabled to observe faults on five different occasions, which could then be removed without delay.

Our methods for determining the place of a fault are as follows :—

1st. When both ends of the cable are at hand let x and y represent the respective distances from each end of the cable to the fault, l the length of the whole cable, G a galvanometer, and W, W_1, two graduated resistance coils (Plate 1, Fig. 3). Then if W and W_1 are so adjusted that the galvanometer needle is perfectly quiet the place of the fault is given by the formula—

$$x = \frac{l\,W}{W + W_1} \quad . \quad . \quad . \quad . \quad \text{(VII.)}$$

This method has already been published by Werner Siemens (see Zeitschrift des Deutsch - Oesterreichischen Telegraphen-Vereins, 1857) having been used by us with perfect success ever since 1849.

In dealing with a single submerged line this method is no longer applicable. Let c denote the resistance of the whole length of the cable, x and y the resistances from each end to the fault, z that of the fault itself ; and a_1 and b_1 resistances observed from each end respectively, the further end being insulated ; a and b the same, whilst the further end is to earth. We have then by means of Ohm's law, the following equations :—

1. $c = x + y$

2. $a_i = x + z$

3. $b_i = y + z$

4. $a = x + \frac{z \cdot y}{z + y}$

5. $b = y + \frac{z \cdot x}{z + x}$

By eliminating z and y the resistance x is found by the following expressions :—

$$x = \frac{a_1 - b_1}{2} + \frac{c}{2} \quad . \quad . \quad . \quad \text{(VIII.)}$$

$$x = a . \frac{c - b}{a - b} \left(1 - \sqrt{\frac{b}{a} \frac{c - a}{c - b}}\right) \quad . \quad . \quad \text{(IX.)}$$

$$\frac{x}{y} = \sqrt{\frac{a}{b} \frac{c - b}{c - a}} \quad . \quad . \quad . \quad \text{(X.)}$$

$$x = a - \sqrt{(a_1 - a)(c - a)}.$$

If the cable was not perfectly well insulated before the fault under consideration appeared, the values a, b, and a_1, b_1, supply the means for determining approximately the resistance γ of the previous leakages. This resistance γ, together with the final readings of insulation a_2, b_2, gives the place of the fault as follows :—

$$x = a_1 - \gamma \sqrt{\frac{a_1 - a_2}{b_1 - b_2}} \quad . \quad . \quad . \quad . \quad \text{(XI.)}$$

In all these measurements the battery power must be so regulated as to keep the polarisation at the faulty place uniform. This is to be accomplished by determining preliminarily the place of the fault, then by regulating in the final measurement the number of cells so as to send from each side an equally powerful current through the fault, taking care not to take the observations till the polarisation has reached its maximum. We attach considerable importance to the last formula, which alone enables us to determine the situation of new faults in old defective cables, if only its previous electrical condition is known. This knowledge is unfortunately wanting in respect of nearly all the cables that have hitherto been laid. In the case of the Rangoon and Singapore cable, we propose to furnish each station with a complete testing apparatus, and to cause daily tests to be instituted upon the cable, when laid, of its electrical conditions on each section.

Records of these observations should be forwarded *daily* to the chief electrician in charge of the line, who will then have the means at his disposal to watch the rise and gradual progress of faults, and to apply the remedy at the proper moment, and with a certain knowledge of the position and magnitude of every defect.

Considering the circumstance that owing to great care, the conductor of the Rangoon and Singapore cable is fully ten times more perfectly insulated than the best conductor hitherto submerged, we confidently expect that the result in practice will also greatly exceed that of previous experience; still the insulating material employed remains the same, and is, therefore, liable to be affected by the same causes of failure.

The chief difficulty has hitherto consisted, in working india-rubber in such a way as to obtain uniform and perfect coatings upon the conductor without injury to the material itself. We have endeavoured to remove this difficulty in constructing a covering machine, which we have brought before Section G. of this Association.

We do not wish, however, to rest upon our individual efforts for the further development of this important new branch of applied science. Great efforts have been made latterly by others eminently qualified to produce useful results. The insulating power of gutta-percha has been vastly improved, and new insulating materials are being produced.

Our object in writing this communication is to show that although submarine electric telegraphs have often failed, owing to insufficient experience and insufficient care bestowed upon their manufacture to guard against defects, the experience gained has not been lost; and that in bringing the present stock of knowledge to bear upon the subject, more complete success may be insured.

The British Government, in promoting these enquiries, has stimulated and directed individual efforts, proving that England fully appreciates the advantages of the submarine electric telegraph, and is determined to realise the same, thus contrasting favourably in this, as in many other cases of practical progress, with other nations.

APPENDIX.

No. 1.—*Resistance of Short Cables.*

One pole of a battery of n elements is joined to the cable while the other pole is to earth; then, if ϕ represents the angle through which the galvanometer is turned to bring the needle again to zero, the following equation is established :—

$$\sin \phi = \frac{n E}{x + W_1}$$

E representing the electromotive power of one element, x the unknown resistance of the cable, and W_1 the resistance of the galvanometer.

In order to arrive at the actual value of the insulation resistances, a known resistance, say of 10,000 units, is introduced into the circuit instead of the cable, the sensibility of the instrument weakened (to $\frac{1}{100}$) by a branch resistance, W^2, and the number of cells reduced to one. Another equation is then obtained, in which I may represent the force of the current in the whole circuit to be

$$I = \frac{E}{10{,}000 + \frac{W_1 \cdot W_2}{W_1 + W_2}}$$ and the strength of the current passing through the galvanometer will be

$$i_1 = \sin \phi_1 = \frac{W_2 \cdot E}{W_1 + W_2} \cdot \frac{1}{10{,}000 + \frac{W_1 \cdot W_2}{W_1 + W_2}}$$

and because $W_1 = 99\ W_2$

$$i_1 = \sin \phi_1 = \frac{E}{100} \cdot \frac{1}{10{,}000 + \frac{99}{100}\ W_2}$$

$\frac{99}{100}\ W_2$ being with our instruments equal to 70 units, we obtain by introducing instead of 10,000 units 9,930,

$$\sin \phi_1 = \frac{E}{100} \cdot \frac{1}{10{,}000}$$

Eliminating E from the first formula, and combining it with the second one, we obtain

$$x = n\ \frac{\sin \phi_1}{\sin \phi}$$

in which formula x is given in millions of units.

No. 2.—*Specific Resistance of Insulating Materials.*

Derivation of the formula for calculating the *specific* insulation resistance.—Mr. Werner Siemens obtained the same formula which Professor William Thomson arrived at in a very elegant manner in a more simple way.

If $d\,x$ represents the thickness of a differential cylinder at the distance x along the length axis of the cable, its resistance will be

$$d\,w = \frac{d\,x}{2\,\pi\,\lambda\,l\,x}$$

and the whole resistance equal to

$$W = \frac{1}{2\,\pi\,l\,\lambda} \int_r^R \frac{d\,x}{x} = \frac{\log \frac{R}{r}}{2\,\pi\,l\,\lambda}$$

No. 4.—*Charge and Distribution along the Wire.*

Let A B (Plate 1, Fig. 4) represent a given length l of uncoiled cable, the end B of which is to earth, and A C the electro-motive force E of a battery, one pole of which is in connexion with A, the other pole being to earth. Then, according to the laws of Ohm, supposing the cable to be of equal section and conductivity throughout, the curve of the electro-motive force at any point along the line is indicated by C B.

In 1849 (see "Poggendorff's Annalen"), Werner Siemens proved that when a current is sent through a submerged cable, a quantity of electricity is retained in charge along the whole surface, being distributed proportionally to the tension of each point. Thus, the tension of the electricity on any small intermediate given length, $d\,x$ of the conductor at the distance x from A may be represented by y; the quantity of electricity $d\,q$, by which the outside cylinder $d\,x$ is charged according to the formula given previously for induction in cables is :—

$$d\,q = y\,K = y\,2\,\frac{I\pi dx}{\log.\frac{R}{r}}$$

This quantity of electricity $d\,q$ has to pass through the resistance of section x in order to arrive at $d\,x$.

The resulting current develops $d\,q$ in the time $d\,t$, and we have accordingly the equation—

$$d\,q = \frac{Edt}{\frac{x}{r^2\,\pi\,\lambda}} = \frac{E\,r^2\,\pi\,\lambda}{x}\,d\,t$$

By equating these values of $d\,q$, a differential equation is obtained :—

$$\frac{y\,.\,2\,I\,\pi\,d\,x}{\log.\frac{R}{r}} = \frac{E\,r^2\,\pi\,\lambda}{x}\,d\,t$$

by substituting for y its equivalent :—

$$E : y = l : l - x$$

$$y = \frac{E\,(l-x)}{l}$$

into the differential equation, we obtain :—

$$d\,t = \frac{2\,I}{l\,.\,r^2 \log .\frac{R}{r}\,.\,\lambda}\,.\,x\,(l-x)\,d\,x$$

$$t = \frac{2\,I}{l\,.\,r^2 \log \frac{R}{r}\lambda}\,.\int_0^l x\,(l-x)\,d\,x$$

$$= \frac{2\,I}{l\,.\,r^2\ log \frac{R}{r}\lambda}\,.\left(\frac{l^3}{2}-\frac{l^3}{3}\right)$$

$$t = \frac{I\,.\,l^2}{3\,r^2\,\lambda\,.\,log \frac{R}{r}}$$

DESCRIPTION OF A MACHINE FOR COVERING TELEGRAPH WIRES WITH INDIA-RUBBER.

By Mr. C. William Siemens, of London.*

A submarine telegraph cable is composed of three essential parts :—1st, the conductor, which generally consists of a strand of seven copper wires twisted together, to give it strength and pliability; 2nd, the insulating coating, which consists almost without exception of several coatings of gutta-percha put on while hot and in a semi-fluid state by means of piston and cylinder machines analogous to the presses used for making lead pipes, with intervening coatings of a bituminous compound, called Chatterton's mixture, to establish a more intimate union of the different layers of gutta-percha; 3rd, the sheathing, which is added to protect the insulated conductor and to give strength to the cable, and consists generally of a hemp serving and a spiral covering of iron or steel wire.

Respecting the conductor, it is important that it should consist

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* Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1860, pp. 137–146.

of the best conducting material, in which quality pure copper far surpasses all but some of the precious metals and possibly pure aluminium. If the conductivity of silver is expressed by 100, that of pure precipitated copper may be taken at 90. The conductivity of the copper of commerce varies, however, between extraordinary limits; and it may be accepted as a rule that all foreign matter contained in it, whether metallic or otherwise, diminishes its conductivity. Thus 2 per cent. of alloy is known to reduce the conductivity of copper from 90 to 13, and even the best selected copper used for telegraph conductors varies in practice as much as 20 per cent. in conducting power. The foreign substance which it is most difficult to remove from the copper is oxygen; and a process to effect this would be of considerable value.

The insulating covering of the conductor is the most delicate and essential part of the telegraph cable. It has to form an effectual barrier against escape of the current throughout the whole length, for a single flaw in this coating causes the failure of an entire cable. Nor does a flaw show itself always in testing cables, however thoroughly, previous to their submersion; for experience has proved that flaws are produced gradually by the chemical action of the galvanic current itself in any places where the thickness of insulating coating has been considerably below the average, either owing to an air bubble forced open by the pressure of the water, or owing to an eccentric position of the conductor. The latter defect may be produced either in the covering machine, or afterwards by exposure of the cable to the heat of the sun, or to a strain producing a permanent elongation of the copper; in consequence of such elongation the gutta-percha endeavours to return to its original length and causes the copper core by degrees to assume a serpentine position in the covering. Gutta-percha was till lately thought almost a perfect non-conductor of electricity; but in dealing with long lines of submarine electric telegraph its conductivity has become well established, and is often a source of painful anxiety to the electrical engineer, obliging him to search for other insulating materials. Glass and other vitreous substances, which possess the highest insulating properties, are of course inapplicable; and amongst the resinous insulators there is none that combines insulating quality with tenacity and other desirable mechanical properties in so high a degree as india-rubber. The accompanying table shows the respective non-conducting or insulating power of gutta-percha, india-rubber, and Wray's mixture, which last is a compound of india-rubber with shellac and pounded flint; and of the two latter substances combined:—

SPECIFIC NON-CONDUCTING AND INDUCTIVE POWER OF GUTTA-PERCHA, INDIA-RUBBER, &C.

	Specific Non-conducting Power.			Specific Inductive Power.		
Temperature Fahrenheit	52°	72°	92°	52°	72°	92°
Gutta-percha	3·01	1·20	0·38	1·00	1·00	1·00
India-rubber	50·70	45·10	27·60	0·68	0·62	0·70
Wray's mixture	23·60	26·00	38·40	0·77	0·63	0·96
Combination of india-rubber and Wray's mixture .	38·40	49·55	38·40	0·77	0·78	...

The great superiority of india-rubber and its compounds over gutta-percha in insulating power is at once apparent, india-rubber itself being 16 times better than gutta-percha as a non-conductor at a temperature of 52°, and 70 times better at 92°; and the combination of india-rubber and Wray's mixture is on the average as good a non-conductor as india-rubber, while its inductive power, which causes retardation of the electric current in its passage along the wire, is only three quarters that of gutta-percha. To these advantages the greater tenacity of india-rubber and its greater power to resist heat have to be added.

India-rubber was tried for the purpose of insulating telegraph conductors more than twenty years ago, when it was employed by Jacobi of St. Petersburg for underground telegraphic lines. In 1846 Dr. Werner Siemens employed it for the same purpose, previous to his application of gutta-percha. About the same time india-rubber was put to the same use in this country, and it is said remains still in good condition in Portsmouth harbour. There is nothing new therefore in substituting india-rubber and its compounds for gutta-percha in insulating submarine or other telegraph conductors: the present paper has special reference to a new method of effecting the covering. The method hitherto adopted consists in cutting the india-rubber into strips, and winding these strips spirally upon the wire to be insulated: a tedious and expensive operation, which has to be repeated several times to afford any security that the water is entirely excluded from the wire. The insulation of the wire depends in fact upon a perfect joint being formed throughout between the strips; for it is evident that where the strips overlap a spiral channel is formed, which if penetrated in any one place will allow the water to spread till it may chance to find a transverse passage into the spiral channel of the next lower coating, and so forth until it reaches the wire. Formerly the layers of india-rubber simply touched one another, and could readily be displaced; but lately a process of soldering the spiral layers has been introduced by Messrs. Silver, which greatly increases the security of the coating, although it does not remove the objections to the spiral channels which must always be formed in lapping. This process of soldering consists in exposing the covered wire to boiling water for about half an hour, when a most perfect cohesion between adjoining surfaces is produced. The india-rubber so treated adheres to the fingers, or feels sticky; it also loses part of its elasticity and strength. It may therefore be inferred that the heat produces some chemical alteration in the material, changing the gum into an oil. It has been observed that india-rubber so heated has gradually changed bodily into a viscid liquid, where it is in contact with the metal conductor, so as to render it unsafe to be used.

The method of covering which it is proposed to substitute for the above combines the advantages of comparative cheapness and certainty of result with that of rendering the application of heat unnecessary. The operation is based on the well-known adhering property of india-rubber, when two fresh-cut surfaces are joined together under considerable pressure. The mechanical problem consisted in the construction of a machine which would draw the india-rubber tight upon the wire, so as completely to exclude air; and would then cut the india-rubber at the proper inclination, and join the fresh-cut edges together at the same instant under a sufficient pressure to make the joint perfect.

The machine finally arranged for this purpose is shown in Figs. 1 to 5, Plates 2, 3, and 4, one quarter full size. Fig. 1,

Plate 2, is a side elevation ; Fig. 3, Plate 3, an end elevation partly sectional ; and Fig. 5, Plate 4, a plan partly sectional.

The machine consists of two grooved pressing rollers A and B, Figs. 1, 2, and 3, Plates 2 and 3, and of two cutting or shearing rollers CC, all of which are of hardened steel, and are shown enlarged to half full size in the section, Fig. 4, Plate 3. On each side of the groove in the pressing rollers A and B is a small cylindrical portion, as shown enlarged to double full size in Figs. 9 and 10, Plate 5, of a breadth equal or nearly so to the thickness of the intended coating to be applied ; but these cylindrical sides must be slightly rounded off towards the groove and sharp on the outer edge, as shown at DD. The cutting rollers C are so placed on each side of the grooved rollers that in turning round their cutting edge crosses the edges of the grooved rollers a little before the centre line of the machine, as shown double full size in Figs. 7 and 9, Plate 5, at a point where the distance between the edges of the grooved rollers is about equal to half the thickness of one of the strips of india-rubber used. The axis of the cutting rollers is slightly inclined to the axis of the grooved rollers, as shown in the end elevation, Fig. 3, and plan, Fig. 5 ; so that being pressed against the latter by means of set screws, they only touch hard at the shearing point, as seen in Figs. 9 and 10, Plate 5. The wire to be covered and the two strips of india-rubber for covering it are guided into the machine by suitable guides E, Figs. 1 and 5. The two strips in closing upon the wire are drawn tight over it by the inner edges of the grooved rollers A and B ; and being caught between the closing cylindrical portions of the grooved rollers, are compressed to one-fourth their original thickness, the material being forced outwards from the middle ; the cutting rollers C then suddenly intersect them, as in Fig. 9, Plate 5, cutting off the superfluous breadth of strips, and at the same time preventing further escape of the material towards the sides. As the edges of the grooved rollers continue to close upon one another, the material remaining between them can only escape inwards, by which means the two fresh-cut edges are brought one upon the other under a heavy rolling pressure, from which they glide inwards towards the groove, as in Fig. 10, and in so doing form a complete and permanent joint, Fig. 11. In order to effect several successive coatings, a train of machines is provided, as shown in Fig. 6, Plate 4, so placed that the wire to be coated passes in a straight line through them all, receiving in each successive machine an additional coating, with the longitudinal seams at right angles to those of the previous and succeeding coatings, as seen in Fig. 11, Plate 5, which is effected by the different angular positions in which the machines are placed. The last machine in the train is supplied with strips of cloth or felt covered with india-rubber, which is also capable of being joined by compression of the fresh-cut edges, and is extremely useful in adding firmness and protection to the insulated conductor.

This machine is also applicable, with certain modifications of details, for covering wire with the compound of india-rubber, shellac, and pounded flint, known by the name of Wray's mixture, which possesses in common with india-rubber very remarkable insulating properties. The machine is also applicable, with great apparent advantage, for the manufacture of india-rubber tubes, and for several other similar purposes. In producing tubes by this process, a spiral or tube of wires is first prepared, which is coated with india-rubber in one or several layers, with or without intermediate layers of canvas previously coated with india-rubber. The spiral wire is then either withdrawn or left to support the tube, which is finally subjected to the vulcanising process.

In order to produce a submarine cable, an outer covering is required for protection and strength. Instead of the ordinary hemp serving and iron sheathing, the author proposes to saturate hemp yarn with a cement consisting of ordinary marine glue mixed with a certain proportion of pitch and shellac, applied to the yarn in a fluid state and under pressure so as to penetrate the fibre completely. Two or more layers of this yarn are put upon the insulated conductor by means of a train of machines, which cause each strand to be drawn tight uniformly, and to pass separately through a heated chamber, so as to soften the cement and unite the yarn in complete layers upon the core, winding alternately right and left. The covering thus produced combines great tensile strength and lightness with the power to exclude the sea water from the core. It thus adds very considerably to the insulating coating, whereby the retarding effect of induction is greatly diminished; and forms a thorough protection to the more tender coating of highly insulating material. The necessity for a metallic sheathing is however not entirely avoided, in order to afford protection against abrasion and against marine animals; and this sheathing is proposed to consist of very thin brass or iron wire wound on in the form of a tight lapping while the cement is still soft, so as to be imbedded completely in it. The cable is then drawn through a hot die, which causes the superfluous cement to cover the wires completely and to preserve them from rusting.

A cable so prepared combines the qualities essential for crossing deep and broad oceans. Its specific gravity will not exceed 1·5, which experience has proved to be the most desirable weight for submersion, and its tensile strength is such that it will support 15 miles of its own length in sea water, instead of only 3 miles, which is the length an ordinary iron-sheathed cable will support. The sheathing of this cable will not be acted upon by sea water, and will retain its full strength therefore in case it should have to be taken up for repairs: it will not be liable to form kinks, which are fraught with danger to the insulation. The chief advantage however is supposed to reside in the insulating coating, which consisting of a succession of perfect tubes of the most highly insulating and tenacious material known, unaltered by heat or solvents and thoroughly protected against external injury, offers the greatest chances for permanent efficiency that could well be realised. For shore ends this cable should receive an additional external covering of strong wires to resist the effects of anchors and violent abrasion; and these wires in their turn should be covered with saturated fibre to render them durable. The experience with long submarine cables has hitherto been anything but satisfactory; but there is in the writer's opinion no reason to prevent their being made very permanent and valuable property, if only the experience now gained is turned to good account.

Mr. Siemens exhibited the machine in action, covering pieces of wire with india-rubber, showing that the joint made by rolling the two fresh-cut edges together under a heavy pressure was so strong that the india-rubber covering would tear at any other part as readily as at the joint. He showed also a number of specimens of the different descriptions of telegraph cable now in use. The process of joining the strips of india-rubber by the machine depended on the well-known property of india-rubber, that when two perfectly clean fresh-cut surfaces were pressed together with great force they would unite as completely as two pieces of iron welded together. After many trials for effecting this by machinery, he had now succeeded perfectly with the machine exhibited, in which the two cut edges made by the cutting wheels on each side were instantly pressed together between the pressing rollers and joined without having been ever exposed to the atmosphere. This was the essential point in the machine, as any exposure of the cut surfaces however momentary interfered with the perfection of the joint. In putting on a series of coats of india-rubber for making telegraph cables, a train of machines was employed through which the wire was passed in a continuous line, the joints in each successive covering being in a line at right angles to those in the previous covering, which gave a greater security against failure at the joint.

This insulating covering had been subjected to severe tests, and proved highly satisfactory and superior to any other mode of insulation. Gutta-percha, which had hitherto been the material used for covering telegraph wires, was a good non-conductor; but its resistance to the passage of an electric current was only relative, like that of all other insulating materials, and it would conduct to a certain extent, the conducting power being about 3 trillion times less perfect than that of mercury, which was adopted as the standard of comparison. But india-rubber had much less conducting power than gutta-percha, being 16 times better as a non-conductor at a temperature of 52°, and 70 times better at 92°. The insulating power of india-rubber was moreover less affected by difference of temperature than was the case with gutta-percha; and in the combination of india-rubber and Wray's mixture, which he had produced, the average insulating power was not less than that of india-rubber, while it was to a less extent affected by change of temperature. Before, however, a current of electricity could pass along the wire, it had to induce a statical charge in the insulating material throughout the whole length of the wire, as in a Leyden jar; and the delay or retardation thus produced depended on the inductive power of the insulating material, which was independent of its insulating or non-conducting power, but affected by its thickness, the inductive power diminishing as the thickness was increased. A thicker coating of the insulating covering therefore offered less resistance by induction to the passage of an electric current, and allowed of more rapid speaking. In this respect also india-rubber and its compounds had an advantage over gutta-percha, its inductive power being about three quarters that of the latter.

In the use of gutta-percha as the insulating material, a great amount of care was necessary in the process of coating the wire, and there was great risk of imperfection in the covering. In the submarine telegraph between Rangoon and Singapore, the cable was very good for many miles, but a point was then found to exist where the insulation failed from a defect in the original construction of the gutta-percha coating; and such defects were liable to arise in the manufacture from various causes. In covering the wire the gutta-percha was squeezed forwards in a semifluid state through the die, by means of a piston in a cylinder; and air bubbles were liable to get enclosed within its substance, which were so minute as not to be detected at the time of manufacture, though the cable was tried under a pressure of 600 to 1000 lbs. per square inch; but they were sufficient to impair the insulation at the part where they occurred, and ultimately cause the failure of the cable. Moreover, the manufacture was a hot process, as the gutta-percha had to be kept soft in coating the wire; and if a slight delay took place in the operation, the gutta-percha was too much softened at that part, and the weight of the wire cable itself made the coating thinner on one side than the other, so that the insulation was defective; the electric current afterwards sent through the wire was constantly leaking out more or less at the imperfectly protected parts, and caused a chemical action on the gutta-percha, gradually decomposing it at the leak and increasing the amount of leakage. If the finished cable were allowed to lie for only a quarter of an hour exposed to a hot sun, it would be completely spoiled, as the heat would soften the covering and the core would take an eccentric position by sinking through the gutta-percha by its weight; and in the event of a strain coming on the cable in laying it, the copper core being non-elastic, would remain permanently stretched, while the gutta-percha would be constantly endeavouring to regain its original length, forcing the copper by degrees into a serpentine curve. These difficulties had at present caused failures to a greater or less extent in all submarine cables constructed with gutta-percha. But in the process now described it was expected that the chances of failure through defects of manufacture would be much diminished, as there was less liability to accidental imperfections in the work, and the durability of the cable was not affected by the temperature to which it was exposed.

The *Chairman* asked what would be the difference in cost per mile between the new cable and one covered with gutta-percha.

Mr. Siemens replied that for equal efficiency, or the same speed of speaking, the new cable would be the cheapest, because a thinner coating of india-rubber would be sufficient to produce an equal insulating effect; but if estimated by weight, a gutta-percha cable would be the cheapest on account of the greater cost of india-rubber. The first cost of the cable was, however, a secondary question, the great object being to obtain a cable that could be depended upon for a number of years. In a gutta-percha cable, if the covering were thin at any one place, then each successive current passing along the wire produced an alteration, since the gutta-percha conducted at the leak by decomposition of the water contained in its substance; and this action gradually disintegrated its substance and destroyed its insulating power at that part, so that the electric current soon made its escape there.

The *Chairman* asked how long the new cable would last at work.

Mr. Siemens replied that there was not one of the new cables laid at present, and it required that several hundred miles should have been down for some years in order to show practically its durability in work; but some miles had been made and tested with very satisfactory results, and there was good reason for expecting this construction of cable would prove far more durable than those hitherto laid.

In the discussion of the Paper

"ON THE MAINTENANCE AND DURABILITY OF SUBMARINE CABLES IN SHALLOW WATERS,"

By W. H. Preece,

Mr. C. W. Siemens * felt more than usual difficulty in approaching the subject, because the paper, although dealing only with the phenomena which presented themselves in the treatment and management of some short lines of telegraphic cables, opened for discussion a branch of science, which embraced many others, from chemistry to naval architecture.

He had engaged, on the part of the contractors, to superintend the electrical condition of the Channel Islands cable, during its submersion, and also to arrange the instruments of the line. At the time the cable was laid, nothing could be more satisfactory than the results it afforded. The electrical condition was, considering the state of perfection then arrived at, very satisfactory; the instruments acted with the greatest facility, and with very low battery power, and he took this opportunity of stating that he considered it an essential point to save cable as much as possible from the strain of great battery power. The paper dealt, more particularly, with the mechanical accidents that occurred to the cable, upon which he would, in passing, make a few remarks. The route, as was justly stated, was not well chosen; it would have been better, no doubt, had it passed direct from the Isle of Wight to Guernsey; but he was under the impression that the choice of the route was not left to the contractors, but that it was, as had generally been the case, determined by the company, in concert with the Government. He also agreed with the author, that the shore ends had, generally, been made too light, and that the specimens he exhibited presented far greater resistance to wear and tear. But Mr. Siemens had adopted the plan, when laying electric wires across rivers or bays, of inclosing them in a

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XX. Session 1860–1861, pp. 53–60, 72 and 90.

succession of tubes, connected together by universal joints. The plan was more advantageous than using a strong cable, for each tube took a firm position upon broken ground, allowing the cable inside to make its own serpentine curve; whereas a strong cable would, owing to its elasticity, always be moving between its supports, and be thus exposed to continual abrasion.

Passing to the larger work of the Red Sea cable, he would first explain the position in which he stood with regard to that undertaking, or rather the position of the firm of Messrs. Siemens, Halske & Co., of which he was a partner. They were employed to superintend the electrical condition of the cable during submersion. Unfortunately they had not had an opportunity of examining the cable regularly until it was on board ship; and it was one of the most prolific causes of failure that cables were not thoroughly tested under water before they were deposited in the ocean. In laying the Red Sea cable faults occasionally occurred, which, by the system of testing adopted, were instantly detected and rectified; but none of these would have happened if the cable had been previously immersed. When the operation was completed, it proved, like most lines when just laid, very successful. The telegraph was worked from Alexandria to Aden, a distance of 1200 miles, with double relay stations at Kosseir and Suakim, at the rate of ten words per minute; and the general condition of the line was such as must be pronounced to have satisfied the terms of the contracts. There were, no doubt, a few embryo faults, which, by their system of testing every five minutes during submersion, they were enabled to trace, and to map by means of diagrams, representing the copper and gutta-percha resistances. The times of observation were not left to the discretion of those on land and on board the ship from which the cable was payed out, but they were prescribed by a peculiar clock-work arrangement, which reduced the work of the observer to a simple registration, and obviated much uncertainty and delay in these operations. The line was in a satisfactory electrical condition when laid, and he believed it might have been worked successfully for a considerable space of time, if a permanent system of daily tests and of timely repairs had been at once established. The author had mentioned some of the difficulties with which the cables had to contend, and the injuries to which they were exposed: but, probably, he had not had an opportunity of watching the effects of tropical heat, or of metallic veins at the bottom of the sea, which also tended to destroy them. He could refer to several cables which had remained perfectly sound for a certain distance, but had been, in certain places, so completely corroded, that in attempting to repair them, they literally fell to pieces. Such had been the case as had been already mentioned with the Atlantic cable. It was too much the fashion to regard a cable when once laid down as of indefinite durability, and in most cases no sufficient means were adopted to test it at regular intervals. No means, for instance, were provided for effecting repairs in the Red Sea cable as the necessity might arise, and under those circumstances it was surprising that it should have lasted for nine months before the first fault occurred, it having given way only the day before the extension was completed to India. Upon the return of the expedition engaged in laying the cable, it was the general opinion that energetic measures should be adopted for its maintenance. The neglect of this might, in a great measure, be attributed to the diverse interests of the several parties concerned. There was the Government who had given an absolute guarantee to the company which had the management of the line, without being sufficiently interested in maintaining it in good working condition ; there was the contractor who had fufilled his engagement when once the line was successfully laid ; there was the pioneer who had laid down the direction it should take, but who had not had sufficient opportunity of testing the nature of the bottom ; there was the engineer who superintended the making and submerging the cable on behalf of the company ; and finally, there was the electrician, who had, probably, the most anxious and trying work of all, but the importance of whose office, had not, he thought, been sufficiently considered in making the general arrangements. The necessity of adopting a better system was, however, beginning to be acknowledged; in proof of which he would instance the Government cable about to be laid between Rangoon and Singapore, where an opportunity had been afforded, for the first time, of carrying out a complete system of testing, before the cable was shipped.

The method of testing employed by Messrs. Siemens differed essentially from those hitherto adopted. He would not, at present, enter upon the mathematical part of this subject, which was very intricate, but would confine himself to giving an outline which would sufficiently show the relative advantages of the system, referring those who might feel more interested in the subject to a paper by his brother and himself read in 1860 before the British Association at Oxford.* The old system was to test the insulation by the galvanometer, and to judge the condition of the line by the angle of deflection and the battery power employed. This was unsatisfactory, for the angle of deflection of an instrument was never the same for two consecutive days, nor could an instrument be constructed with a constant amount of deflection for the same current; there was, therefore, no means of comparing results. If a mile of cable was measured by one instrument, and several miles by another, or by the same instrument the next day, no useful comparison could be made. But Messrs. Siemens adopted the method of expressing the conductivity of the insulating coating as well as of the conductor by certain units of resistance. The unit adopted by the author, and which might suffice for the particular case mentioned in the paper, was the mile of No. 16 copper wire. It resulted, however, from the investigations of Dr. Matthiesen that the copper of commerce varied in its conductivity, between the limits of 100 and 7; in speaking therefore of the resistance of a mile of copper wire, no distinct estimate of its value could be formed. The unit developed by his brother, and which had since been adopted by them in their operations, was the resistance of a column of pure mercury of one metre in length, and one millimetre in sectional area; this unit possessed, over others, the advantages of being invariable and of being easily reproduced.

Coils of resistance were next formed of German silver wire, representing respectively, units, tens, hundreds, thousands, and tens of thousands of units of resistance. By introducing these variable resistances into the three sides of a Wheatstone's bridge, or electric balance, the resistance of the fourth side, which was the gutta-percha or copper conductor of the cable under examination, could be ascertained with the utmost certainty, the limit of error

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* Vide "Outline of the Principles and Practice involved in dealing with the Electrical Condition of Submarine Electric Telegraphs," by M. Werner and C. W. Siemens, in the Report of the Joint Committee appointed to Inquire into the Construction of Submarine Telegraph Cables. Folio. London, 1861, p. 455, and pp. 47–65, *ante*.

not exceeding, practically, one in one thousand. It was desirable, sometimes, to determine fractions of units in measuring copper conductors ; and at others millions of units in measuring the gutta-percha resistance of a short piece of cable, to accomplish which the apparatus could be modified in different ways. Another feature of this method of testing consisted in the close observance of the time during which the electric current was allowed to act before the observation was taken. This was of the utmost importance, in order to obtain results that could be relied upon, for the conductivity of gutta-percha was changed, even for days, by the application of electric currents. Their method of ascertaining the inductive capacity of cables was also peculiar, being based upon their discovery that inductive tension, in passing from the conductor through the insulating covering, followed the simple law of Ohm regarding electric condition, and admitted, therefore, of being subjected to the same precise methods of measurement. Although this system of testing cables had not been long in use, resistance coils had been employed by them since the year 1849 for determining the position of faults in subterranean lines.

In the case of the Rangoon cable each mile of core was tested after submersion during twenty-four hours in water at a temperature of 75°. Comparative testing would be useless unless made at the same temperature, because the conductivity of gutta-percha increased in very unequal ratio with the increase of temperature. After submersion the cable was placed in Reid's pressure tank in order to discover the existence of any cavities in the covering, but the pressure that could be applied was insufficient to force the water into the cavities of the lower coatings. The results of the electrical tests were then noted in tables reduced to units of resistance per nautical mile. Having thus obtained a complete record of the copper and gutta-percha resistances of each mile of cable, it was sent to the wire-works to be covered with hemp and iron. By this complete record or table it would be possible to detect the slightest fault, where lengths of the core equal to, say one hundred miles, had been joined together ; the copper resistance should not, in that case, exceed the sum of all the resistances contained in the table, due allowance being made for change of temperature, whilst the resistance of the gutta-percha should not be less than the sum of the resistances divided by 100. If it varied it was a sure indication that there was some defect, which, after the cable was laid, would probably develop itself into a fault. But the value of these tests extended much further, if either during the laying of the cable, or afterwards, any slight decrease of insulation occurred, it would at once show the existence of a slight fault, although the line, if measured by others unacquainted with the previous tests, might appear perfect. The position of that fault should be immediately determined, and be carefully watched from day to day. In fact complete records of the condition of the cable ought to be telegraphed each day, or each second day, to the chief superintendent of the line, in order that he might be able to direct timely operations of repairs. So long as there was a single fault in the line, they could by their methods of testing, find out its position with the greatest certainty.

It had been originally intended that the Rangoon cable should be immersed in water during its entire progress. After having been tested at the gutta-percha works it was to have been placed at the contractor's works in tanks, leaving them only for the short space of time necessary for passing the cable through the machine. From these tanks it was eventually to have been coiled into others on board the ships, in order that it might be payed out from water into the sea. But the tanks of the contractor were unable to support the great pressure of water, and thus the cable became exposed to atmospheric influences. It was soon observed that there was a loss of insulation, indicating an increase of temperature, which eventually became so great that mist was seen to arise from one portion of the cable and it became necessary to pour water over it. Thereupon the Government requested Professor Miller to investigate the subject chemically, and they called upon Mr. Siemens to make a report on the electro-thermal phenomena. It was requisite for this purpose to test the temperature of every part of the coil, for which Mr. Siemens devised a peculiar thermometer constructed upon the principle of the resistance of copper wire to the electric current, varying, in a fixed ratio, with the changes of temperature. It consisted of a rod or tube of metal, round which were wound several layers of fine wire covered with silk; and the whole was hermetically sealed with india-rubber and gutta-percha to prevent the access of the water. The two ends of the wire were then brought in contact with the instrument for measuring resistances. Supposing the coil to have been adjusted to represent at zero 100 units of resistance, then for every 1° Fahrenheit the resistance would increase by 0·4 of a unit. The advantages of this thermometer were, that whilst it could be placed at almost inaccessible points, it could be read at all times with great accuracy. In coiling the cable on board he inserted several of these thermometers at different layers of the coil. The coil remained nearly a week on board without his being able to test it; at the end of that time it was at once apparent that there had been a spontaneous generation of heat. On the 10th November, 1859, the tests of the cable had given 553 millions of units per nautical mile at the temperature of 49° Fahrenheit. On the 21st of the same month, when the cable was first tested on board, the gutta-percha resistance per nautical mile had diminished to 199 millions, showing a considerable increase of heat, unless, indeed, the decrease was due to a fault. On the 1st December it was only 61 millions, showing a further rise of temperature. At the gutta-percha works the standard resistance per nautical mile was 100 millions of units, at the temperature of 75° Fahrenheit. The different resistance thermometers inserted in the cable gave the following temperatures: 84°, 75°, and 62°; thus proving that the heat was unequally developing itself throughout the mass, the highest temperature being about 3 feet below the upper surface of the coil. On the 2nd of December the insulation or gutta-percha resistance had decreased to 54 millions of units, and the temperature had increased about 3° Fahrenheit in every part. Water was then applied to the cable, and after some hours the temperature was sensibly diminished. The cable had, till then, given no external signs of heat; the temperature of the hold itself was not greater than 60°, nor would a mercury thermometer, placed in any part of the hold, indicate a higher temperature; yet, when large quantities of water at 42° Fahr. were poured on, it issued from the bottom of the hold at 72°, corroborating the results of the electrical observations. This occurrence proved that it had been most injudicious not to have carried out the original plan, of having the cable placed in water-tight tanks on board the ships. It also led to the supposition, that the destruction of several previous cables, more particularly the Atlantic cable, which had been coiled wet on board, might, very probably, have been owing to the same cause. If the Rangoon cable, while in its heated condition, had been tested on board the *Queen Victoria*, with the most accurate galvanometer, it would have been pronounced more perfect than any other cable hitherto sent out, because the Red Sea cable gave, at ordinary temperatures, only 22 millions of units, and the Atlantic cable, when reduced to the same sectional area, only 7 or 8 millions of units; whereas the Rangoon cable did not fall below 61 millions. Yet if the heating had been allowed to continue only a few days longer, it was absolutely certain that the gutta-percha would have been softened, and the copper conductor would have sunk in the insulating medium.

A great desire was generally manifested for some improvement upon the present construction of cables; and he believed there was great room for amelioration. An iron cable, without an external covering to protect it against the action of the water, should never be adopted. So far from the iron being an element of strength, it became an element of actual weakness, when the cable required to be raised for repairs. It had frequently been observed, that the iron was oxidized, and in certain parts, rapidly destroyed; and if the ground was uneven, the cable would even then break by its own weight, between the points of support. But the outer covering should not be of hemp, for there had been cases of hemp-covered cables having been completely destroyed by marine animals. As to the cause of the generation of heat in the Rangoon cable, his own impression had been, that it was due to the fermentation of the hemp covering; he was bound, however, to add, that, in Professor Miller's opinion, it arose simply from the rusting of the iron. His own view was founded upon his observations that the resistance thermometers between the coils in contact with the iron, exhibited a less temperature than would follow from the resistance of the copper of the cable itself; showing that the core of the cable was 5° hotter than the spaces between the iron covering. It might be, that both causes had been active in producing the rapid increase of heat which had been observed.

The most important part, perhaps, of the cable was the insulating medium, for which many new substances had been proposed, each possessing some degree of merit. The great disadvantages attending the use of gutta-percha were, that it was readily softened by heat, that it was affected chemically by every current that passed it, and that it frequently contained cavities. The passage of electricity through gutta-percha was due, not to its conductivity, but to a slight decomposition of the water which it contained. The consequence was, that in places where the thickness of the covering was much reduced by any accidental cause, a fault would gradually be produced by the electrolytic action of the currents employed. He was, therefore, a strong advocate of low battery power, so long as gutta-percha was employed for the insulating medium; and his instructions to the electrical staff proceeding to Rangoon were, that not more than 22 Daniell's cells should ever be used. India-rubber possessed a much higher power of resistance to electricity than gutta-percha. Wray's mixture, composed of india-rubber, shellac, and powdered flint, and other compounds of india-rubber possessed valuable properties as insulating materials. He had made some attempts to combine them in a cable, but he should refrain from entering further into this question, for his present object was rather to inquire into the causes of failure of the cables hitherto laid, than to consider the comparative merits of new projects.

In answer to the PRESIDENT, MR. SIEMENS stated, that the Red Sea telegraph was worked between Aden and Suez from the summer of 1859 till February, 1860.

MR. C. W. SIEMENS, in answer to a question from the PRESIDENT, replied, that the testing of the cable had been continued on board the *Queen Victoria*, with results confirming his previous statements. Unless the cable was effectually cooled by pumping cold water over it daily, the heat increased at the rate of about 3° Fahr. per day. Considering that the weight of cable on board that vessel amounted to more than 1,000 tons, it was evident that the amount of heat generated daily was considerable, and that very effective measures would have to be adopted if it was to reach its destination in safety.

MR. C. W. SIEMENS, in answer to the PRESIDENT, said that recently, the *Queen Victoria*, which carried the Rangoon cable, had been wrecked, and the hold being filled with water, the cable was now cool. It was about to be transferred to other vessels, and he should again carefully watch it. The temperature, at present, had descended to below 60°, and the insulation was very perfect. His experience had not been the same as that of a previous speaker, for he had invariably found, that after a cable had once been heated, it never returned to the same perfect state of insulation as before ; implying that some slight change had taken place in the constitution of the gutta-percha, which had not, hitherto, been well ascertained. He would also observe, that less tar than usual had not been used in the Rangoon cable, for the sake of facility in testing ; if it was made drier than other cables, it was for a reason entirely disconnected with the department of the electrician.

ON A NEW RESISTANCE THERMOMETER.

By C. W. Siemens.*

To Professor John Tyndall, F.R.S., &c., Royal Institution.

3, Great George Street, Westminster, S.W.
December, 1860.

My dear Sir,

You will probably be interested to hear about a very direct application of physical science to a purpose of considerable practical importance, which I had lately occasion to make. Having charge, for the British Government, of the Rangoon and Singapore telegraph cable, in so far as its electrical conditions are concerned, I was desirous to know the precise temperature of the coil of cable on board ship at different points throughout its mass, having been led by previous observations to apprehend spontaneous generation of heat. As it would have been impossible to introduce mercury thermometers into the interior of the mass, I thought of having recourse to an instrument based upon the

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* Excerpt Philosophical Magazine, Vol. XXI. 1861, pp. 73–74.

well-ascertained fact that the conductivity of a copper wire increases in a simple ratio inversely with its temperature. The instrument consists of a rod or tube of metal about 18 inches long, upon which silk-covered copper wire is wound in several layers so as to produce a total resistance of, say 1,000 (Siemens) units at the freezing temperature of water. The wire is covered for protection with sheet india-rubber, inserted into a tube and hermetically sealed. The two ends of the coil of wire are brought, by means of insulated conducting wires, into the observatory, where they are connected to measuring apparatus, consisting of a battery, galvanometer, and variable resistance coil. The galvanometer employed has two sets of coils, traversed in opposite directions by the current of the battery. One circuit is completed by the insulated thermometer coil, and the other by a variable resistance coil of German silver wire. Instead of the differential galvanometer, a regular Wheatstone's bridge arrangement may be employed.

You will readily perceive that if the thermometer coil before described were placed in snow and water, and the variable resistance coil were stoppered so as to present 1,000 units of resistance, the currents passing through both coils of the differential galvanometer would equal one another, and produce, therefore, no deflection of the needle. If, however, the temperature of the water should rise, say 1° Fahr., its resistance would undergo an increase of $1{,}000 \times {\cdot}0021 = 2{\cdot}1$ units of resistance, necessitating an addition of 2·1 units to the variable resistance coil in order to re-establish the equilibrium of the needle.

The ratio of increase of resistance of copper wire with increase of temperature may be regarded as perfectly constant within the ordinary limits of temperature; and being able to appreciate the tenth part of a unit in the variable resistance coil employed, I have the means of determining with great accuracy the temperature of the locality where the thermometer resistance coil is placed. Such thermometer resistance coils I caused to be placed between the layers of the cable at regular intervals, connecting all of them with the same measuring apparatus in the cabin.

After the cable had been about ten days on board (having left a wet tank on the contractors' works), very marked effects of heat resulted from the indications of the thermometer coils inserted into the interior of the mass of the cable, although the coils nearer the top and bottom surfaces did not show yet any remarkable excess over the temperature of the ship's hold, which was at 60° Fahr. The increase of heat in the interior progressed steadily at the rate of about 3° Fahr. per day, and having reached 86° Fahr., the cable would have been inevitably destroyed in the course of a few days, if the generation of heat had been allowed to continue unchecked.

Considering the comparatively low temperature of the surface of the cable, much incredulity was expressed by lookers-on respecting the trustworthiness of these results; but all doubts speedily vanished when large quantities of cold water of 42° temperature were pumped upon the cable, and found to issue 72° Fahr. at the bottom.

Resistance thermometers of this description might, I think, be used with advantage in a variety of scientific observations,—for instance, to determine the temperature of the ground at various depths throughout the year, or of the sea at various depths, &c. &c. In the construction of this instrument, care has to be taken that no sensible amount of heat is generated by the galvanic currents in any of the resistances employed.

By substituting an open coil of platinum wire for the insulated copper coil, this instrument would be found useful also as a pyrometer.

But, finding this letter already exceeds its intended limits, I shall not enlarge upon these applications, which, no doubt, are quite obvious to you.

I am, dear Sir,
Yours very truly,
C. Wm. Siemens.

In the discussion of the Paper

"ON SUBMARINE TELEGRAPHY,"

By Mr. Thomas Webster,

Mr. C. W. Siemens * said he was of opinion that discussions like these did a great deal to spread a perfect knowledge of matters connected with so vast an undertaking as the Atlantic cable. There was no doubt that a light cable could be made to speak. It was a question for the shareholders how quick they wished it to speak, and then it was a question what quantity of material could speak the best. With regard to the outer coating of the cable, he thought that most important. Electricians knew pretty well what could be done with a given material, and there might be different plans of putting it on. Some might be in favour of one material and some of another ; and they knew that with a given quantity of gutta-percha or india-rubber they could obtain insulation, but in deep-sea cables the quantity of outer covering was of great consequence. It was most generally admitted that a heavy cable was not suited for deep waters; and it was also admitted that a sheathing of some sort was necessary in order to protect the insulated conductor, not only in trans-shipment and paying out the cable, but afterwards in protecting it against the inroads of marine animals, or the accidental strains to which it might be exposed in lying on a rough bottom. As to what the best form of covering might be, he supposed the meeting would not agree, because, like most problems, after it had been plainly stated it might be solved in various ways, and most of those who were professionally engaged in those matters would form a rather strong opinion in favour of one form or another. But in meetings like this opinions were brought together, and he hoped to see the great enterprise of the Atlantic cable accomplished by one or various modes. He thought there was plenty of room for two Atlantic cables at least, probably for more. Before he sat down he would only remark that there seemed to be much misapprehension respecting the

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* Excerpt Journal of the Society of Arts, Vol. XI. 1862–63, p. 224.

effects of earth currents upon the working of submarine telegraphs. It would appear, from Mr. Varley's observations on the former occasion, that the disturbing influence of these currents was very great, whereas, in point of fact, they were of no practical importance. The earth was no doubt a powerful magnet, as was proved by the appearance of the magnetic light at the polar surfaces, but no current would result from the terrestrial magnetism, except at the time of any change occurring in its intensity, and it was well known that these changes took place only very gradually. In making the necessary arrangements for the working of the Malta and Alexandria line, he had made no provision against earth currents, and the fact that the battery power in working this line had been limited to eight cells was the best proof that no such provision was necessary.

In the discussion of the Paper

"ON THE ART OF LAYING SUBMARINE CABLES FROM SHIPS," by Capt. J. SELWYN, R.N.

MR. C. W. SIEMENS [*] said they must all feel much indebted to Capt. Selwyn for having brought this subject so fully and ably before them. He (Mr. Siemens) could not, however, go so far as to say he entirely agreed with him in all his statements. The curve made by the cable while being laid was no doubt a very important consideration in dealing with this subject, and he did not agree with Capt. Selwyn that its form was such as he had described it to be. He thought it was capable of demonstration, that when a ship was proceeding at a uniform rate of speed, and paying out a cable of fixed density, the latter must descend in a direct inclined plane. Capt. Selwyn had stated that the moment the cable left the ship it would commence its downward course, at the rate of

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[*] Excerpt Journal of the Society of Arts, Vol. XIII. 1864–65, p. 434.

about two miles per hour ; then, if the ship was going at six miles an hour, the inclination at which the cable would remain would practically be 1 to 3, or if it went at four miles an hour 1 to 2, or if at two miles an hour 1 to 1 ; so that, unless the velocity of the ship changed, the cable must descend nearly in a straight line. Then came the question why it was found impossible in practice to do without a certain retarding force during the operation of laying. While the cable was, as it were, sliding down the inclined plane, the force exerted was so great that, if it were not resisted, it would cause the cable to run out with such velocity as to produce an immense waste of cable. When it got to the depth of 1,000 fathoms the force with which the cable ran out was very great indeed, and it required to be resisted, otherwise twice or thrice as much cable as was required would be paid out. He thought it was of comparatively little importance what method of paying out was adopted so long as it was a safe one, affording the means of varying the retarding force at will. It appeared to him that the great point was to make the apparatus as simple as possible, so that no kinks or other disturbances could arise. With regard to the measure of the retarding force, that would depend entirely upon the specific gravity of the cable and the depth. The laying of a heavy iron-coated cable in 2,000 fathoms water was a difficult and critical operation. One of very small specific gravity might perhaps go out nearly in the upward curve described by Capt. Selwyn ; and if it did so, although there was no retarding power acting upon it, there would be danger that sufficient slack would not be produced at the bottom. Then came the considerations as to the nature of the bottom. If the cable were laid along a great plateau, then moderate slack was sufficient, but with a precipitous bottom, it was difficult to lay out sufficient slack for the safety of the cable. He would mention a case which came within his own knowledge. A cable had to be laid not far from the Spanish coast, and, according to the soundings previously taken, the bottom descended in a slope of about one in four, but it turned out that in reality the shore was very mountainous, and of volcanic nature. At about eleven knots out at sea, there was a deep valley with precipitous sides. The depth of one edge of this valley was about 700 fathoms ; of the valley itself 1,600 fathoms, and of the other edge 900 fathoms, so that the cable was suspended between the two precipices, involving great danger of rupture, which actually did take place shortly after the cable had been successfully laid. In cases where such gulfs were known to exist, the only safe plan was to stop the ship, and allow the cable to run out so as to furnish enough to lie on the bottom at every point, however deep. This was a serious source of danger, against which it was important that every precaution should be taken. Deep sea soundings were not generally taken at sufficiently frequent intervals, and cables were seldom laid in the line of soundings.

ON THE ELECTRICAL TESTS EMPLOYED DURING THE CONSTRUCTION OF THE MALTA AND ALEXANDRIA TELEGRAPH, *and*

ON INSULATING AND PROTECTING SUBMARINE CABLES.

By Charles William Siemens,* M. Inst. C.E.

The subject of submarine telegraphs having been fully discussed at this Institution during the last session,† the author feels some hesitation in again introducing it. But several important circumstances have arisen since then, which render its further consideration desirable. The publication of the "Report of the Joint Committee on the Construction of Submarine Telegraph Cables" has disembarrassed the question of much uncertainty, by providing an impartial and complete record of the principal facts in connection with past experience. The experimental researches undertaken on behalf of that committee have also added considerably to the stock of theoretical information, which was wanting to form a secure basis for further progress; and the successful completion of the Malta and Alexandria telegraph cable is another important

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* Excerpt Minutes of Proceedings of the Institution of Engineers, vol. xxi., Session 1861–62, pp. 515–530.

† *Vide* Minutes of Proceedings Inst. C.E., vol. xx., pp. 26–106.

fact, tending to inspire the public mind with fresh confidence in long lines of ocean telegraph. The author having been employed by Her Majesty's Government, as the electrician to superintend the manufacture and shipment of this cable, can testify to its actual state of insulation, at the different stages of its progress, and to its general superiority as compared with former lines. The methods of testing employed differed essentially from those resorted to on former occasions; and although the system adopted was very much relaxed towards the conclusion of the work, it has contributed, nevertheless, to the establishment of a long submarine telegraph cable, far surpassing former attempts in apparent permanency and in transmitting power.

At the time the Atlantic Cable was manufactured, little was known of the requirements for such a line. The electric conductor was insufficient in size, and its insulation was so imperfect, even before it was shipped for its destination, that its momentary and partial success appears, at present, more surprising, than its subsequent entire failure. Since then, the Red Sea and India telegraph cable has been laid, in the years 1859 and 1860, without permanent success. The size of the conductor and the thickness of the insulating material were, in the latter case, well proportioned to the length of the intended sections of the line, which were not to exceed 600 miles. Some of the sections are asserted to remain in good working condition up to the present time, while others began to give way nine months after they were submerged, from causes which will partly be dealt with hereafter.

The author's connection with the Red Sea and India Telegraph was limited to the period of submerging the cable, when the firm with which he is connected undertook to superintend the electrical supervision and instrumentation of the line, on behalf of the contractors, Messrs. R. S. Newall and Co. Its insulation when laid, was superior to any previously manufactured, although it did not nearly approach the standard of comparative perfection, which has been reached with the same material, in the case of the Malta and Alexandria line. The latter may be said to be the first which was tested systematically during the progress of its manufacture and shipment; and had the same system of tests been continued during the outward voyage, and when submerging the cable, a valuable record might have been obtained, throwing additional light on the remarkable changes to which gutta percha is subject. Enough, however, can be shown to prove the importance of a uniform and well-devised system of electrical tests, to be carried on during the manufacture, shipment, laying, and subsequent working of submarine cables.

Electrical Tests.—The following system of tests was adopted in reference to the Malta and Alexandria (formerly the Falmouth and Gibraltar) cable :—The covered strand of conducting wire, in lengths of one nautical mile, was placed for 24 hours in tanks filled with water maintained at 75° Fahr. They were then removed into one of Reid's pressure tanks, containing water of the same degree of temperature. The coils of wire under operation, being by this time heated throughout to the above-named temperature, were tested both for conductivity and insulation ; and the result expressed in units of resistance, noted down opposite the number of reference peculiar to each coil. A pressure of 600 lbs. per square inch was thereupon applied, and the same electrical tests were repeated. Before the coil under examination was approved, it was required that the copper resistance should not exceed 3·5 Siemens units of resistance, or possess 80 per cent. of the conductivity of chemically pure copper ; and that the gutta percha resistance per knot, at 75° Fahr., should not exceed 90 millions of units, which also corresponds to about 80 per cent. of the highest insulation that can be attained with the best gutta percha of commerce. It was further required, that the insulation should improve when the pressure was applied, which is invariably the case if the coatings are sound. The approved coils of insulated conductor were transferred to the cable works at Greenwich, where they were kept submerged in tanks until the moment when they were required for the sheathing machine. The sheathed cables were coiled into large tanks, where they were intended always to be covered with water ; but, owing to some defect in the construction of the tanks, this regulation could only be partially carried into effect. It was also intended, in the first instance, that the ships should be provided with water-tight tanks, to receive the cable during the outward voyage ; but owing to the passive resistance with which every deviation from previous routine is usually met, these tanks were not provided, until the heating of the cable on board the steam ship *Queen Victoria* had proved, at great cost, that they were absolutely necessary. There are other important advantages obtained through the adoption of the water tanks : without them the electrical tests during the paying out are, to a great extent, illusory, partly on account of disturbances produced by irregular variations of temperature, and partly on account of the impossibility of detecting any fault in the insulating covering, unless the conductor and outer covering are brought into wet contact. If, therefore, faults of insulation occur on board ship, either through partial exposure of the cable to heat, or through an imperfect joint, or through accidental causes, they only make their appearance at the moment when the defective piece enters the sea, or sometimes even days after submersion has taken place. The sudden appearance of such faults has been hitherto of frequent occurrence, giving rise to interruption of the operation of paying out, and entailing great risk by kinks, or bad joints, or of losing the cable entirely when in deep water. In paying out the cable from a wet tank into the sea, these causes of failure are avoided, and the operation is rendered comparatively safe and easy.

In conducting the electrical tests of the Malta and Alexandria cable in the course of its manufacture, the author made it his chief object to obtain throughout strictly comparative results. For this purpose, it was necessary to adopt a standard measure of resistance, by which to express both the conductivity of the copper conductor, and of the insulating covering. This standard measure has been supplied by the author's brother and partner Dr. Werner Siemens. The unit of resistance according to this system is that of a column of pure mercury (contained in a glass tube), one metre in length between the contact cups, and of one square millimetre sectional area, taken at the temperature of melting ice. Of this unit, which can be readily reproduced with great accuracy, multiples are produced in the form of coils of German silver wire, covered with silk. A number of such coils, representing different values of resistance from 1 to 10,000, are fastened separately to a board of ebonite, the ends being soldered to contact pieces, by which means any number of the coils can be joined, so as to form one electric circuit of known total resistance, expressible in units. The testing apparatus is formed of three such scales of variable resistance, a battery, and a delicate sine galvanometer, or instead, where the space admits of it, a Weber's reflecting magnetometer. These are arranged together in the manner of a Wheatstone's bridge or balance, the cable to be measured forming the fourth resistance, or the unknown quantity in the equation, expressing the condition of a balance between the adjusted resistances. By an instrument of this construction electric resistances varying from the one hundredth part of a unit, to one million units, can be measured with great accuracy.

For resistances exceeding one million units, a different method was adopted, in which the resistance was calculated from the deflection of a very delicate sine galvanometer, acted upon by a battery of ascertained electro-motive force. The limits of this paper will not admit of a detailed description of the testing apparatus which was used, or of the mathematical formulæ which were employed in reducing the observations into comparative numerical measurements.*

The diagrams in Plates 6 and 7 represent graphically the results of observations upon two different sections of the cable, taken at various stages of their progress : the one section being between Malta and Tripoli, and the other between Alexandria and Benghazi.† In Plate 7 are shown the tests applied to the parts constituting the Malta and Tripoli section of the cable, with the exception of a few knots, the presence of which has no perceptible influence upon the condition of the whole. In the diagrams marked A A, the insulation tests at the Gutta Percha Works are given. The length of the coils are taken as abscissæ, and the ordinates as resistances at 75° Fahr. The black line connects those taken in vacuo, and the broken line those taken under a pressure of 600 lbs. per square inch. The lower horizontal broken line represents the standard of 90 millions of units at 75° Fahr., to which it was found necessary to reduce the original standard of 100 millions, in consequence of the inability of the Gutta Percha Company to provide sufficient material of such high insulating qualities. The upper horizontal black line and the

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* For further details on these subjects, see "Report of the Joint Committee on the Construction of Submarine Telegraph Cables," Appendices Nos. 7 and 12.—C. W. S.

† These observations are given in a series of Tables which are appended to the original communication, and may be consulted at the Institution.

upper horizontal broken line, indicate the average derived from the observed resistances. Diagrams B B, Plate 7, give the curves of charge, discharge, and loss per minute, the ordinates of the latter curve, when the original charge is taken as unity, expressing in fractions the amount of current passed through the dielectric during the minute. The different diagrams, severally marked C and D, represent the results obtained at the Sheathing Works of Messrs. Glass, Elliot, and Co. ; C represents the insulation, and D the charge, &c., as in the previous cases, but with the difference that there is no test under pressure, and that the abscissæ represent time. The diagram marked E shows the tests of insulation on board ship, and that marked F gives a comparison between the average resistances at 75° Fahr. observed at the Gutta Percha Works, after the sheathing, and finally on board ship shortly before submersion. The abscissæ represent in this case the length of cable. The diagram marked G gives the insulation actually observed during a certain period after submersion.

The results obtained from the cables composing the section between Alexandria and Benghazi, are shown in a similar manner in Plate 6. The diagrams of insulation resistance in Plate 7 illustrate the state of a portion of this section (Cable No. 5), which having been in a defective tank at the Sheathing Works, exposed to atmospheric influence, began to show symptoms of spontaneous heating. The insulation-tests fell low, while the resistance of the copper showed, by the ratio of its increase, that the mass of the cable must have attained a temperature of 80° Fahr. Upon this, the cable was cut into three pieces, which were marked 5 *a*, 5 *b*, and 5 *c*, and tested separately, when it appeared that 5 *a*, the portion which was at the bottom of the tanks, had suffered most, while the two remaining portions still showed the marked effects of a gradual transition from a wet to a dry state.

The three portions of this cable, after being coiled over into another tank, and being kept under water, gradually returned to a normal state of tests, except 5 *a*, which never returned to its former high state of insulation, and which for some time gave rise to doubts, whether the effect of the heating had not rendered it unfit for use.

The ordinates in the diagram, connected by a black line, represent the resistances of insulation. The broken line shows the corresponding temperature in degrees Fahrenheit, calculated from the observed copper resistances; the abscissæ represent time. As might be expected from the limits between which the temperature varied, no material change was perceptible in the inductive capacity of the material.*

In order more effectually to guard against and to discover at an early stage, an undue accumulation of heat in any portion of the coil, the Government electricians placed between the coils, at regular intervals, certain instruments which may be termed resistance thermometers, based upon the principle, that the resistance of copper wire varies in a constant ratio with the temperature. The instrument consists of several layers of insulated wire of a known resistance, at the standard temperature, protected by an iron case, and brought into connection with the testing apparatus. By simply measuring the resistances of these coils, deposited at different places between the layers of the cable, any rise or fall of temperature throughout its mass is easily ascertained, by comparison of the resistance observed with the original standard. This system was found extremely useful, when applied, in observing the generation of heat in a portion of the Malta and Alexandria cable, then on board the *Queen Victoria,* which had been kept dry during twelve days, and had attained a temperature of above 80° Fahr.; although the ordinary mercury thermometers, suspended in different parts of the hold of the ship, only indicated, during the same period, a maximum temperature of 60°.

The diagram of copper resistances in Plate 7 exhibits the tests applied to the copper conductor in the Gutta Percha Works; the ordinates representing the resistances, in Siemens units, per 1,000 yards, of the various coils contained in Cable 6. It will be seen, from this example, that the differences in the specific conductivity of the copper vary up to 16 per cent.

Of the two sections under consideration, the Alexandria and Benghazi cable invites some further comment. It was laid at two

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* For further information on this subject, see the detailed reports by Dr. Allen Miller and Messrs. Siemens, Halske and Co., upon the phenomena of the spontaneous generation of heat in this portion of the cable, which are given in Appendix No. 11 to the "Report of the Joint Committee on the Construction of Submarine Telegraphs."

different times. The tests of the first portion, though appearing rather low in insulation, proved otherwise regular, and tend rather to establish a general decrease of the insulating properties of the gutta percha, than to point to any local defect. In the second portion, however, a fault suddenly appeared, when the cable was coiled on board the *Rangoon* in August, 1861, indicated chiefly by a fluctuating resistance owing to electrolytic action. If several days could have been allowed to the Government electricians to observe and gradually develop this fault, it might have been cut out before the ship left the Thames; but such a delay was deemed unnecessary, inasmuch as the fault, if any, would certainly increase during the outward voyage, and the defective piece could be removed at Malta before the laying of the cable was commenced. On testing the same cable at Malta, it was found that its insulation had actually gone down from 120 millions of units to 3 millions of units; but it rose again to 70 millions of units after the faulty piece had been cut out. This piece has since been stripped of the iron sheathing, when a place about an inch in length appeared, where the gutta percha had been deeply indented, apparently by some weight falling upon it during the manufacture of the sheathing, which did not expose the copper conductor to sight, but which had assumed the character of a fault in coiling the cable from the tanks into the ship's hold.

On comparing the insulation of the cables after being laid down, with the insulation observed shortly before on board ship, there is a decided improvement after submersion. This is partly due to the pressure upon the cables, the insulation improving 2 per cent. on an average, per 100 lbs. of pressure upon the square inch, and partly to the lower temperature at the bottom of the sea. The latter circumstance influences the tests, by the introduction of a factor, the value of which it is difficult, if not impossible, to ascertain, owing to the changeable nature of gutta percha; thus preventing a direct comparison between the last tests on board ship and those after submersion. All that could be expected was, therefore, to find an adequate increase of insulation in testing the submerged cable, as compared with the last test on shore; and that the electrification, both by positive and negative currents, should show the regularity due to a homogeneous condition of the insulating medium. Both these conditions were fulfilled in the case of the Malta and Alexandria cable.

Excepting in the one instance mentioned, no fault appeared in the cable after shipment, and having been laid very carefully, under the direction of Mr. H. C. Forde, the Government Engineer, there is every probability of its continuing for some years in good working condition.

The instruments for working the line were made by Messrs. Siemens, Halske, and Co., under the author's general superintendence. They are ink-recording instruments, fitted with peculiar arrangements for discharging the residuary charge of the cable, and having the peculiarity of being capable of being worked by an exceedingly feeble battery power. One single Daniell's cell suffices for the transmission of messages through each section of the line, and when regularly working, not more than from eight to ten cells need ever be employed. Although the line is divided into three electrical circuits, messages are transmitted mechanically, and instantaneously, at the intermediate station, by a system of double relay, or translation, first introduced by Messrs. Siemens and Halske upon the continent. By this system of working, messages can now be sent instantaneously, from London to Omsk in Siberia, and there would be no electrical difficulty in establishing the same direct inter-communication between London and Calcutta. A detailed description of these instruments would fall beyond the limits assigned to the present paper. The author also passes over an important branch of the general subject, namely the methods and apparatus employed for ascertaining the position of faults in submarine lines, referring those interested to the documents published in the report of the Joint Committee on Submarine Telegraphs.*

The general superiority of the Malta and Alexandria cable over other long submarine lines, previously constructed, cannot admit of a doubt; and it is to be hoped that it may continue in good working condition, both on account of its national importance, and also for the sake of ensuring progress in this branch of engineering science. At the same time, it will hardly be asserted by the most ardent defender of the present form of submarine cable, that its

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* *Vide* "Report of the Joint Committee on the Construction of Submarine Telegraph Cables," Appendix No. 12, &c.

construction is perfect, and should therefore be adhered to in the future. The difficulties encountered in the progress of this work are suggestive, both of the perishable nature of the outer sheathing, and of the tenderness of the insulating medium employed. Past experience has taught, at great cost, that the unprotected iron sheathing will be destroyed by rust, sometimes in the course of a year; that the hemp serving will be eaten by a marine insect * wherever it becomes exposed; and that the gutta percha is apt to develop faults, by slow degrees, wherever an irregularity in the insulating covering, although perfectly harmless at first, has escaped observation in the process of manufacture. Impressed with these views, the author has for some years directed his attention to the construction of a cable of a more permanent character, and he is sanguine that his endeavours have led to some useful results, which he now proposes briefly to lay before the members.

INSULATING MATERIALS.—Respecting the insulating covering, nature seems to have provided only two suitable substances combining permanent pliability, at all ordinary temperatures, with high insulating property, namely, india rubber and gutta percha. India rubber wrapped round the conductor in the form of strips, or bands, was first applied for insulating underground line-wires, by Jacobi, of St. Petersburg, about the year 1840. Gutta percha was applied for the same purpose, in 1847, by Dr. Werner Siemens, in Prussia, partly at the suggestion of the author, the material being put upon the wire in a heated and plastic state, by means of a die. This latter process has since been exclusively adopted for insulating subterranean and submarine wires, until very lately when india rubber has been again put forward, in various forms, claiming a preference over gutta percha on account of its higher insulating power, its lower specific induction, and its power to resist higher temperatures.† On the other hand, gutta percha possesses the important advantage, that it admits of being put upon the wire in a plastic state, by means of a die, thereby producing a homogeneous covering, which may be applied in several layers, and which gives greater security against faults, than the lapped india rubber covering. It is, moreover, harder and stronger than india rubber at

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* The Xilophaga, according to Huxley.

† *Vide* "Report of the Joint Committee on the Construction of Submarine Telegraph Cables," pp. xvii. and xxiii., and also Appendices No. 1 and No. 2.

ordinary atmospheric temperatures, and therefore is less liable to receive accidental injuries. It is not liable to become sticky, or semi-fluid, when exposed to the atmosphere, and lastly, it resists the action of water more perfectly than india rubber.

The absorption of water by insulating materials, including gutta percha, india rubber, and compounds of india rubber, such as vulcanized india rubber, Wray's mixture, and a compound with mica, under various pressures at different temperatures, and from water containing different degrees of salt in solution, is a subject which has been very fully investigated by Dr. Werner Siemens and the author. The results of this inquiry, extending over three hundred days, are partly contained in Appendix No. 7 of the Report of the Joint Committee on Submarine Telegraph Cables, but have been much extended since the publication of that document. The results are graphically shown in a diagram in Plate 7. The ordinates in that diagram show the percentage of increase in weight of the materials examined at different periods after submersion : the abscissæ represent the time of immersion in days. The experiments were made on plates, as nearly equal in size as were procurable—1 millimetre thick, 100 millimetres long, and 50 millimetres broad, of the following materials, namely :—Raw india rubber, unvulcanized block india rubber, india rubber and mica, vulcanized india rubber, and gutta percha. The data given in relation to the latter material are taken from the results already published in the above report. The specimens were immersed in a bath of distilled water, and in another bath containing 5 per cent. of sea salt, both vessels being kept at a temperature of from 60° Fahr. to 70° Fahr.

The following are the principal deductions to be derived from the experiments :—

1st. Increase of pressure up to the limit of 50 lbs. per square inch does not increase the rate of absorption in any of the materials operated upon. Absorption is favoured, however, to some extent, by a vacuum, owing probably to the absence of condensed air upon the surface of the material.

2nd. The absorption is more rapid from pure water, than from sea water ; and more rapid from sea water, than from brine.

Notwithstanding the faculty of absorption of the gutta percha in both sea and pure water, and that of the remaining four materials in fresh water, had not quite attained the maximum after three hundred days' immersion, the relative absorptions of the several materials under examination may safely be assumed to be in the following proportions :—

	In Fresh Water. Per Cent.	In Salt Water. Per Cent.
Raw india rubber . . .	25	3
Unvulcanized block do. . .	23	3·8
India rubber and mica . .	19	3·9
Vulcanized india rubber . .	10·14	2·9
Gutta percha	1·5	1·0

India rubber in its raw and in its unvulcanized state is thus proved to absorb water in greater quantities than the other materials; while, next to gutta percha, vulcanized india rubber shows, both in fresh and salt water, the greatest insensibility to absorption.

3rd. In pure water the rate of absorption with increase of temperature is least influenced in regard to gutta percha, the rate being little more than double for an increase of 39° Fahr. to 120° Fahr. For india rubber the rate is about eight times, and for Wray's mixture about sixteen times greater at 120° Fahr. than at 39° Fahr., the latter being the temperature of water of the maximum density, as found at the bottom of deep oceans. In sea water, india rubber absorbs about double the quantity at 120° Fahr. that it does at 39° Fahr.; for gutta percha and Wray's mixture the rate of absorption is not materially altered by temperature.

4th. On removing the india rubber from the baths, previously to weighing, the surfaces were invariably found to be slimy, an observation which first led to the belief, that india rubber was soluble to a small extent in water. This fact is corroborated, and settled beyond doubt, by the curves of this material depressing towards the ends, as well as by the weights of the specimens, taken after the tests were concluded, showing a decrease from 0·4 per cent. to 1·2 per cent. It is also apparent, that the unvulcanized india rubber is likewise subject to solution in water, but in a less degree. The results of experiments, instituted with a view of ascertaining the dependence of absorption on the thickness of the material, only served to express a law, which might have been foreseen, that thicker plates absorb comparatively less than thinner ones. The difficulty of procuring materials physically equal leaves only a remote chance of arriving at satisfactory results in this respect.

The author considers it important to notice some other experiments, at different temperatures, on the insulation and inductive capacities of wires coated with india rubber, and with gutta percha combined, as compared with those coated with gutta percha, or with pure india rubber alone. The lengths experimented upon varied from 600 yards to 2,500 yards, and consisted of—

1st. Wire covered by common gutta percha.
2nd. Ditto by two layers of special gutta percha alone.
3rd. Ditto by one coat of india rubber and two of gutta percha.
4th. Ditto by one coat of india rubber and one of gutta percha.
5th. Ditto by two coats of india rubber and one of gutta percha.
6th. Ditto by pure india rubber.

India rubber, both as the better insulator and the more absorbing material, should be applied nearest to the core.

The observations, ranging over temperatures between 50° Fahr. and 85° Fahr., are recorded in the columns of the following Table :—

TABLE I.

SPECIFIC RESISTANCE OF GUTTA PERCHA AND INDIA RUBBER, ALONE AND COMBINED, AT DIFFERENT TEMPERATURES.

No.	Materials.	Radius.		$\frac{R}{r}$.	50° Fahr.			85° Fahr.		
		Outer R.	Inner r.		No. of Cells.	Defl.	Spec. Res.	No. of Cells.	Defl.	Spec. Res.
					a	b	c	a	b	c
1	Gutta percha . .	13	1	13	256	3·6	1·45	...	...	...
2	Special ditto . .	0·103	0·033	3·12	86	1·5	9·1	89	1·6	8·0
3	1 coat of india rubber and 2 coats of gutta percha . .	0·131	0·028	4·68	...	...	...	76	0·1	11·9
4	1 coat of india rubber and 1 coat of gutta percha . .	0·109	0·032	3·41	81	0·3	10·1	81	0·3	10·1
5	2 coats of india rubber and 1 coat of gutta percha . .	0·150	0·05	3·0	86	0·5	21·7	86	0·6	17·3
6	Pure india rubber .	7·2	1	7·2	256	1·4	48·9	...	...	...

No.	Materials.	60° Fahr.			65° Fahr.			70° Fahr.		
		No. of Cells.	Defl.	Spec. Res.	No. of Cells.	Defl.	Spec. Res.	No. of Cells.	Defl.	Spec. Res.
		a	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*
1	Gutta percha . .	...	...	...	...	...	...	64	12·5	0·64
2	Special ditto . . .	89	3·0	4·2	91	4·1	3·2	89	3·8	2·5
3	1 coat of india rubber and 2 coats of gutta percha . .	71	0·1	11·3	71	0·2	5·6	71	0·3	3·8
4	1 coat of india rubber and 1 coat of gutta percha . .	81	0·3	10·1	73	0·2	6·4	73	0·3	4·3
5	2 coats of india rubber and 1 coat of gutta percha . .	89	0·9	11·5	91	1·2	8·8	88	1·4	7·5
6	Pure india rubber .	...	...	...	...	...	...	512	4·4	41·3

No.	Materials.	75° Fahr.			80° Fahr.			85° Fahr.		
		No. of Cells.	Defl.	Spec. Res.	No. of Cells.	Defl.	Spec. Res.	No. of Cells.	Defl.	Spec. Res.
		a	*b*	*c*	*a*	*b*	*c*	*a*	*b*	*c*
1	Gutta percha . .	...	...	...	...	...	...	...	...	...
2	Special ditto . . .	90	6·0	1·9	90	8·0	1·5	...	...	...
3	1 coat of india rubber and 2 coats of gutta percha . . .	...	...	...	71	0·5	2·3	71	0·7	1·6
4	1 coat of india rubber and 1 coat of gutta percha . . .	74	0·3	4·3	71	0·3	3·6	...	...	...
5	2 coats of india rubber and 1 coat of gutta percha . . .	90	1·5	7·3	90	1·6	7·2	...	...	...
6	Pure india rubber . .	...	...	...	...	...	...	...	...	...

The subdivisions of the above columns contain, under *a*, the electromotive force employed; *b*, the observed deflection; and *c*, the specific resistance of the material.

Table II. shows the mean specific induction observed on these specimens, that of gutta percha being taken as unity.

TABLE II.

SPECIFIC INDUCTION OF GUTTA PERCHA AND INDIA RUBBER, ALONE AND COMBINED.

No.	Materials.	Radius.		$\frac{R}{r}$.	No. of Cells.	Defl.	Spec. Ind.
		Outer R.	Inner r.				
1.	2.	3.	4.	5.	6.	7.	8.
1	Gutta percha	13	1	13	...	...	1
2	Special ditto	0·103	0·033	3·12	15	37·0	0·75
3	1 coat of india rubber and 2 coats of gutta percha	0·131	0·028	4·68	...	13·5	0·69
4	1 coat of india rubber and 1 coat of gutta percha	0·109	0·032	3·41	...	3·5	0·77
5	2 coats of india rubber and 1 coat of gutta percha	0·150	0·05	3·0	...	20·5	0·67
6	Pure india rubber	7·2	1	7·2	5	12·8	0·66

In the above Table, columns 1 to 5 inclusive give the description of the cable tested; R is the outer radius and r the inner radius of the insulating covering; $\frac{R}{r}$ expresses the ratio; column 6, the tension, column 7, the deflection observed; and column 8, the specific induction. It will be observed that the fall of insulation, with the increase of temperature, is the more apparent and rapid, the greater the proportion of gutta percha in the combination. Thus, the specific resistance of the special gutta percha decreases from 9·11 at 50° Fahr. to 1·50 at 80° Fahr., or to about one-sixth of its original value; while the combination of two coatings of india rubber and only one of gutta percha (No. 5), has, under the same circumstances, only gone down to about one-third of its insulation at 50° Fahr. On pure india rubber (No. 6) the effect of temperature is still less. It was also found, that the inductive capacity of the combined india rubber and gutta percha wire is not greater than that of pure india rubber covered wire, that of gutta percha being equal to 1, that of the combination being about 0·7. It may further be remarked, that vulcanized india rubber, although some of its qualities are in favour of its application as an insulator for submarine cables, still cannot be recommended as being well suited for that purpose, both from its tendency to injure the conductor, by giving off a portion of its sulphur, and converting its surface into a sulphuret, and also from the danger arising from the amount of heat which it is necessary to employ during the manufacturing process.

On the other hand, notwithstanding the comparatively high insulating property of india rubber, its low inductive capacity, and its power to resist heat, its gradual dissolution in sea water is a circumstance which alone renders it inadmissible as an insulator of submarine telegraph wires, unless it is securely enclosed in another waterproof medium. Gutta percha appears in every respect well suited for such an outer covering, being itself sufficiently insulating to improve by its presence the insulation, and still more the loss by induction of the covered wire; applying itself closely to the india rubber; being susceptible of forming secure joints; and of resisting the sea water perfectly.

The mechanical problem of forming a sound india rubber and gutta percha covered wire, presented considerable difficulty. It appeared to the author desirable that the india rubber should be brought upon the wire without the application of heat, or solvents, both of which often entail a gradual decomposition of that material, particularly when it is exposed to atmospheric influence, in contact with copper, or other metallic surfaces. Dr. Miller, in his Report to the Joint Committee on the Construction of Submarine Telegraph Cables,* states that the liquefaction of india rubber is the result of a process of oxidation; from which it may be inferred that the effect cannot take place where oxygen is entirely excluded. It was important, moreover, that the india rubber covered wire should be perfectly cylindrical, or it could not be covered properly with gutta percha in the die. Taking advantage of a peculiar property of india rubber cohering perfectly where two fresh cut surfaces are brought together, under considerable pressure, the author has constructed a machine, by which any number of coverings of india rubber can be applied, in joining strips with longitudinal joints, tightly upon the wire, care being taken that the joints of consecutive coatings are at right angles to each other.

Wires, and strands of wires, so covered with india rubber and gutta percha, with and without intermediate layers of Chatterton's compound, have been under trial under various circumstances, exposed to the atmosphere, to water, or the moisture of the ground, for nearly two years, without betraying any signs of gradual

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* *Vide* Appendix No. 4 to the above Report.

deterioration of the india rubber, or the appearance of sudden faults. A circumstance greatly in favour of a wire covered in this manner is, that the gutta percha shrinks upon the india rubber, and when any mechanical injury to the covering occurs, the yielding india rubber is forced into the gap, by the elastic pressure exercised by the gutta percha, and thus prevents the formation of a fault.

The outer covering of cables, as hitherto constructed, is certainly the least perfect part. An iron sheathing is very necessary to protect the insulated core in shallow waters, where it is subject to tidal currents, and to the dragging of ships' anchors. The error which has been committed is, that wires of insufficient thickness have frequently been adopted. But, for deep-sea cables, that is, for cables in more than thirty fathoms, or, under special circumstances, forty fathoms of water, the iron sheathing is, the author submits, an element rather of weakness than of strength, by rendering the cable ponderous, and its shipment expensive. The paying out is also rendered hazardous, partly on account of the heavy brake-power required, and partly through the risk which would be occasioned, by the breaking of a wire between the ship's hold and the brake-wheel. Repairs also could not be conveniently made, and in some cases they would be impracticable, owing to the difficulty of safely raising a heavy cable from a great depth, under any circumstances, and the impossibility of doing so after corrosion of the iron wire has made any progress. When the Falmouth and Gibraltar cable was first contemplated, the author, in conjunction with Mr. Forde, proposed that each iron wire should be covered with gutta percha, with a view to prevent oxidation ; but the system was not acted upon, except experimentally.*

Mere protection of the iron wire is, however, not sufficient, in the author's opinion, to constitute a good deep-sea cable. It is capable of mathematical demonstration, that in paying out a wire-sheathed cable, with a considerable strain upon the brake-wheel, it will untwist, while in suspension in the water, to a considerable extent, causing elongation of the core to the amount of say one per cent., or even more. On reaching the bottom, the strain and consequent twist will be released, and throw the cable into frequent

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* The results of the experiments referred to are given in the Report of the Joint Committee on the Construction of Submarine Telegraph Cables, Appendix No. 10.

kinks. But it appears, from experiments made at Camden Town by the Joint Committee on the Construction of Submarine Telegraph Cables (Appendix No. 9 of their Report), that copper wire cannot be elongated more than 2 per cent. without receiving a permanent set. It is also a well-ascertained fact, that when telegraph core has been stretched at any time beyond the limits of elasticity of the copper, the latter being henceforth too long for the more elastic covering, will tend to assume a serpentine form, and will push its way through the insulating material by slow degrees, particularly in places where short bends, or kinks, occur.

Based upon these views, the author designed a sheathing of the following description :—The conducting strand of copper wires consists of seven comparatively strong and six thinner wires, which latter fall into the spiral grooves between the former, and produce a near approach to the cylindrical form, presenting the least surface, for a given conducting area. The small remaining interstices are filled up with Chatterton's, or some other, compound before the conductor is covered, first with pure india rubber, in two layers, by the process before described. The india-rubber-covered conductor is carefully tested, and thereupon covered with gutta percha by the ordinary die ; Chatterton's compound being used to solder the two materials. The insulated conductor, or core, thus formed, is passed in the sheathing works through a series of three machines in close succession. In passing through the hollow spindle of the first machine, a close spiral covering of hemp, previously saturated with Stockholm tar, is applied, in such a way that each string is, and remains, under a given strain, which may be adjusted by friction-brakes. The second machine is similar in construction to the first, but it supplies a second covering of hemp, wound in the opposite direction to the first. The rope thus formed passes next through a stationary clip, with longitudinal grooves, to prevent it from turning round, in the operation immediately following. This consists in the application, under the influence of great pressure, of from three to six strips of copper, or other metal, which may best resist the action of sea-water. The strips are coiled upon reels, and are accurately guided into the revolving covering tool, so as to overlap each other equally for nearly half their breadth ; the pressure applied being sufficient to crush, or socket, the one metal down where it is covered by the other. The cable thus formed passes over a capstan-wheel, by which it is drawn through the three machines, notwithstanding the retarding form applied to the numerous hemp strings constituting the strength of the cable. In passing away from the capstan-wheel, the extended hemp strings would naturally shorten to their original length ; but are prevented by the tight grasp of the metal sheathing. For the same reason they are not at liberty to shrink, when the cable is immersed in water.

This cable has no tendency to untwist. Its extension, with half the breaking strain upon it, does not exceed 0·3 per cent., and being very strong, and of only double the weight of water, it will support from 7 miles to 8 miles of its own weight in the sea.

Considering that good ship's sheathing lasts from ten years to twelve years, when the ship is at rest, and that this cable has a double layer of metal, with hardened tar between the layers, it appears not unreasonable to suppose that the sheathing will last, at the tranquil bottom of the ocean, not less than from twenty years to thirty years. Several short lengths of this cable are now being tried, under various circumstances, and the results, so far as they go, promise to be very successful upon a larger scale.

In conclusion, the author wishes to acknowledge the valuable assistance he has received, in preparing the statistical portion of this paper, from Messrs. Loeffler and Deede, electricians in the employ of Messrs. Siemens, Halske, and Co.

The paper is illustrated by a series of diagrams, from which Plates 6 and 7 have been compiled.

In the discussion of the paper,

Mr. C. W. Siemens, after exhibiting the instruments used in testing the cable, which had been described in his paper, said, that no doubt a much higher rate of working through the Malta and Alexandria line could have been obtained, if the object had simply been to transmit the greatest number of words per minute, irrespective of other considerations ; but the principal object had been to produce a safe instrument, which, in the hands of an ordinary working clerk, would transmit without failure the greatest number of messages, with the least amount of battery power. The high temperature of the Mediterranean near the African coast, also rendered the use of a very low battery power a matter of great practical importance. The term "working speed" was often understood in a different manner from that in which he regarded it. He considered it to mean the speed obtained when working ordinarily, and fully spelling all the words with the recording instrument. By using abbreviations, and by dispensing with translations at intermediate stations, the speed might be increased. Since the Malta and Alexandria cable had been laid, further improvements in telegraphic instruments had been made by the firm to which he belonged, by means of which the required conditions were fulfilled, and a higher rate of working was attained.

As regarded the Gutta Percha Company, he wished it to be distinctly understood, that it was not in disparagement of their work that he had stated in the paper, that it was found necessary to reduce the standard from 100 millions of units to 90 millions of units. On the contrary, he thought the company had made extraordinary progress in the manufacture of gutta percha during the last few years. In fact, the gutta percha in the core of the Malta and Alexandria cable, taken specifically, insulated, material for material, several times better than that in any other cable previously manufactured ; but, to give a faithful record of what had happened, it was incumbent upon him to mention the fact that the standard had been lowered. It had been stated that neither gutta percha nor india rubber were soluble in water, and that they were capable of being made insoluble by covering them with a preparation of copal and collodium. He would refer to the diagram, Plate 7, showing the results of experiments extending over an interval of 300 days ; and from that it would be seen, that in sea water, india rubber, and especially bottle india rubber, lost considerably in weight after 100 days' immersion. At first a slimy skin formed upon the surface, which by degrees increased, and, although the quantity of india rubber actually dissolved was small, yet that fact of its dissolution had to be borne in mind in the construction of submarine telegraph cables. However slow the action might be, it would ultimately prove destructive to the cable, as the places which would suffer most would be those where the insulation was already feeble from injury, or from partial defect in the coating. Before venturing to propose the outer covering of copper referred to in the paper, he had collected all the data upon the subject which he could obtain. The motion of a vessel had the effect of washing off the chloride of copper and magnesia which formed upon the metal. So that although copper sheathing on ships which were kept in motion did not last longer than from 5 years to 7 years, yet in the case of vessels at rest, copper sheathing had been known to last as long as 20 years. In the case of a cable lying tranquilly at the bottom of the ocean, still greater durability might be calculated upon. The copper ordinarily used for ships' sheathing was by no means the most durable that could be obtained. Dr. Percy had found that a small proportion of phosphorus put into the copper had the effect of making it less soluble in sea water than pure copper, and that result was corroborated by his own experiments. Mr. Siemens proposed to use a compound of that character. Small admixtures of silver, or tin, appeared to have the same effect, and it was possible that an alloy of that character might be applied, with equal or greater advantage.

In the discussion of the Paper

"ON THE TELEGRAPH TO INDIA, AND ITS EXTENSION TO AUSTRALIA AND CHINA,"

By Sir Charles Tilston Bright, M.P., M. Inst. C.E.

Mr. C. W. Siemens * said, the author had well described the construction of this cable, and some of the difficulties that had been met with in submerging and working it. With regard to the difficulties in the transmission of messages through Turkey and India, it was to be borne in mind that in India, nearly all the lines were made without insulators in the first instance, the line-wires having been suspended from wooden cross pieces fastened to the poles, and afterwards insulators of unsuitable and untried forms were used. No wonder, then, that the lines should be in a bad condition, independently of the other reason given—that the staff was wholly inadequate to the business. He considered that, electrically, the Persian Gulf cable was well adapted to its purpose,

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXV. Sessions 1865-1866, pp. 18, 19, 62.

being in accordance with the proportions he had recommended, when the question was referred to him by the Indian Government in the first instance. Messages could be sent through it, with the instruments used, at almost any speed at which the clerks were capable of working the keys. He did not think the form of the conductor was, however, one that would be generally adopted hereafter. It was certainly very tempting, to adopt a conductor of several wires, with the guarantee of the tenacity of the different members of the whole, and at the same time presenting the least development of surface for conductivity ; but he believed there were risks connected with it, particularly as regarded the joints, which had to be made the same as in a solid wire. The outer covering seemed to have succeeded perfectly. He had had some fear lest the application of the hot material on the outside of the cable might, at a moment when perhaps the machine was badly attended to, cause the more fusible gutta-percha to melt ; but that did not appear to have been the case. His experience rather went to prove that a cable would work (provided it had been manufactured under proper superintendence) exactly as long as the outer covering lasted ; therefore every effort should be concentrated on making an outer covering which should be durable for many years. With regard to shallow sea cables, there was a remedy, though perhaps a coarse one, by using large galvanized wires. The covering which Messrs. Bright and Clark had applied to the Persian Gulf cable was another solution ; but such an outer covering could not be used for deep seas, and he thought the most important question of the present day, in regard to deep sea telegraphs, was what outer covering would combine lightness with permanent strength ? The solution which Mr. Siemens had worked out, was a flexible armour of copper or iron, which had already been brought before the Institution.* He was gratified at hearing the high eulogium which the author had passed upon the late Colonel Patrick Stewart. Too high praise could not be bestowed upon that most able and disinterested officer, who had fallen a victim to his anxiety in carrying out this great work.

Mr. C. W. Siemens had intended to enter into some explanations with regard to the electrical tests, and of the value of the different

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* Vide Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXI. p. 529.

units, and the reason of their occurring in such enormous numbers; but owing to the length of the discussion, he would confine himself to some of the practical points, on which he wished to make a few observations. Both india-rubber and gutta-percha could be put upon the wire in such a perfect manner that, provided those materials were not tampered with by solvents or excessive heat, permanent insulation could be insured, which was, in the case of gutta-percha, materially improved by great hydrostatic pressure at the bottom of the sea; therefore it was chiefly to the outer covering and to the laying of cables, that attention should be directed in order to find out where improvements could be made. He would lay stress upon one important fact with regard to deep, as well as to shallow-water cables, namely, that a cable would only remain electrically in good condition, so long as the outer covering remained intact. He did not believe that a cable could be laid safely without some kind of outer sheathing. The accidents which had occurred to the last Atlantic cable showed how necessary it was to have an outer sheathing which could not be pierced by pointed wires; but in adopting an outer sheathing, it ought to be a permanent one. It was maintained, in answer to this assertion, that when a cable was laid in deep water, it would probably last for years after the outer covering had decayed. But it was known practically, that cables had failed after the outer covering was gone. In the Red Sea the cable failed, and the Malta-Alexandria cable broke down, wherever oxidation had been most active, through the exposure of the iron wire sheathing. But it had been asked, what forces were there to destroy a cable laid upon a smooth bottom? If the bottom could be supposed perfectly smooth and oozy, and if the cable was supposed to have sunk into the ooze equally throughout, then he could indeed find no reason for its failure after the iron had decayed. But was it reasonable to suppose the existence of such a bottom? Would it not be probable that a dead fish, or the bone even of a dead fish, falling on this soft bottom, would support the cable falling over it, which would be for part of its weight, suspended at that point. The part most exposed at the point of suspension would be first attacked by the corrosive action of the salt water, and it was indeed a fact, that the iron wires of cables corroded away in points before the whole was materially affected.

Supposing the iron wire to be corroded away in a place of partial support, then the cable to the right and left of that place would naturally sink. Thus readjustment of the weight of the cable upon the bottom naturally took place at the expense of the part where the strength had gone, causing the insulated conductor to elongate and to break. He thought, therefore, the outer sheathing ought to be permanent, in order that the cable itself might be so. He had advocated for years a particular form of cable, made of hemp laid longitudinally, and covered with a flexible armour of sheet copper. He had laid about 200 miles of that cable in different parts of the world, and he found it to be very permanent, where it was not broken by mechanical agencies. The specimen exhibited had been taken from a depth of 1,500 fathoms, after being a year submerged. It was quite intact, whereas the iron-covered shore end was much corroded. He mentioned that fact in answer to what had been observed as to the impossibility of picking up a cable from a great depth.

Mr. Longridge said his remark applied principally to the Atlantic cable. If the end could be recovered, the cable could be under-run; but with the end lost, to pick it up and raise it to the surface from a depth of 2,000 fathoms, he believed, was impossible.

Mr. Siemens agreed in that case with Mr. Longridge. Practically it was of the utmost importance that the cable should be smooth, so as to be suitable for being picked up, or under-run. A cable covered with hemp could only be got up at a slow rate, but a smooth cable would come up with very little resistance. With regard to the lines in India, it had been said that iron posts did not answer so well as wooden ones. He differed from that opinion, as he had put up above a thousand miles of land telegraph upon iron posts, with most satisfactory results. Of course better insulation was required with iron than with wood; and one of the great faults of insulators was, that they were generally put upon brackets, and the wire was either suspended on the top or by the side of them. That was an imperfect form of insulator, inasmuch as on a heavy rain falling upon the bracket, the spray rose into the cup and destroyed insulation during the rain. All wires should be suspended from the insulating cup, a system which he had followed for many years, in constructing his bell insulator with vulcanite stalk. With a properly insulated line, with a proper staff of electricians and with good instruments, there would be no difficulty in working through from London to Calcutta in the course of an hour. He had worked through from London to Omsk, in Siberia, without any hand transmission ; there had been mechanical transmissions, but the moment the signal was given in London it was received in the middle of Siberia.

With regard to the messages arriving in a mutilated condition, he might state, that on the continent there were many through lines working with a mechanical transmitter, which might be used with advantage for these great lines. The message was put into type, passed through the instrument, and transmitted mechanically through the line ; it was received in a printed form, according to the Morse alphabet, at the rate of from 60 words to 80 words per minute. With such a system, well organized, there would be no difficulty in keeping up an immediate communication with India ; or if there was any difficulty in the way of such a system, it would arise from political causes.

In the discussion of the Paper,

"DESCRIPTION OF THE PAYING-OUT AND PICKING-UP MACHINERY EMPLOYED IN LAYING THE ATLANTIC TELEGRAPH CABLE," by Mr. George Elliot, of London,

Mr. C. W. Siemens * considered they were much indebted for the very interesting and valuable particulars given in the paper respecting machinery which had been successful in achieving such an important work as the laying of the Atlantic telegraph cable, and which appeared in all its details to have been most carefully arranged. In the previous expedition of 1865 there was no doubt that a great mistake had been made in having the picking-up machinery separated by a distance of more than 600 feet from the paying-out machinery, the two being at opposite ends of the great ship ; and this he considered had caused the loss

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* Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1867, pp. 35–41.

of the cable in that attempt. There was no occasion, however, for the use of separate machines for paying out and picking up, and machinery had previously been designed by himself for serving the double purpose of paying out and picking up (shown in Figs. 1 to 8, Plates 8 to 10), which was made by Messrs. Easton and Amos, and fitted on board the *Dix Décembre*, a French telegraph ship that had done a great deal of actual service in laying and picking up telegraph cables in the Mediterranean. The engine A, Figs. 2 and 4, was placed on deck near the paying-out gear B, at the stern of the vessel; and the machinery was driven by the strap C tightened by the hand lever D, so that it could be thrown out of gear at any time. By this means he had frequently reversed the action of the machinery from paying out to picking up within only a minute or two, which he considered was a point of special advantage in such operations, by admitting of readily hauling in the cable for examination or repairs; for if any accident happened to the cable in paying out, such as any deficiency occurring in the electrical tests, it was most essential that there should be facilities for picking up the cable immediately and examining the injured place. In picking up by this arrangement, the vessel had of course to be drawn backwards by the strain upon the cable in being slowly hauled in; and in the case of a head wind this strain was diminished by working the ship's engines slowly in the backward direction. The *Dix Décembre* had been largely employed in picking up the Port Vendre and Minorca cable and other old Mediterranean cables, hauling the cable in at the stern, with most satisfactory results. If, on the contrary, in order to haul in the cable it had to be taken round along the whole side of the vessel from the stern to the bows, there was very great risk of injuring the cable, or of losing it altogether as occurred in 1865, because during the whole time of making the change the vessel was in effect riding at anchor upon the cable. He could only suppose that that mode of procedure arose from the old notion of sailors that the bows of a ship were the proper place for taking in a cable, because the anchor rope was always taken in at that end.

In paying out a cable, the dynamometer for showing continuously the strain under which the cable ran out was a most essential instrument, and in the arrangement described in the paper a weighted pulley working between guides rested upon the cable midway between two carrying wheels over which the cable passed. This construction, however, he thought was not so free in its action as the arrangement shown in Fig. 5, which he had adopted and had found completely successful, having the weighted pulley E carried at the extremity of a lever F, so that it rested freely upon the cable G; the lever was loaded either by a weight H, or by springs, the latter being preferable on account of their greater steadiness of action. A scale attached to the lever showed at all times the amount of strain upon the cable.

In place of loading the friction brakes of the paying-out drum by means of dead weights, as had previously been done, with the addition on the *Great Eastern* of water cylinders to prevent undue oscillation of the weights, as described in the paper, he had adopted a plan of loading the brakes which he thought had an advantage in delicacy and certainty of action, by the use of a hydraulic cylinder K, Figs. 5 and 6, loaded by a constant pressure of water upon the piston, according to a suggestion originally made by Professor Rankine and embodied in the design of the machinery on the *Dix Décembre*. The supply of water to the cylinder K was maintained by the pump L, Fig. 5, driven constantly by a strap from the shaft of the paying-out drum B; and the water being delivered along the pipe M communicated by the four-way cock I with the top of the brake cylinder K, and passed up the rising pipe N to the regulating valve J in the tank P, shown to a larger scale in Fig. 7. The cylindrical casing of the valve J had four V-shaped orifices, through which the water entering from the pipe N escaped continuously into the external tank P, the rate of escape being regulated by the position of the plunger J forming a piston valve, which was loaded by a spring-balance Q, like an ordinary safety valve. By this means a constant pressure was maintained upon the top of the piston in the brake cylinder K, and the load upon the brake R in paying out was thus readily adjusted by screwing the spring-balance Q to the desired pressure; while at the same time the load upon the brake could never exceed the maximum to which the spring-balance was adjusted. The bottom of the brake cylinder K communicated by the four-way cock I, and the exhaust pipe S with the tank P, but outside the regulating valve J, so that the loading pressure was not conveyed to the underside of the piston in the brake cylinder; and in order to allow of a slight amount of yielding in the hydraulic brake, and to give it a sort of elastic action, the four-way cock I was itself made with a small hole through the plug, as shown to a larger scale in Fig. 8, through which a constant small leakage took place from the pressure side into the exhaust pipe. The brake R was rendered self-relieving by the lever T, Fig. 6, in the same manner as described in the paper. There were two of the friction brakes R R, Fig. 2, one on each side of the paying-out drum B; and the two rising pipes N N from the brake cylinders K K both entered under the same regulating valve J, Fig. 7, whereby the load was maintained always exactly equal upon both brakes. The water escaping from under the valve J into the tank P was delivered by a pipe U upon the top of each of the brakes for the purpose of lubricating them. When driving the drum B in the contrary direction, for picking up the cable, it was only necessary to reverse the four-way cock I; and the pressure then acting below the piston in the hydraulic cylinder K slacked the brake strap off the brake, while at the same time the lubrication by the waste water from the tank P continued undiminished, so that the drum B revolved freely without the action of the brakes.

In the laying of the Atlantic cable he was glad to see that water-tight tanks had been adopted for containing the cable on board the *Great Eastern*, as that was a measure which he had recommended for many years, because it preserved the cable from injury and allowed of a system of continuous tests during the whole time of the laying. In addition to the water-tight tank containing the cable in flakes or layers, an arrangement employed for light cables in the *Dix Décembre* was to carry the coil of cable upon a circular turntable revolving on a set of live rollers in the water tank, as shown in Fig. 3.

With regard to the grappling for the recovery of the cable of 1865, one fortunate circumstance connected with the Atlantic Ocean was that its bottom appeared to be perfectly smooth and homogeneous. In the Mediterranean sea, however, where he had grappled a cable at a depth nearly but not quite equal to that of the Atlantic, the circumstances had been much less favourable in those respects; and the dynamometer was therefore by no means so steady, but was liable to fly up suddenly to perhaps 15 cwts. and drop down again to 4 or 5 cwts., so that it was impossible to decide with such accuracy as in the Atlantic whether the cable was taken hold of by the grapnel or not. The reason no doubt was that the Mediterranean had a rocky bottom, full of coral formations in moderate depths, and rocky even at its greatest depths; whereas the bottom of the Atlantic appeared to be almost perfectly smooth. Another circumstance highly in favour of the Atlantic cable was that there seemed to be no animal life at the bottom of the ocean; whereas in the Mediterranean, if such a cable covered partially with hemp were put down to the bottom, it would be utterly destroyed in a few months by animals breeding upon the cable and eating up the hemp, leaving the wires unprotected and a mere burden upon the tender core. This had been the case with the early Candia and Chios cable, and with the Toulon and Algiers cable laid in 1859, both of which had become entirely useless after being down, the former only six months, and the latter only about eight months. On the contrary, the specimen now exhibited of the recovered Atlantic cable of 1865, which had been down at the bottom of the ocean for twelve months, was evidently as perfect as at the time that it was laid, although its construction was the same as that of the Toulon and Algiers cable. On all accounts, therefore, there seemed every reason to hope that the Atlantic cable now laid would prove a durable one.

Mr. C. W. Siemens further said he had seen the hemp covering of a cable entirely eaten away, and even the gutta-percha indented about 1-16th or 1-8th inch, but beyond that point the action of the marine animals did not appear to go. With regard, however, to the safety of the cable when deprived of its hemp covering, although it might continue efficient if lying upon a perfectly smooth bottom and with the tensile strain perfectly equalised throughout its length, yet that would certainly not be the case in the Mediterranean, where he was satisfied that as soon as the cable had ever worn weak at any point it would break, because the bottom of the sea was so uneven that the cable hung unsupported in a great many places. He had seen pieces of Mediterranean and Red Sea cables which had evidently given way at points of suspension by the rusting of the iron wires; and wherever the cable had parted, the core had been elongated several inches, and had shown electrical faults. Wherever the bottom was not perfectly smooth, the strains produced by the suspended portions of the cable must ultimately lead to fracture, if the iron wires became laid open to the action of rusting. So long as a cable retained its full strength, it was not indeed necessary that it should be perfectly supported throughout its entire length; but unless it were proved that the bottom of the Atlantic was so perfectly even that no portion of the cable would be in suspension, the continuance of the cable in working order must be dependent he considered upon the durability of the hemp covering protecting the iron wires from corrosion, which in the absence of marine animals might be for many years.

In addition to preserving the iron wires from rust, it must be borne in mind that the durability of the hemp covering was also essential to the strength of the cable, in consequence of the hemp acting as a packing to keep the wires at the proper distance apart. If the hemp were eaten away, the wires would be left like a loose cage round the core, and the latter would stretch and become faulty in consequence.

ON THE CONVERSION OF DYNAMICAL INTO ELECTRICAL FORCE WITHOUT THE AID OF PERMANENT MAGNETISM.

By C. W. Siemens, F.R.S.*

Since the great discovery of magnetic electricity by Faraday in 1830 electricians have had recourse to mechanical force for the production of their most powerful effects; but the power of the magneto-electrical machine seems to depend in an equal measure upon the *force expended* on the one hand, and upon *permanent magnetism* on the other.

An experiment, however, has been lately suggested to me by my brother, Dr. Werner Siemens of Berlin, which proves that permanent magnetism is not requisite in order to convert *mechanical*

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* **Excerpt from the Proceedings of the Royal Society, Vol. XV. 1867, pp. 367–369**

into *electrical* force ; and the result obtained by this experiment is remarkable, not only because it demonstrates this hitherto unrecognized fact, but also because it provides a simple means of producing very powerful electrical effects.

The apparatus employed in this experiment is an electro-magnetic machine consisting of one or more horseshoes of soft iron surrounded with insulated wire in the usual manner, of a rotating keeper of soft iron surrounded also with an insulated wire, and of a commutator connecting the respective coils in the manner of a magneto-electrical machine. If a galvanic battery were connected with this arrangement, rotation of the keeper in a given direction would ensue. If the battery were excluded from the circuit and rotation imparted to the keeper in the opposite direction to that resulting from the galvanic current, there would be no electrical effect produced, supposing the electro-magnets were absolutely free of magnetism ; but by inserting a battery of a single cell in the circuit, a certain magnetic condition would be set up, causing similar electro-magnetic poles to be forcibly approached to each other, and dissimilar poles to be forcibly severed, alternately, the rotation being contrary in direction to that which would be produced by the exciting current.

Each forcible approach of similar poles must augment the magnetic tension and increase consequently the power of the circulating current ; the resistance of the keeper to the rotation must also increase at every step until it reaches a maximum, imposed by the available force and the conductivity of the wires employed.

The co-operation of the battery is only necessary for a moment of time after the rotation has commenced, in order to introduce the magnetic action, which will thereupon continue to accumulate without its aid.

With the rotation the current ceases ; and if, upon restarting the machine, the battery is connected with the circuit for a moment of time with its poles reversed, then the direction of the continuous current produced by the machine will also be the reverse of what it was before.

Instead of employing a battery to commence the accumulative action of the machine, it suffices to touch the soft iron bars employed with a permanent magnet, or to dip the former into a position parallel to the magnetic axis of the earth, in order to produce the same phenomenon as before. Practically it is not even necessary to give any external impulse upon restarting the machine, the residuary magnetism of the electro-magnetic arrangements employed being found sufficient for that purpose.

The mechanical arrangement best suited for the production of these currents is that originally proposed by Dr. Werner Siemens in 1857,* consisting of a cylindrical keeper hollowed at two sides for the reception of insulated wire wound longitudinally, which is made to rotate between the poles of a series of permanent magnets, which latter are at present replaced by electro-magnets. On imparting rotation to the armature of such an arrangement, the mechanical resistance is found to increase rapidly, to such an extent that either the driving-strap commences to slip or the insulated wires constituting the coils are heated to the extent of igniting their insulating silk covering.

It is thus possible to produce mechanically the most powerful electrical or calorific effects without the aid of steel magnets, which latter are open to the practical objection of losing their permanent magnetism in use.

ON A RESISTANCE-MEASURER.

By C. W. Siemens, F.R.S.†

For the measurement of small resistances the method formerly employed was that of the tangent galvanometer, which method is still valuable in the determination of resistances which are inseparable from a difference of electrical potential, such, for instance, as a galvanic element.

In measuring wire-resistance, more accurate and convenient methods have been devised, amongst which that of the common differential galvanometer and that known as Wheatstone's balance hold the most prominent places.

But both these systems have disadvantages which render them insufficient in a great many cases. For instance, in the first

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* See Du Moncel "Sur l'Electricité," 1862, page 248.

† Excerpt Philosophical Magazine, Vol. 34, 1867, pp. 270–273. Communicated through the Electrical Standards Committee.

method a well adjusted variable-resistance coil is necessary, which, if the method is intended to be applicable between wide limits, will have impracticably large dimensions. The bridge method, though very beautiful, requires three adjusted coils, and frequently gives rise to calculation, which renders it unavailable for unskilled operators. The sine method, which is the most suitable for measuring great resistances, requires even a superior amount of skill and mathematical knowledge on the part of the operator. Many years' experience of these methods made me feel the want of an instrument which would, by its simplicity of construction and ease of manipulation, be capable of employment by an unskilled operator with a degree of correctness equal to that of the bridge method.

The conditions upon which such an instrument could be successful appeared to be the following :—

1. The employment of a zero method, by which the galvanometer-needle would always be brought to the direction of the magnetic meridian or the same given point upon the scale and, therefore, be independent of the unknown function of the angle of deflection.

2. The readings to be made upon a simple linear measure divided into equal parts signifying equal units of resistance.

3. The employment of a single and unalterable comparison-resistance.

The apparatus constructed to fulfil these conditions is represented by the diagram, Plate 11.

Two equal and parallel helices, *h* and *h'*, are fixed upon the common slide *s s'*, which moves in the direction of its length between guide rollers. This motion is effected by the end *s'* armed by a facing of agate, which presses against the face of the metal curve *c c'*. The latter is fixed upon a slide moving in a groove in the rule *d d'*, at right angles to the direction of *s s'*. The curve is moved in the direction *d d'*, by means of a milled head *i*, on the axis of which is a pinion gearing into a rack underneath the straight edge of the curve *c c'*. The rule *d d'* is graduated into equal parts ; and opposite to the divisions is a nonius up the straight edge and the curve, to divide each degree into ten parts. Whenever the milled head *i*, therefore, is turned, the position of the curve is altered ; and as the point *s'* of the bobbin slide is pressed against it by means of a spring, the bobbin follows it in all its movements.

The wires of the two bobbins are connected together, in the common point a, with the pole of a galvanic battery E, the other pole being connected with two resistances R and x, and through these with the other ends of the galvanometer-helices. The resistance R is made constant, and adjusted so that when $x = 0$ the index of the curve stands exactly opposite the zero of the graduated scale dd', the unknown resistance being represented by x.

It is evident that, the resistance in the bobbins being equal, as also their dimensions and initial magnetic effects upon the needle suspended between them, if we make the resistance x equal to R, the currents in the two branches will be equal, and the magnet-needle therefore balanced between them only when the helices are equally distant from it. Should, however, either of these resistances preponderate, the strength of current in that branch will be lessened ; and in order to re-establish the balance it will be necessary to shift the bobbins, approaching the one in which the weaker current is circulating towards the suspended magnet.

The instrument is erected upon a horizontal metal table standing upon three levelling-screws. The bobbins, with the suspended magnet, and dial-plate for observing the deflection and zero of the pointers are contained in a glass case with glass cover, supported by four brass pillars. The instrument is supplied with terminals for the battery-connexions, and a current-breaker for interrupting the battery-circuit. Opposite to these are four terminal screws for receiving the ends of the resistances R and x, with contact-plugs between them in order to quickly establish a short circuit in case the operator should be in doubt towards which side he has to move the adjusting-curve. Two constant resistances accompany the apparatus, R that which is used during the measurement, and a, a resistance of known value, which is introduced between the terminals x in order to enable the operator for his own security to make a control measurement by which he may convince himself of the adjustment of the instrument at any time. Another purpose of this resistance is to facilitate the readjustment of the zero-point, in case the galvanometer should at any time be cleaned or a new silk fibre put in.

In constructing the sliding curve of this instrument, it might be determined by calculation from the formula given by Weber for the deflection effect of a circular current of known dimensions upon a magnetic point, and from the given distance of the coils from each other. I prefer, however, in practice to determine the curve of each separate apparatus empirically, because it is not possible to coil a helix mathematically true, or to set it, when coiled, absolutely at right angles to the plane of its horizontal motion.

In the determination of each curve I use a delicately adjusted rheostat or scale of resistances in the circuit of x, giving it varying values corresponding to the equal divisions of the engraved scale, and constructing the curve according to the position which it is found necessary to give to the point s' in order to arrive at the magnetic balance. With each instrument it would be possible to have two values of R one expressed in mercury and the other in B.A. units ; and in order to measure at pleasure in either of these units, it would only be necessary to insert the one or other between the terminal screws for R.

The instrument has been found to be very convenient for the measurement of the wire resistances of overland lines, or for the reading of resistance thermometers ; it reduces the operation to the observation of the zero position of a needle, and the reading upon a graduated scale, which can be performed by a person of ordinary intelligence without experience in electrical measurement. In accuracy and range it fully equals the bridge method, while as regards portability and cheapness of apparatus the advantages are decidedly in its favour.

In the discussion of his Paper

"ON PYROMETERS,"

MR. SIEMENS [*] said : With regard to the first question—whether any permanent change occurs in the conductivity of the metal wire when exposed to heat, I have found that such is not the case. The wire no doubt elongates when exposed, but with it the lateral dimensions increase. In taking the resistance of the wire, we do not deal with the length only, but with the length divided by the

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[*] Excerpt Journal of the Iron and Steel Institute, Vol. I. 1869–70, pp. 54, 55.

area, which proportion is not altered by a general expansion of the metal. A series of very protracted experiments, which it would have been too much to record in this paper, have proved that the conductivity of a metal at a fixed temperature is an exceedingly constant quantity. If copper or iron wire were exposed to intense heat the electrical resistance would be inverted in consequence of the substance diminishing through oxidation ; but that is not the case with platinum at reasonable temperatures, and in measuring extremely high temperature it is necessary not to expose the platinum wire itself to the heat, because it would be partially volatilized. In this case the wire is surrounded by a casing of platinum, and is exposed only to radiation heat from the sides of that casing. It is not even necessary to heat the wire to the ultimate temperature of the furnace, but it suffices to expose it for a given time, say, three or five minutes, to take the reading, and to remove the gauge. In operating thus, we can with this instrument measure temperatures reaching to the melting point of platinum itself. With regard to the second question—whether the cylinder of fire clay does itself conduct, I have made the experiment this way. I have taken a clay cylinder with the wire wound upon it, and cut the wire in one place, before exposing the cylinder to an intense white heat. I have therefore measured the amount of current passing through, which current gives the amount of leakage we may expect at a very high temperature. I have thus found that there is indeed conductivity rising with the temperature ; but it is exceedingly small, never reaching more than half per cent. of the conductivity of the wire itself. The case would be different if the pipe clay cylinder were itself exposed to the fire : under which circumstances, a fluxed surface would be formed, which would be conductive. But by excluding the flame from the cylinder, and from the wire, this error has been reduced to an altogether diminished quantity. I forget the third question.

Mr. Snelus : The method of checking the instrument after it has been in use for some time ?

Mr. Siemens : That can only be done by a practised electrician, though it would be a desirable thing to do. If the wire was to break, the instrument would cease to indicate altogether ; and in electrical experiments generally, faults, if they occur, are of a very perceptible, and decided nature. For instance, if there was contact between the wires, the results registered would at once show such utter discordance with probable facts, that attention would be drawn to it. Of course, the instrument could be destroyed if exposed to intense heat; but in that case it would tell its own tale.

Mr. Bell: I should like to ask Mr. Siemens what he estimates would be the cost of this instrument? That is rather an important thing.

Mr. Siemens: I can hardly answer that question in a very decided manner, your lordship; because this instrument, which is, as it were, the mother instrument, has been very expensive. Another complete instrument would cost, I think, about £16—including one or two pyrometer coils, which latter are not expensive except for very high temperature, when cases of platinum have to be resorted to. A platinum tube would probably cost about £10; but if an iron or copper tube, such as would suffice for a temperature below whiteness, the cost in this part of the instrument is but trifling.

In the discussion of the Paper

"ON A MODIFIED FORM OF 'WHEATSTONE'S BRIDGE,' AND METHODS OF MEASURING SMALL RESISTANCES," by PROF. G. C. FOSTER,

THE PRESIDENT * (Mr. C. W. Siemens) said, having been connected more or less with the Wheatstone bridge in its early application to telegraphic measurement, he would say that the instrument was first attempted to be used in testing coils of insulated line wire by his brother in 1847–48. It was soon found that the range of the instrument was insufficient, and it occurred to his brother to construct these resistance-boxes so as to make the two arms of the balance variable, which gave a much larger range of reading. Instead of simply adjusting one weight with another on equal arms of the balance, he made, so to speak, the length of the arms variable, and got thereby much wider limits within which

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. I. 1872–73. p. 207.

the instrument could be applied. Moreover, by the adoption of resistance-boxes a great deal of the difficulty which Professor Foster had met with was avoided, because the stopper made a very safe contact. Moreover, the wire with sliding contact was apt to wear if much used, and the resistances of comparison consisted of fractions of units only, whereas the resistances to be measured amounted usually to hundreds, thousands, hundreds of thousands, and millions; it also followed from this condition that a galvanometer of exceedingly small resistance must be used, in order to get a proper proportion between those resistances, to give a maximum effect; whereas with box-resistances, they could use a galvanometer of many thousand units in its point of maximum sensitiveness. He would ask Professor Foster to be good enough to state whether he had compared his improved instrument—which appeared to be very ingenious as a method of avoiding errors in dealing with small resistances—whether he had compared that with the instrument with the resistance-boxes?

In the discussion of the Paper

"ON ELECTRICAL IGNITION OF EXPLOSIVES,"

By Major Stotherd,

The President* (Mr. C. W. Siemens), in closing the discussion said the subject was of very great interest, which had partly occupied his attention for a long period. Members might not be aware that the first application of torpedoes and submarine mines was made as early as 1848, in the harbour of Kiel. The torpedoes consisted of bags of gunpowder connected with batteries on land by wires insulated with gutta-percha. He believed that was the first application to this purpose of insulated wires. These mines were to be exploded when a ship came over them, and the position of the ship was determined by a reflector. In the latter period of the Austrian war, a great number of torpedoes were

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. I. 1872-73, p. 223.

laid, depending upon double action, one contact being established between the ships and the torpedoes, and the second contact on land. If both contacts were established the mine would explode, but if one or the other was not in contact it would not explode. He agreed with Captain Dawson that it was altogether a very difficult subject; but a great deal would no doubt be done, and submarine mines would form an essential point in modern warfare, not only for defence, but for attack.

In the discussion of the Paper

"ON LIGHTNING AND LIGHTNING CONDUCTORS,"

By Mr. W. H. Preece,

The President* (Mr. C. W. Siemens) rose to move a vote of thanks to Mr. Preece for his valuable paper. The paper dealt with two subjects—one a purely theoretical, and the other a practical telegraph subject.

With regard to the first, the distribution of atmospheric electricity, they had heard a very able discussion, and he thought they had all learned a great deal respecting that most difficult question. Practical observation on lines in countries where atmospheric electricity abounds, indicates that its distribution is perhaps more local than most electricians suppose.

For instance, if they erected a line with posts, each of them carrying a lightning discharger, even that might be an insufficient protection, seeing that between pole and pole there might be an accession of electricity in the atmosphere and discharge into the wire. The only absolute protection to a land wire would be probably to make all the post conductors, either iron posts or wooden posts, with lightning conductors, and make them carry at the top a wire connected with earth all the way; but that would be expensive, and would involve many practical difficulties; therefore, the next best thing was to protect the station itself. He thought it was not sufficient to protect the coil of the instrument,

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. I. 1872–73, p. 378.

but the station should also be protected, and that was the case on all continental lines he had been connected with, where lightning abounds. The form of lightning discharger was perhaps a question open to controversy, but from all he had seen of lightning dischargers those with a great many points seemed to him to be the safest guard against accidents to the instruments. One pair of points in a vacuum was a very good protector, but it could not be relied upon permanently, inasmuch as a single discharge would destroy the points, and the discharger under these circumstances was worse than useless. The plate protector was devised by his brother many years ago. Although it seemed an arrangement of two plates opposite each other, it was really an arrangement of many points opposed to each other, at very short distances apart, inasmuch as the surfaces were planed in opposite directions at right angles with each other, forming a series of thousands of points, and if by the discharge some of the points were burned away, there were always plenty remaining to do the work. It was necessary that care should be taken to clean these surfaces very frequently, and there was no reason why this should not be done as part of the duty of the office. The vacuum and many pointed dischargers seemed to him to be the best protectors.

"ON IRON TELEGRAPH POLES,"

BY MR. C. W. SIEMENS, F.R.S., D.C.L.*

THE construction of an iron telegraph pole has occupied my attention for many years. The object was to combine lightness and convenience of construction with the attainment of a maximum of strength and resisting power to sudden jerks, and to oxidation. This consideration led me to abandon the ordinary mode of fastening poles by setting considerable lengths of them underground, and to the adoption in its stead of a buckled wrought iron plate, which combines very great rigidity with a certain toughness, enabling it to yield to sudden and excessive strains.

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. II., 1873–74, pp. 49–51, 65–70 and 79.

It is evident that a straight pole which has been fastened into the ground by only burying a part of it will, if once shaken by some sudden jerk, never attain its former firmness again, whereas a pole which is fastened to a plate has always the whole weight of earth resting on its foot-plate to keep it steady even if it should be shaken by any temporary cause. The portion of the post which is partly buried in ground, and, therefore, exposed to the simultaneous action of moisture and air, is made of cast iron, and is of tubular form. This tube is fastened to the buckled plate by means of four bolts, and is provided at its upper end with a suitable socket to receive the upper tube. The latter, which forms the principal part above ground, is made of wrought iron. The shape which has been adopted for it is approximately a parabolic one, that is to say, the tube is cylindrical for about 2 feet, thence tapering off towards the top. By these means a distribution of metal is obtained, which, with a minimum expenditure of material, gives a maximum amount of rigidity.

The proportion of the diameter of the tube to the thickness of metal is such, that a horizontal strain just establishes a balance between the tendency of collapsing or flattening of the tube and that of breaking it. A tube, therefore, of the same weight per foot, but of a larger diameter, would collapse ; whereas if the diameter was to be decreased and the thickness of the metal to be increased proportionately, the tube would break. It is also important to observe that the metal of the conical tube is of sufficient thickness to resist the action of oxidation for an indefinite length of time, it being a well established fact, that a thin plate of say an eighth of an inch thickness rusts through in much less than half the time that another of a quarter of an inch thickness does.

The manufacture of these tubes presented practical difficulties at first, which were overcome by Mr. Brown, the manager of Messrs. Russell, with the aid of a furnace specially contrived by me for that purpose.

The upper tube is usually cemented into the socket of the cast-iron pedestal tube by pouring into the annular space, between the two tubes, a fused cement, consisting of a mixture of sulphur and oxide of iron, which upon congealing sets extremely hard. Lately my firm have also adopted a method of setting the iron tube with an inverted conical end into a conical socket at the upper extremity of the cast-iron tube.

The iron posts above described were first erected by my firm in Spain, South Africa, and other places, in the year 1863, and have remained in perfect working condition ever since. Since 1863 upwards of 180,000 of these posts, representing more than 9,000 miles of telegraph line, have been erected in New Zealand, Ceylon, Egypt, India, Persia, Russia, Mexico, Brazil, River Plate, Chili, and other parts of South America, with the same satisfactory results.

The height and dimensions of these posts vary according to circumstances. If only one or two wires are to be carried and cheapness is an object, posts of a total length of 19 feet 8 inches are used, standing 17 feet above ground when erected, it being usual to place the post 2 feet 8 inches in the ground. The total weight of this post is 184 lbs., and as it can be carried in three separate parts the weight of the heaviest one will be less than 100 lbs. Such a post will support a dead weight of 560 lbs. suspended horizontally from its upper extremity over a pulley, without breaking. In other cases posts which weigh 254 lbs., and bear a testing strain of 900 lbs., have been adopted. At all points where the line is exposed to an extraordinary strain, heavier poles are introduced, and usually for the first-mentioned kind poles which weigh 295 lbs., and bear a strain of 1,120 lbs., and in the latter case poles of a weight of 340 lbs., and 1,350 lbs. breaking strain. The cost of these iron posts has varied from 22*s.* 6*d.* to £3 16*s.*, according to their dimensions, and the fluctuating price of iron.

As a rule, they may be taken to be from two to three times dearer than ordinary wooden posts of the same strength. In many countries, however, where both timber and iron posts would have to be carried over great distances, and by such means of transport as are available in half-civilized countries, iron becomes as cheap as wooden posts at the point of erection, owing to their less weight and the facility of transport resulting from their being carried in pieces of convenient weight and bulk.

But, considering their greater durability and consequently reduced cost of maintenance, they would be, I believe, the cheaper material even in this country.

In southern countries, where wood is subject to dry rot, and where wooden posts have to be renewed every two or three years, the relative advantages of iron posts of this description have proved to be very great indeed.

In the discussion of the above Paper and those

"ON THE APPLICATION OF IRON TO TELEGRAPH POLES," by MAJOR WEBBER, R.E.,

"ON TELEGRAPH POLES," by LIEUT. JEKYLL, R.E., *and*

"ON THE RIBAND TELEGRAPH POST," by MR. R. B. LEE,

MR. SIEMENS said that, as a paper of his was one on the list, he wished to premise by stating that it was not intended to be a paper, but simply a statement of facts regarding the construction of the posts which he introduced many years ago, and that statement was intended as being in addition to the information which was brought before the members by Major Webber in his paper. He had hoped that Major Webber would have brought before the meeting the construction of posts of that type, in order that they might have benefited by the results of his investigation and inquiry into the subject; and he was somewhat disappointed to find that Major Webber confined himself to criticisms on the existing constructions and to a sort of *résumé* of what had been done. There were several parts of his paper from which he (Mr. Siemens) dissented, and he was quite sure that his friend, Major Webber, would be quite pleased to find that his propositions would lead to a discussion that would tend to the understanding of the facts of the case.

Twelve years ago his (Mr. Siemens's) attention was first drawn to the necessity of iron posts for countries where wooden posts were subject to dry rot. He found that in all southern countries wood did not last above one-third or one-fourth the length of time that wooden posts lasted in this country—in fact, two or three years seemed to be the lifetime of a wooden post in South America or Africa. Though these posts might be specially prepared, injected with sulphate of copper, or creosoted, such processes did not materially prolong their lives, and the necessity of providing a stronger material for the purpose became evident to him. The necessities of the case pointed to iron as the best material, and then arose the question, what is the best form to put iron into in order to support the lines of wires at a given height? In the first instance he turned his attention to the tripod. The tripod was a very strong form, a very stable form, and by means of that construction he succeeded in making a strong post. But this construction was not satisfactory, because each limb of the tripod was not sufficiently strong in itself to stand independently. Each member of the structure required support. These supports had necessarily to be lateral supports, and these lateral supports, while they added to the weight and to the expense, did not contribute to bear the strain that might be applied on the top of the posts. Therefore, he abandoned that form in favour of a tubular construction. Now the tube was evidently the right form for bearing a strain in all directions equally, because each part of the material was at the greatest possible distance from the centre; but a cylindrical tubular form was evidently a bad construction, because it would give an excess of strength near the point of suspension, and a minimum or relatively insufficient strength at the base. Therefore the conclusion he drew from this was that a conical tube would be the best. But, on going minutely into the question, a conical tube did not seem to fulfil these conditions, but a tube of a parabolic form externally was the form which gave the maximum of strength. The drawings on the wall showed the upper portion of such a post to be a wrought-iron tube of that parabolic form which, when a weight was horizontally applied at the top, gave equal deflections in all parts. The next question was, What is the proper thickness of the material of such a tube? Captain Mallock had just said that the larger the tube and the thinner the metal, the greater would be its strength. He dissented from that entirely. If more stiffness was gained by increasing the diameter at the expense of the metal, the strength was not increased in a tube such as that shown, which had a thickness of metal of about a quarter of an inch, and a diameter of about four inches. If reduced in thickness it would collapse, and collapse with a less weight than the actual tube would bear. If, on the other hand, the diameter was decreased and the thickness of metal increased, it would break or bend with a less weight. Therefore, there was a point of proportion between diameter and thickness, a proportion which gave an absolute maximum of strength. He did not agree with Major Webber in saying that telegraph poles might be constructed in a hundred different ways to suit the requirements or the fancy of their designer. He looked upon the construction of a telegraph post as one of the most definite things which an engineer could have put before him. Whether the pole was to support straight wires, with only moderate lateral strains to bear, or whether it was to be an angle post to resist the strains of wires pulling in opposite directions, the problem always was to support a strain at a certain height above the ground. This was the one of the points which could be solved in a thoroughly mechanical manner. He had found that the lower portion of the post, which was exposed not only to the strain of the wire but to the moisture of the ground, ought to be of a different material to wrought iron, which corroded very readily, and, therefore, took cast iron, which seemed to be the most suitable material. Then came the next point. How is this base to be fixed in the ground? The natural suggestion was: make the whole tube uniform, and put the post in and ram it all round. But what was got then? A very small post indeed as compared with a wooden post—small in diameter, necessarily so because limited in weight and constructed of a material of greater density. It would, therefore, be necessary to put the iron post deeper in the ground than the wooden post. Major Webber claimed for the posts constructed on his principle two advantages over those proposed and used by Mr. Siemens, (1) less excavation, and (2) saving of material. He thought that in both instances Major Webber was mistaken. Firstly, as regarded excavation, Captain Mallock had already stated that if a hole could be made 6 feet deep, it had to be made of such a size that a man could get in. Now the base plate for supporting his (Mr. Siemens's) post was less than 3 feet in diameter, 2 feet 8 inches, or thereabouts, so that in reality no larger excavation would be required for the pole with the base plate than for the pole without a base plate, the only difference being that for the former the excavation need only be 2 feet 6 inches, or 3 feet deep, while for the latter it would have to be 6 feet deep. With regard to the weight of metal, Major Webber had said that the weight of the base plate might be saved by extending the length of the tube into the ground. The depth he gave—it was a very ordinary depth—was 6 feet. Now the extra length of the cast-iron tube—the difference between 2 feet 8 inches and 6 feet, *i.e.*, 3 feet 4 inches—would weigh

89½ lbs. or 90 lbs. His (Mr. Siemens's) base plate, which was a dished wrought-iron plate, weighed 30 lbs. Therefore a saving of 60 lbs. in weight was effected. But that was not all. Major Webber had said that he (Mr. Siemens) derived the strength of his post from the base plate, but that he (Major Webber) derived his strength 2 feet from the bottom. Therefore Major Webber's post, the same absolute height out of the ground, of the same absolute length between the turning-point of his level of 18 inches, to be of really the same height out of the ground, ought to be 18 inches longer than his (Mr. Siemens's). Not only did he (Mr. Siemens) save 54 lbs. in absolute weight (supposing he made his tube of the same strength as Major Webber's), but he was enabled to raise the strength of his post as if he had it 18 inches longer. But the base plate had another advantage. If it were put into the ground, and the earth filled up over it, the post was absolutely fixed. The strain might come in sufficient severity to move the base plate, but the moment the strain left it the earth fastens it down. A mere iron or wooden post put into the ground, if once shaken, would always be loose, and the iron post had a very great disadvantage as compared with the wooden post, because it had less surface, and that surface was so smooth that it slipped through the earth much more readily than wood. The advantage of the base plate was that the weight of earth itself fastened the post. With regard to the amount of earth, that, he thought, according to Major Webber's opinion, seemed to be much lighter than was absolutely requisite. He had never found an iron post put 2 feet 8 inches in the ground to be torn up. The line in which the earth would pass, if the strain were applied, would be such as to render the earth on the base plate not only a dead weight, but the earth would be lifted away at an angle of about 45 degrees, and the frictional resistance to moving the earth would come in aid of that dead weight. It was quite remarkable how firm posts of this description stood in the ground. In fact, it was only imitating nature. If a tree was uprooted, it would be found that it took its strength near the surface of the ground. The roots spread at once, and if the enormous pressure brought to bear against a large tree standing alone in a field with a gale blowing against it were calculated, it would be surprising to account for its holding its ground against a pressure of perhaps from 40 tons to 100 tons acting against it. He, therefore, strongly maintained the base plate to be a most important feature in the construction of a telegraph post. Again, the base plate enabled the load to be much more equally divided. If a socket were carried down to the 6 feet limit into the ground, it would be necessary to carry it, in order to be safely out of the ground, 4 feet longer, and this would necessitate a cast-iron tube of 10 feet and perhaps 12 feet in length. This would be too ponderous a piece of metal to carry into countries where transport was difficult. The cast-iron base was by far the heaviest portion of the post he proposed, and by carrying the post in three parts of conveniemt weight and size, mules and other country transports were quite equal to the work. Captain Mallock had described the method which he had used in India of making short into long posts. and he mentioned it as an advantage in favour of the particular construction which he had adopted in India. It was certainly an advantage to be able to increase the height of a post when necessary ; but it should be remembered that the base was the most important part of the post, and if the base were carried to where the post was required, this could be more satisfactorily effected ; for, unless it was known beforehand whether the post was to be a high one or a low one, it would be necessary to carry the strongest base everywhere. The way he should generally manage was this : There was one strength of post to support the line where it was straight, and another strength of post for corners, known as "stretching-posts." These corner posts bore half a ton generally of horizontal strain brought to bear upon them, and the others generally about 5 cwt. But if a higher post were required to cross the road or otherwise, a stronger post was taken and a lighter tube put upon it by being dropped simply upon it. By that means much higher posts were obtained and the strains throughout were proportioned to their strength. In that way he accomplished what Captain Mallock gained in the way he described. He would still draw attention to the fact that, although a great many varieties of construction were talked about in reference to a telegraph pole, yet in reality there could only be one construction in principle, whatever were the variations in detail. There could be only one construction to give the maximum of strength for a given height. Then there were other considerations which ought to be attended to. The post ought to be light, and ought to resist oxidation for the greatest possible length of time. This was a desideratum which ought to be met, and the conditions on which it ought to be met were not manifold. They would find that they would come very much to a definite mode of operation or construction in applying all these different requirements.

Mr. Siemens believed he had succeeded in making this pole equally strong in all respects. The strain applied at the top would perhaps bend the whole pole over to such a point as to approach the ultimate strength; then it would be matter of accident whether it broke in the cast iron. Most likely it would break near the ground line in the cast iron. There was an advantage he believed in the base plate being flexible—it saved the pole giving way in this joint. It would begin to yield a little, and if the ground was not firm possibly it might lift the earth up; but that he considered was the most likely point where the pole would give way. It would be useless to make it firmer in the earth than it was now.

THE STEAMSHIP "FARADAY" AND HER APPLIANCES FOR CABLE-LAYING,

By C. William Siemens, D.C.L., F.R.S, M.R.I.*

The speaker in his introductory remarks observed that an electric telegraph consisted essentially of three parts, viz., the electromotor or battery, the conductor, and the receiving instrument. He demonstrated experimentally that the conductor need not necessarily be metallic, but that water or rarefied air might be used as such within moderate limits; at the same time, for long submarine lines, insulated conductors strengthened by an outer sheathing were necessary to ensure perfect transmission and immunity from accident. The first attempts at insulation, which consisted in the use of pitch and resinous matters, failed com-

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* Excerpt Journal of the Royal Institution of Great Britain, Vol. VII. 1874, pp. 310–313.

pletely, and in the years 1846 and 1847 the two gums, india-rubber and gutta-percha, were introduced, the former by Professor Jacobi of St. Petersburg, and the latter by Dr. Werner Siemens of Berlin. This last gum soon became almost indispensable to the cable manufacturer on account of its remarkable plasticity at low temperatures and its insulating property.

The first outer sheathing used was a tube of lead drawn tightly over the insulated wire, and this again was covered with pieces of wrought-iron tubing connected by ball and socket joints; in this way the Elbe and other rivers were crossed successfully in 1848—50. This method was superseded later on by the spiral-wire sheathing, first proposed by Mr. Brett in 1851 for the Dover and Calais cable; since then, with few modifications and exceptions, this form has been universally adopted.

The speaker next enumerated the casualties to which submarine cables are liable, and the precautions employed to obviate them. He showed specimens destroyed by rust and the ravages of a species of teredo. On the Indo-European cable line a curious case of fracture occurred; a whale, becoming entangled in a portion of cable overhanging a ledge of rock, broke it, and in striving to get free had so wound one end round its flukes that escape became hopeless, and so had fallen an easy prey to sharks, which had half-devoured it when the grappling iron brought his remains to the surface. Other enemies to be dreaded are landslips, ships' anchors, and abrading currents.

The new Atlantic cable consists, for the deep-sea portion, of copper conductors, gutta-percha insulators, and a sheathing of steel wires covered with hemp; the shallow water part consists of similar conductors and insulators sheathed with hemp, which in turn is covered with iron wire.

In paying out, no catenary is formed, as might be supposed, but the cable passes in a straight line from the ship to the sea bottom —a proposition which the speaker demonstrated experimentally by means of a long trough with glass sides filled with water. The retaining force applied by the brake-wheel should be equal to the weight of a piece of cable hanging vertically downwards to the bottom of the sea. In picking up, a catenary is formed, but a vertical position is the best, because it produces the least resistance.

From the peculiar nature of the service for which a telegraph-ship is required, it is evident that she must possess properties somewhat different from those of ordinary ocean-going steamers; thus speed is not so important as great manœuvring powers, which will enable her to turn easily in a small space, or by which she may be maintained in a given position for a considerable time. In the ship about to be described an attempt had been made to meet these requirements.

The "Faraday," of 5000 tons register, was built at Newcastle by the eminent firm of Messrs. Mitchell and Co. She is 360 feet long, 52 feet beam, and 36 feet depth of hold; there are three large water-tight cable tanks, having a capacity of 110,000 cubic feet; these are each 27 feet deep; two are 45 feet in diameter, and one is 37 feet; they can take in 1700 miles of cable $1\frac{1}{4}$ inch in diameter. After the cable is coiled in, the tanks are filled up with water to keep it cool; for the speaker had found, when conducting experiments on the Malta and Alexandria cable with his electrical resistance thermometer, that heat was spontaneously generated in the cable itself, whereby its insulation was seriously endangered.

The "Faraday" has stem and stern alike, and is fitted with a rudder at each end; both are worked by steam-steering apparatus placed amidships, and are capable of being rigidly fixed when required. She is propelled by a pair of cast steel screw propellers 12 feet in diameter, driven by a pair of compound engines constructed with a view to great economy of fuel. The two screws converge somewhat, and the effect of this arrangement is to enable the vessel to turn in her own length when the engines are worked in opposite directions. On the voyage from Newcastle to London a cask was thrown overboard, and from this as a centre the ship turned in her own length in 8 minutes 20 seconds, touching the cask three times during the operation. This manœuvring power is of great importance in such a case as repairing a fault in the cable, as it enables the engineer to keep her head in position, and, in short, to place her just where necessary, in defiance of side winds or currents.

The testing-room of the electrician in charge is amidships, and so placed as to command the two larger tanks, while the ship's speed can be at all times noted on the index of a Berthon hydrostatic log.

The deck is fitted with machinery to be used in laying operations, which will be best described by tracing the path of the cable from the tanks to the sea. Let us begin with the bow compartment: the cable, which lies coiled round one of Newall's cones, begins to be unwound, passes up through an eye carried on a beam placed across the hatch, next over a large pulley fitted with guides, and by a second pulley is gently made to follow a straight wooden trough fitted with friction rollers, which carries it aft to near the funnels; here it passes through the "jockey," a device for regulating the strain, consisting of a wheel riding on the cable, which can be adjusted by a lever, and a drum fitted with a brake. Thence it passes on to a "compound paying-out and picking-up machine," which consists of a large drum provided with a friction brake, and round it the cable passes three times; it is also furnished with a steam-engine, which by means of a clutch can be geared on to the drum when required. Now, in paying out, the cable causes the drum to revolve as it runs over it, and the brakes regulate the speed as the vessel moves onward; but should a fault or other accident render it necessary to recover a portion, the drum is stopped and geared on to the engine, the ship's engines are reversed, the stern rudder fixed; and so what was formerly the bow is now the stern, while the little engine hauls in the cable over the same drum which before was used to pay it out; thus it is coiled back into the same tank whence it started. By this means the necessity of passing the cable astern before proceeding to haul it in is avoided. It was during this operation that an accident befell the Atlantic cable in 1865, causing its loss for a time.

The next apparatus is a dynamometer, consisting of three pulleys, one fixed, and the centre one, which rests on the cable, movable in a vertical plane; by this strain is registered and adjusted. After passing this the cable runs into the sea over a pulley carried on girders and constructed so as to swing freely on an axis parallel to the length of the ship, so that, should the vessel make lee-way, the pulley will follow the direction of the cable, and thus friction and sharp bends are avoided. The bows are also fitted with a similar pulley, compound machine, and dynamometer. We see that by these devices the cable is kept perfectly under control, and should a fault be discovered a simple process of reversal of ship and machinery brings home the faulty portion.

Another great point is to keep the vessel trimmed and steady. For the former requirement nine separate water-tight compartments, including the cone in each tank, which also is hollow, are provided, so that water may be admitted as the tanks are emptied of cable, and thus the ship is kept trimmed. To ensure steadiness and avoid the rolling to which telegraph ships are subject, two bilge keels are set on at an angle of 45° ; this was done at the suggestion of Mr. Wm. Froude, whom, said the speaker, "I have to thank for valuable advice and assistance on several new points connected with the 'Faraday.'"

A steam-launch is carried on deck, whence she can be lowered into the water with steam up, ready to land shore ends and perform other useful operations.

Another class of work for which the vessel is fitted is "grappling" for lost or faulty cable. In shallow seas this is a very simple operation, but in deep water it is rather a delicate matter, and requires the co-operation of two or even three vessels, so as to lift the cable without forming an acute angle, and thus to lessen the chance of fracture. A special rope, made of steel wire and hemp and of great strength, is provided for this work. Some specimens shown could bear strains up to 16 tons.

In conclusion, the speaker adverted to the late Professor Faraday, noticing the great services he had rendered to electrical science, his singleness of purpose, and the invariable kindness with which he had encouraged younger labourers in the same field. The friendly encouragement which he himself had experienced from him would ever remain a most pleasing remembrance. He had seized with delight on the present opportunity to pay a tribute to the honoured name of Faraday, and was happy to be able to do this with the full consent of the revered lady who had stood by the philosopher's side for forty years, while labouring under this very roof for the advancement of knowledge. The name of the vessel and her mission in the service of Science would combine, he thought, to create an interest in her favour in the minds of the members of the Royal Institution, and he hoped that on the morrow she would put to sea accompanied by the earnest wish, "God speed the 'Faraday.'"

ON THE DEPENDENCE OF ELECTRICAL RESISTANCE ON TEMPERATURE.

By C. William Siemens,* D.C.L., F.R.S.

Part First.

On the Influence of Temperature upon the Electrical Resistance of Metallic Conductors.

The experimental researches hitherto published on this subject have been limited to temperatures ranging from the freezing to the boiling point of water, and great uncertainty still prevails regarding the law of increase at temperatures exceeding 100° Cent.

The early experiments made by Arndsten † and Dr. Werner Siemens ‡ tend to show that copper, silver, and other pure metals offer electrical resistances which increase with the temperature in an arithmetical ratio within the limits of their experiments, which extended from 0° to 100° Centigrade, whilst subsequent researches by Dr. Matthiessen indicate a slightly divergent ratio between the same limits of temperature.

Platinum, which is, in many respects, a suitable metal for extending these enquiries to higher temperatures, has been left out of consideration in the otherwise exhaustive researches of Matthiessen, and when I first directed my attention to this metal, I observed very extraordinary differences in the electrical conduction of different specimens.

Platinum Wire.—I found it impossible to obtain platinum wire of such a degree of purity that its co-efficient of increment should have a value corresponding with that of silver, and the other pure

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. III. 1874, pp. 296–338.

† Vide Annal. de Chimie, Vol. LIV. 1858, pp. 440–443.

‡ Vide Poggendorff's Annalen, Vol. CX. p. 1, Vol. CXII. p. 353.

metals. Some platinum wire, drawn for me by Messrs. Johnson and Matthey some years since, gave, when measured, a conducting power only 4·7 times that of mercury. Its increase of resistance was from 0·95 units at 20° C. to 1·12 units at 100° C.; or 22·4 per cent. This platinum had been prepared by fusion in a Deville furnace. Platinum recently supplied to me by the same firm, prepared by the old method of forging, had a conducting power of 8·2, whilst it increased in resistance from 0·97 units at 20° C. to 1·23 units at 100° C., or 33·5 per cent. This led me to believe that the process by which platinum is prepared has much to do with its behaviour as a conductor, owing probably to a slight admixture of iridium and other metals of that class, in the fused metal; a supposition which is sufficiently proved by the results tabulated below, and from which it follows, that great caution is necessary in selecting platinum-wire for electrical experiments; and that the fusion of a wire of a given length and diameter for instance, is by no means a test of the strength of an electrical current.

	Kind of Platinum.	Diameter (inches).	Length (inches).	Resistance at 73° Fahr.	Conducting power at 73° Fahr.
1	Pure melted, No. 1	0·062	507·5	0·790	8·6
2	Common soft	0·062	580·5	0·985	7·9
3	Platinum with 5 per cent. iridium	0·021	112·5	1·800	7·2
4	Pure melted, No. 2	0·021	195·0	2·805	8·16
5	Pure forged	0·021	292·0	4·000	8·85
6	Impure melted	0·021	...	...	4·7

The percentage increment of increasing resistance of all these specimens was lower than that of pure silver or copper; but this is really of little practical importance in view of the second part of this inquiry, provided that its coefficient is known, and that it remains constant. A higher coefficient would be of advantage only in so far as by giving greater differences of resistance for given differences of temperature, the readings with it would be proportionately more delicate.

In carrying out my experimental inquiry regarding the dependence of electrical resistance upon temperature, I employed platinum wire of ·009 inches diameter, which had been prepared, by the old welding process (which gives, as already stated, a much more conductive, and therefore, a purer wire than the more recent process by fusion in a Deville furnace). In one of the series of experiments, this wire was wound upon a cylinder of pipeclay, in helical grooves to prevent contact between the convolutions of the wire. To arrive at a knowledge of its electrical resistance, when subjected to various temperatures, I placed it, together with a delicate mercury thermometer made for me by Messrs. Negretti and Zambra, in a copper vessel, contained in a bath of linseed oil, which (in order to prevent the too sudden radiation of its heat, and consequent variation of temperature) was placed within a larger vessel, the space between the two being packed with sand. The leading wires of the platinum-coil were then connected with a Wheatstone's balance and a delicate galvanometer. The bath was very gradually heated by a series of small Bunsen's burners, and whilst the oil was kept in continual motion, the resistance of the platinum wire was read off at intervals of 4° or 5° Centigrade. When the highest point had been reached, the bath was allowed to cool down gradually, and measurements were taken at the same points of temperature as before. This was repeated several times, until about six readings of the resistance of the wire at each point of temperature had been obtained. The mean readings are contained in the first table given at the end of this Part. The platinum wire was carefully annealed, and maintained for several hours at the maximum heat before the observations were taken.

Not satisfied with this single series of experiments, I undertook a second series under somewhat different conditions. Instead of coiling the wire upon a pipe-clay cylinder, I employed a spiral contained in a glass tube and hung by its leading wires in a rectangular air-chamber, about 6 inches long, 3 inches broad, and 3 inches deep, the space between the walls being filled with sand to insure a very steady temperature inside. Three mercury thermometers were inserted through the cover of this double chamber, so that their bulbs stood around the platinum coil in the same horizontal plane. This box was heated externally, by five small Bunsen's burners, a gas pressure regulator being applied to give steadiness of heat. Irregular losses of heat by radiation, or by atmospheric currents, were prevented by a metallic screen surrounding the flames and the heated box.

This apparatus is represented in Fig. 1, Plate 12.

The temperature of the box was gradually raised to 350° Centigrade, and then lowered; and observations were taken at regular intervals of increasing and decreasing temperature.

The results obtained in this further set of experiments are given in the second table. The wire employed was not the same as that employed in the first series, which accounts for certain differences in the ratio of increase observed, although in other respects the accordance of the two series may be considered satisfactory.

In order to test these discrepancies, a third set of experiments was undertaken, with the same platinum wire which had been employed in the second set, with the difference, that the chamber containing the tube and wire and the thermometers was filled with linseed oil. The results are given in the third table, in two brief series, the object being, in this case, to test the former experiments by a few very careful observations in which the flames were so adjusted by a gas-pressure regulator, that a perfectly steady heat could be maintained for an hour, or more, to insure identity of temperature in every part of the chamber.

The general accordance between these results is best shown in the diagram No. 1, Plate 14, where the first, second, and third series of observed results are represented by the lines marked 1, 2, and 3, respectively. The horizontal divisions of the sheet represent Centigrade degrees of temperature measured from the absolute zero of temperature; and the vertical divisions units of resistance divided into tenths.

With the exception of one observation, which has evidently been taken or noted in error, the accordance between the second and third series of observations is satisfactory. They represent a line, curved downwards towards the X axis, which it crosses at a point near the absolute zero, or 274° Centigrade below the freezing point of water.

Copper, Iron, Silver, Aluminium.—No general conclusion could, however, be drawn from the bearing of one metal. I procured, therefore, wires of comparatively pure copper, of fused iron (or mild steel), of silver, and of aluminium, which were subjected to the same series of observations as before described. The results are given in tables 4 to 7, and are also laid down on the diagram according to the same scale as the platinum curve.

Setting aside some palpable errors, these results also produce lines curving downwards to the absolute zero on the abscissal axis, and agree very closely with the measured results obtained by Dr. Matthiessen * between the limits of 0° and 100° Centigrade. They also agree, generally, with the results I obtained by means of another series of observations which I undertook for testing the progressive increase of resistance beyond the range of the mercury thermometer, and which will be noted further on.

Encouraged by these concordant results, I have endeavoured to find a general expression for the increase of electrical resistance in conductors with rise of temperature, which should be based upon a rational dynamic principle.

The experimental curves represented on the diagram differ so little from a straight line, between the limits of 0° and 100° Cent., that the early observers, whose observations did not go beyond those limits, naturally concluded that the electrical resistance increased in an arithmetical ratio with the temperature. In taking the amount of increase between these limits, in copper or silver wire, it was, moreover, found to coincide very nearly with the increase of volume of permanent gases by heat.

Clausius has drawn from these data the conclusion "That the resistances of metals are directly proportional to their absolute temperatures." † Matthiessen, however, found that the increment of increase of resistance was not absolutely constant between the limits of 0° and 100° Cent., but that the ratio of increase in pure metal was expressed by the formula

$$R^t = \frac{R^\circ}{1 - {\cdot}0037647\, t + 0{\cdot}00000834\, t^2},$$

where R° represents the resistance at zero Centigrade and R^t at any other temperature on the same scale, which ratio agrees very closely with my own results between those limits; whereas, at temperatures exceeding 100°, great discrepancies are at once apparent. This will be seen from the following statement of

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* Vide Philosophical Transactions, 1862.

† Vide Poggendorff's Annalen, Vol. CIV. p. 650, 1858.

calculated resistances for the higher temperatures by Matthiessen's formula :—

Temperature in degrees Cent.	Resistance in Units.
t = 0°	Rt. = 1·0000
= 100°	= 1·4146
= 300°	= 1·6098
= 600°	= 0·8314
= 1000°	= 0·1794
= 2000°	= 0·0373

His formula is indeed inapplicable to temperatures exceeding 100° Cent. He adds, it is true, a fourth member to his denominator, containing t^3, which has the effect of harmonizing it more completely with the observed values at low temperatures, without, however, producing more reasonable values for high temperatures. This formula, then, is applicable only within the narrow range of the experiments by which it was determined.

Law of Increased Resistance.—Now if we apply the mechanical laws of work and velocity to the vibratory motions of a body which represent its free heat, we should define this heat as directly proportional to the square of the velocity with which the atoms vibrate. We may further assume that the resistance which a metallic body offers to the passage of an electric impulse from atom to atom is directly proportional to the velocity of the vibrations which represent its heat. In combining these two assumptions, it follows that the resistance of a metallic body increases in the direct ratio of the square root of the free heat communicated to it.

Algebraically, if (r) represent the resistance of a metallic conductor at the temperature T, reckoning from the absolute zero, and α an experimental co-efficient of increase peculiar to the particular metal under consideration, we should have the expression $r = \alpha T^{\frac{1}{2}}$.

This purely parabolical expression would make no allowance for the probable increase of resistance, due to the increasing distance between adjoining particles with increase of heat, which would depend upon the co-efficient of expansion, and may be expressed by βT, which would have to be added to the former expression. To these factors a third would have to be added, expressing an ultimate constant resistance of the material itself at the absolute zero, and which I call γ. The total resistance of a conductor at any temperature, T, would, therefore, be expressed by the formula

$$r = \alpha T^{\frac{1}{2}} + \beta T + \gamma.$$

The law of increase expressed by this formula is graphically represented by diagram No. 3, Plate 16, the spaces between the abscissal axis and the parabola expressing the resistances due to the absolute motion of the particles; the arithmetically increasing field of resistance above the parabolic curve expressing the increase due to increase of distance between adjoining particles; and the field below the X axis, the constant resistance of the material under all conditions. It remained to be seen whether, in giving suitable values to the co-efficients α, β, and γ, this law of increase could be made to coincide with the observed results.

In deciding the abscissal axis according to temperature, and fixing the zero Centigrade at the point where the ordinate equals a unit of resistance in the first diagram, or an amount equal to the specific resistance at that temperature in the second, it will be observed that the portion of the curve where the absolute zero (or any other point of the thermal scale) falls, is completely fixed; and it was important to see whether, in starting from that point, the curvatures as represented by the above formula would agree with those of the experimental lines of the different metals.

Three points of each of the experimental curves, including the zero Centigrade, were taken, and the experimental values for T and r at these points being put into the above formula, the numerical values for α, β, and γ were obtained for each metal. If written down with these numerical co-efficients, the formula is as follows:

For platinum $r = \cdot 0021448T^{\frac{1}{2}} + \cdot 0024187T + \cdot 30425$
$r = \cdot 039369T^{\frac{1}{2}} + \cdot 00216407T - \cdot 24127$
$r = \cdot 092183T^{\frac{1}{2}} + \cdot 00007781T - \cdot 50196$
For copper $r = \cdot 026577T^{\frac{1}{2}} + \cdot 0031443T - \cdot 29751$
For iron $r = \cdot 072545T^{\frac{1}{2}} + \cdot 0038133T - 1\cdot 23971$
For aluminium $r = \cdot 05951436T^{\frac{1}{2}} + \cdot 00284603T - \cdot 76492$
For silver $r = \cdot 0060907T^{\frac{1}{2}} + \cdot 0035538T - \cdot 07456$

Curves constructed in accordance with these expressions are shown in the portions below 0° C. and above 350° C., of diagram 1,

Plate 14, with a constant resistance of 1 unit for each metal at the zero Centigrade, and in diagram No. 2, Plate 15, with each metal represented by its own specific resistance at zero Centigrade, and the close coincidence of the calculated resistances with the experimental resistances as shown in the tables, excepting a certain number of evidently erroneous observations, proves the entire applicability of the law of increase expressed by the formula to various metals at temperatures between 0° and 350° Centigrade. It remained to be proved, however, whether the same law would apply to higher degrees of temperature.

PLATINUM BALL PYROMETER.—For this purpose I had recourse to a pyrometer, constructed upon the supposition that the specific heat of solids and liquids is the same at all temperatures.

An instrument of this description was designed by me some years since, and is used by ironmasters in determining the temperature of their hot blast. It is represented at Fig. 2, Plate 12, and consists of a cylindrical vessel of thin sheet copper capable of containing an imperial pint of water. The inner vessel is surrounded by the two external vessels of thin metal plate, the narrow space between the first and second being filled with air; and the space between the second and third, or the outer vessel, with cow-hair or other non-conductor of heat. A delicate thermometer is fixed against the side of the innermost vessel, being protected from injury by a perforated plate. It is provided with a sliding scale having divisions equal in breadth to the degrees on the thermometer, but each division counting as the equivalent of 50 degrees. A copper or platinum ball is provided, the weight of which is so adjusted that the heat capacity of 50 balls is equal to that of an imperial pint of water at ordinary temperature. This is dropped into the vessel and the sliding scale thereupon fixed so that its zero index shall coincide with the position of the mercury level in the thermometer tube. The copper or platinum ball is perforated, in order that it may be placed at the end of a rod to be exposed to the heat which is intended to be measured.

Upon being fully heated, the ball is dropped into the water, and the reading indicated upon the sliding scale, added to that of the mercury thermometer, gives the temperature of the ball.

Although a high degree of accuracy cannot be claimed for this instrument, its indications are, nevertheless, useful for obtaining fixed ratio indications of the higher temperatures. It has enabled me to test the general accuracy of the ratio of increase of electrical resistance beyond the limits of the more correct tests obtained at the lower temperatures. The accuracy of these corroborative results depends upon the supposition that the specific heat of the metal ball is the same at high and low temperatures ; but, although this may not be, strictly speaking, the case, there is evidence to show that the variations are not of serious import, except probably in nearing the melting points.

The following are some comparative results which have been obtained by placing in the same heated chamber a copper ball of known capacity of heat, and a coil of platinum wire wound in the spiral grooves of a porcelain cylinder and protected from injury by a cylindrical casing of platinum ; both the copper ball and the protected spiral wire were placed inside the heated chamber in a piece of wrought-iron tubing, to ensure more complete identity of temperature, when the resistance of the spiral was taken, and the copper ball dropped into the apparatus just described.

Observed temperature by copper ball pyrometer.	Observed resistance of coil when heated.	Resistance of the same coil at 0° C.	Temperature of coil according to formula $r = \cdot0021448\,T^{\frac{1}{2}} + \cdot0024187\,T + 0\cdot30425$	Difference.
835 C.	30·5	10·56	811° C.	– 24°
854 ,,	32·0	10·56	882° ,,	+ 28°
810 ,,	29·6	10·56	772° ,,	– 38°

It remains to be proved whether the law of increase of electrical resistance, which I have here ventured to put forward, holds good for all conductors ; and whether it may be trusted at temperatures approaching either the point of absolute zero or the melting point of the metal under consideration. The whole subject, indeed, requires further and fuller investigation than I could devote to it with the principal object of my investigation in view, which, having been the construction of a reliable instrument for measuring low and high temperatures by electrical resistance, I have followed up this branch of the enquiry only to such a point as to supply a tolerably reliable basis for such practicable purposes.

FIRST TABLE.

SHOWING THE MEASURED INCREASE OF RESISTANCES WITH THE INCREASE OF TEMPERATURE OF A COIL OF PLATINUM WIRE OF 0·009 INCHES DIAMETER IN OIL.

Mean temperature of three mercury thermometers in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0·0	1·0000	1·0000	...	by inference.
37·8	1·0989	1·0985	− ·0004	
43·3	1·1129	1·1141	+ ·0012	
48·9	1·1269	1·1243	− ·0026	
54·4	1·1405	1·1362	− ·0043	
60·0	1·1543	1·1457	− ·0086	
65·6	1·1676	1·1578	− ·0098	
71·1	1·1812	1·1678	− ·0134	
76·7	1·1947	1·1792	− ·0155	
81·8	1·2068	1·1912	− ·0156	
87·8	1·2209	1·2030	− ·0179	
93·3	1·2338	1·2237	− ·0101	
98·9	1·2469	1·2369	− ·0100	
104·4	1·2695	1·2493	− ·0202	
110·0	1·2723	1·2611	− ·0112	
115·6	1·2851	1·2743	− ·0108	
121·1	1·2974	1·2952	− ·0022	
126·7	1·3099	1·2856	− ·0243	
132·2	1·3222	1·3195	− ·0027	
137·8	1·3344	1·3317	− ·0027	
143·3	1·3465	1·3392	− ·0073	
148·9	1·3587	1·3511	− ·0076	
154·4	1·3707	1·3644	− ·0063	
160·0	1·3826	1·3773	− ·0053	
165·6	1·3945	1·3911	− ·0034	
171·1	1·4061	1·4088	+ ·0027	
176·7	1·4179	1·4426	+ ·0247	
182·2	1·4249	1·4327	+ ·0078	
187·8	1·4420	1·4411	− ·0009	
193·3	1·4524	1·4530	+ ·0006	
198·9	1·4685	1·4615	− ·0070	
204·4	1·4760	1·4745	− ·0015	
210·0	1·4864	1·4797	− ·0067	
215·6	1·4977	1·4909	− ·0068	
221·1	1·5087	1·5096	+ ·0009	
226·7	1·5198	1·5132	− ·0066	
232·2	1·5307	1·5323	+ ·0016	
237·8	1·5363	1·5450	− ·0087	
243·3	1·5526	1·5535	+ ·0009	
248·9	1·5634	1·5596	− ·0038	
254·4	1·5741	1·5778	+ ·0037	
260·0	1·5849	1·5908	+ ·0059	
265·6	1·5935	1·6007	+ ·0048	
271·1	1·6060	1·6001	− ·0059	
276·7	1·6167	1·6195	+ ·0028	
282·2	1·6210	1·6295	+ ·0085	
287·8	1·6375	1·6375	...	

SECOND TABLE.

SHOWING THE MEASURED INCREASE OF RESISTANCES WITH THE INCREASE OF TEMPERATURE OF A COIL OF PLATINUM WIRE OF 0·009 INCHES DIAMETER, IN AIR.

Mean temperature of three mercury thermometers, in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0·	1·0000	1·000	...	In snow and water completely surrounding the oil chamber.
8·5	1·0135	1·012	-·0010	
12·0	1·0296	1·029	-·0006	
14·5	1·0358	1·036	+·0002	
22·3	1·0541	1·048	-·0661	
33·8	1·0835	1·085	+·0015	
34·0	1·0840	1·085	+·0010	
46·2	1·1141	1·107	-·0071	
86·0	1·2123	1·213	+·0007	
100·0	1·2468	1·250	+·0032	In the steam of boiling water. Barometer at 30 in.
112·7	1·2771	1·287	+·0099	
123·2	1·3040	1·309	+·0050	
148·0	1·3486	1·357	+·0264	
159·0	1·3922	1·404	+·0118	
160·3	1·4002	1·404	+·0038	
171·3	1·4224	1·426	+·0036	
187·3	1·4618	1·485	+·0132	
193·5	1·4770	1·492	+·0150	
196·0	1·4832	1·494	+·0108	
197·7	1·4873	1·496	+·0087	
201·0	1·4955	1·499	+·0035	
206·0	1·5080	1·507	-·0010	
206·3	1·5085	1·511	+·0025	
212·3	1·5233	1·529	+·0057	
214·0	1·5275	1·532	+·0045	
214·7	1·5292	1·535	+·0058	
222·0	1·5461	1·559	+·0129	
231·0	1·5693	1·556	-·0033	
238·6	1·5877	1·588	+·0003	
247·0	1·5986	1·603	+·0044	
254·0	1·6258	1·669	+·0332	
264·6	1·6518	1·654	+·0022	
275·0	1·6774	1·698	+·0106	
282·0	1·6946	1·702	+·0074	
304·0	1·7486	1·741	-·0076	
323·0	1·7953	1·780	-·0153	
334·0	1·8220	1·801	-·0210	
340·0	1·8370	1·824	-·0130	

THIRD TABLE.

SHOWING THE MEASURED INCREASE OF RESISTANCES WITH THE INCREASE OF TEMPERATURE OF A COIL OF PLATINUM WIRE OF 0·009 INCHES DIAMETER, IN OIL.

Mean temperature of three mercury thermometers, in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
First Series.				
0	1·0000	1·00	...	In ice surrounding the casing.
15	1·0372	1·04	+ ·0028	
100	1·2454	1·25	+ ·0046	In boiling water.
176	1·4356	1·43	− ·0056	
198	1·4900	1·49	...	
234	1·5788	1·58	+ ·0012	
287	1·7095	1·71	+ ·0005	
312	1·7710	1·77	− ·0010	
340	1·8400	1·84	...	
Second Series.				
0	1·0000	1·00	...	In ice surrounding the casing.
15	1·0372	1·04	+ ·0028	
100	1·2454	1·25	+ ·0046	In boiling water.
162	1·4001	1·40	− ·0001	
208	1·5147	1·52	+ ·0053	
239	1·5911	1·70	+ ·1089	Evidently an error of observation or notation.
303	1·7489	1·75	+ ·0011	
346	1·8547	1·85	− ·0047	

FOURTH TABLE.

SHOWING THE MEASURED INCREASE OF RESISTANCE WITH THE INCREASE OF TEMPERATURE OF A COIL OF COPPER WIRE OF 0·008 INCHES DIAMETER.

First Series.

Mean temperature of three mercury thermometers, in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·0000	1·00	...	In ice surrounding the casing.
23	1·0905	1·09	– ·0005	
100	1·3876	1·38	– ·0076	In boiling water.
166	1·6396	1·63	– ·0096	
168·2	1·6480	1·64	– ·0080	
210	1·8053	1·80	– ·0053	
215	1·8240	1·82	– ·0040	
276	2·0514	2·04	– ·0114	
280	2·0662	2·05	– ·0162	
315	2·1958	2·17	– ·0258	
322	2·2316	2·20	– ·0316	
342	2·2958	2·26	– ·0358	

SHOWING THE MEASURED INCREASE OF RESISTANCE WITH THE INCREASE OF TEMPERATURE OF A COIL OF COPPER WIRE OF 0·008 INCHES DIAMETER, IN OIL.

Second Series.

0	1·0000	1·00	...	In ice surrounding the casing.
15	1·0591	1·06	+ ·0009	
100	1·3886	1·39	+ ·0014	In boiling water.
182	1·7000	1·70	...	
220	1·8427	1·85	+ ·0073	
255	1·9734	1·98	+ ·0066	
296	2·1256	2·13	+ ·0044	
327	2·2400	2·24	...	
346	2·3100	2·32	+ ·0100	

FIFTH TABLE.

SHOWING THE MEASURED INCREASE OF RESISTANCE WITH THE INCREASE OF TEMPERATURE OF A COIL OF IRON WIRE OF 0·0086 INCHES DIAMETER.

Mean temperature of three mercury thermometers, in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
		First Series.		
0	1·0000	1·00	...	In ice surrounding the casing.
15	1·0897	1·09	+ ·0003	
100	1·5857	1·57	− ·0157	In boiling water.
134	1·7758	1·78	+ ·0042	
148	1·8540	1·86	+ ·0060	
216	2·2292	2·10	− ·1291	
260	2·4676	2·47	+ ·0024	Evidently an error of observation or notation.
305	2·7085	2·71	+ ·0015	
347	2·9300	2·93	...	
		Second Series.		
0	1·0000	1·00	...	In ice surrounding the casing.
15	1·0897	1·09	+ ·0003	
100	1·5857	1·57	− ·0157	In boiling water.
140	1·8094	1·75	− ·0594	
198	2·1300	2·13	...	
256	2·4461	2·45	+ ·0039	Evidently an error of observation or notation.
313	2·7457	2·75	+ ·0043	
347	2·9300	2·93	...	

SIXTH TABLE.

SHOWING THE MEASURED INCREASE OF RESISTANCE WITH THE INCREASE OF TEMPERATURE OF A COIL OF ALUMINIUM WIRE OF ·008 INCHES DIAMETER.

Mean temperature of three mercury thermometers, in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
		First Series.		
0	1·	1·	...	In ice surrounding the casing.
14·17	1·06548	1·062	– ·00348	
118·5	1·53118	1·530	– ·00118	
144·25	1·64253	1·642	– ·00053	
211·17	1·92675	1·920	– ·00675	
241·37	2·05288	2·044	– ·00888	
283·2	2·22569	2·221	– ·00469	
304·8	2·31413	2·313	– ·00113	
		Second Series.		
304·8	2·31413	2·313	– ·00113	
263·17	2·1432	2·124	– ·0192	
170·2	1·75358	1·709	– ·04458	
137·77	1·61463	1·565	– ·04963	
89·9	1·40602	1·356	– ·05002	
24·73	1·11388	1·071	– ·04288	

SEVENTH TABLE.

SHOWING THE MEASURED INCREASE OF RESISTANCE WITH INCREASE OF TEMPERATURE OF A COIL OF SILVER WIRE OF ·008 INCHES DIAMETER.

First Series.

Mean temperature of three mercury thermometers, in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·	1·	...	In ice surrounding the casing.
19·93	1·07443	1·074	– ·00043	
66·57	1·24817	1·26	+ ·01183	
118·53	1·44109	1·45	+ ·00891	
150·63	1·55999	1·56	+ ·00001	
219·1	1·81307	1·81	– ·00307	
262·5	1·97313	1·98	– ·00687	
303·8	2·12524	2·13	– ·00476	
Second Series.				
303·8	2·12524	2·13	– ·00476	
275·1	2·01956	2·02	– ·00044	
235·1	1·8721	1 87	+ ·0021	
219·5	1·81454	1·81	+ ·00454	
164·5	1·61132	1·60	+ ·01132	
115·5	1·42988	1·41	+ ·01988	
19·3	1·07208	1·069	+ ·00308	

PART SECOND.

ON MEASURING TEMPERATURES, INCLUDING FURNACE TEMPERATURES, BY ELECTRICAL RESISTANCE.

IN the early days of submarine telegraphs, it frequently happened that the insulated conductor, which had tested well at the cable works, proved faulty after the cable had been submerged, and, upon examining such faulty cable, the metallic conductor was found to have sunk through the gutta-percha covering, an effect which could not be satisfactorily accounted for by accidental causes, such as may arise in joining wires during the process of manufacture; whereas the effect of heat of an intensity of at least 38° Centigrade, or of sufficient intensity to soften or melt the gutta-percha covering of the cable, was generally traceable.

In 1860, when professionally engaged on behalf of Her Majesty's Government in superintending the examination of the electrical condition of the Malta and Alexandria Telegraph Cable, during its manufacture and submersion, it appeared to me that heat, as revealed by its disastrous effects, might be spontaneously generated within a large mass of cable, either when coiled up at the works or on board ship, owing to the influence of the moist hemp and iron wire composing its armature. In considering the means by which such rise of temperature within the mass might be observed, my attention was directed towards that property of metallic conductors of offering, in a rising temperature, an increasing resistance to an electrical current, to which attention has been drawn in the first part of this lecture.

Now, an instrument constructed on the principle of the increase of electrical resistance with rise of temperature, would possess the obvious advantage that the metallic conductor under observation might be at some distance from the observing instrument, and need not be disturbed for making observations. Accordingly, I prepared coils of copper wire insulated with silk, whose electrical resistance having been ascertained and adjusted, were enclosed in iron tubes, with the ends hermetically sealed, but allowing thick insulated leading-wires to pass outward.

These protected coils were placed at various points within the mass of cable as it was coiled in the ship's hold, the insulated leading-wires being taken into the testing cabin. These arrangements proved of great utility in saving this and subsequent cables from destruction; for, although the external layers of cable remained cool to the depth that mercury thermometers could be inserted, the coils placed in the interior of the large mass indicated a steady rise of temperature which had reached 98° Fahr. when the official test was made. A few degrees of additional rise of temperature must have destroyed the insulation of the cable; I therefore urged that cold water should be poured over it. This was not effected without strong opposition on the part of the incredulous; but when at last the water of the Thames, which was covered at the time with floating ice, was pumped over the cable, it issued therefrom at the temperature of 78° Fahr., thus proving the general correctness of the electrical indications previously observed.

It may be here remarked, that in consequence of this practical test, the Government consented to the construction within the ship's hold of water-tight iron tanks, and also to the cable being submerged in water during its passage from the works to its destination, precautions which have ever since been adopted in laying submarine cables.

Stimulated by these results, it occurred to me that an instrument of more general application might be constructed for measuring the temperature of inaccessible places; and that on the same principle, a reliable pyrometer might be made, an instrument of great requisition in the useful arts for obviating the uncertain and contradictory statements regarding the temperature at which smelting and other operations are accomplished. Various practical difficulties were encountered in working out these problems, which have, however, been gradually lessened or overcome, and my labours have resulted in the production of several types of thermometrical and pyrometrical instruments.

When the temperature of an inaccessible place whose temperature has to be measured is not above the boiling point of water, the thermometer coil is variously constructed, according to the position in which it may have to be placed.

THERMOMETRIC RESISTANCE COIL.—The simplest of these is shown in Fig. 3, Plate 12, and consists of a spiral of insulated wire wound upon a cylindrical piece of wood or metal enclosed in a cylindrical silver casing, the two extremities of the wire being soldered to thicker insulated wires, a third thicker wire being joined to one of the other two, the three forming a light cable. This instrument I use for measuring ordinary temperatures on land, and in this form the apparatus would, I conceive, be useful to the physiologist or the medical man for ascertaining the temperature of the human body under certain influences without disturbing it. The instrument is extremely sensitive, and temperatures may, with a good Wheatstone balance, be read off to within a tenth of a degree Fahrenheit.*

In this arrangement of apparatus the indications of the thermometric resistance coil, or instrument described, are read off by direct comparison with a mercury thermometer, which latter will represent the exact temperature of the former at a distance, it may be, of several miles.

The principle is as follows :—

When two similar thermometer coils have different temperatures, they have also different resistances, and, therefore, in order to make them equal, the temperature of the one in the room must be made equal to that of the other at a distance. A plan of the way in which this is arranged is shown in Fig. 4, Plate 12. The two resistances, A and B, forming the left-hand side of the parallelogram, consist of coils of silk-covered German silver wire, each of 500 units, and both wound upon the same bobbin, so as to have the same temperature. The resistance thermometer, T′, of about 500 units, is placed at the distant point, whilst the comparison thermometer, T, precisely equal in respect of material and resistance to T′, is placed in the testing room, and these are connected with the other resistances by the two leading wires, l and l′. The lower end of is put to earth at T′, but the corresponding end of l′ is connected with one side of the resistance thermometer, T′, and then with the

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* An instrument similar in arrangement to the one here mentioned was described by me before the Physical Section of the British Association at Manchester, in 1861 ; and a modified arrangement for measuring deep sea temperature was presented, in the joint names of Dr. Werner Siemens and myself, to the Berlin Academy in 1863.

earth. In the testing room the leading wire, l′, is connected directly with the resistance, B, and with the galvanometer ; whilst l is connected with the resistance A, the galvanometer and the balance thermometer, T. The leading wires, l and l′, are of copper, of the same gauge, insulated with gutta-percha and spun up together, so that they are equally affected by changes of temperature at intermediate places, and have therefore always equal resistances. For protection against mechanical injury, the leading wires are covered with hemp and sheathed with a laminated covering of copper.

Thus arranged, the balance thermometer, T, is immersed in a bath of water, the temperature of which can be varied.

When electrical equilibrium is to be obtained, it is evident that the relation $\frac{A}{B} = \frac{T+l}{T'+l}$, must first be established. And since $A = B$ and $l = l'$, it follows that this equilibrium can only occur when $T = T'$; that is to say, when the resistances of the distant and of the balance thermometers are equal, or in other words, when their temperatures are alike.

In making an observation with this apparatus, it is therefore only necessary to heat or cool the water in which T is immersed, and to read off its temperature upon an ordinary mercury thermometer the moment that electrical equilibrium is observed. The temperature thus noted is that of the distant station.

THERMOMETRIC COMPARISON-COIL.—The comparison-coil, the temperature of which has to be adjusted, consists of a coil of fine silk-covered iron or copper wire, corresponding with the wire employed for, and of a resistance precisely equal to, that of the thermometer-coil at a standard temperature. It is wound upon a short length of metal tube and enclosed in an outer protecting capsule of silver, or other metal, to guard it against mechanical injury and against the ingress of water, which, by causing short circuits between the convolutions, would render its indications inexact. The open end of the protecting capsule is fitted with a vulcanite stopper through which two thick copper leading wires, forming the end of the resistance coil, are passed.

The water bath used with this instrument, and which I have found very convenient for raising or lowering the temperature of the comparison-coil to that of the distant spot, consists of a cylindrical copper vessel, on one side of which a mercury thermometer is fixed in a suitable frame; the bulb and lower part being protected by a perforated shield. There are two funnels for supplying hot and cold water respectively. The cold water pipe ends near the top of the vessel, and is bent outwards, so that the cold water entering and falling to the bottom may distribute itself as it falls. The hot water pipe, on the other hand, ends at the bottom of the vessel, so that the hot water may rise and diffuse itself. In addition to this, the latter pipe is provided with a flexible tube, through which air is blown from the mouth, and bubbling up through the water keeps it well mixed and of uniform temperature.*

When the deflection of the galvanometer needle is towards the left it indicates that the bath is too cold, and vice-versâ. The operator then adds hot or cold water, as the case may be, until the balance of electrical resistance is established, when the mercury thermometer gives a true reading of the temperature at the distant place.

By the use of a similar arrangement of apparatus and burying the resistance thermometers at various depths in the ground, the temperature may, without disturbing the coils, be registered with the utmost accuracy at different periods from year's end to year's end. In like manner, the temperature of the atmosphere at elevated points may be registered in a consecutive manner.

Resistance Coil protected against Water.—In constructing a thermometer adapted for measuring deep-sea temperatures it was necessary to fulfil the following conditions:—(1) The resistance must increase or decrease with a higher or lower temperature, sufficiently to allow of an exact reading to one-tenth of a degree Fahrenheit. (2) The wire must be so protected mechanically that, under the pressure of a column of water of 3,000 fathoms, it would remain perfectly insulated. And (3) the wire must be so coiled as to be readily affected by slight changes of temperature in its vicinity. To effect this, a fine iron or copper

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* Since the above was written, I have adopted a modified arrangement of this apparatus shown in Fig. 5, Plate 13. It consists of a plain cylindrical vessel, into which a moveable tube is immersed, containing the coil and the mercury thermometer. A flange at the bottom of the tube serves to agitate the water in moving the tube up and down, and thus serves to equalize the temperature of the liquid.

wire, insulated with silk, is coiled in two or three layers upon the brass tube, aa, as shown in section in Fig. 6, Plate 13. One end of this wire is soldered to the tube: the other to a copper wire insulated with gutta-percha and carried through a hole to the interior. Over each end of the tube is drawn a piece of vulcanized india-rubber pipe, b and b′, in the space between which the wire is coiled. Over the whole is then drawn a larger india-rubber pipe, cc, which, after being padded outside with hemp yarn, is lashed tightly down by a stout binding wire. The gutta-percha-covered wire forming the insulated end of the coil is placed between the india-rubber pipes, b and c, which are so compressed by the lashing as to close in upon it on all sides. The end of this wire is soldered to one of the leading wires; the other leading wire being soldered to the top of the brass tube. The whole is carried upon the end of the cable or sounding line, which contains the leading wires. The reason for leaving the interior tube open at both ends is to allow a free passage for the water through it, in order to ensure the coil taking quickly the surrounding temperature.

Thermometer coils constructed in this manner are found to be unaffected by any hydrostatic pressure to which they may be subjected. As a test of their insulation, I subject all those intended for deep-sea soundings to pressure under water before being finally connected with the sounding lines.

An instrument of this description was prepared, in 1869, for the Dredging Committee, by which readings could be obtained to one-tenth of a degree of Fahrenheit's scale, with the greatest accuracy, in lowering the thermometer coil to the bottom of the harbour. Unfortunately, however, accurate results could not be obtained in deep water, because the motion of the ship rendered the needle of the galvanometer employed too unsteady to allow of dependence being placed upon its indications.*

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* A similar apparatus has been taken out on board H.M.'s steam-ship *Challenger*, in her exploring expedition, in which a Thomson marine galvanometer was substituted for the more simple instrument used on the previous occasion, and which is better suited for taking readings notwithstanding the motion of the vessel. Considering that the zero position of the galvanometer has only to be ascertained, the difficulty of operating with this instrument would not be considered great by those who are accustomed to electric observations on board ship, although they are still considerable to the uninitiated in this class of observations, and renders the production of a more simple current detector a matter of considerable interest.

Resistance Coil protected by Platinum.—The very high degree of heat to which pyrometers have to be raised, renders it necessary to construct them as nearly indestructible by fire as possible, and of a material which is not liable to any permanent change by sudden variations in and elevation of temperature. Platinum is a metal which is well suited for this purpose, in every way, as it does not, when annealed, alter its specific electrical conductivity by the application of heat; whilst the variation of its measured resistance, due to change of temperature, is sufficiently great to allow of exact readings. But special precautions had to be observed in providing a resistance wire of suitable quality, and in protecting the same from the hot gases of furnaces, which would exercise a chemical action upon it.

The pyrometer coil which I prefer is made of fine platinum wire of 0·01 inch diameter, the resistance of which averages 3·6 units per yard of length. This wire is coiled upon a cylinder of hard baked pipe-clay in which a double threaded helical groove is formed, to prevent the convolutions from coming into contact with each other. The form of pipe-clay cylinder is shown in Fig. 7, Plate 13. At each end of the spiral portion, BB, it is provided with a ring-formed projecting rim c and c′, the purpose of which is to keep the cylinder in place when it is inserted in the outer metal case, and to prevent the possibility of contact between the case and the platinum wire. Through the lower ring c′, are two small holes, bb′, and through the upper portion two others aa′. The purpose of the upper holes, aa′, is for passing the ends of the platinum wires through, before connecting them with the leading wires. From these two holes, downwards, platinum wires are coiled in parallel convolutions round the cylinder to the bottom, where they are passed separately through the holes bb′. Here, they are twisted, and, by preference, fused together by means of an oxy-hydrogen blow-pipe. At this end, also, the effective length and resistance of the platinum wire can be adjusted, which is accomplished by forming a return loop of the wire, and providing a connecting screw-link of platinum, L, by which any portion of the loop can be cut off from the electric circuit.

The pipe-clay cylinder is inserted in the lower portion, AA, of the protecting case, shown in Fig. 8, Plate 13. This part of the case is made of iron or platinum, and is fitted into the long tube, CC, which is of wrought iron, and serves as a handle. When the lower end of the casing is of iron, there is a platinum shield to protect the coil on the pipe-clay cylinder. The purpose of the platinum casing is to shield the resistance wire against hot gases, and against accident. At the points, AA, fig. 7, the thick platinum wires are joined to copper connections, over which pieces of ordinary clay tobacco-pipe tube are drawn, terminating in binding screws fitted to a block of pipe-clay, closing the end of the tube. A third binding screw is provided, which is likewise connected with one of the two copper connecting wires, serving to eliminate disturbing resistances in the leading wires, as will be explained in the third part of this paper.

If temperatures not exceeding a bright red heat are to be measured, the platinum protecting tube may be dispensed with and iron or copper substituted.*

INSULATION OF PIPE-CLAY CYLINDER.—The pipe-clay tube, upon which the platinum wire is wound, is, when cold, highly insulating; when heated, its conducting power increases, though not to such an extent as to occasion any perceptible error.

In order to investigate the extent of this increasing conductivity, I coiled a length of platinum wire round a pipe-clay pyrometer cylinder in the ordinary way between the leading wires, and then cut the wire at the bottom, so that the current passing between the leading wires would have to traverse the body of the pipe-clay,

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* In experimenting with pyrometers with platinum casings, no appreciable deterioration of the platinum wire or change in its conductivity at 0° Centigrade has been observed, beyond what is due to the complete annealing of the wire in the first instance. With a view, however, of saving expense, the protecting tube of subsequent instruments was made of wrought iron; and an instrument of this construction was submitted for trial to a committee appointed by the British Association in 1872–73. To my surprise it was found that each time, after the coil had been exposed to intense heat, the platinum resistance at standard temperature was permanently increased; and, on examining the wire, it was found to present a rough surface, and had become brittle. Prof. A. W. Williamson, the Chairman of this committee, suggested that this change might be owing to the reducing atmosphere produced by the highly heated iron casing, which would cause the platinum to combine with a trace of reduced silicon, taken from the pipe-clay cylinder in contact with the same. An analysis by Prof. Williamson of the altered wire confirmed this view, and proved beyond doubt the necessity of an oxidizing or neutral atmosphere within the protecting chamber. This condition will be best obtained in making the protecting casing of platinum; but for ordinary purposes an iron casing well enamelled on the inner surface, or containing a lining of porcelain, will answer equally well.

and then measured its resistance at various temperatures, with the following results :—

Cold	1,000,000	units.
At intervals whilst red-hot .	12,000	,,
	8,000	,,
	7,000	,,
	6,000	,,
	3,700	,,
	4,000	,,
At white heat	3,700	,,
At intervals, in a gas furnace intensely heated . . .	700	,,
	650	,,
	650	,,
	550	,,
	500	,,

The resistance of the cylinder, when cold, returned to its original value, and after repeated experiment, produced the same results, whence it follows that the amount of error caused by conduction of the pipe-clay cylinder, is practically inappreciable until a white heat has been reached : but that in measuring temperatures exceeding a white heat, it is the tendency of the instrument to indicate a slightly lower value than the true one.

In order to avoid inaccuracy from this source, it is desirable to expose the instrument to intense heat for three minutes only, on an average, at the end of which time the observation should be taken. This period of exposure will have sufficed to heat the protecting capsule, and the platinum resistance wire, to within narrow limits of the full temperature of the furnace, whilst it will have been insufficient to penetrate and soften the pipe-clay cylinder. The error caused by an invariable and insufficient period of exposure is, moreover, proportional to the temperature, and can be determined by experiment at a temperature below white heat.

In adapting the resistance thermometer to the measurement of high temperatures, a wide range of resistance is obtained, and it is no longer necessary to determine these resistances with the same precision as in measuring slight variations of ordinary temperature. In this case I dispense with the use of galvanometers and substitute for the same an instrument which I propose to call a differential voltameter. The method of measuring electrical resistances by the aid of this instrument will be described in the Third Part of this Paper.

Although the principle involved in the increase of electrical resistances with increasing temperatures is an extremely simple one, the difficulties which had to be overcome in constructing practically useful instruments for measuring high and low temperatures, were considerable. Various combinations and appliances had to be tried for protecting the thermometer coils against hydrostatic pressure, or against the destructive heat of furnaces. The disturbing effect of leading wires had to be eliminated, and the reading of the instrument rendered independent of mechanical or magnetic influences, and brought within the compass of observers untrained for the delicate work of the electrician.

But the greatest drawback consisted in the imperfect state of electrical science respecting the ratio of increase of electrical resistance with increase of temperature, for temperatures exceeding the boiling point of water. Platinum is the only available metal for high temperatures, and little was known of the ratio of increase of this metal even at ordinary temperatures. I was, therefore, obliged to undertake the series of experiments, with the view of determining the increase of platinum resistance up to high temperatures, tending to the establishment of the general law with regard to electrical resistances—which has been dealt with in the First Part of this Paper.

The resistance thermometer and pyrometer have already been applied to useful work. Professor Bolzani, of Kasan, uses them for registering cosmical temperatures at points above and below the surface of the earth. Mr. I. Lowthian Bell, the eminent metallurgist, employs the latter for determining the temperatures at which the various operations of the blast furnace are carried on; and I have had various occasions, in addition to the one already referred to, of obtaining useful information regarding the temperature of furnace gases, &c., by the aid of these instruments.

PART THIRD.

ON A SIMPLE METHOD OF MEASURING ELECTRICAL RESISTANCES.

RESISTANCE MEASURERS AND GALVANOMETERS.—Although the Wheatstone balance furnishes the electrician with the means of measuring the resistance of electrical circuits with great accuracy, provided only that reliable resistance scales and a delicate galvanometer are at hand, its application is, in many cases, rendered difficult on account of the delicacy of the apparatus and of extraneous disturbing causes.

In cases where a portable instrument is required which may have to be entrusted to inexperienced hands, the want of a more simple method of ascertaining electrical resistances makes itself particularly felt. Having had occasion to require such an instrument for measuring temperatures at inaccessible places, I projected, some years since, a "resistance measurer," which has been described by the Electrical Standard Committee of the British Association, in their report, at Dundee, of 1867, and which is based upon the power of balancing the potential values of two equal coils upon a magnetic needle, by changing their relative distance from it, according to the intensity of the two branch currents emanating from the same battery; this distance being made the measure of the unknown resistance inserted in one of the two branches.

Dr. Werner Siemens has produced a measuring instrument of greater scope and convenience, in which an index handle (moving a contact roller upon a wire in a circular groove) is carried round upon a divided scale until a magnetic needle in the centre of the apparatus assumes its zero position, when the unknown resistance is indicated upon the scale. The same instrument is suitable for measuring greater resistances by the sine method; it is also a tangent galvanometer, and has received the appropriate appellation of a "universal galvanometer."

These and other ready methods which have been projected for measuring electrical resistances are useful auxiliaries to the Wheatstone bridge, from which they differ chiefly in obviating the necessity of elaborate resistance scales, without, however, removing the difficulty of dealing with a delicate galvanometer.

Professor Sir William Thomson has produced a marine galvanometer, which is nearly independent, in its action, of external magnetic influences and of the disturbing influence of the ship's motion. But these advantages are not realized without the sensitiveness of the instrument being, to a very great extent, sacrificed. By mounting the magnetic needle of the instrument upon a vertical spindle resting upon the end of a lever vibrating under the influence of a Neef's hammer, I succeeded in obtaining greater sensitiveness, but at the cost of a more complicated apparatus.

THEORY OF DIFFERENTIAL MEASUREMENT.—At this stage of my inquiries, it occurred to me that both the resistance scales and the galvanometer might be dispensed with in measuring electrical resistances, by reverting to the principle of the voltameter in combination with that of differential measurement.

Faraday established the law that the decomposition of water in a voltameter in an unit of time is a measure of the intensity of the current employed; or, that $I = \frac{V}{t}$; I being the intensity, V the volume, and t the time.

According to Ohm's general law, the intensity, I, is directly governed by the electro-motive force, E, and, inversely, by the resistance, R, of the electric circuit, or, it is $I = \frac{E}{R}$.

Combining the two laws, we have $V = \frac{E}{R} t$, which formula would enable us to determine any unknown resistance, R, by the amount of decomposition effected in a voltameter in a given time, and by means of a battery of known electromotive force.

Practically, however, such a result would be of no value, because the electromotive force of the battery is counteracted by the polarization, or electrical tension, set up between the electrodes of the voltameter, which depends upon the temperature and concentration of the acid employed, and upon the condition of the platinum surfaces composing the electrodes. The resistance to be measured would, moreover, comprise that of the voltameter, which would have to be frequently ascertained by other methods, and the notation of time would involve considerable inconvenience and error. For these reasons the voltameter has been hitherto discarded as a measuring instrument, but the disturbing causes just enumerated may be eliminated by combining two similar voltameters in one instrument, which I propose calling a "differential voltameter," and which is represented in the drawing, Fig. 9, Plate 13.

DIFFERENTIAL VOLTAMETER.—It consists of two similar narrow glass tubes, A and B, of about 2·5 millimetres in diameter, fixed vertically to a wooden frame, F, with a scale behind them divided into millimetres or other divisions. The lower ends of these tubes are enlarged to about 6 millimetres in diameter, and each of them is fitted with a wooden stopper saturated with paraffin and pierced by two platinum wires, the tapered ends of which reach about 25 millimetres above the level of the stopper. These form voltametric electrodes.

From the enlarged portion of each of the two voltameter tubes a branch tube emanates, connected, by means of an india-rubber tube, the one to the moveable glass reservoir G and the other to G′, Fig. 9. These reservoirs are supported in sliding frames by means of friction springs, and may be raised and lowered at pleasure. The upper extremities of the voltameter tubes are cut smooth and left open, but weighted levers, L and L′, are provided, with india-rubber pads, which usually press down upon the open ends, closing them, but admitting of their being raised, with a view of allowing the interior of the tubes to be in open communication with the atmosphere. Having filled the adjustable reservoirs with dilute sulphuric acid, on opening the ends of the voltameter tubes, the liquid in each tube will rise to a level with that of its respective reservoir, and the latter is moved to its highest position before allowing the ends of the tubes to be closed by the weighted and padded levers.

The ends of the platinum wire forming the electrodes may be platinized with advantage, in order to increase the active surface for the generation of the gases.

PYROMETER AND VOLTAMETER CONNECTED.—Figure 10 represents the connections of the voltameter with the pyrometer, and also shows the necessity for the third leading-wire referred to at p. 165 in the Second Part of this Paper. One electrode of each voltameter is connected with a common binding screw, which latter may be united, at will, to either pole of the battery, whilst the remaining two electrodes are, at the same moment, connected with the other pole of the same battery; the one through the constant resistance coil, X, and the other through the unknown resistance, X′. This unknown resistance, X′, is represented to be a pyrometer-coil described in the Second Part of this Paper.

By turning the commutator seen at Fig. 9 either in a right or left hand direction from its central or neutral position (in which position the contact springs on either side rest on ebonite), the current from the battery flows through the two circuits, causing decomposition in the voltameters; and the gases generated upon the electrodes accumulate in the upper portions of the graduated tubes. By turning the commutator half round every few seconds the current from the battery is reversed, which prevents polarization of the electrodes, as already stated. When through the position of the commutator the current flows from the copper, it passes first through the connected electrodes to the voltameters, where it divides, one portion passing through the constant resistance, X, through the leading wire, X, to the pyrometer, returning by the leading wire, C, to the battery, the other passing through X′, through the leading wire, X′, through the platinum coil, returning by the leading wire, C, to the battery. When the current flows from the zinc it passes first through the leading wire, C, the current dividing at the pyrometer, one portion returning by the leading wire, X, through the constant resistance, X, through one voltameter tube to the battery, and the other through the platinum coil, X′, through the leading wire, X′, to the other voltameter tube, and thence to the battery. The value of the third leading wire, C, in eliminating the disturbing effect which long and short leading wires with varying temperature would certainly have upon the correct indications of the instrument is at once evident.

The relative volumes, v and v′, of the gases accumulated in an arbitrary space of time within each tube must be inversely proportional to the resistances, R and R′, of the branch circuits, because $v : v' = \frac{E}{R} t : \frac{E}{R'} t$, and, therefore, $v : v' = R' : R$.

The resistances, R and R′, are composed, the one of the resistance, C, plus the resistance of the voltameter, A, and the other of the unknown resistance, X, plus the resistance of the voltameter, B. But the instrument has been so adjusted that the resistances of the two voltameters are alike, being made as small as possible, or equal to about 1 mercury unit, to which has to be added the resistances of the leading wires, which are also made equal to each other, and to about half a unit; these resistances may therefore both of them be expressed by γ.

We have, then $v' : v = C + \gamma : X + \gamma$
or

$$X = \frac{v}{v'}(C + \gamma) - \gamma$$

which is a convenient formula for calculating the unknown resistance from the known quantities C and γ, and the observed proportion of v and v′.

The constant of the instrument (γ) is easily determined, from time to time, by substituting a known resistance for X, and observing the volumes, v and v′, after the current has been acting during an arbitrary space of time, when in the above formula, γ, has to be separated as the unknown quantity, giving it the form

$$\gamma = \pm\frac{v' X - v C}{v - v'}.$$

The condition of equality between the internal resistances of both voltameters is ascertained by inserting equal known resistances in both branch circuits, when $v = v'$ should be the result. Failing this, the balance is generally re-established by reversing the poles of the battery, the reason being that hydrogen electrodes are liable to accumulate metallic or other deposit upon their surfaces, which is effectually removed by oxygen.

Such reversals of current should be effected at frequent intervals during the observation. Should this not suffice to establish a balance of resistance, it will be necessary to push the electrodes of the voltameter of greater resistance a little further into the tube.

The constant resistance of the instrument should, as nearly as possible, represent a geometrical mean of the range of resistances intended to be measured, because the greatest degree of accuracy is obviously obtained when the quantities, v and v′, are nearly alike. If the difference between v and v′ is very great, the constant γ introduces an error into the result, because $\frac{C + \gamma}{X + \gamma}$ is not equal to $\frac{C}{X}$ unless C equals X. In order to work this instrument between wide ranges of temperature, it becomes necessary to make C variable, and nearly equal to X. It is also obviously desirable to have γ very small as compared with X. Reliable observations can, however, be obtained between the limits of $v = 10\ v'$ and $10\ v = v'$, from which it follows that, with a fixed coil, $C = 10$ units, resistances may be measured (subject to correction for the disproportion introduced by the value of γ), between the limits of 1 and 100 units. In adding a reserve coil of 1,000 units, the scope of the instrument can be extended from 1 unit to 10,000 units. Greater accuracy for resistances between 50 and 500 units would, however, be insured by providing a third resistance of 100 units.

PRECAUTIONS NECESSARY IN USING THE INSTRUMENT.—Certain precautions have to be taken to insure reliable results in using the instrument.

1. The dilute acid employed in both tubes should be of the same strength, a condition which is easily realized in preparing a standard solution of about 9 measures of distilled water for one measure of chemically pure sulphuric acid; to be kept in a bottle for replenishing the instrument when required. The moveable reservoirs being closed by a cork, with but a small hole for the admission of air, will rarely require replenishing.

2. When the instrument has been refilled or has not been used for some days, it is advisable to verify the equality of resistance of both voltameters and their connection by passing the battery current through them for some minutes with equal resistances inserted in each branch. If a difference between the volumes of gases should be observed, the binding screws and the pads of india-rubber closing the tubes should be examined and the experiment repeated. It is possible that an irregularity may be observed in the first trial, owing to a difference in the condition of polarity between the two sets of electrodes, which will disappear when both shall have been subjected to reversed currents proceeding from the same battery; the solutions will, moreover, be fully and equally saturated with gases, and absorption of the gases avoided.

3. The battery power used should be proportional to the re-

sistances to be measured, viz. :—For resistances not exceeding 100 units, from 5 to 6 Daniell or Leclanché elements, which cause an active decomposition without sensibly heating the coils or effecting a partial insulation of the electrodes by excessive generation of gases; for resistances of from 100 to 1,000 units, the number of elements may be increased with advantage to 15 or 20, and a still greater number of elements may be employed in measuring resistances exceeding 1,000 units.

It is not advisable under any circumstances to use less than five Daniell's elements, although active decomposition may be obtained with a less number, for the reason that the voltameter itself exercises an opposing electro-motive force by polarisation, which may vary under certain conditions from 1·1 to 1·3 Daniell's elements, and that these variations would exercise a sensible difference in the result if the electromotive force of the battery did not very decidedly predominate.

In using large battery power the heating of the coils has to be guarded against, which may, however, be easily done by arresting the current, in reversing it, from time to time, whilst allowing the gases in the tubes to accumulate until a sufficiently precise reading can be obtained. From two to four minutes duration of current will, under general circumstances, suffice to fill the tubes.

4. The india-rubber pads should from time to time be smeared with a waxy substance, to prevent escape of gas between them and the edge of the glass tube, and I find that paraffin answers well for this purpose.

5. The state of the barometer has no influence upon the reading of this instrument, because fluctuations of the atmospheric pressure affect both branches equally. A slight error through difference of pressure would, however, arise if the reading of the instrument were taken after the current had ceased to act, and the reservoirs were to remain in their elevated position opposite the zero point of the scale, exercising a hydrostatic pressure equal to the depression of the liquids in the tubes. In order to eliminate this source of error, the two moveable reservoirs must be lowered until a balance of levels is established on each side between the tube and its reservoir before the reading is taken. This being done, the weighted lever is raised from each tube for the discharge of the gases, and the moveable reservoirs are raised back to their zero position.

6. Although, by careful selection, two tubes of nearly equal diameter may be obtained, it would not be safe to depend upon such uniformity where accurate results are required. Each tube should, therefore, be calibrated, and provided with its own scale; and, in case of a tube having to be replaced, a suitable new scale should also be provided. The smaller the diameter and the greater the length of the tubes, the greater will be the accuracy of the observations; but a limit is here imposed, by the necessity of the gas-bubbles rising freely to the surface, which limit is reached in reducing the tubes to 2 millimetres of diameter.

A much smaller diameter would suffice, if the gases were merely to propel a water-column before them in a horizontal tube, but I found that under such circumstances the resistance of the liquid by adhesion to the sides, caused considerable error and inconvenience in the manipulation of the instrument.

Having measured numerous resistances by this instrument, and compared the results with measurements obtained by a very perfect Wheatstone bridge arrangement, I find that it may be relied upon within one-half per cent. of error of observation, excepting at the extremes of the range, where a somewhat greater amount of error easily occurs unless special care be taken in reading the comparatively few divisions on the one side. A higher degree of accuracy is, in such a case, to be attained by filling the one tube several times (noting the volume each time), and allowing the other to continue accumulating, until at least 100 divisions of the scale shall have been passed.

A table has been prepared which gives the temperatures corresponding to the volumes of the gases of decomposition observed in the tubes, thus saving all calculation on the part of the metallurgist, or other observer.[*]

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[*] The manner in which the equation of the curve of increase of resistance with temperature is applied to the construction of the table here referred to is the following: the coefficients of the platinum wire employed, that is, the quantities α, β, γ, have first to be calculated, from a series of experiments made for that purpose, with one unit of resistance at zero Centigrade. The constant of the voltameter γ has next to be obtained in the manner explained at p. 172, and the resistance X of equation, p. 172, has then to be equated with that of r, given at p. 148.

The following is the calculation employed for the construction of the tables. The constant C is equal to 17 units, the resistance γ to 2 units, the platinum coil in the pyrometer has a resistance of 10 units at zero Centigrade, and the coefficients of the platinum wire employed are $\alpha = \cdot039369$, $\beta = \cdot00216407$, $\gamma = -\cdot24127$;

In using such a table, the temperature measured by the apparatus is found indicated at the intersection of the two columns of figures, expressing the volumes of gases observed in the two tubes V and V_1; these figures commence only with 40, because it is not considered advisable to take an observation until at least 40 unit volumes of gas have been developed in each tube. Care is to be taken that no leakage of gas takes place under the weighted cushions, which is easily observed in allowing the depressed columns to stand without lowering the reservoirs when the levels between gas and liquid should remain constant. Although the differential voltameter here proposed for measuring electrical resistances not exceeding the limits of metallic and earth circuits does not surpass, or even equal the Wheatstone bridge arrangement for accuracy, when the latter is carefully prepared, and in the hands of a skilful operator, it yet possesses advantages of its own which will, I trust, recommend it to the notice of electricians. One of

then equating the values of the resistances as given by the equations of p. 148 and p. 172 respectively :—

$$10\ (\alpha t^{\frac{1}{2}}+\beta t+\gamma)=\frac{v}{v'}\ (17+2)-2$$

$$\alpha t^{\frac{1}{2}}+\beta t+\gamma=\frac{1{\cdot}9v}{v'}-{\cdot}2$$

$$\alpha t^{\frac{1}{2}}+\beta t=\frac{1{\cdot}9v}{v'}-\ ({\cdot}2+\gamma)$$

$$t+\frac{\alpha}{\beta}t^{\frac{1}{2}}+\left(\frac{\alpha}{2\beta}\right)^2=\frac{1{\cdot}9}{\beta}\cdot\frac{v}{v'}-\left(\frac{{\cdot}2+\gamma}{\beta}\right)+\frac{\alpha^2}{4\beta^2}$$

$$t^{\frac{1}{2}}+\frac{\alpha}{2\beta}=\left\{\frac{1{\cdot}9}{\beta}\cdot\frac{v}{v'}-\left(\frac{{\cdot}2+\gamma}{\beta}\right)+\frac{\alpha^2}{4\beta^2}\right\}^{\frac{1}{2}}$$

$$t^{\frac{1}{2}}=\left\{\frac{1{\cdot}9}{\beta}\cdot\frac{v}{v'}-\left(\frac{{\cdot}2+\gamma}{\beta}\right)+\frac{\alpha^2}{4\beta^2}\right\}^{\frac{1}{2}}-\frac{\alpha}{2\beta}$$

$$t=\left[\left\{\frac{1{\cdot}9}{\beta}\cdot\frac{v}{v'}-\left(\frac{{\cdot}2+\gamma}{\beta}\right)+\frac{\alpha^2}{4\beta^2}\right\}^{\frac{1}{2}}-\frac{\alpha}{2\beta}\right]^2$$

Substituting the values α, β, γ, and remembering that the formula is calculated for the absolute scale of temperature, the formula for the Centigrade scale will take the following form, which is that given at the foot of the table :—

T° Centigrade

$$=\left[\left\{877{\cdot}975\times\frac{v}{v'}+19{\cdot}070544+82{\cdot}738226\right\}^{\frac{1}{2}}-9{\cdot}0960553\right]^2-274$$

$$=\left\{(877{\cdot}975\times\frac{v}{v}+101{\cdot}80877)^{\frac{1}{2}}-9{\cdot}0960553\right\}^2-274.$$

By means of this formula, the temperature of the resistance coil, which gives a ratio of volumes in the voltameter tubes greater than the maximum given in the Table can be calculated, and the constants required for the calculation have been its intrinsic advantages is, that it gives the resistance to be measured in "work done," which is independent of the momentary changes in the strength of a current, by charge or electrification, that influence the temporary reading of a magnetic needle.

It recommends itself for use on board ship, not being in the slightest degree influenced either by the motion of the vessel, or by the magnetic influence of its moving mass of iron.

Its simplicity of construction is such, that each part can easily be examined and verified.

It can be used satisfactorily by persons unaccustomed to the delicate handling requisite in dealing with galvanometers, and elaborate resistance scales; it is very portable; and lastly its cheapness of construction brings it within the reach of students and others, who might not be well able to afford an expensive apparatus.

The following tables of actual measurements of resistances, made by Mr. Lüdtge, Ph.D., shows the degree of accordance between the findings of this instrument, and those of a very complete Wheatstone bridge arrangement, which may be deemed satisfactory.

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given in the Table for that purpose. The following is an instance of its application, in which V = 127 and V′ 41 volumes.

$$\begin{array}{lll}
\text{log. } 877{\cdot}975 & = & 2{\cdot}9434822 \\
+\text{log. } 127 & = & \underline{2{\cdot}1038037} \\
 & & 5{\cdot}0472859 \\
-\text{log. } 41 & = & \underline{1{\cdot}6127839} \\
\text{log. } 2719{\cdot}518 & = & 3{\cdot}4345020 \\
+\quad \underline{101{\cdot}80877} & & \\
\text{log. } 2821{\cdot}32677 \div 2 & = &)\underline{3{\cdot}4504534} \\
\text{log. } 53{\cdot}11616 & & 1{\cdot}7252267 \\
-\quad \underline{9{\cdot}0960553} & & \\
\text{log. } 44{\cdot}0201047 \times 2 & = & 1{\cdot}6436510 \\
 & & \underline{2} \\
\text{log. } 1937{\cdot}7 & = & 3{\cdot}2873020 \\
-\quad \underline{274} & & \\
1663{\cdot}7 & = & 1664^{\circ} \text{ Centigrade nearly.}
\end{array}$$

The resistance of 17 units in the voltameter is made of German silver wire, so that the variation of its resistance with that of atmospheric temperature shall be so small as not to affect the correctness of calculated results.

FIRST SERIES.

Resistance according to Wheatstone's Diagram.	Resistance according to Proposed Differential Voltameter.	Difference.	Constant Resistance. C.	Battery (Daniell's Elements).
0·2	0·2	0·0	0	5
0·5	0·5	0·0	...	...
0·8	0·8	0·0	...	...
1·2	1·21	0·01	...	...
2·0	2·0	0·0	...	...
4·0	4·0	0·0	5	...
6·0	5·95	−0·05	...	...
7·5	7·6	0·1	...	...
10·0	10·03	0·03	10	...
14·0	13·89	−0·11	...	...
20·0	20·0	0·0	...	6
27·5	27·47	−0·03	...	...
30·0	30·1	0·1	...	...
34·0	33·82	−0·18	10	6
42·0	41·9	−0·1	...	...
50·0	49·8	−0·2	...	...
54·0	54·0	0·0	100	...
60·0	60·0	0·0	...	8
68·0	68·42	0·42	...	...
74·0	74·06	0·06	...	...
82·0	81·9	−0·1	...	...
90·0	90·0	0·0	...	...
95·0	95·2	0·2	...	...
100·0	99·97	−0·03	...	10

SECOND SERIES.

Resistance according to Wheatstone's Diagram.	Resistance according to Proposed Differential Voltameter.	Difference.	Constant Resistance. C.	Battery (Daniell's Elements).
0·5	0·5	0·0	5	8
0·8	0·5	– 0·3	...	...
1·0	1·09	0·09	...	...
5·4	5·32	– 0·08	...	...
10·0	9·82	– 0·18	...	...
14·0	13·70	– 0·30	...	...
50·0	48·83	– 1·17	10	...
80·40	80·02	– 0·38	...	...
98·00	98·20	0·2	50	...
105·00	105·15	0·15	...	...
130·40	130·40	0·0	...	9
156·00	155·80	– 0·2	...	...
192·00	192·00	0·0	...	...
205·00	204·70	– 0·3	...	10
240·00	240·14	0·14	...	...
284·00	283·95	– 0·05	...	...
300·00	300·30	0·30	100	15
312·00	312·00	0·0	...	...
321·00	321·00	0·0	100	15
335·00	334·90	– 0·1	...	...
350·00	350·02	0·02	...	...
385·00	385·00	0·0	...	...
400·00	400·30	0·3	...	...
425·00	425·00	0·0	...	...
432·00	432·00	0·0	...	...
465·00	467·70	– 0·3	...	...
500·00	500·40	0·4	...	20
520·00	519·90	– 0·1	...	...
557·00	556·70	– 0·3	...	...
582·00	582·00	0·0	...	...
605·00	605·05	0·05	...	...
624·00	624·00	0·0	...	...
674·00	674·00	0·0	...	...
700·00	699·50	– 0·5	...	...
723·00	723·00	0·0	...	...
750·00	749·80	– 0·2	...	25
805·00	804·30	– 0·7	500	...
846·00	847·00	1·0	...	...
906·00	906·00	0·0	...	...
928·00	929·20	1·2	...	...
1008·00	1006·50	– 1·5	1000	35
1060·00	1062·00	2·0	...	...
1130·00	1130·00	0·0	...	...
1250·00	1254·00	4·0	1200	...
1300·00	1303·50	3·5	...	40
1340·00	1340·00	0·0	...	...
1410·00	1406·20	– 3·8	1400	45
1470·00	1471·20	1·2	...	...
1500·00	1500·00	0·0	1500	50
1550·00	1553·00	3·0	...	55

In the discussion of the Paper

"ON THE TELEGRAPH CABLE-SHIP 'FARADAY,'"

By C. W. Merrifield,

Dr. Charles W. Siemens* said : As the owner and user of the ss. "Faraday," I may be expected to make a few observations. First, with regard to her construction. Our object was not to produce a ship of extraordinary dimensions or of peculiarity of construction, but rather to accomplish an engineering object, that of laying a cable across the Atlantic on more advantageous terms than the "Great Eastern" would or could have done it. I may therefore say that this ship grew under our hands. The carrying capacity of the ship was given by the amount of cable which had to be carried. The questions then arose how to arrange this load in the ship, and how to make the ship manageable under all circumstances. The paper has stated already that the ship was constructed by Messrs. Mitchell, who have discharged their duty in a very perfect manner ; and I should hardly have ventured upon so many points of novelty if I had not been supported by my friend Mr. Froude, who, as regards several of the details adopted, assisted us with his advice throughout. One of the difficulties in cable ships is that they roll enormously. It is true that the load is in large masses in the cable tanks, and very low, but still that hardly seems to account for the tendency these ships have to roll. One of our objects was to make this ship free from that great inconvenience, because in laying a cable, and still more in grappling for one and in splicing, the rolling motion is a very great evil. Mr. Froude suggested that there should be two enormous bilge keels instead of an ordinary keel to this ship, and the result has been very satisfactory indeed. But what is of still greater importance is that a cable ship should be able to be manœuvred in a way which is quite unnecessary in the case of ordinary mercantile ships ; for not only has she to obey her helm

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* Excerpt Transactions of the Institute of Naval Architects, Vol. XVII. 1876, pp. 206–208.

very rapidly, but she has to remain in a certain position with her head to the wind perhaps for hours, and to go at a speed perhaps of one mile an hour only with a side wind upon her. Unless she is able to do this she is of no use for bringing up a cable from the depths of the Atlantic except during exceptionally fine weather. With this ship we have accomplished the most delicate operations on the Atlantic when it was blowing almost a gale. The chief cause why she is so under command is the twin screw arrangement. By turning one screw full speed forward and the other half speed backward, we hold the head of the ship up against the wind, and go slowly along dragging, or holding her in position when we want to splice. But the ordinary twin screw would hardly have been sufficient; we wanted greater force acting simply in order to tend to turn the ship: and then, as the paper states, it occurred to me to put the two propeller shafts at a certain angle to each other converging towards the stern. By this simple arrangement we get a distance between the two propeller shafts, if they were continued to the midship section, of some 40 or 45 feet, and that is the real angle and the real leverage with which we turn the ship, in turning the one screw forward and the other sternward. On making the trial on the open sea, by throwing a barrel over the stern, we found that we could turn the ship round and round touching that same barrel again in eight minutes, so that we have a power there of turning the ship quite irrespective of her onward motion through the water.

Mr. J. Scott Russell, F.R.S.: Could you turn her standing in eight minutes?

Dr. Siemens: Yes.

The Chairman (Sir F. W. E. Nicholson): What you mean is, that you turn her without leaving the barrel?

Dr. Siemens: Yes; her stern would come back to the barrel or nearly so, thus showing the turning power that we have. The only part of the ship which has given us some trouble is the rudder, or rather the two rudders. The forging, although very strong, has shown some signs of weakness, and one has been replaced; the other is about to be replaced by a stronger forging. But this failure has given proof of the great manœuvring power which we possess in this ship. On the last occasion, when the cable had been ruptured, the rudder was in a somewhat shaky condition, and in going across the Atlantic it showed certain signs of weakness. It was thought prudent to lock the rudder, although the rudder was not disabled, and to manœuvre entirely by the propellers. We brought up the two ends of the cable where it was ruptured in 100 fathoms of water, and laid the connecting bit, and made the splice whilst it was blowing very hard, and the sea was very rough : I say that accomplishing all that without a rudder proves the advantage of the arrangement adopted. Then there is one other point of difference between this and an ordinary ship of that size, which is that it has a rudder at each end, and has no stern. I was told that we should be pooped. An empty cable ship may be pooped if she lie still, trying to hold fast to the end of a cable ; but it occurred to us that if we gave her no stern she could not very well be pooped, and the result has proved that she can lie in a seaway without the least harm arising. On the whole I may say that the ship in actual work has proved a thorough success.

The Chairman : Before you sit down would you inform us how many miles of cable you carried ?

Dr. Siemens : Two thousand miles of Atlantic cable.

The Chairman : Was the bow rudder of any advantage ?

Dr. Siemens : Occasionally it has been an advantage. If you had to take back a cable it would be an advantage to take it back by the same machine as paid it out previously, and in that case you would steer by the bow rudder. Another case where the bow rudder has been an advantage is in manœuvring the ship against the wind, because for turning it one way or the other you get an additional power of manœuvring.

The Chairman : But if you built another would you put a bow rudder to her ?

Dr. Siemens : I think so.

Mr. Charles W. Merrifield, F.R.S.: The bow rudder in that case does not actually act as a bow rudder, but it is only when the ship has stern-way ?

Dr. Siemens : Yes.

The Chairman : You alluded to the weakness of your stern rudder, but a weakness forward would of course be of still more importance. Of course one would not like to have a weak moveable joint in any part of a sea-going ship.

Dr. Siemens: We have never found any inconvenience in having a rudder there instead of a solid block.

The Chairman: That is what I wanted to ask.

Dr. Siemens: I may say that the rudder is locked by a strong bolt, which makes it actually like part of the ship; it goes in a frame.

In the discussion of the Paper—

CONTRIBUTIONS TO THE THEORY OF SUBMERGING AND TESTING SUBMARINE TELEGRAPHS,

By Dr. Werner Siemens,

Mr. C. W. Siemens,* F.R.S. said: I thoroughly concur with the concluding remarks of the last speaker that submarine telegraphs are specifically English enterprises. I might go further, and say every submarine cable which is now working is, almost without exception, the produce of this country, and has been shipped from the Thames.

With regard to my brother's paper, it was remarked on the last occasion that it is essentially a theoretical paper. It was intended to be such, and I am glad it has elicited such able remarks as those which have fallen from Mr. Varley. We have all heard of "Varley's fault" in the French Atlantic Cable, and I have been glad to hear the method employed for finding the position of that fault with such accuracy. The difficulty, and the only difficulty in the way of determining the position of such faults, is the earth currents, and Mr. Varley has dealt with great success in this instance with those disturbing influences, and has worked upon a different method to that pursued by my brother, who wished to reduce the effect of polarisation at the point of the fault to a minimum by eliminating for the time being the earth current, and taking the earth current and battery current together, producing

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. V. 1877, pp. 81–85.

an equilibrium at the point of the fault. That is a method which I think is well worthy of the consideration of practical telegraphists, but there are more roads than one leading to Rome, as is proved by the success of Mr. Varley's method.

Regarding the early history of gutta-percha which was discussed at the last meeting of the Society I wish to make a few remarks. I may say I stood on the threshold when gutta-percha was first introduced into this country. That was I believe in the winter of 1844-5, and not in 1843 as stated by Mr. Willoughby Smith, because I recollect well seeing the first specimen of gutta-percha exhibited at the Society of Arts, I think by Mr. Montgomerie. At that time I was young and enthusiastic, and I begged Mr. Montgomerie to give me a piece of this wonderful stuff, the contemplated application of which did not seem to go beyond the formation of whips and similar articles. He was kind enough to give me a piece, which I forwarded to my brother Dr. Werner Siemens, who was at that time an officer in the Prussian service, and a junior member of a Commission appointed to report upon the feasibility of telegraphs. He had the idea that the wires should be covered with india-rubber and laid under ground, and I sent him this piece of gutta-percha in order that he might try whether it was not superior to india-rubber for insulation purposes. He did so, and after some time, having procured for him at his request a further supply, he made experiments, and in the course of about twelve months he proposed to the Prussian Government the use of gutta-percha for insulating the telegraphic line wire. In the first place he tried to unite two strips of gutta-percha round the wire, and the line from Berlin to Grossbeeren was laid in 1846 in that way. It was soon found, however, that the moisture penetrated to the wires, and this led my brother to design a machine which is still in existence and was exhibited at Vienna, and which is very similar to that used for macaroni making. This machine was designed in 1847, and in the early part of 1848 some hundreds of miles and in 1849 some thousands of miles of wires made by means of it were laid in Germany. My brother did not at that time take out a patent for his machine because he was in the Government service, and as it had been done partly on behalf of the Government it had become public to a great extent: the patent referred to as having been taken out by him in 1850 will be found to embrace only some improvements in this machine. Hence it is an undoubted fact that gutta-percha was applied to the insulation of wire in Germany several years before the patents mentioned by the President this evening as having been taken out in 1848. I should correct myself. The patents taken out in England in 1848 were for covering the wire between strips of gutta-percha, a method which had been tried by my brother in Germany in 1846; but the covering of gutta-percha by means of a machine working on the principle of a lead piping or macaroni machine was, I think, not adopted by the Gutta-percha Company until 1850. Therefore, although submarine telegraphy is decidedly an English enterprise it must be admitted that much has also been effected abroad to bring appliances to their present state of perfection.

Another remark I think fell from Mr. Varley with reference to water tanks on board vessels, and he implied that my brother claimed the introduction of those tanks. If he refers to the paper Mr. Varley will find that is not the case. He does not claim the tanks, but says they were introduced in England. But it so happens I have had a great deal to do myself with the employment of these tanks. Whether I was absolutely the first to broach the idea or not I will not say. It might have occurred to several, but may I say this, that in 1859, when the Rangoon and Singapore cable was carried out for the Board of Trade, I was employed to test that cable, and I strongly urged upon the Government the construction of water-tight tanks on board the steamship *Queen Victoria*. The matter was referred by Messrs. Glass and Elliott to the constructors of the ship at Newcastle, who wrote a letter to the Board of Trade stating that they thought it impracticable, that water-tight tanks constructed on board ship would inevitably fail on account of the natural motions of the ship, and my recommendation was negatived. This was, perhaps, fortunate, because it gave rise to the first application of the resistance thermometer for ascertaining the fact that a cable is subject to spontaneous generation of heat when coiled in a dry tank, and of proving the absolute necessity of water-tight tanks, which, as is well-known have been in use ever since.

I should like to make a few remarks regarding an observation that occurs in the paper where my name has been mentioned in connection with a method of finding the depth of water below the ship in paying out a cable, and as this is a matter of some interest I will explain more fully in what this method consists. It was used first, I may say, by myself in laying the first section (the shore end) of the Direct United States Cable, the other section having been laid partly by my brother Mr. Carl Siemens, and partly by Mr. Loeffler. We passed across considerable depths of water. The first cable laid was laid upon the solid bottom of the sea. The second cable was laid very much to the south of the first, so as to leave sufficient distance between the two cables. We did not know the depth of water between the shore and the extreme end of this headland (*illustrating on the board*); and as the cable was a heavy one it was important to know the depth. Most of you know that in paying out a cable from a drum there is really no direct indication of the depth of sea below the ship. The strain which is applied is meant to be such as to balance the weight of the cable from the ship down to the bottom of the sea; but if the depth is not known it is difficult to say what the retarding force should be. By applying too much you get a tight cable; with too little, much cable is lost in depths which are considerable. The motion of the ship through the water is not a sufficient criterion, because you may be moving with the water at a considerable rate. But there is, nevertheless, a method which the practical cable-layer may resort to for finding out whether he is paying out the proper amount of slack or not, and by the same means ascertain the depth of water below the ship. Assume that the cable runs out over the drum, with a dynamometer attached to it, at the rate of five knots an hour, and the strain is one ton. This may be a proper amount of cable to be paid out upon the ground; but it may be the ship is going only three knots an hour over the ground instead of five. To ascertain whether it is so or not—the strain being twenty cwt. on the dynamometer—increase the strain by another cwt., and then carefully note the number of revolutions of the wheel per minute. If the increase of one cwt. has no effect upon the number of revolutions of the paying-out drum, then it is pretty sure that unnecessary slack is not being paid out; but if the increase of one cwt. on the dynamometer causes the number of revolutions to fall sensibly—say from fifteen or sixteen revolutions per minute to fourteen—then too much slack is being paid out, and the weight should be increased. If the case is doubtful I would put on a considerable amount, say three or four cwt. This would (if a great deal of slack is being paid out) stop the brake-wheel, and the ship will pass over the ground without paying.

In the discussion of the Paper

"ON SOME RECENT IMPROVEMENTS IN DYNAMO-ELECTRIC APPARATUS," by Richard William Henry Paget Higgs, LL.D., Assoc. Inst. C.E., *and* John Richard Brittle, Assoc. Inst. C.E.,

Dr. Siemens * said, although the authors were connected with him in business, the paper had been written without reference to himself. It set forth correctly the scientific principles upon which the action of the dynamo-electric machine and electric lamps was based, and stated in moderate terms the results that had been practically arrived at. For years past the marvels of the electric light had been spoken of; but it was only within the last year that effects had been produced which would bear comparison with other practical methods of obtaining light. The most remarkable results had been realised, by the experiments extending over six months, at the South Foreland by Dr. Tyndall and Mr. Douglass, the Chief Engineer of Trinity House. A careful analysis of the amount of the light, its nature, its permanency, and the conditions under which it was produced by different machines, had resulted in the recommendation of the most approved machine for extended application to lighthouse purposes. In estimating the power of a new agent of that kind, it was safer to go to first principles and see what consumption of coal was necessary to produce a given effect of light, and to contrast it with the amount of carbon consumed in burning oil or gas. This

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. LII. Session 1877, 1878, pp. 57–59, 60. 61, 80, 81.

could be done by comparing the results obtained by the Trinity House engineers, with the well-known facts as regarded gas light. The electrical machine produced, with 1 HP., 1250 candle power, at the South Foreland. What, then, was the amount of coal used in generating that amount of light? It would in an average engine be 3 lbs. of coal. Therefore, 1 lb. of coal elicited by the electric machine 417 candle power. In lighting by gas 6 cubic feet of gas gave 18 candle power if the gas was fairly good, so that 417 candles would be equal to 139 cubic feet of gas. A ton of coal yielded 10,000 cubic feet of gas, so that 30 lbs. of coal would represent the 139 cubic feet of gas necessary to furnish the same amount of light afforded by the electric candle with 1 lb. of coal. There was, therefore, apparently a comparison of 30 to 1; but with gas, after allowing for the heating of the retorts, &c., half the weight of fuel might be considered as returned in the form of coke; therefore 15 lbs. of fuel were actually consumed in producing the amount of luminous effect that could be obtained in the case of the electric light with 1 lb. of fuel. He did not say that practical illumination could at once be effected at that enormous difference of cost. The authors of the paper had given the data of actual working results, which were already sufficiently favourable. Hitherto, however, the light had been exhibited on a small scale: but, in order to institute a fair comparison, it should be carried out on a somewhat similar scale to that of gas lighting. He believed in time electric light stations would be established within squares and large blocks of houses. A 100 HP. engine would be sufficient to supply conductors for a large number of lights, and they could be increased indefinitely.

The second question brought forward in the paper was that of the transmission of power, which, although new and untried, was one of considerable interest. By electrical transmission of power, an amount of from 40 to 50 per cent. was recovered at the end of the line. By putting one such machine to work with an expenditure of, say 3 HP., a power could be produced and utilised at a distance not exceeding half a mile or a mile, according to the size and length of the conductor, equal to nearly one-half that amount. If at certain stations, 100 HP. were so exerted, it would be possible to distribute over a town power which would be exceedingly convenient and free from the dangers and troubles attending caloric motors, and with an expenditure of fuel certainly not greater; because, although perhaps only 40 per cent. of the power exerted at the central station was actually obtained at the further station, it was nevertheless obtained at a very low rate. A 100 HP. engine, economically constructed, would produce 1 HP. with less than 3 lbs. of coal; whereas a small motor of 2 or 3 HP. would consume probably 6 to 8 lbs. of coal per HP. per hour. Bearing that difference in mind, the magneto-electric engine would be an economical one. How far the principle might be applicable ultimately, for the utilisation of such natural forces as water-power from a distance, remained to be seen. The difficulty was in regard to the length of the electrical conductor. Its resistance increased in the ratio of its length; and as the increased resistance would mean loss of useful effect in the same proportion, it would be necessary to double the area of the electric conductor in doubling its length, in order to maintain the same ratio of efficiency; but if that were done, the resistance might be increased to many miles, and he believed profitably, without further loss of power.

He desired to direct attention to the dynamometer employed in the experiments to ascertain the power consumed in the magneto-electric machine which received the power of the engine. The first experiments, made by indicating the steam-engine with the machine on, and with the machine off, gave very imperfect results; but a dynamometer had been contrived which he thought was of sufficient interest to be brought before the Institution. The belt that drove the machine was nipped between two pulleys, which rested in a slide and were held by a spring adjustment and screws. If the resistance increased, one side of the belt tended to become straight, and it could not become straight without pulling in the slack side of the belt; but it was held back by the elastic pressure on the slide crossways, which indicated the amount of force necessary to pull the strap straight; and that simple indication, multiplied into the number of revolutions, gave the absolute measure of the power transmitted. The want was often felt of such means of telling how much power a machine consumed. It was not sufficient to say, "If we stop it we shall see how much power the steam-engine indicates, and how much it indicates when working;" because between the two there was the friction of the machine, and there were all sorts of disturbing elements to be taken into account. With the dynamometer in question the measure was direct and absolute. While the machine was taking its power it indicated the amount of power without loss. In that way it was possible to get accurate results. The authors had stated that one-half of the power was necessarily lost. It was remarkable how nearly the best experimental results had come up to the maximum. He believed that 49 per cent. had been actually realised. He was not sure whether the theory in question held good, that the maximum effect was produced when the velocity of the machine that received the power, and gave it off at the further station, was only one-half of the velocity of the motor. He was under the impression, after some consideration, that about two-thirds of the velocity would give the maximum result. The subject, however, was too new to speak positively on so intricate a point. Enough, he believed, had been said to show that this method of electric lighting and transmission of power was more than a mere speculation ; and that it had entered the ranks of practical application of natural science.

Dr. Siemens said he believed the flickering of the light was due to imperfection in the carbons. No doubt if the moving power at the distant station were uncertain, if it should vary in speed, there would be cause for irregularities in the working, but at present the carbons were the most imperfect portion of the whole arrangement. They had been much improved, but were not yet perfect. The difficulty, however, he thought, was not an insuperable one. With care and attention homogeneous carbons would no doubt be produced. Every now and then foreign matter, when it came to the front, would cause a little explosion and a little separation in the piece of carbon, and so occasion a flickering. In order to light a room electrically, at least two electric candles ought to be used, so that the flickering of the one would melt away, as it were, in the steadiness of the other. The room in which the meeting was assembled was very unfavourable for electric lighting. The screen put up to intercept the rays was an imperfect one, and would allow a large portion of the luminous rays to pass through. If there had been a whitewashed ceiling, and the light were spread over its entire surface, the steadiness and intensity of the light would have been much greater. With regard to the question of cost, he believed the price of a machine of the size exhibited was about £70, and the cost of the lamp was £15. The estimate, however, did not include the conductor, the value of which would vary with the length, but it would not form a material part of the total expense.

Mr. W. H. Barlow (Vice-President) inquired whether by, what was called, the electric candle greater steadiness of light was obtainable, and if so, whether it was accompanied by any disadvantages?

Dr. Siemens replied that this inquiry had reminded him of an omission of the authors in not mentioning the attempt made to modify the electric light in such a way that it assumed the form of a candle. Mr. Jabloschkoff, a Russian gentleman, had overcome the difficulty of approaching the two carbons from end to end mechanically, by placing them parallel to one another, with an intervening layer of kaolin, or of ordinary plaster of Paris. By placing them in that way, the points were ignited and consumed one with the other, and as they were consumed they could still maintain their absolute position in space. There was, however, one inconvenience inseparable from that mode of arranging the carbons, namely, that the current must continually change from right to left, and from left to right, otherwise the carbon on one side would be consumed at the expense of the other. In order to burn both sides equally, the current had to change continually, and that mode of working with a reversed current was less economical than working with a continuous current. Whether, notwithstanding that drawback, the electric candles would come into general use remained to be seen. The mode of lighting which had been exhibited was due to a suggestion by the Duke of Sutherland. He had stated to the Duke that the difficulty with regard to the introduction of electric lights was to prevent the glare, and his Grace said, "Why not throw the light up to the ceiling?" The method exhibited was the result of that suggestion, and he believed it was an exceedingly good way out of the difficulty.

Dr. Siemens said he had been quoted against himself, and he had certainly ventured to express the opinion referred to, because he had at that time made a few observations upon the electric light then established at the South Foreland; and in observing it on the way from Dover to Calais, it seemed to diminish much in the same ratio as the light of the oil lamp diminished ; but there would still remain the difference in favour of the more intense light. If it possessed the same volume—lit up the same area of lenses—its greater intensity would carry it to a greater distance ; although he had ventured upon the supposition that the obstruction to that light would be greater on the part of matter suspended in the air. Such might be the case ; and yet, as was now known, that there was in the electric light a much greater volume than in the ordinary oil or candle light, it still followed that the electric light would, with its greater volume and greater intensity, penetrate to a much greater distance.

He desired to add a few explanations with reference to the transmission of electric power to a distance, whether for the production of light or for the production of force. The paper stated that the weight of the conductor would increase as the square of the distance ; but that proposition, although true in itself, would, if it were accepted, lead to erroneous ideas with regard to the power of transmitting force to a distance exceeding perhaps ½ mile. In order to get the best effect out of a dynamo-electric machine there should be an external resistance not exceeding the resistance of the wire in the machine. Hitherto it had not been found economical to increase the resistance in the machine to more than one ohm ; otherwise there was a loss of current through the heating of the coil. If, therefore, there was a machine with one ohm resistance, there ought to be a conductor transmitting the power either to the light or the electro-magnetic engine not exceeding one ohm. If, instead of going 1 mile, it was desired to go 2 miles, it would be necessary first of all to employ a conductor twice the length, but that conductor would give two ohms resistance, and would therefore destroy much of the effect. To bring it back to one ohm resistance it would be necessary to put down a second wire, or to double the area of the first ; and in that case there would be a wire of twice the length and twice the area, therefore four times the weight and four times the cost. That pointed to an increase in the cost and in the weight of the conductor in the square ratio of the distance. But one circumstance had been lost sight of in the calculation—that having twice the area to deal with a second generator could be put on, and electricity enough to work two lights could be sent through the double area to a double distance. The moment that was done the conductor was increased, for the power was transmitted only in the proportion of the increase of the length ; but that was not enough. The electric conductor did not resist the motion of electricity in the same manner as a pipe resisted the flow of liquid through it, but an Ohm's resistance was an Ohm's resistance for a larger as well as for a smaller current flowing through it, which resistance was only increased by a rise of temperature in the conductor. This rise of temperature was kept down by dissipation of heat from the conductor ; or considering that the longer and doubled conductor would possess four times the amount of surface for the dissipation of heat than the single and short conductor, it would be capable of transmitting four times the amount of electric current. It might therefore be said that it was no dearer to transmit electro-motive force to the greater than to the smaller distance, as regarded weight and cost of conductor, a result which seemed startling, but which he nevertheless ventured to put forward with considerable confidence. In uniting the two longer conductors into one, the surface would, however, be increased only in the ratio of $\sqrt{2:1}$; therefore the relative transmitting power between the longer and shorter conductor would, strictly speaking, be increased to the ratio of $1 : 2 \sqrt{2}$, or 1 : 2·83, and the longer conductor would be dearer than the shorter per unit of electro-motive force transmitted in the proportion of 4 : 2·83.

In the discussion of the Paper

"ON THE TELEGRAPH ROUTES BETWEEN ENGLAND AND INDIA," by MAJOR BATEMAN CHAMPAIN, R.E.,

DR. SIEMENS, F.R.S.,* said the paper was remarkable for its clearness and candour. Every one who had contributed towards

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* Excerpt Journal of the Society of Arts, Vol. XXVI. 1877-78, p. 532.

the lines connecting this country with India had had his meed of praise except one individual, who had been very slightly touched upon, and that was the reader of the paper himself. Major Champain had not only been the director of the Indo-European system during its palmy days, but there had been periods in the course of his administration when the days had not looked so sunny as they had generally proved to be by the results. What was more, to Major Champain was due, in great measure, the very fact of this alternative route through the south of Russia, Persia, and Germany. It was in consequence of his initiative that his (Dr. Siemens's) attention and that of his brothers was directed towards this enterprise; and having business connections in most of the countries through which these lines would pass, they had less difficulty in getting from those Governments exceptional powers which enabled them to construct a line from London to Teheran which was practically independent of the Government administrations of the different countries through which it passed. This was a great necessity, in order to make a line as efficient as it must be, in order to be a telegraphic highway between such great centres of commerce as London, Calcutta, and Bombay. Major Champain had alluded to the controversy which took place at the time when the two routes were in contemplation, the Eastern submarine, and the Indo-European, which was essentially a land line. It was fortunate that the prognostications on either side were not verified; the submarine lines had not broken down as frequently as might have been expected at that time, judging by their experience, nor had they on the land-line buried a guard under every telegraphic post as was then prophesied. Both lines had done their work well, and proved, not only that lines by land and by sea might be worked efficaciously, but that two lines were absolutely necessary in order to give safety to telegraphic communication. He did not believe in telegraphic monopolies. If one line only existed between two places the management of that line, although it might be a duplicate line, could not be as perfect as it would if two lines existed. He wholly deprecated competition for cheapness, which to a great extent meant nastiness; but a competition for quality of work, with arrangements for a fairly remunerative traffic, such as would give the public an inducement to telegraph, and make a margin of reasonable profit, seemed to him an essential condition to the advancement of telegraphy. Sir James Anderson had paid land lines rather a high compliment, inasmuch as he foresaw that, in the case of war, the submarine lines would have to be supplemented to a great extent by the land lines, the points of land being connected by means of dispatch boats. He hardly expected to hear that admission from him, because if the lines were laid in tolerably deep water it would not be very easy for an enemy to break such a line. They would not know the locality, and would probably not succeed in breaking the cable unless it happened to be of a very weak description. But however that might be, the traffic between this country and India was pretty well secured even in the event of a Russian war. In making the arrangement for the Indo-European Company's telegraph they took the precaution of inducing the contracting Governments to make a treaty, according to which the telegraph line was guaranteed as a neutral property, and he had that confidence in the continental governments, that although they might be at war with this country he thought they would respect an absolute engagement of that sort. Every Monday morning they saw a long telegram in the *Times*, giving very inflammatory war news from Calcutta; they heard of the great enthusiasm for war, and of the desire expressed on all sides to go into combat with Russia. Those telegrams all passed through the heart of Russia, and there had been no word of any interference with them. He firmly believed that if war should break out the Russian Government would respect this engagement for the sake of its own honour, and the people were sufficiently under control, as it happened, in Russia, not to destroy a line which the Government said was necessary to be maintained. He thought it perhaps more likely, and in this perhaps he did not quite agree with Sir Frederick Goldsmid, that a line passing through Asia Minor, or through a country where there was an immense population, would be in greater danger of interruption than where the line was entirely placed under the direction of one Government. However that might be, he hoped with the other speakers that it would not come to an actual war; but, whatever happened, he thought our communication with the East was well secured.

In the discussion of the Lecture

"ON THE CONNECTION BETWEEN SOUND AND ELECTRICITY," by Mr. W. H. Preece,

The President * (Mr. C. W. Siemens), said, as time is advancing, and as Mr. Preece, I believe, has some further experiments to exhibit at the close of the meeting, I will make a few observations only on the very interesting matter which has been brought before us. The discussion that has taken place is remarkable for the excellent temper which has been shown by two great rival discoverers. I think all of us must have been pleased to have seen how these two gentlemen, Professor Bell and Professor Hughes, have described and brought before us their particular views regarding certain actions in the two instruments, the telephone and microphone, which, when we come to compare them, will be found to have many points of analogy, and though essentially different in detail, tend towards the accomplishment of the same important end. Mr. Preece and Professor Bell differ with regard to the action which takes place in the microphone, and Professor Hughes favours naturally the views which Mr. Preece has expressed; but I think there is probably not so much difference between those two views. It is quite evident that the action of the microphone is due to variation in electrical resistance produced by vibrations in an imperfect conductor, such as carbon, or an aggregate of divided pieces of metal, and the question for consideration is how this variation in resistance is effected. When two pieces of carbon are pressing one upon the other, and vibration is imparted to one of them, it is easily conceived that in consequence of this vibration the pressure between the adjoining points of the carbon will be modified, and in consequence of such variation in pressure, the electric conductivity of the carbon is also influenced, whilst according to Professor Hughes's explanation, the cause of variations in the electrical resistance must be looked for in the lateral increase of points of contact.

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. VII. 1878, pp. 290–292.

We have another discoverer who has already thrown light upon this subject, viz., Mr. Edison, of New York, the well-known discoverer of the phonograph, who, in constructing a form of telephone of his own, introduced carbon contact, which gave him resistances variable with the amount of physical pressure he brought to bear upon the carbon ; and I must say that this question of varied resistance due to vibration will probably resolve itself simply into a question of pressure between particles of matter which are conducting in themselves, but which are held so lightly in contact that pressure is needed in order to establish conductive continuity.

I should have liked that something more had been said of this discovery of the microphone, with reference to its two elder sisters, the telephone and the phonograph, being of opinion that the three are only separate steps in the achievement of an advance in physical science which bids fair to be considered hereafter as one of great moment, not only as regards telegraphy, but as a means also of affording a more perfect insight into the nature of molecular action. We have heard from Mr. Willoughby Smith that in substituting crystalline selenium for carbon in the microphone, a ray of light produces an effect analogous to mechanical vibration, and announces itself in a report comparable to a clap of thunder, and I can quite follow him in his arguments with respect to the matter. His Grace the Duke of Argyll alluded to the application of this discovery to physiological research, and I could have wished that some of our learned physicians had taken up this point in the discussion, because I believe myself that the influence of these discoveries upon physiological research will be very great indeed. One thing has occurred to me in considering these matters, which I will take the liberty of mentioning. We have the remarkable effect of the phonograph producing a record of sounds simply from the indents given to a slip of tinfoil, which record can be reproduced at any time. This strikes me as being an exceedingly analogous case to the impress produced upon the brain by what we hear and see. An impress is produced, which for the moment seems lost, but which in a vigorous mind can be reproduced at any time. Now, the faculty of memory is not conceivable on any other hypothesis but that of a mechanical record being left on the brain and stored up for perhaps half a century to be restored in the succession in which it has been laid down ; otherwise how could the human mind reproduce impressions imparted years ago at will, or how could they be involuntarily revealed in our dreams? The discoveries which are now brought before us will undoubtedly serve to increase our stock of knowledge on physiological and metaphysical, as well as physical subjects, regarding molecular action, of which we have hitherto had but very imperfect indications, and I think we cannot be too thankful to those gentlemen who have enabled us to discuss them as we have done, and I will therefore move that a hearty vote of thanks be given to Mr. William Preece for his communication. I think our thanks are also due to the two discoverers who are here to-night, and have given us the benefit of their views. I therefore propose that we also return our thanks to Professor Hughes and Professor Graham Bell.

In the discussion of the Paper

"ON THE PRACTICAL APPLICATION OF ELECTRICITY TO LIGHTING PURPOSES," by Mr. James N. Shoolbred,

The Chairman * (Dr. C. W. Siemens) said the electric light was not a thing of yesterday. The first proposal to produce light by electricity dated back to Sir Humphrey Davy and the beginning of this century. Sir Humphrey Davy produced a very brilliant arc with 3,000 Wollaston cells; and shortly afterwards a still larger arc was produced with 600 Bunsen cells. They were now approaching the end of the century, and were still wondering whether the electric light would be a success. The difficulty which immediately presented itself was its great cost; for to maintain 600 Bunsen cells for a single light meant an expenditure which put the light altogether out of the question for commercial purposes. It was not until 1831 that a new ray of light was thrown upon this question, through the brilliant discovery of Professor Faraday. In that year he elicited an electric spark from

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* Excerpt Journal of the Society of Arts, Vol. XXVII. 1878–79, pp. 34, 35.

a steel magnet; and, small as it appeared, it was the origin of a great revolution in the applied arts. The effects obtained by the experiment were very small, and it was not until Pixii, in 1833, constructed an electro-magnetic machine, which was soon after improved upon by Clarke, that a continuous succession of electric sparks or currents was obtained by means of a permanent magnet. This was taken advantage of by Professor Holmes, in 1856, when he constructed his celebrated magneto-electric machine, which was still used for illuminating many lighthouses in France and elsewhere. The next important step was the invention or discovery of dynamo-electric currents, which was claimed by several men of science. Professor Wheatstone and himself had brought papers before the Royal Society at the same time, and his brother had brought one before the Berlin Society somewhat earlier. No doubt, as often happened, the same idea occurred to them all. Mr. Varley, although he did not publish what he had done until lately, had also worked in the same direction. With the dynamo-electric machine they had the power of magnetism developing a current, turning mechanical energy into electrical energy, without much loss, for the loss in converting energy into current was not more than 30 per cent.; this was less than the result obtained in any other mechanical conversion. With this power, therefore, they had an engine which converted mechanical force into electrical force, and that electrical force into light, by a process which was now perfectly well understood. There remained, however the further question to be solved:—Given the power of the light, how could it be put in such form as to be suitable for the purposes of mankind? The room was at present lighted by one of his own electric lamps, worked by his machine. He was sorry to say that it had not always been steady; but this want of steadiness was not the fault of the lamp. If the motive force had been uniform, the light would have been uniform; and he had just been informed that at the time when a considerable alteration took place, the steam in the little engine used to drive the machine had fallen from 70 lbs. pressure to 55 lbs., and the dynamo-machine was brought almost to a standstill. It was always a difficulty, in temporary arrangements, to maintain steam at anything like a steady pressure, and hitches always occurred in getting up hurried experiments. There was, however, a more serious difficulty in the electric lamps of the present day, namely, the carbon consumed; for its re-adjustment required mechanism which was not yet absolutely perfect, nor was the carbon absolutely perfect. Great improvement had been made in the carbon rods, and he had worked an electric lamp which for hours remained almost absolutely steady; but when the power varied, the imperfections of the carbon also showed themselves in an increased degree. Mr. Shoolbred had alluded to a great many proposals for overcoming the practical difficulties which now stood in the way of making the electric light successful. There were two different systems: the one with fixed carbons along with alternate currents, of which the Jabloschkoff was a type, and the other with a continuous current. The reversed current had many advantages for distributing and subdividing the light, whereas the fixed had the advantage of being more economical. He considered that, in resorting to reversed currents, and bringing the light down to the position which gas lamps generally occupied, 60 to 70 per cent. of the maximum effect was sacrificed, and the result was that the electric light was expensive; whereas if concentrated, and distributed over a large area, it could be got cheaply. It was only for the engineer who had the arrangement of it to make it face in such a way as not to be inconvenient. He had heard a great deal about inventions for subdividing the electric light indefinitely, but he did not attach so great an importance to that, as the electric light would not take the place of gas for our streets or in our houses, though it would come in largely for lighting halls and large public places of every description; but even if they could subdivide the light to any extent, it would be found that such sub-division would reduce the economy. As far as his experience went, it was rather a question of concentrating than subdividing.

ON CERTAIN MEANS OF MEASURING AND REGULATING ELECTRIC CURRENTS.

By C. William Siemens,* D.C.L., F.R.S.

The dynamo-electric machine furnishes us with a means of producing electric currents of great magnitude, and it has become a matter of importance to measure and regulate the proportionate amount of current that shall be permitted to flow through any branch circuit, especially in such applications as the distribution of light and mechanical force.

On the 19th of June last, upon the occasion of the Soirée of the President of the Royal Society, I exhibited a first conception of an arrangement for regulating such currents, which I have since worked out into a practical form. At the same time, I have been able to realize a method by which currents passing through a circuit, or branch circuit, are measured, and graphically recorded.

It is well known that when an electric current passes through a conductor, heat is generated, which, according to Joule, is proportionate in amount to the resistance of the conductor, and to the square of the current which passes through it in a unit of time, or $H = C^2 R$.

I propose to take advantage of this well-established law of electro-dynamics, in order to limit and determine the amount of current passing through a circuit, and the apparatus I employ for this purpose is represented on Figs. 1 to 3, Plate 17. Letters of reference to the principal parts of the instrument are given on the foot-note of the drawing.

The most essential part of the instrument is a strip (A) of copper, iron, or other metal, rolled extremely thin, through which the current to be regulated has to pass. One end of this thin strip of metal is attached to a screw (B), by which its tension can be regulated; it then passes upwards over an elevated insulated pulley (I), and down again to the end of a short lever, working on an axis, armed with a counter-weight and with a lever (L), whose angular position will be materially affected by any small elonga-

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* Excerpt Proceedings of the Royal Society, Vol. XXVIII. 1879, pp. 292–297.

tion of the strip that may take place from any cause. The apparatus further consists of a number of prisms of metal (P), supported by means of metallic springs (M), so regulated by movable weights (W) as to insure the equidistant position of each prism from its neighbour, unless pressed against the neighbouring piece by the action of the lever (L), in consequence of a shortening of the metallic strip. By this action, one prism after another would be brought into contact with its neighbour, until the last prism in the series would be pressed against the contact spring (S), which is in metallic connexion with the terminal (T).

The current passing through the thin strip of metal will, under these circumstances, pass through the lever (L) and the line of prisms to the terminal (T), without encountering any sensible resistance. A second and more circuitous route is, however, provided between the lever (L) and the terminal (T), consisting of a series of comparatively thin coils of wire of German silver or other resisting metal (R, R), connecting the alternate ends of each two adjoining springs, the first and last spring being also connected to the lever (L) and terminal (T) respectively.

When the lever (L) stands in its one extreme position, as shown in the drawing, the contact pieces are all separate, and the current has to pass through the entire series of coils, which present sufficient aggregate resistance to prevent the current from exceeding the desired limit.

When the minimum current is passing, the thin metallic strip is at its minimum working temperature, and all the metallic prisms are in contact, this being the position of least resistance. As soon as the current passing through the apparatus shall increase in amount, the thin metallic strip will immediately rise in temperature, which will cause it to elongate, and will allow the lever (L) to recede from its extreme position, liberating one contact piece after another. Each such liberation will call into action the resistance coil connecting the spring ends, and an immediate corresponding diminution of the current through increased resistance; additional resistance will thus be thrown into the circuit, until an equilibrium is established between the heating effect produced by the current in the sensitive strip, and the diminution of heat by radiation from the strip to surrounding objects. In order to obtain uniform results, it is clearly necessary that the loss of heat by radiation should be made independent of accidental causes, such as currents of air or rapid variations of the external temperature, for which purpose the strip is put under a glass shade, and the instrument itself should be placed in a room where a tolerably uniform temperature of say 15° C. is maintained. Under these circumstances, the rate of dissipation by radiation and conduction (considering that we have to deal with low degrees of heat) increases in arithmetical ratio with the temperature of the strip ; the expansion of the strip, which affects the position of the lever (L), is proportionate to the temperature which is itself proportionate to the square of the current—a circumstance highly favourable to the sensitive action of the instrument.

Suppose that the current intended to be passed through the instrument is capable of maintaining the sensitive strip at a temperature of say 60° C., and that a sudden increase of current takes place in consequence either of an augmentation of the supply of electricity or of a change in the extraneous resistance to be overcome, the result will be an augmentation of temperature, which will continue until a new equilibrium between the heat supplied and that lost by radiation is effected. If the strip is made of metal of high conductivity, such as copper or silver, and is rolled down to a thickness not exceeding 0·05 milim., its capacity for heat is exceedingly small, and its surface being relatively very great, the new equilibrium between the supply of heat and its loss by radiation is effected almost instantaneously. But, with the increase of temperature, the position of the regulating lever (L) is simultaneously affected, causing one or more contacts to be liberated, and as many additional resistance coils to be thrown into circuit : the result being that the temperature of the strip varies only between very narrow limits, and that the current itself is rendered very uniform, notwithstanding considerable variation in its force, or in the resistance of the lamp, or other extraneous resistance which it is intended to regulate.

It might appear at first sight that, in dealing with powerful currents, the breaking of contacts would cause serious inconvenience in consequence of the discharge of extra current between the points of contact. But no such discharges of any importance actually take place, because the metallic continuity of the circuit is never broken, and each contact serves only to diminish to some extent the resistance of the regulating rheostat. The resistance coils, by which adjoining contact springs are connected, may be readily changed, so as to suit particular cases; they are made by preference of naked wire, in order to expose the entire surface to the cooling action of the atmosphere.

In dealing with feeble currents, I use another form of regulator, in which disks of carbon are substituted for the wire rheostat. The Count du Moncel, in 1856, first called attention to, and Mr. Edison more recently took advantage of, the interesting circumstance that the electrical resistance of carbon varies inversely with the pressure to which it is subjected, and by piling several disks of carbon one upon another in a vertical glass tube, a rheostat may be constructed which varies between wide limits, according as the mechanical pressure in the line of the axis is increased or diminished. Fig. 4, Plate 19, represents the current regulator based upon this principle, and the foot-notes below the figure furnish the explanation of parts. A steel wire of say 0·3 milim. diameter is drawn tight between the end of a bell-crank lever (L) and an adjusting screw (B), the pressure of the lever being resisted by a pile of carbon disks (C) placed in a vertical glass tube. The current passing through the steel wire, through the bell-crank lever, and through the carbon disks, encounters the minimum resistance in the latter so long as the tension of the wire is at its maximum; whereas the least increase in temperature of the steel wire by the passage of the current causes a decrease of pressure upon the pile of carbon disks, and an increase in their electrical resistance; it will thus be readily seen that, by means of this simple apparatus, the strength of small currents may be regulated so as to vary only within certain narrow limits.

The apparatus described in Figs. 1 to 3, Plate 17, may be adapted also for the *measurement* of powerful electric currents—an application which is represented by Figs. 5 and 6, Plate 18. The variable rheostat is in this case dispensed with, and the lever (L) carries at its end a pencil (P) pressing with its point upon a strip of paper drawn under it in a parallel direction with the lever by means of clockwork. A second fixed pencil (D) draws a second or datum line upon the strip, so adjusted that the lines drawn by the two pencils coincide when no current is passing through the sensitive strip. The passage of a current through the strip immediately causes the pencil attached to the lever to move away from the datum line, and the distance between the two lines represents the temperature of the strip. This temperature depends, in the first place, upon the amount of current passing through the strip, and, in the second place, upon the loss of heat by radiation from the strip; which two quantities balance one another during any interval that the current remains constant.

If C is the current before increase of temperature has taken place; R the resistance of the conductor at the external temperature (T); H the heat generated per unit of time at the commencement of the flow; R′ the resistance, and H′ the heat, when the temperature T′ and the current C′ have been attained;

Then by the law of Joule, $H' = R'C'^2$. But inasmuch as the radiation during the interval of constant current and temperature is equal to the supply of heat during the same interval, we have by the law of Dulong and Petit, $H' = (T' - T)\,S$, in which S is the radiating surface. Then

$$R'C'^2 = (T' - T)\,S$$

$$C'^2 = (T' - T)\frac{S}{R'}.$$

But $T' - T$ represents the expansion of the strip, or movement of the pencil m, and considering that the electrical resistance of the conductor varies as its absolute temperature (which upon the Centigrade scale is 274° below the zero Centigrade) according to a law first expressed by Clausius, and that we are only here dealing with a few degrees difference of temperature, no sensible error will be committed in putting the value of R for R′, and we have the condition of equilibrium

$$C'^2 = m\frac{S}{R} \therefore C' = \sqrt{m\frac{S}{R}}, \quad (1)$$

or, in words, the current varies as the square root of the difference of temperature or ordinates.

For any other condition of temperature T″ we have

$$C''^2 = \frac{S}{R}(T'' - T)$$

$$\therefore \quad C'' = \sqrt{\frac{S}{R}(T'' - T)}.$$

and $(C''^2 - C'^2) = (T'' - T - T' + T)\frac{S}{R} = (T'' - T')\frac{S}{R}$, but for small differences of C'' and C' we may put $(C''^2 - C'^2) = 2C''(C'' - C')$, that is to say, small variations of current will be proportional to the variation in the temperature of the strip.

In order to facilitate the process of determining the value of a diagram in webers or other units of current, it is only necessary, if the variations are not excessive, to average the ordinates, and to determine their value by equation (1), or from a table prepared for that purpose. The error committed in taking the average ordinate instead of the absolute ordinates, when the current varies between small limits, is evidently small, the variation of the ordinates above their mean value averaging the variations below the same.

The thin sensitive conductor may thus be utilized either to restrict the amount of electricity flowing through a branch circuit within certain narrow limits, or to produce a record of the amount of current passed through a circuit in any given time.

In the discussion of the Paper

"ON THE ELECTRIC LIGHT APPLIED TO LIGHT-HOUSE ILLUMINATION,"

By James Nicholas Douglass, M. Inst. C.E.,

Dr. Siemens * said Mr. Wigham, in speaking of the penetrating power of the electric light, had quoted some remarks made by him (Dr. Siemens) twelve years ago; and he appeared to be so confident of the superiority of gas over the electric light that he had mentioned those observations in a somewhat taunting manner, as though Dr. Siemens had had occasion to alter the views he had previously expressed. Such, however, was not the case. He had then come to the conclusion, perhaps a venturesome one, at a time

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. LVII. Session 1878–1879, pp. 155–157.

when little reliable information existed on the subject, that the electric light at Dungeness lost its brilliancy in a more rapid ratio than the oil light with which it was put into juxtaposition. He had attempted to find an explanation in the fact of the electric light being of a more refrangible character than the oil light, so that in meeting with any obstructing medium it would suffer more, and would more rapidly be brought down to a common level with the other light; and he had held that, in order to get more penetrating power, not intensity alone, but intensity with quantity, as represented by large surface, would be required. The electric light then exhibited had a dioptric lantern only 30 centimètres (1 foot) diameter; and it seemed reasonable that, with that illuminated surface, although the light might be an intense one, no distant effect through an obscuring medium could be effected. Moreover, the electric lamps of that day were not very perfectly regulated, and it was probable that the focus of that lamp fluctuated considerably. The results mentioned in the paper bore out, he thought, the views he had then expressed. At Dungeness the oil lamp had a power of 250 candles, and the electric light a power of 670 candles. Whatever standard was employed, he presumed it was the same for both lights. The proportion was only that of 2½ to 1, and the disadvantage in the case of the electric light was that it had a smaller lamp. At La Hêve, Dr. Barnard ascertained that the oil lamp was nearly equal in penetrating power to the electric light, although the latter had about six times greater intensity—again showing a relative advantage in favour of the oil lamp. At the Lizard, the electric light had a penetrating power to double the distance of the oil lamp. In that case a large dioptric apparatus was employed, and the circumstances, or conditions, which he had formerly contended for had been realised. The electric light was not presented as a point, but as an illuminated surface. It was said by those who advocated oil in preference to electricity, that if an oil lamp, as Mr. Vernon-Harcourt had put it, of 3,000 candle-power were substituted, the same penetrating power would be obtained, and that with 5,000 candles the power would be greater; but Mr. Vernon-Harcourt had not explained how he would get that amount of light into the focus. It was true that Mr. Wigham had said he liked his light ex-focal, in order to give a glare; but

Dr. Siemens apprehended that few persons would coincide with Mr. Wigham in that view. It might be an advantage in looking at a light at a short distance, but the ex-focal light would give very little effect at a great distance. In regard to a powerful light, therefore, and a large dioptric apparatus, the question was, how much light could be produced within a reasonable focal sphere? and in that respect the electric light had a great advantage. In the case of the Lizard light, as much as 16,000 candle-power was developed, virtually, in a point; and if that amount of light was distributed over a reasonable surface, he was sure that greater penetrating power would be obtained than could be produced in any other way. Then the question arose, was not the electric light too expensive? In that respect the paper furnished valuable information. It showed that the electric light was relatively expensive when it was produced in small quantities, but that it became cheaper when produced in a large volume. Thus, the electric light at the Lizard, though twenty-three times more powerful than the oil lamp, was only double the cost; and, inasmuch as it had on foggy nights penetrating power to double the distance, he thought a satisfactory result had been actually obtained, considering that the effect of light diminished as the square of the distances, and that it was necessary to allow a large margin of loss in the haze. In fact, the electric light, when regarded *à priori*, was necessarily a very cheap light, because, as Dr. Hopkinson had said, a well-constructed dynamo-electric light apparatus produced in the shape of current 90 per cent. of all the energy expended in moving the machine. That was a result which, he believed, was unique in the transformation of energy. He never ventured to claim more than 70 per cent., but he saw no reason to doubt Dr. Hopkinson's investigation. With such a power it was only a question of judicious application in order to realise the advantages which it promised. He was sorry to learn that any trouble had occurred with the Lizard light. He had not heard of it, and was under the impression that the light had answered exceedingly well. It might be said, however, that in riding a racehorse more care had to be exercised than in riding a carthorse; and that for the same reason the use of a light equal to 16,000 candles required more care than an oil lamp of 700 candles. He had no doubt that, under the able management which Trinity House commanded, the minor difficulties to which reference had been made by Admiral Collinson would soon be removed, and that they would have a light, not only of 16,000, but perhaps of 20,000 or 30,000 candles, at a reasonable cost, and with great advantage to the navigation of the country.

ON THE TRANSMISSION AND DISTRIBUTION OF ENERGY BY THE ELECTRIC CURRENT.

By C. William Siemens,* D.C.L., F.R.S.†

In the autumn of 1876, when standing below the Falls of Niagara, the first impression of wonderment at the imposing spectacle before my eyes was followed by a desire to appreciate the amount of force thus eternally spent without producing any other result than to raise the temperature of the St. Lawrence a fraction of a degree,‡ by the concussion of the water against the rocks upon which it falls.

The rapids below the fall present a favourable opportunity of gauging the sectional area and the velocity of the river; and from these data I calculated that the fall represents energy equivalent to nearly 17 million horse-power, to produce which by steam would require about 260 million tons of coal a year, or just about the entire amount of coal raised throughout the world.

If one fall represents such a loss of power, what must be the aggregate loss throughout the world from similar causes? and is it consistent with utilitarian principles that such stores of energy should go almost entirely to waste? But the difficulty arises, how such energy (occurring as it does for the most part in mountainous countries) is to be conducted to centres of industry and population.

Transmission by hydraulic arrangements or by compressed air would be very costly and wasteful for great distances; but it

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* Excerpt Philosophical Magazine, Vol. VII. 1879, pp. 352–356.

† Read at the Meeting of the Physical Society on February 22, 1879.

‡ The vertical fall being 150 feet, the increase of temperature would be $\frac{150}{772}=\frac{1}{5}$° Fahr. nearly.

occurred to me that large amounts of energy, produced by means of the dynamo-electric current-generator, might be conveyed through a metallic conductor, such as a rod of copper fixed upon insulating supports. Such a conductor would no doubt be expensive; but, if once established, the cost of maintenance would be very small, and its power of transmitting electric energy would be limited only by the heat generated in it through electric resistance.

In venturing to give expression to my thoughts upon this subject, in my address to the Iron and Steel Institute in March,* 1877, I stated that a copper rod 3 inches in diameter would be capable of transmitting energy to the extent of a thousand horsepower a distance of 30 miles, there to give motion to electro-dynamic engines, or to produce illumination sufficient to light up a town with 250,000 candle-power.

Although this statement was considered by many a bold one at the time it was made, I now find that a conductor such as I then described might be able to transmit three or four times the amount of power then named, and that the light producible per horse-power was also, according to our present more advanced state of knowledge, very much understated.

No serious difficulty need be apprehended as to the production of a current sufficient in amount to fill a conductor of such large proportions as here indicated. Although it would perhaps be impossible to construct a single dynamo-electric machine of sufficient power for that purpose, any number of smaller machines could be easily coupled up both for intensity and quantity to produce the desired aggregate amount.

A difficulty would, however, arise at the other end, where the electric energy was to be applied, and where it would therefore be requisite to have an arrangement for its distribution over a number of branch circuits, so that each might receive such a proportion of the total current in the main conductor as to produce the number of lights, or the amount of power intended to be supplied. An accidental increase of resistance in one or other of the branch circuits would produce the double inconvenience of starving the circuits in which such increased resistance had occurred and of supplying an excess of current to the other circuits.

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* Published in Vol. III. of the Scientific Papers of Sir William Siemens, F.R.S.

In order to carry out such a system of supply, it would be necessary to have the means of so regulating the current in each branch circuit, that only a predetermined amount should be allowed to flow through the same ; it would be desirable also to furnish each circuit with the means of measuring and recording the amount of electric current passed through the same in any period of time.

It is my special purpose to bring before you an instrument by which these two purposes can be accomplished. The current-regulator (as represented in Plate 19) consists principally of a strip of metal (of mild steel or fused iron by preference), which by its expansion and contraction regulates the current passing through it. This strip is rolled down to a thickness not exceeding 0·05 millim., and is of such a breadth that the current intended to be passed through the regulated branch circuit would raise the temperature of the strip to say 50° C.

This strip of metal (A) is stretched horizontally between a fixed support and a regulating-screw (B), at which latter the current enters, passing through the strip, and thence through a coil of German-silver wire (C) laid in the form of a collar round the centre, and connected at its other extremity with a binding-screw (D), whence the current flows on towards the lights or other apparatus to be worked by electricity. Upon its middle the strip carries a saddle of insulating material, such as ebonite, upon which rests a vertical spindle, supporting a circular metallic disk (E), with platinum contacts arranged on its upper surface. Ten or any other number of short stout wires connect the helical rheostat at equidistant points with adjustable contact-screws (F), standing above the platinum contacts on the surface of the metallic disk. These wires are supported upon the circular frame (G) of wood or other insulating material, but are free to be lifted off their support if the metallic disk should rise sufficiently to be brought into contact with the screws. These latter are so adjusted that none of them touches the metallic disk when it is in its lowest position, but that they are brought one after another into contact with the same as the disk rises ; and it will be easily seen that for every additional contact-screw that is raised *seriatim* by the disk, a section of the helical rheostat between attachment and attachment is short-circuited by the metallic disk, and thus excluded from the circuit. When the disk is in its uppermost position the whole of the rheostat is short-circuited, and the regulator offers no other resistance to the current than that of the horizontal strip itself. In setting the regulator to work the regulating-screw (B) is drawn on sufficiently to bring the whole of the contact-screws into contact with the disk. The passage of the current through the strip will have the effect of raising its temperature to an extent commensurate with the electrical resistance; and in the same measure the strip itself will be elongated, and cause the spindle with the contact-disk to descend.

Another form of this instrument depends for its action upon the circumstance discovered by the Count du Moncel in 1856, and more recently taken advantage of by Mr. Edison, that the electrical resistance of carbon varies inversely with the pressure to which it is subjected. A steel wire of 0·3 millim. diameter is attached at one end to an adjusting-screw, B, and at the other to one end of a bell-crank lever, L, by means of which the pressure is brought to bear upon a pile of carbon disks, C, placed in a vertical glass tube. The current enters the instrument at the adjusting-screw, B, and, passing through the wire and bell-crank lever, leaves below the pile of carbon disks. Its effect is to cause a rise of temperature in the steel wire, which, through its expansion, diminishes the pressure upon the carbon disks, and thus produces an increase in their electrical resistance. This simple apparatus thus supplies a means of regulating the strength of small currents, so as to vary only within certain narrow limits.

According to Joule's law the heat generated in the strip per unit of time depends upon its resistance, and upon the square of the current; or

$$H = C^2R, \therefore C = \sqrt{\frac{H}{R}}$$

On the other hand, the dissipation of heat by radiation depends upon the surface of the strip, and upon the difference between its temperature and that of the air. Therefore, in order that the current C may remain constant, it must, at every moment, be equal to the square root of the temperature divided by the resistance; and this function is performed automatically by the regulator, which throws in or takes out resistance in the manner described, according as the temperature increases or diminishes.

The regulating instrument may also be adapted to the measurement of powerful electric currents, by attaching to the end of the sensitive strip a lever, with a pencil pressing with its point upon a strip of paper drawn under it in a parallel direction with the lever by means of clockwork, a datum line being drawn on the strip by another pencil. The length of the ordinate between the two lines depends, in the first place, upon the current which passes at each moment, and, in the second place, upon the loss of heat by radiation from the strip.

If R' is the resistance and H' the heat with a current C' and temperature T', then, by the law of Joule, $H' = R'C'^2$, and the loss by radiation is equal to $H' = (T' - T)S$, in which T' is the temperature of the strip, T that of the atmosphere, and S the surface of the strip.

Considering that the resistance varies as the absolute temperature of the conductor, according to a law first expressed by Clausius, the value of R may be put for R' for small variations of temperature; and as during an interval of constant current the heat generated and that radiated off will be equal, we obtain

$$C'^2 = (T' - T)\frac{S}{R}, \therefore C' = \sqrt{\frac{(T' - T)S}{R}} \quad . \quad . \quad . \quad (1)$$

in which $T' - T$ represents the movement of the pencil, and S is constant.

For any other temperature T'',

$$C'' = \sqrt{\frac{(T'' - T)S}{R}}.$$

For small differences of C'' and C',

$$(C'' - C')^2 = 2C''(C'' - C');$$

that is to say, small variations of current will be proportional to the variations in the temperature of the strip.

To determine the value of a diagram in webers or other units of current, it is only necessary, if the variations are not excessive, to average the ordinates, and to determine their value by equation (1), or from a table.

These observations may suffice to show the possibility of regulating and measuring electric currents with an ease and certainty quite equal to that obtained in dealing with currents of liquids such as gas or water; and the time may not be far distant when the use of such an instrument will also become a public necessity.

Other forms of the instrument will readily suggest themselves to the mind of the constructive engineer; but the typical form I have described on this occasion will suffice, I think, to show its general character.

ON THE DYNAMO-ELECTRIC CURRENT, AND ON CERTAIN MEANS TO IMPROVE ITS STEADINESS.

By C. William Siemens,* D.C.L., F.R.S.

On the 14th February, 1867, I communicated a short paper to the Royal Society, describing the accumulative or dynamo-electrical principle of action, the conception of which I attributed to my brother Dr. Werner Siemens. When the paper was read, another paper followed by Sir Charles Wheatstone (sent in on the 24th February) also describing this principle of action, thus showing that the same line of thought had occupied that eminent philosopher.

In illustration of my paper I exhibited a machine of my design, embodying the accumulative principle of action, which furnished abundant evidence of the powerful nature of the current that could be thus produced. It consisted of two horseshoe electro-magnets, between the poles of which a Siemens armature could be made to rotate, the machine being furnished with a handle or pulley for that purpose. A commutator was provided, by which the alternating currents set up in the rotating coil (after a first impulse had been given) were directed through the coils of the stationary electro-magnets in a continuous manner, and proceeded

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* Excerpt Philosophical Transactions of the Royal Society, 1880, pp. 1071–1074.

thence outward to ignite a platinum wire of some 12″ in length, or to perform other work.

This machine, although the first of its kind, has done good service ever since its construction, having been found very efficacious in exciting powerful permanent magnets at the telegraph works of Siemens Brothers at Woolwich.

Since 1867 the accumulative principle has been employed in the machines of different makers, and one form of dynamo-electric machine, that of M. Gramme, differs very materially from the machine above referred to, and has met very deservedly with extensive recognition. M. Gramme embodied in his machine the principle of Professor Pacinotti's magnetic ring, which enabled him to produce powerful electric currents without much of the loss of energy caused in previous machines through the heating of the rotating armature.

Another modification of the dynamo-electrical machine is one devised by Mr. Von Heftner Alteneck, an engineer and physicist employed under my brother Werner Siemens, at Berlin. This machine differs from that first submitted by myself in several important particulars. Instead of the Werner Siemens armature, Von Heftner Alteneck adopted a rotating coil of iron wire wound with insulated copper wire in more than one direction, the several coils of wire being connected seriatim with the commutator, and through it, with the wire surrounding the soft iron bars, and with the electric lamp or other resistance on the outer circuit.

The advantage claimed for this mode of construction is that all the wire forming the rotating coil or helix is brought into the magnetic field, excepting only those portions crossing from side to side of the coil; and in order to reduce this unproductive resistance to a minimum, the rotating coil or helix has been made comparatively long, and the number of electro-magnets has been increased generally to six or more.

The principal advantage of the dynamo-electrical machine over all other current generators consists in its power of producing currents of great magnitude, and of an intensity up to 100 volts, with a small primary resistance, and therefore with a comparatively small expenditure of mechanical energy. It labours, on the other hand, under the disadvantage that the power of the current depends, at a given velocity, upon the magnetic force developed in the electro-magnets. This force depends upon the amount of current passing through the coils of the magnets, which in its turn is dependent in an inverse ratio upon the resistance in the outer circuit. If from some accidental cause the external resistance is increased, the electro-motive force of the machine, instead of rising to overcome the obstruction, diminishes, and thus aggravates the resulting disturbance. If, on the other hand, the resistance of the outer circuit diminishes, as in the case when the carbons of an electric regulator touch one another, the electro-magnets are immediately excited to a maximum, and the electro-motive force of the machine is increased. The power absorbed and its equivalent, the heat generated in the circuit, is equal to the square of the electro-motive force divided by the resistance; hence the work demanded from the engine will be greatly increased, the machine may be dangerously overheated, and powerful sparks may injure the commutator. It is chiefly owing to this instability of the dynamo-electric current that its application to electric illumination has been retarded, and that magneto-electric machines and machines producing alternating currents have been again used, although they are inferior to the dynamo machine in the current energy produced for a given expenditure of mechanical energy.

The properties of dynamo-electric machines have been examined by several observers. Messrs. Houston and Thomson (Franklin Institute) compared the efficiency of the Gramme, Brush, and Wallace Farmer machines. Dr. Hopkinson (Institution of Mechanical Engineers, 25th April, 1879) examined a medium-sized Siemens machine, determined its efficiency, and expressed the electro-motive force as a function of the current. Herren Mayer and Auerbach (Wiedemann's "Annalen," November, 1879) experimented on a Gramme machine. M. Mascart has experimented on the Gramme machine, and Mr. Schwendler on both Gramme and Siemens machines.

The radical defect of the dynamo machine of ordinary construction, may be inferred from the results of these experiments. The remedy has, however, been in our hands from the time of the first announcement of the principle of these machines before the Royal Society, when Sir Charles Wheatstone pointed out that "a very remarkable increase of all the effects, accompanied by a diminution in the resistance of the machine, is observed when a cross wire is placed so as to divert a great portion of the current from the electro-magnet."

Some of the constructors of dynamo machines, namely: Mr. Ladd in this country, and Mr. Brush in the United States of America, have taken advantage of this suggestion, the latter with the avowed object in view of obviating spontaneous changes of polarity in effecting electro-precipitation of metals, and without perhaps having realised all of the advantages of which this mode of action is capable; others have refrained from doing so on account of difficulties resulting, as I shall endeavour to show, from an insufficient examination into some important physical conditions that require attention in order to realise economical results.

An ordinary medium-sized Siemens-Alteneck dynamo-electrical machine has wound on its rotating helix insulated copper wire of 2·5 m.m. diameter in 24 sections, representing a resistance of ·4014 S. U.* The four electro-magnet coils connected seriatim are composed of copper wire of 5·5 m.m. diameter, representing a total resistance of 0·3065 S. U.

If (as has frequently been done) the wires of this machine were to be connected as suggested in Sir Charles Wheatstone's original paper, thus making the outer circuit not continuous with but parallel to the coil circuit, and if the outer circuit had a resistance of one unit, it would follow that the total resistance to the current set up by the rotation of the armature would be reduced from $\cdot 4 + \cdot 3 + 1\cdot = 1\cdot 7$ to $\cdot 4 + \frac{\cdot 3 \times 1}{1 + \cdot 3} = 0\cdot 61$ unit, causing a great increase of current, the major portion (in the proportion of 10 to 4) would flow through the electro-magnets, thus causing a great increase of heating effect. The resistance of the field magnet must therefore be greatly increased, but if it were attempted to

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[*] The resistance coils used in these experiments were graduated according to the mercury system introduced by Dr. Werner Siemens, and adopted by the Telegraphic Convention at Vienna in 1868. The B. A. unit was determined in 1874 by Kohlrausch to be 1·0493 S. U., or combined with Lorenz's value of the S. U. afterwards adopted, 0·9797 × 10^9 C. G. S. units—as much as 2 per cent. below its ascribed theoretical value. Later determinations by H. F. Weber (Phil. Mag., March, 1878) makes the S. U. to be equal to 0·955 × 10^9 C. G. S. units, and thus the Ohm to be 0·2 per cent. higher than its ascribed value; if this latter value is used, the numerical results must be correspondingly altered.

increase that resistance simply by reducing the diameter of the wire, and increasing the number of convolutions until the same thickness of coil was obtained, the magnetic excitement and with it the electro-motive force of the current produced at a given velocity of rotation would suffer a material decrease. The current flowing through the helix coil would moreover have to divide itself, and in order to reach the same limit in the outer circuit its intensity in the helix coil would have to be increased, causing it to heat more readily than before. It was necessary, therefore, to raise the effect of the magnet current to the same level as before with as small a proportion of the helix current as possible, in order to leave a maximum proportion of the current for the outer circuit. In order to effect this, the magnet bars had to be increased in length, and placed further apart so as to provide room for coils of greatly increased weight and dimensions; at the same time the helix wire had to be increased in diameter to give room for the aggregate current, but in reality I found it advantageous to increase the diameter of the same in a much greater proportion.

These general conditions having been determined by preliminary experiment, Mr. Lauckert, electrician engaged at my works, undertook a series of comparative experiments which are given in the appendix * attached to this paper, and the results are given numerically and exhibited in curves.* On examining the curves it will be remarked:

1. That the electro-motive force, instead of diminishing with increased resistance, increases at first rapidly, then more slowly towards an asymptote.

2. That the current in the outer circuit is actually greater for a unit and a-half resistance than for one unit.

3. With an external resistance of one unit, which is about equivalent to an electric arc when 30 or 40 webers are passing through it, 2·44 horse-power is expended, of which 1·29 horse-power is usefully employed: an efficiency of 53 per cent. as compared with 45 per cent. in the case of the ordinary dynamo machine.

4. That the maximum energy which can be demanded from the

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* It has not been considered necessary to reproduce these.

engine is 2·6 horse-power, so that but a small margin of power is needed to suffice for the greatest possible requirement.

5. That the maximum energy which can be injuriously transferred into heat in the machine itself is 1·3 horse-power, so that there is no fear here of destroying the insulation of the helix by excessive heating.

6. That the maximum current is approximately that which would be habitually used, and which the commutator and collecting brushes are quite capable of transmitting.

Hence I conclude that the new machine will give a steadier light than the old one, with greater average economy of power, that it will be less liable to derangement, and may be driven without variation of speed by a smaller engine; also that the new machine is free from the objection of having its currents reversed when used for the purpose of electro deposition.

The same peculiarity also enables me to effect an important simplification of the regulator to work electric lamps, to dispense with all wheel and clock-work in the arrangement, as shown in Plate 21. The two carbons, being pushed onward by gravity or spring power, are checked laterally by a pointed metallic abutment, situated at such a distance from the arc itself that the heat is only just sufficient to cause the gradual wasting away of the carbon in contact with atmospheric air. The carbon holders are connected with the iron core of a solenoid coil, of a resistance equal to about fifty times that of the arc, the ends of which coil are connected with the two electrodes respectively. The weight of the core, which has to be maintained in suspension by the attractive force produced by the current, determines the distance between the electrodes, and hence the electric resistance of the arc. The result is that the length of the arc is regulated automatically so as to maintain a uniform resistance, signifying a uniform development of light.

THE DYNAMO-ELECTRIC CURRENT IN ITS APPLICATION TO METALLURGY, TO HORTICULTURE, AND TO LOCOMOTION,

By C. William Siemens,* D.C.L., LL.D., F.R.S.

In the inaugural address† which I had the honour to deliver to the Society of Telegraph Engineers on the 23rd January, 1878, I called special attention to the applicability of the dynamo-electric current for purposes beyond the range of what electricity had theretofore been employed in effecting. Among these purposes I specially referred to the transmission of power, and to the accomplishment of large chemical results, such as the decomposition of metallic salts, &c.

My object to-day is to corroborate my statement by describing some further experimental results obtained by means of the dynamo-electric current, which I hope will prove to be of interest.

So long as the electric current was produced by the decomposition of zinc in a galvanic battery, it was too expensive a form of energy to do our behests in the way of massive effects, it was resorted to principally when energy, in the form of heat or mechanical power, failed to accomplish our ends; and the rapid rate at which electrical energy passes through a metallic conductor rendered it the only available means of producing instantaneous effects at great distances. The dynamo-electric current, on the other hand, provides us with the means of transforming mechanical energy into electric energy without much loss, and it enables us further to accumulate electrical impulses in such a manner as to produce mechanical effects measurable not by grains or ounces, but by pounds and tons of weight lifted through a considerable height.

It is not my present intention to enter into particulars regarding the dynamo-electric machine, but to describe three somewhat novel applications of it, viz., as a means of effecting the fusion of

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. IX. 1880, pp. 278–303.

† Published in Vol. III. of the Scientific Papers of Sir Wm. Siemens, F.R.S.

refractory materials in considerable quantities in an electric furnace; in its effects upon horticulture, as a promoter of the chemical changes by which the plant takes its chief ingredients of food from the atmosphere; and as a means of mechanical propulsion, in which the dynamo-electric current enters the list as a rival of steam, to work either stationary machinery, hoists, or lifts, or to propel trains along rail or tramways.

ON THE APPLICATION OF THE DYNAMO-ELECTRIC CURRENT TO THE FUSION OF REFRACTORY MATERIALS IN CONSIDERABLE QUANTITIES.

Amongst the means at our disposal for effecting the fusion of highly refractory metals, and other substances, none has been more fully recognised than the oxy-hydrogen blast. The ingenious modification of the same by M. H. Ste.-Claire Deville, known as the Deville furnace, has been developed and applied to the fusion of platinum in considerable quantities by Mr. George Matthey, F.R.S.

The regenerative gas furnace furnishes, however, another means of attaining extremely high degrees of heat, and this furnace is now largely used in the arts—among other purposes, for the production of mild steel. By the application of the open-hearth process, 10 to 15 tons of malleable iron, containing only traces of carbon or other substances alloyed with it, may be seen in a perfectly fluid condition upon the open hearth of the furnace, at a temperature probably not inferior to the melting point of platinum. It may be here remarked that the only building material capable of resisting such heats is a brick composed of 98·5 per cent. of silica, and only 1·5 per cent. of alumina, iron, and lime, to bind the silica together.

In the Deville furnace, an extreme degree of heat is attained by the union of pure oxygen with a rich gaseous fuel under the influence of a blast, whereas in the Siemens furnace it is due to slow combustion of a poor gas, potentiated, so to speak, by a process of accumulation through heat stores or regenerators.

The temperature attainable in both furnaces is limited by the point of complete dissociation of carbonic acid and aqueous vapour, which, according to Ste.-Claire Deville and Bunsen, may be estimated at from 2,500° to 2,800° C. But long before this extreme point has been reached, combustion becomes so sluggish that the losses of heat by radiation balance the production by combustion, and thus prevent further increase of temperature.

It is to the electric arc, therefore, that we must look for the attainment of a temperature exceeding the point of dissociation of products of combustion, and indeed evidence is not wanting to prove the early application of the electric arc to produce effects due to extreme elevation of temperature. As early as the year 1807, Sir Humphrey Davy succeeded in decomposing potash by means of an electric current from a Wollaston battery of 400 elements; and in 1810 the same philosopher surprised the members of the Royal Institution by the brilliancy of the electric arc produced between carbon points through the same agency.

Magneto-electric and dynamo-electric currents enable us to produce the electric arc more readily and economically than was the case at the time of Sir Humphrey Davy, and this comparatively new method has been taken advantage of by Messrs. Huggins, Lockyer, and other physicists, to advance astronomical and chemical research by the aid of spectrum analysis. Professor Dewar quite recently, in experimenting with the dynamo-electric current, has shown that in his lime tube or crucible several of the metals assume the gaseous condition, as demonstrated by the reversal of the lines in his spectrum, thus proving that the temperature attained was not much inferior to that of the sun.

My present object is to show that the electric arc is not only capable of producing a very high temperature within a focus or extremely contracted space, but also such larger effects, with comparatively moderate expenditure of energy, as will render it useful in the arts for fusing platinum, iridium, steel, or iron, or for effecting such reactions or decompositions as require for their accomplishment an intense degree of heat, coupled with freedom from such disturbing influences as are inseparable from a furnace worked by the combustion of carbonaceous material.

The apparatus which I employ to effect the electro-fusion of such material as iron or platinum is represented in the accompanying drawing, Plate 20. (This diagram was explained in full detail to the meeting.) It consists of an ordinary crucible, *a*, of plumbago or other highly refractory material, placed in a metallic jacket or outer casing, the intervening space being filled up with pounded charcoal, *b*, or other bad conductor of heat. A hole is pierced through the bottom of the crucible for the admission of a rod of iron, platinum, or dense carbon, *c*, such as is used in electric illumination. The cover of the crucible is also pierced for the reception of the negative electrode, by preference a cylinder of compressed carbon, *d*, of comparatively large dimensions. At one end of a beam supported at its centre is suspended the negative electrode, *d*, by means of a strip of copper, or other good conductor of electricity, the other end of the beam being attached to a hollow cylinder of soft iron, *e*, free to move vertically within a solenoid coil of wire, presenting a total resistance of about 50 units or ohms. By means of a sliding weight, *g*, the preponderance of the beam in the direction of the solenoid can be varied so as to balance the magnetic force with which the hollow iron cylinder is drawn into the coil. One end of the solenoid coil is connected with the positive, and the other with the negative pole of the electric arc, and, being a coil of high resistance, its attractive force on the iron cylinder is proportional to the electro-motive force between the two electrodes, or, in other words, to the electrical resistance of the arc itself.

The resistance of the arc was determined and fixed at will within the limits of the source of power, by sliding the weight upon the beam. If the resistance of the arc should increase from any cause, the current passing through the solenoid would gain in strength, and the magnetic force overcoming the counteracting weight, would cause the negative electrode to descend deeper into the crucible; whereas, if the resistance of the arc should fall below the desired limit, the weight would drive back the iron cylinder within the coils, and the length of the arc would increase, until the balance between the forces engaged had been re-established.

Experiments with long solenoid coils have shown that the attractive force exerted upon the iron cylinder is subject only to slight variation within a range of several inches, that is, within the limits when the iron cylinder has just entered the coil, and when it has advanced a little beyond the point of half immersion, which circumstance allows of a range of several inches of nearly uniform action on the electric arc. The accompanying diagram, Fig. 2, Plate 21, represents the attractive force of a solenoid coil of this description upon its iron core, the abscissæ representing the depth of immersion of the uppermost end of the iron in the coil in centimetres, and the ordinates the attractive force in grams.

This automatic adjustment of the arc is of great importance to the attainment of advantageous results in the process of electric fusion; without it the resistance of the arc would rapidly diminish with increase of temperature of the heated atmosphere within the crucible, and heat would be developed in the dynamo-electric machine to the prejudice of the electric furnace. The sudden sinking or change in electrical resistance of the material undergoing fusion would, on the other hand, cause sudden increase in the resistance of the arc, with a likelihood of its extinction, if such self-adjusting action did not take place.

Another important element of success in electric fusion consists in constituting the material to be fused the positive pole of the electric arc. It is well known that it is at the positive pole that the heat is principally developed, and fusion of the material constituting the positive pole takes place even before the crucible itself is heated up to the same degree. This principle of action is of course applicable only to the melting of metals and other electrical conductors, such as metallic oxides, which constitute the materials generally operated upon in metallurgical processes. In operating upon non-conductive earth or upon gases, it becomes necessary to provide a non-destructible positive pole, such as platinum or iridium, which may, however, undergo fusion, and form a little pool at the bottom of the crucible.

In this electrical furnace some time, of course, is occupied to bring the temperature of the crucible itself up to a considerable degree, but it is surprising how rapidly an accumulation of heat takes place. In working with the modified medium-sized dynamo machine, capable of producing 36 webers of current with an expenditure of 4 horse-power, and which, if used for illuminating purposes, produces a light equal to 6,000 candles, I find that a crucible of about 20 centimetres in depth, immersed in a non-conductive material, is raised up to a white heat in less than a quarter of an hour, and the fusion of 1 kilogram of steel is effected within, say, another quarter of an hour, successive fusions being made in somewhat diminishing intervals of time. It is quite feasible to carry on this process upon a still larger scale by increasing the power of the dynamo-electric machine and the size of the crucibles. (These remarks were illustrated by an actual arrangement on the plan described, and 1 lb. weight of broken files, which were placed in a crucible through which a dynamo-current of about 70 webers was passed, was brought to a liquid state in thirteen minutes, and poured out of the crucible in that condition.)

By the use of a pole of dense carbon, the otherwise purely chemical reaction intended to be carried into effect may be interfered with through the detachment of particles of carbon from the same; and although the consumption of the negative pole in a neutral atmosphere is exceedingly slow, it may become necessary to substitute for the same a negative pole so constituted as not to yield any substance to the arc. I have used for this purpose (as also in the construction of electric lamps) a water pole, or tube of copper, through which a cooling current of water is made to circulate. It consists simply of a stout copper cylinder closed at the lower end, having an inner tube penetrating to near the bottom for the passage of a current of water into the cylinder, which water enters and is discharged by means of flexible india-rubber tubing. This tubing being of non-conductive material, and of small sectional area, the escape of current from the pole to the reservoir is so slight that it may be entirely neglected. On the other hand, some loss of heat is incurred through conduction in the use of the water pole, but this loss diminishes with the increasing heat of the furnace, inasmuch as the arc becomes longer, and the pole is retired more and more into the crucible cover.

The dynamo-electric machine consuming 4·25 horse-power, or 3·17 ergtens, per second, will send a current of 40·5 webers through 1 unit electrical resistance; replacing the resistance by an arc maintained by the balance-weight constantly at 37·8 volts electromotive force, the same current will flow.

Neglecting the connecting wires, there will be developed in the arc an energy of $1{,}531{\cdot}2 \times 10^7$ ergs. per second $= 9{,}187{\cdot}2 \times 10^8$ ergs. per minute, or $1{,}378{\cdot}1 \times 10^{10}$ ergs. per 15 minutes $= 32{\cdot}8 \times 10^4$ gram water degree units of heat.

Assuming steel to have the same specific heat as iron, viz., at $t^0 = 0{\cdot}1040 \times 0{\cdot}000144\, t^0$, and that the melting point of steel is 1800° C., then 420·5 units of heat will be expended in raising the steel to this temperature; and assuming the latent heat of fusion of steel at 29·5 units (silver is 21, and zinc 28), there are roughly 450 units required to melt a gram of steel, and 225,000 to melt half a kilogram, that is about $\frac{2}{3}$ of the heat generated in the crucible, and $\frac{1}{3}$ of the horse-power actually expended. A good expansive condensing steam engine converts the heat energy residing in coal into mechanical energy, with a loss of over eighty per cent., or, in other words, $\frac{1}{6}$ only of the 7,000 units residing in a gram of ordinary coal is represented as work in the engine. It hence follows that the useful effect attainable in the electric furnace is $\frac{1}{3} \times \frac{1}{6} = \frac{1}{18}$ of the heat energy residing in the fuel consumed under the boiler of the engine.

To melt a gram of steel in the electric furnace takes, therefore, $450 \times 18 = 8{,}100$ units, which is within a fraction the heat actually contained in a gram of pure carbon. It results from this calculation that, through the use of the dynamo-electric machine, worked by a steam engine, when considered theoretically, one pound of coal is capable of melting nearly one pound of mild steel. To melt a ton of steel in crucibles in the ordinary air furnace used at Sheffield, from 2½ to 3 tons of best Durham coke are consumed, the same effect is produced with one ton of coal when the crucibles are heated in the regenerative gas furnace, whilst to produce mild steel in large masses on the open hearth of this furnace, 12 cwt. of coal suffice to produce one ton of steel. The electric furnace may be therefore considered as being more economical than the ordinary air furnace, and would, barring some incidental losses not included in the calculation, be as regards economy of fuel nearly equal to the regenerative gas furnace.

It has, however, the following advantages in its favour:—1st. That the degree of temperature attainable is theoretically unlimited. 2nd. That fusion is effected in a perfectly neutral atmosphere. 3rd. That the operation can be carried on in a laboratory without much preparation, and under the eye of the operator. 4th. That the limit of heat practically attainable with the use of ordinary refractory materials is very high, because in the electric furnace the fusing material is at a higher temperature than the crucible, whereas in ordinary fusion the temperature of the crucible exceeds that of the material fused within it.

Without wishing to pretend that the electric furnace here represented is in a condition to supersede other furnaces for ordinary purposes, the advantages above indicated will make it a useful agent, I believe, for carrying on chemical reactions of various kinds at temperatures and under conditions which it has hitherto been impossible to secure. (A second charge of steel was now put into the crucible, and was poured out in a molten state at the end of eight minutes.)

THE EFFECT OF DYNAMO-ELECTRIC ENERQY UPON HORTICULTURE, AS A PROMOTER OF THE CHEMICAL CHANGES BY WHICH THE PLANT TAKES ITS CHIEF INGREDIENTS OF FOOD FROM THE ATMOSPHERE.

A consideration of the extremely elevated temperature of, and of the effects produced in experimenting with, powerful electric arcs, such as causing blistering of the skin and a feeling akin to sunstroke in the incautious observer, has led me to reflect whether the action of the arc was not analogous to that of the sun in its effect also upon vegetable life. The solar ray, in falling upon the leaf of a plant, not only produces the colouring matter called chlorophyll, but effects within the vegetable cell decomposition of the carbonic acid and aqueous vapour absorbed from the atmosphere for the formation of starch and woody fibre.

I mentioned my views on this subject to several botanists, from whom I received some encouragement to put the question to practical test, which I accordingly did, commencing in the early portion of the present year, at my country residence of Sherwood, near Tunbridge Wells.

The apparatus I use consists:—1st, of a vertical Siemens dynamo-machine, weighing 50 kilograms, with a resistance of 0·717 unit on the electro-magnets. This machine makes 1,000 revolutions a minute; it takes two horse-power to drive it, and develops a current of from 25 to 27 webers of an intensity of 70 volts. 2. A regulator or lamp, constructed for continuance currents, with two carbon electrodes of 12 mm. and 10 mm. diameter respectively. The light produced is equal to 1,400 candles. 3. A three-horse-power Otto gas engine as motor.

My object was to ascertain by experiment whether electric light affected the growth of plants. For this purpose I placed in the open air a regulator contained in a lamp having a metallic reflector, about 2 metres above the roof of a sunk melon house. Several pots were provided, and planted with quick-growing seeds and plants, such as mustard, carrots, melons, &c. The plants were arranged to be brought at suitable intervals, without moving them, under the influence of daylight and electric light, both falling upon them at approximately the same angle. The pots were divided into four groups or series. One group was kept entirely in the dark, another was exposed to the influence of electric light only, the third to the influence of daylight only, and the fourth was exposed successively to both day and electric light.

In this first trial the electric light was supplied during six hours, from 5 to 11 each evening, the plants being left in darkness the rest of the night, but in experiments hereafter to be referred to, the electric light was kept on during the whole night.

In every instance the differences of effect were unmistakable. The plants kept in the dark were pale yellow, thin in the stalk, and soon died. Those exposed to electric light only, showed a light green leaf, and had sufficient vigour to survive. Those exposed to daylight only were of a darker colour and greater vigour. Those exposed to both sources of light evinced a decided superiority in vigour over the rest, and the colour of the leaf was a dark rich green.

It must be remembered that in this trial of electric against solar light, the period of exposure was in favour of the latter in the proportion of nearly 2 to 1, but after making every allowance, the average daylight in these latitudes in the early portion of the year appears to have about twice the effect of electric light. It was evident, however, that the electric arc was not so placed as to give out its light to the greatest advantage. The nights were cold, and the plants under experiment were for the most part of a character to require a hot moist atmosphere ; the glass thus became covered very thickly with moisture, obstructing thereby the action of the light, besides which the electric light had to traverse the glass of its own lamp. Notwithstanding these drawbacks, the electric light clearly formed chlorophyll and its derivatives in the plants. But it was, besides, interesting to observe the mechanical action that took place, for the mustard seed stem, when placed obliquely, turned completely towards the light in the course of two or three hours, and the stems of cucumber and melon plants also did so, though more slowly. The cucumber and melon plants which have been exposed to both day and electric light have made great progress, and my gardener tells me that he could not have brought on the latter without the aid of the electric light during the early winter. These preliminary trials go to prove that electric light can be called to the aid of solar light by using it outside of green houses, but the loss of effect in such cases is considerable.

I next directed my observations to the effect of electric light upon plants, when both were placed in the same enclosure. A portion of the melon-house already referred to was completely darkened with a covering of thick matting, and was whitewashed inside. The electric lamp was placed over the entrance door, and shelves were arranged in a horse-shoe form, with pots containing the plants to be experimented upon, the plants being placed at distances from the source of light varying from 0·5 metre to 2·3 metres. The first time the naked electric light was tried in this manner, some of the plants, and especially some melon and cucumber plants, from 20 cm. to 40 cm. in height, less than a metre distance from the lamp, suffered, the leaves nearest the lamp turning up at the edges, and presenting a scorched appearance. In the later experiments the stands were so arranged that the distance of the plants from the light was from 1·5 to 2·3 metres. The plants were divided into three groups; one group was exposed only to daylight, a second group received only electric light during eleven hours of the night, being in darkness during the day, and the third group had the benefit of 11 hours day and 11 hours electric light. These experiments were continued during four consecutive days and nights, and the results are very striking and decisive as regards the effect upon such quick-growing plants as mustard, carrots, &c. The trial was unsatisfactory in this one respect, that during the third night the gas-engine working the dynamo machine came to a stand-still, owing to a stoppage in one of the gas channels, and the electric light was only applied half the night. Notwithstanding this drawback the plants were evidently benefited by the electric light. The plants that had only been exposed to daylight (with a fair proportion of sunlight) presented the usual healthy green appearance; those exposed to electric light alone were of a somewhat lighter hue than those exposed to daylight, except in one instance when the reverse was the case; while the plants that had the benefit of both day and electric light far surpassed the others in darkness of green and general vigour. A fear had been expressed that the melon and cucumber plants which had been scorched by the electric light on the first evening would droop or die under continued exposure to that agency, but they were replaced at a distance from the light exceeding 2 metres, and they have all shown signs of recovery. A pot of tulip buds was placed in this electric stove, and the flowers opened completely after an exposure of two hours.

Another object I had in view in this experiment was to observe whether the plants were injured by carbonic acid, and the nitrogenous compounds observed by Professor Dewar to be produced within the electric arc. All continuous access of air into the stove was stopped, and in order to prevent excessive accumulation of heat, the stove pipes were thickly covered over with matting and wet leaves. But although the access of stove heat was thus stopped, the temperature of the house continued through the night at 72° Fahr., proving that the electric light furnished not only light, but sufficient heat also. No injurious effect was observed on the plants from the want of ventilation, and it is probable that the supply of carbonic acid given off by the complete combustion of the carbon electrodes at high temperature, and under the influence of an excess of oxygen, sustained their vital functions. If nitrogenous compounds were produced in large quantities, it is likely the plants would have been injured; but they could not be perceived by their smell in the stove, when all ventilators were closed, and no injurious effects on the plants have been observed.

These experiments are instructive in proving that electric light alone promotes vegetation, and the important fact that diurnal repose is unnecessary to plant life, although the experiments have perhaps not lasted long enough to furnish that proof absolutely. We may argue, however, from analogy, that such repose is not necessary, as crops grow and ripen very quickly in northern latitudes, where the summer is only two months in length, during which period the sun is almost altogether above the horizon.

I next removed the electric light into a palm house constructed of framed glass (8·6 m. × 14·4 m. × 4·42 m.) In its centre, a banana palm and a few other small palms are planted, whilst a considerable variety of flowering plants are placed around the interior. The electric light was placed at the south corner of the house, as high as practicable, that its rays might fall upon the plants in the same direction and at the same angle as those of the sun during the middle of the day. A metallic reflector was placed behind the lamp, so as to utilise all the rays as far as possible. Some young vines are planted along the eastern side of the house. Three pots of nectarines just on the bud were placed on the floor at different distances from the light, and also some roses, geraniums, orchids, &c. The temperature of the house was maintained at 65° F., and the electric lamp was kept alight from 5 p.m. to 6 a.m. for one week, from February 18th to February 24th, excepting Sunday night. The period of the trial was hardly sufficient to produce very striking effects, but the plants remained healthy. The vine nearest the light made most progress, and the same statement could be made regarding the nectarines and roses. Other plants, such as geraniums, continued to exhibit a vigorous appearance, and the electric light appeared to impart the vitality necessary to prevent the plants being injured through excessive temperature. This experiment is important in showing that the electric light, when put into conservatories, improves the appearance and growth of the plants—the leaves become darker, the plants more vigorous, and the colouring of the flowers brighter ; but a further period of time is necessary to establish this observation absolutely. The effects produced by electric light in conservatories are very striking, owing to the clearer definition of form and colour due thereto.

I decided in the next place to try the effect of the electric light upon plants in the open air and under glass at the same time. The regulator was returned to its first position, two metres above the ground, with a sunken melon house on the one side, and a sunken house containing roses, lilies, strawberries and a variety of other plants on the other. Upon the ground between these were placed boxes sown with early vegetables, and protecting walls were erected across the openings of the passage between the two houses, in order to protect the plants from cold winds. The effects could thus be simultaneously observed upon the melons and cucumbers in the one house, upon the roses, strawberries, etc., at a lower temperature, in the other, and upon the early vegetables unprovided with covering.

That growth was promoted under all these varying circumstances, I proved clearly, by shading a portion of the plants both under glass and in the open air from the electric light, without removing them from their position of equal temperature, and exposing them to solar light during day-time. Upon flowering plants the effects are very striking, and the electric light is apparently more efficacious to bring them forward than daylight in winter. Although the quantity of heat given off by the electric light is not so great in amount as that from burning gas, yet the heat rays from the arc counteract that loss of heat from the leaves by radiation into space, which causes hoar frost on a clear night. An experiment made during a night of hoar frost clearly proved that although the temperature on the ground did not differ materially within the range of the electric light and beyond it, the radiant effect of the light was such as to prevent frost entirely within its range. For this reason I anticipate the useful application of electric light in front of fruit walls, in orchards, and in kitchen gardens, to save the fruit bud at the time of setting.

Considering the evident power of the electric light to form chlorophyll, there seemed reason to suppose that its action would also in the case of ripening fruit resemble that of the sun, and that saccharine matter, and more especially the aromatic constituents, would be produced. To test this opinion practically, several plants of early strawberries in pots were placed, as in the last experiment, in two groups, the one being subjected to daylight only, and the other to solar light during the day and to electric light at night. Both groups were placed under glass, at temperatures varying from 65° to 70° Fahr., those that received daylight only being shielded from the effect of the electric light.

At the commencement of the experiment the strawberry plants were partly setting fruit, and partly in bloom. After a week the fruit on the plants exposed to electric light had swelled very much more than that on the others, some of the berries showing signs of ripening. The experiment was interrupted for two nights at this stage, and when the electric light was resumed, very rapid progress was observable ; and after four days' continuous exposure to both day and electric light, the majority of the berries had become ripe, and showed a rich red colour, while the fruit on the plants that had been exposed to daylight only, scarcely showed even a sign of redness.

Melons that have been brought forward with the aid of the electric light also clearly prove its advantageous effects in promoting the setting of fruit, the process of ripening, and the production of aroma.

These experiments go to prove that electric light is efficacious in promoting the formation of fruit rich in bloom and aroma, and if these results should be confirmed, the horticulturist will be able to make himself practically independent of solar light for producing a high quality of fruit at all seasons of the year.

Although I have shown that a maximum beneficial result on vegetation is produced at a distance of 2 metres with a lamp of 1,400 candle power, the influence is very marked upon plants at greater distances. This action at a distance was proved by the condition of three melon plants situated towards the back of the house, which throve remarkably well for about a fortnight that the electric light was in front of the house, at a distance of from 5 to 6 metres from the plants. After the electric light was removed in front of the other end of the same house, and the plants in question were deprived of its influence, they continued their growth, but have shown a very decided falling-off in vigour.

An important consideration is the cost of electro-horticulture. This depends upon the cost of the fuel or other source of energy, and upon the scale of application. To work one electric lamp only with a small steam or gas engine is expensive, both as regards fuel and cost of attendance. If steam has to be resorted to, it is important to employ an engine of sufficient size to produce economical results per horse-power of energy expended, and the electric arc should be of sufficient brilliancy to give a good effect for the power employed. Experience in electric illumination has established a form and size of machine both convenient and suitable for the attainment of economical results, viz., the medium dynamo-electric machine, which, if applied to a suitable regulator, produces fully 6,000 candle-power of diffused light with an expenditure of 4 horse-power. The experiments already referred to show that the most effective height at which to place the naked electric arc of 1,400 candle-power is about 2 metres. By using a metallic reflector, the major portion of the upward rays may be thrown down upon the surface to be illuminated, and that height may be taken at 3 metres. If an electric arc of 6,000 candles was employed, the height would be $\frac{\sqrt{6,000}}{\sqrt{1,400}} \times 3 = 6{\cdot}2$ metres, at which such an electric light should be fixed. In operating upon an extended surface, several lamps should be so placed as to make the effect over it tolerably uniform. This would be so if the radiating centres were placed at distances apart equal to double their height above the ground; for a square foot of surface midway between them would receive from each centre one-half the number of rays falling upon such a surface immediately below a centre. A plant at the intermediate point would, however, have the advantage of presenting a larger leaf surface to both sources of light; and to compensate for this advantage, the light centres may be placed yet further apart, say at distances equal to 3 times their elevation, or 18 metres. Nine lights so placed would suffice for an area 54 metres square, or about $\frac{3}{4}$ acre. If a high fruit wall were to enclose this space, this will also get the full benefit of electric radiation, and would serve at the same time to protect the plants from winds. By subdividing the area under forced cultivation by vertical partitions of glass, as has been done with excellent results by Sir William Armstrong, protection is insured against injury from this latter cause.

There would be required to maintain this radiant action a $9 \times 4 = 36$ horse-power engine, involving the consumption of $36 \times 2\frac{1}{2} = 90$ pounds of fuel per hour, which, for a night of 12 hours, with 40 pounds for getting up steam, amounts to half-a-ton, costing, at 16*s.* a ton, 8*s.* a night. This does not include, however, the cost of carbons, or of an attendant, which would probably amount to as much more, making a total of 16*s.* If, however, an engine could be utilised doing other descriptions of work during the day, the cost of steam power and attendance for the night work only would be considerably reduced.

I have assumed in the calculation just given the use of fuel to produce mechanical energy, but the question will assume a totally different aspect if natural sources of power, such as waterfalls, can be rendered available within a short distance. The cost of power will in such case be almost entirely saved, and that of attendance greatly diminished, and it seems probable that under such circumstances electro-horticulture may be carried out with considerable advantage.

In reply to questions that have been frequently asked regarding the cost of maintaining an experimental electric light of 1,400 candle-power, such as I have used in these experiments, I may state that the 3 horse-power Otto gas engine employed in driving the dynamo machine consumes nearly 900 cubic feet of gas during the night of twelve hours, or 75 cubic feet an hour, which, at 3*s.* 6*d.* per 1,000 cubic feet, represents with the carbons a cost of 5*d.* an hour. This, however, does not include superintendence or incidental expenses, the amount of which must depend upon the circumstances of each case.

The experiments furnish proof that no particular skill is required in the management of the electrical apparatus, as the gas engine, dynamo machine, and regulator have been under the sole management of my head gardener, Mr. D. Buchanan, and of his son, an assistant gardener. The regulator only requires the replacement of carbons every four or five hours, which period may easily be extended to twelve hours, by a slight modification of the lamp.

I am led to the following conclusions as the result of my experiments :—

1. That electric light is both efficacious in producing chlorophyll in the leaves of plants and in promoting growth.

2. That a light-centre equal to 1,400 candles, placed at a distance of two metres from growing plants, appears to be equal in effect to average daylight in February, but more economical effects can be attained by more powerful light-centres.

3. That carbonic acid, and the nitrogenous compounds generated in diminutive quantities in the electric arc produce no sensible deleterious effects upon plants enclosed in the same space.

4. That plants do not appear to require a period of rest during the 24 hours of the day, but make increased and vigorous progress if subjected during daytime to sunlight and during the night to electric light.

5. That the radiation of heat from powerful electric arcs can be made available to counteract the effect of night frost.

6. That while under the influence of electric light, plants can sustain increased stove heat without collapsing, a circumstance favourable to forcing by electric light.

7. That the light is efficacious in hastening the development of flowers and of fruit; the flowers produced by its aid are remarkable for intense colouring, and the fruit both for bloom and aroma, without apparent augmentation of the saccharine constituents.

8. That the expense of electro-horticulture depends mainly upon the cost of mechanical energy, and is very moderate when natural sources of such energy, as waterfalls, can be made available.

Some observations made by Dr. Schübeler, of Christiania, to which my attention has been drawn, fully confirm the conclusion indicated by my experiments with electric light. According to Dr. Schübeler, plants are able to grow continuously; and when under the influence of continuous light, they develop more brilliant flowers and larger and more aromatic fruit than when under the alternating influence of light and darkness.

The useful influence of the electric light in horticulture having been thus established, I have taken steps to test the principle upon a working scale. Natural sources, such as water power, not being available, I have had to resort to steam as the motive agent. With this object I have laid down a 6 horse-power horizontal engine, by Tangye Brothers, and a Cornish boiler, fitted with 2 Galloway tubes in the flue, close to the conservatories at Sherwood, and at a distance of somewhat less than a quarter-of-a-mile from the farm buildings. The power of this engine is sufficient to give motion to two dynamo machines, capable of producing 12,000 candle-power of light. The steam, after doing its work in the engine, will be made available as a heating agent for the hothouses; but it having been found undesirable to pass such steam directly into the pipes leading to the houses, an intermediate tubular heater is used to effect the condensation of the steam, and to communicate its latent heat to the water circulating through the ranges of pipes in the usual manner. The fires now necessary to maintain the heat of the circulating pipes are suppressed, and that below the steam boiler substituted, which, admitting of an arrangement more suitable for economical results, will, it is expected, require little or no additional expenditure of fuel beyond what has hitherto been necessary to work the stoves. The power required for the electric light can therefore, under the peculiar circumstances of the case, be obtained at a comparatively low cost during the time the electric light is needed. The stoves, however, will require firing during the daytime ; and in order to utilise the engine power when not required for electric lighting, arrangements will be made so that the machines for chaff and turnip cutting at the farm, and also some wood-cutting machinery, shall be worked by electric transmission of power, the current being derived from the same dynamo-electric machine which works the electric light during the night. By means of these arrangements the subject of electro-horticulture will be put during the ensuing winter to the test, not only of what can be accomplished by its aid, but as to the cost at which those effects may be obtained.

A question of considerable interest connected with this subject is that of determining which portion of the rays constituting white light is efficacious in producing chlorophyll, starch, and woody fibre, and which in effecting the ripening of fruit. For this purpose arrangements are in preparation to distribute the spectrum of a powerful electric light in a darkened chamber over a series of similar plants exposed seriatim to the actinic, light-giving, and thermal portions of the spectrum. Some experiments have been made with solar light in this direction, but no very conclusive results could be obtained, because the short periods of time during which the solar spectrum can be maintained steadily in the same place is so short that the effects produced upon vegetation have not been of a sufficiently decided character ; whereas, with the aid of electric light, the same spectrum may be kept on steadily for a series of days without intermission.

For this purpose, and for electro-horticulture generally, it is important to employ a lamp with its focus unchangeable in space, and without obstruction to the rays of light falling downward. The lamp which I have designed for this purpose, and which has already been referred to in the paper read before the Society by Mr. Alexander Siemens, is represented in the accompanying drawing, Plate 21.

The principal point of novelty involved in it consists in the mode of advancing the carbons, which, instead of being effected by clockwork (as has been the case hitherto in constructing regulators), is effected simply by the force of gravity, or by spring power urging the carbons forward towards the point of meeting, in which forward motion each carbon is checked by a metallic abutment, in the form of a point or edge of copper or other metal of high conductivity, the exact position of which can be regulated by a screw.

This metallic ridge touches the carbons laterally, at a distance of 10 to 15 millimetres from the luminous point, where the temperature of the carbon is sufficient to cause its gradual decomposition in contact with the atmosphere, without being high enough to fuse or injure the metal.

In the lamp before you, represented in Plate 21, the carbons are contained in horizontal holders, suspended from the lamp frame by means of four suspension rods: a solenoid coil is placed vertically above the point of light, the iron core of which is connected to the suspension rods on either side by means of rods, whereby horizontal motion is applied to the metallic carbon-holders, tending to separate them when the current flowing through the solenoid coil diminishes, and approaching the carbons when the current passing through the coil increases; the effect is, that an increased resistance in the electric arc causes a shortening, and a decrease, a lengthening of the arc itself. In order to steady the action of this regulator, the iron core carries a piston, working freely up and down in a vertical cylinder, having a throttled aperture for the ingress and egress of atmospheric air above the piston. The two metallic holders are put into conductive connection with the two wires leading up from the dynamo-electric machine, and the regulating solenoid coil is connected to the two holders respectively, so that the current active in this coil is always proportionate to the potential between the two electrodes, which by this arrangement is made practically constant. If this lamp is worked by alternating currents, the two carbons are made equal in diameter; but if worked by a continuous current, the carbon connected with the positive pole should be made larger than the one connected with the negative pole, in the ratio of about 3 : 2.

Instead of the solenoid regulator, the steel tape regulator may be employed, which I described in a paper read before the Royal

Society on the 16th January, 1879, in reference to certain means of measuring and regulating electric currents. This thin strip or wire is in metallic connection at one end with one of the suspension rods connected with the positive pole, and at the other with one of the suspension rods connected with the negative pole, being led up and down over pulleys in order to produce a total resistance of from 20 to 30 ohms. The tension on this wire produced by balance weights prevents the carbon points from coming into contact with each other. Whenever the current is turned on, the iron strip becomes heated and elongates, allowing the carbons to approach each other. From the moment they touch the arc is formed, causing less current to pass through the iron strip or by pass, which consequently contracts on cooling, and causes the carbon poles to separate, thus effecting the proper regulation of the arc.

In its application to horticulture, a metallic parabolic reflector of considerable diameter is placed over the luminous centre, in order to reflect downwards all the rays of light and heat which would otherwise pass upward, an arrangement which may be advantageously carried out in these lamps as used for illumination when placed at a considerable elevation above the ground.

The horizontal carbon-holders may be made of considerable length, and one rod of carbon may be made to follow up the preceding one, in order to obtain a continuous action of the lamp; it is necessary, however, to join the one carbon to the succeeding one, by drilling the ends and introducing a short piece of steel connecting-wire between the two. The metallic connection between the carbon and the carbon-holder is effected by contact springs and levers, the latter of which may be so arranged that, when through some mistake carbons have not been supplied to a lamp at the proper time, the contact lever in tipping up, short-circuits the working current, causing the extinguishment of the lamp, without stopping the working of other lamps within the same electrical circuit.

Further experiments have shown that the colour of flowers and fruit subjected to continued electric light is much intensified. We have been able to bring strawberries to ripeness by means of the electric light fully a fortnight before the usual time, such fruit being remarkable for its colour and aromatic flavour. But it seems that the formation of sugar is not dependent upon this continuous light, and I might almost suggest the idea that the formation of sugar is the very last action that goes on in fruit; after the fruit has formed, developed, and acquired its aromatic qualities, then the formation of sugar seems to step in, as though it were the first stage of decay. Several botanists of high standing (Professor Cohen, of Leipzig, and others) have expressed the opinion that the growth of plants takes place chiefly at night: and there seems to be no doubt that during the night delicate and quick-growing plants, such as cucumbers and melons, make very considerable progress. But in that case they remain thin and yellow, whereas with continuous light they make less progress in length, but develop more in colour, in breadth, and in vigour; so that the truth may lie between the two views—that, for most rapid growth, intermittent light and darkness may be necessary, but that for vigorous development, and for the formation of fruit, it is not desirable.

The experiments now in preparation will perhaps settle some of these points, and will further show what can be done in this direction from a practical point of view.

Is it possible to make use of electric aid for growing plants and developing fruit that could be brought to the market? This is a mixed question. It depends, in the first place, upon the amount of effect that can be produced, and, in the second place, upon the cost. My opinion is, that the result of experiments on a large scale will come out favourably as regards its application to high-class horticulture. By working a steam engine, and using the waste steam to heat the water that circulates through the stoves, I believe that there will be very little extra consumption of fuel; and if that view be realised, the expense of the electric light will not be great at all, and the steam engine could be used in the daytime for various other ordinary useful purposes.

Then with regard to the spectrum experiments. It is an open question which portion of solar light is really efficacious in forming chlorophyll, and which in promoting growth and in producing starch and fibrous matter. The difficulty in experimenting with solar light is obvious. Since the days of Joshua the sun has not been standing still, and the power of making it do so is beyond the skill of botanists, hence the difficulty of obtaining a standing spectrum by which to notice sensible effects produced from any portion of it. In this respect the electric light has every advantage, for, by placing an electric lamp in focus to produce a permanent spectrum, series of plants can be placed in different portions of it, and the various results noted for any required period.

ON THE APPLICATION OF THE DYNAMO-ELECTRIC CURRENT TO LOCOMOTION.

I have frequently before this taken occasion to refer to the electric transmission of mechanical energy, and it is not my intention to revert to this subject generally, but to confine myself to an application for the propulsion of carriages along a railway, which has recently been carried into effect by my brother, Dr. Werner Siemens.

On the occasion of a local exhibition held in Berlin a year ago, a narrow gauge railway was laid down in a circle 900 yards long. Upon this railway a train of 3 or 4 carriages was placed, and upon the first carriage a medium-sized dynamo-electric machine so fixed and connected with the axle of one pair of wheels as to give motion to the same. The two rails, being laid upon wooden sleepers, were sufficiently insulated to serve for electric conductors. Between the two rails a bar of iron was fixed on wooden supports, through which the current was conveyed to the train by means of metallic brushes fixed to the driving carriage, while the return circuit was completed through the rails themselves. At the station the centre bar and two rails were connected electrically with the poles of a dynamo-electric machine similar in every way to the machine on the carriage, and which received motion from one of the engines on the ground. (A diagram was exhibited and explained, showing the arrangement, Dr. Siemens saying that he was indebted to Mr. Shoolbred for it, who had prepared it for his own purposes.)

Between twenty and thirty persons could be accommodated on the carriages composing this train, the conductor riding on the first carriage, to which the form of a small locomotive engine has been given. Instead of the steam valve used in the latter, this engine is fitted with a commutator, by moving which the stopping, starting, and reversing of the engine can be effected.

It is a remarkable circumstance in favour of the electric transmission of power, that while the motion of the electro-magnetic or power-receiving machine is small, its potential of force is at its maximum, and it is owing to this favourable circumstance that the electric train starts with a remarkable degree of energy. With the increase of motion the accelerating power diminishes until it comes to zero, when the velocity of the magneto or driven machine becomes equal to that of the dynamo or current-producing machine. Between the two limits of rest and maximum velocity the driving power regulates itself according to the velocity of the train; thus, on an ascending gradient the speed of the train diminishes, but the same effect is automatically produced which results from the turning on of more steam in the case of the locomotive engine. When running on the level, the velocity of the train should be such that the magneto-electric machine should make one-half to two-thirds as many revolutions per minute as the dynamo-electric. When descending, the speed of the magneto-electric machine will be increased, in consequence of the increased velocity of the train, until it exceeds that of the dynamo-electric machine, from which moment the functions of the two machines will be reversed; the machine on the train will become a current generator, and pay back, as it were, its spare power into store, performing at the same time the useful action of a brake in checking further increase in the velocity of the train. If two trains should be placed upon the same pair of rails, the one moving upon an ascending portion, the other upon a descending portion of the same, power will be transmitted through the rails from the latter to the former, which may therefore be considered as connected by means of an invisible rope.

The effects obtained with dynamo-electric machines under varying circumstances of load and velocity have been very fully investigated and brought forward by Dr. Hopkinson, F.R.S., in two papers read by him recently before the Institution of Mechanical Engineers, so that it would be superfluous for me to dwell upon this portion of the subject on the present occasion. Suffice it to say, that in transmitting the power of a stationary engine to a running train, the proportion of power actually transmitted varies with the resistance to, or speed of the train, reaching practically a maximum when the velocity of the machine on the train is about equal to two-thirds that of the current-generating machine, at which time more than fifty per cent. of the power of the stationary engine is actually utilised.

This little railway has been in operation daily for several months, affording great amusement to the visitors at the Exhibition. The magneto-electric engine exerts 5 horse-power, and it travels at a velocity of 15 to 20 miles an hour. Crowded trains left the station every five or ten minutes ; and the pennies paid for the privilege of a seat have produced a considerable sum for the benefit of charitable institutions. Many who were not so fortunate as to secure a seat on the train, amused themselves by touching the centre bar and one of the two rails after the train had passed, when a succession of electric discharges was distinctly felt.

The success attending this toy railway has given rise to the idea of useful applications upon a larger scale. An elevated tramway to connect one end of the city of Berlin with the other, has been projected, but its execution has hitherto been delayed in consequence of the objections raised by the inhabitants of the streets through which the tramway was to pass. These objections would not apply, however, in many cases ; and I have little doubt that before long we shall have electric tramways in connection with our mines, and for the conveyance of passengers along the roads between populous centres.

In passing through an adit or tunnel, the entire freedom of smoke from the electro-motor is a matter of great importance ; and the administration of the St. Gothard Tunnel contemplate seriously the application of an electro-motor for conveying trains through that gigantic tunnel. Circumstances are in this case highly favourable to the employment of an electro-motor, because at both ends of the tunnel turbines of enormous aggregate power are actually established (having been employed in the operation of boring the tunnel), and all that has to be done is to insulate one of the rails, and to connect dynamo machines of sufficient power to the turbines and to the train itself.

Instead of the central bar, a copper or other conducting rope may be used to convey the current from the dynamo machine to the train. This conducting rope would rest upon wooden or glass supports, to be picked up by the train in order to pass over one or more contact pulleys, and to be again deposited behind the train. The central rail or copper conductor may, however, be entirely dispensed with if the two rails laid upon wooden sleepers are connected the one with the positive and the other with the negative pole of the dynamo machine. In this case care must be taken to insulate the wheels on one side of the train from those on the other, an object that can be attained by the adoption of wheels with wooden centres, and the metallic tires of the wheels on the one side must be put into metallic connection with the one pole, and the other with the other pole of the machine or machines on the train. Practice alone can determine which of these modes of construction is the best, but each can be made efficacious, and the preference will be due to economical or structural considerations.

The length of this paper has already exceeded, I fear, reasonable limits, or I might be tempted to enlarge upon the subject of the electric transmission of power. Enough has been said, however, to illustrate some of the uses to which this new form of energy may be rendered available for the purposes of man.

(At the close of the paper a dynamo machine was set to work, and supplied motive power to a circular saw, which cut up several pieces of timber from two to three inches square.)

In the discussion of the Paper

"ON LIGHTHOUSE CHARACTERISTICS,"

By Sir William Thomson,

Dr. C. W. Siemens, F.R.S.,[*] said the subject was one of great interest to him, but he had not given sufficient attention to the details to be able to speak with any authority upon it. He might say a word or two, however, on the probability of seeing the electric light established for the purpose of giving those flashes which had been referred to. He might certainly say that the electric

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[*] Excerpt Journal of the Society of Arts, Vol. XXIX. 1880–81, p. 313.

light appeared destined to take the place of all other lights for that purpose. In dealing with light produced by the combustion of oil or gas, they were necessarily limited to the amount to be obtained under given conditions. A large amount of light could be obtained by combustion, but it could not be concentrated within the focus of a lamp. It was only by the electric current that small surfaces could be heated to a point far exceeding the temperature attainable by combustion, and send out rays of light second in energy only to those of the sun. It had been proved by Ste.-Claire Deville, and Bunsen, that the utmost temperature to be obtained by combustion was about 2,400° Cent., when a point was reached at which combustion ceased and dissociation set in: and therefore it was impossible to obtain rays of high intensity by means of combustion. There were, no doubt, practical difficulties to be overcome in applying the electric light to some situations where power could not be easily raised; but means of producing power were continually being improved. Where you could not raise steam you could decompose oil; and where you could not work a steam-engine you could use a gas-engine or an oil-engine, as was already done in the United States to a large extent. With the electric light also an admirable system of flashes of any desired rapidity could be attained; and he would conclude by expressing a hope that they would soon attain the desired point when each lighthouse would not only tell its own tale, but also give that information to the greatest possible distance.

In the discussion of the Paper

"ON RECENT ADVANCES IN ELECTRIC LIGHTING,"

By Mr. W. H. Preece,

The Chairman (Dr. C. W. Siemens),* in moving a vote of thanks to Mr. Preece for his valuable paper, said that gentleman had passed the whole subject of electric lighting in review, in a

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* Excerpt Journal of the Society of Arts, Vol. XXIX. 1880-81, p. 435, 436.

manner which must have struck home to the minds even of those among the audience who had not before given particular attention to the subject. He had followed the energy pent up in the coal in former ages through its transformations in the steam-engine and the dynamo machine, where it was manifested as an electric current, passing through the conductor into the lamp regulator, where, through the resistance offered to its passage, heat was again generated, being the very form of energy with which they started, with the difference, however, that the heat produced in the electrodes of the lamp was of a much more intensified nature than the heat developed by the combustion of the coal. Hence, after all, electric lighting meant nothing else but carrying energy from the coal to the carbon in the lamp. But simple as the problem appeared when thus put, it had required the combined ingenuity and labour of philosophers and of practical electricians, extending not indeed over centuries but over decades; and even now a point had only been arrived at where it could be said that electric lighting was feasible. At the present day, advances were made more rapidly than had ever been the case before, and before long it might be possible to say that electric lighting was an accomplished fact. The great experiment soon to be made in the City of London would be an event of the greatest importance, and the greatest city in the world was now leading the way in utilising this new agency in a way which would leave no doubt as to its efficacy. Photometry, the sub-division of the electric light, and various applications of electricity, had also been touched upon in the paper, and though most of the propositions put forward in it would be accepted by all who understood the subject as natural facts, still, naturally enough, in so new a science, there were other points which were controvertible, and which he (the Chairman) would like to argue with Mr. Preece, but that he feared to try the patience of the meeting. They had sometimes argued questions very strongly, but had always been very good friends afterwards. If he had understood aright, Mr. Preece hoped, and great philosophers had entertained the same hope, that the divided light would ultimately equal the centralised light in economy. He begged to differ from that conclusion. Divided light meant light brought nearer to the eye, and the eye could not bear a light of such intensity in close proximity as it could bear at a distance. Mr. Preece had very well said that light was nothing but heat of the intensest kind. In order to have the greater number of light rays over heat rays emanating from a centre, it was necessary that the temperature should be raised to the utmost attainable point; even in the electric arc, then burning in the lamp before them, probably nine-tenths of the rays emanating from that centre were not luminous, but heat rays, otherwise, even with the lamp so far removed, they would not be able to bear the light. Although he believed that divided lights would be very largely used, and with great effect, where centralised light was not applicable, yet it might well be argued from *a priori* reasoning that a central light must be always more economical than a divided light. With regard to his own experiments, mentioned by Mr. Preece, he had carried them on since last year for the purpose of promoting the growth of plants by the electric arc, with the object, not so much of ripening strawberries and cucumbers sooner than his neighbours, but of ascertaining to what extent it was possible to produce rays capable of acting in substitution for solar rays, and also to what extent plants could be accustomed to bear this agency without intermission. He hoped to be able to lay further results before the scientific societies before long. One point of interest was the fact that the steam-engine he employed to produce the electric light at night, afterwards yielded, through condensation of the waste steam, the heat for the greenhouses, so that the electric light did not add materially to his coal consumption. Having to keep a fire under the boiler day and night, he thought it a good opportunity for utilising the steam power during the daytime, and he had done so by means of leading wires from the dynamo machine, to another similar machine at the farmstead working a chaff-cutter, to another for working a pumping engine nearly half a mile distant, and to a saw-bench in another direction; so that while doing its work near the greenhouses, the engine was also cutting chaff and wood in one direction, and pumping water in another, and he hoped yet to make it available for ploughing the land also. Those facts showed that this mode of energy was extremely pliable, and could with great ease be made available at a distance. It is also important to remark that no other electrician was employed to keep the apparatus in order than the head gardener, without certainly any special training for this work.

In the discussion of the Paper

"ON ELECTRICAL RAILWAYS AND TRANSMISSION OF POWER BY ELECTRICITY,"

By Mr. Alexander Siemens,

Dr. C. W. Siemens, F.R.S.,* said he would only make a few remarks that evening, and speak more at length when the discussion was resumed next week. Professor Ayrton had remarked that the dynamo machine would be superseded by the magneto machine, or by a dynamo machine with a separate exciter, and he confessed that he went a long way with him in his argument; indeed, last year he communicated a paper to the Royal Society in which he showed certain defects in the dynamo machine, and suggested certain remedies. The dynamo laboured under this defect, that, with an increase of work, the power to overcome the resistance diminished. The current produced by the rotation of the coils in the magnetic field had to excite the coils of the magnet itself, and the current then passed on to the second machine or to the light, to the place where the work was to be performed. Now if that work should present increased resistance, the machine which had to overcome it should increase in energy, whereas the greater resistance caused a weakening of the current and a falling off in the power of the magnets by which the current was produced, thus causing those fluctuations which were so troublesome in electric lamps, but which, by different arrangements, had been almost overcome, and would be entirely overcome by the aid of further experience. It was quite true that in the City they were working with dynamo machines having separate exciters, but the dynamo machine could be so arranged that a portion only of the current was set aside to excite its own magnet, and if that arrangement were properly applied, he believed all the advantages of a separate exciter could be secured with a single machine. The subject especially before them, however, was the application of

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* Excerpt Journal of the Society of Arts, Vol. XXIX. 1880–81, p. 574, and pp. 588, 589.

electricity to the propulsion of railways and the transmission of power, of which the propulsion of carriages was only one branch. Several other methods by which propulsion could be effected might be mentioned. Only a few days ago he had been in Paris, and had arranged for the construction of a short line of comparatively broad gauge, which was to be carried out by the omnibus company of Paris, in connection with the Electrical Exhibition. An ordinary tram-car would be run from the Place de la Concorde to the Exhibition, upon rails laid in the usual manner, having a suspended conductor along the side of the railway. This conductor would have a little carriage passing along it, in order to transmit the electric current from the suspended wire to the machine, and back through the rails themselves. That arrangement, which was devised by Dr. Werner Siemens, made them independent of partial insulation of the rails upon which the carriage ran, and also independent of the partial insulation of the wheels of one side from the other, leaving the rolling stock very much the same as at present, transferring the current to a separate conductor, something analogous to a single wire telegraph, upon which the contact roller ran and conveyed the current to the machine. Another arrangement by which an ordinary omnibus might be run upon the street would be to have a suspender thrown at intervals from one side of the street to the other, and two wires hanging from these suspenders; allowing contact-rollers to run on these two wires, the current could be conveyed to the tram-car, and back again to the dynamo machine at the station, without the necessity of running upon rails at all. He merely mentioned this to show that the system was not one which must be carried out in one particular way only, but was capable of very wide modification and extension according to circumstances. The paper referred to certain applications which he had made of electricity, near Tunbridge Wells, to horticulture, and on the table was a melon which had been produced by the aid of the electric light. He hoped the Chairman would take it home, and report upon it next week.

In the adjourned discussion of the above Paper,

Dr. Siemens, F.R.S., said Mr. Preece, with his well-known ability, had just shown that the power to be obtained from the motor machine could not exceed one-half that communicated to the generator. That, however, was a question which had been much discussed amongst electricians, and Mr. A. Siemens had adopted the safer course, of rather under than over-stating the results, which might be and had been obtained. There was by no means such a limit as 50 per cent. Experiments of undoubted accuracy had shown that you could obtain 60 or 70 per cent., and that the point of maximum effect was not limited to half the velocity; though he quite agreed with Mr. Preece that there was a limit. If the velocities were equal theoretically, the maximum result should be obtained, but the counter current produced in that case was also a maximum, so that practically the maximum lay between the two results of half velocity and equal velocity. He had in his hand a report, received only that day, with regard to the working of the little railway at Berlin, in which his brother put it, as the result of observation and measurement, that 60 per cent. of useful effect was realised; but this was under very peculiar conditions. And it was one of the remarkable features connected with the electric transmission of power, that as the resistance to be overcome in the railway carriage increased, so did the force increase to overcome the resistance. Thus, in going on a level, the power used to propel the train might be 10 h.p.; but when the train ascended a gradient of 1 in 80, which was the steepest on the line, then the power necessary to drive the dynamo machine at the station increased, and the power transmitted to the carriages increased in a still greater ratio. Indeed, it was a surprise to everyone who had investigated this little railway to see with what determined force the carriages ascended the incline, with comparatively little decrease of velocity. Of course, in order to overcome the greater resistance, the velocity had to decrease. It was stated in the paper that the velocity of the train had been limited to ten miles an hour; but, seeing the facilities with which the train ran, greater speed had been allowed, and the carriages had gone to the distant station and back in seven and a-half minutes, which meant an average speed of about twenty-five miles an hour. A difficulty had arisen, as happened with most new inventions, and this difficulty was of a most peculiar kind. In the Berlin railway, one rail conducted the current towards the carriages, and the other took it back to the station. Now, if a man passed over the line at a level crossing, no harm was done, because he put his foot on only one rail at a time; but a horse being endowed with four feet, he sometimes put one foot on one rail and another on the other, and thus experienced a most inconvenient shock, so much so, that horses decidedly objected to these level crossings, and it became necessary to make some special arrangements to avoid this inconvenience. It sufficed to put one rail at the crossings out of circuit, and to connect the backward and forward rail electrically. This experiment showed the practicability of the system, but his brother entirely agreed with him that it was not by any means to be supposed that the electric railway would banish locomotive engines from our great thoroughfares. The electric transmission of power would be efficacious, no doubt, for local traffic, such as tramways, and also for lines conveying minerals from the interior of a mine to the bank, and in exceptional cases for the transmission of heavy trains along rails. One of these cases was presented by the St. Gothard Tunnel. The company to which that belonged were fully alive to all modern improvements, and had requested them to work out a plan for utilising the hydraulic power, which could be had in great abundance near the mouth of the tunnel, for the passage of the train through the tunnel. By the accomplishment of that object, very great advantages would be gained; for, as those who had travelled through the Mont Cenis Tunnel, or through the one on the line between Alessandria and Genoa, were aware, great inconvenience resulted from the emission of the products of combustion from the engines during the transit. If a train could be sent through this long Alpine tunnel by electric force, a great inconvenience would be saved to the passenger, and at the same time a great saving would be effected for the company. Nearer home there was a case which would lend itself admirably to electric transmission—the Underground District Railway. All those who were in the habit of using that railway appreciated the facilities it offered in going to the City or from it; but they also felt the inconveniences of the products of combustion choking the atmosphere. Plans had been proposed for more thoroughly ventilating the tunnel, but they were only palliatives; the cure would consist in finding a source of power without the inconvenience of combustion being carried on in the tunnel. A plan had been proposed for working the engines by compressed air, and he had nothing to say against it, but it did not do away with the necessity of having an engine nearly as heavy as the present locomotive. He believed if electric transmissions were tried on that railway in such a way as to make the rails act as the return conductor, making them all "earth," and fixing guide rails under the roof for the conveyance of the current, to be taken into each carriage by means of a metallic rope, great certainty of action would be obtained, and the trains would be propelled through the tunnel at a very economical rate, and without fear of their being stopped midway. These were the features of this innovation; that it lent itself to the conveyance of power to any reasonable distance, and that it could be applied without any of those inconveniences which now beset our locomotive traffic. He hoped that before long a trial would be made of the system, not, perhaps, on a very large scale at first, but sufficient to show its merits.

ON SOME APPLICATIONS OF ELECTRIC ENERGY TO HORTICULTURE AND AGRICULTURE.

By C. Wm. Siemens,* D.C.L., LL.D., F.R.S., Mem. Inst. C.E.

On the 1st of March, 1880, I communicated to the Royal Society a paper, "On the Influence of Electric Light upon Vegetation, &c.," in which I arrived at the conclusion that electric light was capable of producing upon plants effects comparable to those of solar radiation; that chlorophyll was produced by it, and that bloom and fruit rich in aroma and colour could be developed by its aid. My experiments also went to prove that plants do not as a rule require a period of rest during the twenty-four hours of the day, but make increased and vigorous

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* Paper read before Section A of the British Association, 1st September, 1881, and ordered to be printed *in extenso* among the reports. Journal of the British Association for the Advancement of Science, 1881, pp. 474–480.

progress if subjected in winter time to solar light during the day and to electric light during the night.

During the whole of last winter I continued my experiments on an enlarged scale, and it is my present purpose to give a short account of these experiments and of some further applications of electric energy to farming operations (including the pumping of water, the sawing of timber, and chaff and root cutting) at various distances not exceeding half a mile from the source of power, giving useful employment during the day-time to the power-producing machinery, and thus reducing indirectly the cost of the light during the night-time. The arrangement consists of a high-pressure steam engine of 6 horse-power nominal, supplied by Messrs. Tangye Brothers, which gives motion to two dynamo machines (Siemens D), connected separately to two electric lamps, each capable of emitting a light of about 5000 candle power. One of these lamps was placed inside a glasshouse of 2318 cubic feet capacity, and the other was suspended at a height of 12 to 14 feet over some sunk greenhouses. The waste steam of the engine was condensed in a heater, whence the greenhouses take their circulating supply of hot water, thus saving the fuel that would otherwise be required to heat the stoves.

The experiments were commenced on the 23rd of October, 1880, and were continued till the 7th of May, 1881. The general plan of operation consisted in lighting the electric lights at first at 6 o'clock, and during the short days at 5 o'clock, every evening except Sunday, continuing their action until dawn. The outside light was protected by a clear glass lantern, while the light inside the house was left naked in the earlier experiments, one of my objects being to ascertain the relative effect of the light under these two conditions. The inside light was placed at one side over the entrance into the house, in front of a metallic reflector, to save the rays that would otherwise be lost to the plants within the house.

The house was planted in the first place with peas, French beans, wheat, barley, and oats, as well as with cauliflowers, strawberries, raspberries, peaches, tomatoes, vines, and a variety of flowering plants, including roses, rhododendrons, and azaleas. All these plants being of a comparatively hardy character, the temperature in this house was maintained as nearly as possible at 60° Fahr.

The early effects observed were anything but satisfactory. While under the influence of the light suspended in the open air over the sunk houses the beneficial effects due to the electric light observed during the previous winter repeated themselves, the plants in the house with the naked electric light soon manifested a withered appearance. Was this result the effect of the naked light, or was it the effect of the chemical products—nitrogenous compounds and carbonic acid—which are produced in the electric arc? Proceeding on the first-named assumption, and with a view of softening the ray of the electric arc, small jets of steam were introduced into the house through tubes, drawing in atmospheric air with the steam, and producing the effect of clouds interposing themselves in an irregular fashion between the light and the plants. This treatment was decidedly beneficial to the plants, although care had to be taken not to increase the amount of moisture thus introduced beyond certain limits. As regards the chemical products, it was thought that these would prove rather beneficial than otherwise in furnishing the very ingredients upon which plant life depends, and, further, that the constant supply of pure carbonic acid resulting from the gradual combustion of the carbon electrodes, might render a diminution in the supply of fresh air possible, and thus lead to economy of fuel.

The plants did not, however, take kindly to these innovations in their mode of life, and it was found necessary to put a lantern of clear glass round the light, for the double purpose of discharging the chemical products of the arc and of interposing an effectual screen between the arc and the plants under its influence. The effect of interposing a mere thin sheet of clear glass between the plants and the source of electric light was most striking. On placing such a sheet of clear glass so as to intercept the rays of the electric light from a portion only of a plant—for instance, a tomato plant—it was observed that in the course of a single night the line of demarcation was most distinctly shown upon the leaves. The portion of the plant under the direct influence of the naked electric light, though at a distance from it of 9 feet to 10 feet, was distinctly shrivelled, whereas that portion under cover of the clear glass continued to show a healthy appearance, and this line of demarcation was distinctly visible on individual leaves. Not only the leaves, but the young stems of the plants, soon showed signs of destruction when exposed to the naked electric light, and these destructive influences were perceptible, though in a less marked degree, at a distance of 20 feet from the source of light.

A question here presents itself that can hardly fail to excite the interest of the physiological botanist. The clear glass does not apparently intercept any of the luminous rays, which cannot therefore be the cause of the destructive action. Professor Stokes has shown, however, in 1853, that the electric arc is particularly rich in highly refrangible invisible rays, and that these are largely absorbed in their passage through clear glass, it therefore appears reasonable to suppose that it is those highly refrangible rays beyond the visible spectrum that work destruction on vegetable cells, thus contrasting with the luminous rays of less refrangibility, which, on the contrary, stimulate their organic action.

Being desirous to follow up this inquiry a little further, I sowed a portion of the ground in the experimental conservatory with mustard and other quick-growing seeds, and divided the field into equal radial portions by means of a framework, excluding diffused light, but admitting light at equal distances from the electric arc. The first section was under the action of the naked light, the second was covered with a pane of clear glass, the third with yellow glass, the fourth with red, and the fifth with blue glass. The relative progress of the plants was noted from day to day, and the differences of effect upon the development of the plants were sufficiently striking to justify the following conclusions:—Under the clear glass the largest amount of and most vigorous growth was induced; the yellow glass came next in order, but the plants, though nearly equal in size, were greatly inferior in colour and thickness of stem to those under the clear glass; the red glass gives rise to lanky growth and yellowish leaf; while the blue glass produces still more lanky growth and sickly leaf. The uncovered compartment showed a stunted growth, with a very dark and partly shrivelled leaf. It should be observed that the electric light was kept on from 5 P.M. till 6 A.M. every night except Sundays during the experiment, which took place in January, 1881, but that diffused daylight was not excluded during the intervals; also that circulation of air through the dividing framework was provided for.

These results are confirmatory of those obtained by Dr. J. W. Draper (see 'Scientific Memoirs,' by J. W. Draper, M.D., LL.D., Memoir X.) in his valuable researches on plant cultivation in the solar spectrum in 1843, which led him to the conclusion, in opposition to the then prevailing opinion, that the yellow ray, and not the violet ray, was most efficacious in promoting the decomposition of carbonic acid in the vegetable cell.

Having in consequence of these preliminary inquiries determined to surround the electric arc with a clear glass lantern, more satisfactory results were soon observable. Thus peas which had been sown at the end of October produced a harvest of ripe fruit on the 16th of February, under the influence, with the exception of Sunday nights, of continuous light. Raspberry stalks put into the house on the 16th of December produced ripe fruit on the 1st of March, and strawberry plants planted about the same time produced ripe fruit of excellent flavour and colour on the 14th of February. Vines which broke on the 26th of December produced ripe grapes of stronger flavour than usual on the 10th of March. Wheat, barley, and oats shot up with extraordinary rapidity under the influence of continuous light, but did not arrive at maturity; their growth having been too rapid for their strength, caused them to fall to the ground after having attained the height of about 12 inches.

Seeds of wheat, barley, and oats planted in the open air and grown under the influence of the external electric light produced, however, more satisfactory results; having been sown in rows on the 6th of January, they germinated with difficulty on account of frost and snow on the ground, but developed rapidly when milder weather set in, and showed ripe grain by the end of June, having been aided in their growth by the electric light until the beginning of May. Doubts have been expressed by some botanists whether plants grown and brought to maturity under the influence of continuous light would produce fruit capable of reproduction; and in order to test this question, the peas gathered on the 16th of February from the plants which have been grown under almost continuous light action were replanted on the 18th of February. They vegetated in a few days, showing every appearance of healthy growth. Further evidence on the same question will be obtained by Dr. Gilbert, F.R.S., who has undertaken to experiment upon the wheat, barley, and oats grown as above stated, but still more evidence will probably be required before all doubt on the subject can be allayed.

I am aware that the great weight of the opinion of Dr. Darwin goes in favour of the view that many plants, if not all of them, require diurnal rest for their normal development. In his great work on 'The Movements of Plants' he deals in reality with plant life, as it exists under the alternating influence of solar light and darkness; he investigates with astonishing precision and minuteness their natural movements of circumnutation and nightly or nyctitropic action, but does not extend his inquiries to the conditions resulting from continuous light. He clearly proves that nyctitropic action is instituted to protect the delicate leaf-cells of plants from refrigeration by radiation into space, but it does not follow, I would submit, that this protecting power involves the necessity of the hurtful influence. May it not rather be inferred from Dr. Darwin's investigations that the absence of light during night-time involved a difficulty to plant life that had to be met by special motor organs, which latter would perhaps be gradually dispensed with by plants if exposed to continual light for some years or generations.

It is with great diffidence, and without wishing to generalise, that I feel bound to state as the result of all my experiments, extending now over two winters, that although periodic darkness evidently favours growth in the sense of elongating the stalks of plants, the continuous stimulus of light appears favourable for healthy development at a greatly accelerated pace through all the stages of the annual life of the plant, from the early leaf to the ripened fruit. The latter is superior in size, in aroma, and in colour to that produced by alternating light, and the resulting seeds are not, at any rate, devoid of regerminating power. Further experiments are necessary, I am aware, before it would be safe to generalise, nor does this question of diurnal rest in any way bear upon that of annual or winter rest, which probably most plants, that are not so-called annuals, do require.

The beneficial influence of the electric light has been very manifest upon a banana palm, which at two periods of its existence—viz. during its early growth and at the time of the fruit development—was placed (in February and March of 1880 and

1881) under the night action of one of the electric lights, set behind glass at a distance not exceeding two yards from the plant. The result was a bunch of fruit weighing 75 lb., each banana being of unusual size, and pronounced by competent judges to be unsurpassed in flavour.

Melons also remarkable for size and aromatic flavour have been produced under the influence of continuous light in the early spring of 1880 and 1881, and I am confident that still better results may be realised when the best conditions of temperature and of proximity to the electric light have been thoroughly investigated.

My object hitherto has rather been to ascertain the general conditions necessary to promote growth by the aid of electric light than the production of quantitative results; but I am disposed to think that the time is not far distant when the electric light will be found a valuable adjunct to the means at the disposal of the horticulturist in making him really independent of climate and season, and furnishing him with a power of producing new varieties.

Before electro-horticulture can be entertained as a practical process it would be necessary, however, to prove its cost, and my experiments of last winter have been in part directed towards that object. Where water-power is available the electric light can be produced at an extremely moderate cost, comprising carbon electrodes, and wear and tear of and interest upon apparatus and machinery employed, which experience elsewhere has already shown to amount to 6*d.* per hour for a light of 5000 candles. The personal current attention requisite in that case consists simply in replacing the carbon electrodes every six or eight hours, which can be done without appreciable expense by the under-gardener in charge of the fires of the greenhouses.

In my case no natural source of power was available, and a steam engine had to be resorted to. The engine, of 6 nominal horse-power, which I employ to work the two electric lights of 5000 candle-power each, consumes 56 lb. of coal per hour (the engine being of the ordinary high-pressure type), which, taken at 20*s.* a ton, would amount to 6*d.*; or to 3*d.* per light of 5000 candles. But against this expenditure has to be placed the saving of fuel effected in suppressing the stoves for heating the greenhouses, the amount of which I have not been able to ascertain accurately, but it may safely be taken at two-thirds of the cost of coal for the engine, thus reducing the cost of the fuel per light to 1*d.* per hour; the total cost per light of 5000 candles will thus amount to 6*d.* plus 1*d.*, equal to 7*d.* per hour.

This calculation would hold good if the electric light and engine power were required during, say, twelve hours per diem, but inasmuch as the light is not required during the day-time, and the firing of the boiler has nevertheless to be kept up in order to supply heat to the greenhouses, it appears that during the day-time an amount of motive power is lost equal to that employed during the night. In order to utilise this power I have devised means of working the dynamo machine also during the day-time, and of transmitting the electric energy thus produced by means of wires to different points of the farm where such operations as chaff-cutting, swede-slicing, timber-sawing, and water-pumping have to be performed. These objects are accomplished by means of small dynamo machines, placed at the points where power is required for these various purposes, and which are in metallic connection with the current-generating dynamo machine near the engine. The connecting wires employed consist each of a naked strand of copper wire, supported on wooden poles, or on trees, without the use of insulators, while the return circuit is effected through the park railing or wire fencing of the place, which is connected with both transmitting and working machines, by means of short pieces of connecting wire. In order to ensure the metallic continuity of the wire fencing, care has to be taken wherever there are gates to solder a piece of wire buried below the gate to the wire fencing on either side.

As regards pumping the water, a 3 horse-power steam engine was originally used, working two force-pumps, of 3½ inches diameter, making 36 double strokes per minute. The same pumps are still employed, being now worked by a dynamo machine weighing 4 cwt. When the cisterns at the house, the gardens, and the farm require filling, the pumps are started by simply turning the commutator at the engine station, and in like manner the mechanical operations of the farm already referred to are accomplished by one and the same prime mover.

It would be difficult in this instance to state accurately the percentage of power actually received at the distant station, but in trying the same machines under similar circumstances of resistance with the aid of dynamometers as much as 60 per cent. has been realised.

In conclusion, I have pleasure to state that the working of the electric light and transmission of power for the various operations just named are entirely under the charge of my head gardener, Mr. Buchanan, assisted by the ordinary staff of under-gardeners and field labourers, who probably never before heard of the power of electricity. Electric transmission of power may eventually be applied also to thrashing, reaping, and ploughing. These objects are at the present time accomplished to a large extent by means of portable steam engines, a class of engine which has attained a high degree of perfection, but the electric motor presents the great advantage of lightness, its weight per horse-power being only 2 cwt., while the weight of a portable engine with its boiler filled with water may be taken at 15 cwt. per horse-power. Moreover, the portable engine requires a continuous supply of water and fuel, and involves skilled labour in the field, while the electrical engine receives its food through the wire (or a light rail upon which it may be made to move about) from the central station, where power can be produced at a cheaper rate of expenditure for fuel and labour than in the field. The use of secondary batteries may also be resorted to with advantage to store electrical energy when it cannot be utilised. In thus accomplishing the work of a farm from a central power station, considerable savings of plant and labour may be effected: the engine power will be chiefly required for day-work, and its night-work, for the purposes of electro-horticulture, will be a secondary utilisation of the establishment involving little extra expense. At the same time the means are provided of lighting the hall and shrubberies in the most perfect manner, and of producing effects in landscape gardening that are strikingly beautiful.

A CONTRIBUTION TO THE HISTORY OF SECONDARY BATTERIES.

BY C. WILLIAM SIEMENS.*

The surprising effects realised by Faure give particular interest at the present time to the general subject of secondary batteries, and it may not be uninteresting to the members of this Section to put before them an account of some early attempts in this direction with which I have been connected.

The earliest and, as regards its principle of action, the most perfect and admirable form of secondary battery is, I venture to think, that proposed by Sir William Grove as early as 1841. It consisted, as is well known, of two test tubes with a strip of platinised platinum suspended in each from an electrode passing through the tube, the two tubes dipping with their open ends into a trough filled with acidulated water. In passing a galvanic current through such a pair, hydrogen is developed in the one tube and oxygen in the other in the well-known proportions, and if the battery be disconnected, and the electrodes be connected by means of a wire, with a galvanometer of high resistance, it will be found that a continuous current is produced, exceeding a Daniell element in electro-motive force, which current continues to flow until the whole of the gases accumulated previously in the tubes by means of the galvanic current have recombined. The current so produced necessarily equals that by which the decomposition was effected, barring only losses by resistance, which, in the case of Grove's gas battery, admit of the utmost reduction. The drawback to any practical use that could be made of the Grove gas battery is that the active surface of triple contact between the metal, the acidulated water, and the gas is exceedingly small, and consequently that the amount of current to be got from such a battery in a given time is also too small for practical use.

In the year 1852 the problem was put to me whether, by some modification of the Grove gas battery, it would not be possible to

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* Paper read before Section A of the British Association for the Advancement of Science, 5th September, 1881.

obtain larger effects, and, applying myself to the question, I undertook a series of experiments, the results of which were embodied in a report, which was deemed satisfactory at the time, but has never been published. Now, however, these results appear to reassume some practical value. Starting with the Grove battery, I endeavoured to obtain a form of electrode presenting a large surface of triple contact. Platinum appeared ill suited for the attainment of such an object, and I consequently directed my attention to carbon, such as is deposited in gas retorts, as being a cheaper material, and one that, owing to its porosity and roughness of surface, seemed well calculated for the development of surface action. Two pieces of such carbon inserted into inverted glass tubes similarly to the strips of platinum already referred to, gave rise to currents of larger quantitative effect, although somewhat inferior in intensity to those produced by the platinum strips. The intensity, however, was greatly increased by subjecting the carbons previous to use to a process of platinisation, or galvanic deposition of pulverulent platinum on their surfaces. The next step was to put carbon into the shape of tubes open at one end and closed at the other. A number of these tubes were inserted in a square box of gutta-percha in rows traversing the box alternately in one direction and the other, the box being ultimately placed edge-ways and connected with two chambers covering respectively the open ends of the two series of tubes, Plate 22. By filling these two chambers, the one with oxygen, the other with hydrogen gas, and filling the square box containing the tubes with acidulated water, I succeeded in converting the entire carbon surfaces into surfaces of triple contact of carbon, acidulated water, and oxygen and hydrogen gas respectively, owing to the porosity of the material of the tubes; and it was only necessary to connect the upper closed and protruding ends of the tubes by means of wire in order to constitute the arrangement a gas battery of considerable power. Nevertheless, the current was insufficient for my purpose, though care had been taken to platinise the tubes.

With a view of increasing the potential of the currents, I directed my attention to the peroxides of metals, and soon found that peroxide of lead was the one giving the greatest promise of results. The tubes were plunged, after drying, into a strong solution of acetate of lead, they were then redried and heated to a dull redness, and again immersed in the lead solution. After repeating this process several times, they were placed in position, and a strong battery current was passed through them, by which the lead was converted into peroxide. The increase of current resulting from this mode of treatment was so remarkable that I was able to effect the decomposition of water by means of one such carbon-lead gas battery by connecting it to a voltameter. No reliable methods of ascertaining the potential of the current were available at that time, but, judging by the results, the power of two volts must have been reached.

It was, however, found difficult to obtain a supply of carbon tubes of the right degree of porosity, and I therefore fell back on a simpler form of battery, consisting of two bars or rods of dense carbon, upon each of which a long series of thin laminæ of porous carbon, pierced laterally by holes to admit the carbon rod, were strung, a certain distance between the laminæ being ensured by washers of the same material. Two such bars of carbon with their laminæ were placed side by side in a cylinder of gutta-percha with a dividing diaphragm of porous clay, and constituted, when impregnated with peroxide of lead, a powerful galvanic cell, Fig. 3, Plate 22. The power of the cell depended more, however, on the power and time of application of the exciting current than upon the gases admitted into the cylinder, showing that it was chiefly due to the presence of the peroxide of lead formed by the exciting current.

These exciting currents produced by a Grove nitric-acid battery were, however, too expensive to render the secondary battery available for practical purposes, whereas by the use of dynamo currents, results might have been obtained comparable to those obtained by means of the Faure battery. By the substitution of porous carbon for sheet lead in the secondary battery of the present day, the intervening layers of felt might be dispensed with, and a large amount of active surface be aggregated in a comparatively small space.

In the discussion of the Paper

ON "LES CHEMINS DE FER ELECTRIQUES,"

By Dr. Werner Siemens,

Dr. C. Wm. Siemens :* Permettez-moi de dire quelques mots sur la communication de mon frère, que nous venons de recevoir. Je crois que ce mémoire indique assez clairement les limites dans lesquelles mon frère pense que le chemin de fer électrique peut être appliqué. Les trois applications que nous avons déjà faites, et dont il parle, présentent entre elles des différences essentielles. Dans l'une, la première, il y a un conducteur en forme de rail situé entre les deux rails sur lesquels les wagons circulent, ces derniers servant de conducteurs de retour. Cette disposition présente un inconvénient évident en exigeant un troisième rail, lequel doit être assez fort pour résister au trafic ordinaire des rues. L'on a pu modifier cette construction à Lichtenfelde de manière à diriger un courant par le rail d'un côté, et celui de retour par le rail de l'autre côté ; cette disposition, qui forme le second système, me paraît la plus convenable pour les chemins élevés, où il est possible de maintenir les deux rails propres et d'obtenir ainsi un contact assez parfait entre les roues et les rails, mais il nécessite une modification dans la construction du matériel de roulement, en ce sens que les roues d'un côté et de l'autre du wagon moteur doivent être isolées les unes des autres, c'est-à-dire que les deux roues d'un côté doivent être construites en une matière isolante, telle que le bois par exemple. Pour l'application que nous avons faite ici à l'exposition, et à laquelle j'ai pris quelque part, il aurait été impossible d'employer l'une ou l'autre de ces dispositions à cause du trafic considérable des rues, et de la boue qui empêcherait le contact entre les roues et les rails ; c'est ce qui a amené la nécessité d'employer deux conducteurs aériens. Cette disposition, qui forme le troisième système, a été exécutée par M. Boistel, le représentant de notre maison à Paris. Mon frère a appliqué une construction semblable à Charlottenbourg, où je crois que les

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. X. 1881, pp. 370–1.

wagons ne rouleront pas sur des rails ; du moins les rails ne sont pas absolument nécessaires, quoiqu'en les supprimant l'on aurait plus de résistance à vaincre. Touchant la perte de force en transmission, dont la mémoire parle, quoique cette perte soit assez considérable, pouvant s'élever entre 40 et 60 pour cent, cependant l'avantage d'avoir une source de force en une machine fixe est très considérable. Je ne parle pas des machines pour les grandes voies, pour lesquelles une machine locomotive peut être très économique, et presque aussi économique que les bonnes, je ne dis pas les meilleures, mais de très bonnes machines fixes. Il en est autrement cependant pour les petites machines que seules l'on peut employer pour les tramways ; là les dépenses de combustible, les difficultés d'avoir une petite chaudière effective, et les inconvénients, causés par l'échappement de la fumée et de la vapeur dans les rues, sont tels que les tramways à vapeur n'ont jamais pû réussir ; on les a essayés de côtés et d'autres, mais ils ont toujours finalement manqué à cause de ces difficultés, et la transmission électrique paraît offrir une solution pratique.

Mon frére a peur que les petits chariots à contact ne tombent des fils suspendus. En vérité c'est arrivé au commencement de nos expériences et il a fallu construire les chariots bien soigneusement pour obvier à cet inconvénient, et pour les faire passer aisément par les courbes. Certainement la méthode employée ici, à Paris, par mon frère et M. Boistel, a des avantages considérables, mais elle est aussi plus coûteuse.

ON A DEEP SEA ELECTRICAL THERMOMETER.

By C. William Siemens, D.C.L., F.R.S.*

In the Bakerian Lecture for 1871, which I had the honour of delivering before the Royal Society,† I showed that the principle of the variation of the electrical resistance of a conductor with its

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* Excerpt Proceedings of the Royal Society, Vol. XXXIV. 1882, pp. 89–95.

† Proceedings of the Royal Society, Vol. XIX. p. 443.

temperature might be applied to the construction of a thermometer, which would be of use in cases where a mercurial thermometer is not available.

The instrument I described has since been largely used as a pyrometer for determining the temperatures of hot blasts and smelting furnaces, and Professor A. Weinhold,* using the instrument with a differential voltameter described in my paper referred to, found its indications to agree very closely with those of an air thermometer within the limits of his experiments from 100° to 1,000° Centigrade. I am not aware, however, that any results have been published of its application to measuring temperatures where a much greater degree of accuracy is required, as in the case of deep sea observations. My friend, Professor Agassiz, of Cambridge, U.S., ordered last year for the American Government an instrument designed by me for this purpose, and during the autumn it was subjected to a series of tests on board the United States Coast and Geodetic Survey steamer "Blake," by Commander Bartlett.

The apparatus consists essentially of a coil of wire T, which is lowered by means of a cable to the required depth; and is coupled by connecting wires to form one arm of a Wheatstone's bridge. The connexions of the bridge are shown in Figs. 1 and 2, Plate 23. The arm CD is the comparison coil S made of the same wire as the resistance coil T, and equal to it in resistance. This coil is immersed in a copper vessel of double sides, filled with water, and the temperature of the water is adjusted by adding iced or hot water until the bridge is balanced. The temperature of the water in the vessel is then read by a mercurial thermometer; and this will also be the temperature of the resistance coil.

To avoid the error, which would be otherwise introduced by the leads to the resistance coil, the cable was constructed of a double core of insulated copper wire, protected by twisted galvanised steel wire. One of the copper cores was connected to the arm BC of the bridge, and the other to the arm DC, and the steel wire served as the return earth connexion for both.

The resistance coil and comparison coil were made of silk-covered iron wire ·15 millim. diameter, and each about 432 Ohms

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* "Annalen der Physik und Chemie," 1873, p. 225.

resistance at a temperature of 66° Fahr. To allow the resistance coil to be readily affected by changes in the temperature of the water, it was coiled on a brass tube with both ends open, allowing a free passage to the water. Sir W. Thomson's marine galvanometer with a mirror and scale was employed to determine the balance of the bridge.

Mr. J. E. Hilgard, assistant in charge of the United States Coast and Geodetic Survey, has sent me the following results of Commander Bartlett's experiments.

The apparatus was set up on board the "Blake," at Providence, in April, 1881, but owing to there being no ice machine on board, only preliminary experiments were made until the following August.

The "Blake" sailed from Charleston on August 4th, running a line over known depths in the current of the Gulf Stream. A 60 lb. sinker used in sounding was attached to the end of the cable near the resistance coil, which was allowed to hang freely below. When well in the strength of the stream a series of temperatures were taken by the Miller-Casella thermometers on the sounding wire, and immediately after the insulated cable was lowered to the surface, and water from the surface placed around the comparison coil on deck. The temperature of the attached thermometer read the same as that determined for the surface by the thermometer attached to the hydrometer case.

Under these conditions the pencil of light from the mirror was on the zero of the scale. During the experiments the vessel was rolling from 10° to 15°, and there was a moderate breeze from south-east. The resistance coil was lowered to five fathoms below the surface, and was allowed to remain five minutes; the circuit being closed, the pencil of light remained at zero. Lowerings were then made to 10, 20, and 30 fathoms, and in each case five minutes were allowed for the resistance coil to assume the temperature of the water, and after adjusting the temperature of the water around the comparison coil, it was allowed to stand five minutes before the final reading was taken.

The rolling of the vessel affected the mirror so as to throw the light about 5° on each side of the zero point when the circuit was open, and nearly the same when closed; but as the deflection was the same on either side it was easy to determine the middle point.

While at work in the stream it was necessary to work the engine in order to keep the wire vertical. The jar of the engine, however, affected the mirror to such a degree that readings could only be taken when the engine was stopped.

The Tables I., II., III., IV. give the results of the several lowerings.

I.			II.		
Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.	Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.
	°	°		°	°
Surface	81·5	81·5	Surface	81·5	81·5
5	81·5	81·5	30	68·5	
10	76·5	76·5	50	65·25	65
20	70·25	69·5	75	60	
30	69·5	69			
30	68·75	68·75			
III.			IV.		
Surface	83·5	83·5	Surface	84·5	84·5
30	68		30	81	80
50	65·25		50	75·5	
75	60·75		75	61·75	
100	56	54			
150	51				
200	47	47	200	49·5	49·75

On August 10th the "Blake" left Hampton Roads, steaming to the eastward until reaching the meridian of 74° 31′ W., when a sounding was taken, giving a depth of 1,024 fathoms. A serial was taken to a depth of 400 fathoms with two Miller-Casella thermometers, which had been carefully compared with the standard and found to agree at different temperatures. Immediately after the serial with the thermometers the insulated cable was lowered into the sea, and the temperature, by the galvanometer and comparison coil, recorded for the same depths as taken in the first serial. Five minutes was allowed at 5 and 10 fathoms, but there was no deflection of the pencil of light. The temperature of the surface was 76°·5. Having lowered to 15 fathoms, at end of one minute the pencil of light was 9° to the left of zero on the scale. At the end of five minutes it was 22°, and at the end of ten minutes still 22°. A number of experiments were made with regard to the time necessary for the resistance coil to assume the temperature of the water. Five minutes was decided on as being necessary and sufficient, and was adopted in all succeeding lowerings.

The first lowering was to 400 fathoms, the temperature at that depth being 40°. The cable was then reeled in to 200 fathoms, when the current was made. There was found to be no deflection, the temperature of the water in the copper vessel having risen from 40° to 43°·5. This temperature agreed with that at 200 fathoms when lowering to the same depth.

During the experiments there was a light south-east breeze, and a very smooth sea. They lasted from 7·18 P.M. until 1·30 A.M., but special care was taken with every reading, and it is probable that fifteen minutes would be a fair average time for each observation with the electrical apparatus.

The results are given in the Table.

I.			II.		
Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.	Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.
Surface	76°·5	76°·5	30	54°	54°
5	76·5	76·5	50	54·25	53·5
10	76·5	76	100	50·5	50·5
15	69	68	150	46·5	46·5
20	58	58	200	43·5	43·5
30	54·25	54			
50	54·25	53·5			
75	52·5	52·5			
100	51	50·5			
150	46	46·5			
200	43·5	43·5			
300	40·5	40·5			
400	40	40			

Early on the morning of August 12th another serial to 800 fathoms was taken with the Miller-Casella thermometer, and immediately after with the electrical apparatus. Several readings were taken from the surface to 100 fathoms, and then the coil was reeled out to 800 fathoms, and the readings taken as it was drawn up.

Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.	Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.
Surface	76°	76°	Surface	77°·5	77°·5
5	76	75·25	5	76·25	75·25
10	73·5	69	10	75·5	69
15	61·25	68	15	66·5	63·5
20	55·5	59	20	58	57
30	51	52·5	30	51·5	51·5
50	53·75	52	50	54·5	53·5
75	52·5	52·5	75	53·5	52·5
100	50	49·5	100	51	49·5
			125	48·5	
			150	46·5	46
			200	43·5	43·25
			300	40·5	40·75
			400	40	39·75
			500	39·25	39
			600	38·75	38·75
			700	38·5	38·5
			800	38·5	38·5

In the last series of observations in reeling back the cable, the temperature at 50 fathoms was 54°·5, and fell to 51°·5 at 30 fathoms. Immediately after another series was taken with the Miller-Casella thermometer, and the same increase of temperature from 30 to 50 fathoms was observed. The cable was lowered three separate times to 50 fathoms, and the readings being taken both when lowering and reeling in with the following results :—

Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.	Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.
				°	°
Surface	77°·5	77°·5			
20	57·25	57	30	51·75	52
30	52·25	52	50	54·5	53·5
50	55·25	53·5	75	53	52·5
20	57·75	57			
30	52·75	52			
50	54·75	54			
75	53	52·5			

During the above experiments the sea was perfectly smooth, with no wind. The ship's engines were not used at all, the vessel lying almost motionless in the water. The temperature of the comparison coil was reduced by water from a carafe, the water contained therein being frozen by a Carré ice machine. Two carafes were prepared at a time, and there was plenty of time to keep one constantly at hand.

In order to allow the Miller-Casella thermometers to record the high temperature of 50 fathoms in the last series, they were lowered very rapidly to that depth, and after eight minutes reeled back at the rate of 200 fathoms per minute, so that the minimum side had not time to assume a lower temperature.

The cable was led from a large reel through an 18-inch leading block, and was lowered and reeled in very slowly, and without jerks.

It may be noted in the above Tables that the two instruments gave precisely the same readings at positions of maximum or minimum temperature, but that in intermediate positions the electrical thermometer, in almost every instance, gave a higher reading. This discrepancy may be accounted for, I think, by the circumstance that the electrical thermometer gives the temperature of the water actually surrounding the coil at the moment of observation, whereas the reading of the Miller-Casella instrument must be affected by the maximum or minimum temperatures encountered in its ascent or descent, which may not coincide with that at the points of stoppage. A strong argument in favour of the electrical instrument for geodetic and meteorological purposes has thus been furnished.

MISCELLANEOUS.

MISCELLANEOUS.

ON AN IMPROVED WATER METER,

By Charles William Siemens,* Mem. Inst. M.E.

The rapid growth of water-works in this and other civilized countries, extending to towns of second and third rate importance, has rendered the production of an efficient water meter a matter of considerable practical interest. The water acquires in its transmission from the source to its destination a certain value, payable by the consumer. If the consumer is a private householder, it is possible to estimate his probable consumption, supposing that no water is wasted by allowing taps to leak or to be left opened ; but calculation entirely fails to estimate the quantity of water consumed in manufactories, baths and wash-houses, &c. The consequence is, the larger proportion of the water supplied to a town is absolutely lost, and falls to the equal charge of the thrifty and wasteful.

A good water meter will not be limited in its application to the purpose of water-works ; it will be found a useful auxiliary to brewers, distillers, and liquid merchants generally ; moreover, to engineers, and indeed to all engine proprietors, it will be of essential service, by furnishing a register of the water pumped into steam boilers ; from which a correct estimate may be found of the evaporative powers of the boiler, and the relative quantity of the fuel employed, independently of the working conditions of the engine.

The meter is required to fulfil the following conditions :—

1. It must register correctly upon a counter the quantity of water passed through the meter, either at high or low speeds.

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* Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1854, pp. 3–14, 15–19.

2. It must not be affected by the pressure of a high column of water upon its working parts.

3. It must allow the water to pass through without obstructing or at intervals checking the same.

4. Its working parts must be protected against the effects of mechanical impurities or corrosive agencies in the water, so as to insure its continuous working without frequent attention.

5. It must be a cheap and compact instrument, adapting itself conveniently and locally to ordinary circumstances.

6. Its working and registering parts must be inaccessible to the employer, in order to prevent fraud.

The fulfilment of these conditions might at first sight appear but an easy problem for a skilled mechanician, but the numerous and fruitless attempts that have been made at its solution have proved the real difficulty of the task. In order to combat these difficulties successfully, it is necessary to discriminate between those that are inseparably connected with certain principles of action, and those of mere detail of arrangement, or choice of material. All meters that have hitherto been proposed may be classed under the four following heads, viz. :—

1. Cistern or Bucket Meters.
2. Piston Meters.
3. Meters by Area of Channel.
4. Meters by Impact.

The intermittent supply system which prevails in London and elsewhere, is indeed a supply by cistern or bucket meter, in its most primitive form. Each house or factory is provided with a cistern capable of holding the necessary supply for a day, or other convenient period of time. The turncock in making his regular rounds fills the cisterns, which are provided each with a separate ball-cock, to prevent their overflowing.

This mode of supply has been found quite inadequate for large and irregular consumers, but even for private houses it entails so much inconvenience and expense (principally to the consumers) that the legislature has thought fit to interfere, and now insists on a continuous supply. With the continuous supply, the liability to the water being wasted is very much increased, unless a self-registering method is applied.

Mr. Mead, of London, proposed a registering bucket meter, of very simple construction, which is represented in Fig 1, Plate 24. It consisted of a mould or double bucket A, that is divided equally by a division B, and is at liberty to rock upon a centre C. Perpendicularly above this rocking centre is the open mouth of the supply pipe D, filling alternately the one and the other bucket. At the extremities of the buckets, small pockets E E are provided, that fill at the instant the mould overflows, and being at the greatest distance from the rocking centre, cause the filled bucket to overbalance the empty one, and to discharge itself into the cistern F below. The supply of water is regulated by means of a float G, and a cock H, as will be readily understood. The rocking shaft C gives motion to a counter that is not shown, by means of a ratchet and wheel.

Mr. Parkinson, of London, has invented a bucket meter, similar in its construction to the ordinary gas meter, which is found to register the water passing through with great accuracy, and is actually used to a great extent in connection with receiving cisterns.

It would be interesting to add to the list of bucket meters, contrivances both cheap in construction and capable of measuring liquids with accuracy, were it not that these meters destroy the onward pressure of the water, and are of necessity incumbered by cisterns at elevations above the premises supplied, which cisterns entail great expense and inconvenience.

The name " Piston Meter " is intended to comprise all meters in which the fluid is measured by displacing a piston, a disc, or a diaphragm, and thereby filling a measured cavity.

The " piston meter " in this respect resembles the " bucket meter," with the advantage of transmitting the onward pressure of the water, and of dispensing with the necessity of a cistern. On the other hand it labours under great and peculiar disadvantages, partly on account of the valves and pistons which are employed being quickly destroyed by the sand and other impurities contained in the water, or broken by its impact against them, and partly on account of their great bulk and expense in proportion to the water measured.

It will only be necessary to mention a few of the multitude of piston meters that have been proposed, for the sake of illustration. Those of Lewis and Taylor, both of Manchester, and of Messrs.

Barr and Macnal, of Paisley, are examples of single cylinder meters, with tumbler arrangements to reverse the valves suddenly, in order not to check sensibly the column of water moving through the pipes. Captain Ericsson, of America, and Mr. Chrimes, of Rotherham, simultaneously proposed a meter consisting of two cylinders working on cranks at right angles to one another, in order to equalise the flow through the pipes, and to be able to apply slide valves, worked by eccentrics, in place of the more complicated tumbler arrangements. Mr. Roberts, of Manchester, constructed, in 1851, a cylinder meter, made to tumble or oscillate by the weight of the piston. Messrs. Bryan, Donkin & Co., of London, invented, in 1850, a disc meter; Mr. Parkenson, of Bury, and Messrs. Chadwick and Hanson, of Salford, have substituted india-rubber diaphragms for the piston and the disc respectively. Mr. Adamson, of Leeds, made a meter resembling the rotary engine, in which direction he has been followed by several others.

The last named meter is the only one of this class that has been practically used for several years (at Leeds), but was finally superseded, on account of excessive wear and tear, and frequent stoppages.

A meter "by area of flow" pre-supposes a constancy of pressure, and knowledge of the time of continuation of flow. It is practically resorted to for measuring approximately large volumes of water, by passing it over an overflow, and taking into account the depth of water column, its breadth, and the time of flowing.

In Paris, Genoa, and other cities on the continent, the water has for many years been supplied to each individual consumer, through very contracted jets at the extremities of the pipes, through which the water continually issues with supposed uniformity of speed into receiving cisterns. It is evident that this mode of supply is fraught with all the inconvenience of the intermittent system, without possessing the advantage of relieving occasionally the supply pipes from pressure, for the purpose of repairs, &c.

The great inconvenience of this system is illustrated by the fact, that many houses in Paris require upwards of ten cisterns for the supply of the different inmates. It is unjust, for it obliges every consumer to pay at a maximum rate.

Several years since (in 1845), the writer of the present paper imagined a meter by area of channel, which dispensed with the necessity of a cistern, and registered the quantity of water actually passed through. It is shown in Fig. 2, Plate 24, and consisted of a piece of square pipe A, containing a common flat valve B, which the water has to raise in order to pass through. The spindle of this valve passes through a stuffing-box, and carries the lever C, which by its motion raises or lowers a driving strap D, upon the reversed cones E F. The cone E receives a regular motion by means of a clockwork G, while the cone F communicates the motion received through the strap to a counter at H, with a dial plate I; if no water passes through, the valve B rests at the bottom, and the clockwork is entirely stopped by means of a detent K; the instant the valve B is raised by the passage of water, the clockwork is released, and imparts a very slow motion to the counter: but in proportion as the flow increases, the strap rises, and the motion of the counter is increased. A correct registration is thus obtained, provided the elevation of the flap-valve is proportionate to the amount of water passing through, which is practically the case, since the constant weight of the valve itself renders the velocity of flow under its edge constant.

A meter differing only in the details from the above has recently been brought out by Mr. Kennedy, of Kilmarnock.

The frequent necessity for winding up the clock movement rendered this meter evidently unfit for general application. To obviate this, the writer thought of abstracting the motive power for the clock from the water itself, by introducing a screw propeller into the pipe.

Being advanced thus far, it became apparent that the valve and clockwork might be entirely dispensed with, if the propeller could be made to rotate in the precise ratio of the moving column of water, and to impart that motion directly to the counter.

Thus the first step was made toward the production of a "Meter by Impact," by which it is contended the conditions above enumerated of a perfect meter are most fully realized.

The writer considers it an essential condition of a "Meter by Impact," that the propelled vanes merely glide edgeways through the water, by partaking fully of its onward motion, without sensibly impeding or agitating the same.

These conditions are most fully complied with, by a perfect screw suspended on two pivots, in the axis of the moving column of water. They are also fulfilled by a Barker's-mill, or turbine of spiral blades, that yield to the motion of the water outward from a centre.

If, on the other hand, the vanes of the propeller are of irregular shape, so as to form eddies or obstructions in the water, it will be theoretically impossible to insure a uniform increasing rate of rotation with increased velocity of current; for the retarding or accelerating effects produced by eddies or concussions increase not in the simple, but in the square ratio with the velocity.

The correctness of this argument was proved indirectly, and unknown to the writer, by the failure of an attempt made at about the period referred to, by Mr. Abraham, to register the water flowing through a pipe by means of a screw propeller of irregular form, although suspended with great care between points of agate.

The same unsatisfactory result was obtained some years later by Mr. Tebay, of London, who formed his propeller by making radial incisions into a disc of brass plate, mounted upon a spindle, and by twisting each segment in the same manner, like the vanes of a windmill. He endeavoured to counteract the inaccuracy of his propeller, by introducing valves so contrived that the water should be able to pass only at a fixed velocity.

In order to obtain correct measurement by an "impact meter," it is not sufficient that the propeller should yield equally in all its parts to the motion of the water, but it must also possess the power to overcome a uniform resistance by friction in its bearings, &c., without diminishing its proportionate rate of rotation at low speeds.

The apprehension of these difficulties deterred the writer, for several years, from proceeding, until the pressing want for a meter to carry out some other improvements induced him to construct, in 1850, the identical meter now before the meeting; and which, in point of accuracy of measurement and compactness, fully satisfied a committee of inquiry of the Manchester Corporation Water Works, by whom its adoption was recommended. The successful results obtained by this meter, which the writer had not even an opportunity to adjust previous to its official trial, were thought strong proofs in favour of the principle involved. He was indebted for the admirable first execution of his idea, and some valuable suggestions, to his brothers at Berlin.

In attempting, however, to put the meter into regular service, under a working pressure of upwards of 200 feet column of water, subject to violent concussions, and acted upon by mechanical as well as chemical impurities in the water, he, and the spirited manufacturers, Messrs. Guest and Chrimes, of Rotherham, had had to encounter many serious difficulties, which had to be dealt with one after another, but which finally determined them to adopt for smaller meters the more simple arrangement of a spiral curve, or Barker's mill.

The two arrangements now actually adopted are shown in Plates 25, 26, and 27. Plate 25 shows a double screw, or balance meter capable of measuring 100,000 gallons per hour, or above two million gallons per day.

Fig. 3, Plate 25, is a sectional elevation of the meter; and Fig. 4, Plate 26, is a transverse section through one of the screw propellers.

This meter consists of a cylindrical casing A, which is lined throughout with a brass tube drawn to a precise gauge, and is connected by its flanges B B to a line of piping of 8 or 9 inches in diameter.

The measuring apparatus contained in this casing consists of two hollow drums E E, carrying on their circumference, the one a set of right-handed, and the other a set of left-handed screw blades; of the conical blocks H H, armed with radial projections or guide-blades K K; a central bracket L, containing support for the bevel wheels N N, on upright spindles, and the wheels M M, on the horizontal spindles of the screw drums; also two double inverted cones at contractions R R, and a grating P, at one end only.

The spindle of the wheel N passes upward through the hollow arm of the central bracket L, into a close chamber F, carrying an endless screw U, which is geared to a pair of reducing wheels V V.

The spindle of the latter wheel is ground air-tight into a socket of the strong metallic plate T, and passes into the upper chamber

G, carrying a pinion X, which is geared into two wheels Y and Z, of equal diameters, but the former with 101, and the latter with 100 teeth. The wheel of 101 teeth carries a large dial plate O, divided in its circumference into 100 equal divisions; and the wheel of 100 teeth is fixed upon the upright spindle, and carries a hand Q, upon the dial. The dial in travelling through the breadth of one division under a fixed pointer is intended to indicate the passage of 100 gallons through the meter. For every one complete revolution of the dial, the hand advances relatively through the breadth of one division, signifying the passage of 100,000 gallons. The millions of gallons are indicated on a separate circle of divisions on the large dial, by a hand R, which receives a reduced motion by a wheel S of 100 teeth rotating bodily with the dial, gearing into a pinion of 10 teeth, fixed to the upright spindle.

The dial face is exposed to view through the cover of plate glass I.

The water enters the meter through the grating P. which is provided to arrest large solid bodies that might obstruct the working of the meter. The inverted cone R directs the current of water toward the centre, where it again spreads over the conical block H, and being directed parallel to the axis between the guide vanes K it impinges obliquely upon the right-handed vanes of the hollow screw drum E. The object of (figuratively speaking) kneading the current of water between the conical surfaces, is to destroy partial currents within the same, and in spreading it from the axis to increase its leverage on the rotating drum; the diameter of the body of the drum is made slightly smaller than the diameter of the conical block, in order to prevent the former from endway pressure of the moving column of water. Some clearance is allowed between the helical vanes and the surrounding casing, but the passage of water outside the vanes is effectually prevented by slight contractions of the water way at both ends. In order to prevent wear and friction on the bearings, the body of the revolving drum is made hollow to such an extent that the water displaced nearly balances the weight of metal. A screw drum of this description moves with a very gentle current of water, but it would, nevertheless, make a very imperfect meter if it were simply connected to the counter, inasmuch as the friction in the bearings and of the counter would retard it most at low speeds, and the friction of the vanes in gliding through the water (which increases in the ratio of the square of the velocity) would again greatly retard it at high speeds, the maximum rate of measurement being obtained at a medium speed.

By the addition of the second, or left-handed drum, these variations in speed are, however, very perfectly compensated. For the sake of illustration, let it be imagined that both screw drums revolve independently of each other (of course in opposite directions), and that the second or left-handed one alone imparts its motion to the dial ; let it also be supposed that the friction of both drums is the same ; the water, in meeting the oblique vanes of the first drum in a direction parallel to the axis, will be deflected from its straight course proportionally to the resistance to rotation of the drum, say an angle of 1°, as shown at A in Fig. 5, Plate 26. Pursuing its fresh course, it will strike the left-handed screw blades of the registering drum at B, in an angle at 1° more obtuse than the previous, and being deflected by the resistance offered through 1° in the opposite direction, it follows that the water passes out in a direction parallel to the axis, as at C, and, consequently, that a true rate of measurement is obtained. The condition of both drums being perfectly alike, it follows that the same compensation must be effected at all speeds. Nor is this compensating effect disturbed by coupling both drums rigidly together by bevil gearing, whereby a great practical advantage is obtained, that, namely, of one drum assisting powerfully to overcome an obstruction offered to the other. Let it be imagined, for instance, that a pebble or piece of vegetable matter has wedged itself between the casing and tip of the vane of the first drum, so as to stop it entirely, and to force the column of water passing through into the helical course ; the water would then impinge upon the left-handed vanes of the second drum rectangularly (supposing the inclination of the reverse vanes to be at an angle of 45° to the axis), and expend its entire momentum upon it, the effect of which would be added to the impact on the first drum through the bevil gearing, to overcome the obstruction. The motion is conveyed to the counter by the upper bevil wheel N, but the opposite wheel N is added to strengthen the connection between the two drums, and to relieve all the spindles from pressure. Before leaving the meter, the current of water is again contracted between conical surfaces, for the same purpose as before, namely, to equalize its flow.

In calculating the quantity of water that will effect one complete revolution of the screw-drums, it is necessary to compute the clear net area between them and the external casing, supposing all the surfaces to be covered with a film of stationary water (by adhesion) $\frac{1}{140}$th part of an inch in thickness, and to multiply the same by the pitch of the screw. The correction for adhesion amounts to an inappreciable quantity for large meters, but constitutes a considerable percentage in the calculation for small meters, being equally exact for both.

The difficulties that have been encountered in the manufacture of this meter apply principally to the spindles; although relieved from all constant pressure, they have nevertheless to maintain the drums in their central position, and to resist a strain endways, caused by the mere friction of the water in passing along the vanes. They have in consequence to be made of hard metal; German-silver was chosen in the first instance, but could not be depended upon for strength. Steel is the best for hardness, but is soon corroded by the water, notwithstanding all attempts to protect it by zinc, or by a casing of brass, through which only the rounded point projected to receive the end strain at the bottom of the bearing. Agate points or plates are rapidly ground away when used in water. A hard bronze was found to be the most suitable metal, and indeed answers well for meters of large size, but it is difficult to produce the spindles for small meters of that metal.

The difficulty at first experienced of producing screw-drums of correct shape and uniform size, without incurring a large amount of workmanship, was successfully removed by casting them, and many other parts, in metallic moulds. Gutta-percha was also tried by the manufacturers, which, being slightly lighter than water, was with its spindle exactly equal to the weight of water which it displaced; but it could not be made sufficiently correct and rigid in the vanes. After some time the manufacturers succeeded in casting drums for the larger meters of bronze, and in dry sand, with great accuracy. There was considerable difficulty at first in finding workmen who would fit the essential parts with the great accuracy they required, without refining on other parts that have only to be strong, to resist the rough usage to which the meters are subjected when taken in use.

The calcareous matter in water deposits only on the surfaces of brass that are not exposed to the current. It exercises therefore no effect on the measuring surface, but if allowed to penetrate into the chamber of the counter it incrusts the small wheels and spindles, and causes them to break or wear rapidly. To alleviate this, the first chamber F is separated completely from the interior of the meter, excepting the capillary space between the upright spindle and its bearing, through which the pressure in the pipes is transferred to the chamber, but which is too narrow to allow of an intermixture of fluids. This chamber is filled, before it leaves the manufactory, with pure olive oil, which affords a complete and continuous protection to the reducing wheels. The upper chamber of the counter is not under the pressure of the water, and contains atmospheric air. The differential motion between the wheels Y and Z, of 101 and 100 teeth respectively, produces a reduction of 100 to 1, or 100,000 to 1000, indicated upon a single circle of divisions, whereby the use of the meter is much facilitated.

For meters of less than two-inch diameter of supply pipe, the spiral form of propeller, or Barker's-mill arrangement, is adapted, except in cases where the water acts impulsively, as for instance, in supplying steam boilers by means of pumps, where the double screw meter is the only one applicable. Fig. 7, Plate 27, is a sectional representation of a spiral meter, intended for a half-inch supply pipe.

The water enters the meter through the pipe N, and traversing a cylindrical grating H, covered with wire gauze, it passes downward through the funnel K, into the propeller E, and issuing from two apertures of its circumference it passes into the chamber P, and thence into the exit pipe G.

The propeller is formed of two discs of metal, which are bulged upward, the upper one to form a funnel, fitting loosely over the inlet K, and the lower one to join to an upright spindle I. The two discs are joined by two spiral blades, as shown in plan in Fig. 6, Plate 26. At the bottom of the propeller a chamber C is formed, that is filled with oil through apertures O and Q, and sealed close, leaving only an eye in the centre through which an upright stud of bronze B enters, which with its steel point abuts against a steel plate in the bottom of the propeller. The lower chamber F of the counter is formed of a white metal casting, cast in one piece with the grating H, and filled completely with oil. The arrangement of the counter itself is precisely similar to that before described.

Theoretically speaking, this meter is less perfect than the compensation screw meter, but it possesses the great advantage of containing only a single bearing, at C, that is at all liable to wear, and that bearing is effectually protected from the action of the water. The practical effect of this simplification of parts has been, that of 150 meters of this description that are at work, not one has as yet been returned disabled or inaccurate.

Mr. Adamson, of Leeds, has lately projected a meter with two sets of spiral blades, upon the principle of a turbine, the inner set being stationary, and the outer set revolving; this meter also gives a very good result.

Another kind of meter lately brought out by Mr. Taylor, of Manchester, having a revolving horizontal drum or water-wheel, acts partially by jet and partially by impact, but on this account it appears to the writer imperfect in principle.

It has been argued before, that no accurate measurement can be effected by the application of jets. To avoid them in the spiral meter, it is essential to make the area of the outlet larger than the area of the supply pipe. Nevertheless the nature of a jet still manifests itself to some extent by increasing the rate of the meter at high velocities. This defect has however been effectually counteracted by the application of rotating flies or drag boards L L, which offer a resistance increasing as the square of the velocity, and can be regulated to equal the effect obtained by the jet. They offer also great facility in adjusting the absolute measurement of the meter.

In order to insure the efficiency of each meter, it is necessary to test the same under variable pressures, and with considerable volumes of water. To this point the manufacturers, Messrs. Guest and Chrimes, have devoted great attention. The apparatus they employ consists of a large cistern, 40 feet high, and a second cistern below, capable of containing 1000 gallons, and accurately graduated throughout. A set of pipes is provided that have been proved to transmit given quantities of water per minute, under the pressure from the upper cistern. From 8 to 12 meters to be tested are coupled in a line, one behind another, to a pipe leading from the upper cistern to the outlet of the meters; the test pipes are then alternately connected, a uniform quantity of water, as shown in the cistern, is passed through each pipe, and the number of gallons indicated on the different counters are noted in a book opposite to the permanent number of the respective meters. An extract from this book shows how nearly correct a measurement is obtained.

MR. SIEMENS exhibited specimens of his meters of the two kinds of construction described in the paper, with specimens of the castings for the spiral drums, &c.; also the first meter he had constructeed on that principle.

He remarked that the mode adopted of insulating all the wheelwork in oil, was a point of great importance practically in water-meters, as wheels working in water were subjected to a deposit taking place upon them, increasing their friction and causing them to wear out; and it was an essential qualification for a good meter, that it should continue in constant action for a very long period without perceptible wear or inaccuracy. The upper chamber filled with oil was found to answer the purpose quite satisfactorily; the oil being lighter kept always in its place, and could not be displaced by the water; the spindle passing from the oil chamber was ground in with a slightly conical shoulder.

The Chairman observed that there was great ingenuity shown both in the principle and the construction of the meter. He inquired how many of the meters there were at work?

Mr. Siemens replied that there were 200 or 300 of the screw meters at work, of very different sizes and pressures, and about 200 of the small meters on the Barker's-mill principle.

In reply to remarks by various speakers Mr. Siemens observed that the body of the meter was made larger in the area of passage than the outlet, and therefore the velocity of the current was slower through the spiral vanes of the meter than anywhere else; and there would be no practical difference of pressure between the two ends of the meter, because there was a free communication always open through the meter of larger area than the orifice of the pipe through which the water was flowing away. He said that the practical uniformity of measure was shown by the table of trials of the meters, the limit of error allowed being about 2 per cent.; they were made to register a little too much at the lowest speed, which was effected by increasing the drag-vanes beyond what was strictly necessary to counteract the tendency of the orifices in the propeller to form joints; it being manifest that the resistance of the drag-vanes, like the force of the jets, would increase as the square of the velocity.

The *Chairman* inquired whether the meters had been employed in regular use for both high and low pressures, and whether they were found to register correctly in both cases?

Mr. Siemens replied that many of them were at work under both circumstances, and no difference had been found in their measuring from being worked under different pressures from 300 feet to 1 foot head of water.

There was always a grating fixed which prevented the entrance of anything into the meter that would be liable to interfere with its action; and the smaller size of meters had a tubular grating (a specimen of which was shown), giving a surface of grating much more extended in proportion, which could be easily got at to remove the deposits whenever they had accumulated sufficiently to obstruct the water.

As to the expense, he thought for the smallest class of houses, consuming only 100 gallons per day, Parkinson's meter would be the cheapest, if the necessity for a cistern and the ascending supply-pipe which it involved were not taken into account; but water meters were scarcely applicable to cases of such small supply. The smallest size made, ½ inch bore, would supply 300 gallons per hour, and cost £3 10*s.*,—a 1 inch meter, for 1200 gallons per hour, £5 5*s.*,—and a large 10 inch meter, to deliver 100,000 gallons per hour, cost £50 or £60.

The amount of pressure was balanced in every part, as the upper oil vessel was also under the pressure; and the extent of reduction of the motion was so great, that no perceptible effect of friction could arise on account of the great leverage, the drums in the water having 20,000 revolutions for one of the index. A great difficulty was experienced with meters having counters working in water, although they might perform very correctly when tested in the shop, and for some time after being fixed; he had always found they became incrusted sooner or later, according to the peculiarities of the water, interfering with the accuracy of working; although brass remained clean much longer than iron, and the deposit was found to take place much less upon the parts in motion than upon those at rest.

He thought there would be a source of inaccuracy in the use of two modes of delivery of the water at different times, the small jet for the slowest velocities, and the full width of orifice for the other cases, as the force of impulse in a small jet was more in the proportion of the square of the velocity, so that a double velocity of jet would drive the drum three or four times faster instead of only twice as fast, which would be required for correct measurement of the stream of water issuing; also the indirect action of the stream on the circumference of the revolving drum, being partly by impulse and partly by friction, gave too uncertain a moving force to form a correct principle of measurement. From his experience he did not think that wheelwork could be kept in correct working order for a long time if exposed to ordinary water, and this difficulty would apply more strongly to any self-acting adjusting valve at the inlet orifice to be opened by the current of water, and regulate the area of discharge upon the drum.

ON AN IMPROVED WATER METER.

By Mr. C. William Siemens.*

In January, 1854, the writer communicated to this Institution a paper on an improved water meter, in which he described several mechanical arrangements, by which he had succeeded in measuring

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* Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1856, pp. 113–120 and 123.

water flowing through pipes, with a sufficient degree of accuracy for practical purposes, and without destroying the pressure or head of water column. In the course of considerable experience with these meters, several important improvements have suggested themselves, and opportunities have occurred of observing the public importance of supplying water by meters, which the writer thinks may not be without interest to the members of this Institution.

The chief difficulty that presented itself in endeavouring to produce a practically perfect high pressure meter was not so much to obtain a correct measurement under varying circumstances of pressure, as to render the instrument sufficiently durable to resist for years the action of the water and of the impurities carried along with it. It was found necessary to protect all the working parts against the chemical action of the water, to prevent deposit of calcareous matter upon the measuring apparatus, and to combine strength with lightness as far as possible in the construction of the movable parts, in order that they might resist the force of a high water column, and might yet be moved by the slender stream produced by a leaky tap, which in the case of the smaller meters may not exceed half a pint of water passing through per minute. Cheapness and compactness of construction were other important considerations not to be lost sight of.

The improved meter, as at present manufactured by Messrs. Guest and Chrimes, is represented one quarter full size in Plate 28, Figs. 1, 2 and 3.

The meter consists of a cast-iron casing A, Fig. 1, divided by a partition into two compartments B and C. The water entering the compartment B through the pipe D passes through a spout E into the revolving drum F. The drum F, shown in the perspective view, Fig. 2, and the plan, Fig. 3, is formed of two stamped disks of brass plate riveted and soldered together face to face, each part containing similar spiral grooves or corrugations forming channels for the water to pass from the centre to the circumference. The foot of the spindle G forms with the lower portion of the drum a chamber H, into which enters a fixed stud J. The point of the stud is of hard steel, and works in contact with a bit of hardened steel let into the bottom of the spindle G, forming a support for the drum F. The chamber H is filled with oil to protect the bearing from the action of the water, and the oil being the lighter fluid cannot be displaced by the water. The drum F carries three or more flat blades KK, intended to produce a resistance in the water increasing as the square of the velocity of revolution, the effect of which is that the drum, which has a tendency to revolve at a rate increasing more rapidly than the velocity of the water, is caused to rotate at a speed proportionate to the quantity of water passing through, whether at a high or low velocity.

The water having issued into the chamber C passes away by the pipe L to the point of delivery. The spindle G passes upward into the chamber M, which contains wheel work to reduce the motion communicated by the drum and is also filled with oil. A small spindle passes finally at a greatly reduced speed through a stuffing box into the upper chamber N, containing the dial on which the quantity of water that has passed through the meter is indicated by hands in gallons or cubic feet. The details of the counting apparatus have been described in the former paper. The cup or dish forming the chamber M is made of stamped brass and corrugated, in order to yield to concussions from the water.

Before entering the meter, the water has to pass through a grating O, which arrests any solid matter, and is made easily accessible for the purpose of removing from time to time the impurities that have collected, when it is found that the passage of the water is obstructed.

It is important to make the area of the inlet E nearly equal to the collective area of the outlets of the drum F, but a little smaller than the latter. If the area of the inlet were made larger than that of the outlets, there would be a greater pressure within the drum than in the surrounding chamber C, and some water would escape unmeasured between the neck of the drum and the outside of the inlet E; on the other hand, if the area of the inlet were made considerably smaller than that of the outlets, a leakage would take place from the chamber C into the drum, because the water passing through E would act in the manner of a blast or as in Mr. James Thompson's jet pump. The area of the inlet should accordingly be for the smaller meters 10 per cent., and for the larger 5 per cent. less than that of the outlets, to allow for loss of velocity by friction in the drum. This loss may be taken to represent with tolerable accuracy the degree of obstruction opposed by the meter to the moving water column. The rapid current of the water through the inlet E and the curvilinear channels of the drum has been found to prevent deposit of calcareous matter in these places, which is an important point, for were it otherwise, the meter would gain in relative speed in proportion as the area of the channels was diminished.

Meters constructed on this plan have now been found to work continuously for nearly three years under the most varied circumstances without requiring any alteration whatever. The arrangement made between the manufacturers and the water companies or purchasers of the meters is that every meter that fails to give satisfaction, in consequence of stoppage or inaccuracy of measurement, shall be exchanged; and experience shows that the number of meters so returned does not exceed $2\frac{1}{4}$ per cent. in the year, and these for the most part have been sent back only from trivial causes. The more serious accidents have been that the meter has become choked with gravel or other impurities, that had entered through a broken grating; or that the regulating vanes have been broken, and the relative velocity of the revolving drum has been much increased; or that some derangement in the wheel work of the counting apparatus has taken place. In winter it has happened that the casing of the meter has been burst by frost, but this class of accidents does not concern the mechanical arrangements of the meter. The manufacturers enter into contracts to maintain the meters supplied by them in good working condition for a term of years, in consideration of the moderate annual charge of 5 per cent. per annum on the first cost, proving thereby their own confidence in the durability of the meters.

A further object of this paper is to prove from actual experience the utility of the meter to water companies and water consumers, and to engineers and others for general purposes.

Although the meter has been as yet but partially applied by water companies as the arbiter between themselves and their irregular or trade customers, the advantages to the companies from prevention of waste, error, and fraud, have been made manifest. The following Table gives the results of the application of fifteen meters, showing the difference between the rate paid previous to their application and the established value of the water actually supplied according to the meters :—

Number of Reference.	Rate Paid.	Value of Water Consumed.
	£	£
1	40	1050
2	40	400
3	50	450
4	40	78
5	35	64
6	18	500
7	365	500
8	15	45
9	7	21
10	7	21
11	7	21
12	20	95
13	12	110
14	13	115
15	16	700
Total . .	£685	£4170

It appears from this Table that the collective rates paid by 15 consumers amounted to £685, whereas according to the established value of the water they ought to have paid £4170, or more than six times the amount. These are no doubt exceptional cases, which have come particularly under the notice of the manufacturers because the correctness of the meters was disputed by the consumers ; but they show the utter impossibility of estimating the quantity of water supplied by a given pipe without the application of a meter. In several of the cases stated in the Table, the consumers themselves applied for the meter, because they thought the rate they paid was excessive. They calculated no doubt correctly the water actually required for their manufacturing operations, but did not take into account the lavish waste that is continually going on by taps leaking or left open, by broken pipes, and by inundating instead of washing floors and utensils, &c. From all the information the writer has been able to collect, he ventures to affirm that fully one half of all the water supplied by the permanent supply system, which at present is made compulsory by Act of Parliament, is absolutely wasted, without utility either to the consumers or to the water companies. It cannot even be said that the water thus wasted is useful in a sanitary point of view, by cleansing the sewers, because the deposit contained in the sewers can be removed only by flushing them from time to time.

The value of the water that is so wasted may be estimated from the fact that one water company alone, the East London, sells at present nearly 800 millions of gallons a year by meter, which at the price of 6*d.* per 1,000 amounts to a rental of £20,000 per annum. They employ for this purpose only about 200 meters, which are however of more than the average dimensions.

In order to detect and prevent all waste of water, it would be necessary to apply a meter not only to the branch pipe of every irregular consumer, but also to every branch main supplying a district or a street. The legitimate consumption of each district or street would then be soon ascertained, and if in any one week it exceeded that amount, the meter would at once draw attention to the fact, the cause of which would frequently be found to be a leakage from the branch main underground into the sewers, which it is at present impossible to detect.

In order to render the system of supply by meter perfect, it should be extended also to private houses. Objection has been raised against this proposition, on the ground that the poorer housekeepers would economize water with detriment to their own sanitary condition, and also that the cost of the meter is too high in proportion to the amount of rent they pay. These objections are applicable however only to the case of labourers' cottages, which indeed might be supplied without restriction, or might be charged a fixed rate till their consumption exceeded a certain maximum. It should, however, be borne in mind that the principal value of meters to water companies consists in the prevention of waste; and it is a question open for discussion, whether the waste going on in houses on the permanent supply system does not far exceed the cost of maintenance and investment of a meter, which indeed would not be more than the cost of the present cistern and ball taps.

A system of supply by meters would relieve the officers of the water companies from much watchful care and unpleasant discussion with the customers about the quantity of water they consume. The advantages derived by consumers from being supplied by meters are,—first, that each consumer pays only for the water actually used by him, whereas at present he must pay also his share of all the waste that is going on :—secondly, the meter is useful to the customer for regulating the distribution of water on his own premises, and for preventing waste by his own servants ;—and thirdly, the general prevention of waste will enable water companies to reduce their charges.

There are many other useful applications of an efficient, cheap, and compact water meter, one of the most important of which is the application to steam boilers. By inserting a meter into the suction pipe of the feed pump, a correct indication is obtained of the water actually evaporated, which serves as a check on the one hand upon the performance of the engine, and on the other hand upon the quality of the fuel employed, or the care of the fireman in burning it.

There are at present upwards of 2,000 of these meters in constant use at several large towns in this country, including London, Bristol, Edinburgh, Newcastle, Yarmouth, and Leeds ; and also upon the continent, at Berlin, Amsterdam, and elsewhere.

The sizes of the meters vary from ½ inch to 12 inches diameter of supply pipe ; and excepting the comparatively few cases of defective meters above alluded to, which, according to a careful register kept by the manufacturers, have amounted to not more than 33 cases in a year out of about 1,500, the results have been highly satisfactory as regards both the correctness of measurement and the durability of the meter.

Mr. Siemens exhibited several meters of different sizes, and specimens of the revolving drums, from the smallest size, with a drum 1½ inches diameter, intended for a pipe ½ inch diameter, delivering 5 gallons per minute, up to one of the largest sizes, with a drum 8 inches diameter, intended for a main 10 inches diameter, delivering 500 gallons per minute. A ½ inch meter was also exhibited, and shown in operation to the meeting at various rates of discharge, having a glass casing allowing the motion of the rotating drum to be seen while in action.

In answer to remarks by various speakers,

Mr. Siemens replied that the tendency of the drum to overrun, when the discharge was stopped, was now successfully prevented by the retarding vanes fixed upon it, by the resistance of which in the stationary water it was speedily brought to rest. It had been ascertained by experiment, that when the drum was rotating at 2000 revolutions per minute, only 5 or 6 revolutions were made by it after the discharge had been suddenly stopped; and the little error arising from this cause was nearly compensated for by the effect produced by the inertia of the drum at the commencement of motion, a small quantity of water having to pass through the drum not registered, before the drum had attained the velocity corresponding with the velocity of the water through the meter. As an extreme test, he might mention an instance in which a meter had been used to measure the water supplied to an engine boiler, and had been placed between the feed pump and the boiler, and consequently the drum was set in motion and stopped at each stroke of the pump, and went with jerks; but even under this severe trial the meter was found to register only about 5 per cent. in excess. In such a case it would be necessary to provide an air vessel to equalize the flow through the meter; but even without such an addition the amount of loss at each stoppage was evidently very small.

For lubrication common oil was not suitable, but any oil might be used that was not acid, so as not to act upon the brasswork; pure olive oil and neat's foot oil answered well, or the oil extracted from peat. As a further precaution against corrosion the brasswork was tinned, and the success of the result was shown in the meter exhibited by Mr. Bell, which he had not seen before, and in which there was scarcely any wear after two years' work. This result might appear remarkable, considering the high velocity of the drum, but he thought that the higher the velocity the less wear there would be upon the bearings, because the drum would spin round in the manner of a top or gyroscope and require in that state no lateral support whatever.

In the discussion of the Paper

"ON THE RESULTS OF THE USE OF CLAY RETORTS FOR GAS MAKING,"[*] by Mr. J. Church,

Mr. C. W. Siemens said, that although he would not attempt to point out the *rationale* of the fact, that the leakage increased in the direct proportion of the pressure, yet he thought the experiments of Professor Graham, upon the transpiration of gases, bore somewhat upon this interesting question. Those experiments went to show, that when gases issued through narrow tubes they did not transpire, or issue in proportion to the law of gravitation, but according to some totally different law, which had not as yet been clearly laid down. If an explanation might be attempted, he should say, that the gas, in issuing through very small capillary spaces, was so much checked in its progress, that its inertia was destroyed at every step, and it was only the excessive friction which retarded it virtually. But if it was merely the friction that it had to overcome, it would be evident that the leakage would be in proportion to the pressure applied. This argument would apply in a greater degree with regard to gas than atmospheric air, because the specific gravity of coal-gas was only 0·5 at its ordinary temperature ; but when it was heated to 800° in the gas retort it would not weigh half as much. The inertia of the gas was, therefore, exceedingly small ; but being in a highly elastic state, the friction was, on the contrary, considerable. Observations had been made on the greater quantity of gas produced from clay retorts than from iron retorts. Although he had no practical experience in this matter, he would suggest, whether in those cases where the greater quantity of gas was obtained, the exhauster had not been worked to a great extent ; so that the leakage through the walls of the retort was from the exterior to the interior or into the retort.

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[*] Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XVI. Session 1856–7, p. 320.

In the discussion of the Paper

"ON THE PROGRESSIVE APPLICATION OF MACHINERY TO MINING PURPOSES,"

By T. J. Taylor,

Mr. C. W. Siemens * remarked that, in reference to the comparison between the beam pumping engines and direct acting engines, there seemed to be some ambiguity as to the power required to put the weight of the heavy beam in motion; the only loss of power arising from the weight of the beam would be the extra friction caused by the increased pressure on the rubbing surface of the beam gudgeons, for all the extra power required for putting the heavier mass into motion in the first portion of the stroke was returned again by dragging the beam forward in the latter portion of the stroke whilst the propelling power of the steam was diminishing.

A force proportionate to the inertia of the beam would be required to set it first in motion; but if it were supposed to be placed between two springs resisting its motion equally on each side of the central position, then the force originally imparted to the beam in starting it into motion would be spent in the compression of the opposite spring, and would be all returned again by the recoil of the spring if of perfect elasticity; and the beam would be propelled back to its first position with the same velocity as before, causing the similar compression and recoil of the other spring. The beam would thus continue to oscillate backwards and forwards like a pendulum, however heavy it might be, without any further power being required beyond what was necessary to overcome the friction of the bearings.

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* Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1859, p. 40.

In the discussion of the Paper

"ON THE CONSTRUCTION OF ARTILLERY, AND OTHER VESSELS, TO RESIST GREAT INTERNAL PRESSURE," by JAMES ATKINSON LONGRIDGE, M.Inst.C.E.,

MR. C. W. SIEMENS * said, many years ago he had some slight practical experience in the use of guns, and had watched, with great interest, the progress which had since been made in their construction. Addressing himself to the subject of the paper, it had been objected, that a gun constructed upon the plan proposed by the author, would not have sufficient longitudinal strength. It had occurred to him, that the longitudinal strength of the gun might be much increased, if instead of winding wire upon it, it was bound with corrugated bands of steel, put on spirally. He estimated, that two-thirds of the whole tensile strength of these bands would thus be made available for longitudinal strength. He proposed, that the core of the gun should be turned with spiral grooves, extending backward beyond the bore, and fitting the longitudinal ribs, or corrugation of the strips. The strips should be put on under varying tension, while the gun rotated in a bath of solder, in order to unite the several layers. He thought the core of the gun ought to be of equally hard and tough material, and he had no doubt, that the most serviceable gun would be one made of solid, but mild, cast steel, well solidified by hammering. Such guns were manufactured by Mr. Krupp, of Essen. From a report made to the Prussian Government by Colonel Orges, it appeared, that the German cast-steel gun had given the most satisfactory results, as regarded strength. A bar of 1 inch square of this material had borne a weight of 50 tons, whereas a bar of wrought iron of the same dimensions broke with 33 tons. Mr. Krupp's gun bore five and a half times the internal pressure of an ordinary cast-iron gun of the same internal and external diameters, and three times the internal pressure which burst a bronze cylinder of the same dimensions. Mr. Krupp was now making three hundred

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XIX. Session 1859–60, pp. 378–380.

guns for the Prussian Government. The weight of his 12-pounder breech-loading gun was 825 lbs. The cost of the forging was about £93, and that of the gun complete was £150. These figures would enable a comparison to be made with the cost of guns of other constructions. With regard to composite guns, he would suggest, that although they might possess greater strength against internal pressure, than a gun of homogeneous metal, yet such a gun would be more liable to injury, when hit by a hostile shot. A composite gun would, he thought, suffer more from that cause, than a gun of homogeneous metal, which would only be indented by a shot; whereas the composite gun would probably be disabled.

Hitherto comparatively few experiments had been made, to determine what the pressure upon the interior of a gun really was in firing, and also what was the resistance of the atmosphere to shot at different velocities. In 1846 his brother, Mr. Werner Siemens, suggested a plan to a Commission appointed by the Prussian Government, the results of which had been published. He determined the velocity of the shot by making it pass insulated wires, in connection with a Leyden jar. The electrical discharge passing through the shot, caused a spark to go from a point upon the polished surface of a steel cylinder revolving at high velocity, causing it to be marked by a speck of burnt metal. The shot in striking other wires, at a given distance, would make another speck upon the polished steel cylinder, and the angular distance between those two points would represent the time that was occupied by the ball in passing from the one place to the other. The results that had been obtained by this apparatus, were, however, not quite satisfactory; and it had occurred to Mr. C. W. Siemens, that in order accurately to ascertain the forces acting in a gun, and also the resistance of the atmosphere to the passage of the projectile, an apparatus of a more simple nature might be constructed, which should record those facts, in the same way that the exact pressure of steam in a steam cylinder, at every portion of the stroke, was arrived at. His object was, in fact, to indicate the forces acting upon the projectile throughout its flight. For this purpose he proposed to employ a hollow shot with open ends, closed by strong doubly-dished steel plates, laid one upon another, with lead plates between. When the shot was fired, the gases of the powder would act upon the end diaphragm, the pressure upon which would in fact urge the shot along. It was important to reduce the inertia of the elastic medium to the lowest possible amount, in order that it might instantly obey a change of pressure. The motion of the centre of the diaphragm was imparted to a scribing point in contact with a disc, made to rotate, during the flight, with a given velocity. It appeared difficult at first sight to obtain a uniform velocity of this disc without clockwork, which was evidently inadmissible ; but an arrangement had occurred to him, by which he expected to effect that purpose. He fixed upon the disc two small fuses, or rockets, acting in opposite directions. If both these rockets were made of equal power, it was evident that no rotating motion would ensue ; but the one being made equal to only about two-thirds of the other, the more powerful jet would accelerate the wheel, until it was balanced by the lesser jet, on account of the negative motion imparted to it. A moderate and remarkably uniform rotation might thus be produced, for the power of the larger jet would diminish, as the square of the diminished relative velocity between the escaping gases and the wheel ; whereas the power of this counter jet would increase, as the square of the increased relative velocity between the gases and the wheel. A small retardation of the wheel, by friction, or otherwise, would consequently produce a great change in the relative power of the two jets. These fuses were lighted the instant the shot was dropped into the gun. Cards of zinc plate were fixed to the sides of the rotating disc, covered first with a black and then with a white varnish, whereon the scribing point would trace a very clear line. Whilst this wheel revolved, a circular line would be obtained, until the pressure upon the steel disc caused the scribing point to ascend, producing a spiral indication of the pressure at all intervals of time. The disc in the front of the projectile was much lighter, being intended to indicate the resisting pressure of the atmosphere, by a line upon the other side of the rotating wheel. The negative pressure of the atmosphere against the back of the projectile might, also, be recorded by a similar arrangement. The diaphragm behind, should, in that case, be made very slight, and be covered by a strong metallic plate, to resist the force of the gunpowder, which plate would separate from the projectile at the mouth of the gun. In the same way, the pressure upon any portion of the curvilinear front surface of the projectile might be indicated, by making, instead of one opening in the centre, several openings in a circle around it. In order to maintain atmospheric pressure inside the projectile, its sides were perforated by a number of small holes. The weight of the moving mechanism need not exceed 6 ounces, and considering its strength and simplicity of arrangement, he did not apprehend any force, less than that which would destroy the shell itself, would interfere with its proper action. The advantages that would be obtained by such a complete record of the forces acting upon projectiles, under different circumstances of charge, form and speed, would, he thought, be very great, not only with regard to the construction of ordnance, and to balistic laws, but to science generally, in affording useful information regarding the nature of fluid resistance. The experiment could be tried with any gun, and at a small expense; and if the proper authorities should think his proposal worth the trial, he should most readily give his services in the matter.

In the discussion of Papers

"ON RAILWAY ACCIDENTS—THEIR CAUSES AND MEANS OF PREVENTION," by JAMES BRUNLEES, M. Inst. C.E.; *and*

"ON RAILWAY ACCIDENTS—SHOWING THE BEARING WHICH EXISTING LEGISLATION HAS UPON THEM," by Captain DOUGLAS GALTON, R.E., F.R.S., Assoc. Inst. C.E.,

MR. SIEMENS * said, the comparative safety of the German and other continental railways was principally owing to the smaller number of trains which were run, and the lower speed at which they travelled. The interference with the personal liberty of the

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* Excerpts Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXI. Session 1861–1862, pp. 383–385.

passengers, which formed part of the continental system, would not, however, suit English habits and notions, and the custom of locking up the passengers could not be considered an element of safety. Passengers were seldom, if ever, injured on a railway platform ; it was while the train was in motion that the principal danger existed. The careful manner in which the rolling stock was manufactured on the German railways also contributed to their safety. Through the agency of Mr. Krupp, cast steel had been very successfully employed in the manufacture of tyres and axles. He believed that gentleman alone had supplied nearly fifty thousand of each, and he had been followed by other manufacturers. In fact, at the present time it was exceptional to find anything but cast steel tyres or axles on the German railways. Much depended upon the manner in which the tyres and axles were prepared. Mr. Krupp had devoted much time and attention to the subject. The hammer which he employed weighed as much as 45 tons, and fell through a space of 10 feet. The anvil was composed of nine pieces, and weighed nearly 1,300 tons. The centre piece was a solid casting, weighing 185 tons. It rested upon the remaining eight pieces, which were of a segmental shape, and each weighed 135 tons. It was only by means of such an agency, that large masses of steel could be welded, so as to form a compact and homogeneous mass. The steel tyres and axles so prepared were used on the northern railways in Germany, where the winter was very severe, and Mr. Siemens was not acquainted with a single instance of a tyre, or an axle breaking during frost.

Another point in regard to the management of the railways in Germany deserved attention. The arrival or passage of each train was telegraphed from station to station, and two trains were not allowed to occupy one section of a line at the same time. He thought that sufficient consideration had not been given to the advantages which would be derived from the adoption of a similar system in England. The trains on the main lines of this country followed each other, in some cases, at as small an interval as five minutes. It would be more correct not to divide the space between them by time, but by distance. But, assuming that a system of telegraphing the train was adopted, as five minutes would represent a distance of 2 miles, or 3 miles, there would be no difficulty in signalling trains at such intervals. If the stations were further apart than 2 miles, or 3 miles, special telegraphic stations might be provided. The mode of signalling adopted in Germany, was to announce a train, at the moment of starting, to the next station, by large bells, placed at intervals of about half an English mile, so that all persons on the line heard when a train had left station A, on its way to station B. Platelayers and switchmen engaged on the line could calculate, within a few seconds, when the train would pass them. The moment the train arrived at station B, the signal was sent back to station A that the line was clear, and the general signal was conveyed to station C, and so on along the line. Perhaps such a system of announcing a train all over the line, would not meet with the approval of English railway engineers; but he thought a system of signalling trains from station to station, and a rule that two trains should never occupy the same section of line together, could be readily introduced without any inconvenience, and he believed, it would effect great saving of time in the working of the lines.

The arrangement adopted upon several of the German railways was shown by the apparatus he had placed on the table. Small bells were fixed at the stations, and at such intermediate points as were thought necessary, and at the moment of leaving, the guard, or other person in charge of the train, turned the handle of the magnetic indicator, which announced at every signalling point that the train had started. Any number of such bells might be rung at the intermediate points. By this signal the train obtained possession of a section of the line, and no other train was allowed to enter that section, until station B had, by another signal, notified that the train had reached it. It was obvious, that such a system might be varied to suit any particular case.

In the discussion of the Paper

"ON THE RELATIVE ADVANTAGES OF THE INCH AND THE METRE AS THE STANDARD UNIT OF DECIMAL MEASURE," by MR. JOHN FERNIE, of Leeds,

MR. C. W. SIEMENS * said he had paid some attention to the subject of the metre system, and had carried out a good deal of work in France with the metre scale, but had not found any inconvenience in working upon that system. His own draughtsmen easily fell into the habit of working with the metre scale, and he had had frequent opportunities of watching its working in the hands of French workmen. There was one misconception frequently entertained in this country with regard to the metre, namely that as the metre was the basis of the system it must necessarily be taken as the unit of measure in all instances. This was not at all the case in France however, where, although the metre was the basis of the system, the millimetre was really the unit in mechanical engineering, and mechanical drawings were figured not in metres but in millimetres. He found the millimetre was a very convenient unit for setting out small mechanical work; for being equal to about 1-25th inch it was smaller than 1-16th inch and larger than 1-32nd inch, and was therefore just such a dimension as a workman could still readily appreciate in following a drawing. Of course the millimetre without further subdivision would not suffice to measure with such wonderful precision as was attained by Mr. Whitworth's system of contact measurement, which had been carried out in connection with the inch divided decimally. But for such accurate measurements the unit of measure employed was of little consequence, since any unit could be decimally subdivided to such an extent as to give the required degree of accuracy; and under the metre system the millimetre was subdivided for the very minutest descriptions of work into 100 parts called centièmes, each of which was equal to about

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* Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1865, pp. 42–44.

1-2500th inch, and was therefore as suitable for very small measurements as the thousandth of an inch.

Moreover independent of the metre being so convenient a measure for ordinary commercial purposes and already so extensively adopted, he thought it deserved serious consideration whether it would be wise to abandon altogether a measure of some such length as the yard or the metre, as would be the case if the inch were taken as the unit of measure. He agreed that in respect to its verification the metre was not an absolute length ; but that was really not a matter of consequence, since, if the quadrant of the earth's circumference were measured a hundred times, each measurement would be likely to differ from all the rest ; and if the measurement were taken several hundred years hence, perhaps the earth itself would have slightly altered in size during that period. The verification of the metre was therefore dependent upon the accuracy of copying an original standard, just the same as in the case of verifying the inch ; and this original standard would always be referred to, instead of measuring the quadrant of the earth over again. It was nevertheless of some importance that the unit of length should be a measure referable to the size of the earth, because it was then easily applied to geographical and even astronomical purposes ; and in this respect the metre had an advantage as the unit of length, in being approximately an even decimal subdivision of the quadrant of the earth's circumference.

He concurred entirely in the desirability of having a system of measure in which there should be a direct decimal relation between linear, square, and cubic measure, and between these and weight, as had been explained to be the case under the metre system. It had been correctly explained that the metre afforded a very great facility for ascertaining the weight of any bulk of material, its linear dimensions and specific gravity being known. There was then the least demand made upon the memory, since the specific gravity of different substances was all that had to be borne in mind, instead of a number of practical rules having to be recollected, which were applicable to one material only. The product of the cubic dimensions of any substance in metres multiplied by its specific gravity gave the weight of the substance in tonnes, being almost identical with English tons, or in kilogrammes when the decimal point had been shifted three places to the right. Upon the whole he considered it would be far better to adopt the metre system in this country, in accordance with the other nations who were already using it, than to decimalise a separate unit which would never work afterwards in harmony with the rest of the world.

In the discussion of the Paper

"ON THE MAINTENANCE AND RENEWAL OF PERMANENT WAY," by R. PRICE WILLIAMS,

MR. C. W. SIEMENS * said, it had been asked, what was to be done with the Bessemer iron after it was worn out? He replied, melt it down, not in a blast furnace, but in a melting furnace, and make cast steel of it. He did not speak at hazard, for it was actually done by means of his Regenerative Gas Furnaces. M. Emile Martin was carrying out at Sireuil, in France, a process of melting scrap steel, sometimes Bessemer metal, in an open reverberatory furnace, which had been built by Mr. Siemens as a puddling furnace. This metal, when melted down, was used for steel tires of railway wheels.

With regard to the paper generally, it contained a mass of valuable facts, which he thought would lead engineers to a thorough knowledge of what they actually required, and that was nearly as valuable knowledge as the mode of carrying the necessary improvements into effect; because the remedy for a mechanical defect might, nowadays, be almost regarded as the necessary consequence of its proved existence. His own interpretation of the facts and experiments brought forward in the paper was, that instead of using laminated metal, which might be regarded as a bundle of iron wires soldered together by cinders, the metal used for rails and tires should be homogeneous; and that in order to get it thoroughly homogeneous it ought to be cast. The Bessemer

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXV. Session 1865–1866, p. 378.

process gave a ready means of melting metal which was called steel, but which might with equal propriety be called homogeneous iron ; and if a bar of iron was melted into a homogeneous mass, the probability was the result would have been a metal not differing greatly from Bessemer metal. In order to melt this metal, when not resulting directly from the Bessemer process, recourse must be had to another process, such as he had before mentioned. Speaking from his own experience, a ton of scrap steel could be melted with less than a ton of common slack, either on an open hearth or in crucibles ; and this was in his opinion a satisfactory answer to Mr. Struvé's objection to steel rails, the cost of re-melting the steel by this process being as small as that of re-rolling iron rails.

Mr. Fowler (President), inquired whether, if the Bessemer metal could be melted and used for tires, there was any reason why the same metal should not be re-made into rails ?

Mr. Siemens had merely stated a fact within his own experience, but he saw not the least objection to re-convert the Bessemer rails into cast-steel rails, which would certainly be improved in quality by the transformation.

In the discussion of the Paper

"ON THE CONSTRUCTION OF IRON SHIPS, AND THEIR PRESERVATION FROM CORROSION AND FOULING BY ZINC SHEATHING," by S. J. MACKIE,

THE CHAIRMAN (Mr. C. W. Siemens *) said, as to the importance of the subject treated in the paper there could be no doubt. It was a national question to overcome the difficulties which still attached to the use of iron vessels. Iron had been so completely proved to be the better material in naval construction that the Government had largely adopted it ; and yet there was one defect

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* Excerpt Journal of the Society of Arts, Vol. XV. 1866–1867, p. 369.

attaching to it, viz., that iron ships would foul very quickly. The means hitherto adopted were clearly insufficient as a remedy for the evil. The poisonous compounds spoken of he thought might be fairly dismissed from their minds as being playthings in connection with a very serious subject. Copper sheathing appeared very inapplicable, for when in contact with iron it invariably had the effect of corroding it, because the salt water would percolate between the two metals; and moreover the copper sheathing itself would fail in its purpose from the want of exfoliation of the surface. With regard to the insulation of the two metals by the interposition of a wooden layer, he agreed with Mr. Reed, that the iron would be effectually protected so long as no metallic contact took place. The moisture between the two metals would not be sufficient to set up galvanic action, the battery would be in the condition of an "open" battery, not a "closed" one. There was, however, great difficulty in maintaining perfect separation, because wire, even of $\frac{1}{16}$ inch gauge, was sufficient to transmit a considerable current, and produce a great amount of mischief upon the iron, and it was hardly to be supposed that the two enormous surfaces could be long kept perfectly separated without metallic connection being formed between them. What surprised him, somewhat, on hearing this paper, was the very slow rate of exfoliation of the zinc. He had himself made experiments on the action of the salt water upon zinc in contact with iron; and he found the zinc acquired weight up to about three months, but after that period a sensible diminution in weight took place. The author of the paper stated that the amount of the oxidation of the zinc was not more than 1¾ oz., or 2 oz., even if the exfoliation was made more active by an increased galvanic action. It would be interesting to ascertain whether there was any increased exfoliation when the ship was in motion. No doubt there would be some increase, but experiment would determine the amount. No doubt, chemically, zinc sheathing would protect the bottom of the vessel entirely, for even if a sheet of the metal were displaced, there would still be the influence of the zinc in contact with the iron. So far, then, the invention appeared to him to be an exceedingly promising one, and one which he thought should certainly be tried seriously by the Admiralty. With regard to the observations of Mr. Reed, he thought he had stated the case very fairly, and he believed personally he would be disposed to afford the invention every trial it required ; but, if the experiment at Portsmouth was to be taken as evidence of the anxiety of the Admiralty to inquire into the merits of new inventions, he thought in this case an injury had been done to the inventor. There was nothing, he thought, more destructive to the interests of an inventor than an imperfect trial.

Mr. Reed thought it due to the Admiralty to say that the experiment was not initiated by them. They merely gave permission to Mr. Daft to put down some plates prepared on his system, but they were in no way pledged to go on with the experiments. At the same time, having gone to the extent they did, it might have been desirable that they should have continued the experiments further. He merely wished to say that the Admiralty did not initiate the experiments and then suddenly drop them.

The *Chairman* said that altered the case in some respects ; but he maintained that even the sanctioning of experiments implied, he thought, a continuation of them ; those who were practically acquainted with the difficulties appertaining to the introduction of inventions, would appreciate more than official personages could possibly do, the great hindrance caused by incomplete experiments to the progress of an invention. If the intervention of the Government were entirely refused, the inventor was free to act as he pleased ; but from the moment he placed his invention in the hands of Government, he was practically shut out from the public until a verdict upon it had been pronounced. He thought the Government might spend a few thousands a year very well in making really serious experiments upon questions of this nature. Even if such an invention as this were tried upon a merchant ship it would be no convincing proof to the Government of its merit. The Government must make its own experiments to determine its value. With regard to the mechanical mode of joining the plates, he thought it sufficient for this invention if it was admitted that there was no inconvenience thus caused. He did not think any great weight was to be attached to the question of the buckling of the plates ; if the back-strap was carefully put on, there would be no fear of fracture unless, as mentioned by Mr. Reed, the back-strap had the fibre in the wrong direction. He thought Mr. Mackie had brought the whole subject very ably and fairly before them, and he was sure they could do no less than give him their thanks for having done so.

In the discussion of the Paper

"ON OPTICAL APPARATUS USED IN LIGHTHOUSES,

By James T. Chance, M.A., Assoc. Inst. C.E.,

Mr. C. W. Siemens * said, it had been objected that the paper was not of an engineering character, but the subject was intimately connected with engineering and had been received with interest by the members. Mr. Chance had confined himself to the optics of lighthouses, which was a large subject by itself, although many would have liked to have heard about their mechanical construction, on which he had so much practical experience, and also on the constitution of the glass, which Mr. Siemens believed was of great importance to the results obtained. The description of glass used in the lenses and prisms was, he understood, generally flint-glass—that was glass which had oxide of lead for its base ; but this glass varied very much in quality. A small addition of lead would increase its refrangibility considerably, and he knew there was difficulty in getting an even mixture at the top and bottom of the glass pot. He therefore thought there must be some special means of obtaining uniform refractibility, or some ready means of adjustment for differences in the degree of refractibility, which he would ask Mr. Chance kindly to explain. One point of great interest had been touched upon, which should be fully discussed. The Astronomer-Royal, in going from Dover to Calais, observed that at a certain distance from the two Foreland lights, one dioptric and the other catoptric, the two showed no essential difference in intensity, though the dioptric light was far more brilliant than the other when viewed from a short distance. No explanation of this observation had been offered, and he would

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXVI. Session 1866–67, pp. 529–532.

merely suggest whether it might not be the case that, although the dioptric light was the more brilliant in itself, it would nevertheless, at a considerable distance, produce the same effect only as the other light for the following reason. If light might be regarded as a vibratory motion of the medium through which it was transmitted, any obstructive matter in the form of haze or smoke must exercise a destructive effect according to the square of the energy of vibration, or intensity of the light. If that were the case, it followed that a brilliant light would in an obstructive medium soon subside into a light of moderate intensity, and thence proceed at a more equal rate of diminution with light proceeding from a less brilliant source but of equal magnitude, the latter being chiefly determined by the extent of light-emitting surface. For instance, one light produced by a candle would be lost sight of, under certain atmospheric conditions, say at a distance of half a mile. But with six lights of the same size placed side by side, a sufficient amount of light would be conveyed to that distance to produce a distinct effect on the eye. In the same way the glare of the gas-lights of London was seen at a distance of twenty or thirty miles, whereas a limited number of more intense lights would be lost to sight at that distance. He therefore thought the quantity of light emitted was of more importance than its intensity in seeking distant effects, a circumstance which had not perhaps been fully considered in estimating the relative value of the electric light, as contrasted with the ordinary optical apparatus of extended surface.

The question had been put, whether the dioptric light was, under all circumstances, better than the catoptric; and the author of the paper seemed to be much in favour of the dioptric system. Now it appeared to him that, for lights of comparatively short range, the catoptric system could be used with advantage, because the reflecting mirror was the more simple arrangement; and if its surface could be kept clean, it would reflect the light in a certain definite direction without much loss, provided the parabolic mirror were extended far enough over the light. The principal drawback appeared to be, that the surface of the parabolic mirror became tarnished; and in order to prevent that, he would recommend those interested to try pure nickel surfaces, produced by the galvano-plastic process. He had tried them, and he thought they were perhaps of all metallic surfaces the least apt to tarnish. Nickel was as hard as hardened steel, and it seemed to remain perfectly bright under all atmospheric influences, even in rooms where sulphuretted hydrogen was present.

There was one other light, which had occupied his attention during the last twelvemonths, to which he would refer:—Mr. Thomas Stevenson, of the Northern Lights, had proposed to establish flashing lights (that was to say, lights giving out flashes at certain intervals) upon beacons and buoys; and Mr. Siemens had been applied to with a view to accomplish that object. The source of light was to be upon the land, because there were periods of the year when a landing could not be effected with safety at the beacons or buoys; and the source of light which naturally suggested itself under these circumstances was electricity. The apparatus that had occurred to Mr. Stevenson was the Ruhmkorff coil placed upon the land, and communicating with the beacon through a cable; but the preliminary experiments at once showed, that the discharge of a Ruhmkorff coil would be absorbed in a cable of only 100 yards in length, and that no spark would be produced on the beacon. The next thing tried was to place the coil on the beacon, and to send simply the battery current through the cable: a cable having a large metallic section was taken, but nevertheless the absorption was such, that no perceptible spark could be produced. Under these circumstances the idea suggested itself to him, that a simple metallic circuit might be established through the coils of an electro-magnet, and that the extra current produced in breaking that circuit would produce a flash, close to the electro-magnet upon the beacon, which would be increased rather than otherwise by the accumulated charge in the connecting cable. If this could be practically accomplished, then the light might be placed at a considerable distance from the shore, without destroying the battery effect which had to be transmitted from the land through a cable. The apparatus was not perfected at once; but he had placed one on the table which would accomplish the object in view. It comprised a heavy electro-magnet, the coils of which were supposed to be in communication with a battery on land through a cable. A clock-work apparatus on land established the electric circuit through the cable at certain predetermined intervals. The electric circuit through the cable was, however, not complete, unless the weighted armature of the electro-magnet was in its distant or unattracted position. The attraction taking place, the circuit was broken at the point of a platinum pin, which was drawn from a mercury bath, and a brilliant discharge of extra current ensued. The current being thus broken, the armature fell back and re-established the circuit, when it was again attracted, and a discharge again took place, and so on during the periods of time when the circuit was established on land. The mercury was continually renewed at the point of contact by means of a circulating pump, worked by the electro-magnet itself, which latter had to be very powerful in order to produce an intense light in its discharge. The point of discharge was placed in the focus of a dioptric or catoptric reflector, upon the beacon or buoy, to be lighted. This apparatus had only lately been completed, and had not yet been tried at sea; but it had been at work experimentally for some time, and appeared to give very constant effects. If this apparatus was constructed for throwing the light only through a limited arc, the effect would be much intensified; and in that form he thought it might be placed with advantage at narrow entrances, where each light would tell its tale by the periods of successive flashes peculiar to itself; and since the succession of flashes could be varied at will by the contact arrangement on land, the apparatus might also be used for conveying special warnings or signals to vessels out at sea. This apparatus was only applicable to a succession of flashing lights.

In the discussion of the Paper

"ON THE PRESENT STATE OF KNOWLEDGE AS TO THE STRENGTH AND RESISTANCE OF MATERIALS," by JULES GAUDARD, Civil Engineer, Lausanne,

[Translated from the French by WILLIAM POLE, F.R.S., M. Inst. C.E.]

MR. C. W. SIEMENS * observed that the author of the paper appeared to base all his calculations, which were very elaborate and valuable in themselves, upon the breaking strain of materials. He thought, for practical information, it would be necessary to follow out a similar investigation, carried only to the limit of elasticity, which the author had entirely ignored. If the limit of elasticity of all materials was proportionate to the breaking strain, the one investigation would cover the two cases ; but materials differed greatly in this respect. The ultimate strength and flexibility of a metal, such as would be conformable to the calculations of the author, as for instance, lead, was, in its property of yielding to moderate force, very different to iron, and in a still greater measure to steel. Steel would yield, within the limit of elasticity, up to a much higher point than, he believed, any other metal. In devising engineering works it was of the utmost importance to know, not merely when a structure would give way, but when any destructive action would commence.

In dealing with transverse strain, the author illustrated the case by a figure, signifying the strain on every fibre in the beam. That figure was perfectly correct for breaking strains, where the fibres first permanently elongated by the strain brought the next into greater tension, and so on in succession, till the limit was reached where the outer fibres would actually break. But before such a diagram of resisting forces could be true, permanent deflection must have taken place ; and it was of importance to engineers to know what was the distribution of strains before any permanent effect had been produced ; and within those limits he main-

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXVIII. Session 1869-1870, pp. 32-34.

tained the form of the diagram would be more nearly represented by straight lines crossing each other in the neutral axis.

He was ready to admit that the limit of elasticity was not an absolute point; that there was a slight set produced in straining a bar for the first time; and that the ultimate limit would be more correctly represented by a bend in a curve than by a sudden change of direction. But, nevertheless, he maintained that the position of this bend in the curve, denoting elongation and compression in each material, was of great importance, and could not be ignored without arriving at erroneous conclusions.

The resisting force of cast steel—a material that would hereafter enter largely into all mechanical construction—was nearly three times greater than that of iron up to the limit of elasticity. He believed that the Railway Inspectors of the Board of Trade would be willing to acknowledge the greater strength of cast steel, if that material could be readily distinguished. No doubt, at first sight, it was difficult to ascertain whether the plates of a bridge were of cast steel or of wrought iron; but this he would suggest might readily be ascertained from the specific gravity of the metal. He had, in his own laboratory, submitted a number of specimens of steel and iron to this test; and he found that in all cases wrought iron was 2 per cent. or 3 per cent. lighter than cast steel of nearly the same chemical composition. Fused wrought iron had a specific gravity of 7·87; but if 2 per cent. of carbon was added to it the specific gravity was reduced to 7·79; common bar iron had a specific gravity of 7·55; and puddled slab 7·53 only. The specific gravity of puddled iron of the greatest purity never reached 7·6; while that of mild cast steel, with carbon varying from nil to 4 per cent., always exceeded 7·7, a distinctive difference that could be easily recognized. There was an easy way of determining roughly the specific gravity of metal: chip off the corner of a plate, suspend it from the arm of a balance, and weigh it both in and out of water; divide its weight in the air by the loss of weight in water and the result was the specific gravity of the metal. But if it were said this was too much trouble for engineers or government inspectors to undertake, the Board of Trade might appoint inspectors for the purpose. The manufacturer might pay the inspector's expenses, and the latter might stamp upon each plate, which he had seen made and properly annealed, a mark signifying that it was cast steel of a certain quality. He thought there should be no practical difficulty in deciding which material existed in a structure ; and with a material such as cast steel, there would be, as he had stated, an available strength three times greater than that of ordinary wrought iron.

In the discussion of the Paper

"ON THE ARTIFICIAL PRODUCTION OF COLD,"

By Professor John Gamgee,

The Chairman * (Mr. C. W. Siemens), in proposing a vote of thanks to Professor Gamgee, said there was no doubt that the machine described was theoretically the same as the ether machine ; it was simply a question of the details of construction ; but sometimes these matters were of great practical importance in the result. Mr. Reece's machine, on the other hand, was of an essentially different character. He did not use mechanical force, but produced the refrigerating action by the evaporation of water and ammonia, and re-absorption of the ammonia by water. That, no doubt, was a different conception of the same problem, but finally it came to the same theoretical result, although the ammonia machine avoided the losses connected with the steam-engine, which were very considerable. It had been correctly stated that a ton of fuel ought to produce something like 80 tons of ice, but considering that a considerable quantity of heat must always be wasted, from 40 to 50 tons was about the practical limit. He could not agree with Mr. Hancock, that in hot climates the liquid to be employed must be different, because the question of the liquid to be chosen did not depend on the external temperature, but upon that which you wanted to produce. In the case of Mr. Gamgee's machine, the amount of compression that had to be performed by the pump would be much less if the water were at 60°, than if it were at 80°

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* Excerpt Journal of the Society of Arts, Vol. XIX. 1870–1871, p. 502.

or 90°. If you wanted to go to the freezing point, or below it, you must select a liquid that would boil at a point considerably above the freezing point of water, under the reduced pressure maintained by the air-pump. He should have liked the air-machines to have been more discussed, as they were well worth attention. There was a well-known method of producing a low temperature by compressing atmospheric air, cooling it, and then allowing it to expand; and some years ago his attention was directed to a machine of that description invented in America, which he found laboured under a most egregious error in its conception. The air was compressed by a very excellent machine, and was cooled by a well-arranged system of tubes, but it was then allowed to expand through a throttle-valve, under the idea that depression of temperature would thus take place. But this was an entire misconception of the facts. Heat was nothing but force, and the reason why air in expansion became lowered in temperature was simply because it developed force in so doing, and if no force were developed in its expansion, no depression could take place. Therefore, if air were compressed to a hundred pounds to the square inch, and then were expanded through a small orifice, there would be precisely the same temperature in the expanded air, as there was before, but if the same air were expanded between the same limits in an expansive engine, a proportionate loss of heat would take place, and the machine would give back a considerable amount of the power expended in compressing the air. Where the problem was simply to cool the air, this kind of machine was, therefore, well worthy of attention. Professor Gamgee had complained of being misled by engineers, and he feared he was not yet quite out of the hands of the Philistines, for the rotary-pump he referred to would not, he thought, be equal to an ordinary honest cylinder and piston. However, as it was not particularly described, he would not condemn it altogether.

In the discussion of the Paper

"PNEUMATIC DESPATCH TUBES: THE CIRCUIT SYSTEM,"

By Carl Siemens, M. Inst. C.E.,

Mr. C. W. Siemens * said he would point out the leading features of this system as compared with other pneumatic systems, which had been in use for many years. In sending a carrier through a pipe on the old system, the pipe was entirely occupied by that carrier; and, if the carrier was sent back by suction in the same pipe, double the time of transit would be occupied. That was sufficient for short distances; but for greater distances the working capacity of the tube became very small, because the piston velocity of a carrier in a tube of small diameter would not exceed 1,000 or 1,200 feet per minute; therefore, in a tube several thousand feet long, the time occupied in sending a carrier and receiving one back would be considerable. Now it had occurred to his firm, that if a line could be made continuous (instead of sending a carrier and waiting for the return carrier to be despatched through the same tube, or even another tube), in that case the carrier would form, as it were, part of the current of air rushing through the entire circuit, and any number of light carriers might follow each other without inconvenience, and largely increase the working capacity of the tube. Moreover, it occurred to his firm that, with a continuous circuit, intermediate stations might be introduced for shunting out and putting in carriers to be sent forward in the same circuit, whereby a multiplicity of tubes, otherwise necessary, would be avoided. Four or five years ago he made a proposition to the Postmaster-General to apply this system to the transmission of letters, but it had not been carried out, though, probably, at a future time, the project might be seriously entertained. By such a system the despatch of letters would, unquestionably, be much accelerated, and he should be

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXXIII. Session 1871–1872, pp. 16, 17.

much surprised if it did not prove the cheapest mode of transit in London and other large towns.

The only other point of interest to which he would allude was the blowing apparatus. This was now working occasionally at the Post Office in the way of trial against the engines. It was not quite equal in steam economy with the engines, but it must be borne in mind that the steam pressure was only 35 lbs. or 40 lbs., and the steam engine employed was a very good one. Comparative trials showed that, with the same boiler power, the steam engine maintained from 2 to 3 inches more vacuum with the tube open than the steam blower; but other experiments with a higher pressure of steam reversed that result. With steam of 70 lbs. pressure, the working results of the steam blower were superior to those of the steam engine.

Mr. Hawkshaw, Past-President, inquired how the risk of cutting the carrier in two by the introduction of the rocking-frame was avoided, supposing it was just passing the joint at the moment the rocking frame was worked?

Mr. Siemens replied, that the attendant heard when the carrier had arrived, as it made a little noise; but a small bell might be made to sound automatically when the carrier had arrived within 20 yards of the station.

In the discussion of the Paper

"ON THE ABA-EL-WAKF SUGAR FACTORY, UPPER EGYPT," by WILLIAM ANDERSON, M. Inst. C.E.,

MR. C. W. SIEMENS [*] said the paper dealt with two separate subjects, one of a mechanical, and the other of a chemical character. It appeared to him that the author had very satisfactorily solved the mechanical questions involved. The arrangements of the mill-gearing had evidently proved successful, but it

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[*] Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXXV. Session 1872–73, pp. 75–78.

was a question open to discussion whether so large a diameter of roller was beneficial in a chemical and economical point of view. By using large rollers a greater amount of saccharine matter could, however, undoubtedly be extracted. The system of raising the juice from one receiver to another by a centrifugal pump, instead of by the old 'blowing-up' arrangement, was also an improvement; because in admitting steam in contact with the saccharine solution, condensation would take place, and the work of evaporation would be increased; and if the centrifugal system was properly arranged, there need be no apprehension that it would churn the saccharine solution. Considerable ingenuity had been displayed in the construction of the evaporating apparatus employed. The steam boiler had been placed immediately below the concentrator, or, as it was called, the 'juice boiler,' and thus the steam generated in the lower compartment at once condensed against the upper surface of the steam boiler, which was also the bottom surface of the juice chamber—the surface contact being increased by means of tubes—and in that way losses of heat by radiation or otherwise were avoided, and the same water was re-evaporated again and again. It was interesting to observe that the surface in contact with the fire could be made three or four times smaller than the surface necessary to convey the heat from the steam to the saccharine solution.

The use of the steam raised from the saccharine solution was another novelty. It was made serviceable not only for working the vacuum pan, but also for driving the engine. There could be no reasonable doubt about the mechanical efficiency of applying the steam raised from the juice to drive the engine, and theory in this case had been justified by the result; but a question of a chemical nature arose, namely, whether the juice itself was not injured by raising from it steam of sufficient pressure for such purposes. It was well known that when juice was concentrated by the direct application of fire, a considerable portion of the sugar was converted into molasses, or uncrystallizable sugar, and it would be desirable that the opinion of persons practically engaged in sugar-boiling operations should be ascertained as to whether the direct application of heat had not been carried too far, or whether the same amount of juice would not have yielded a larger amount of crystallizable sugar if the direct application of heat in the vacuum pans had been resorted to exclusively.

It might be urged that such an amount of heat could not have been obtained, owing to the larger consumption of fuel which would have been requisite ; but there was a process now in course of trial, partly suggested by himself, to evaporate entirely by vacuum pans, and at the same time to economise heat by forcing the steam generated at low pressure within the vacuum pan mechanically into the tubes surrounded by the juice.

The main feature of the paper was the substitution of sulphurous acid for charcoal in removing the colouring matter. If sulphurous acid could be applied without practical drawbacks, great saving must undoubtedly arise, because animal charcoal was an expensive substance, and involved the employment of complicated apparatus to revivify it for repeated use. But the question arose—whether in using the sulphurous acid method a portion of the crystallizable sugar was not converted into uncrystallizable, or grape sugar ? It was a well-known fact that if sulphuric acid was put into a solution of cane sugar, its mere contagious action, so to speak, would convert an indefinite amount of the solution into uncrystallizable, or grape sugar, which, though very similar as to chemical constitution, was of a much less sweetening character. It was doubtful whether the sulphurous acid was always free from sulphuric acid, particularly if, as was suggested, oxydising agents, such as peroxide of manganese, were also employed. An increase of grape sugar would not necessarily imply a diminished yield, because when the solution came to a certain consistency it might solidify. He would inquire whether in the sugar prepared at the Aba mills there was not a proportion of uncrystallizable, or grape sugar precipitated with the crystallizable, or cane sugar ? He did not believe in the theory put forward that galvanic action would be set up between the charcoal and the colouring matter. The conditions of sugar solution were totally at variance with what might be expected in a galvanic battery, where two conductors of different affinity for oxygen were brought into metallic contact while immersed in acidulated water.

In regard to the experiments which he had referred to as having been conducted for effecting the concentration under a vacuum so as to attain the advantage of using the steam repeatedly, he might add that many years ago he proposed to evaporate liquid cane juice by pumping the steam from the vacuum pan, which was in the form of a locomotive boiler, into tubes surrounded by liquid in order that it might be condensed there. To sustain the further evaporation of the same liquid, a steam engine and pumping cylinder were employed, whereby the steam generated within the evaporating pan at about half the pressure of the atmosphere was compressed, and forced into the tubes at double, or atmosphere pressure. Its condensing point was raised in compression from 180° to 212°, which difference sufficed to cause the recondensation of the steam within the tubes and a continued ebullition of the juice, the same latent heat being made to serve over and over again. The only real expenditure in this operation was mechanical force, but the steam employed in generating this force was necessarily much less in amount than the steam compressed by it through half an atmosphere. Moreover, the exhaust steam of the engine was made available to make up for the loss by radiation, and for bringing the cold juice up to the boiling point.

Such a process could not fail to work practically, but the mechanism involved in it was of a nature to make its application costly. The project was revived last year by Mr. Robertson, who after hearing the Paper on "Pneumatic Despatch Tubes; the Circuit System," by Mr. Carl Siemens, M. Inst. C.E.,* conceived that the steam blast apparatus referred to, being capable of maintaining a vacuum of 20 inches of mercury, could be made to serve also to force the vapour raised in a vacuum pan into the evaporating tubes of the same or another pan, and thus to combine the advantage of evaporating the juice under a reduced pressure with that of repeatedly using the latent heat of the steam. Mr. C. W. Siemens considered this plan to be superior to that originally proposed by himself, inasmuch as the apparatus employed was simple and inexpensive. When tried in London, Mr. Robertson found that a vacuum of 20 inches could be maintained in his vacuum pan. In that case, as in the former, the steam employed in compressing the vapour in order to fit it for

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* Vide Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXXIII., p. 1.

recondensation was added to the compressed steam and served to make up for losses by radiation, &c. It was evident that by such an arrangement the latent heat of the same steam could be used, not once or twice, but several times, the heat passing again and again through the same metallic surfaces. Whether such a plan could be worked advantageously in practice in the East and West India sugar factories was a question upon which Mr. C. W. Siemens had no experience to adduce, but the experiments had at any rate proved the correctness of the principle involved.

In the discussion of the Paper

"ON THE MECHANICAL PRODUCTION OF COLD,"

By Alexander Carnegie Kirk, Assoc. Inst. C.E.,

Mr. Siemens [*] said if he wished to be critical he might find fault with the title of the paper. The author spoke of a machine for "the production of cold." Cold was the absence of heat, and it might be open to question whether it was possible to produce the absence of a thing. Refrigeration, which he thought the preferable word, meant the transfer of heat from one substance to another of the same or a superior degree of temperature, and the author evidently agreed in that definition. The subject was one of considerable interest at the present time. Refrigerating machines were now largely used in breweries, since fermentation went on to advantage only at a temperature a little above freezing point; and to attain that point during all seasons of the year rendered artificial means of maintaining a low temperature necessary, unless native ice was employed for the purpose. For preserving meat also in hot climates and transporting it, artificial means of reducing the temperature were coming into use, and would be more extensively employed if a cheap and ready method could be devised. Refrigeration was of great importance in hot

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[*] Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXXVII. Session 1873–74, pp. 283–287, 300–301.

climates in a sanitary point of view, and the time was not distant, he thought, when houses and places of public resort would be refrigerated with the same care and regularity as they were now heated when necessary. He believed this might be accomplished at a cheap rate. It was stated in the paper, and it was an undoubted scientific fact, that mechanical refrigeration might be obtained at a cheaper rate if the reduction of temperature required were only slight than if it were considerable. Thus, an air-machine producing ice would work much less economically than one producing only cool air. In order that the subject might be more fully opened out for discussion, he proposed to refer shortly to the different methods that had been devised for producing refrigeration. There were four methods in use. The first was the old system by the evaporation of alcohol, ether, or other volatile substances. Even water when allowed to evaporate under a current of air produced refrigeration. Alcohol did so to a greater extent, and ether to a still greater extent. This method had been adopted, perhaps, for centuries; but in recent times it had been improved by Siebe and by Harrison, who had contrived that the vapour produced by evaporation should not be lost, but that it should be mechanically compressed and condensed, in order to serve over and over again. The method of producing the reduced temperature was the same in both cases, but instead of losing the ether or alcohol, a certain amount of power was expended in the improved arrangement. Another method was the chemical one of producing refrigeration by evaporation in connection with absorption. Many vapours—ammonia being one of them—were readily absorbed by water; but could be separated again by the application of heat to the mixed liquid. A machine on that principle was shown at the Universal Exhibition, in 1851, by M. Carré, and very good results had been realised by it. It consisted of a boiler which was filled with ammoniacal liquor, and the ammonia vapour was driven off under considerable pressure into a surface condenser composed of tubes surrounded by cool water. A separation was thus effected by heat of the ammonia from the water; and the ammonia, after being withdrawn into a vessel of lower pressure, evaporated at a very low temperature, and thus produced refrigeration, the vapours of ammonia being eagerly absorbed by water of ordinary temperature forming mother liquid for re-evaporation in the boiler. The machine was largely used, especially on the Continent; and, from information he had received, it produced a hundredweight of ice at the expense of about a shilling. An ingenious modification of this machine for small applications on board ship or for household use had been devised, consisting of two vessels connected by a pipe, but hermetically sealed. One of the vessels contained the mother liquid, which was alternately heated and cooled, to drive off the ammonia, and to re-absorb it from the second vessel, which served alternately as condenser and refrigerator for the production of ice. Another method was by the solution of crystalline substances. There were various refrigerating mixtures, one of the salts so employed being carbonate of ammonia, and another chloride of calcium. When crystals of chloride of calcium were dissolved in water, a considerable reduction of temperature—about 30° Fahr.—took place. Although that would not be sufficient to produce ice from water of 60° or 65° temperature, an arrangement could be made by which the water to be cooled exchanged heat with the spent liquor, thus producing an accumulation of the effect in the centre of the machine. He constructed a machine on this principle many years ago, which produced ice at a considerable rate, but the salt employed—chloride of calcium—was not a pleasant substance to deal with. It had to be re-evaporated and crystallised, and this process was inferior to the purely mechanical methods which had since been adopted. The most perfect of these, as regarded cleanliness and freedom from loss, was the air machine. Atmospheric air was compressed to one half or one atmosphere above atmospheric pressure. The compressed air was allowed to cool in contact with water, either by external application or by injection, and to expand again in a working cylinder. The amount of heat that disappeared in the second working cylinder was the exact measure of the refrigeration produced, and it could be easily calculated; whereas the power expended was the difference of force involved in compressing the air at a higher and of expanding it at a lower temperature. In 1857 a machine of that description was invented by Dr. Gorrie, an American, and was brought to London. Mr. Siemens was asked to report upon it. The machine did not produce satisfactory results. The engine was a good one, and the air-pump was judiciously constructed; but the connection between the reservoir containing the compressed air and the air-expansion engine was too narrow, and was provided with a throttle valve, there being evidently a vague idea in the mind of the inventor that the air would produce more refrigeration in expanding spontaneously without doing work than in expanding behind a working piston, an idea which was permissible at that time when the dynamical theory of heat was little understood. That was one of the defects which he pointed out. Another was that the hot or compressed air was not sufficiently cooled before it was expanded, and was not deprived of its moisture. The moisture in air played a considerable part in those machines. At a temperature of 65° Fahr. saturated air contained 1 per cent. of vapour of water, and this had not only to be reduced into the liquid, but also into the solid condition, representing a total absorption of heat to the amount of 1,140 units of heat per lb. of condensed vapour, which, upon the quantity of air, would represent 15° Fahr. of loss in the effect produced by the expansion. He believed, if these faults had been remedied, the machine would have given satisfactory results. Since that time, a German engineer, Mr. Windhausen, had constructed machines on similar principles, and had, after many fruitless attempts, obtained remarkable results. It was stated, at a meeting in connection with the Vienna Exhibition, that a machine of 150 h.-p. produced 30 cwt. of ice per hour, the theoretical result being that that amount ought to be produced by 90 h.-p. The cost of producing a hundredweight of ice by this machine was stated to be one shilling—a similar result to that obtained in M. Carré's machine. The machine described by the author of the paper was also an air machine—a reversed air engine, so to speak—and therefore, in a certain sense, analogous to those he had before mentioned. The author did not compress the air, cool it, and then transfer it into a separate cylinder to be re-expanded, but he combined these operations in an engine similar, in every way, to Stirling's air-engine, on a supposition that that was the most perfect air-engine known, and that, in inverting it, he would be likely to obtain the best result of refrigeration. He could not, however, agree in the opinion that the Stirling engine was a perfect one. It was the first engine containing a regenerator ; but (as he had pointed out in a paper read before the Institution in 1853) it realised at most only from one-fifth to one-sixth part of the theoretical duty of the heat expended. The reason was that all the air cooled and heated alternately did not enter the working cylinder; but the diagram of the force obtained in the working cylinder formed only a sixth part of the diagram that would be produced if the whole of the air were allowed to expand behind a working piston and between the same limits; and that proportion really indicated the dynamic value of the engine. Therefore, although he admired the ingenuity with which the author had enlarged the available heating surfaces of Stirling's arrangement, and elaborated the best form of regenerator for the purpose, he could not agree in the application of that principle to refrigeration. He believed better results would have been obtained if the compressing apparatus had been separated from the expanding apparatus as had been done by others. That opinion appeared to be corroborated by the results given in the paper. With 37 h.-p. 20 gallons of water were reduced from 61° to $47\frac{1}{4}$° per minute, which was equal to 2·8 lbs. of ice per hour, whereas the Windhausen engine was said to produce 20 lbs. of ice with 1 h.-p. Generally speaking, he believed the air-compressing engine, on the purely mechanical mode of producing refrigeration was applicable with the greatest advantage where moderate refrigeration was required. Where the production of ice in large masses was desired, he believed the method adopted by Siebe and by Harrison was superior, for this reason: in compressing and expanding air, 25,000 cubic feet of air were required to produce the effect of 1 lb. of ice; whereas, in compressing sulphuric ether after evaporation, only 5,100 cubic feet were required, the reason being that when sulphuric ether was transferred from the liquid into the gaseous condition, the whole of the latent heat was obtained. A much higher result was arrived at by using a still more volatile substance—methylic ether—which at a depression of temperature equal to 15° Centigrade had a pressure of $1\frac{1}{2}$ atmosphere, and the displacement of piston to produce the same effect was only about 340 cubic feet. Therefore a pumping engine, with a displacing capacity of piston of 340 cubic feet per minute, would produce the same effect as an air engine of 25,000 cubic feet displacement per minute, and of 5,000 cubic feet in the case of a sulphuric-ether engine. This meant a much smaller engine and a less costly machine in the case of the methylic ether pump, although the expenditure of power might be the same; but, on the other hand, there was the set-off of having to deal with a highly inflammable material like methylic ether instead of with atmospheric air. For producing a depression of temperature in houses or breweries he believed the air engine was the best contrivance that could be adopted.

MR. SIEMENS explained that he had no desire to disparage Mr. Kirk's ingenious contrivance—on the contrary, he wished it every success—but he could not help observing upon the drawbacks which he conceived were incidental to the construction of the machines. He admitted that the construction had the advantage of giving a greater amount of power in a limited space than the Windhausen machine, but, as a set off, the greater back pressure or lost effect incidental to the engine must be taken into account. Other speakers had alluded to the thermo-dynamic theory, and had argued that, inasmuch as a unit of heat could only develop 772 units of force, so 772 units of force were necessary to abstract one unit of heat in the production of ice. If the object was to create a unit of heat he could agree with these remarks, but refrigeration meant only the displacement of heat from one temperature to another, and involved the amount of force necessarily due to that step. In starting from the absolute zero point water of the ordinary temperature of 60° was in reality just about 500° hot; and in depressing this temperature to 10°, work had to be accomplished amounting to $\frac{60}{500}$, or but little more than $\frac{1}{10}$th of the mechanical equivalent of the heat so transferred. This proposition could be verified by means of two diagrams, one representing the curve of air compression with simultaneous injection of cold water, and the other the air expansion after cooling; the difference of magnitude between the two was only $\frac{1}{10}$th of the air compression diagram, and $\frac{1}{9}$th of the air expansion diagram, which latter represented the work of refrigeration which was accomplished. This result followed generally from the formula by Clausius just submitted by Mr. Thomson.

In the discussion of the Paper

"ON GUN-CARRIAGES AND MECHANICAL APPLIANCES FOR WORKING HEAVY ORDNANCE,"

By George Wightwick Rendel, M. Inst. C.E.,

Mr. Siemens * said it might have occurred to the minds of many that too much attention had been directed to the mechanical arrangements for gun carriages, and that the tendency ought rather to be towards introducing fewer elements in the working of a large gun, and more particularly in the working of ordinary guns. No doubt some mechanical appliances were required for moving shot from the hold of the ship, loading, and using the ramrod, in the case of very large guns ; but for ordinary gun practice he thought the machinery now proposed was of too complex a nature. In 1865 the laminar compressor, which had been perfected at Elswick, was coming into general use. It certainly was a most ingenious contrivance for multiplying the friction due to a moderate pressure, and for spreading it over a large surface, so as to produce a considerable aggregate amount of retardation without cutting action upon any portion of the surface, and without its being necessary to lubricate those surfaces. In 1867 he was invited by the Gun Carriage Department at Woolwich to advise them with respect to a project for improving that compressor. The contemplated improvement had in view to obtain the pressure between the laminæ of the Elswick compressor in a different way. It was suggested that perhaps, by setting up a powerful magnetic action, friction might be produced to any desired extent. He told the authorities that such a plan was feasible, but that he knew too much about the disappointments in the use of electricity to recommend such a plan for practical application ; and after consideration, he proposed the hydraulic reaction apparatus. His plan consisted simply of a cylinder with a piston and piston-rod connected with the gun, and a passage

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXXVIII. Session 1873–74, pp. 114–116.

covered with an elastic valve, in order that, as the recoil took place, there should always be the same amount of resistance per square inch throughout the stroke, and that the gun might come to an absolute stand the moment that amount of power was consumed. Colonel Clerk, with whom he had principally to deal, took up the idea warmly, and effected several modifications. He wished to do away with the tail-rod, which was unnecessary in his plan, in order to balance the area on both sides of the piston, and he accomplished this by filling the cylinder with water and air mixed. By that means an elastic resistance was opposed to the recoil action of the gun, and the first shock was greatly diminished. But this plan had its disadvantages; for, as the communicating orifice was invariable, it was never certain what amount of pressure would have to be dealt with, and as the thoroughfare was always open, the gun did not come to an absolute stop. The authorities at Elswick, who clearly perceived this drawback, had now introduced a plan not very dissimilar to that which he himself originally suggested, but going much farther than he then contemplated; for they proposed to drive the water by the recoil through a loaded valve into a separate cistern, whence it was to be forced by steam power into an accumulator to work the gun forward. This plan was also susceptible of the power of propulsion, a hand pump being applied to force the water from the front to the back of the piston: or the loaded valve might simply be raised while the gun-carriage was being pushed forward as usual. This was a matter of secondary importance: and he did not profess to judge whether it would be better merely to have the valve, and to push the carriage forward, or to complicate the arrangement by the addition of a pump. The pump would, he thought, be found the preferable agency on board ship, especially in working heavy guns. Colonel Clerk, in a pamphlet "On the Application of Hydraulic Buffers, to prevent the destructive effects of Railway Collisions," published in the year 1868, handsomely acknowledged the part which he had taken in the matter in the following words:—"In consequence of a suggestion made to me last year by C. W. Siemens, Esq., C.E., F.R.S., to try the effect of water to check the recoil of heavy guns, I submitted to the Secretary of State for War a compressor or buffer on the above principle. It has been tried with guns varying in weight from only 150 lbs. up to 18 tons, and in all cases the results have been most satisfactory." But he had received no other acknowledgment from the Woolwich authorities; and by degrees his connection with the subject appeared to have been forgotten, thus furnishing an illustration of Major Moncrieff's disappearing principle. Major Moncrieff had also, in 1868, recommended the use of hydraulic resistance, coupled with air, instead of balancing weight, in working out his beautiful principle, and this improved arrangement had been modified again by the Elswick Company. The discussion as to whether or not the recoil would always be sufficient to bring the gun up to its position might be safely left between the Elswick Company and Major Moncrieff. *Primâ facie*, it certainly ought to be sufficient. The gun in descending gave off the whole force due to its descent to some reservoir which might be provided to receive that store of force. In addition to this there was the recoil; and recoil and descending weight ought surely to be sufficient to raise the gun to its original height. On the other hand, it must be considered whether, in retarding the descent of the gun sufficiently, it would not be necessary to throttle the passages to such an extent as virtually to destroy the surplus power. On this point practice alone could decide; but, certainly, the power itself, if it could be made available and be stored up in a compressor, ought to be amply sufficient to raise the gun again to its former height.

In the discussion of the Paper

"ON THE FIXED SIGNALS OF RAILWAYS,"

By R. C. Rapier,

Mr. Siemens * said, it was now generally conceded that the block and interlocking systems were conducive to the safety and

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXXVIII. Session 1873–1874, p. 225.

development of railway traffic. Nothing could exceed the ingenuity displayed in the contrivances exhibited; but he observed that the electric telegraph was left out of the interlocking arrangements which had been brought forward. It was used only as an auxiliary to signal trains from station to station, but it formed no part of the interlocking system. In Germany and Belgium an interlocking system had been adopted lately with most satisfactory results, in which the three elements of the switch, the optical signal, and the telegraphic signal were combined into an automatic system; so that it was impossible for a train to leave a station, for the optical signal to be raised for its departure, and for the switch to be put right, until the telegraphic signal had arrived from the next station to say that the line was clear. He thought that no interlocking block system could be looked upon as safe and complete until it combined the three elements alluded to; and he was strongly of opinion that a block system, if adopted at all, should be made absolute and complete, and not permissive, as had been advocated in the course of the discussion.

In the discussion of the Paper

"ON DEEP-SEA SOUNDING BY PIANOFORTE WIRE,"

By Sir William Thomson,

Mr. C. W. Siemens * said: I may be allowed to make one or two observations upon this interesting communication which Sir William Thomson has made to us; and I would say, like many other mechanical arrangements which have been brought before us, this is not absolutely new, and I am not surprised to hear that attempts have been made to sound by wire instead of hemp line. But the merit of the present apparatus, as well as of any other well-devised mechanical arrangement, consists of the appliances to make the result a perfect one, and in that respect I think

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* Excerpt Journal of the Society of Telegraph Engineers, Vol. III. 1874, pp. 225–226.

the apparatus described this evening commends itself without any words from me. There are many difficulties which present themselves at first sight against the use of wire for soundings, but these have been met in the most perfect and ingenious manner. First of all, to get wire of such uniform strength as to reach to a depth of 3 miles required very considerable attention. Nevertheless, pianoforte wire offers extraordinary strength and toughness, and is, undoubtedly, the right material; but how to join these wires in such a manner as to be reliable was a matter of great consideration, and that difficulty has been met in the most perfect manner. Then the mode of checking the motion of the drum by a single rope, although in itself involving only a Prony brake, is a very ingenious mode of adapting a means to a particular end, and this is brought in usefully for telling in the most absolute manner when the weight strikes the bottom. As Sir William Thomson says, attaching the weight itself to a piece of line, and adjusting the friction in such a manner as that the motion of the machine is stopped the moment the lead reaches the bottom, is another stage in the perfection of this method of sounding. There are other points of great ingenuity in the apparatus now before us. With regard to the practical value of taking deep-sea soundings by wire I have no doubt. I have myself made deep-sea soundings, and I know that in depths of 2,400 or 2,700 fathoms it occupied from four to five hours, and it was a difficult matter sometimes to keep the ship over the line. The lateral friction of the line in the water was so great that the lead did not pull and therefore the ship had to be kept over the line. Instead of occupying five hours this apparatus completes a deep-sea sounding in about 35 or 40 minutes, and that is a matter really of the highest importance, especially in making soundings for submarine cables, where time is a great object. Flying soundings are matters of great interest. I did not quite follow Sir William Thomson's illustration. He shows that the lead touches the ground at a distance at least equal to the depth. I should have thought the point where it struck the bottom would be a distance from the stem of the ship not exceeding one-fourth part of the depth of the water, and the result would be that this (pointing to the board) would be 10 or even 50 per cent. longer than the vertical line. In this respect I think Sir William demonstrates against himself; but if we can lay down any certain rule this apparatus is a great achievement.

I have sent the wire sounding apparatus out with every ship I have had lately to fit for sea; and I am quite sure the meeting will accord a hearty vote of thanks to Sir William Thomson for his valuable communication.

In the discussion of the Paper

"ON COMPRESSED AIR-MACHINERY FOR UNDERGROUND HAULAGE," by MR. WILLIAM DANIEL,

MR. C. W. SIEMENS* observed that in the preceding discussion the question of the transmission of power by hydraulic pressure had been considered, and certain losses attendant upon that plan had been pointed out, while on the other side the great advantages of hydraulic power in admitting of direct application to the work had been duly appreciated. Another equally important question was that now brought forward—the transmission of power by an elastic medium. The application of air power must necessarily be quite different from that of water pressure, and might be resorted to with great advantage in cases where steam could not be used direct or where long steam-pipes would be objectionable; and among the numerous applications for which it was particularly suitable, the most prominent and useful was that of underground haulage, which formed the subject of the present paper. The advantages of air power for this purpose were self-evident: in a mine, surcharged as it was with heat, the use of steam would be attended with great inconvenience; whereas air, being rendered by expansion so much colder than the prevailing temperature of the mine, was the very medium required for such a situation.

The subject therefore resolved itself into the question whether the

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* Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1874, pp. 217–219.

transmission of power by air was attended with such losses as would render its application of doubtful advantage. This was a question of considerable interest, and one which could happily be dealt with in a very definite manner. Having had occasion some time ago to look into this question, the conclusion he had then arrived at was that in the ordinary mode of transmitting power by compressing air, cooling that air, and then letting it expand again, the attainable limit of the useful effect was about 50 per cent. of the power exerted in the compression. In the least favourable of the practical results obtained in the experiments described in the paper the useful effect was only about 25 per cent. of the power, implying a loss of as much as 75 per cent. The machinery, however, which had been employed in this instance appeared to him to be very far from perfect. Some mechanical imperfections in one of the air-compressing engines had indeed been pointed out by the author of the paper, the indicator diagram showing that the air was neither able when compressed to get out of the cylinder freely, nor before compression to get into it freely : two great evils which could easily be remedied by a proper construction of the air-valves.

But there was another defect, which was independent of mere mechanical construction. As there was no injection of cold water into the compressing cylinder, the compression curve developed in that cylinder was a dynamical curve, not following the simple hyperbolic line of Boyle and Marriotte, but rising in a more abrupt manner, owing to the accumulation of heat during the act of compression. It was well known that in compressing air the whole of the force exerted in the compression appeared in the form of heat, and this heat expanded the compressed air ; so that a much larger volume had to be expelled after compression had taken place, than would have been the case if the temperature had been kept the same as before compression. But the remedy for this loss was a very simple one, consisting merely in injecting cold water in the form of spray into the compressing cylinder, in sufficient quantity to keep the temperature practically uniform throughout the stroke. The saving of power thereby realized would be very considerable, because the air when compressed to four times its original pressure would be heated by 250° Fahr., and the consequent increase of volume would be about as 2 to 3, involving a loss of power of

33 per cent., which, if the precaution he had mentioned were taken, would be saved almost entirely. Again, in expanding the compressed air, the difficulty of getting rid of the ice formed in the passages of the expanding cylinder might be altogether surmounted if the water were injected into that cylinder at the ordinary temperature, or better still at the temperature of 80° or 90° commonly existing in the bottom of a coal mine. This water imparting its heat to the expanding air would prevent the formation of ice, and would produce precisely the same advantage as that obtained in the compressing cylinder by the injection of water; and these two savings together would very materially alter the result obtained in percentage of useful effect. The most perfect arrangement indeed, if it could be carried out, would be to take the very same water which had been injected into the compressing cylinder, and inject it again into the expanding cylinder, so that the heat taken from the air during its compression should be restored to it during its expansion. By that means, if the quantity of water injected were such as to keep the temperature practically uniform throughout the stroke, the whole of the loss at present arising from the heating and cooling of the air would be avoided, and there would be no loss of power beyond that due to the friction of the machinery and pipes. The injection of warm water into the expanding cylinder had not been made before, he thought; but with it air transmission might be accomplished without greater loss of theoretical effect than water transmission.

In the discussion of the Paper

"ON THE IRON ORES OF SWEDEN,"

By Mr. C. Smith.

Dr. Siemens * said that he had listened (as he believed everybody in the room had done) with very great interest to the paper which had just been read; there was, however, one point which

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* Excerpt Journal of the Iron and Steel Institute, Vol. I. 1874, pp. 320 and 325.

he thought was open to attack, viz., the suggestion that the deposits of hematite or other ore might have been aided by electricity. That proposition touched him rather closely as an electrician, and, therefore, he felt bound to respond to the call of their President to say a few words. He (Dr. Siemens) could not conceive any condition of things which would bring electricity into play in producing such deposits; the electric current caused a deposit of metal if it passed from one conducting surface to another through a metallic solution. But where would they find such conditions? The rock upon which the deposit of brown ore had taken place was not a conductor; the iron ore could not have been in solution, unless it had been a sulphate, and if it should, nevertheless, have been deposited electrically under conditions which they might have some difficulty in conceiving, but which, nevertheless, might have existed, the deposit would have been metallic iron, and not magnetic or peroxide. He thought they should be very slow in accepting a speculation which could not be brought into direct and tangible connection with electric science as it was actually understood. His inclinations were in favour of another theory regarding the origin of hematite ore, to the effect that it was solid deposit resulting from the denudation of red sandstone. If they imagined the state of the surface of their earth at a time when the water which now filled the ocean was still contained in a vaporous condition in the atmosphere, they must easily conceive what enormous power of dissolution must have existed; if they considered the water of the ocean to be in a state of vapour, the pressure upon the surface of the earth must have been equal to at least fifty of our present atmospheres, and the temperature of that water must have been fully 400 degrees.

The President: Centigrade?

Dr. Siemens: No, Fahrenheit. They knew that if they operated with a weak alkaline solution on flint under such conditions of temperature and pressure in a boiler that the flint readily dissolved, and as alkaline substances must have been contained in the water in an infinitely larger proportion than at present, the silica of the red sandstone must have been dissolved, liberating the oxide of iron which would be deposited more or less mixed up with other substances that had been mechanically carried away by the same current. Such a working theory would, he thought, account more satisfactorily for those irregular deposits which they found, particularly in the Barrow district. The somewhat analogous deposits of pipe-clay in Devonshire had been caused, he thought, through similar agencies in the decomposition of granite.

Dr. SIEMENS said, with regard to the observations that had just been made as to the dip of the needle over a field of magnetic ore, he thought it was quite natural and evident that the needle must dip more or less as they approached the one end or the other of the magnetic deposit. He should look upon a deposit of magnetic ore, between faults and its natural limits, as a magnetic needle polarised by the earth's magnetism ; one end of the deposit would, therefore, be positive, and the opposite end negative magnetic ; the one would, towards the south, be the north pole, and the other, towards the north, would be the south pole of the magnetic needle ; therefore, if they travelled over that deposit with a dipping needle, they would find its north pole dip down to one end, and the south pole to the other end, whereas, about the middle of the deposit, no dip would take place. This would explain Mr. Maynard's observation regarding the Lake Champlain deposits, and also the reference in Mr. Smith's paper to the Swedish lodes of magnetic iron ore, without adopting the conclusion that ore, over which the needle did not dip, was necessarily of a different constitution from other portions of the same over which the needle did dip, either with the south or north pole.

In the discussion of the Paper

"ON THE HELICAL PUMP,"

By Mr. JOHN IMRAY,

MR. C. W. SIEMENS * said, he was much struck with the novel mechanical idea involved in the helical pump of propelling the water by putting it as it were into a sling, and slinging it forwards. That this pump would compare favourably with the ordinary

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* Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1874, p. 290.

centrifugal pump he had no doubt, because, as pointed out in the paper, the water was not diverted from its course by abrupt changes of direction, and therefore very little power could be lost by eddies. The height of lift, as shown in the paper, was readily determined; for if the velocity of the pump were given, the height was known to which the column of water would rise to be in equilibrium, and half that height would in all probability produce the greatest amount of useful effect. It occurred to him that this pump would be extremely useful for lower lifts than other rotary pumps were generally employed for. As a turbine also he thought it would have certain advantages, inasmuch as the water in passing through a large wheel of that kind would be very little mutilated, and there would be very little loss in the way of eddies, provision being made for the water to expand into a larger channel before its final discharge, and it appeared to him that the construction admitted of almost unlimited extension in dimensions or number of turns of the helix, so as to utilize large amounts of water-power. He should like to know whether it had been so applied and with what results. It appeared to him also that if ever the idea of ship propulsion on Ruthven's plan of water jet should be taken up, this pump would particularly recommend itself as a propeller for such a purpose, because the water would not be taken into the ship in one direction and discharged in another after having its motion changed or even reversed, but it might be taken in at the front and expelled toward the stern in the same line, passing in a continuous course through the helical channel of the propeller. These were applications that occurred to him on first becoming acquainted with the helical pump; and he hoped to see many applications made of the principle, which appeared to him to be very novel and ingenious. With regard to the blades of the paddle-wheel, it had naturally occurred to him at first that it would be better to place these at an inclination across the face of the wheel, so as to be exactly at right angles to the curve of the helix, and this he thought would be the proper position theoretically; but as that inclination would produce an end pressure on the axis of the rotating wheel which would probably do as much harm as would counterbalance the increased efficiency obtained, it was no doubt preferable to set the blades parallel to the axis of the wheel, as shown in the drawing.

In the discussion of the Paper

"ON THE EXPEDIENCY OF PROTECTION FOR INVENTIONS," by F. J. Bramwell, C.E., F.R.S.,

Dr. Siemens, F.R.S.,* said he wished to be allowed to say a few words with regard to the Vienna Congress, which had been referred to at the last meeting, as he had taken a somewhat prominent part in connection with it. The idea of that Congress originated with Baron Schwartz Senborn, Chief Commissioner of the Vienna Exhibition, and invitations were issued to all nations, in the name of the Austrian Government, with a view to establishing international relations regarding the Patent Law. However, before the Congress assembled, the Austrian Government, like Frankenstein, became somewhat alarmed at their own creation, and the Congress, instead of being an official one, was simply an assemblage composed of manufacturers and others, especially the jurors who had attended the Vienna Exhibition, though Baron Schwartz still had the management of it. He (Dr. Siemens) was summoned to Vienna from Switzerland to conduct the business of this heterogeneous body, and amongst his duties was that of explaining if not translating the speech of any member of the Congress into any other of the four languages which were in use there. It was evident, therefore, that his position was not at all a bed of roses, and if in the end the Congress arrived at any resolutions which would stand the test of scrutiny, and form the basis for further efforts in the direction of an international relationship with regard to patent laws, he thought it might be said they had not met in vain. Mr. Webster, Q.C., who represented England at that Congress, worked most arduously in the endeavour then made to arrive at some reasonable conclusions, and he had since worked still more arduously in putting the transactions of the Congress before the English government in an intelligible form. He believed that a better law would shortly be introduced into the German legislature, which would compare favourably with that of

Excerpt Journal of the Society of Arts, Vol. XXIII. 1874-75, p. 101.

other countries; and in this country also he believed legislation might be expected from the present government. With regard to the English Patent Law, he might say that with all its faults he loved it still. But though its administration left much to be desired, there were elements in it extremely advantageous both to the inventors and to the public. One of the chief of these was that the tax was progressive, not by driblets, as was the case in France, but there were fixed periods during which the patentee might try to give life to his invention, and if at the end of the period it did not answer, he might relinquish the claim by declining to pay any further fees. The American Patent Law had certain advantages of its own, and he thought the preliminary examination a good institution, though both in America and Prussia it was carried beyond the limits of usefulness. One speaker had compared an invention to a reclamation of land on the sea shore, and he understood him to draw from it the reference that the right of an inventor was indefeasible. Now he was strongly opposed to the idea of indefeasible right, and taking the same idea he would say that though a man was entitled to the fruits of his labour in gain of ground from the sea, there might be circumstances under which it might not be desirable, from its effect upon the tideway or otherwise, to make the reclamation. He would, therefore, rather compare an invention to a new-born child, which might become a man of great power, but in its actual state was utterly powerless. The parent of the child had not only rights but important duties; and so with the patentee, he had a public trust to perform, he ought to carry the idea which presented itself to him into practice and give form and substance to his invention. For so doing he was justified in taking his share of the benefit which it might produce, but for a certain time only, after which it would be given over to the community. He thought this was the view put forward by Mr. Bramwell; and in conclusion, he congratulated all those interested in this important question on having had the advantage of hearing this most able address and the almost equally valuable discussion which had followed it, for both, he believed, would lead to most important practical results.

In the discussion of the Paper

"ON THE EROSION OF THE BORE IN HEAVY GUNS, AND THE MEANS FOR ITS PREVENTION; WITH SUGGESTIONS FOR THE IMPROVEMENT OF MUZZLE-LOADING PROJECTILES," by CHARLES WILLIAM LANCASTER, Assoc. Inst. C.E.,

DR. SIEMENS * observed that the oval bore which the author advocated in preference to grooves,—either furrows in the metal or projections from the cylinder of the bore, forming, as it were, broad grooves,—appeared to him, on general mechanical principles, open to serious objections; it was, in fact, a cylinder with two grooves, chamfered off in such a way as to present a surface most unfavourable for turning the shot. He could hardly imagine that a gun so grooved would keep its form after long usage. The wedging action, and the consequent friction on the side of the gun, must be enormous. He wished to ask whether experiments had been made comparing the effect of an oval gun with that of a grooved gun, as regarded the effective force of the gunpowder behind the shot. Another point on which he wished to remark was, that the author appeared to advocate a muzzle-loading gun. It had been stated on high authority that nearly all nations except England had now adopted the breech-loading gun. In this country breech-loaders were at one time adopted, but had since been abandoned in favour of muzzle-loaders; but he thought that breech-loading guns possessed great advantages over the muzzle-loading guns. All the proposals with regard to expanding wads were mere palliatives, in order to attain approximately the same effect from a muzzle-loading gun as could be easily obtained from a breech-loading gun. He was at a loss to understand why the breech-loading gun should have been abandoned,

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XL. Session 1874–75, p. 129.

and he believed that Sir Joseph Whitworth fully maintained its superiority. Practical difficulties would no doubt present themselves in the first instance, but these were not insuperable, and judging from the absence of the authorities who advocated the muzzle-loading gun, he was disposed to conclude that it was beating a retreat.

In the discussion which took place at the Special Meeting

"ON THE PATENT LAWS,"

Mr. C. W. Siemens[*] supported the resolution, and observed that the patent bill under discussion had done good in one respect, in showing to many somewhat impatient friends of the patent cause that there was a really valuable patent law in this country, which, though it might be susceptible of improvement in detail, contained important provisions that distinguished it from those of other countries. The opposition that had been raised to the provisions of the proposed bill had also shown so very plainly how difficult it would be to go on without patents, that it might be anticipated some other bill would be introduced at a future time which would not attempt to undermine the patent laws, but would be conceived with a view to improve them. In that case all friends of industrial progress would, he was sure, support the measure. The most difficult point for consideration was that of preliminary examinations; and looking to the working of the system in other countries, it was seen that in the United States it existed with a bias in favour of inventions, the legislature favoured the applicant, and if any abuses arose they were inherent in the system of examination combined with the power of rejection. In Prussia, on the other hand, there was a system of examination with a bias against the patent altogether. It appeared to him that under the provisions of the present bill the examinations

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[*] Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1875, pp. 179, 180.

would approach more nearly to those of Prussia; the Commissioners appointed would be instructed to seek for an excuse to refuse the application, rather than to try to modify the application in such a way as to give the applicant the benefit of a patent. The question of the best form of examination was involved in difficulty, and he must admit that he had not yet been able fully to satisfy himself about it. Examination was decidedly useful, if it stopped at the point where it gave the applicant information that was useful to him. In seeking a patent the applicant sometimes had an elaborate search made by his agent, which was naturally costly, and many an inventor would not be willing or able to incur the necessary expense of that examination. But the applicant had to pay a considerable sum of money to the Patent Office for procuring his patent; and it seemed very natural to propose to relieve him from the onus of having to make this search for himself, but to give him the information he desired for the fees he had to pay. If that plan were carried out, with the idea neither to baffle the applicant nor unduly to encourage him, but simply to give him such information as would enable him to adopt the correct course with regard to his invention, that would be an undoubted benefit. He suggested that it would therefore be sufficient for the examiners clearly to state what had been done and what had been proposed to be done, and so to warn the applicant what he had to avoid in his specification. There was not any occasion to go the length of endorsing a condemnation upon his patent, but simply to inform him of what was known and published, and was therefore to be avoided, without adding any advice as to proceeding or not proceeding with the application. Some such medium course might probably be the means of meeting the difficulty, which was a real one.

In the discussion of the Papers

"ON THE PNEUMATIC TRANSMISSION OF TELEGRAMS," by RICHARD SPELMAN CULLEY, M. Inst. C.E., and ROBERT SABINE, Assoc. Inst. C.E.; *and*

"ON EXPERIMENTS ON THE MOVEMENT OF AIR IN PNEUMATIC TUBES," by M. CHARLES BONTEMPS,

[Translated from the French by JAMES DREDGE.]

DR. SIEMENS* said it was exactly four years since a paper on the subject of pneumatic propulsion had been brought before the Institution by Mr. Carl Siemens.† The object of that paper was to describe a system of propulsions in tubes which had been matured by his brothers and himself in the course of years, it being a system of continuous flow, or a circuit system. This had been established in Berlin in 1864, in London in 1869–70, and in a modified form in Paris in 1871–2. The scheme had been submitted to the Postmaster-General several years previous to its application in London. The object was to despatch letters throughout the metropolis by a system of circuits, uniting in one or two common centres or pumping stations, whence parcels would be sent out every five minutes to a number of receiving and transmitting stations lying in a circle (similar in appearance to that shown on the diagram representing the Paris system). The current flowed round always in the same direction, conveying with it a succession of carriers passing from any one station to any of the others. The system differed materially from the former method, by which one carrier was sent through a tube in one direction, and went back by vacuum in the opposite direction. Sir Rowland Hill looked favourably upon the scheme, and he was indebted to Mr. E. A. Cowper, M. Inst. C.E., who was at that time frequently consulted on engineering matters by the Post Office,

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[*] Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XLIII. Session 1875–76, pp. 135–140 and 156–160.

[†] Vide Minutes of Proceedings of the Institution of Civil Engineers, Vol. XXXIII. p. 1.

and had previously worked on a similar subject, for his support in recommending its adoption. The present paper discredited, to some extent, the circuit system, for which it proposed to substitute a "radial system." He was not inclined, however, to accept the verdict of the authors of the paper, who, he believed, had not stated all the elements upon which this question should be judged. The circuit system, when first established between Telegraph Street, the General Post Office, Fleet Street, and Charing Cross, was considered a complete success; the postal authorities asked several scientific men and gentlemen connected with the Press to observe its results, and they were extremely pleased with them, but since that time there had been a disposition on the part of their engineeers to substitute the radial system. The first objection raised against the circuit system was, that no advantage was derived from it between Telegraph Street and Charing Cross, and that consequently the circuit had been broken up. Plate 29 represented the circuit as originally established. Pressure was maintained in one reservoir, and vacuum in another, and the flow of air was always in one direction, carriers being introduced at the points indicated on the diagram through switches of a simple construction. It would be observed that the circuit was a very oblong one, the intermediate stations on both halves being locally united for the convenience of the traffic. The alteration since made consisted in the removal of the arc connecting the two branches ending at Charing Cross, so that the air flowing from the pressure reservoir was discharged into the atmosphere, and the atmosphere introduced at Charing Cross flowed to the vacuum reservoir. It so happened, however, that the pressures marked at each station remained at every point of the circuit the same, Charing Cross being just half-way on each branch of the circuit; and, although he quite agreed that it might be convenient to take away the connecting link, and to work each half with the atmosphere inserted in circuit, it made no difference whatever in the principle of working. It had been intended originally to extend the circuit to Westminster; and if that intention had been carried out, the intermediate instruments at Charing Cross would have been indispensable. Although the present system was not worked as a circuit, it was worked on the same continuous method, and it would be observed that the postal authorities had adopted another similar open circuit between the General Post Office, Cannon Street, and Thames Street, which he had no doubt worked equally well, and went far to prove the advantages of the system. Another complaint was, that the iron pipes employed by him (Dr. Siemens) in laying down the Charing Cross Circuit were apt to rust in consequence of the use of injection water in the air-pump which had since been discontinued ; it was also stated that injecting water into the air-pump was accompanied by a waste of power. He entirely dissented from the latter proposition. He had prepared a diagram (Plate 29) showing the curve of compression : if a piston travelled in the cylinder, the pressure would rise, in the manner indicated by the dynamical curve, which compression was accompanied by a rise of temperature from 60° to 170° Fahr., in bringing up the pressure to double that of the atmosphere. By injecting cold water, not only was the cylinder lubricated as stated in the paper, but the heat was absorbed by the water, the result being that the increase of pressure would not take place in the ratio indicated by the dynamical line, but in that indicated by the other line, which represented the ratio of isothermal compression. Injecting water therefore was not a source of loss of power, but of gain of power. Probably the quantity of water injected had been too small, and in that case no doubt vapour would be carried over into the reservoir. The postal authorities had done away with the reservoir altogether, which, he thought, was a mistake, because it was necessary to allow the water to settle and the air to become dry and cooled down to the point at which it was fit to enter the pipes. As regarded rusting, the authors themselves stated that in Paris, where the air was compressed by water, no inconvenience had been observed on that score, nor had any such effects been experienced in Berlin. If rust had given trouble in the circuit in question, he considered it was entirely due to the mode of working. No doubt a lead pipe was better in some respects for small diameters, but when he designed the circuit, cost was a very important element. He could not afford to have lead pipes inside cast-iron, the cost of which might suit the Post Office, now that the authorities were accustomed to spend their millions somewhat freely ; but at the time to which he alluded they were in the habit of going closely into estimates. No doubt it would have been better to line the inside of the pipes with softer metal, such as tin or lead, in order to obtain a smooth working; and he had proposed to tin the inside of the pipes, but that had to be negatived because it would have been too costly. He should have been glad of the opportunity of comparing the estimates of the two descriptions of pipe, believing, as he did, that one would cost several times as much as the other. Another objection raised in the paper against the circuit system had been that time was lost at the intermediate stations. He did not, however, see the force of that objection. It was very important that the time of transit from the central station to the extreme end of the system should be as short as possible; but there could be no practical object in shortening the times of transit to the intermediate stations. He would take the case of the second continuous circuit established in London. It had been stated that, in working the circuit from the central station to Cannon Street and Thames Street continuously, the time of transit from the central station to Cannon Street was sixteen seconds more than when the latter was worked as a terminal station, the times of transit being seventy-two and fifty-six seconds respectively. That might be so; but he thought it was rather an advantage than otherwise to retard the flow in so short a tube, and the intermediate station in Cannon Street did not in any way diminish the speed of the flow from the central station to Thames Street. If the two were worked as separate continuous circuits, more than double the air would be consumed as compared with that required to work the three stations on the circuit system. If it were so desirable to diminish the time of transit, it would be much better to increase the diameter of the pipe. In that case there would be an advantage for both stations in point of speed and working capacity, and engine power would at the same time be saved. He thought, therefore, that the objection raised against the intermediate station did not hold good. Another objection was that the iron pipe caused more friction than the lead tubes. No doubt there was a little more friction to the carrier; but, seeing that this constituted a very slight amount in the total friction of the transit of air through the pipe, it was not a serious matter, and could have been avoided if the inside of the iron tube were simply covered with a soft metal.

He had certain objections to make to the theoretical part of the paper. The authors started with Zeuner's formulæ, expressing the dynamical effect produced in allowing air to expand from one pressure to another. They presumed that if the air flowed into a long tube, it expanded in the same manner as if it were allowed to push a working piston forward, which, however, was not the case. If compressed air were allowed to re-expand behind a working piston the temperature would fall in precisely the same ratio in which it would rise in compression, the heat lost being the equivalent of the force communicated to the piston. But was it the same if air expanded into a long pneumatic pipe? Certainly not. There was in that case no working piston with resistance behind it, the carrier piston consisting only of a piece of hose containing some slips of paper, which offered practically no obstacle. All the resistance that had practically to be dealt with in the pneumatic pipe was that of the air itself. Suppose air of 2 atmospheres pressure were admitted at one end of the pipe (which might be one mile or three miles long), the pressure would taper down to atmospheric pressure at the opposite end. No work was accomplished here, except that exerted upon the air itself in being pushed through the tube, which, therefore, became the recipient, in the shape of heat, of all the force which had been exerted, and the result was that the expansion of the air from two atmospheres to atmospheric pressure would not be accompanied by any decrease of temperature. Therefore the dynamical formulæ regarding the force and volume of air expanding behind a working piston did not apply to the case of a pneumatic pipe. Assuming that the pipe itself was a non-conductor of heat, and that the temperature of the air on entering the pipe was the same as the temperature of the pipe itself, he maintained that the air would flow out of the other end of the pipe at exactly the same temperature as that at which it entered. Taking the case of a pipe of conducting material, and assuming that the air entered the pipe at two atmospheres pressure and at the temperature of the pipe itself, the temperature at which the air left the pipe must be in excess of that of the compressed air when it entered, inasmuch as the latter had work to perform; it had to push forward the air and overcome its friction against the side of the tube; and inasmuch as work was performed in the early part of the operation, the temperature of the air would diminish. Heat would be communicated from the tube to the expanded and cooled air; but towards the end of the transit no work, excepting friction, had to be performed and all the heat that had been picked up by the air in the early part of its transit would appear in the form of additional free heat at the end. After this explanation, he hoped that the authors would agree with him that the co-efficients in their formulæ, taken from the dynamical action of expanding air, were not applicable. It might be mentioned that the experiments given at the end of the paper exactly confirmed his view. In other respects the theoretical considerations involved in this subject had been put forward in a complete and elegant manner, and some of the experimental results were extremely valuable.

Regarding a comparison of the radial with the circuit system, he believed that the advantage was with the latter. The radial system implied a greater number of tubes; and it was, therefore, wasteful in point of cost. It implied, if the radii were worked on the continuous system—which was almost necessary where there was so large a traffic as in London—a greatly increased consumption of compressed or rarefied air, as the case might be. Moreover if there were, say, twenty or thirty stations round the central station it would be practically impossible to lay as many tubes radiating from one centre, each tube consisting of a leaden pipe surrounded by an iron one. The streets would not be sufficient to contain such a number of tubes. Although the radial system might do for collecting messages from offices in the immediate neighbourhood of the central station, he felt sure that whenever the time came for the establishment of the pneumatic despatch system on a large scale, requiring larger diameters and a combination of hundreds of stations (so that a parcel could be sent from any one station to any other), it would be impossible to carry out such an object by the radial system, and a return to the circuit system would be absolutely necessary.

DR. SIEMENS said he desired to congratulate the Institution upon the very lucid explanation and scientific exposé of Professor Unwin, with every word of which he agreed. He had already discussed Mr. Sabine's paper, but now proposed to offer a few remarks on the theoretical principle involved in M. Bontemps' communication. An interesting account had been given of experiments to determine the velocity of carriers in pneumatic tubes by electrical markers, with records of the observations on a chronograph. The results thus obtained must, he thought, be accepted as indisputable ; but he was inclined to doubt some of the generalisations attempted in the paper. It was perfectly true that when two carriers followed one another in a tube worked by a continuous current, the time occupied by each carrier in traversing the same section of the tube from one marker to another must be the same, because the current flowing through the tube was always the same ; but it did not follow that the absolute speed, the number of feet traversed per second, should be the same in each portion of the tube. M. Bontemps appeared to have found that that was substantially the case—that after a short period of acceleration, the speed of the carrier fell into a uniform rate until almost the very end of the journey, when it again increased, and he stated that these results seemed to verify Fournier's theorem, according to which "equal impulses given throughout the journey of an accelerated body must produce the same velocity." These results did not coincide with the common-sense view of an elastic fluid expanded behind a light working piston, but he thought that an explanation of the experimental results was nevertheless possible. The air, say of 2 atmospheres pressure at one end expanded down gradually to atmospheric pressure, and the same index of air between the two carriers must elongate as the carriers went along ; and expansion must take place throughout the course because working power was required at every point. But in taking the case of a carrier not fitting the tube entirely, and yet causing some friction against the sides, he should expect the results which were stated in the paper. In that case the impulse given to the carrier in the tube would be carried by the rush of air past it, and this would be the same throughout, and there would practically be the same power active to overcome friction at every step of the course. The result would be a uniform speed for the chief part of the course, till the very end, when the rush of air past the piston would greatly increase. It was to be hoped that the author would continue his observations with the appliances he had made in order to obtain further information on the interesting subject of gaseous friction in long tubes. An explanation had been attempted by Mr. Preece, of the apparent sluggishness of the air to expand throughout its course, by the fact that the medium was not pure air, but air mixed with vapour of water, which mixture would follow another law of expansion than that of either fluid taken separately. He dissented entirely from that view of the case. He had shown, and Professor Unwin had quite confirmed that view, that the air expanded isothermally—that both air and vapour would pass through a tube without altering in temperature; therefore no condensation of the vapour would take place; and as vapour and air both followed the law of Mariotte in precisely the same manner, there could be no difference whether dry air was used or air containing a slight proportion of vapour.

In advocating the use of the radial system in preference to the continuous or circuit system, Mr. Preece said that he had travelled over the continent of Europe with a view of ascertaining the working of those systems elsewhere; and that, while he found the radial system established in Brussels, he ascertained that at Berlin the circuit system, which had been adopted in 1863, had failed. This was startling news to him; because, although he had never described the system as established at Berlin, he had referred to it, and his brother also had referred to it, in his paper, as an historical step towards the accomplishment of the circuit or continuous system as established by them in London. He accordingly wrote to Berlin for information, and he had ascertained that, so far from the system having failed there, it had been during the last twelve years in uninterrupted operation, and that the only thing that could be construed into a partial failure was the circumstance that after the one circuit from the telegraph office to the Bourse had been established, a second circuit from the telegraph office to the Brandenburg Thor was added, and it had been found that the boiler power was not sufficient to work both systems continuously together. For a time, therefore, and probably at the very time when Mr. Preece paid his visit to Berlin, the one system was shut off when the other was worked between the telegraph station and the Exchange during the busy part of the day. With that exception, which he understood had since been set right by the addition of boiler power, the system had been working precisely in the same manner as it had been established twelve years ago, and it had given no cause of complaint nor inconvenience in the working. Mr. Preece further stated that the cost of the iron pipes, in connection with the circuit system as established in London, was at any rate higher than the cost of the system of tubes advocated by the Engineers at the Post Office, and that his (Dr. Siemens's) firm charged for the iron pipe at the rate of 15*s.* per yard, whereas another contractor had laid lead pipes at a rate of 13*s.* 8*d.* He would not dispute those figures, but Mr. Preece had fallen into the error of making, no doubt unintentionally, a very unfair comparison. In the first place, he compared a 3-inch tube with a tube much less in diameter ; he was not quite certain whether it was a $1\frac{1}{2}$-inch or a $2\frac{1}{4}$-inch tube that he referred to as having been laid for 13*s.* 8*d.* He also compared a mere tube which had been laid in connection with an established apparatus, with the system of tubes and instruments, carriers and other matters, required to constitute a complete circuit system. In the one case the instruments, carriers, and station fittings were not included in the estimate, and in the other they were included. There were also to be added in the case of the circuit system the engineering and general expenses which fell upon his firm in designing, making, and laying down the new system in London. He was employed as Engineer of the Post Office in designing not only the tube, but also the engines, boilers, reservoirs, and pumping machinery to work the system, and the contracts were let to three firms :—Messrs. Easton and Amos, who made the engines and pumping apparatus ; Messrs. Aird, who laid the tubes and completed the earthworks ; and Messrs. Siemens Brothers, who made the other mechanical arrangements. It should also be stated that as the system had been matured by his firm at great expense, and patented, they had a perfect right to superadd to their cost a reasonable amount for patent right. Including all the charges the Post Office paid for the first circuit the sum of £5,212, which was at the rate of 15*s.* per yard ; but of this sum £2,900 were paid for the tube and the earthwork, including Mr. Aird's profit on the latter, all the rest being taken up by other work. Thus the figures for comparison were 8*s.* 4*d.* per yard for a 3-inch iron pipe, as against 13*s.* 8*d.* per yard for a lead tube of about half that area, which figures fully justified, he thought, his former argument. Mr. Preece likewise stated, that although the continuous or circuit system of working might be suited for such places as Paris, Vienna, and Berlin, it would never do for London, where speed was a principal object. He should be very sorry to have put forward for London a system that was not capable of the greatest development of speed, knowing as he did the value of time. But Mr. Preece, in describing the advantages of the radial system, seemed to forget that the two principal distances worked by the Post Office at the present time were worked on the continuous system, in exact accordance with the principles laid down by himself. All that had been done in the first circuit laid down by him was to take out about 3 yards of pipe at the neutral point at Charing Cross. One branch was worked by pressure, the other by a corresponding vacuum, and at the extreme point the pressure was neutral, so that the connecting link between the two sides might be taken out with impunity without altering the system in the least. The only difference would be that instead of bringing the same air back to Telegraph Street or to the General Post Office, there would be air which had travelled through the instrument room at Charing Cross and which had taken up a good deal of vapour from the numerous persons engaged there, giving rise, probably, in a measure to the inconvenience of rust in the iron tubes; an inconvenience which had not made itself felt in Paris, Vienna, or Berlin, where iron tubes were used. He thought that with proper care that might be completely prevented in London. He admitted that it would have been expedient—indeed, he proposed it at the time—to have the inside of the iron tubes tinned, which would have given all the advantages of the lead tube coupled with the comparative cheapness of iron tubes. Mr. Preece seemed to imply that a circuit system of iron tubes was a roundabout system by which, in order to get from Charing Cross to Telegraph Street, it would be necessary to go round by Islington. That was not the case, nor had he proposed any such thing in laying down the first circuit between those places. The continuous system, if worked in circuits, could be so arranged that the distances between the two principal points on the circuit would be minimum distances, even though the intermediate stations might be a considerable distance apart. If a tube were established on the circuit system between Great George Street and the City, one branch might pass by the Strand, or the Embankment, and the other over the bridges through Southwark: both would be equally near, and the intermediate stations upon the two branches would be a considerable distance from each other, and be thus accommodated by pneumatic communication without increasing the time of transit between the principal stations, and without involving an extra consumption of air or power. On the whole, he thought that the radial system was well adapted for very short distances, and for very light carriers. If the object was to collect telegraphic messages from the streets immediately adjoining St. Martin's-le-Grand, it would be absurd to speak of establishing a circuit system, and Messrs. Clark and Varley had established that communication in a very efficient way. But whenever it was desired to carry pneumatic communication beyond those limits, to extend it over considerable spaces, so that not only a few offices in the City, but the whole of the metropolis might derive benefit from it, it would be absolutely necessary to resort to some such system as he had advocated.

In the discussion of the Paper

"ON THE VENTILATION AND WORKING OF RAILWAY TUNNELS,"

By Gabriel James Morrison, M. Inst. C.E.,

Dr. Siemens [*] remarked that the plan proposed by Mr. Barlow was ingenious; but it would be purchased at the cost of two lines of valve, which would be a serious consideration, though not presenting an insuperable difficulty. Another plan had some years ago occupied his attention. When the Metropolitan line was in course of construction he was consulted, through Mr. Fowler, Past-President, as to some means of preventing the emission of the products of combustion, and he then proposed a plan by which an ordinary locomotive might run through the tunnel without being

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[*] Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XLIV. Session 1875–76, pp. 67–68.

accompanied by the emission of any such products. The plan consisted simply in filling the fire-box with a clamp of bricks so arranged that between the masses of brick innumerable channels were provided through which air might circulate and rob the brickwork of its heat in order to communicate it to the boiler. It might appear, at first sight, that the quantity of brickwork required for such a purpose would be enormous, but a little consideration would show that was not the case. Firebrick had a heat capacity about equal to that of water of the same volume ; therefore if a cubic foot of brickwork was heated 1° that heat would suffice to heat a cubic foot of water 1°. But the brick had the advantage of being susceptible of being heated 1,000° above the standard which it must retain to evaporate a cubic foot of water. The question was, how many cubic feet of water were required to be evaporated, in passing from Dover to Calais. Taking as the rate of evaporation 5 cubic feet per mile, and taking the length of the tunnel as 30 miles, that would give a total evaporation of 150 cubic feet of water, and the brickwork necessary to yield that amount of heat would be 150 or 160 cubic feet, which would weigh about 17 tons, constituting a mass 6 feet by 6 feet by 10 feet, if one-half of it was allowed for air spaces. He proposed to heat the brickwork at the station in a furnace, and to lift it bodily, being clamped with iron, into the fire-box of the engine, where it would be fixed by bolts. Currents of air would be directed through it at the place of the ordinary fire-door, which, passing through the tubes of the boiler, would produce steam. At the end of the journey the engine would go over an empty pit, where the mass of brickwork would be lowered by an hydraulic ram, to be heated again for another journey. The boiler would, at the same time, be filled with water at 300° Fahr., so that evaporation would at once commence under the most favourable conditions. If the proposed scheme of the Channel Tunnel were ever carried out, he thought this plan ought to be fully considered, because he saw no difficulty in carrying a store of heat sufficient to take a locomotive engine from Dover to Calais, whereby mechanical ventilation would be saved and a wholesome atmosphere insured in the tunnel.

ON DETERMINING THE DEPTH OF THE SEA WITHOUT THE USE OF THE SOUNDING-LINE.

By C. W. Siemens,* F.R.S., D.C.L., M. Inst. C.E.

Introduction.—It occurred to me some years ago that the inferior density of sea-water as compared with solid rock, such as that composing the crust of our earth, might be taken advantage of to devise a method of determining the depth of sea below a vessel. If an instrument could be constructed which, when suspended on board ship, would indicate extremely slight variations in the total attraction of the earth, those indications might be referable to the depth of sea, and a scale be obtained whose divisions would give the depth in fathoms, or other units, without having recourse to the laborious process of sounding by means of the sounding-line.

Terrestrial attraction : Newton.—Our knowledge regarding terrestrial attraction dates from Newton, who proved that "the attraction of a spherical shell on an external particle is the same as if the mass of the shell were collected at the centre," and that the earth might be considered as consisting of an aggregate of such shells. Bearing in view, however, the fact of the earth's rotation, he proved its ellipticity, and that partly in consequence of that form, and partly on account of the centrifugal force engendered by its rotation, the total attraction of the earth in reference to a point on its surface must vary with latitude.† He determined the ratio of increase on the supposition that the earth is homogeneous, and showed that it varies as the square of the sine of the latitude. It is actually represented by the formula $g = g' \ (1 + {\cdot}005133 \sin^2\lambda)$, in which g signifies gravitation at a place in latitude λ, and g' $(=32{\cdot}088)$ gravitation at the Equator.

Recent researches : Stokes and Airy.—The recent researches by Stokes and others have shown that these determinations are correct only approximately, and that the actual total attraction of the earth at any one point, even if taken upon the

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* Excerpt Philosophical Transactions of the Royal Society, 1876, pp. 671-692.

† Newton's "Principia," Book III. proposition xx. problem iv.

sea-shore, is influenced by the rising land of continents, or by cavities in the interior of the earth. He also established a reason for an observation made previously by Airy, that total gravitation is greater on an island than it is near the sea-shore of a continent, and greater on the sea-shore than on an estuary inland.*

EMPLOYMENT OF SECONDS PENDULUM.—The seconds pendulum has been the instrument employed in all cases to determine variations in the total attraction of the earth upon its surface, this being the method first proposed and adopted by Newton.

SPIRAL SPRING PROPOSED BY HERSCHEL.—Sir John Herschel has proposed to use instead of the pendulum a weight attached to a spiral spring, and he has shown that with increase of the force of gravitation, the spring must be proportionately elongated. Sir John Herschel writes, that "the great advantages which such an apparatus and mode of observation would possess, in point of convenience, cheapness, portability, and expedition, over the present laborious, tedious, and expensive process, render the attempt to perfect such an instrument well worth making." † It appears, however, that this proposal by Sir John Herschel has never been practically realized, and that, indeed, no serious attempt has been made to construct an instrument of such delicacy as to show statically minute variations in total gravitation, notwithstanding the great oscillations to which a weight so suspended would be liable, and notwithstanding the influence of changes of temperature and atmospheric density.

GENERAL CONDITIONS.—Neither the pendulum nor the apparatus suggested by Sir John Herschel would be applicable to the measurement of the height of a mountain or plateau above the sea-level, owing to the considerable error which would be caused by changes in gravitation, through the local attraction of the mass of the mountain itself above the horizon, nor would either instrument be serviceable on board ship for obvious reasons. But if an instrument could be devised which would be capable of indicating extremely slight variations in the total gravitation of the earth, subject only to comparatively slight causes of error, it would be found, I contend, that these indications would vary with the varying depth of water below the instrument, in such a definite

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* Cambridge's Philosophical Transactions, Vol. VIII. pp. 672–695.

† Herschel's "Astronomy," Cabinet Cyclopædia, foot-note, p. 125.

ratio as would render it possible to construct a working scale, the divisions of which would represent depth of water.

Attraction Influenced by Depth of Water: General Statement.—The reason why the total attraction upon the surface of the ocean must be less than on the shore, is evident from the fact that the density of sea-water is nearly three times less than that of such calcareous, siliceous, and aluminous rocks as constitute the principal portion of the crust of the earth; and it is also evident that, although the total mass of the earth, and the distance of the instrument from its centre remains the same in gliding along the liquid surface of the sea, the total gravitation must be influenced in a greater measure by the mass near at hand, and that in proportion to the thickness of the layer of the substance of inferior density the total gravitation must be affected.

Ratio of Decrease of Gravitation with Depth.—The ratio of decrease depends, in the first place, upon the ratio of the density of sea-water to that of solid rock. The mean density of sea-water may be taken at 1·026, and the density of the rock composing the crust of the earth may be taken to be the mean of the following densities:—

Mountain limestone	2·86
Granite	2·63 to 2·76
Basalt	3·0
Red sandstone	2·3 to 2·52
Slate	2·8 to 2·9

Average density of above 2·763 nearly.

It is dependent, in the second place, upon the total gravitation of the earth in reference to a point on its surface, and upon the influence exercised in that general result by the strata of matter in the immediate vicinity of that point.

Mathematical Investigation.—In Plate 30, Fig. 1, the circle represents the circumference of the earth, which I propose to consider for the present irrespectively of its rotation, and as being spherical and of uniform density.

Let P be the point upon the surface of the globe where the attraction is to be measured; then, in order to calculate the amount of variation that will be produced in the total attraction of the earth, supposing it to be of uniform density, by a given depth of water below the attracted point P, a line is drawn from that point to the centre of the earth, and the same is divided into an unlimited number of indefinitely thin slices, by planes perpendicular to that line.

In taking one of these slices at the distance h from the attracted point, an expression is obtained representing its aggregate attraction, thus—

The slice is composed of concentric rings of sectional area $dh\,.\,dx = dh\,.\,z\,.\,d\alpha : \cos\alpha$, and of the capacity $2\pi\,.\,z\,.\,\sin\alpha\,.\,dh\,.\,z\,.\,d\alpha : \cos\alpha$, which gives $\dfrac{2\pi\,.\,z\,.\,\sin\alpha\,.\,dh\,.\,z\,.\,d\alpha}{z^2}$ as the differential of the attraction, where z and z in the numerator and z^2 in the denominator, although variable quantities, always vary together, or $ddA_1 = 2\pi\,.\,dh\,.\,\sin\alpha\,.\,d\alpha$.

This expression has to be integrated between the limits of h and 0, and α and 0 ; thus—

$$\int_0^h \int_0^\alpha 2\pi\,.\,dh\,.\,\sin\alpha.\,d\alpha = \int_0^h 2\pi dh \int_0^\alpha \sin\alpha.\,d\alpha = 2\pi\int_0^h dh\,(1-\cos\alpha).$$

Since

$$\int_0^\alpha \sin\alpha.\,d\alpha = 1 - \cos\alpha,$$

also

$$\cos\alpha = \frac{h}{z} = \frac{h}{(x^2+h^2)^{\frac{1}{2}}} = \frac{h}{\{(2\mathrm{R}-h)\,h+h^2\}^{\frac{1}{2}}} = \frac{h}{\sqrt{2\mathrm{R}h}} = \frac{\sqrt{h}}{\sqrt{2\mathrm{R}}},$$

$$\therefore 2\pi\int_0^h dh(1-\cos\alpha) = 2\pi\int_0^h\left(1-\frac{\sqrt{h}}{\sqrt{2\mathrm{R}}}\right)dh = 2\pi h - 2\pi\int_0^h \frac{\sqrt{h}}{\sqrt{2\mathrm{R}}}\,dh$$

$$= 2\pi h - 2\pi\frac{1}{\sqrt{2\mathrm{R}}}\,.\,\tfrac{2}{3}\,.\,h^{\frac{3}{2}} = 2\pi h\left(1-\tfrac{2}{3}\sqrt{\frac{h}{2\mathrm{R}}}\right) = \mathrm{A}_1 \quad . \quad (1)$$

is the total attractive force exercised by the uppermost portion of the globe to the depth h.

For small values of h, the expression $\sqrt{\dfrac{h}{2\mathrm{R}}}$ may be neglected, and the formula may be written

$$\mathrm{A}_1 = 2\pi h \quad . \quad . \quad . \quad (2)$$

In substituting 2R for h in formula (1) we obtain $A = \frac{4}{3}R \, . \, \pi$, the expression for the total attraction of the earth, which was determined by Newton; a verification is thus furnished of the correctness of the above calculation.

The proportion between the attraction exercised by the upper segment and the whole earth, supposing them to be composed of uniform material, is therefore as $A_1 : A = 2\pi h : \frac{4}{3}R\pi$, or as $h : \frac{2}{3}R$.

Ratio of variation of Attraction, as the depth to the Earth's Radius.—If sea-water had no weight, the total force of gravitation at the point P would be diminished in the ratio $\frac{\text{depth of sea}}{\frac{2}{3}\text{ radius}}$; but, inasmuch as the ratio of the difference of mean rock and sea-water to mean rock is $\frac{2\cdot763 - 1\cdot026}{2\cdot763} = \frac{1\cdot737}{2\cdot763}$, it follows that the real influence of depth, on the supposition of the earth's density being throughout that of mean rock, would be represented by the expression

$$\frac{\frac{1\cdot737}{2\cdot763}h}{\frac{2}{3}R} = \frac{h}{\frac{614}{579}R} = \frac{h}{1\cdot06\,R},$$

or approximately as the depth to R.

Thus, for a depth of one thousand fathoms, gravitation diminishes by $\frac{1}{3691}$ of itself.

Necessity for modifying result, neither Compression great enough to be sensible in its effect, but the two not equal.—The rock composing the crust of the earth will be under compression, and therefore denser at the depth corresponding to the depth of sea; but sea-water itself will increase in density with depth in a somewhat similar ratio, so that the comparison between sea-water and solid rock remains virtually the same for all depths. The greater density of the earth towards its centre will, however, greatly influence the measure of this dependence as established by the foregoing calculation; but in constructing a measuring instrument it will be safer to rely upon the result of actual measurement, in the absence of reliable information regarding the increase of density towards the centre, by comparing its indications with those obtained by means of the sounding-line. It may here be remarked, however, that the indications of variation of gravitation with variation in the depth of water, which have been obtained by the use of the instrument, show in excess of what the above calculation gives with the mean density of the rock composing the crust of the earth as a factor, and agree more nearly with what would result if the upper strata of the earth were of a density equal to the mean density of the whole earth. Actual observations, as given in the Table further on, confirm, in a remarkable degree, the arithmetical ratio of decrease of gravitation by depth which results from the foregoing calculation.

First attempt to construct a Bathometer.—Several years ago I constructed an instrument in which the gravitation of the earth was represented by a column of mercury in a glass tube closed at its upper end, and resting upon a cushion of air enclosed in a large bulb, which air, when kept at a perfectly uniform temperature, represented uniform elastic force unaffected by gravity or atmospheric density. The principal difficulty that presented itself in designing a workable instrument on this principle, consisted in obtaining a scale sufficiently large to show such extremely slight variations in the total gravitation of the earth as would result from ordinary variation in the depth of water. From the calculation given under the previous head, assuming the mercury column to have a height of 760 millims., each fathom of depth of water would represent a variation of potential force in that column equal to a height of ·0002059 millim., a quantity which it would be impossible to show on any scale. A scale would in reality not even realize this quantity of decrease in the upper surface of the column, because a portion of the adjustment of height would take place in the air-bulb below, partly from the rise of mercury into the bulb, and partly through increase of pressure of the imprisoned air due to its compression. I succeeded, however, by means of an arrangement of the instrument with three liquids of different densities, in increasing the effect of a change of gravitation upon the mercury column three hundredfold, whereby a change of 10 fathoms depth would be represented by a movement of ·6177 millim. of the boundary between the two liquids in the vertical tube, a quantity sufficiently large to be appreciated in the divisions of a scale. This instrument is shown in Plate 30, Fig. 2.

Tests of Instrument.—This instrument was tested by me in

1859 on board H.M.S. "Firebrand," commanded by Capt. Dayman, during a trip undertaken for the Admiralty for the purpose of determining a line of soundings across the Bay of Biscay, with a view to the establishment of a submarine cable: it proved successful to the extent that I was able to predict, approximately, the depth that would be found on the use of the sounding-line. The difficulty, however, of observing the instrument was great, owing to the excessive pumping-action, the consequence of the oscillations of the ship, as well as to the difficulty of obtaining perfect uniformity of temperature. The method of observation pursued was to take series of ten observations of alternate maxima and minima positions of the film, or boundary line between the liquids, of which the mean was taken to be its true position upon the instrument; but occasionally oscillations of extraordinary amount occurred, tending to vitiate the value of even these means. The instrument was both bulky and delicate, and it was found impracticable at the time to provide the ship with a sufficient store of ice (to be used in maintaining the instrument at a uniform temperature) to last during a lengthy voyage. In consequence of these drawbacks, I relinquished for a time the idea of constructing a reliable bathometer.

Present construction of Bathometer.—Last year the practical difficulties encountered in laying submarine cables in water the depth of which had not been accurately ascertained beforehand, revived in me the conviction that an accurate instrument would be of considerable value, not only to the cable-layer but to the navigator generally, when unable to determine his position astronomically. In the instrument about to be described, the mercury column is retained as the representative of the force of gravitation, but the balancing force is obtained through two spiral springs, which are so adjusted to the force of the mercury column that changes of temperature are entirely eliminated from the result.

The instrument, which is represented on Plates 31, 32, consists of a tube of steel, with cup-like extensions at the two extremities, which is suspended in a vertical position from a universal joint, at some little distance above the centre of gravity of the system, with a view of preventing pendulous action.

The upper cup-like extension of the tube is closed with a lid, provided with a closed stopper, which is screwed down when the instrument is not in use, and released for the access of atmospheric pressure shortly before observations are about to be taken. The lower portion is closed by means of a thin diaphragm of corrugated plate of steel, similar to the corrugated plates used in the construction of aneroid barometers. The centre of the diaphragm rests upon a crosshead, to which two carefully tempered steel springs are attached, which pass upwards on opposite sides of the mercury column, and are held at the upper extremities by adjusting-screws in the sides of the upper cup. The neck of the vertical pipe where it opens out into the upper cup is nearly closed by means of a disk or stopper of steel, perforated by a hole of only ·2 millim. diameter, the object being to reduce the pumping-action on board ship to a minimum. Before screwing-in this stopper the tube is filled with boiled mercury up to about the middle of the upper cup.

Available Force.—The mercury column represents the potential of force resulting from the area of the lower cup, multiplied into the height of column and the density of mercury.

The instrument of which the results have been chiefly recorded in the Table given further on has cups of 90 millims. in diameter and a height of mercury of 600 millims., representing an available force of 51·9 kilogrammes susceptible to variation in gravitation; whilst the instrument of which the drawing is given has cups of 50 millims. diameter and a mercury column of 500 millims., representing an available force of 13·35 kilogrammes. These amounts are amply sufficient to overcome by their variations any slight frictional resistance in the liquid column or in the diaphragm. But this frictional resistance is really eliminated from consideration by oscillations of the vessel, which cause certain pumping-action (kept within narrow limits by the contracted orifice), and bring the diaphragm into the true mean position, notwithstanding slight frictional resistances.

Range of Scale.—Under this head we have to consider what will be the effect on the instrument by a given change in the total attraction. Assuming a diminution of gravitation equal to say $\frac{1}{370,000}$, representing about 10 fathoms of depth, this would be equalized by a reduction in the height of column of $\frac{600}{370,000}$ millim. = ·00162 millim. The column of mercury in rising under this changed condition of equilibrium will, however, not become shortened, as in the case of the barometer when affected by a diminution of atmospheric pressure, or as was the case in the instrument before described, but for every fraction of a millimetre which the top level rises the centre of the diaphragm will rise also, and in an increased ratio, depending upon the proportion of the diameter of the solid central portion of the diaphragm to the diameter of the cup. If the central solid part of the diaphragm was only a point, it is easy to see that for every fractional rise of the mercury in the upper cup the centre of the diaphragm would rise three similar fractions, and the real height of the mercury column would diminish two fractions instead of increasing one. But in reality the central portion of the diaphragm is so proportioned to the cup, that for a rise of one increment of height of mercury the centre of the diaphragm would rise to about double that amount, and the effectual height of the mercury column would decrease instead of increasing to the amount of readjustment required. If the elastic range of the springs balancing the pressure of the mercury were equal to the height of the mercury column, the increase of height on the one hand would be exactly balanced by the increase of elastic force on the other, and the instrument would be in a condition of unstable equilibrium, similar to that of a balance-lever suspended at its centre of gravity. If, on the other hand, the elastic range of the springs were equal to one half the height of column, the increase of elastic force would proceed at double the rate of the increase of potential of the column, and the result would be a scale proportionate to the simple height of column.

It follows from this that the elastic range of the springs must be less than the length of the mercury column. In the actual instrument the elastic range of the spring exceeds to some extent half the length of the column, so that one division of the instrument represents less than its seeming proportion of the total gravitation. It would be difficult to determine the actual scale of the instrument *à priori;* and I therefore adopted the easier and safer method of relying for its final adjustment upon the result of actual working. The limits to the sensitiveness of action of the instrument are chiefly imposed by the diaphragm itself, which must be maintained near its neutral position, because its elastic range is limited and discordant with the range of the spiral springs. It is desirable on this account to make the diaphragm of as thin and flexible metal as possible, and to make the annular indentations as deep as they can be made. This consideration led me to try a diaphragm of silk impregnated with solution of india-rubber, which diaphragm has the advantage of being more flexible than one made of metal, but is liable, on the other hand, to stretching under the constant pressure of the mercury. A diaphragm of thin steel plate has been found to be sufficiently flexible for the purposes of the instrument.

It was desirable to avoid levers, pulleys, and other such working parts in the instrument, which parts are liable to derangement from stretching, bending, and abnormal expansion, which would make the instrument liable to change its zero position. I have therefore had recourse to a micrometer-screw with electrical contact, which, with great solidity and simplicity of parts, affords the advantage of a long and accurately divided scale.

READING OF BATHOMETER. — The micrometer-screw passes vertically through a boss below the centre of the diaphragm, which is attached to the tube by means of two insulating supports of ebonite. A galvanic battery is connected through one pole to the body of the tube, and by the other to the boss through which passes the micrometer screw. An alarum or galvanometer is comprised in the electrical circuit, which is closed whenever the end of the micrometer-screw touches the extreme point of the crosshead supporting the centre of the diaphragm, and therefore the weight of the mercury column. The galvanometer and alarum are so constructed that one element is sufficient to produce the signal, as, if a number of elements were employed, discharges of currents would ensue and affect the surfaces of electrical contact. It is important to clean these surfaces from time to time, by passing a sheet of stout paper or of fine emery-paper between them. A graduated circle is provided to indicate the precise angle through which the micrometer-screw is moved from its zero position when its point touches the end of the crosshead, an event marked by the sounding of the alarum or motion of the galvanometer-needle. The points of contact on the crosshead and on the micrometer-screw are made of platinum in the usual way; but the contact-piece carried by the screw is attached to the same through the medium of a strong and short horseshoe spring, the object of which is to soften the contact between the two points, and thus allow of the natural oscillations of the weighty column as influenced by the motion of the vessel. The pitch of the micrometer-screw being 5 millims. nearly, and the graduated circle being divided into 1000 equal parts, it follows that each division of the scale through which the screw is turned raises the contact-point ·005 millim., a quantity which is intended to represent the depth of a fathom. The micrometer-screw is turned by a wheel geared into a pinion, which is brought up to a place near the point of suspension of the instrument, where it can be turned by means of a milled-head, without the observer being inconvenienced by the oscillations of the instrument relatively to the vessel. Instead of two spiral springs three might be applied, dividing the circle equally, probably with some advantage, viz. that of imparting additional steadiness to the crosshead in its horizontal position. The letters of reference on the drawing, with the references given below, sufficiently describe the mechanical details of the instrument. It remains to be shown how an instrument answering to this description can be depended upon for giving true indications of the varying depths of water below the same, notwithstanding changes of temperature, of atmospheric pressure, and of geological formation and condition of the bottom of the sea.

INFLUENCE OF TEMPERATURE.—In considering the influence of temperature upon the instrument, it was necessary to investigate its action upon the component parts separately. The effect of temperature upon the linear dimensions of mild steel, of which the instrument is mainly composed, is sufficiently well known. Steel expands, according to the experiments of Dulong and Petit, ·000012 of its length for every degree Cent. rise of temperature between 0° and 100° C.; and this number agrees closely with experiments by Regnault, who found the cubic expansion of mercury to be ·00018153 per degree C., between 0° and 100°. C.; in both these metals the ratio of expansion by heat may be considered as strictly arithmetical between ordinary limits of temperature.

INFLUENCE OF TEMPERATURE ON STEEL SPRINGS.—Regarding the influence of temperature upon the elasticity of springs, we have investigations by M. G. Wertheim,* which show a diminution of elasticity with rise of temperature in all metals except iron. This latter metal attains its maximum elasticity (according to this author) at 100° C.; but annealed cast steel agrees with gold and silver and other metals in showing a diminution of elasticity with rise of temperature. The results given in the table prepared by M. Wertheim show a coefficient of diminution of elasticity for cast steel of ·00033768 per degree Centigrade, the modulus of elasticity at 0° C. being 19561, and at 80° C. 19014. Before the bathometer was set up, I had experiments made on the variation of the elasticity of its spiral steel springs in the range of ordinary temperature, which proved this important result,—that the elastic force of well-tempered steel springs diminishes with increase of temperature, within the limits of ordinary temperature, in an arithmetical ratio. The coefficient which I obtained from these experiments was ·000258 of diminution of elasticity per degree Centigrade rise of temperature; and the small difference between this and the coefficient deduced from Wertheim's table will be due most likely to a difference of temper in the steel.

In the bathometer the linear expansion of the springs is compensated by the linear expansion of the tube to which they are attached; and we have therefore only to deal with the variation of elastic force which has to be compensated for, in order to make the indications of the instrument independent of temperature.

Compensation for Temperature-effects.—The means of such compensation is provided in the mercury column. If this column were to consist of a plain cylindrical vessel, not subject to change in diameter by temperature, it is evident that its pressure upon the diaphragm would be the same whatever the temperature of the mercury might be; for with increase of temperature the height of the column would increase, and the density of the mercury decrease in precisely the same degree: such a column might be called one of *uniform potential*, and would not afford the means of compensation here desired. If, on the other hand, the column were made to consist of two shallow cups at top and

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* Annales de Chimie et de Physique, sér. 3, 1845, xv. 119. "Sur l'influence des basses températures sur l'élasticité des metaux."

bottom, connected by a tube of such diameter that its area, compared with that of the cups, might be neglected in calculation, it is evident that the potential of such a column would vary with the temperature in the ratio of the dilatation of mercury; in other words, the absolute height of the column would remain practically the same at all temperatures, whereas the density of the mercury would vary in the well-known ratio of ·00018153 per degree C. If a spring could be found whose ratio of variation was less than that required for the mercury, it is evident that between these extreme forms one might be found in which the two ratios of variation would be exactly alike. The ratio of variation of the steel springs depend upon their degree of hardness; and in the case of the instrument here referred to it amounted to ·000258, or was in excess of the compensating power furnished by the mercury. Complete compensation could therefore in this case not be obtained, although the remaining error is extremely small, and was rendered practically inappreciable by allowing the comparatively inelastic diaphragm to take a portion of the mercurial pressure.

The proportion, as resulting from calculation, would at any rate have to be modified in order to allow for the linear expansion of the steel composing the tube as affecting its capacity; but this expansion proceeding also in an arithmetical ratio will only affect to a small extent the precise relative diameter to be given to the tube, without in any way disturbing the ratios of arithmetical increase upon which the compensation of the instrument is based. An easy verification of this arrangement, which may be called a *parathermal system of adjustment* between gravitation and elastic force, is furnished in suspending the complete instrument in the hot-air chamber in which the experiments for variation of elasticity were made, when the variations of temperature gradually and artificially produced within the chamber should remain without effect upon its reading.

On subjecting the first instrument constructed on this principle to this test, a variation was discovered amounting to ·00000125 per degree C., which was not corrected, however, in trying the instrument on board the steam-ship "Faraday;" and the results then obtained, and given below, have had to be adjusted to this extent for variation in temperature.

INFLUENCE OF VARIATION IN ATMOSPHERIC DENSITY.—The atmosphere presses equally upon the surface of the mercury in the upper cup of the bathometer and upon the diaphragm below, and variations in the height of the barometer, therefore, exercise, *per se*, no influence upon the instrument; but inasmuch as the mercury column exercises a preponderating gravitating influence only in the measure of its superior density to the atmosphere which the mercury replaces in the tube, it follows that changes in atmospheric density must exercise an influence upon the readings of the instrument. The atmospheric density depends upon barometric pressure, temperature, and admixture of aqueous vapour, the amount of which can be easily ascertained by readings of the dry- and wet-bulb thermometers and the barometer at the time of taking the bathometrical observations. These corrections have been made and applied to the observations taken on board the steam-ship "Faraday"; the readings, however, having been taken at sea, the air was regarded as saturated with vapour, and the tension of the vapour at the temperatures has been employed. In ordinary usage of the instrument these corrections might be neglected without serious error, or a table might be constructed giving the amount of these corrections for observed changes of the barometer and thermometer.

GEOLOGICAL INFLUENCES.—The readings of the bathometer depend upon the inferior density of sea-water as compared with the solid constituents composing the earth's crust, which have been taken, in the calculation at page 673, as 2·763. No account was taken, in assuming the above average density of the earth's crust, of the presence of denser materials, such as metallic ores, heavy spar, &c., on the one hand, or of subterranean cavities on the other. But these abnormal occurrences are not frequent on dry land, being chiefly confined to mountainous districts, and may be assumed to be of less frequent occurrence in the great depressions constituting the sea-basins. Their relative effect upon total gravitation, as measured upon the surface of the water, is less, moreover, than it would be if measured upon the solid surface, on account of their greater distance from the instrument. The uniform density of the sea is an element eminently favourable to the attainment of uniform indications on its surface.

GEOGRAPHICAL INFLUENCES.—The configuration of the bottom of the sea below the instrument must also exercise a sensible influence upon its readings. The instrument would not indicate, for instance, the existence of a local depression surrounded by elevated ridges or plateaux, nor would it indicate the existence of a peak. Considerable variations must therefore be occasionally expected between the readings of this instrument, however correctly adjusted, and the results of actual soundings; but it may be observed that broken ground, such as would cause these differences, is comparatively rare below the sea, which deepens gradually from the land in such a way that the contour lines of uniform depth can generally be distinctly traced; and the principal value of the instrument would consist in its indicating its passage above varying depths. The indications of the instrument must coincide very nearly with those of a sounding-line upon an even slope, because the comparative proximity of the ground towards the rise of the slope will be balanced by the absence of solid matter towards its descent.

Attention has already been called to Sir George Airy's observation of the greater apparent gravitation on islands than on the sea-shore, and there than inland, and also to Professor Stokes's explanation of the matter. The working zero of the bathometer may be taken as a maximum or island indication; and the diminution due to the depth of water is therefore not influenced by the irregularities met with on solid land, in consequence of the matter raised above the natural surface of the sea. It has, however, been shown by Archdeacon Pratt and others that continents exercise an influence upon the level of the sea, that level being raised up towards the masses piled above the surface; and such disturbance of the natural water-level must necessarily exercise an influence upon the readings of the instrument. But this influence would be perceptible only in estuaries or upon the sea-shore of a mountainous continent, and may be neglected in dealing with the surface of the sea under all ordinary circumstances.

The more important disturbing cause affecting the instrument under this head is that of the ellipsoidal form of the earth and the varying centrifugal tendency on its surface, to which reference has already been made.

Effects of Latitude.—The determinations of the effect of latitude upon gravitation as made by Newton, Clairaut, M'Laurin, and others have already been alluded to, and it is important that the influence of this disturbing cause upon the instrument should be accurately ascertained in order that allowance may be made for latitude in its ordinary use. In order to test separately the effect of latitude upon the instrument, its indications were taken on the 8th of December at Westminster, lat. 51° 31′ N., long. 0° 7′ W., and afterwards at Brighton, lat. 50° 50′ N., long. 0° 10′ W., which is nearly due south of Westminster 41 nautical miles. At Westminster the indications were

Bathometer.	Barometer.	Thermometer. ° Fahr.
2 turns 432	30·425	43·4
431	30·425	43·6
430	30·425	43·4

These readings were taken at intervals of 5 minutes. The instrument was then carefully packed and removed to Brighton, where it was again set up. The first reading was taken an hour after arrival, and the readings taken during the afternoon are noted below.

	Bathometer.	Barometer.	Thermometer. ° Fahr.
From	2 turns 449·5	30·315	40·5
12·55 to	449	30·315	40·5
1·10 P.M.	451·5	30·32	41
From	2 turns 449·5	30·31	42·8
1·44 to	448·8	30·31	42·8
2 P.M.	449·5	30·31	42·8
From	2 turns 449·7	30·3	43·4
4·13 to	449	30·29	44
4·30 P.M.	449·5	30·29	44

The readings of the instrument taken the next morning at Westminster were—

2 turns 440	30·42	43
439·5	30·42	43·25
440	30·42	43·2

It will be found that, on correction being made for variation of temperature and atmospheric density, and taking the mean of the several readings (the first observed Westminster indications being taken as the standard), the above indications may be reduced to the following :—

	Bathometer.
Before leaving Westminster	431
At Brighton	452
On return to Westminster	439·25

Taking the mean of the Westminster readings, there would be a difference on the scale of 17 divisions, equivalent to a diminution in attraction of ·0000046, whereas calculation gives a difference of ·000066.

I have not succeeded in finding a satisfactory explanation of this apparent anomaly, which can hardly be attributable to defects of the instrument or to errors in observation, because on taking the instrument on board the steamship "Faraday" from the Thames down the Channel, the variations observed (as recorded in the Table, p. 375) accord very fairly with the increasing depth of water, but give no evidence of the great variations in total gravitation due to differences in latitude. In order to test the influence of latitude further, I caused the instrument to be taken to Scarborough, which is 207 miles north of Westminster; and the observations there taken confirmed generally those of Brighton, in showing insufficient variation, although their absolute value was rendered unreliable by an accidental disturbance of the instrument in transit.

It must be borne in mind that both Brighton and Scarborough are on the sea-shore, and that Westminster is upon an inland estuary, which circumstance would exercise an influence in the direction of equalizing the total gravitation at Brighton and Westminster.

Actual trial of the Instrument on board Ship.—The foregoing may suffice to show what are the disturbing influences to be met with in the use of the instrument which forms the subject of this paper; but it was important to ascertain what would be the actual indications of the instrument in taking it on board ship over seas of varying and known depth, in order to compare the indications of the instrument with those of the sounding-line. For this purpose two instruments, the smaller of which is represented in Plates 31, 32, were placed on board the steamship "Faraday." They were suspended in a closet adjoining the electrician's room, near the centre of motion of the vessel, and were observed carefully in Victoria Docks before starting, continuously during the voyage, and on the return of the vessel from Nova Scotia, where it had been sent for the purpose of re-uniting the Direct United States Submarine Cable, which had been fractured, where it crossed the Newfoundland Bank, by the dragging of an anchor. The observations during this first trial of the instrument were made by Dr. Higgs, the chief of the electrical staff accompanying the expedition. The following Table gives the results of these observations :—

TABLE I.

BATHOMETER RECORD: STEAMSHIP "FARADAY," OCTOBER, 1875.

Date.	Hour.	Position.	Thermometer.	Barometer.	Bathometer Divisions.	Depth.
			° Fahr.			Fathoms.
Oct. 15	Noon.	Victoria Docks	64·5	29·7	Zero.	2
,, 18	Noon.	Tidal Basin	65	29·95	3·5	
,, 19	8 A.M.	Lower Fort, Tilbury	60	30·00	9·0	
,, 19	10.35 A.M.	Off Southend	59	29·7	11·5	
,, 21	11.45 A.M.	Off Lizard	60	29·6	47·5	
,, 22	9 A.M.		56·3	29·5	92·5	
,, 23	Noon.	51° 0′ N.; 14° 37′ W.	Bad	weather.		By Chart.
,, 25	Noon.	51° 25′ N.; 26° 25′ W.	56	29·15	2130	1900
,, 26	Noon.	51° 7′ N.; 31° 14′ W.	56	29·75	2500	2000
,, 27	Noon.	Dead Reckoning	56	29·15	2870	2100
,, 28			Bad	weather.		

In this Table no correction for latitude has been made; and although the differences of latitude are not very great, they would nevertheless be more than sufficient to swamp the results of such minute differences of depth as are met with, for instance, in passing from the Thames down through the Channel. The concordant results shown in the Table seem to prove either that the correction for latitude is (for some reason, which, as already stated, I am not able to explain) much less in this instrument than it would be in the case of pendulum indications, or that the reading of the instrument had not been taken with a proper degree of care. It might be assumed that the known depths of the channel might have betrayed the observer involuntarily into a mistake when observing only small divisions on the instrument, although I must personally dissent from such a supposition, because I entertain the

TABLE II.—RECORD OF BATHOMETER READINGS COMPARED WITH SOUNDINGS TAKEN ON BOARD THE STEAM-SHIP "FARADAY," OCTOBER AND NOVEMBER, 1875.

Date.	Hour. G. M. T.	North Latitude.	West Longitude.	Thermometer.	Barometer.	Bathometer Divisions uncorrected.	Corrections for variations in Temperature and Atmospheric Density.	Bathometer Divisions corrected.	Sounding.	Difference.
	h. m.	° ′ ″	° ′ ″	° Fahr.	″		(—)		Fathoms.	
Oct. 29	3 0 P.M.	47 50	47 0	56	29·5	216	14·55	201·45	197	+ 4·45
,, 29	8 55	47 34	48 23	57	29·7	113	13·5	99·5	100	− 0·5
,, 30	1 52 A.M.	... ...	... ...	58	29·5	74	10·95	63·05	54	+ 9·05
,, 31	12 0 P.M.	... ...	... ...	57·5	30·05	96	13·7	82·3	82	+ 0·3
,, 31	1 8	45 5	54 28	58	30·05	231	12·85	218·15	204	+ 14·15
,, 31	2 20	... ...	... ...	60	30·05	87	9·15	77·85	69	+ 8·85
,, 31	2 59	... ...	... ...	59·5	30·05	66	10·2	55·8	54	+ 1·8
,, 31	3 27	45 9 45	54 14	60·5	30·9	66	11·2	54·8	54	+ 0·8
,, 31	5 47	45 10 42	54 18 20	57·5	30·7	66	15·9	50·1	56	− 5·9
,, 31	7 27	45 11 15	54 17 53	56	30·7	66	18·55	47·45	54	− 6·55
Nov. 1	12 15	45 10 12	54 18 25	55·5	29·5	66	15·7	50·3	58	− 7·7
,, 1	2 18	45 7 15	54 21 30	55	29·0	82	15·65	66·35	69	− 2·65
,, 1	3 25	45 6	54 22 48	60·5	28·9	87	4·6	82·4	73	+ 9·4
,, 2	12 50	... ...	... ...	59	28·78	63	6·85	56·15	47	+ 9·15
,, 2	2 58	... ...	... ...	55	28·9	63	14·35	48·65	46	+ 2·65
,, 2	5 30	... ...	... ...	60·5	29·0	85	4·9	80·1	69	+ 11·1
,, 3	7 57	45 5 48	54 25 42	58·5	29·3	120	9·4	110·6	100	+ 10·6
,, 3	9 20	45 4 50	54 28 34	58	29·5	226	10·95	215·05	200	+ 15·05
,, 4	2 10	45 10 36	54 20 6	57·5	29·7	82	12·5	69·5	64	+ 5·5
,, 5	12 35	... ...	... ...	57	30·0	95	14·5	80·5	80	+ 0·5
,, 5	1 45	... ...	... ...	58·5	30·0	98	11·8	86·2	86	+ 0·2
,, 5	4 16	... ...	... ...	58	30·0	90	21·65	68·35	76	− 7·65
,, 7	5 19	46 45	47 17	58	29·7	400	11·65	388·35	353	+ 35·35
,, 7	6 35	46 35	46 57	58	29·8	811	11·95	799·05	698	+101·05
,, 7	9 50	46 26	46 20	58·5	29·9	617	9·4	607·6	503	+104·6
,, 8	11 25	46 23	41 11	59	30·05	2800	11·05	2788·95	2516	+272·95
,, 10	2 31	48 12 30	33 12	58·5	29·95	2400	11·6	2388·4	2320	+ 68·4
,, 11	1 4	48 49	28 55	58	30·05	1920	12·85	1907·15	1861	+ 46·15
,, 11	6 46	48 56	28 3	57	30·0	1630	14·5	1615·5	1700	− 84·5
,, 24	1 0	Victoria Docks	... ...	57	30·05	11	16·05	5·05		

highest opinion of the conscientious care peculiar to the observer; but no such cause could possibly have operated regarding the observations of the instrument recorded in the series of observations given in the second Table, when the vessel passed through seas which had not been before sounded, but which were sounded after each observation of the instrument had been made.

In this Table, p. 376, columns 1 and 2 contain the dates and hours observations were made; 3 and 4, the latitude and longitude of the locality when ascertained; 5, the indications of the thermometer; 6, the indications of the barometer; 7, the indications of the bathometer; 8, the corrections for variations in temperature and atmospheric density; 9, the readings of the bathometer so corrected; 10, the soundings taken; 11, the difference of these and the bathometer indications. The soundings were made by means of Sir William Thomson's steel-wire sounding-apparatus, by which admirable improvement over the old sounding-line it is now possible to take soundings exceeding 2,000 fathoms in an hour, when 5 or 6 hours were formerly required, and by the application of mechanical power to recover the steel wire itself in from 15 to 20 minutes when a detaching weight is employed.

The reading of the bathometer was in each case reported to Captain Trot, of the steamship "Faraday," before the sounding-line had reached the bottom; and the fair accordance between the results obtained by sounding and those given by the instrument furnishes ample proof of the reliable nature of the bathometer indications. The series of observations was unfortunately interrupted during the homeward voyage by a heavy gale, whereby the instrument was exposed to splashes of sea-water from the deck; it had to be taken down, and was only remounted when the vessel had arrived at the Victoria Docks. It will be observed that the readings taken in the Victoria Docks, before and after the voyage, agree, after allowing for difference of temperature and atmospheric density, within 5 divisions on the scale of the instrument, representing 5 fathoms of depth, an accordance which must be considered highly satisfactory.

INFLUENCE OF ELEVATION ABOVE THE EARTH'S SURFACE.—The bathometer is applicable also to the measurement of height, for which purpose it possesses the advantage over the aneroid barometer that its indications are not affected by changes of atmospheric pressure, excepting the small correction for change of atmospheric density before referred to, which could be avoided in excluding the atmosphere from the extremities of the mercury column.

The total attraction of the earth varies in the inverse ratio of the square of the distance from the centre of the earth; and the ratio of the attraction on the surface of the earth, and at a height h above the surface (supposing the earth to be a sphere), will be expressed by $\frac{w}{w'}=\frac{(\mathrm{R}+h)^2}{\mathrm{R}^2}$, which for relatively small values of h may be written $\frac{w}{w'}=\frac{\mathrm{R}+2h}{\mathrm{R}}$ or $\frac{w-w'}{w}=\frac{h}{\frac{1}{2}\mathrm{R}}$, proving that attraction decreases with elevation in the simple ratio of $\frac{1}{2}$R. The decrease on account of depth of sea takes place, as shown on p. 362, nearly in the ratio of R, or the readings of fathoms on the bathometer may be taken for yards in raising the instrument above the sea-level.

The corrections for latitude necessary for reading depth of sea are also applicable for height; but in the latter case another correction will have to be made for the attractive force exercised by the mass composing the mountain or elevation above the sea-level supporting the instrument, and this will vary greatly with the breadth, being a maximum in the case of an elevated plateau. The instrument will, in such cases, give indications of height considerably below the real elevation, and it is doubtful on that account whether it can be made available for such a purpose.

TEST FOR ELEVATION.—Being desirous to test the instrument for height, I decided to take it up a tower; and having obtained the permission of the Board of Works, through my friend, Dr. Percy, to make use of the Clock Tower for the purpose, the instrument was tested on the 18th of December, the readings being as below:—

	Bathometer. (Mean.*)	Thermometer.	Barometer.
At top of tower .	1067·75	45°0	29·64
At foot of tower . .	1022·5	45·63	29·88

being a difference of 45·25 divisions, equivalent to a difference of

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* Including correction for variation in atmospheric density.

height of 135 feet, the aneroid indicating a difference of 208 feet. This difference of readings may appear at first sight excessive, but may be accounted for by disturbance of the instrument in taking it by hand up the steep steps of the tower, where little time was allowed to insure the complete readjustment of the column. In this case also the reading of the instrument gives a result inferior to the indications of theory as compared with its indications on board ship, which latter indications I consider are the more reliable, because the instrument, when once suspended, is not disturbed, and its indications are rendered more delicate through the oscillations of the vessel.

Modifications in the Instrument.—The instrument, as constructed at present, leaves room for such improvements as have partly been, and are likely still to be, suggested by experience. It would be possible to eliminate entirely the effect of variation of temperature by more carefully proportioning the diameter of the mercury column to that of the cup. The influence of variation of density of the atmosphere might also be entirely eliminated if the spaces in the cups above and below the mercury column were closed against the atmosphere, and were brought into communication with each other. The mode of reading the instrument may also be simplified in various manners, or the instrument may be made self-recording by the addition of a chronograph. My present object has been to demonstrate the possibility of constructing a bathometer capable of giving indications of moderate variations in the depth of sea below a vessel, and to describe rather the instrument actually used than such modifications as may prove more advantageous hereafter.

Practical uses of Bathometer.—The useful purposes for which a bathometer, so arranged as to be observable without difficulty by the commander of a ship, may be employed, are, I think, apparent. It often happens at sea that through clouded skies and fogs it is impossible for astronomical observations to be taken, and it is well known that the compass and dead-reckoning are very uncertain guides to the position of a ship; and as the sounding-line can only be of assistance after the ship has arrived at such depths as are positively dangerous, many calamities are on record where, under such circumstances, not only sailing-vessels, but well-equipped steamers have run ashore. The indications of the bathometer would warn the commander of a vessel of the gradual approach of shallow water; and if in possession of accurate charts, he would in many cases be able to determine his actual position by noting in which direction and at what rate the depth varies.

POSITION OBTAINED BY SOUNDINGS.—An illustration from actual practice may serve to show how accurate a guide a knowledge of the depth of the sea can be made. In laying the Direct United States Cable to America, of which operation Mr. Carl Siemens took the principal charge, it occurred that, in November, 1874, heavy weather had prevented the taking of observations for three days, when an increasing gale, and the suspicion of a slight fault having passed overboard, rendered it necessary to cut the cable and buoy the end. Before cutting the cable a sounding was taken by Sir William Thomson's wire, and the depth was found to be 800 fathoms. The gale lasted several days; and when the "Faraday" returned to the spot where the end was supposed to be buoyed, no buoy could be found, and it became evident that it had been torn away from the anchor-chain by the violence of the gale. The sounding taken at the point where dead-reckoning had placed the ship at the time of buoying the cable gave a depth of 521 fathoms, lat. 48° 32′ N., long. 45° 21′ W., and showed at once that the end of the cable must be looked for elsewhere. There exists no chart of the part of the Atlantic in question, giving such soundings as might have assisted in the search; but special soundings were taken in all directions, from which the dip of the Atlantic basin in that locality could be ascertained. The cable was parted over a depth of 800 fathoms; and in constructing the contour-lines of the Atlantic basin in the locality, which was dipping towards the N.E., it became evident that in order to obtain the cable with the grapnel, it must be caught up in a line parallel to the contour-line, but a mile or two to the eastward. The expedient adopted proved successful, and the cable was recovered in lat. 48° 44′ N., long. 44° 44′ W., or at a point 25 nautical miles removed from the place where it was supposed to have been lost (see Plate 30, fig. 3). If complete information regarding the depth of the Atlantic Ocean had been available in laying the cable, and if the steamship "Faraday" had at that time been furnished with a reliable bathometer, the uncertainty regarding the position of the vessel when the cable was buoyed would never have arisen, and much anxiety and time would have been saved in recovering the end. In cable-laying a bathometer is more particularly of use, because the amount to which the retarding-brake has to be weighted bears a definite relation to the depth of sea traversed ; and an accurate knowledge of that depth is essential to prevent either loss of cable from excessive slackness, or permanent danger through an insufficiency.

A bathometer of careful construction would be extremely useful in increasing our knowledge of the depth of the ocean, whilst instruments of inferior accuracy would serve the useful purpose of furnishing the navigator with timely warning of approaching shallows.

It is chiefly with a view to this latter result that I venture to place my inquiries into this subject before the Royal Society. In doing so I wish to acknowledge the valuable assistance I have received from Mr. Bamber and Dr. Higgs, the former having conducted the experiments to determine the influence of temperature on the elasticity of springs, and effected the adjustment of the instruments on land, while the observations on board ship were taken by Dr. Higgs.

ADDENDUM.

ON AN ATTRACTION-METER.

At the reading of the foregoing paper, I exhibited an instrument for measuring horizontal attractions, which, at the same time, illustrates the action of the bathometer. This instrument (Plate 33) consists of a horizontal tube of wrought iron 400 millims. long, terminating at each end in a horizontal transverse tube of cast iron of 60 millims. diameter and 300 millims long. The first-named horizontal tube is partially closed at its ends, and communicates with the transverse tubes below their horizontal mid section. The transverse tubes communicate also by means of a horizontal glass tube of 2 millims. diameter at a superior level to the former.

The whole apparatus being mounted upon three set-screws is filled to the level of the half-diameter of the transverse tubes with mercury, which mercury fills also the whole of the longitudinal connecting-tube; the upper halves of the cast-iron transverse tubes and the glass connecting-tube are filled with alcohol tinted with cochineal, comprising, however, a small bubble of air, which can be made to occupy a central position in the glass tube by raising or lowering the set-screws.

If a weighty object is approached to either extremity of the connecting-tube an attractive influence will be exercised upon the mercury, tending to a rise of level in the reservoir near at hand, at the expense of the more distant reservoir; and this disturbance of level between the two reservoirs must exercise a corresponding effect upon the index of air in the horizontal glass tube, moving it away from the source of attraction. The amount of this movement must be proportionate to the attractive force thus exercised, and is considerable, because the transverse cross section of each reservoir-tube is $60 \times 300 = 18{,}000$ square millims., whereas the section of the glass tube is only about 3 millims.; the motion produced by the effect of gravity is thus increased 3,000 fold, and could easily be increased, say 30,000-fold, by simply increasing the horizontal area of the transverse or reservoir-tubes. Variations of temperature have no effect upon this instrument, because the liquids contained on either side of the index of air are precisely the same in amount; and the total expansion of the liquids is compensated for by an open stand-tube rising up from the centre of the connecting-tube, through which the apparatus can be easily filled. By means of this instrument the effect of 1 cwt. approached to one end or the other of the mercury connecting-tube causes a sensible motion of the air index.

It is suggested that an instrument of this description may be employed usefully for measuring and recording the attractive influences of the sun and moon which give rise to the tides. The instrument, which is of simple construction and not liable to derangement from any cause, would have to be placed upon a solid foundation with its connecting-tube pointing east and west, records being taken either by noting the position of the index upon the graduated scale below, or by means of a self-recording arrangement through photography.

This mode of multiplying the effect produced by gravitation is applicable also to the bathometer; and one of these instruments was shown which was fitted with a spiral glass tube laid horizontally upon the upper surface of the bathometer upon a regularly divided scale, which horizontal tube is connected at one end with the uppermost chamber of the bathometer above the mercury, while the other end remains open to the atmosphere. The space above the mercury in the upper chamber is filled by preference with oil, which terminates in the horizontal spiral glass tube at a point which will vary with the total attractive influence of the earth, and thus furnish a means of reading the instrument. The electric contact arrangement described in the paper is thus rendered unnecessary, and the reading of the instrument much simplified.

Since presenting my paper on the bathometer to the Royal Society in February last, I have continued my endeavours to produce an instrument in such a form as to be practically independent of the disturbing influences to which reference is made in my paper, and of a construction so simplified as to render the instrument available for practical uses.

It is my intention to present before long a supplementary paper to the Royal Society describing the improved instrument, and giving an account of the further trials which I have had the opportunity of making, for the purpose of verifying the indications of the instrument by actual sounding.

The first set of observations was made by Mr. Alexander Siemens, on board the steamship "Faraday," in American waters of a depth not exceeding 100 fathoms, when the readings were found to accord closely with the results of sounding. Besides this, several trials of the instrument have been made: one under my immediate superintendence in crossing lately from New York to Liverpool, on board the steam-ship "Bothnia," Capt. M'Mickan (who rendered me every facility); another on board H.M. steam-ship "Fawn," between Southampton and Gibraltar; while another has been made, at the instance of Dr. Higgs, with a modified form of apparatus, on board a sailing-ship in its passage from Southampton to Rio Janeiro. The results of the observations on board the "Fawn" were unsatisfactory, owing to a mechanical defect in the apparatus, whereas the others confirmed generally the results given in my paper confirming also the observation there referred to, that differences of latitude do not seem to exercise the full amount of effect upon the instrument which might be expected, in consequence of the combined influence of centrifugal force and ellipticity of the earth.

Criticisms have appeared in several papers questioning the applicability of the bathometer for determining the depth of the sea, owing to the disturbance of the sea-level by continental attraction. This cause of disturbance had not escaped my attention in writing my paper *; and it should be borne in mind that the instrument cannot do more than indicate comparatively small variations in total terrestrial attraction, which the hydrographer or navigator using the bathometer will have to interpret according to the circumstances of the case. The zero-point of the instrument must vary no doubt with latitude, continental attraction, and also in consequence of special geological causes; but it is important to observe that these causes are of a permanent character, and that if an ocean has been once surveyed with the aid of the bathometer, such special local conditions would become observed facts, and so far from hindering the advantageous use of the instrument, would serve, on the contrary, to increase its measure of usefulness in the hands of the navigator.

In the Addendum to my paper of the 23rd February, I described a modification of the principle of the bathometer, designed for the purpose of measuring horizontal attraction; and I take this opportunity of stating that I have constructed an instrument of this description, which has been erected upon a solid foundation at the Loan Exhibition, South Kensington. The measure of sensitiveness of this instrument is given by the fact, that the weight of a person stepping from one side of it to the other causes the indicating bubble to travel through one division (of 1 millim.) of the scale. It would not be difficult to construct such an instrument of still greater sensitiveness; and I believe that it could be made a useful adjunct at physical observatories, for the observation of diurnal changes in the horizontal attraction produced by the sun and moon as well as of terrestrial causes of disturbance of the superficial equilibrium of the earth.

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* See page 372.

In the discussion of the Paper

"ON THE CHALK WATER SYSTEM," by JOSEPH LUCAS,

DR. SIEMENS said * an observation had fallen from Mr. Baldwin Latham which, he thought, ought not to pass unchallenged—that the water flowing from deep wells was warmer than that flowing from shallow wells, and that the increase in temperature in it might be attributed to the greater friction of the water through the Chalk Formation. Mr. Latham had correctly given the coefficient of increase, 1° for every 772 feet of water percolating downwards ; but Mr. Latham had apparently not considered the fact that this difference of level did not include the depth of the water in the well, but only the depth from the surface where the rain fell to the level of the water in the well, because the depth of water in the well balanced so much of the hydrostatic pressure as would urge the water through the chalk, and therefore did not add to the accelerated force, or the force to be developed into heat by friction. It was therefore necessary to consider what was the difference of level between the water in the well, and the level where the rain fell and sank down into the ground ; and there could be no doubt that that amount of hydrostatic pressure was lost, and therefore converted into heat. But would that heat appear as temperature in the water ? He doubted it very much, because before the well was pumped the chalk was filled with water, and that water was in static equilibrium. It was only when the well was worked that the water would flow and friction be generated. That amount of friction would not only heat the water, but it had to heat the stratum of chalk before it could be sensible to a thermometer, and considering the enormous mass of material which would thus have to be heated by a comparatively small amount of water, Dr. Siemens thought the idea of heat being derived from mechanical friction in the chalk must be dismissed. Mr. Latham's observations seemed almost to imply that he attributed heat-engendering power to the horizontal distance traversed by the water ; and it

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XLVII. Session 1876–77, pp. 134–135.

therefore should be clearly understood that the amount of heat engendered by friction could in no case exceed the equivalent head of water, or accelerating force expended; or, in other words, the 772 feet difference of level was necessary to produce as much heat as would raise the temperature of water percolating through narrow passages 1° Fahr. Another explanation, that of the depth to which the water might dip on its way to the well, appeared to him to account much better for the difference of temperature in different levels in the chalk. Of course, if the water dipped to a very great depth and afterwards rose again, the dip would not be productive of heat by friction, inasmuch as the available heat for friction was due to the difference of final levels, but it might give rise to an increase of temperature owing to the warmer strata which the water had touched on its way.

In the discussion of the Paper

"ON THE TRANSMISSION OF POWER TO DISTANCES," by Henry Robinson, M. Inst. C.E.,

Dr. Siemens [*] thought the discussion should not be limited to that portion of the paper which referred to hydraulic transmission and hydraulic presses. The author had dwelt upon the subject of the transmission of power, and Dr. Siemens desired, therefore, to make a few observations on the general question. Hydraulic transmission, as had been correctly stated, was the most economical mode at present known. It had the advantage that in forcing water forward very little power was lost. As had been explained, the friction of the hydraulic ram could be reduced almost to a minimum, and the steam power was applied in the most direct manner to that resistance; in that respect, therefore, hydraulic power could be produced very economically, and the loss of power

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[*] Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. XLIX. Session 1876–77, pp. 31–34.

in transmission could be reduced to a small amount. But in regard to the application of the power to cranes or presses there was a loss—a loss which might be exceedingly small if the resistance to be overcome was nearly equal to the available force multiplied into the area of the working piston or ram ; but if the load was small, the power expended remained the same as it would be if the maximum resistance was applied, and there was consequently great loss. It was interesting to compare that with the case of power transmitted by elastic fluids. In compressing air great power was lost, because the steam in urging the piston forward in the air-compressing pump reduced its volume and raised its temperature, and the rise of temperature occasioned increased resistance and loss. The elastic condition of the air was a source of great diminution of power in the first instance, but it was recoverable, inasmuch as the air engine at the other end could be made to work expansively, and thus recover that portion of power which was consumed in compression through loss of volume. But there remained the double loss of heat—the heat generated in the compressing pump, which augmented the resistance, and the heat lost in the working of the air engine, which lessened the pressure towards the end of the stroke of the piston. These losses could never be entirely avoided, but they might be reduced, he believed, to 50 per cent., by injecting spray into the compressing cylinder, so as to keep the temperature in compression and in expansion as uniform as possible. Professor Rankine gave the loss as 62 or 64 per cent., but that was under the condition of injecting no water, of compressing air and generating heat in its compression. There was no reason why the air engine should not be made to work as economically as steam. The air did not condense, but there was a loss of steam by condensation in the cylinder and in the pipes leading towards it. By injecting warm water into the cylinder the loss of refrigeration might also be avoided ; but that had never been done practically. In transmission air certainly would be less economical than water, for this reason, that water could be transmitted under a pressure of 1 ton or ½ ton to the square inch, whereas air could scarcely be compressed to such a degree ; therefore it was necessary to deal with larger volumes requiring larger pipes and greater frictional surface. Nevertheless for many purposes air would be a preferable medium, as in the case of coal-cutting machines in mines, tunnelling machines, and machines in building, where water would be inconvenient. One mode of transmitting hydraulic power had only been partially alluded to in the paper, such as that which took place at Schaffhausen, where turbines gave motion to quick-working pulleys, on which steel ropes worked, transmitting power to a considerable distance.

Another mode in which such rotating power might be obtained, and which was obtained more frequently perhaps on the Continent than in this country, was by sending the water through high-pressure mains, and then making it work rotating hydraulic engines, such engines generally working with oscillating cylinders; that, he thought, was a handy way of getting rotative power.

He might also refer to another mode of transmitting power to a distance, which, did not seem to have occurred to the author, perhaps because it was of recent date, viz., by electric conductors. If the dynamo-electric machine were employed for the production of intense currents, such currents could be used for giving motion to electrical engines for precipitating metals and for producing light. The latter application was of practical interest, as it had actually been employed for the illumination of lighthouses, as well as for electric lamps armed with reflectors, so as to enable public works, such as bridges, to be carried on during the night, and for lighting large buildings. One or two facts might be interesting with regard to that mode of transmission. A 4-HP. engine would produce per hour a light equal to 1,000 candles; therefore 100 HP. exerted in that way would produce a light equal to 25,000 candles, or to 1,250 Argand burners, which would be equal to 25,000 cubic feet of gas burned per hour, representing a value, at 4*s.* 6*d.* per thousand, of £5 12*s.* 6*d.* The 100 HP. converted into an electric current could be conveyed through a copper rod 2 inches in diameter, and say a mile long; such a rod would give a resistance of only about $\frac{1}{4}$ electrical unit, which would not in any way impede the electric power. Therefore the power could be transmitted to a distance of 1 mile by means of such a rod of copper, and give there an aggregate amount of light equal to 25,000 cubic feet of gas. He thought that the method was of sufficient interest to be added to the other modes of transmission, especially as it was gradually coming into use.

ON THE CONSTRUCTION OF VESSELS TO RESIST HIGH INTERNAL PRESSURE.

By Dr. C. William Siemens,* D.C.L., F.R.S.,
Past-President Inst. M.E.

In constructing vessels intended to withstand a great internal pressure, considerable practical difficulty has hitherto been encountered. If boiler plate is used in their construction, the seams of rivets are sources of weakness, and of uncertainty as to resisting power, increasing with the thickness of the plate required to withstand the intended strain. In consequence of these practical difficulties, it has generally been thought advisable to limit the diameter of cylindrical vessels intended to bear great strain, and to resort to a multitubular construction. But here again the difficulty of many joints is encountered; and the vessels constructed upon this principle necessarily occupy much more room than a plain cylindrical vessel would do. When cast iron is resorted to in the construction of such vessels, as in the case of hydraulic presses and accumulators, the thickness required is so great as to render the vessels extremely ponderous and costly; and it sometimes happens that the fluid under pressure finds its way through the pores of the metal. At the present time the occasions for the use of high-pressure vessels increase daily with the application of compressed air as a motive agent, with the application of hydraulic transmission, and with the introduction of high-pressure steam for marine purposes, where the large diameters of the boiler shells required necessitate the construction of cylindrical vessels of great strength.

The writer's attention was specially directed to this subject last year by Colonel Beaumont, who asked him to advise regarding the construction of a vessel of not less than 100 cubic feet capacity, and capable of resisting an internal pressure of at least 1000 lb. per square inch. The dead weight of this vessel was not to exceed $2\frac{1}{2}$ tons, as it was intended to act as a reservoir of highly com-

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[*] Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1878, pp. 271–275, and pp. 286–290.

pressed air, to supply air for working his tramway locomotive engine.

In designing this vessel, the writer acted upon the principle that a metal should be employed to resist the bursting pressure, which should combine strength and toughness in the highest degree, and so disposed that its continuity should not be disturbed by any sudden changes in dimensions or by perforations of any kind. The material selected was steel, of such quality as to be capable of resisting a tensile strain of 45 tons per square inch, and of extending from 8 to 10 per cent. before breaking.

The vessel itself is represented in Figs. 1 and 2, Plate 34. It consists of several cylindrical rings, A, of 40 inch internal diameter and 12 inch depth, rolled out of solid steel ingots in a tyre mill; and of two hemispherical ends B beaten out of steel boiler plate. The hemispherical ends and the rings are strengthened at the edges by projecting dwarf flanges, as shown in Fig. 3, and full size in Fig. 4. The only tooling necessary to these rings and ends consists in turning a V groove in each face, Fig. 4, care being taken that all the grooves should be at the same distance from the centre, irrespective of the precise diameter of the several rings A. Packing rings of well annealed copper wire of $\frac{5}{16}$ inch thickness were prepared, the diameter of these rings being precisely the same as that of the V grooves, so that the packing rings should lie true in the grooves, as shown in Fig. 4. Two rings C of cast steel, each perforated with twenty holes of $1\frac{5}{8}$ inch diameter, fit over the hemispherical ends, but rest chiefly against their projecting dwarf flanges, as shown in Fig. 3; through these holes are passed twenty steel bolts D of $1\frac{1}{4}$ inch diameter, of such quality as to resist 50 tons per square inch, care being taken to enlarge the screwed ends of the bolts, in order not to weaken their total strength in the ends, but to allow of uniform elastic action throughout their length.

The different parts composing this vessel having been thus prepared, the vessel was built up as represented in Fig. 1, Plate 34, and the bolts were gradually tightened up to a point just sufficient to resist the intended internal pressure. This being accomplished, the vessel was filled with water, and the pressure of a hydraulic accumulator loaded to 1000 lb. per square inch was applied. No sign of leakage was observed except at one joint, where the thickness of the copper packing ring appeared to have been insufficient to fill the groove. This defect was remedied by passing the edge of a thin chisel in between the flanges, and pressing the copper ring in that place by gentle hammering, which had the immediate effect of stopping the leakage. The internal pressure was thereupon gradually raised to 1300 lb. per square inch, at which point nearly all the joints began to weep, showing that a pressure had been reached at which the bolts commenced to elongate. Each nut was thereupon tightened up another eighth of a turn, and the pressure again applied; when the vessel was found to be perfectly tight at the previous pressure of 1300 lb. per square inch, but began to show leakiness at all the joints when the pressure reached 1400 lb. On lowering the pressure again to 1300 lb. per square inch, no further leakage was observed, showing that the joints had been completely closed again by the elastic pressure of the bolts.

Considering that the intended working pressure of this vessel is only 1000 lb. per square inch, it was thought unnecessary to draw the bolts any tighter, although, according to calculation, the rings as well as the bolts are capable of resisting with safety above 2000 lb. per square inch. It was thought safer on the contrary to allow the bolts to be tightened up to such a point only, that, if by any accident the pressure should considerably exceed the ordinary working limit, they would yield by slightly elongating, and would thus act the part of an elastic safety valve in allowing the fluid pressure to escape through the metallic joints. The great length of the bolts ensures a sufficient elastic range of action for this purpose; and being made of steel containing 0·5 per cent. of carbon, they will retain their elasticity for an indefinite length of time.

This vessel, which was constructed at the Landore Steel Works, has now been delivered to the makers of the engine, Messrs. Greenwood and Batley of Leeds; and the engine will shortly be employed at Woolwich Arsenal as an air locomotive for shunting purposes.

The same principle of construction in the writer's opinion is applicable to hydraulic cylinders and accumulators, as represented in Fig. 5, Plate 34. In this case the longitudinal bolts need only be strong enough to tighten the copper joints, whereas the cylindrical steel rings have to be made strong enough to resist the hydraulic pressure. Taking, for instance, a hydraulic cylinder of 2 feet diameter, and an internal working pressure of 2 tons per square inch, the rings have to be rolled of a thickness of 1·6 inch, which corresponds to a working strain of 15 tons per square inch, or one-third of the breaking strain of the material composing the rings. This press would give a hydraulic pressure of 904 tons total, and would weigh probably not more than one-fourth of a press of the ordinary construction. The same argument would apply to accumulators of large dimensions, which could be built up of rings at a comparatively cheap rate, and of practically unlimited range.

In Figs. 6 to 8, Plate 35, is represented the application of this mode of construction to marine boilers. These boilers are necessarily of large diameter, and in constructing them of wrought iron, or even of mild steel, plates exceeding 1 inch in thickness have to be employed, and it is not easy to work and rivet plates of such thickness, nor is the riveted seam nearly as reliable as that of thinner plates. In Figs. 6 and 7 is represented a boiler shell of 10 feet diameter, of the proposed construction. It consists of twelve continuous rings, of $\frac{7}{8}$ inch thickness of metal, fastened together by sixty-four steel bolts of $1\frac{1}{16}$ inch diameter, which pass through the end plates, as shown in Fig. 8, and thus bind the whole fabric together. The front end plate is fitted with furnaces and steam tubes in the usual manner. A boiler of this construction and of these dimensions could be safely tested up to 200 lb. per square inch, the rings being sufficiently strong to withstand an internal pressure of 600 lb. per square inch; and it possesses, in common with the air vessel already described, the advantage of leaking, through the yielding of the elastic bolts, long before there is the least danger of explosion. It possesses moreover the additional advantage that it can be carried in pieces to be put together *in sitû*, thus facilitating carriage and avoiding the necessity of providing hatchways of extraordinary dimensions for putting the boilers on board.

In order to prevent galvanic action between the copper and steel rings, it will be found desirable to caulk the joints from within the boiler with india-rubber or with string saturated with some resinous compound, or simply to brush such a compound into the joints from within the boiler.

The interest at present manifested in the substitution of steel for iron for engineering purposes has induced the author to bring this paper before the Institution without waiting for practical confirmation upon an extended scale of the construction involved: the question is one rather of mechanical detail than of principle, the object being to treat material in such a way as to develop its maximum of resisting power when applied to the construction of vessels to resist high internal pressure.

Dr. Siemens said he should be glad to hear the opinion of practical engineers as to the probable advantage or disadvantage of the mode of construction described in the paper; but certainly the vessel constructed on this principle had so far given very satisfactory results; and he believed that vessels, such as air-vessels, could be constructed with rings in the manner described, both cheaply and with great safety. It would be observed that each ring had not to be turned throughout the entire width of the flanges, but had simply a groove turned in it at each end; and these grooves were all of the same diameter and section, so that when the copper packing-rings were put into position in the grooves the whole vessel was built up and the bolts were tightened, and there was no further labour expended upon it.

The copper packing-rings were from $\frac{1}{4}$ inch to $\frac{3}{8}$ inch thick, according to the diameter of the vessel for which they were used.

Dr. Siemens, in replying upon the discussion, said the plan of the compound cylinder, which had been described by Mr. Weems, was not at all the mode of construction that he considered the best adapted for vessels intended to withstand the high pressures contemplated in the paper.

With regard to Mr. Tweddell's suggestion that the joints shown in the drawing should be made with leather or hemp instead of with copper rings, he feared those soft materials would not stand very high pressure. Where there was a pressure of 1,000 or 2,000 lb. per square inch he had found india-rubber, for instance, always to give way. Moreover to make a joint with any of those materials, there would have had to be a sufficiently broad face. The ends of the rings or cylinders would have had to be faced completely, and a considerable breadth of face given to them. It was easy to roll out of a solid ingot a cylinder or ring with only a dwarf flange upon it; but the moment it was attempted to roll a large flange upon it the difficulty would be much increased, and the strength would be very much diminished, because, unless the rings were made of very soft material, they would stretch very much, and there would consequently be a tension between the flange and the cylindrical part. He had found the copper packing-rings to answer the purpose remarkably well. Except in one place the vessel was as tight as a bottle up to 1,300 lb. pressure per square inch. The ends of the cylinders did not touch within $\frac{1}{10}$th inch, and that gave a great opportunity for tightening up a bad place, if such a thing should occur. It had been mentioned in the paper that at one place the copper ring had probably been injured, and there was a leak; but by taking a narrow chisel and driving back the copper ring by gentle taps, it was rendered entirely tight.

He would not quarrel with Mr. Adamson for not immediately approving of this mode of boiler construction, which was so far different from his practice; only, if an objection were made on the score of the joints, it must be borne in mind that riveting was jointing all over; and surely if the total length of the riveted seams were taken, and compared with the length of joint in the construction now described, it would be found that there was less length of joint in this construction than in a riveted boiler.

Attention had however been called to a very important question of the unequal temperature to which this construction would be exposed when used as a marine boiler. He considered the mode of meeting that difficulty was rather a feather in his cap than otherwise. If a large horizontal cylinder, made of solid plate without riveted joints, were heated on its upper side while the lower portion was filled with cold water, a cross strain was naturally induced upon the metal. If that metal was not ductile, it would certainly break in a longitudinal direction; whereas if a number of rings could be put together, and these rings were fastened round the circumference at a definite number of points by independent elastic connections, then he maintained that the unequal expansion did not in any way affect the strength of the structure. Each ring could become by longitudinal expansion a little wider at the top or sides than at the bottom; but it had only to fight against the longitudinal tie-bolts, and these bolts were purposely made long. If they were not naturally long he should make them long, and he should make them of steel containing at least 0·5 per cent. of carbon; because a bolt of that description would not only bear a tensile strain of 50 tons per square inch with perfect safety, but its length would allow for all those variations in distance between the flanges which from time to time had to be contended with. Moreover it must be borne in mind that each bolt partook of the temperature of the boiler; where the boiler was hotter the bolt would be slightly hotter, and it would therefore adjust itself naturally to its work in that respect. Accordingly he did not see why a vessel constructed on this plan should become leaky through heating unequally in the way in which a marine boiler was heated unequally, that is, less at the bottom than at the top.

It had also been suggested by Mr. Adamson that it might be better to weld the circular flanges together. The objections he would urge against that plan were that the welding would introduce, to begin with, a great deal of labour, the material might be found to be injured, and the tensile strength of the material would certainly be very considerably reduced in the longitudinal direction. In the preceding paper there had been under consideration boilers constructed of very mild steel. But in this construction he proposed a departure from that principle as regarded the outer shell of the boiler. Instead of steel capable of bearing 28 or 30 tons tension per square inch, he would employ a material that would stand 45 tons per square inch. If such a material were to be riveted, it would be unreliable; or if there were to be holes bored in it, however carefully, for the reception of bolts, it would still be unreliable. But if a ring of such metal were rolled, the rolling being in the direction in which its strength was required, it might, provided its continuity was not broken in any way, be loaded up to one-half of its breaking strength with the utmost safety. The result was that he could get on these rings a working load of 15 tons per square inch, or probably more than double the amount he otherwise would venture to put upon the metal. Therefore he maintained that this was a light construction.

Attention had been drawn by Mr. Hawksley in the preceding discussion to the fact that very pure metals were more apt to corrode than metals containing a higher proportion of carbon, such as steel. Now as the outer shell of a boiler with internal flues had only to resist internal pressure, and was apt to corrode more than the fire-tube of the boiler, the shell, when constructed of the steel he proposed to use, would have all the advantage, that there would be undoubtedly less corrosion in steel of that temper than there would be in steel of a very mild temper.

A defect in the construction described in the paper had been pointed out by Mr. Paget, which must be patent at first sight, namely, that the metal of the rings did not aid in any way to give longitudinal strength to the boiler as a whole. That no doubt was the case, and it involved the necessity of providing for that longitudinal strength, which was done by the separate bolts; but a little calculation would show that the amount of metal necessary to provide against longitudinal rupture in a cylindrical boiler under any given pressure was very much less than the amount of metal necessary to provide against the lateral bursting strain. Although he could not answer the question absolutely, whether the bursting strain upon the metal would or would not incapacitate it for bearing its full quota of longitudinal strain—as he did not think there were any experiments to show this exactly, and it was always very hazardous to theorise upon a question of that sort—yet, supposing the metal was not in any way incapacitated for bearing its full amount of longitudinal strain, he could show that a vessel constructed on this very principle, notwithstanding the weight necessarily put into the bolts, was a very much lighter vessel for the same strength than if the plates were riveted together at the circular joints in the ordinary way. In the case of a hydraulic cylinder made in lengths, the bolts holding the successive lengths together had no strain to bear; only the lateral bursting pressure had to be dealt with, and the bolts had only to be strong enough to make the joints good.

Mr. A. Paget inquired whether Dr. Siemens had intended to say that in a hydraulic cylinder there was no tendency to burst the cylinder endways. He could understand that to be the case so long as the ram was free to move; but as soon as the ram was stopped by the resistance, how then?

DR. SIEMENS said that whether the ram was free to move or was held from moving, there was no tendency to strain the cylinder endways, *i.e.*, provided the end away from the ram was supported by an abutment of some kind, as was usually the case, to take the thrust, as shown in Fig. 5, Plate 34.

With regard to the inquiry about any tendency of the separate rings or cylinders composing the boiler described in the paper to slip transversely upon one another, or to become depressed vertically when the boiler was arranged horizontally, he had made no special provision against this, and there was really, as far as he could see, no need to do so. The copper packing-ring of about $\frac{3}{8}$ inch thickness, forming the joint, was wedged in between the four faces of the grooves in which it was laid, and would, he thought, be capable of resisting any amount of accidental side straining that might come upon the separate lengths of the vessel. The pressure of the steam would of course have no tendency to slide one length past another. If it was thought necessary, a hoop round the joint could easily be provided on the outside to prevent slipping; but he would prefer not to do so, but to let each cylinder be perfectly at liberty to take its own position and to expand as it liked. The utmost lateral pressure that could be brought to bear upon the vessel to displace any cylinder in that way would be amply borne by the mere friction, even if there were nothing else.

In the discussion of the Paper

"ON THE DESIGN GENERALLY OF IRON BRIDGES OF VERY LARGE SPAN FOR RAILWAY TRAFFIC,"

By Thomas Curtis Clarke, M. Inst. C.E.,

Dr. Siemens* remarked that it was stated in the paper that the strength at which the iron was to be tested was equal to 60,000 lbs. (nearly 30 tons) to the square inch, and that it was to be subjected only to 10,000 lbs. of strain in tension. Surely there must be some error in that statement, as he knew of no iron that

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. LIV. Session 1877-78, pp. 234-235.

would stand a breaking weight of 60,000 lbs., except puddled steel which was not the material used according to the specification, and if such iron could be obtained the weight with which it was to be loaded, viz., 10,000 lbs., appeared needlessly small, being equal to $\frac{1}{6}$ only of the total breaking weight. American engineers, if the author represented them correctly, had very little confidence in steel as a building material. Perhaps he might be allowed to make a few remarks with regard to that material, to which he had paid considerable attention. A distinction ought to be drawn between steel and steel. There was a material called steel, but which, in reality, was the purest iron ever introduced to the notice of engineers. It contained 99·6 per cent. of metallic iron, and only 0·4 per cent. of foreign matter of every description, whereas the best so-called iron contained between 3 and 4 per cent. of foreign material. Mild steel was really iron of the best character, and he could not conceive how such a material could be thought unreliable in its application. It was produced, not like puddled iron, in small quantities to be welded together with the chance of enclosing foreign matter, and producing irregular results; but it was produced in large masses—10 or 12 tons of fluid substance—and there was every probability that such a material was uniform to the utmost degree. Practice had, indeed, fully substantiated the fact that there was no material more uniform than that very mild steel. It would bear 28 tons breaking strain to the square inch, simply because it was iron, for no one would pretend that cinder would bear a strain equal to that of iron. In mixing between 3 and 4 per cent. of cinder with the iron, some of its strength must necessarily be lost; otherwise it had all the qualities which iron ought to possess. But for engineering purposes he would restrict the use of very mild steel. It was excellent for the construction of boilers, and for the construction, perhaps, of continuous girders, because it was very reliable. It might be loaded to one-half its breaking strain without any sensible permanent set, and if it were loaded beyond that, it would not rupture, but it would elongate to the extent of 25 per cent.; therefore, in constructing a girder bridge of such material, the chances were that it would bend down to the bottom of the river rather than break. But it was not the strongest material that could be used. In the New York bridge, which he saw in progress last year, the steel wire was tested to nearly 100 tons per square inch. In his own practice he generally applied 80 or 90 tons as the weight which steel wire, for telegraphic purposes, ought to bear. But between the two limits—between the material that would bear a breaking strain of 28 tons, and elongate 25 or 30 per cent. before breaking, and the highly wrought material which would bear 80 or 90 tons to the square inch—there were a great many steps, all of which were applicable under circumstances such as practice must indicate. For links, he should consider that a material capable of bearing from 45 to 50 tons to the square inch would be the best material. It would be sufficiently yielding to excessive strain ; it would yield 10 per cent. before rupture took place, and at the same time it could be loaded to certainly 15 tons to the square inch with safety. In constructing long bridges, the difference was so great, whether using a material that could be loaded safely and practically with 15 tons, or a material safely bearing a strain of only 5 tons, which was the limit now imposed upon iron, that there could be no question, in the long run, which was the right material for such large structures as were referred to in the paper. As regarded compressive strain, steel must be looked upon as potentiated wrought iron, bearing a strain of compression equal to that of extension ; but it must be borne in mind that the smaller scantling of the steel under compression necessitated a change in construction with a view to giving the requisite amount of lateral support ; it was therefore not sufficient to make a design in iron and reduce the scantlings in substituting steel, as had been the practice to some extent.

In the discussion of the Paper

ON THE CONSTRUCTION OF ARMOUR TO RESIST SHOT AND SHELL," by Capt. C. O. Browne, R.A.,

Dr. C. W. Siemens * said he had been present at the experiments on board the "Nettle," to which the paper referred; and the conclusion he had arrived at was that "compound" armour plating, such as Mr. Wilson's, would not give all the results that were expected of it. It stood to reason that if two metals, different in their character and in their rate of expansion, were united, there must be a tension set up between them at the surface of junction. If such a compound body was struck by a shot, the inevitable consequence must be, either that the two metals would separate, or that the weaker metal would be torn to pieces by the stronger. The latter seemed to have been the case in the experiments; the iron seemed to be almost in a condition of powder wherever the projectile had penetrated into the iron plate. The plate of Sir Joseph Whitworth was on a different principle, and that plate no doubt withstood the action of the shot remarkably well. But he thought Sir Joseph Whitworth had not at that time fully developed his idea; because the steel plugs there used acted as so many starting points for cracks. These cracks started not only where the shots struck, but also at other points, where it happened that there was a tension existing. It was known that a steel plate which would resist almost any amount of ill-treatment while it remained a continuous whole, would crack or tear wherever the continuity was broken; hence in dealing with steel any break of continuity ought to be strictly avoided. Sir Joseph Whitworth had since constructed armour plates that had resisted shot exceedingly well; and in these he had substituted rings of steel carefully tempered and then screwed together, one ring inside the other. By this means he had avoided the tendency to form starting points for cracks. But that construction had the disadvantage of being one which few works except Sir Joseph

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* Excerpt Minutes of Proceedings of the Institution of Mechanical Engineers, 1879, pp. 71–73.

Whitworth's could be trusted to carry into effect; because screwing large plates, after being tempered, one into the other, required an amount of accurate and careful workmanship which he was afraid could not be depended upon under all circumstances, and would probably be exceedingly costly. It appeared to him on general principles to be right that the projectile should be very tenacious against suffering any alteration in form, in order to penetrate any resisting surfaces it might encounter; on the other hand the resisting surface of the armour plate should be of a yielding character. No doubt if armour were made very hard and very thick, and were then fired against with a shell of inferior tenacity (*e.g.* a Palliser chilled-iron shell), that shell would fly into a thousand pieces. But it would always be possible to make the shell of a superior material to the plate which had to resist it; the shell was a smaller piece, a piece that could be dealt with by oil-hardening and tempering much more effectually than the plate, and could be put through all the preparatory processes much more systematically and uniformly; therefore he believed the projectile would always have the superior metal, and if that was the case, then the resistance should be of such a character as to involve the greatest amount of work in overcoming it. The plate should not be designed to resist absolutely, by destroying the projectile; but to yield to the greatest possible extent to the penetration. Hence, of the different constructions represented in the drawings, the sandwich target recommended itself exceedingly well to his favour. It must involve a great amount of work for the shot to penetrate to the extent there shown. He believed the French had tried to make armour plates of steel that should resist in a similar way, by using a very ductile and uniform quality of steel; and it would be interesting to see which system would ultimately carry the day. At the Creusot works plant had been laid down at great expense to produce large masses of mild steel; and if they succeeded in giving those large masses such uniformity and ductility as to yield without cracking, they would no doubt produce a very formidable kind of armour. But the "sandwich" system presented another way out of the difficulty, which probably possessed at least one advantage, that of cheapness.

In the discussion of the Paper

"ON THE CONSTRUCTION OF HEAVY ORDNANCE,'

By James Atkinson Longridge, M. Inst. C.E.,

Dr. Siemens * said the author complained, and with some reason, that on the occasion of the reading of his previous paper, nineteen years ago, the discussion had wandered away a good deal from the lines laid down by him. The present paper presented such clear lines that it was not difficult to keep within them, and he would endeavour to do so. The paper might be divided into two parts. The first was the critical, and, he might say, destructive part, because the criticisms were such that, if completely borne out, one would almost be afraid to contemplate the result of a general war. The second part was suggestive or constructive. With reference to the criticisms on the Woolwich system, he was ready to admit that the author was very clear in his mathematical deductions. He had pointed out some error in one of the formulæ used at Woolwich. Passing that over, he thought that the formulæ which were used—those of Professor Barlow and Dr. Hart—were not erroneous in themselves; but, as the author had shown, were insufficient to meet the case of the strain in every portion of the structure. The author had from that drawn the conclusion, that in not following out a formula, such as he had presented, the authorities had gone altogether wrong, that the strains by shrinkage which were applied at Woolwich were totally opposed to theory, and that much better results might be obtained in strictly following out the dictates of mathematical reasoning. Mathematics, however good a handmaiden, might be a very dangerous master; and he thought he could show where the author had made mathematics his master. He had given his idea of a gun constructed on the Woolwich principle—a 9-inch gun. There was a steel tube of 8 inches thickness of side, around which he shrank iron to a thickness of 28 inches, with a shrinkage exceeding that usually given at Woolwich, namely, of 1 in 1,000.

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. LVI. Session 1878–1879, pp. 207–212.

Dr. Siemens knew from experiments of his own that a shrinkage of 1 in 1,000 meant a strain of 12 tons per square inch. If the shrinkage was 1 in 1,000 upon the inner diameter, there would be a strain of 12 tons upon the inner surface of the coil. That in itself presented a very good strain for a material such as iron. If, however, round a tube of 8 inches inside, 28 inches of iron were shrunk in such a way as to put the whole into tensions varying in elastic strain inversely as the diameter, what would be the result? That the strain on 28 inches would be opposed by the 8 inches; that the area of the outer metal would be resisted by the lesser area forming the side of the lining. The author, in all his calculations, spoke only of what would take place when the powder pressure of 24 tons to the inch was applied; he did not say what would be the case when the shrinkage pressure was applied in the first instance. That shrinkage pressure would undoubtedly have the effect of crushing or deforming the tube permanently. The tube would be no more able to resist such an amount of shrinking pressure than if it did not exist at all; and therefore such shrinkage as was contemplated by Mr. Longridge could not practically be applied. The author had criticised rather severely some remarks in the Woolwich reports. Referring to the statement that in "guns of present construction the heavy breech coil compresses the steel barrel to such an extent that the latter becomes in some instances as much as $\frac{1}{100}$th of an inch smaller in diameter during the process of shrinking," the author had remarked, "It is difficult to imagine a more complete confusion of ideas than that which pervades this sentence."

Dr. Siemens believed the statements in the official report were perfectly consistent, not with mathematical, but with engineering or physical facts. It would, he maintained, be impossible to give such shrinkage to the mass of iron surrounding the inner tube as would leave that mass of metal under a sufficient tension to take its full proportion of work when an inner pressure was applied equal to 24 tons to the square inch. But suppose that the inner tube could be made strong enough to resist such a pressure, the use of the gun had then to be considered, and the result of the first few rounds. Shrinkage would then again take place, but upon the wrong side of the gun; that was to say, expansion by heat would occur inside the gun, which would still farther increase the strain upon the inner tube; and if it was able to resist the crushing pressure in the first instance, the heating of the inner tube would greatly augment the strain. He thought that was the reason why men like Sir William Armstrong and the Woolwich authorities, who well knew what they were about, had not ventured to give that distribution of strain by shrinkage which mathematical reasoning would lead them to adopt. The author would probably admit that the mathematical knowledge which he brought forward nineteen years ago would not have remained idle for so long a time if it could have been advantageously applied; but Dr. Siemens was clearly of opinion that in the construction of the Woolwich guns it was impossible to apply that reasoning properly. But the author did not really intend to make a gun by shrinking on iron rings upon a steel core; he had brought forward a construction of his own, which had something very pleasing to recommend it at first sight. He took a lining tube, and bound upon it several layers of wire, increasing and varying the strain of the wire in such a way that when the inner pressure—the maximum powder pressure—was applied, each portion of the material bore the same amount of tensile strain. There again, however, the author had not contemplated, or, at all events, had not brought forward, the result of the crushing action of that amount of binding force upon a comparatively small tube. The tube was made of cast-iron, which resisted compression better than extension, and to that extent the construction might be very proper; but the powder pressure in heating the tube would, he apprehended, work a change in the tensions, the same as it would do in the iron coils, either causing the wires to be overstrained, or the tube to be crushed under that strain. The question was, however, whether it would be possible to put the material into any more satisfactory form. When the powder pressure acted, artillerists would like to distribute the force uniformly over the material; but they had to contend with the different portions under the two conditions of rest and of action from within. One great drawback to the author's proposed system, which he himself admitted, was that he had a resistance against bursting strain without resistance in the longitudinal direction. The author remedied that by putting a very heavy mass into the breech piece of the gun, connecting that heavy mass with a protecting tube which did nothing towards resisting the bursting strain of the gun. This was a great drawback to that mode of construction. The amount of metal which actually resisted the bursting strain was probably not more than one-third of the total weight of the gun, the other two-thirds being used for a totally different purpose—to resist recoil and longitudinal action. Why should a wire coil be used? Was metal in the form of wire susceptible under ordinary circumstances of bearing any greater resistance than solid metal? Certainly not. The same strain per inch could be resisted by solid material. In the case of wire there was certainly this advantage, that inasmuch as the iron of commerce was a mixture, or concrete, of the metal iron and a glassy substance called cinder, if that glassy substance were mixed in an irregular way with the iron it would give no tensile strength at all; but inasmuch as the layers of glass were drawn out in a longitudinal direction, they did less harm than they would do if mixed promiscuously; and for that reason wire gave a larger resisting strain than solid metal. That, however, was not the case with steel. Steel wire resisted no better than solid steel. On the contrary, steel wire, while resisting no better per square inch, would give less elongation than solid steel; but the author would say, "You cannot, with solid steel, distribute your strains in the manner I tell you to do; there ought to be more strain near the outer layers, and less towards the inside of the gun." But there again he thought the steel maker would not be at a loss. If it was desired to distribute a compressive strain on the inner surface of the ring and a tensile strain on the outer surface, could that not be obtained in any other way than by cutting the whole thing up into wire and winding it round, thereby losing all the advantage of longitudinal resistance? He thought it was quite possible. If a cylindrical mass of steel were heated in a furnace to redness, then, while the steel was in the furnace, a cooling action applied inside—say, a spray of water—would immediately cause a solid tube of what was called set or cold metal to be formed inside the mass. The inner diameter would remain the same, because it was governed by the general mass of the tube. The metal would shrink, not in the direction of the diameter, but in the direction of the thickness of the tube, which thickness would become less. If the cooling action went on while the outside was at its full temperature, layer after layer of metal would be formed at a gradually reducing temperature, all of which would be, under those conditions, in a state of perfect rest and equilibrium. After some time, perhaps an hour, there would be an outside temperature of say 600° Cent., and at the inside 100° Cent. The coil being taken out of the furnace and allowed to cool, the result would naturally be that in each layer a tension would be formed proportionate to the relative temperatures. The contraction on the outer surface would be very great, and it would act as shrinkage upon the rest. The shrinkage of the surrounding layers would be less and less, and the total result would be a tension which might be represented by a diagram showing a maximum positive strain near the outside of the gun, and a maximum negative strain in the metal nearest the inside of the gun, the neutral axis being somewhere midway between the two. The explosive action of gunpowder upon such a tube would cause the internal negative pressure to be transferred into a positive pressure; it would act on the larger diameter in a less and less degree; and, when the full pressure was acting, there would be a field of compression which might possibly be represented by a diagram showing equal positive tension throughout the mass. In that way it would be possible to obtain, by means of a single ring, all the advantages which the author claimed for his wire system, with the additional advantage of having strength in all directions, it being unnecessary to amplify the gun after all the required strength was obtained, by two or three times the amount of dead weight. He believed that an inner tube would always be necessary; but it should not be a resisting tube. A tube of hard metal surrounded by comparatively weak metal, appeared to him to be a great mistake, which was shared both by the Woolwich system and by the plan proposed by the author. The inner tube should be of metal that accommodated itself entirely to the outer tube and to the necessities of the gun. It should be extra mild steel, which would expand or extend 30 per cent. without break in its continuity. Any hard metal, such as hard steel or cast iron, would be subject to such action and reaction as to bring about its final destruction; whereas a lining of metal that was like putty in its constitution, coupled with great strength, was, in his opinion, the proper lining of a gun surrounded, not by a series of rings, but by one ring or tube, in which the strains were in the first instance so arranged as to give each portion of the metal an equal strength when the maximum powder-gas pressure was applied. He had made a great many experiments with steel in the form of wire and in the solid state. He had not, perhaps, explained as fully as he ought to have done what he meant with regard to wire being no stronger than solid metal. In speaking of steel wire he supposed it to be annealed wire, that had not been subjected to any hardening process; but if the wire were cold-drawn, or oil-hardened, its elastic range would certainly be greatly increased. Any kind of steel, the mildest and the hardest, yielded in the same manner and to the same extent by applying the same amount of weight per square inch. The only difference between hard and strong and weak steel was, that the weak or mild steel came sooner to the limit of its elasticity. If it was desired to compare steel wire with steel in bulk, the steel in bulk should be put into a similar aggregate condition. Sir Joseph Whitworth could produce steel to bear a strain of 80 tons or more in the bulk after he had oil-hardened it; and Dr. Siemens knew from experience that it was exceedingly difficult to get steel wire, even when oil-hardened, that could be depended upon to resist more than 80 tons per square inch; occasionally 100 tons, or even 110 tons, might be reached in very thin wire, owing to great success in the mode of hardening. In order to make a fair comparison, a metal in the form of wire should be compared with metal in bulk subjected to analogous processes, when it would be found that the absolute tensile strength was nearly the same, whereas the solid steel had generally the advantage of elongating to a greater extent than wire, before rupture took place.

In the discussion of the Paper

"ON SOME PHYSICAL CHANGES OCCURRING IN IRON AND STEEL AT HIGH TEMPERATURES,"

By Mr. T. Wrightson,

Dr. Siemens * said that owing to a bad cold he should have difficulty in making himself understood, but he would endeavour to say something on Mr. Wrightson's paper. He thought they were indebted also to their friend Mr. Bell for having taken up the question experimentally. Mr. Bell's results were to a certain extent confirmatory of Mr. Wrightson's, and opposed to the hypothesis brought forward some years ago before the Royal Society by Mr. Mallet. There seemed to be now no doubt that cast iron expanded in setting, and that it followed the general law of solids in contracting with diminution of temperature only after it had set. That was in itself a very important fact, and with its assistance they might be able to discover the true cause of such a physical phenomenon. Mr. Bell had a difficulty in accounting to the full for Mr. Wrightson's assertion that a metal ball floated on the surface when at a considerably lower temperature than that which would follow from the physical consideration brought before them by Mr. Bell. It appeared to Dr. Siemens, however, that Mr. Bell might have added one other cause to those which he had very ingeniously mentioned to account for his brick of partially heated metal rising to the surface much sooner than it could be expected to do, judging simply by the fact of its expansion by heat. Mr. Bell assigned the action partly to the occlusion of gas on the surface of the solid. No doubt that was a good reason, but it was a reason which would apply more forcibly to small balls or pieces of metal than to large ones; and so far as they could learn, the phenomenon was not influenced in any sensible degree by the volume of metal they were dealing with. Mr. Bell also brought forward a reason, which to Dr. Siemens's mind did not apply—namely, that the currents of hot fluid metal set up in a bath would

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* Excerpt Journal of the Iron and Steel Institute, 1880, pp. 35–37.

tend to float up the brick of solid metal. Dr. Siemens was decidedly of opinion that the fact of the brick being at a lower temperature than the metal would cause downward currents, and, therefore, the mechanical effect of such currents must be to draw the brick itself downwards rather than upwards. But there was one cause Mr. Bell had not mentioned, which he thought might have considerable influence in producing flotation. In pushing a mass of iron below fluid metal, it began to heat on the surface, and consequently expanded superficially, and that expansion caused a tension on the metal in the interior. From a hurried calculation he found that the amount of heat required on the surface in order to expand the interior mass 1 per cent. was not beyond the limit of what they could expect. Iron expanded, for 1° Fahr., 0·000018 of its bulk, and, therefore, it would expand 1 per cent. for an increase in temperature of 550° Fahr. This difference of external and internal temperature, in the opinion of Dr. Siemens, would account for the expansion in the interior, and would help them over the difficulty of accounting for the rising of the briquette. The difference of volume was at any rate not so great as to invalidate the theory brought before them by Mr. Wrightson. As regarded the cause of the observed phenomena, it appeared to Dr. Siemens that there was nothing unnatural or improbable in assuming that metal would change its density at certain points, accompanied by changes in other physical conditions. As to changes in its physical character, there were other substances to guide them, and they had a remarkable illustration in the metal selenium. That metal had been fully investigated on account of the extraordinary phenomenon it presented of becoming less conductive of electricity when under the influence of a ray of light. His brother (Dr. Werner Siemens) had examined the conditions under which this change took place, and he found that when selenium was allowed to cool gradually, it suddenly at a certain point changed its capacity for heat. The thermometer which dropped in a uniform ratio until this critical point was reached, suddenly rose, showing that the selenium at that point parted with a considerable quantity of latent heat. He believed that if Mr. Wrightson could extend his experiments to thermometrical measurement of an accurate kind, he would find that cast iron when it began to expand absorbed a great deal of heat which became latent, a circumstance which would account for the greater bulk assumed at that point. He might mention a circumstance, analogous to that already given with regard to selenium, appertaining to iron itself. If an iron wire were heated to whiteness, allowed gradually to cool, and the variations in its length and colour observed, they would find that when it was almost becoming black it suddenly lighted up again and expanded, showing that in this wire of iron which was exposed to no sort of external heating agency a sudden evolution of heat was produced. This evolution could only be accounted for by the sudden departure, out of the mass of iron, of latent heat. It was probable that the phenomena brought forward in Mr. Wrightson's able paper might be found to be the result also of such sudden changes of specific heat.

In the discussion of the Paper

"ON THE PHOTOPHONE," by Professor A. G. Bell,

The Chairman (F. J. Bramwell, F.R.S.) having invited Dr. Siemens to explain his "Selenium Eye,"

Dr. Siemens, F.R.S.,* said he had listened with intense interest to the discourse which Professor Graham Bell had given. The world had been astonished before with his invention of the telephone, and now he came forward with an instrument equally marvellous in its results. The property of selenium to alter its electrical resistance under the influence of light, was, as had been stated, first brought before the world by Mr. Willoughby Smith, and so remarkable was this discovery that many physicists turned their attention to the subject. His brother, Dr. Werner Siemens, took up the inquiry with a view of determining the cause of this extraordinary variation in resistance caused by light, and the conclusion to which

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* Excerpt Journal of the Society of Arts, Vol. XXIX. 1880–81, pp. 43, 44.

his researches, which were communicated to the Berlin Academy, led him, was that the resistance of selenium, and probably, indeed, of all substances, varied inversely to the amount of heat which they contained; and the reason why selenium showed such extraordinary changes under the influence of light was, that under that influence, it changed from one aggregate condition to another—from an amorphous to a crystalline condition; and that at the moment when this change took place, a great deal of heat was absorbed, and therefore the specific heat of the selenium was very much increased. This was strictly a molecular change, and bore on the further discovery which Professor Graham Bell had made, that he could hear the changes going on even in gaseous bodies, produced by the passage of light. The little instrument which he (Dr. Siemens) had constructed to show the members of the Royal Institution was on the table. It had the form of an eye, and on opening the lids, a lens was presented to the light; through that lens, the light, falling upon it, was concentrated upon a spot in the interior of the ball. At that spot one of the selenium gratings, which had been described, was placed, a grating not larger than a threepenny piece, consisting of five wires laid in zigzag fashion; one wire was connected to the positive, and the other to the negative pole of a battery. These wires, lying close together, but not touching, were laid on a plate of mica; a drop of selenium was placed upon them, and this small quantity sufficed to produce the desired results. The principal object he had in devising it was to construct a selenium photometer; but a difficulty arose in using it for that purpose, because selenium got fatigued under the influence of light. The eye, after being exposed for any considerable period to an intense light, became insensitive, and the lids had to be closed; it had to go to sleep for some time before it regained its sensitiveness, and the analogy to the human eye went even further than that. If the eye were used after having been kept in the dark for a length of time, it would detect the slightest gleam of light, and mark it on the galvanometer, whereas after it had been once used in intenser lights, a small gleam would be utterly lost upon it, until it had again had ample rest. The instrument before them had not been used for some years, and it might still be active, but the audience would have to take the Chairman's word for it, since the galvanometer in circuit with the "eye" was not one whose indications were visible to a number of persons at once. [Dr. Siemens then experimented with variously-coloured sheets of cardboard prepared for the purpose, and the reflected light was found to cause a deflection of the galvanometer in each case, the slightest effect being produced with light reflected from a black piece of paper, and successively increasing with green, red, and white, the greatest of all being produced by exposing it to the direct light of an argand burner.] These experiments showed the great sensitiveness of selenium ; but Professor Bell had gone much further, and had prepared an instrument with concentric plates of selenium and intervening plates of mica, and operating upon a much larger surface. He had gone much further than had been done previously. Then came the further step which he had so boldly taken, of making light become the carrier of speech. As he had justly said, this seemed marvellous at first, but when you knew how to do it, it became simple, like everything else, and he (Dr. Siemens) must congratulate the Society on having had the method of doing it so clearly explained.

In the discussion of the Paper

"ON THE WEIGHT AND LIMITING DIMENSIONS OF GIRDER BRIDGES," by MAX AM ENDE, Assoc. M. Inst. C.E.,

DR. SIEMENS * said, though the subject was one rather of construction of bridges, than of the durability or mode of treatment of materials, he thought that a few observations might not be inappropriate with reference to the remark of Mr. Bender, to the effect that steel gave way with a strain of 60 per cent. of the calculated strain. Steel was essentially a different material from iron. The difference was like that between parchment and woven fabric. In iron there were fibres which acted separately, so to

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. LXIV. Session 1880–81, pp. 289–290.

speak; whereas in steel, although there was a greater total strength, the material must not be strained to an undue degree at any one point. To make his meaning clear, he might be permitted to describe an experiment which he had occasion to make some time ago, with the view of constructing a link for a large bridge. The first idea was to rivet two bars together in the way usually adopted. In tying two steel bars riveted together in the way represented, both bars being tapered breadthways to a point, and the rivets forming a diamond shape, he found that the breaking strain across the joint was, as Mr. Bender had described, about 60 per cent. only of the strain which the net area ought to give. But what was the result of the experiment? The steel did not break across the full section of the united bars, or across the section of the greatest number of holes, but in bringing on the strain to the bar, the first rivet and the last rivet would receive nearly the whole strain. The material itself, being highly elastic, acted as one body in the middle, and the elastic strain was thrown to the end rivet on either side. The end rivet generally gave way with shearing. The two rivet-holes that followed had sufficient resistance to commence what might be called a tearing action through the solid bars, and the total resistance was certainly very much below what it should have been. Another result which came before him in connection with Lloyd's surveys, was this: A test-bar was riveted to a strong pair of tongs; and, to the surprise of the gentleman who made the test, the bar, instead of giving way at the point of least section, gave way at the foremost bolts attaching the bar to the tongs, again showing that the two rivets receiving the strain in the first instance, set up a tearing action, which made the line of breakage about three times the length of the line of least section. In all steel structures that ought to be borne in mind. Bars should never be joined by simple riveting; they should be dovetailed together in such a manner that no tearing action could be set up. He believed, when engineers had mastered the art of constructing with steel, it would be found to be a thoroughly reliable material.

In the discussion of the Paper

"ON THE SOCIETY OF ARTS' PATENT BILL,"

By Sir Frederick Bramwell, F.R.S.,

Read before Section G. (Mechanical Science) of the British Association, at the York Meeting, September, 1881,

Dr. Siemens, F.R.S.,* said if anything were needed to show the difficulty surrounding the framing of a good and just patent law, the observations that have fallen from the last two speakers would furnish incidental proof. Mr. Head, who is so well known for his mechanical talent, suggests that the obtaining of a patent should be made very difficult—that the patentee should not only prove that it had novelty, but that it had usefulness. I am afraid that, if that suggestion were adopted, many valuable patents would fall to the ground or be stillborn. It is the very essence of an invention that it cannot be worked in its first conception, because an invention is not a mere idea. An idea may strike the mind in an instant, but an invention is necessarily the result of labour—mental and physical—and of expenditure, and there is hardly an invention ever brought out that in its first stage would have stood such a test. I cannot agree with Mr. Head in supposing that all those inventions that have not taken immediate effect, and enriched the patentees, are so much loss to the country. On the contrary, although the inventor is to be felt for who has not reaped any benefit from his invention and for his labour, yet the public at large profits by it, because it may form the stepping-stone for somebody else to carry the idea to its practical point. The patent law must not be based upon the idea that all difficulties will be done away with, that all men are to be made happy, and that there is to be no legal contention of any sort. That would be a chimera such as could not reasonably be expected. If it is difficult to establish a title to landed property, surely it may be reasonably supposed that it is as difficult to establish a title to the product of the mind; and all we can do is to render the ad-

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* Excerpt Journal of the Society of Arts, Vol. XXIX. 1880–81, pp. 813–814.

ministration of that property as simple and as just all round as it possibly can be made, humanly speaking. The patent law worked out nominally by the Society of Arts, but in reality by my excellent friend, Sir Frederick Bramwell, is, I think, the best considered, and, perhaps, the most perfect attempt at a just and equitable law on the subject ; and I, as one of the committee, can only hope that it will find favour in this Section in order that it may be strengthened by the weight of the British Association, and that the Legislature of the country may take a similar view. It is idle to discuss partial questions connected with such a law, as, for instance, that the fees to be paid by a patentee should be a great deal less. It is now proposed also to extend the operation of the patent over twenty-one years instead of seventeen. You may depend upon this that all these questions have been very carefully considered by the committee, and also tested by legal opinion, and that this Bill is the result of the careful and long meditations on the subject by Sir Frederick Bramwell, and of the discussions that took place in the committee, of which he was the chairman. I may go further, and say that it is the result of previous discussions that have taken place, not only in this country but abroad—in Vienna, where the Patent Congress met at the opening of the Universal Exhibition. Again, in Paris, where at the time of the last Exhibition, a very long discussion took place, all these questions have been considered, and the best thing we can do is to accept it *en bloc*, and not attempt in the course of the slight discussion, such as we can afford to give to it, to alter any of its more important clauses.

In the discussion of the Paper

"ON IRON PERMANENT WAY," by C. Woods,

Dr. Siemens* following Mr. R. Rawlinson, C.B., who had remarked that "no doubt Dr. Siemens, who knew so much,

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers, Vol. LXVII. 1881–82, p. 27.

particularly of modern improvements, would say that iron would soon be a past manufacture," said he was hardly bold enough to make such an assertion ; at the same time he believed that steel was on the whole preferable for the new application. While continental nations had been giving great attention to the introduction of metal permanent way, England had remained perhaps too partial to timber as the material to be employed. It was true that English engineers would now have the advantage of the experience already gained, and when turning their minds to the subject they would soon perceive not only the advantage of the metal system, but the most practical mode of carrying it into effect. Iron should be strong enough for the purpose, because each portion of an iron cross-sleeper was apparently not strained beyond its capability of resistance ; still he apprehended that homogeneity was of great importance, because where the fastening held, the metal was strained to a very considerable extent—an extent which could hardly be determined *à priori*, and in all such cases steel yielded before it fractured under compressive strains. Then again, he believed that the introduction of steel sleepers would be of great advantage to the manufacturer, even though he should not realize large profits from the actual operation. It was well known that in Germany, where iron and steel sleepers had been largely introduced, the manufacturer was glad to supply them at a comparatively low cost, because he could go on with the manufacture without waiting for specifications. It was always the same thing, and whenever he had no other work on hand he could turn to rolling sleepers ; moreover, although a good metal should be employed, it was not necessary to use entire ingots, but considering the short length of a sleeper, odds and ends of the material could be utilized to a considerable extent. Therefore he had no doubt that, for the two reasons he had mentioned, the comparatively low price at which sleepers could be supplied, and their great permanency when once laid down, iron or rather steel permanent way would soon be very largely, if not universally, introduced into this country.

In the discussion of the Paper

"ON FORCES AND STRAINS OF RECOIL CONSIDERED WITH REFERENCE TO THE ELASTIC FIELD GUN-CARRIAGE," by HENRY JOSEPH BUTTER, M. Inst. C.E.,

DR. SIEMENS * thoroughly agreed with the mathematical proposition put forward by Professor Unwin, which, indeed, admitted of no doubt. At the same time, as Mr. Cowper had already pointed out, there were great deductions to be made. All the friction had to go in reduction of recoil, and that friction must necessarily be largest at the commencement of the action when the charge was rammed tight home. Then, again, the friction of the gun-carriage upon the ground might be very considerable, and that had to go in reduction; so that theory and practice, as propounded, and so well argued by the author, seemed to agree nearly enough for general acceptance. He should like to add a word with regard to an observation from Mr. Cowper regarding his connection with the question of hydraulic compressors. All that Dr. Siemens could claim was the mere suggestion of hydraulic compression for gun-carriages, and that had been gracefully acknowledged by the then head of the department (Colonel H. Clerk, R.A., F.R.S.), in a Paper, read about a year after the suggestion was made, before the British Association. The fact of his suggestion, however, in no way detracted from the great merit due to the officers of Woolwich, and especially to the author, for the thorough way in which the hydraulic pressure had been worked out for stationary guns, and had been now brought forward as applicable to field guns. He could not help thinking that the term "elastic gun" was unfortunate, because it gave a wrong idea. Although the author had explained that it meant only one portion of the elastic action without the elastic rebound, it was essentially an inappropriate

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* Excerpt Minutes of Proceedings of the Institution of Civil Engineers. Vol. LXVII. Session 1881–82, pp. 148, 149.

name. One question which presented itself was whether the hydraulic compressor applied to a field-gun was a complication. It was, unquestionably an additional part; but he did not think that every additional part to a machine meant a complication. If by the addition of 1 cwt. 1 ton could be saved without subjecting any portion of the material to a greater strain, that really meant simplification; an effect was produced with less weight of machinery. The hydraulic cylinder with a single piston was an extremely simple mechanism. No valves were connected with it; there was nothing that could possibly take harm; and he believed that Colonel Shakspear would have found that a gun of that description would not come to any harm from the rough usage to which he had alluded; and after it had fallen would have been more readily put on its wheels again than the older form. The author's proposal was a move essentially in the right direction—the lightening the material of the field-gun—and he thought it was done without complication, if complication meant liability to get out of order.

In the adjourned discussion

"ON THE PATENT LAW AMENDMENT BILL,"

Dr. C. W. Siemens, F.R.S.,* said, on listening to the discussion, he had been reminded of a fable of antiquity which he had learnt in his youth, to the effect that Jupiter, at one time when there were a few people in the world, thought that as he had heard a good deal of grumbling about the weather, he would give them the option of fixing their own weather; and, accordingly all the craftsmen met together, the agriculturist, the grazier, the miller, and the potter, in order to debate what weather would suit them best. He need hardly say what was the result of the meeting—they all thoroughly disagreed. The potter thought it was outrageous to have a shower nearly every day, which suited

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* Excerpt Journal of the Society of Arts, Vol. XXX. pp. 115–117.

the grazier; the grazier thought a long drought, such as the agriculturist required, was most detrimental to him; and the miller thought unless he had a deluge of rain every week, his water-power could not be kept up. The case before them was almost parallel to that of his fable. The Committee had undertaken to frame a Bill which should be agreeable to the lawyers, to the patent agent, to the inventor, both rich and poor, and to the consumer, the public at large. At the previous meeting they heard how the representatives of the High Court of Justice found they had trespassed on their prerogative. They thought law without the High Court of Justice would be an abortion, because there could be no compensating claims, such as breach of promise of marriage, brought into a Patent Law suit, and that would be a great pity. Probably the Bill required some amendment as regarded legal procedure, but what was wanted in the patent interest was cheap justice; a law which did not take up inventors' time for years and years in contending patents, which might probably be of interest to lawyers, but which prevented patentees from following the peaceful mission which it was their province to pursue. Passing from the purely legal question, to that of administration, he came to the objections brought forward by a very eminent patent agent, who no doubt, being very confident of his own skill and power to advise his client, rather disparaged the interference of a body of examiners and commissioners. Well, what was the object of these examiners? They might be used for putting down such inventions as, according to the arbitrary mind of the examiners, were not worthy of a patent; but a careful examination of the draft Bill would show those interested that this point had been properly guarded against, that the examination of the application would act rather as a protection for the applicant than to his detriment. He knew from his own experience, and probably many would agree with him, that sometimes one lodged the provisional specification, and, notwithstanding all care on the part of the patent agent, some specification or some publication turned up to interfere with it. It was not the applicant's intention naturally to repeat an old thing, but his ignorance of what had been done before made him spend his time and money needlessly. He thought it a matter of great importance that intending patentees should have, for the fees paid, good and trustworthy information, such as the Patent Office alone could furnish, by means of official examiners. There were large funds in the Patent Office, which, instead of accumulating farther, should be utilised for the benefit of patentees. One of the most essential things was, that it should be clearly pointed out what had been patented and published, in order that the inventor might see whether he had made a mistake, or whether his application required to be modified, in order that he might have a good patent. At present it was simply to pay your money, and take your certificate. If you paid your fee you got your grant; and if the Patent Office had taken the same fee for precisely the same invention the day before, who cared. The second man lost his money, and the Treasury gained. He thought the most valuable part of the present scheme was, that the examination should not be carried on to the extent to which it used to be carried in Germany, and to which it was perhaps carried still in the United States, but that there should be such an examination as would aid the applicant to a true perception of his position. They had heard some very strong observations against the Bill on the part of the poor inventor, and he (Dr. Siemens) felt disposed to go some length with what Mr. Ley had said, only it would be impossible to carry a measure involving a very large reduction of fees. The fee to be paid by the inventor, in the first place, should certainly not be more than any careful working man could afford to pay, but after having obtained his grant, the question was, how were they to discern whether a patent was a workable patent, and whether the inventor did apply himself to the introduction of it or not? In France, and in some other countries, the law stepped in, and required the patentee to bring some proof after the lapse of one or two years that the invention had been practically introduced, but that provision was very objectionable. If you invented a mouse-trap, you could put it into use within a week; but if you invented a process, it would take some years before you could possibly expect any practical result. The previous speaker instanced the case of James Watt as one where an invention came perfect into the world; but he would ask him how it was that Watt spent seven years before he could obtain any practical results, and how it was that he first, in combination with Dr. Roebuck, came to the point that he would have had to abandon his patent if he had not been taken up by Boulton, who thus enabled him to give his invention the development which made it the foundation of a great advancement in civilisation. The invention by Watt of the separate condenser and air-pump was just one of those which required a great deal of knowledge and mechanical skill in order to develop its merit, and such must be the case in every instance where any important change was contemplated. Then he came to the last class of interested parties who, so far, had not been represented in the discussion—the user; and, although he himself belonged to the class of inventors, he thought the user had, after all, the first right to be considered. In connection with this point, he thought a little anecdote which he heard with regard to a Minister of State under Louis XV. was appropriate. Pensions had been granted to poets, and poetry was a very good thing, of course; then a poet came to him claiming a pension, but the Minister declined to grant it. Well, said the poet, "*Il faut que je vive.*" "*Je n'en vois pas la nécessité,*" replied the Minister, politely shrugging his shoulders. If the public could do without inventions, they surely would have the right to say, we will not have any inventors. If they could do without them, and could be happy without them, they had a perfect right to say, we will not have patents. But nearly all thinking men now were agreed that they could not get on without patented inventions. The cry of "No patents" had died away, because it was founded on error, and they had now to consider what was the best form of grant to give to an intending patentee, not for his own aggrandisement, but for the public advancement. If they kept the public interest involved in the question chiefly in view, they would be much more likely to arrive at a fair and reasonable conclusion than if they started with the idea of an indefeasible right in the inventor. After all, letters patent were not property, in the sense of real estate. Real property was absolute, and was not taken away after a term of fourteen or fifteen years. But no country had ever proposed, and no inventor had ever asserted, the right to a perpetual monopoly in his invention. The granting of a patent was a temporary endowment, in order that the patentee might have, first, time to develop the invention, which was the important thing as far as the public were concerned, and in doing so have the opportunity of earning a proper compensation for the expenditure of ingenuity, time, and money which he had made. The duration of a patent should be sufficient to enable a patentee to earn a fair remuneration ; and as regards this term, the Bill proposed to substitute seventeen for fourteen years, a period which he thought would generally satisfy the justice of the case ; but in case of exceptional circumstances, there would still be power to grant extension. Another important point in the Bill was that the government would be bound by patents. At present it was a crying evil that government departments stood above patents ; and speaking from his own experience, he could prove that, so far from this benefiting government departments, they were left to their own resources, and, instead of applying an invention properly, they were likely to apply it improperly, simply because they had not the guidance and advice of the patentee. There was no reason why a public department should not be liable to remunerate an inventor as much as any of her Majesty's subjects, provided the claims made upon them were not unreasonable ones. This latter consideration brought him to another provision of the Bill, which he would urge very much on the inventor class, viz., that for compulsory licenses. This had not an agreeable sound in the ears of many inventors, who maintained that their invention was their property, and they should have liberty to deal with it just as they thought proper. But he could not admit that doctrine of absolute property. A patent was a trust, the inventor was made the guardian of the invention in order that he might bring it into public use. If he should assume the position of the dog in the manger, the law ought to step in and say, "No, that is not the bargain ; it is for public use, and for the public benefit, that the grant has been made." There were many inventions which could be carried out quite well by the inventor himself, as, for instance, if it were a new machine for a special purpose, such as a meter, the patentee or his friends might erect works to supply the public, and there would be no necessity for compulsory licenses ; but if it were a process which applied to an important industry, such as the iron or steel industry, or to spinning, it would be a public injustice if the inventor were to say, "I will empower this one factory only, to carry out this invention, to the detriment of the whole country." It was only just that under such circumstances the law should step in and arbitrate between the parties concerned.

Such arbitration would, he believed, greatly benefit inventors as a class. Speaking for himself, he had often been a great deal pressed by intending licensees to grant them exclusive licenses, and if not for the whole country, at any rate for a county, but he had always set his face against it, because it would be sure to bring him to a place to which he had an insuperable objection, viz., the Law Courts. He had, therefore, always refused to grant exclusive licenses, and if there were such a clause as that under certain circumstances the inventor would be obliged to grant licenses, he would have a capital answer to give to the would-be monopolists. In conclusion, he would say that objections were naturally raised against the provisions of the Bill by the several interests he had alluded to; but in discussing this question, he would submit that it should be looked upon from the point of view of making the law acceptable all round, and for the greatest benefit to the public at large.

"ON THE CONSERVATION OF SOLAR ENERGY,"

By C. William Siemens, D.C.L., LL.D., F.R.S., Mem. Inst. C.E.*

The question of the maintenance of Solar Energy is one that has been looked upon with deep interest by astronomers and physicists from the time of La Place downward.

The amount of heat radiated from the sun has been approximately computed, by the aid of the pyrheliometer of Pouillet and by the actinometers of Herschel and others, at 18,000,000 of heat units from every square foot of his surface per hour, or, put popularly, as equal to the heat that would be produced by the perfect combustion every thirty-six hours of a mass of coal of specific gravity = 1·5 as great as that of our earth.

If the sun were surrounded by a solid sphere of a radius equal to the mean distance of the sun from the earth (95,000,000 of miles), the whole of this prodigious amount of heat would be intercepted;

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* Excerpt Proceedings of the Royal Society, Vol. XXXIII. 1882, pp. 389–398

but considering that the earth's apparent diameter as seen from the sun is only seventeen seconds, the earth can intercept only the 2,250-millionth part. Assuming that the other planetary bodies swell the intercepted heat by ten times this amount, there remains the important fact that $\frac{224999999}{225000000}$ of the solar energy is radiated into space, and apparently lost to the solar system, and only $\frac{1}{225000000}$ utilised.

Notwithstanding this enormous loss of heat, solar temperature has not diminished sensibly for centuries, if we neglect the periodic changes—apparently connected with the appearance of sun-spots—that have been observed by Lockyer and others; and the question forces itself upon us how this great loss can be sustained without producing an observable diminution of solar temperature even within a human lifetime.

Amongst the ingenious hypotheses intended to account for a continuance of solar heat is that of shrinkage, or gradual reduction of the sun's volume suggested by Helmholtz. It may, however, be urged against this theory that the heat so produced would be liberated throughout his mass, and would have to be brought to the surface by conduction, aided perhaps by convection; but we know of no material of sufficient conductivity to transmit anything approaching the amount of heat lost by radiation.

Chemical action between the constituent parts of the sun has also been suggested; but here again we are met by the difficulty that the products of such combination would ere this have accumulated on the surface, and would have formed a barrier against further action.

These difficulties led Sir William Thomson to the suggestion that the cause of maintenance of solar temperature might be found in the circumstance of meteorolites falling upon the sun, not from great distances in space, as had been suggested by Mayer and Waterston, but from narrow orbits which slowly contracted by resistance until at last the meteorolites became entangled in the sun's atmosphere and fell in; and he shows that each pound of matter so imparted would represent a large number of heat units without disturbing the planetary equilibrium. But in considering more fully the enormous amount of planetary matter that would be required for the maintenance of the solar temperature, Sir William Thomson soon abandoned this hypothesis for that of simple transfer of heat from the interior of a fluid sun to the surface by means of convection currents, which latter hypothesis appears at the present time to be also supported by Professor Stokes and other leading physicists.

But if either of these hypotheses could be proved, we should only have the satisfaction of knowing that the solar waste of energy by dissipation into space was not dependent entirely upon loss of his sensible heat, but that his existence as a luminary would be prolonged by calling into requisition a limited, though may be large, store of energy in the form of separated matter. The true solution of the problem will be furnished by a theory, according to which radiant energy which is now supposed to be dissipated into space and irrecoverably lost to our solar system, could be arrested, wholly or partly, and brought back in another form to the sun himself, there to continue the work of solar radiation.

Some years ago it occurred to me that such a solution of the solar problem might not lie beyond the bounds of possibility, and although I cannot claim intimate acquaintance with the intricacies of solar physics, I have watched its progress, and have engaged also in some physical experiments bearing upon the question, all of which have served to strengthen my confidence and ripened in me the determination to submit my views, not without some misgiving, to the touchstone of scientific criticism.

For the purposes of my theory, stellar space is supposed to be filled with highly rarefied gaseous matter, including probably hydrogen, oxygen, nitrogen, carbon, and their compounds, besides solid materials in the form of dust. This being the case, each planetary body would attract to itself an atmosphere depending for its density upon its relative attractive importance, and it would not seem unreasonable to suppose that the heavier and less diffusible gases would form the staple of these atmospheres ; that, in fact, they would consist mostly of nitrogen, oxygen, and carbonic anhydride, whilst hydrogen and its compounds would predominate in space.

But the planetary system, as a whole, would exercise an attractive influence upon the gaseous matter diffused through space, and would therefore be enveloped in an atmosphere, holding an intermediate position between the individual planetary atmospheres and the extremely rarefied atmosphere of the stellar space.

In support of this view it may be urged that in following out the molecular theory of gases as laid down by Clausius, Clerk Maxwell, and Thomson, it would be difficult to assign a limit to a gaseous atmosphere in space and, further, that some writers, among whom I will here mention only Grove, Humboldt, Zoellner, and Mattieu Williams, have boldly asserted the existence of a space filled with matter, and that Newton himself, as Dr. Sterry Hunt tells us in an interesting paper which has only just reached me, has expressed views in favour of such an assumption. Further than this, we have the facts that meteorolites whose flight through stellar, or at all events through interplanatary space, is suddenly arrested by being brought into collision with our earth, are known to contain as much as six times their own volume of gases taken at atmospheric pressure; and Dr. Flight has only very recently communicated to the Royal Society the analysis of the occluded gases of one of these meteorolites taken immediately after the descent to be as follows:—CO_2, 0·12; CO, 31·88; H, 45·79; CH_4, 4·55; N, 17·66.

It appears surprising that there was no aqueous vapour, considering there was much hydrogen and oxygen in combination with carbon, but perhaps the vapour escaped observation, or was expelled to a greater extent than the other gases by external heat when the meteorolite passed through our atmosphere. Opinions concur that the gases found occluded in meteorolites cannot be supposed to have entered into their composition during the very short period of traversing our atmosphere, but if any doubt should exist on this head, it ought to be set at rest by the fact that the gas principally occluded is hydrogen, which is not contained in our atmosphere in any appreciable quantity.

Further proof of the fact that stellar space is filled with gaseous matter is furnished by spectrum analysis, and it appears from recent investigation, by Dr. Huggins and others, that the nucleus of a comet contains very much the same gases found occluded in meteorolites, including "carbon, hydrogen, nitrogen, and probably oxygen," whilst, according to the views set forth by Dewar and Liveing, it also contains nitrogenous compounds such as cyanogen.

Adversely to the assumption that interplanetary space is filled with gases, it is urged that the presence of ordinary matter would cause sensible retardation of planetary motion, such as must have made itself felt before this ; but assuming that the matter filling space is an almost perfect fluid not limited by border surfaces, it can be shown on purely mechanical grounds, that the retardation by friction through such an attenuated medium would be very slight indeed, even at planetary velocities.

But it may be contended that, if the views here advocated regarding the distribution of gases were true, the sun should draw to himself the bulk of the least diffusible, and therefore the heaviest gases, such as carbonic anhydride, carbonic oxide, oxygen and nitrogen, whereas spectrum analysis has proved on the contrary a prevalence of hydrogen.

In explanation of this seeming anomaly, it can be shown in the first place, that the temperature of the sun is so high, that such compound gases as carbonic anhydride and carbonic oxide, could not exist within him ; it has been contended, indeed, by Mr. Lockyer, that none of the metalloïds have any existence at these temperatures, although as regards oxygen, Dr. Draper asserts its existence in the solar photosphere. There must be regions, however, outside that thermal limit, where their existence would not be jeopardised by heat, and here great accumulation of those comparatively heavy gases that constitute our atmosphere would probably take place, were it not for a certain counterbalancing action.

I here approach a point of principal importance in my argument, upon the proof of which my further conclusions must depend.

The sun completes one revolution on its axis in 25 days, and its diameter being taken at 882,000 miles, it follows that the tangential velocity amounts to 1·25 miles per second, or to 4·41 times the tangential velocity of our earth. This high rotative velocity of the sun must cause an equatorial rise of the solar atmosphere to which Mairan, in 1731, attributed the appearance of the zodiacal light. La Place rejected this explanation on the ground that the zodiacal light extended to a distance from the sun exceeding our own distance, whereas the equatorial rise of the solar atmosphere due to its rotation could not exceed $\frac{9}{20}$ths of the distance of Mercury. But it must be remembered that La Place based his calculation upon the hypothesis of an empty stellar space (filled only with an imaginary ether), and that the result of solar rotation would be widely different, if it was supposed to take place within a medium of unbounded extension. In this case pressures would be balanced all round, and the sun would act mechanically upon the floating matter surrounding it in the manner of a fan, drawing it towards itself upon the polar surfaces, and projecting it outward in a continuous disc-like stream.

By this fan action, hydrogen, hydrocarbons, and oxygen, are supposed to be drawn in enormous quantities toward the polar surfaces of the sun ; during their gradual approach, they will pass from their condition of extreme attenuation and extreme cold, to that of compression, accompanied with rise of temperature, until on approaching the photosphere, they burst into flame, giving rise to a great development of heat, and a temperature commensurate with their point of dissociation at the solar density. The result of their combustion will be aqueous vapour and carbonic anhydride or oxide, according to the sufficiency or the insufficiency of oxygen present to complete the combustion, and these products of combustion in yielding to the influence of centrifugal force will flow toward the solar equator, and be thence projected into space.

The next question for consideration is : What would become of these products of combustion when thus rendered back into space ? Apparently they would gradually change the condition of stellar material, rendering it more and more neutral, but I venture to suggest the possibility, nay, the probability, that solar radiation would, under these circumstances, step in to bring back the combined materials to a condition of separation by a process of dissociation carried into effect at the expense of that solar energy which is now supposed to be lost to our planetary system.

According to the law of dissociation as developed by Bunsen and Sainte-Claire Deville, the point of dissociation of different compounds depends upon the temperature on the one hand, and upon the pressure on the other. According to Sainte-Claire Deville, the dissociation tension of aqueous vapour of atmospheric pressure and at 2800° C. is 0·5, or only half of the vapour can exist as such, its remaining half being found as a mechanical mixture of hydrogen and oxygen, but with the pressure, the temperature of dissociation rises and falls as the temperature of saturated steam rises and falls with its pressure. It is therefore conceivable that the temperature of the solar photosphere may be raised by combustion to a temperature exceeding 2800° C., whereas dissociation may be effected in space at comparatively low temperatures.

These investigations had reference only to heats measured by means of pyrometers, but do not extend to the effects of radiant heat. Dr. Tyndall has shown by his exhaustive researches that vapour of water and other gaseous compounds intercept radiant heat in a most remarkable degree, and there is other evidence to show that radiant energy from a source of high intensity possesses a dissociating power far surpassing the measurable temperature to which the compound substance under its influence is raised. Thus carbonic anhydride and water are dissociated in the leaf cells of plants, under the influence of the direct solar ray at ordinary summer temperature, and experiments in which I have been engaged for nearly three years [*] go to prove that this dissociating action is obtained also under the radiant influence of the electric arc, although it is scarcely perceptible if the source of radiant energy is such as can be produced by the combustion of oil or gas.

The point of dissociation of aqueous vapour and carbonic anhydride admits, however, of being determined by direct experiment. It engaged my attention some years ago, but I have hesitated to publish the qualitative results I then obtained, in the hope of attaining to quantitative proofs.

These experiments consisted in the employment of glass tubes, furnished with platinum electrodes, and filled with aqueous vapour or with carbonic anhydride in the usual manner, the latter being furnished with caustic soda to regulate the vapour pressure by heating. Upon immersing one end of the tube charged with aqueous vapour in a refrigerating mixture of ice and chloride of calcium, its temperature at that end was reduced to —32° C., corresponding to a vapour pressure, according to Regnault, of $\frac{1}{1800}$th of an atmosphere. When so cooled no slow electric discharge took place on connecting the two electrodes with a small induction coil. I then exposed the end of the tube projecting out of the freezing mixture, backed by white paper, to solar radiation (on a

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[*] See Proceedings of the Royal Society, Vol. XXX., p. 208, and Paper read before Section A., British Association, and printed in full in the Report for 1881, Part I., p. 474, p. 252, *ante*.

clear summer's day) for several hours, when upon again connecting up to the inductorium, a discharge, apparently that of a hydrogen vacuum, was obtained. This experiment being repeated furnished unmistakable evidence, I thought, that aqueous vapour had been dissociated by exposure to solar radiation. The CO_2 tubes gave; however, less reliable results. Not satisfied with these qualitative results, I made arrangements to collect the permanant gases so produced by means of a Sprengel pump, but was prevented by lack of time from pursuing the inquiry, which I purpose, however, to resume shortly, being of opinion that, independently of my present speculation, the experiments may prove useful in extending our knowledge regarding the laws of dissociation.

It should here be observed that, according to Professor Stokes, the ultra-violet rays are in a large measure absorbed in passing through clear glass, and it follows from this discovery that only a small portion of the chemical rays found their way through the tubes to accomplish the work of dissociation. This circumstance, being adverse to the experiment, only serves to increase the value of the result observed.

Assuming, for my present purpose, that dissociation of aqueous vapour was really effected in the experiment just described, and assuming, further, that stellar space is filled with aqueous and other vapour of a density not exceeding the $\frac{1}{2000}$th part of our atmosphere, it seems reasonable to suppose that its dissociation would be effected by solar radiation, and that solar energy would thus be utilised. The presence of carbonic anhydride and carbonic oxide would only serve to facilitate the decomposition of the aqueous vapour by furnishing substances to combine with nascent oxygen and hydrogen. It is not necessary to suppose that all the energy radiated from the sun into space should be intercepted, inasmuch as even a partial return of heat in the manner described would serve to supplement solar radiation, the balance made up by absolute loss. To this loss of energy must be added that involved in keeping up the circulating movement of the gas, which, however, would probably not be relatively greater than that concerned in the tidal retardation of the earth's rotation. By means of the fan-like action resulting from the rotation of the sun, the vapours dissociated in space would be drawn towards the polar surfaces of the sun, be heated by increase in density, and would burst into flame at a point where both their density and temperature had reached the necessary elevation to induce combustion, each complete cycle taking, however, years to be accomplished. The resulting aqueous vapour, carbonic anhydride and carbonic oxide, would be drawn towards the equatorial regions, and be then again projected into space by centrifugal force.

Space would, according to these views, be filled with gaseous compounds in process of decomposition by solar radiant energy, and the existence of these gases would furnish an explanation of the solar absorption spectrum, in which the lines of some of the substances may be entirely neutralised and lost to observation. As regards the heavy metallic vapours revealed in the sun by the spectroscope, it is assumed that these form a lower and denser solar atmosphere, not participating in the fan-like action which is supposed to affect the light outer atmosphere only, in which hydrogen is the principal factor.

Such a dense metallic atmosphere could not participate in the fan action affecting the lighter photosphere, because this is only feasible on the supposition that the density of the in-flowing current is, at equal distances from the gravitating centre, equal or nearly equal to the outflowing current. It is true that the products of combustion of hydrogen and carbonic oxide are denser than their constituents, but this difference may be balanced by their superior temperature on leaving the sun, whereas the metallic vapours would be unbalanced, and would therefore obey the laws of gravitation, recalling them to the sun. On the surface of contact between the two solar atmospheres intermixture, induced by friction, must take place, however, giving rise perhaps to those vortices and explosive effects which are revealed to us by the telescope in the intermediate or stormy region of the sun, and which have been commented on by Sir John Herschel and other astronomers. Some of the denser vapours would probably get intermixed and carried away mechanically by the lighter gases, and give rise to that cosmic dust which is observed to fall upon our earth in not inappreciable quantities. Excessive intermixture would be prevented by the intermediary neutral atmosphere, the penumbra.

As the whole solar system moves through space at a pace estimated at 150,000,000 of miles annually (being about one-fourth of the velocity of the earth in its orbit), it appears possible that the condition of the gaseous fuel supplying the sun may vary according to the state of previous decomposition, in which other heavenly bodies may have taken part. May it not be owing to such differences in the quality of the fuel supplied that the observed variations of the solar heat may depend? and may it not be in consequence of such changes in the thermal condition of the photosphere that sun-spots are formed?

The views here advocated could not be thought acceptable unless they furnished at any rate a consistent explanation of the still somewhat mysterious phenomena of the zodiacal light and of comets. Regarding the former, we should be able to return to Mairan's views, the objection by La Place being met by a continuous outward flow from the solar equator. Luminosity would be attributable to particles of dust emitting light reflected from the sun, or by phosphorescence. But there is another cause for luminosity of these particles, which may deserve a passing consideration. Each particle would be electrified by gaseous friction in its acceleration, and its electric tension would be vastly increased in its forcible removal, in the same way as the fine dust of the desert has been observed by Werner Siemens to be in a state of high electrification on the apex of the Cheops Pyramid. Would not the zodiacal light also find explanation by slow electric discharge backward from the dust towards the sun? and would the same cause not account for a great difference of potential between the sun and earth, which latter may be supposed to be washed by the solar radial current? May not the presence of the current also furnish us with an explanation of the fact that hydrogen, while abounding apparently in space, is practically absent in our atmosphere, where aqueous vapour, which may be partly derived from the sun, takes its place? An action analogous to this, though on a much smaller scale, may be set up also by terrestrial rotation giving rise to an electrical discharge from the outgoing equatorial stream to the polar regions, where the atmosphere to be pierced by the return flood is of least resistance.

It is also important to show how the phenomena of comets could be harmonised with the views here advocated, and I venture to hope that these occasional visitors will serve to furnish us with positive evidence in my favour. Astronomical physicists tell us that the nucleus of a comet consists of an aggregation of stones similar to meteoric stones. Adopting this view, and assuming that the stones have absorbed in stellar space gases to the amount of six times their volume, taken at atmospheric pressure, what, it may be asked, will be the effect of such a mass of stone advancing towards the sun at a velocity reaching in perihelion the prodigious rate of 366 miles per second (as observed in the comet of 1845), being twenty-three times our orbital rate of motion. It appears evident that the entry of such a divided mass into a comparatively dense atmosphere must be accompanied by a rise of temperature by frictional resistance, aided by attractive condensation. At a certain point the increase of temperature must cause ignition, and the heat thus produced must drive out the occluded gases, which in an atmosphere 3000 times less dense than that of our earth would produce $6 \times 3000 = 18,000$ times the volume of the stones themselves. These gases would issue forth in all directions, but would remain unobserved except in that of motion, in which they would meet the interplanetary atmosphere with the compound velocity, and form a zone of intense combustion, such as Dr. Huggins has lately observed to surround the one side of the nucleus, evidently the side of forward motion. The nucleus would thus emit original light, whereas the tail may be supposed to consist of stellar dust rendered luminous by reflex action produced by the light of the sun and comet combined as fore-shadowed already by Tyndall, Tate, and others, starting each from different assumptions.

These are in brief the outlines of my reflections regarding this most fascinating question, which I venture to put before the Royal Society. Although I cannot pretend to an intimate acquaintance with the more intricate phenomena of solar physics, I have long had a conviction, derived principally from familiarity with some of the terrestrial effects of heat, that the prodigious and seemingly wanton dissipation of solar heat is unnecessary to satisfy accepted principles regarding the conservation of energy, but that it may be arrested and returned over and over again to the sun, in a manner somewhat analogous to the action of the heat recuperator in the regenerative gas furnace. The fundamental conditions are :—

1. That aqueous vapour and carbon compounds are present in stellar or interplanetary space.

2. That these gaseous compounds are capable of being dissociated by radiant solar energy while in a state of extreme attenuation.

3. That these dissociated vapours are capable of being compressed into the solar photosphere by a process of interchange with an equal amount of reassociated vapours, this interchange being effected by the centrifugal action of the sun itself.

If these conditions could be substantiated, we should gain the satisfaction that our solar system would no longer impress us with the idea of prodigious waste through dissipation of energy into space, but rather with that of well-ordered self-sustaining action, capable of perpetuating solar radiation to the remotest future.

"ON THE DEPENDENCE OF RADIATION ON TEMPERATURE,"

By Sir William Siemens,* F.R.S., D.C.L., LL.D.

Sir Isaac Newton held that the radiation of heat from a hot body increased in arithmetical ratio with the difference of temperature between it and the surrounding bodies. This law forms a rough approximation to the truth over a very limited range of temperature. MM. Dulong and Petit carried out an elaborate experimental research on the rate of cooling of hot bodies by radiation, extending to somewhat higher temperatures, and deduced from their observations the empirical formula—Rate of cooling $= m(1{\cdot}0077)^{t}(1{\cdot}0077^{T-t}—1)$.

Here T is the temperature of the hot body in degrees Centigrade, t the temperature of the surrounding matter, and m is a constant depending on the nature of the radiating body. This formula agrees very fairly with experimental results for ordinary temperatures, but, like Newton's law, it has been shown that it cannot be applied for a wider range.

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* Excerpt Proceedings of the Royal Society, Vol. XXXV. 1883, pp. 166–177.

The anomalous results which Newton's law and the formula of MM. Dulong and Petit lead to, when applied to the cooling of bodies at a very high temperature, are well illustrated by the attempts at deducing therefrom the temperature of the solar photosphere. Waterston, and Père Secchi (in his work entitled "Le Soleil"), following Newton's hypothesis, obtained 10,000,000° C. as the probable solar temperature, and Captain J. Ericsson, on the same hypothesis but assuming other constants, arrived at a temperature between 2,000,000° and 4,000,000° C. Strangely contrasting with these determinations are those of Pouillet in 1836, and Vicaire in 1872, who, employing Dulong and Petit's empirical formula, deduce the values 1461° and 1398° C. for the solar temperature. Between these extreme estimates we have those of Dr. Spoerer, 27,000° C., of Zoellner, 27,700°, Professor James Dewar (1872), 16,000°, Rosetti (1878), 9000°, and Hirn (1882), 20,000°.

In my own investigations on this subject, by comparing the spectrum of the sun as regards the proportion of luminous rays with those of the electric arc and gas flames, I have arrived at the conclusion that the temperature of the photosphere does not exceed 2800° C., which is in close agreement with the limit assigned by M. Sainte-Claire Deville, deduced from the observations of Frankland and Lockyer on the hydrogen lines in the solar spectrum. Sir William Thomson, in a paper communicated to the Philosophical Society of Glasgow (1882), has compared the power of the sun's radiation per unit of surface with that of a Swan incandescent carbon filament, and has shown that it is about sixty-seven times greater; he concludes from these data that the estimate I had formed of the solar temperature, *i.e.*, nearly 3000° C., cannot be very far from the true value.

These diverse and indirect results have long impressed me with the need of further experimental investigation of the dependence of radiation on temperature; and it has occurred to me lately, that the difficulties with which Dulong and Petit had to contend in making their measurements by means of a mercurial thermometer, where the losses due to conduction and convection are very great, and exceedingly difficult to determine, might be avoided in adopting a method of conducting the experiment which forms the principal subject of my present communication.

It is well known that the measurement of electrical currents and resistance is susceptible of very great accuracy compared with all thermal measurements : hence my endeavour has been to estimate thermal effects entirely by electrical methods. In the Bakerian Lecture for 1871, which I had the honour of delivering before the Royal Society ("Proc. Roy. Soc.," vol. 19, p. 443), I showed that the resistance of a platinum wire can be expressed as a linear function of its temperature by an empirical formula, the constants of which must be determined for each individual wire ; hence conversely, if resistance of a wire previously calibrated is measured, its temperature can be deduced. From theoretical considerations I showed that $\frac{r}{r_0} = \alpha T^{\frac{1}{2}} + \beta T + \gamma$ might be expected to represent the relation between the resistance and absolute temperature. This formula agreed closely with my own experimental results for platinum, copper, silver, iron, and aluminium wires ("Journal of the Society of Telegraph Engineers and Electricians," vol. i. p. 123, and vol. iii. p. 297),* and has since been verified by Professor A. Weinhold in the case of platinum from 100° to 1000° C. ("Annalen der Physik und Chemie," 1873, p. 225).

The apparatus (Fig. 3, Plate 23) which I propose for determining the dependence of radiation on temperature consists of a platinum or other wire, 0·76 millim. in diameter, suspended between two binding screws, marked (A) and (B) on the diagram, carried on two suitable wooden stands. The binding screws are connected through an electro-dynamometer (D), for the purpose of measuring the current, to a secondary battery, the number of cells in which can be varied. A high resistance galvanometer (G) is also inserted between the binding screws as a shunt to the platinum wire.

The electro-dynamometer is of the ordinary form, in which the current passes through a fixed coil, and a movable coil consisting of a single twist, hung by a torsion spring in a vertical plane at right angles to the plane of the fixed coil. The couple due to the current is balanced by the torsion of the spring, hence the angle of torsion is proportional to the square of the current. The current through the high resistance galvanometer being a measure of the difference of potential between the extremities of the plati-

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* See *ante*, p. 148.

num wire, the reading of the galvanometer, divided by the main current as determined by the electro-dynamometer, is proportional to the resistance of the wire. Hence the constant of the instrument and the resistance of the galvanometer being known, the resistance of the platinum wire could be calculated, as the current was varied by altering the number of cells composing the battery.

The measurements were made in all cases when equilibrium had been established between the radiation and the energy of the current, as evinced by the constancy of the readings of the electro-dynamometer and galvanometer.

Having made a rough preliminary series of experiments to test the suitability of the method and apparatus, with satisfactory results, on April 17th I made a second series, the results of which are recorded in Table I. Column I gives the current in ampères passing through the wire; column II the difference of potential in volts between the terminals as deduced from the readings of the galvanometer; column III the rate at which the energy of the current was converted into radiant energy, represented by the product of the electromotive force and current, and therefore measured in volt-ampères or watts; column IV the resistance of the wire, being the ratio of the electromotive force to the current; column V the corresponding temperature of the wire in degrees Centigrade. Finally, column VI describes the condition of the wire as apparent to the eye.

TABLE I.

LENGTH OF WIRE 102 CENTIMS. DIAMETER 0·76 MILLIM.

TEMPERATURE OF ROOM 65° FAHR.

I.	II.	III.	IV.	V.	VI.
Ampères.	Volts.	Watts.	Ohms.	° C.	
2·91	1·192	3·468	·4096	...	Just warm to touch.
3·999	1·639	6·555	·4099	...	
5·738	2·831	16·24	·4933	100	
8·943	5·662	50·64	·6331	282	
12·27	9·536	117·00	·7772	570	Chars wood.
16·66	16·39	273·0	·9838	881	Very dark red.
13·19	11·175	147·4	·8472	653	Red heat.
20·90	22·052	460·9	1·055	1075	Bright red.
23·73	26·82	636·4	1·130	1194	Very bright.

On April 18th, three further series of experiments were made, the results of which are set forth in a similar manner in Tables II, III, and IV.

TABLE II.

LENGTH OF WIRE 102 CENTIMS. DIAMETER 0·76 MILLIM.

CURRENT INCREASING.

Temperature of the Room.	Ampères.	Volts.	Watts.	Ohms.	Corresponding Temperature of Wire.	
° Fahr.					°	
63·5	2·565	·895	2·295	·3489	...	Just warm.
,,	3·217	1·340	4·310	·4165	...	
,,	6·36	3·204	20·377	·5037	120	Hot.
,,	8·511	5·146	43·798	·6046	250	
,,	10·714	7·599	81·416	·7029	420	Chars cotton.
66·0	13·192	11·026	145·45	·8358	645	Discolouring.
,,	13·698	11·927	163·38	·8707	690	Dark red.
,,	15·595	14·602	227·72	·9363	816	Light red.
67·0	16·222	15·510	251·60	·9561	852	Bright red.
,,	17·869	19·072	340·02	1·0698	960	Yellow.
,,	25·094	29·80	747·86	1·1875	1260	White.

NOTE.—The temperatures corresponding to the very small currents are not given, as for very small deflections the electro-dynamometer readings could not be regarded as perfectly trustworthy.

TABLE III.

LENGTH OF WIRE 102 CENTIMS. DIAMETER 0·76 MILLIM.

CURRENT INCREASING.

Temperature of the Room.	Ampères.	Volts.	Watts.	Ohms.	Corresponding Temperature of Wire.	
° Fahr.					°	
60	2·744	·908	2·491	·3309	...	Just warm.
,,	3·629	1·483	5·382	·4086	...	
,,	6·79	3·278	22·258	·4827	125	Hot.
,,	8·995	5·364	48·251	·5963	270	Nearly chars cotton.
,,	11·072	7·465	82·653	·6742	430	Chars cotton.
,,	14·048	11·925	167·52	·8489	700	Dark red.
70	16·247	15·496	251·76	·9538	855	Light red.
,,	19·299	19·97	385·40	1·0348	1005	Bright red.
,,	20·073	20·577	413·04	1·0251	1037	Very bright red.
,,	22·948	25·643	588·45	1·1175	1164	Yellow.
,,	23·634	26·25	620·40	1·1107	1185	Bright yellow.
,,	25·171	28·31	712·59	1·1247	1240	White.
,,	26·190	29·80	780·46	1·1379	1272	,,

TABLE IV.

WIRE THE SAME AS IN III.

CURRENT DECREASING.

Temperature of the Room.	Ampères.	Volts.	Watts.	Ohms.	Corresponding Temperature of Wire.
° Fahr.					°
63	25·101	28·31	710·61	1·1278	1240
,,	23·016	25·33	582·99	1·1005	1160
,,	18·578	18·327	340·48	·9864	960
,,	16·997	15·794	268·45	·9292	875
,,	15·098	13·410	202·47	·8882	775
,,	12·796	10·132	129·65	·7918	605
,,	11·06	7·599	84·044	·6870	440
,,	9·454	5·662	53·530	·5988	295
,,	7·513	4·097	30·780	·5452	180
,,	6·507	3·278	21·330	·5037	130
,,	5·04	2·384	12·016	·4730	...
,,	3·217	1·371	4·407	·4258	...
65	26·856	31·29	840·33	1·1651	1290

The results given in the four tables are plotted out on the curve marked (A) (Plate 36). The abscissæ give the rate at which the energy of the current is converted into heat, and the ordinates the corresponding resistance of the wire.

To determine the temperature of the wire corresponding to each resistance, another series of experiments was made, which are described hereafter. The values of α, β, and γ obtained were—

$$\left.\begin{array}{l}\alpha = 0\cdot0119\\ \beta = 0\cdot00112\\ \gamma = 0\cdot512\end{array}\right\}$$

hence $\frac{r_t}{r_0} = \cdot0119T^{\frac{1}{2}} + \cdot00112T + \cdot512$; where r_0 is the resistance of the wire at the freezing point. By giving to T various values in this formula, a curve can be constructed showing the relation between the resistance and absolute temperature. Such a curve was drawn, and approximated for high temperatures to a straight line, as evidently must be the case from the form of the equation. By solving the equation for the maximum value of $\frac{r_t}{r_0}$ observed, it was found that the temperature of the wire when bright red hot was about 1100° C. It is known that platinum wire melts at approximately 1800° C.

The curve of relation between the temperature of the wire and the electrical energy absorbed can now be constructed. Taking the abscissæ of the curve proportional to the watts absorbed, and the ordinates proportional to the temperatures in degrees Centigrade, the curve marked B represents the relation between the power and the temperature for the results given in the tables.

I have sought to express this relation by an empirical formula in order to carry the curve to still higher temperatures. The equation Temperature $= A (\log x)^2 + B (\log x) + C$, where x represents watts, agrees with the experimental results. The constants A, B, C have the values, $A = -63$; $B = 1177$; $C = -1603$.

Mr. McFarlane, in a paper communicated to the Royal Society on January 11th, 1872, has arrived at the equation—Rate of energy $= a + bt + ct^2$, where a, b, c are empirical constants and t is the difference of temperature, viz., about 60° C. ("Proc. Roy. Soc.," vol. 20, p. 90, 1872). Professor James Dewar, from experiments extending from a temperature of 80° to the boiling points of sulphur and mercury, also deduces a parabolic formula. ("Proceedings of the Royal Institution," vol. 9, p. 266.)

Making use of the equation I have given, the rate of energy absorbed for a temperature of 2780° C., is 155,000 watts, or sixty-seven times the rate of absorption at a temperature of 1670° C. Since 1670° C. is not much below the temperature of an incandescent filament (reverting to Sir William Thomson's calculation for the ratio of the radiant power per unit of surface of the sun to that of the incandescent filament), the temperature of the sun comes out to be about 2780°; which is in very close agreement with my former estimate based on other grounds. The effect of absorption between the sun and the earth would bring the two estimates into still closer agreement.

If we attempt to form a natural equation to the curve, it is apparent that it will consist of two terms—

(i.) The term due to radiation.

(ii.) The term depending on the convection and conduction of the air. The conduction of heat by the wire into the terminals may be neglected, as by taking a considerable length it becomes a small quantity of the second order. The first term I take to be proportional to some power of the absolute temperature, the second may for the present be represented by $mF(t)$. Hence we have—Rate of conversion of energy $= AT^n + mF(t)$.

According to Prevost's theory of exchanges, the hot body is itself receiving radiant energy from the surrounding bodies; hence the radiant energy is more appropriately represented by $A(T^n - t^n)$, where t is the temperature of the surrounding bodies. Similarly it would appear probable that the conduction and convection will depend on the difference of temperature. Hence—Rate of energy $= A(T^n - t^n) + mF(T - t)$.

The constants A and m will depend on the nature of the radiating body and on the surrounding medium.

Although for theoretical purposes it is important to eliminate the conduction and convection, yet in most cases a medium is present, and it has been shown by Mr. Crookes that, within limits, variations in pressure have only a very small effect on the amount of heat lost by conduction and convection.

I have not as yet been able to make any experiments on the determination of the term $mF(T - t)$, but it is my intention to make further investigations on this point. I am indebted to Professor Stokes for suggesting a method which appears to me likely to yield useful results. He proposes to construct a chimney of white paper, and to fix it over the wire through which the current is passing. The chimney will collect all the heated air ascending by convection, and by suitable means its temperature and the rate of flow can be measured, and hence the rate of loss of heat by convection estimated.

It might be supposed that conducting the experiment *in vacuo* would diminish the convection. According to the original researches of Dulong and Petit, the rate of cooling diminished in a geometrical progression, whose ratio was $\frac{1}{1 \cdot 366}$, as the pressure diminished in a second geometrical progression, of which the ratio was $\frac{1}{2}$. Mr. Crookes, in a paper communicated to the Royal Society ("Proc. Roy. Soc.," 1880, vol. 31, p. 239) described some experiments on this point, and showed that a diminution of pressure from 760 millims. to 120 millims. had a very slight effect on the convection. From 120 to 5 millims. the effect was somewhat more marked. A reduction of pressure from 5 millims. to 2 millims., however, produced twice as much fall in the rate of cooling as the whole exhaustion from 760 millims. to 1 millim. Hence to eliminate the effect of convection a very high exhaustion must be obtained.

It still remains to describe the experiments by which the constants a, β, γ of the empirical formula connecting the resistance of the wire with its absolute temperature were determined. The wire was enclosed in a glass tube, stopped at either end with a plug, through which the wire passed centrally. The tube was fixed in a metallic trough, with an aperture in its cover sufficiently large to admit a mercurial thermometer placed in contact with the tube. In the first instance, the trough was filled with melting ice, and the resistance of the wire measured by a Wheatstone bridge. The ice was then removed, and two Bunsen burners were placed below the trough, and the temperature gradually raised by increasing the pressure of the gas in the burners.

In this way a series of simultaneous observations were made of the temperature of the wire and its corresponding resistance up to 100° C. The results are given in the subjoined table. Care was taken at each reading that the thermometer had become stationary, and really represented the temperature of the wire. A second series of observations were taken as the wire cooled from 100° to zero; and the results are likewise given in the table.

TEMPERATURE RISING.			TEMPERATURE FALLING.		
Temperature.	Resistance Ohms.	$\frac{r_t}{r_0}$	Temperature.	Resistance Ohms.	$\frac{r_t}{r_0}$
° C.			° C.		
0	·5847	1·0000	100	·6827	1·1680
0	·5837	...	97·7	·6815	1·1660
0	·5827	...	95·5	·6798	1·1631
0	·5827	...	90·0	·6741	1·1533
66·3	·6467	1·1064	78·5	·6619	1·1324
66·6	·6469	1·1068	76·6	·6601	1·1294
67·2	·6477	1·1081	62·5	·6463	1·1057
68·5	·6547	1·1201	48·3	·6308	1·0792
70·2	·6557	1·1218	46·6	·6299	1·0777
72·2	·6567	1·1235	32·2	·6147	1·0517
81·6	·6597	1·1286	31·6	·6140	1·0505
85·0	·6657	1·1389	21·6	·6052	1·0354
86·1	·6697	1·1458	0	·5857	1·0000
93·2	·6727	1·1509	0	·5857	...
95·0	·6747	1·1543			
98·8	·6777	1·1594			
99·5	·6817	1·1663			

For the reduction of the 26 equations obtained from these observations, the method of least squares was employed, giving $\alpha = 0 \cdot 0119$; $\beta = 0 \cdot 00112$; $\gamma = 0 \cdot 512$.

The following are the results in substituting for the platinum a wire of platinum with 20 per cent. of iridium.

DIAMETER OF WIRE ·73 TO ·75 MILLIM.

TEMPERATURE OF ROOM 59° FAHR. LENGTH OF WIRE 100 CENTIMS.

CURRENT INCREASING.

Ampères.	Volts.	Watts.	Ohms.	Corresponding Temperature of Wire. °	Condition.
2·169	1·638	3·553	·7552	...	Just warm.
4·652	3·045	14·165	·6546	...	Warm.
6·858	6·815	46·742	·9936	442	Hot.
10·17	11·745	119·48	1·1545	725	Chars cotton.
11·477	14·21	163·09	1·2381	873	Dark red.
12·932	16·67	215·58	1·2891	965	Red.
15·198	22·04	334·97	1·4502	1252	Light red.
17·807	29·00	516·40	1·6286	1587	Yellow.
20·791	36·25	753·67	1·7436	1787	White.

DIAMETER OF WIRE ·73 TO ·75 MILLIM.

TEMPERATURE OF ROOM 59° FAHR. LENGTH OF WIRE 100 CENTIMS.

CURRENT DECREASING.

Amperes.	Volts.	Watts.	Ohms.	Corresponding Temperature of Wire. °	Condition.
16·762	24·65	413·19	1·4706	1289	
14·210	19·865	282·28	1·3980	1160	
11·828	14·935	176·65	1·2627	918	
10·62	12·76	135·51	1·2015	806	
8·40	8·845	74·299	1·0530	545	
5·487	4·93	27·051	·8985	279	
4·338	3·625	15·725	·8364	...	

A second series were taken with the same piece of wire, and the current increased until the wire broke.

Ampères.	Volts.	Watts.	Ohms.	Corresponding Temperature of Wire.	Condition.
2·743	1·907	5·23	·6952	° ...	Just warm.
7·062	7·005	49·47	·9919	439	Hot.
10·492	12·66	132·86	1·2066	816	Chars cotton.
15·634	23·69	370·38	1·5153	1372	Light red.
19·324	33·53	647·93	1·7351	1771	White.
21·044	37·99	799·47	1·8053	1899	
22·414	41·72	935·10	1·8613	2001	
23·913	45·19	1080·60	1·8898	2053	Incandescent.
25·475	49·91	1271·50	1·9592	2185	
26·33	53·70	1413·90	2·0395	2325	Broke into several pieces immediately after reading.

The relation between the resistance and temperature is given in the following table :—

TEMPERATURE RISING.			TEMPERATURE FALLING.		
Temperature.	Resistance Ohms.	$\frac{r_t}{r_0}$	Temperature.	Resistance Ohms.	$\frac{r_t}{r_0}$
	1·0072		Boiling water .	1·0924	1·0852
Melting ice, 0° C.	1·0061	1·0000			
	1·0061		13·8° C. . .	1·0198	1·0131
12·1° C. . .	1·0184	1·0117			
Boiling water .	1·0924	1·0852	Melting ice . .	1·0072	1·0000

The values for α, β, γ deduced by the method of least squares are—$\alpha = {\cdot}005$; $\beta = {\cdot}000694$; $\gamma = -{\cdot}7285$.

In conclusion I have pleasure in acknowledging the assistance I have received in conducting the experiments, and in the preparation of this Paper, from Messrs. E. Lauckert and Edward Hopkinson, D.Sc.

SOME OF THE QUESTIONS INVOLVED IN SOLAR PHYSICS.

By Sir William Siemens,* D.C.L., LL.D., F.R.S., M.R.I.

The lecturer introduced his subject by drawing attention to the circumstance that the idea of the sun being an exceedingly hot body was of very modern date; that both ancient and modern writers up to the early portion of the present century attributed to him a glorious and supernatural faculty of endowing us with light and heat of the degree necessary for our well-being; whilst even Sir William Herschel had attempted to find an explanation in justification of the time-honoured conception that the body of the sun might be at a low temperature and inhabitable by beings similar to ourselves, which he did in surrounding the inhabitable surface by a non-conducting atmosphere—the penumbra—to separate it from the scorching influence of the exterior photosphere.

It was not till the views of Kant, the philosopher, had been developed by La Place, the astronomer, in his famous "Mécanique Céleste," that the opinion gained ground that our central orb was a mass of matter in a state of incandescence, representing such an enormous aggregate as to enable it to continue radiation into space for an almost indefinite period of time.

The lecturer illustrated by means of a diagram the fact that of all the heat radiated away from the sun, only $\frac{1}{2250000000}$ part could fall upon the surface of our earth, vegetation and force of every kind being attributable to this radiation; whilst all but this fractional proportion apparently went to waste.

Recent developments of scientific research had enabled us to know much more of the constitution of the sun and other heavenly bodies than had formerly been possible. Comte says in his "Positive Philosophy" (Martineau's translation of 1853) that "amongst the things impossible for us ever to know was that of telling what were the materials of which the sun was composed;" but within only seven years of that time Messrs. Bunsen and

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* Excerpt Proceedings of the Royal Institution of Great Britain, 1883, pp. 315–321.

Kirchhoff published their famous research, showing that by connecting the dark Fraunhofer lines of the solar spectrum with the bright lines observed in the spectra of various metals, it was possible to prove the existence of those substances in the solar photosphere, thus laying the foundation of spectrum analysis, the greatest achievement of modern science. Dr. Huggins and others, applying this mode of research to other heavenly bodies, including the distant nebulæ, had extended our chemical knowledge of them in a measure truly marvellous.

Solar observation had thus led to an analytical method by which chemistry had been revolutionised; and it would be, in the lecturer's opinion, through solar observation that we should attain to a much more perfect conception of the nature and effect of radiant energy in its three forms of heat, light, and actinism, than we could as yet boast of. The imperfection of our knowledge in this respect was proved by the circumstance that whereas some astronomers and physicists, including Waterston, Secchi, and Ericsson, had, in following Sir Isaac Newton's hypothesis, attributed to the sun a temperature of several millions of degrees Centigrade, others, including Pouillet and Vicaire, in following Dulong and Petit, had fixed it below 1500° C. Between these two extremes, other determinations, based upon different assumptions, had fixed the solar temperature at between 60,000° and 9000°.

The lecturer having conceived a process by which solar energy may be thought to a certain extent self-sustaining, had felt much interested for some years in the question of solar temperature. If the temperature of the solar photosphere should exceed 3000° C., combustion of hydrogen would be prevented by the law of dissociation, as enunciated by Bunsen and Sainte-Claire Deville; and his speculative views regarding thermal maintenance must fall to the ground. To test the question, he in the first place mounted a parabolic reflector on a heliostat with a view of concentrating solar rays within its focus, which, barring comparatively small losses by absorption in the atmosphere and in the metallic substance of the reflector, should reproduce approximately the solar temperature. By introducing a rod of carbon through a hole at the apex of the reflector until it reached the focus, its tip became vividly luminous, producing a light comparable to electric light. When a gas-burner was arranged in such a way that the gas flame played across the focal area, combustion appeared to be retarded, but was not arrested, showing that the utmost temperature attained in the focus did not exceed materially that producible in a Deville oxy-hydrogen furnace, or in the lecturer's regenerative gas furnace, in which the limit of dissociation is also reached.

Having thus far satisfied himself, his next step was to ascertain whether terrestrial sources of radiant energy were capable of imitating solar action in effecting the decomposition of carbonic acid and aqueous vapour in the leaf-cells of plants, which led him to undertake a series of researches on electro-horticulture, extending over three years, a subject he had brought before the Royal Society and the Royal Institution two years ago. By these researches he had proved that the electric arc possessed not only all the rays necessary to plant-life, but that a portion of its rays (the ultra-violet) exceeded in intensity the effective limit, and had to be absorbed by filtration through clear glass, which, as Professor Stokes had shown, produced this effect without interference with the yellow and other luminous and intense heat rays. He next endeavoured to estimate the solar temperature by instituting a comparison between the spectra due to different known luminous intensities. Starting with the researches of Professor Tyndall on radiant energy, supplementing them by experiments of his own on electric arcs of great power, and calling to his aid Professor Langley, of the Alleghany Observatory, to produce for him a complete spectrum of an Argand burner, he concluded that with the temperature of a radiant source, the proportion of luminous rays increased in a certain ratio; whereas in an Argand gas-burner only 2½ per cent. of the rays emitted were luminous and mostly red and yellow, the most brilliant portion of a gas flame emitted 4 per cent., as shown by Tyndall, the carbon thread of an incandescent electric light between 5 and 6 per cent., a small electric arc 10 per cent., and in a powerful 5000-candle electric arc as much as 25 per cent. of the total radiation was of the luminous kind. Professor Langley, in taking his photometer and bolometer up the Whitley mountains, 18,000 feet high, had proved that of the solar energy not more than 25 per cent. was luminous, and that the loss of solar energy sustained between our atmosphere and the sun was chiefly of the ultra-violet kind. These rays, if they penetrated our atmosphere, would render vegetation impossible, as proved by the lecturer's own experiments above referred to. It was thus shown that the temperature of the solar photosphere could not materially exceed that of a powerful electric arc, or, indeed, of the furnaces previously alluded to, leading him to the conclusion already foreshadowed by Sainte-Claire Deville, and accepted by Sir William Thomson, that the solar temperature could not exceed 3000° C. The energy emitted from a source much exceeding this limit would no longer be luminous, but consist mainly of ultra-violet rays, rendering the sun invisible, but scorching and destructive of all life. The diagram (Plate 37) of the spectra alluded to shows clearly the gradual advance of the luminous band, as marked by the letters A. to H.

Not satisfied with these inferential proofs, the lecturer had endeavoured to establish a definite ratio between temperature and radiation, which formed the subject of a very recent communication to the Royal Society.* The experiment consisted in heating, by means of an electric current, a platinum or iridio-platinum wire, a metre long, and suspended between binding screws, as shown in the sketch (Fig. 3, Plate 23) ; the energy of the current was measured by two instruments—an electro-dynamometer, giving it in ampères, and a galvanometer of high resistance giving the electro-motive force between the same points in volts. The product of the two readings gave the volt-ampères, or watts of energy communicated to the wire, and dispersed from it by radiation and convection. A reference to the lecturer's paper on the Electrical Resistance Thermometer, which formed the Bakerian Lecture of the Royal Society in 1871, would show that the varying electro-motive force in volts observed on the galvanometer was a true index of the temperature of the wire while being heated by the passage of the current. By combining his former experiments on the dependence of resistance upon temperature, with his recent one, a law of increase of radiation with temperature was established experimentally up to the melting-point of platinum ; this, when laid down in the form of a diagram, gave very consistent results expressible by the simple formula $\mathrm{Rad}^{\mathrm{tn}} = \mathrm{M}\ t^2 + \phi\ t$, M being a coefficient due to substance radiating ; an expression represented in the diagram (Plate 36), in which the

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* Proceedings of the Royal Society, Vol. XXXV. p. 166, p. 434, *ante*.

in which the abscissæ represent energy dispersed and the ordinates the corresponding temperatures.

Sir William Thomson had lately shown that the total radiating energy from a unit of surface of the carbon of the incandescent lamp amounted to $\frac{1}{67}$th part of the energy emitted from the same area of the solar photosphere, and taking the temperature of the incandescent carbon at 1800° C. (the melting-point of platinum, which can just be heated to the same point), it follows in applying Sir William Thomson's deductions to the lecturer's formula that the solar photosphere does not exceed 2700° C., or, adding for absorption of energy between us and the sun about 2800° C., a temperature already arrived at by the lecturer by a different method. The character of the curve was that of a parabola slightly tipped forward, and if the ratio given by that curve held good absolutely beyond the melting-point of platinum, it would lead to the conclusion that at a point exceeding 3000° C. radiation would become, as it were, explosive in its character, rendering a surface temperature beyond that limit physically difficult to conceive.

Clausius had proved that the temperature obtainable in a focus could never exceed that of the radiating surface, and Sainte-Claire Deville that the point of dissociation of compound vapours rises with the density of the vapour atmosphere. Supposing interstellar space to be filled with a highly attenuated compound vapour, it would clearly be possible to effect its dissociation at any point where, by the concentration of solar rays, a sufficient focal temperature could be established; but it was argued that the higher temperature observable in a focal sphere was the result only of a greater abundance of those solar vibrations called rays, within a limited area, the intensity of each vibration being the outcome of the source whence it emanated: thus, in the focal field of a large reflector the end of a poker could be heated to the welding-point, whereas in that of a small reflector the end of a very thin piece of wire only could be raised to the same temperature. If, however, a single molecule of vapour not associated or pressed upon by other molecules could be sent through the one focus or the other, dissociation in obedience to Deville's law must take place irrespective of the focal area; but, inasmuch as the single solar ray represented the same potential of energy or period of vibration as numerous rays associated in a focus, it seemed reasonable that it should be as capable of dealing with the isolated molecule as a mere accumulation of the same within a limited space, and must therefore possess the same dissociating influence. Proceeding on these premisses, the lecturer had procured tubes filled with highly attenuated vapours, and had observed that an exposure of the tubes to the direct solar rays or to the arc of a powerful electric light effected its partial or entire dissociation; the quantity of matter contained within such a tube was too slight to be amenable to direct chemical test, but the change operated by the light could be clearly demonstrated by passing an electric discharge through two similar tubes, one of which had, and the other had not, been exposed to the radiant energy from a source of high potential. If space could be thought filled with such vapour, of which there was much evidence in proof, solar rotation would necessarily have the effect of emitting such vapour equatorially by an action of circulation which might be likened to that of a blowing fan. When reaching the solar photosphere, by virtue of solar gravitation this dissociated vapour would, owing to its increased density, flash into flame, and could thus be made to account in great measure for the maintenance of solar radiation, whilst its continual dissociation in space would account for the continuance of solar radiation into space without producing any measurable calorific effect.

Time did not permit him to enter more fully on these subjects, which formed part of his solar hypothesis, his main object on this occasion having been to elucidate the point of cardinal importance to that hypothesis, that of the solar temperature.

INDEX TO VOLUME II.

ELECTRICITY.

MISCELLANEOUS.

END OF VOLUME II.

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SUBMARINE ELECTRIC TELEGRAPHS.

Fig. 1.

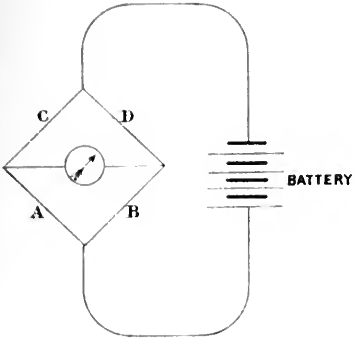


Fig. 2.

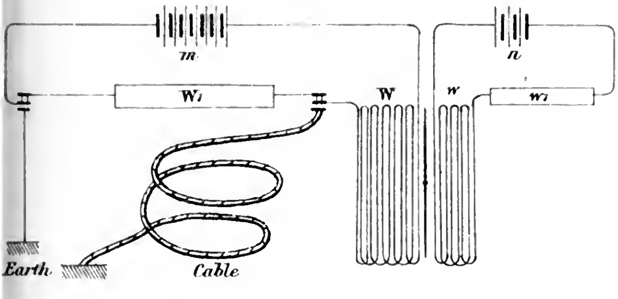


Fig. 3.

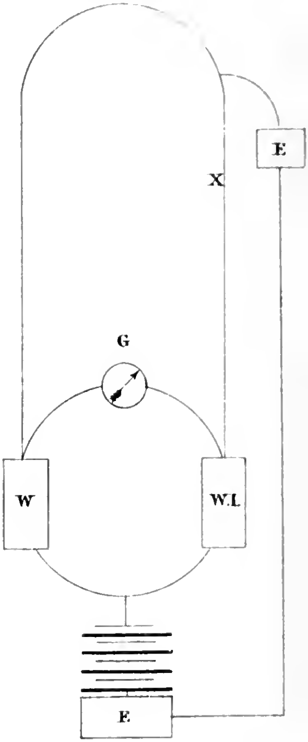
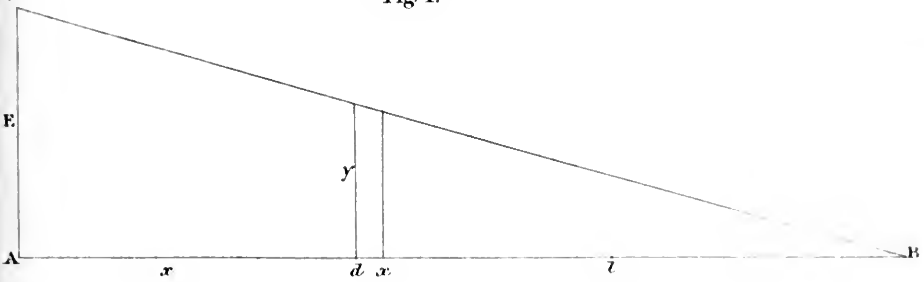


Fig. 4.



INDIA RUBBER COVERING MACHINE.

Fig 1.
Side Elevation.

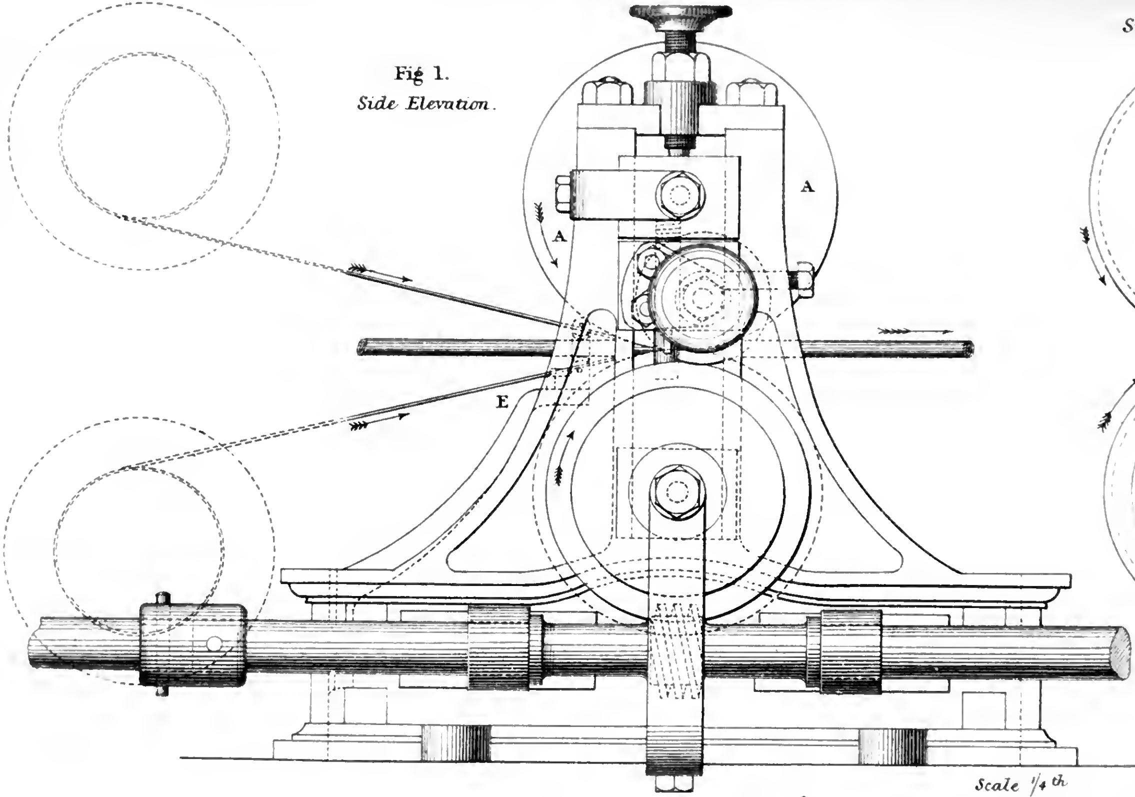
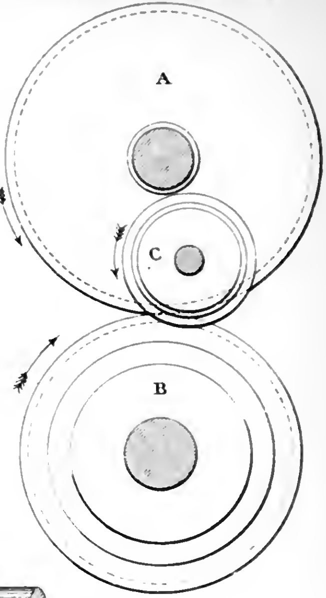


Fig 2.
Side Elevation of Rollers.



Scale ¼th
0 5 10 Ins.

(Excerpt Proc. Inst. M.E. 1860. Page 137.)

Robt. J. Cook & Hammond, Lith. Broadway, Westminster, S.W.

INDIA-RUBBER COVERING MACHINE

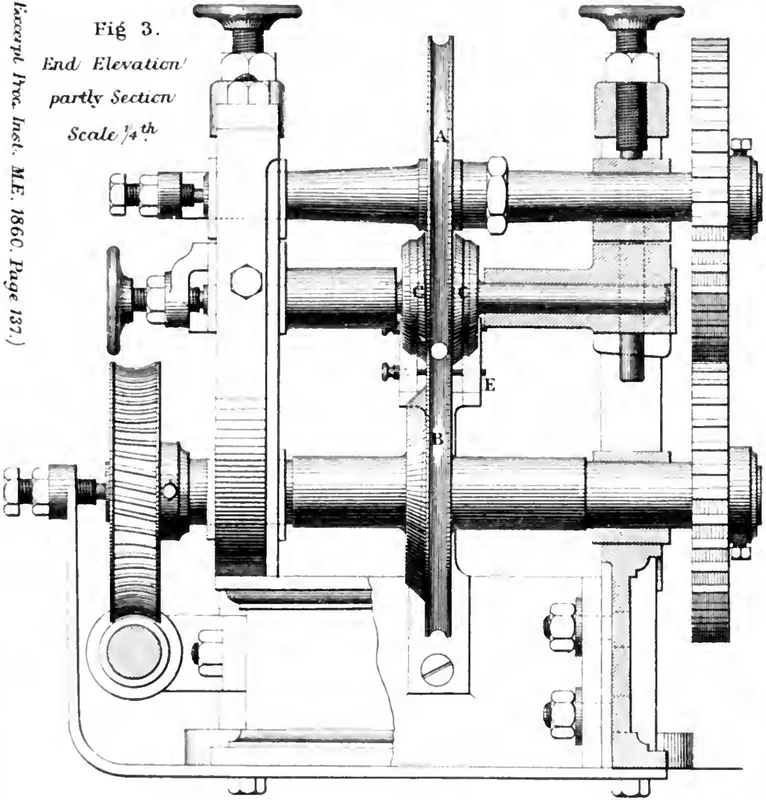


Fig 3.
End Elevation
partly Section
Scale ¼th.

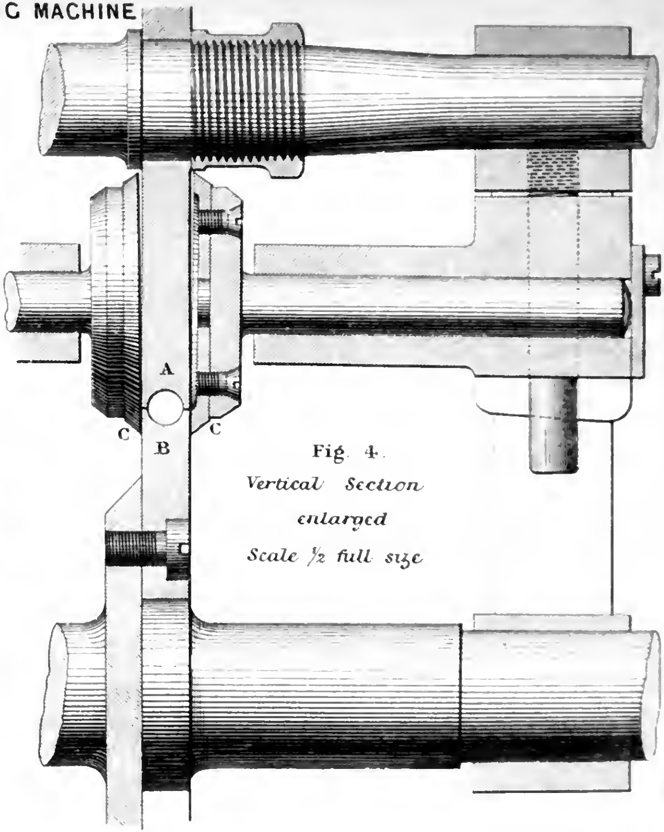


Fig. 4.
Vertical Section
enlarged
Scale ½ full size

(Excerpt Proc. Inst. M.E. 1860. Page 137.)

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INDIA RUBBER COVERING MACHINE.

Fig. 5.
Plan partly Sectional.

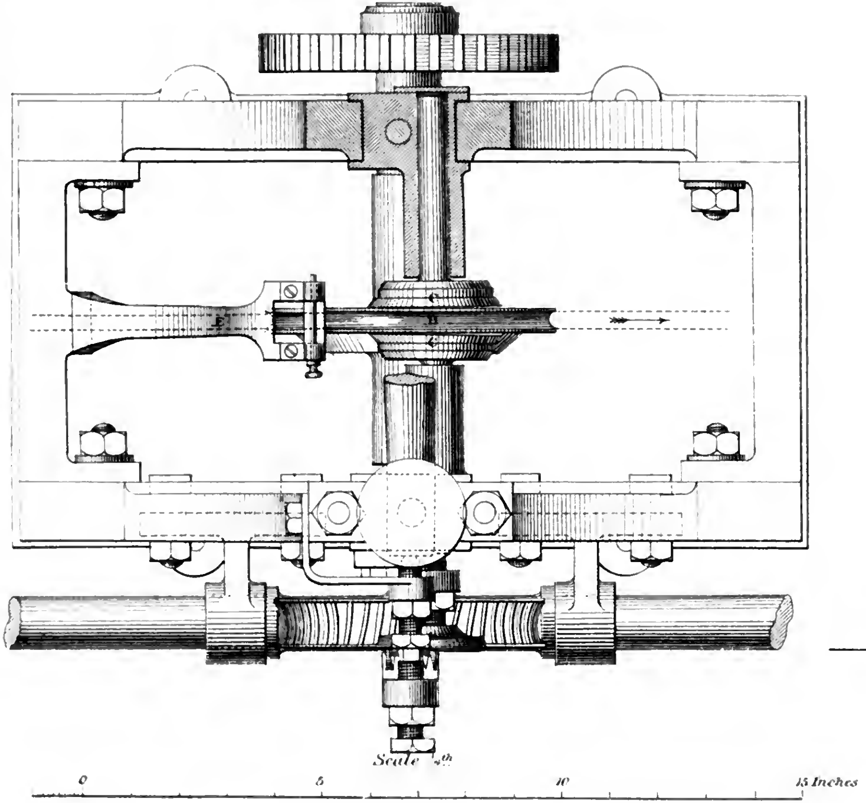
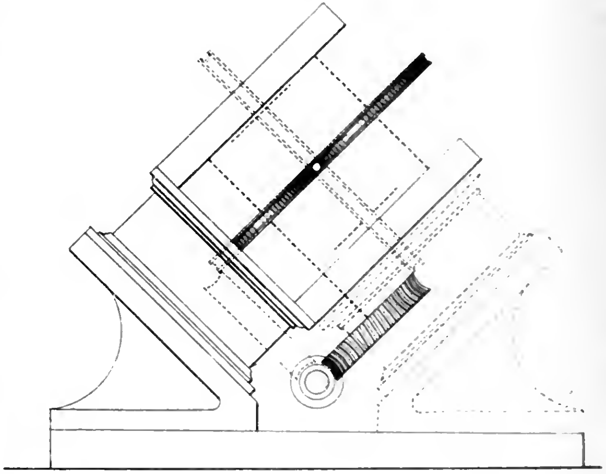


Fig. 6.
General Arrangement of Train of Machines.



Scale ⅛th

(Excerpt Proc. Inst. M.E. 1860. Page 137.)

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INDIA-RUBBER COVERING MACHINE.

Fig. 7. *Longitudinal Section through Pressing Rollers.*

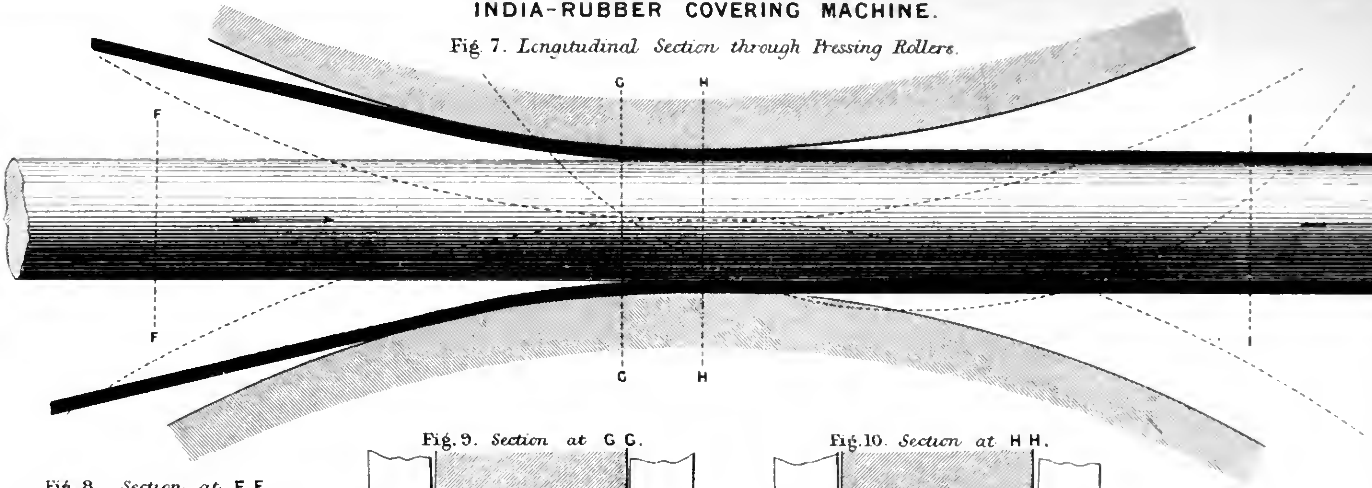


Fig. 8. *Section at* F F.

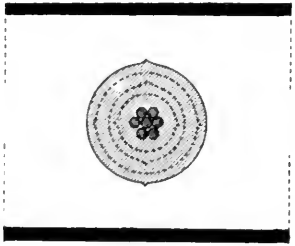


Fig. 9. *Section at* G G.

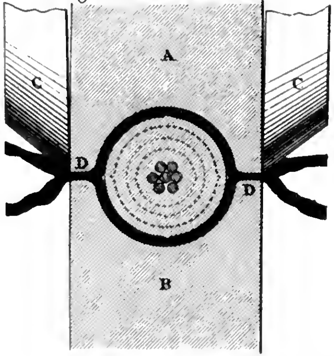


Fig. 10. *Section at* H H.

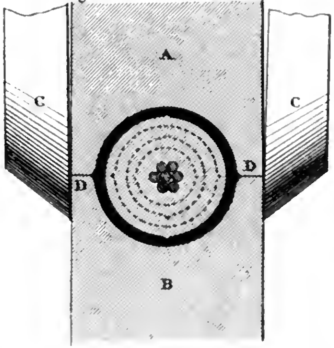
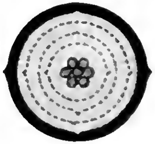


Fig 11
Section at H.



Scale double full size.

Excerpt Proc. Inst. M.E. 1860. Page 137.)

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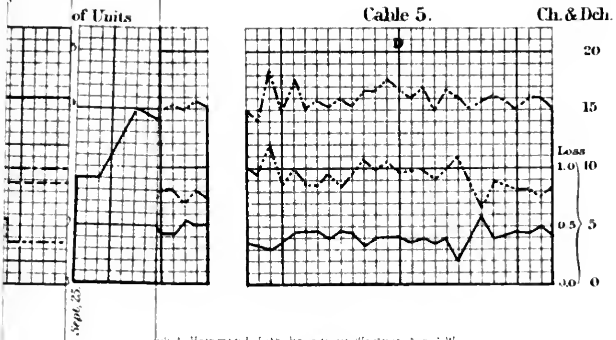

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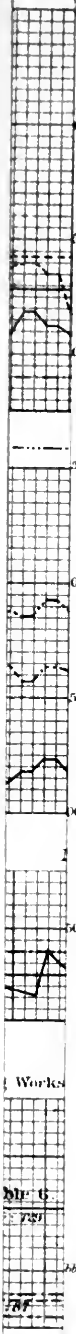
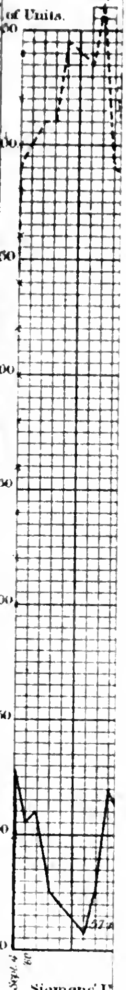
Plate 7.

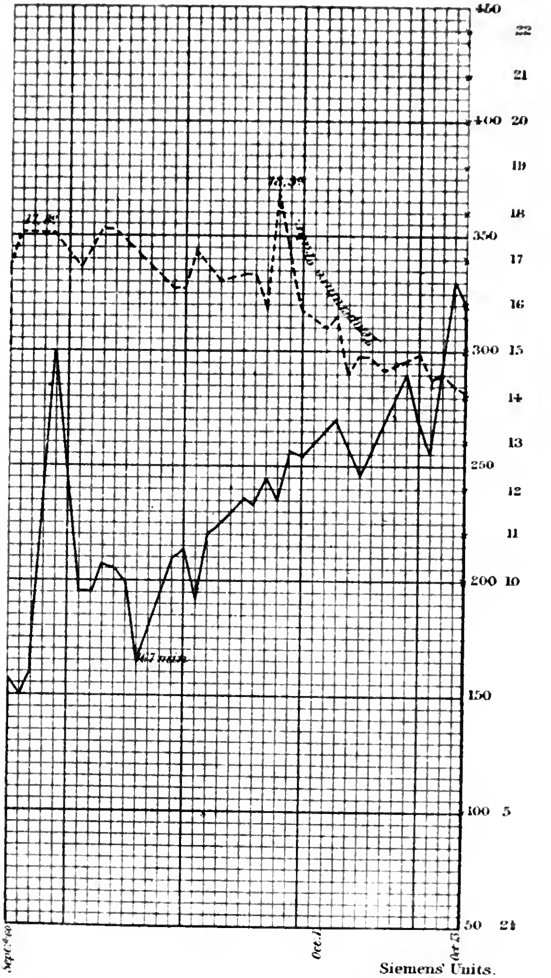
Vorks.

d after selfheating.

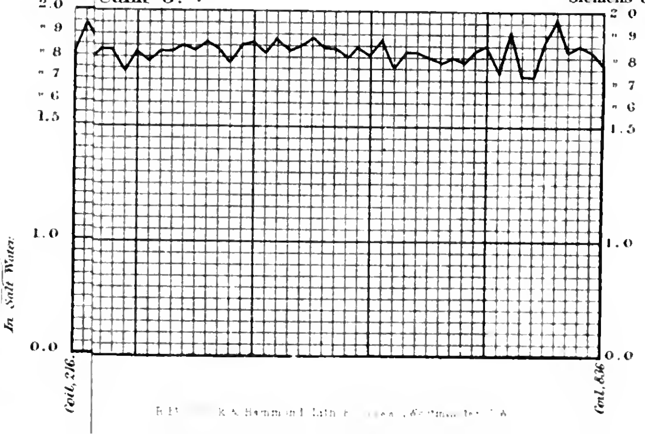




Cable 5°



Cable 6.



R. H [illegible] & A. Hammond Lith. [illegible] Westminster

MEDITERRANEAN TELEGRAPH MACHINERY.

Fig 1. *General arrangement of Machinery in the "Dix Decembre."*

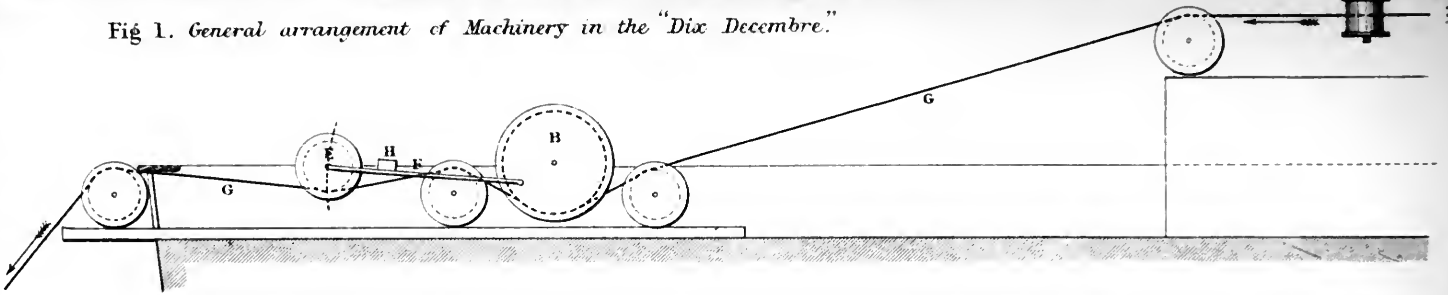
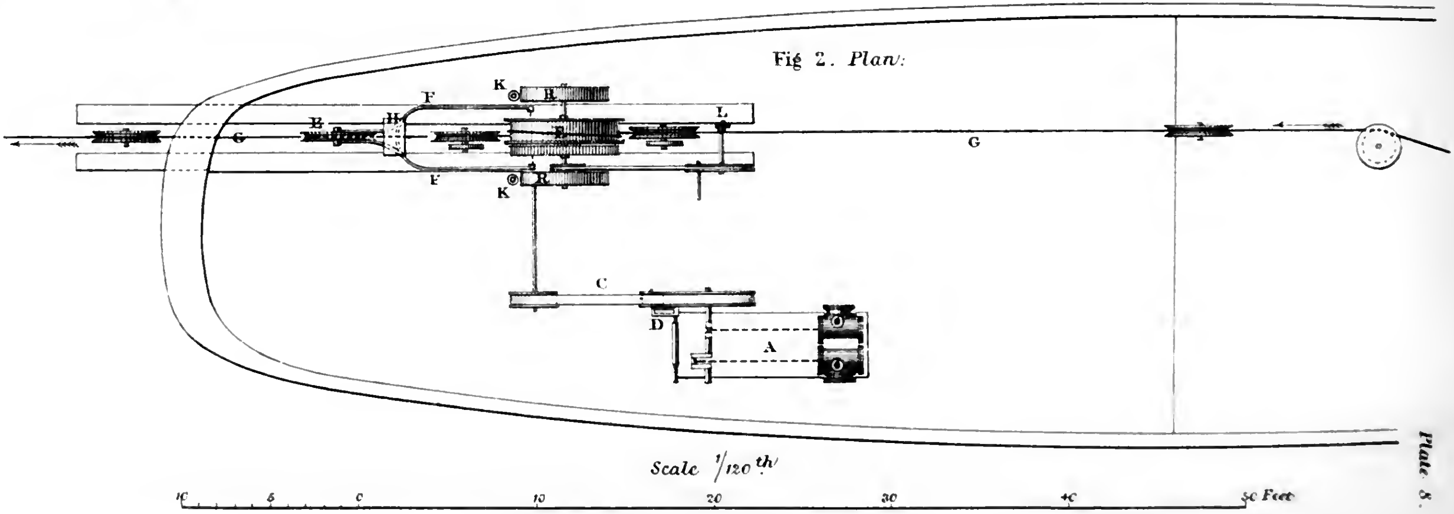


Fig 2. *Plan.*



(Excerpt Proc. Inst. M.E. 1867 Page 35.)

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MEDITERRANEAN TELEGRAPH MACHINERY.

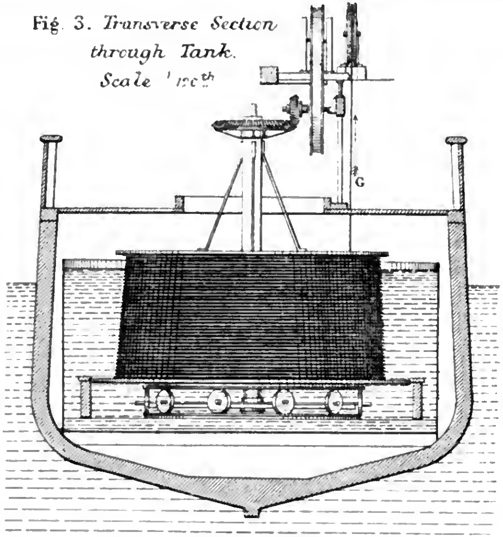


Fig. 3. *Transverse Section through Tank.*
Scale 1/120th

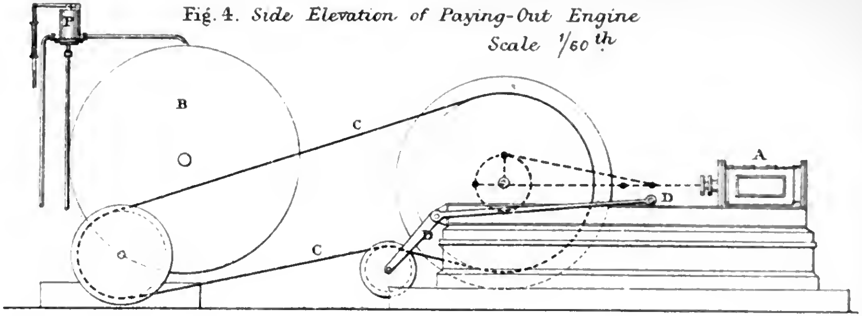
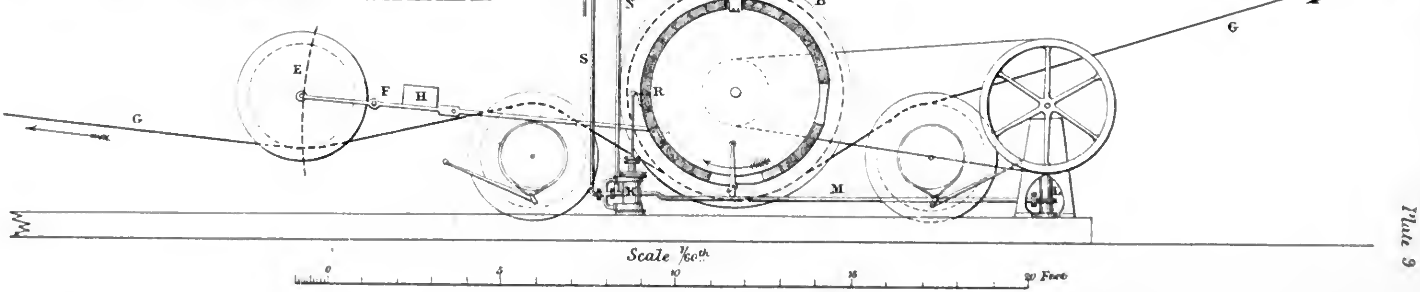


Fig. 4. *Side Elevation of Paying-Out Engine*
Scale 1/60th

Fig. 5. *Side Elevation of Paying-Out Drum and Dynamometer*
Scale 1/60th



(Excerpt Proc. Inst. M.E. 1867. Page 35.)

MEDITERRANEAN TELEGRAPH MACHINERY.

Fig. 7. Scale 1/6th Enlarged Section of Regulating Valve.

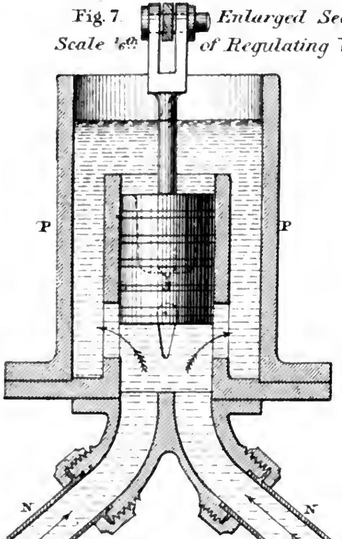


Fig 8. Scale 1/6th Section of Four Way Cock.

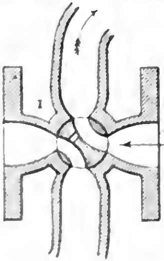
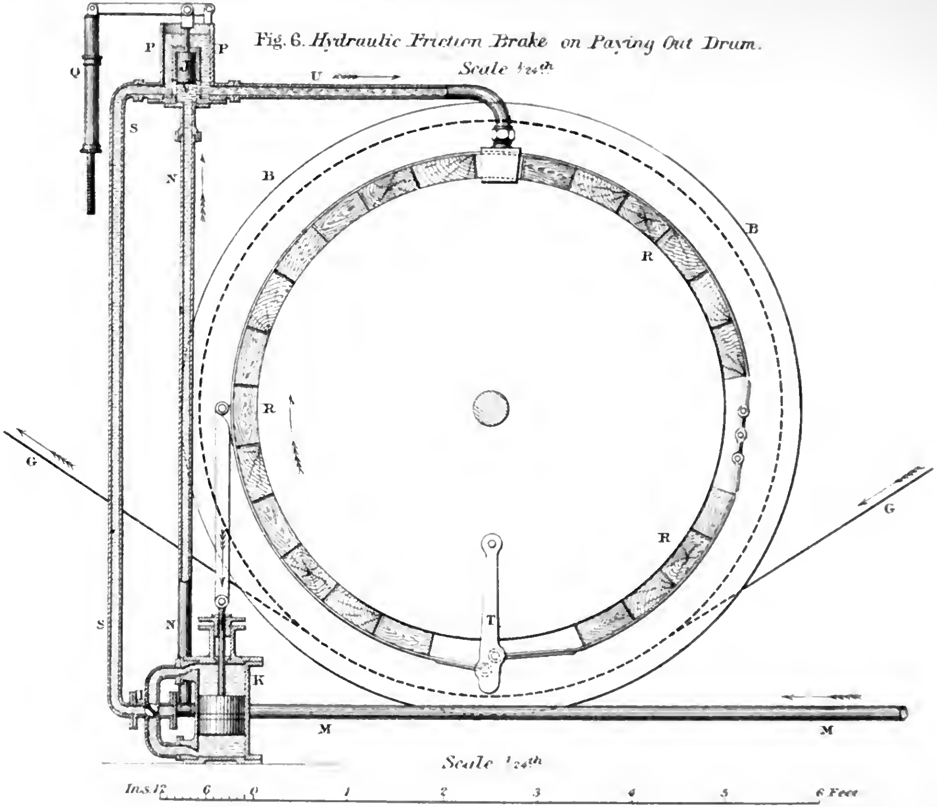


Fig. 6. Hydraulic Friction Brake on Paying Out Drum. Scale 1/24th



(Excerpt Proc Inst M.E. 1867 Page 37.)

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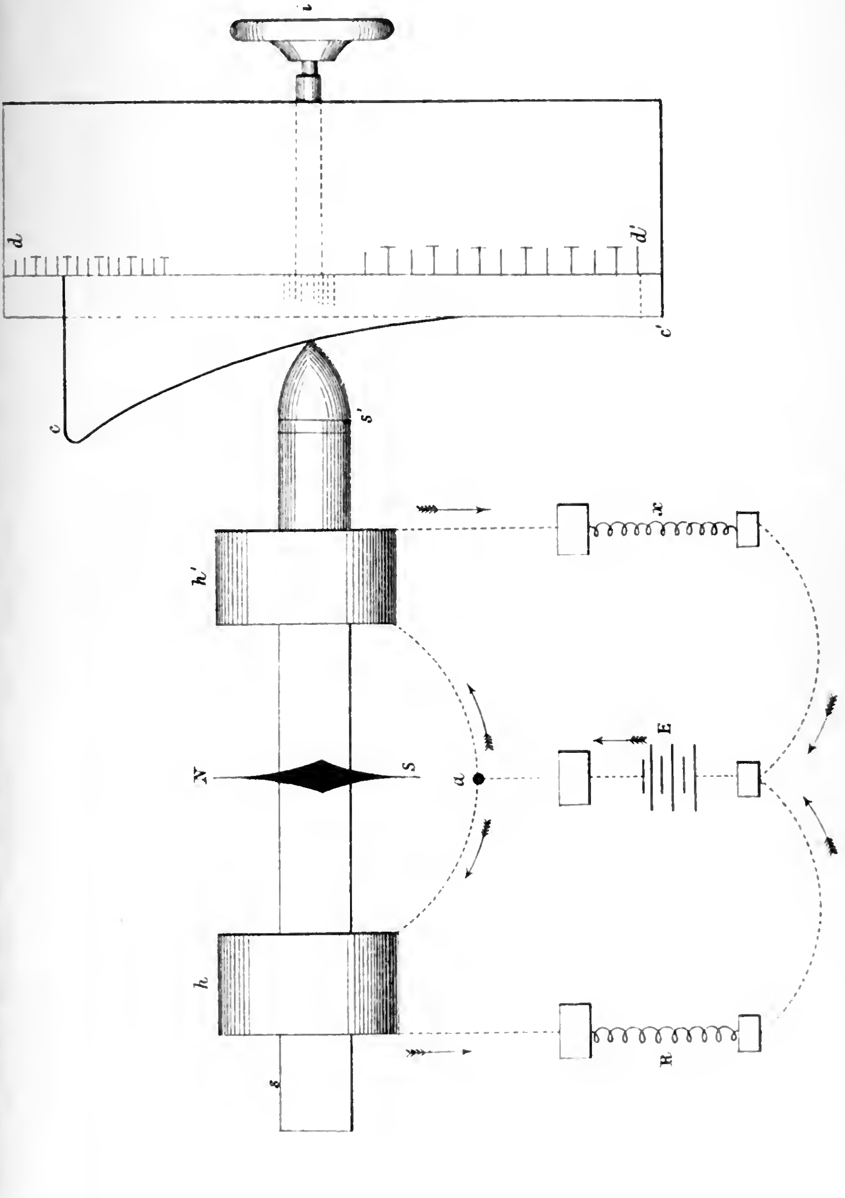
RESISTANCE MEASURER.



(Excerpt Phil. Mag. Vol. 1867)

Rob^t J. Cook & Hammond Lith. Broadway, Westminster, S.W.

DEPENDENCE OF ELECTRICAL RESISTANCE ON TEMPERATURE.

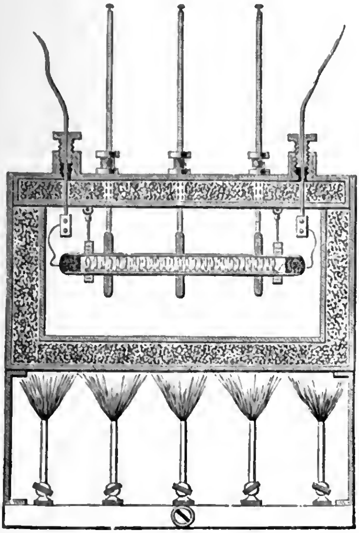


Fig. 1.

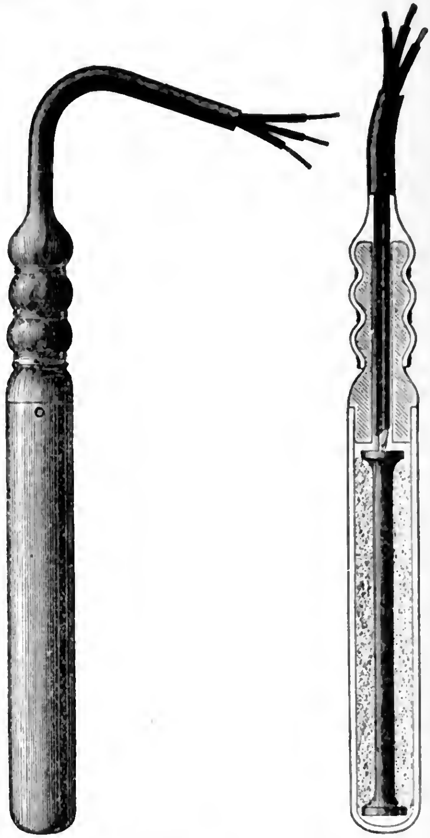


Fig. 3.

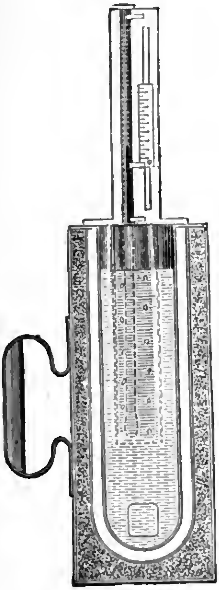


Fig. 2.

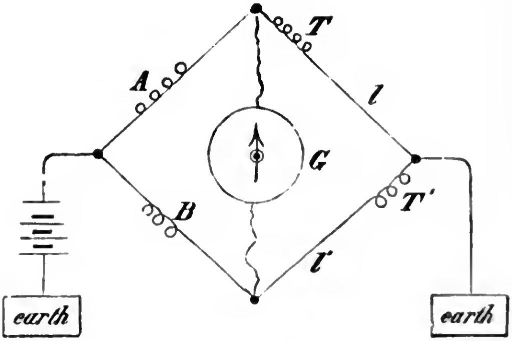
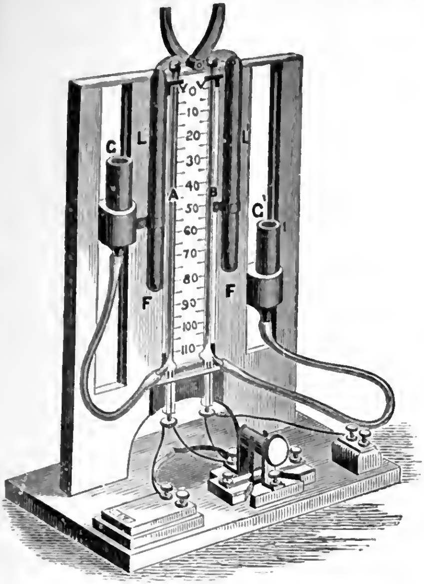


Fig. 4.

(*Excerpt Jour. Soc. T.E.*, 1874.)

DEPENDENCE OF ELECTRICAL RESISTANCE ON TEMPERATURE.



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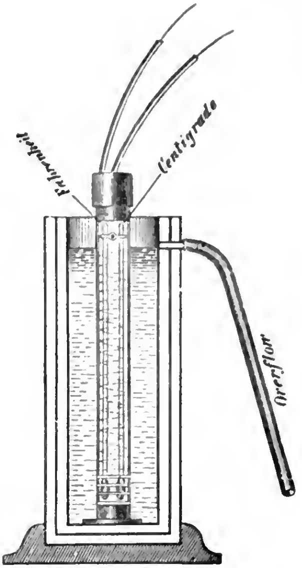


Fig. 5.

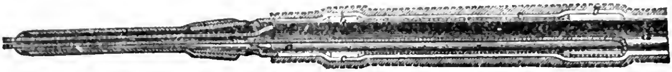


Fig. 6

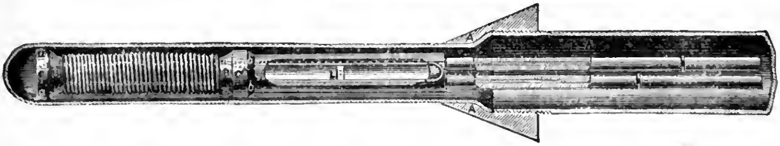


Fig. 7.

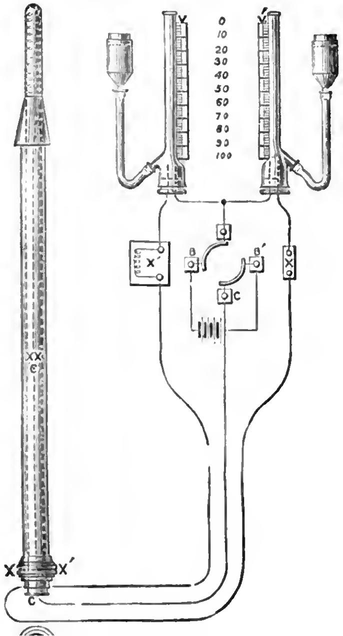
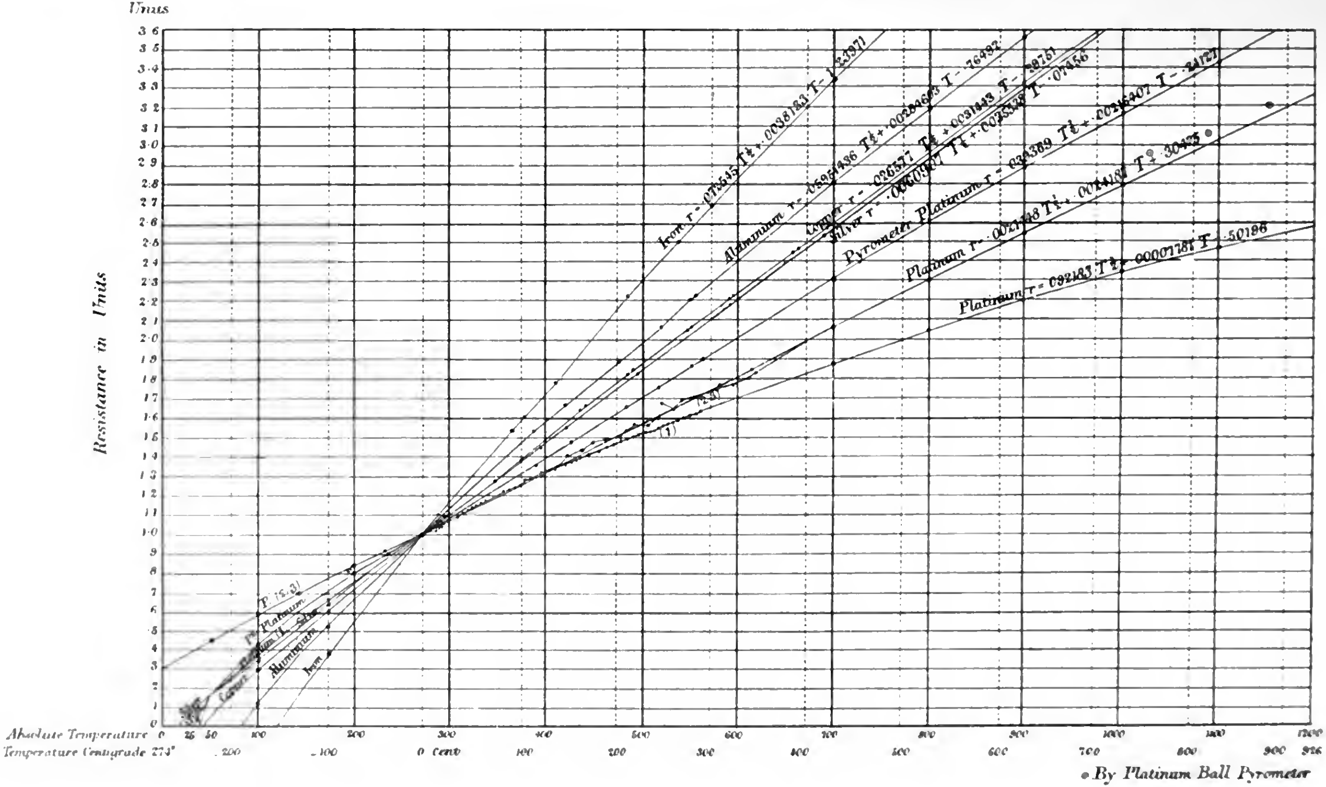


Fig. 10.

Fig. 8.

Diagram N.º 1.



(Excerpt Jour. Soc. T.E. 1874.)

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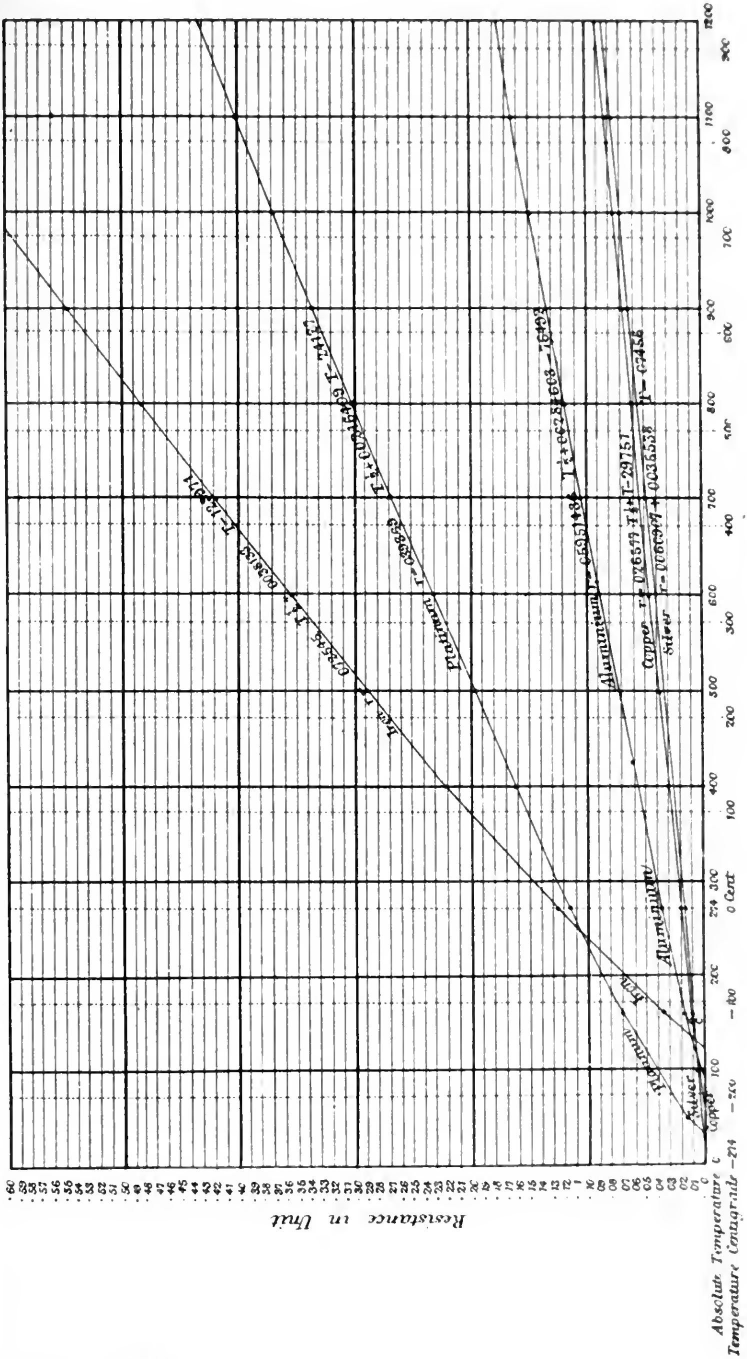
DIAGRAM Nº 2.



(Excerpt Jour. Soc. T.E. 1874.)

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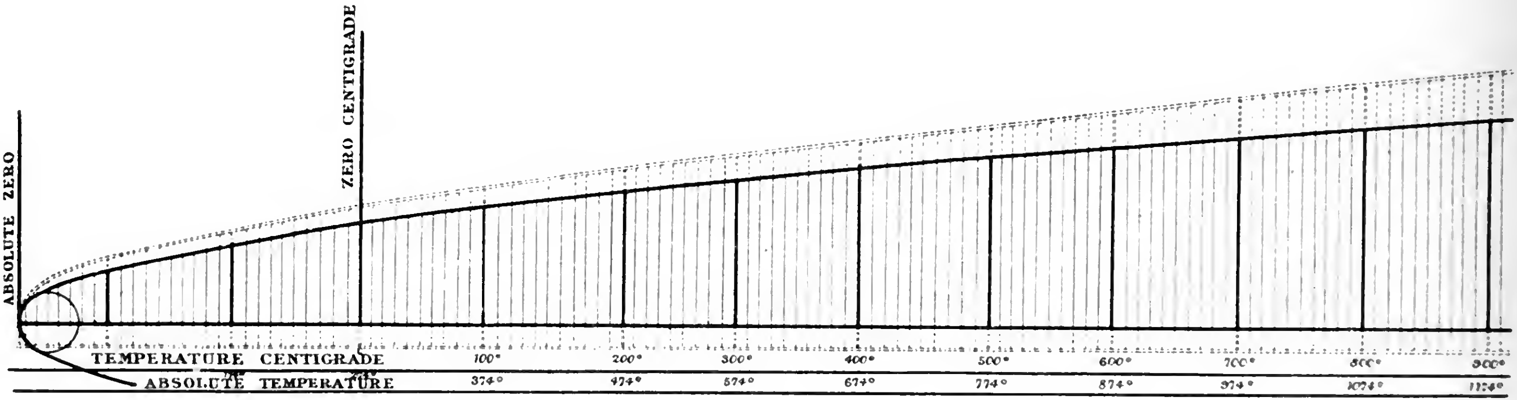
DEPENDENCE OF ELECTRICAL RESISTANCE ON TEMPERATURE.

DIAGRAM Nº 3.

$$r = \alpha \; T^{1/2} + \beta \; T + \gamma$$



(Excerpt Jour Soc T.E. 1874.)

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ELECTRIC CURRENT REGULATOR.

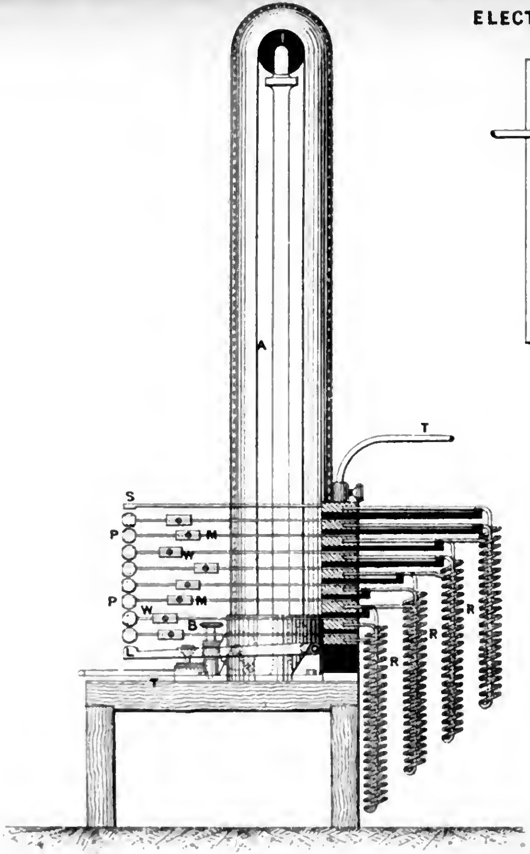

Fig. 1.

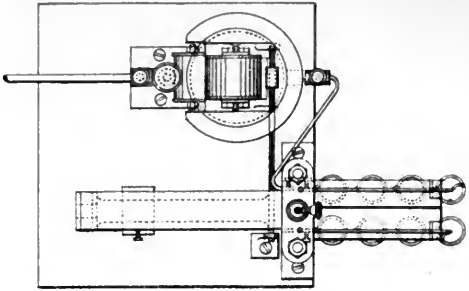

Fig. 3.

A.—Sensitive strip.
B.—Tension screw.
L.—Lever.
M.—Metal springs.
I.—Insulated pulley.
W.—Adjusting weights.
P.—Contact prisms.
S.—Contact spring.
T.—Terminals.
R.—Resistance coils.

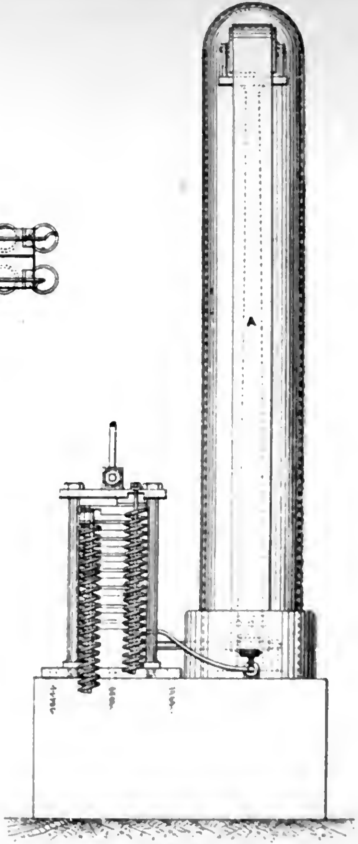

Fig. 2.

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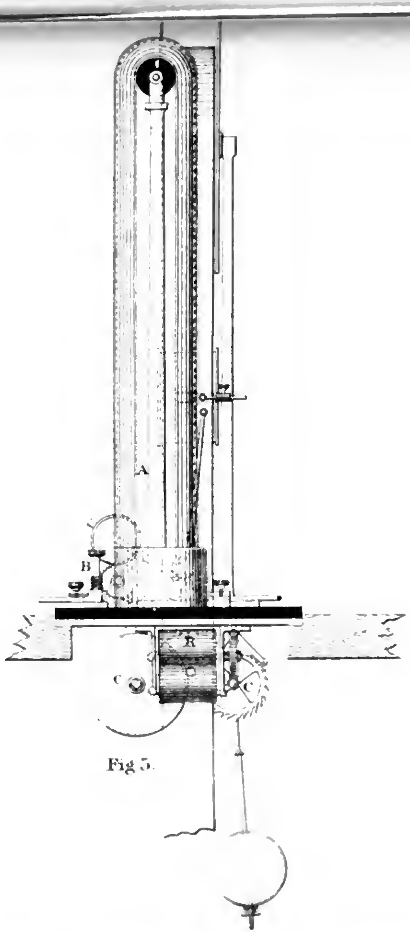


Fig 5.

A. *Sensitive strip*
B. *Tension screw*
I. *Insulated pulley*
L. *Lever*
D. *Datum pencil*
P. *Movable pencil*
C. *Clockwork*
R. *Friction roller*

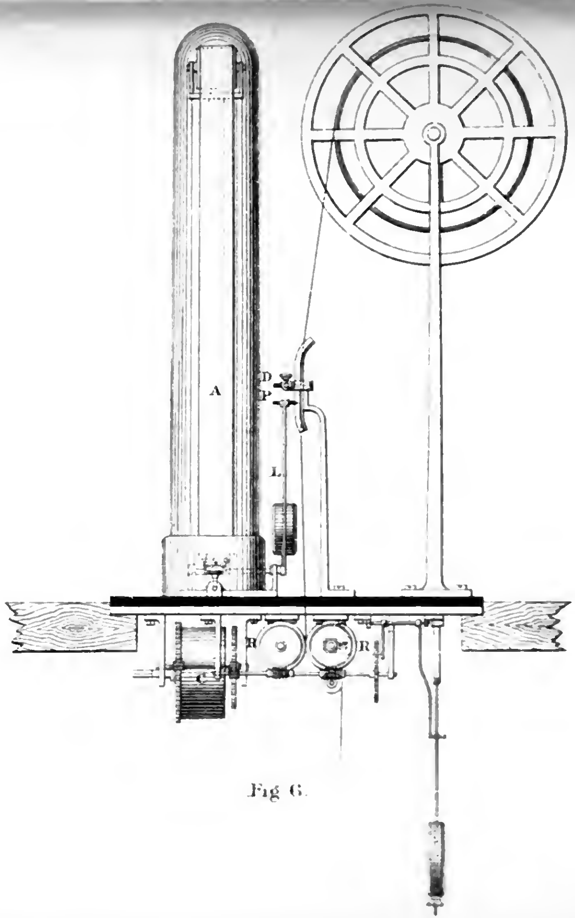
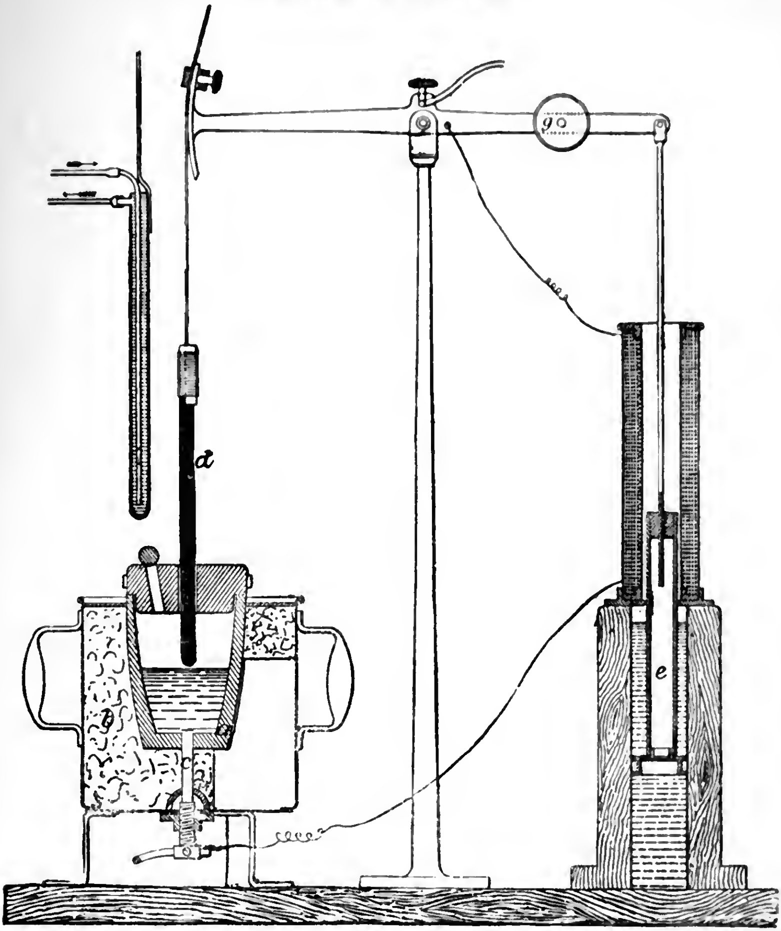


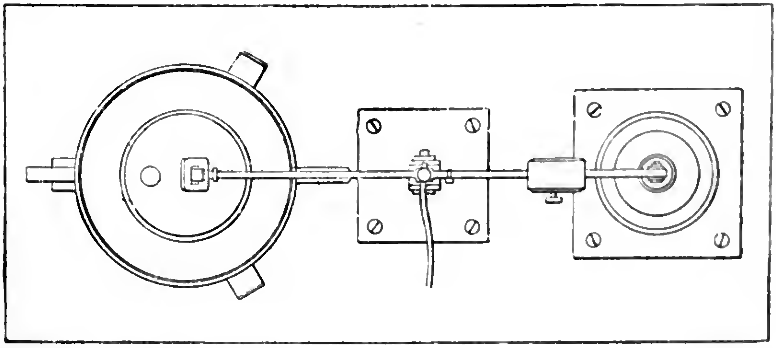
Fig 6.

(Excerpt Proc Roy Soc Vol. 28)

& Hammond Lith. Broadway, Westminster, W.

ELECTRIC FURNACE

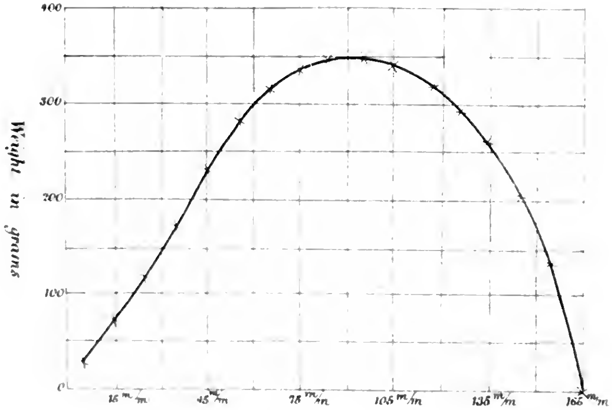




(*Excerpt Jour. Soc. T.E.* 1880.)

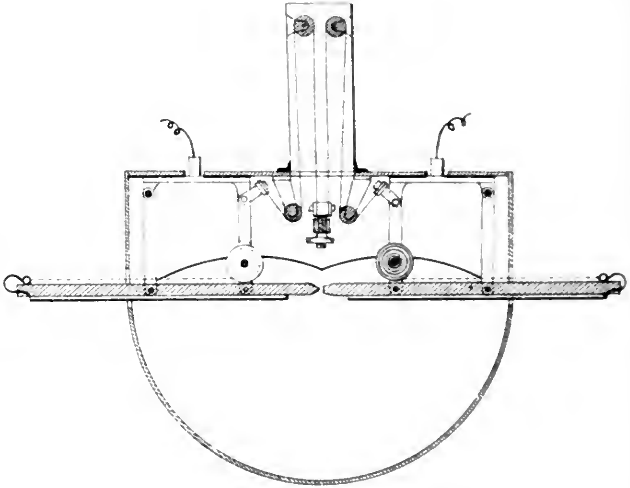
SOLENOID CURVE.

FIGURE. 2.



HORIZONTAL ELECTRIC LAMP.

FIGURE. 3.



(Excerpt Jour Soc T.E 1880.)

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SECONDARY BATTERY.

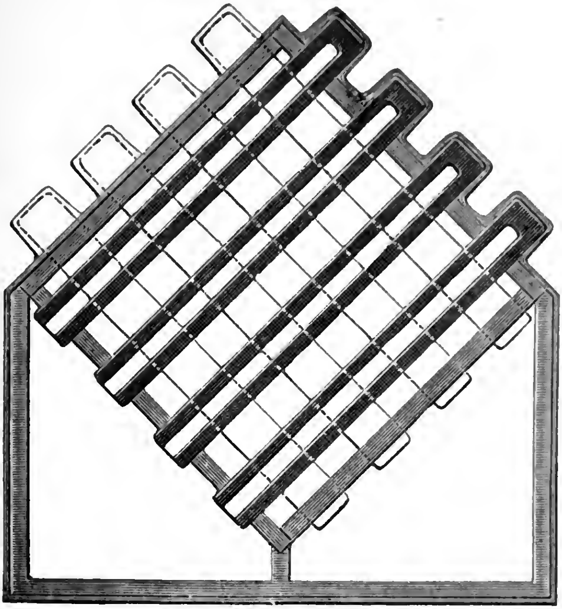


Fig. 2.

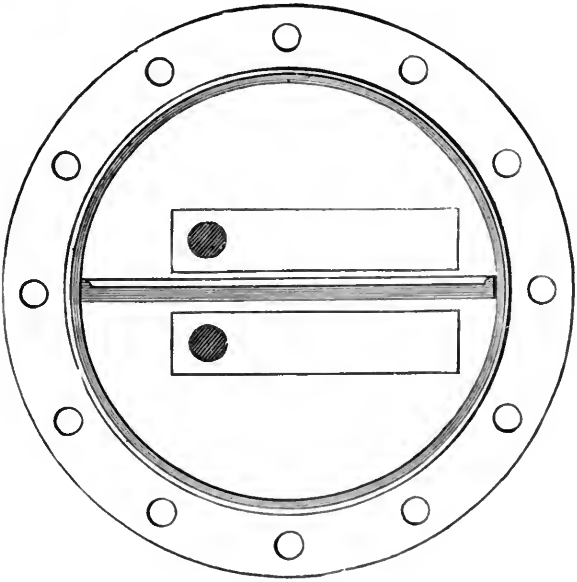


Fig. 3.

DEEP SEA ELECTRICAL THERMOMETER

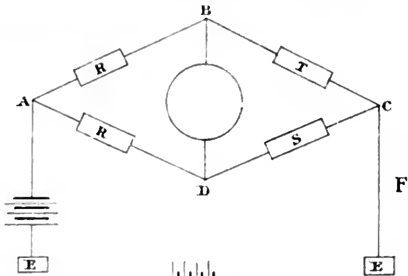


Fig. 1.

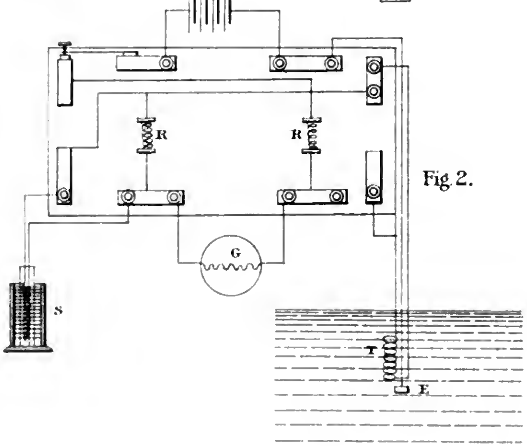
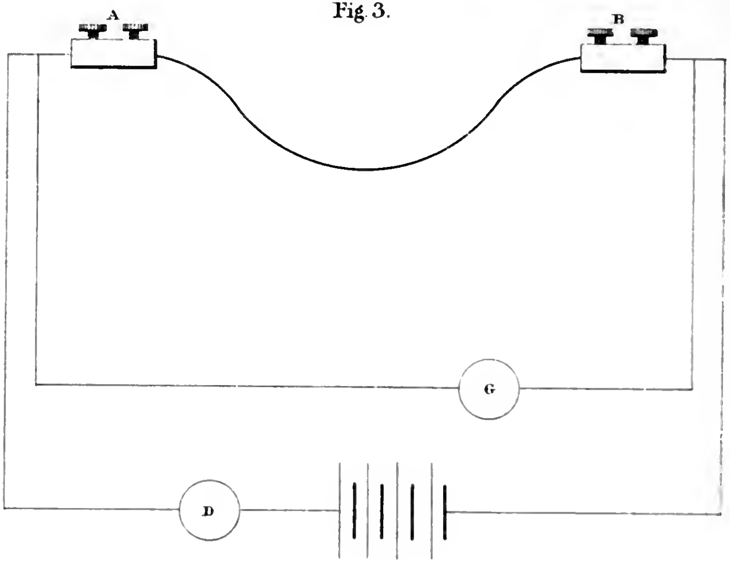


Fig. 2.

DEPENDENCE OF RADIATION ON TEMPERATURE.

Fig. 3.



Excerpt Proc. R. S Vol XXXIV & XXXV.

WATER METER.

Fig. 1. *Bucket Meter.*

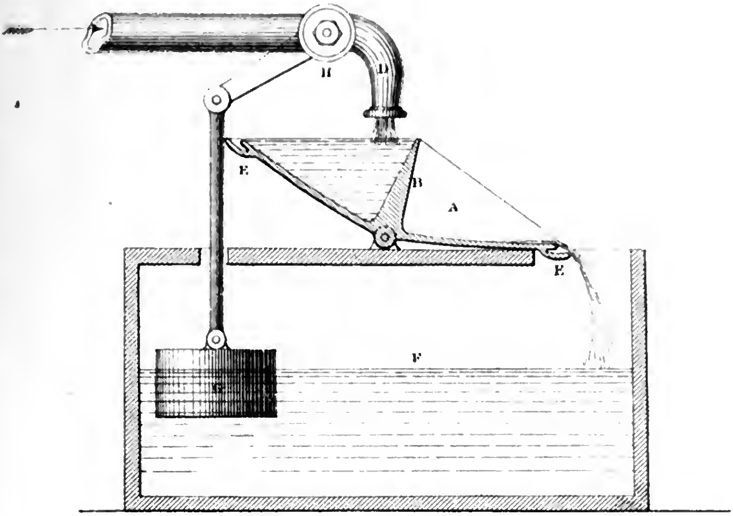
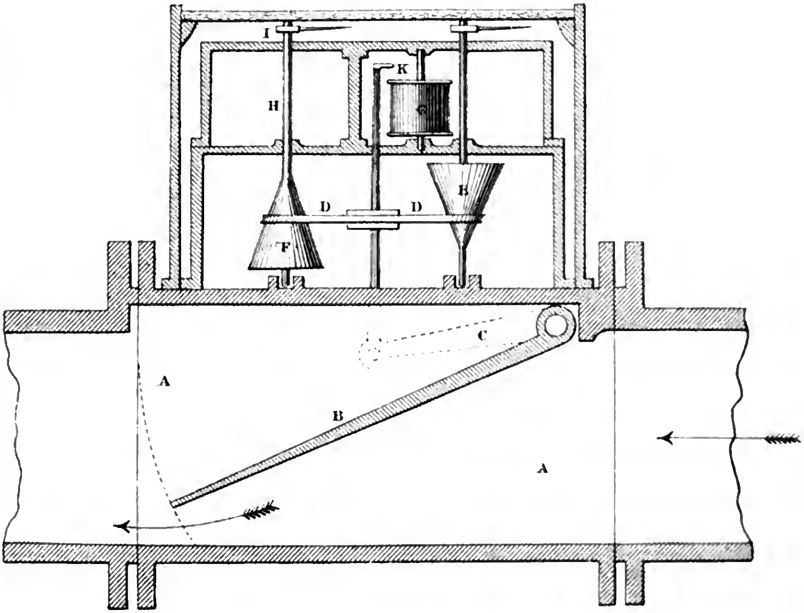


Fig. 2. *Meter by Area of Channel.*



(Excerpt Proc. Inst. M.E. 1854)

IMPROVED WATER METER.

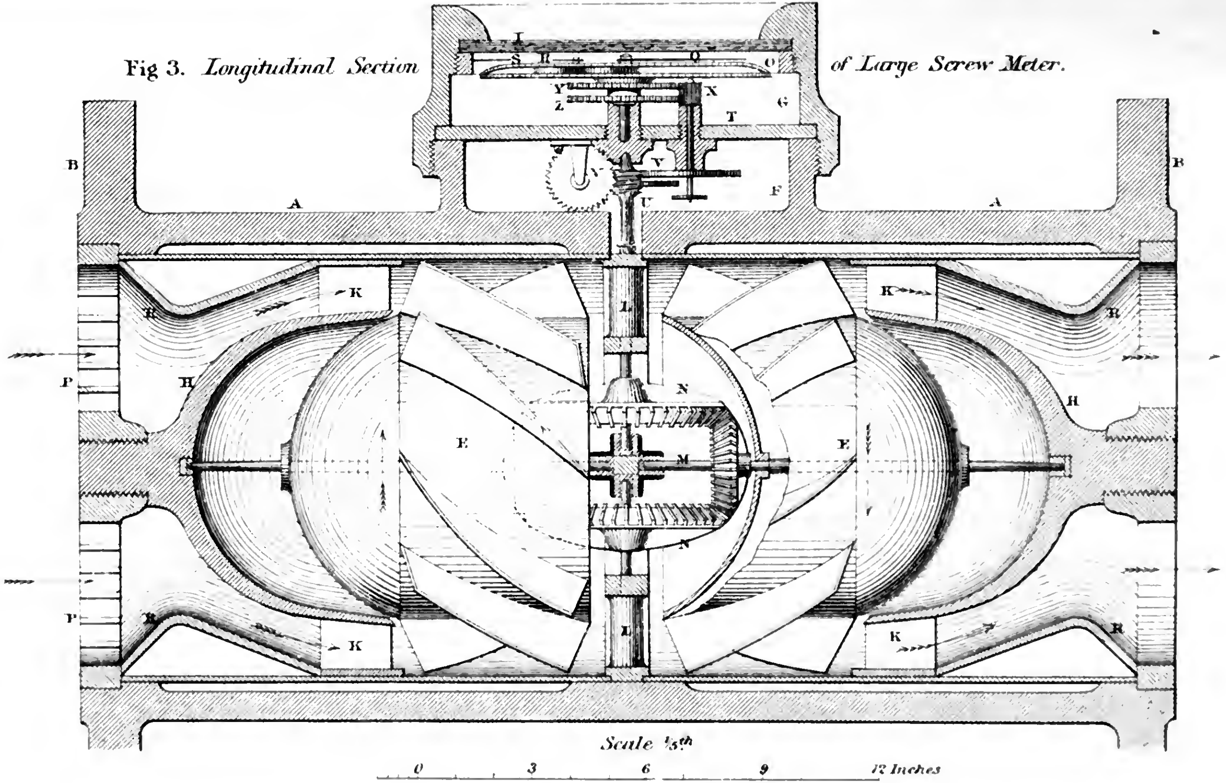


Fig 3. *Longitudinal Section of Large Screw Meter.*

IMPROVED WATER METER.

Fig. 4. *Transverse Section. of Large Screw Meter.*

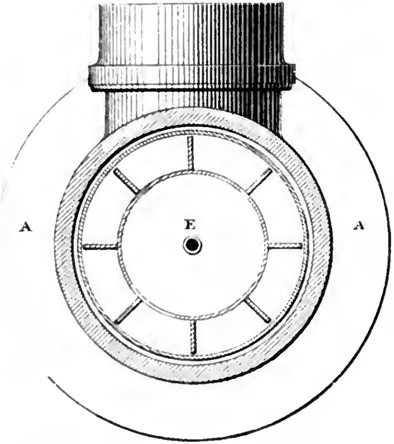
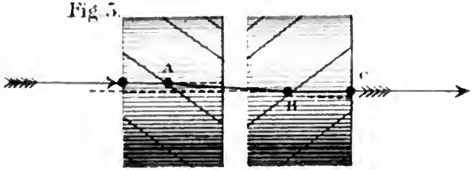
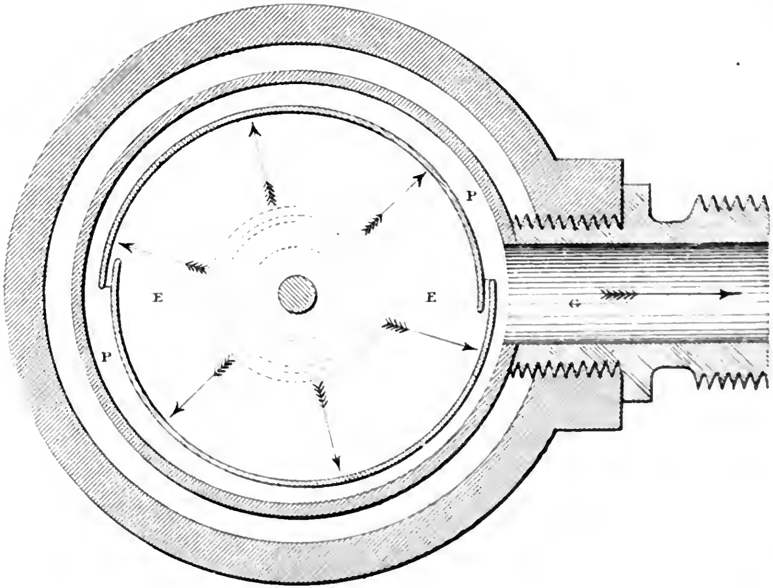


Fig. 5.



Scale [illegible] size.

Fig. 6. *Plan of Small Spiral Meter.*

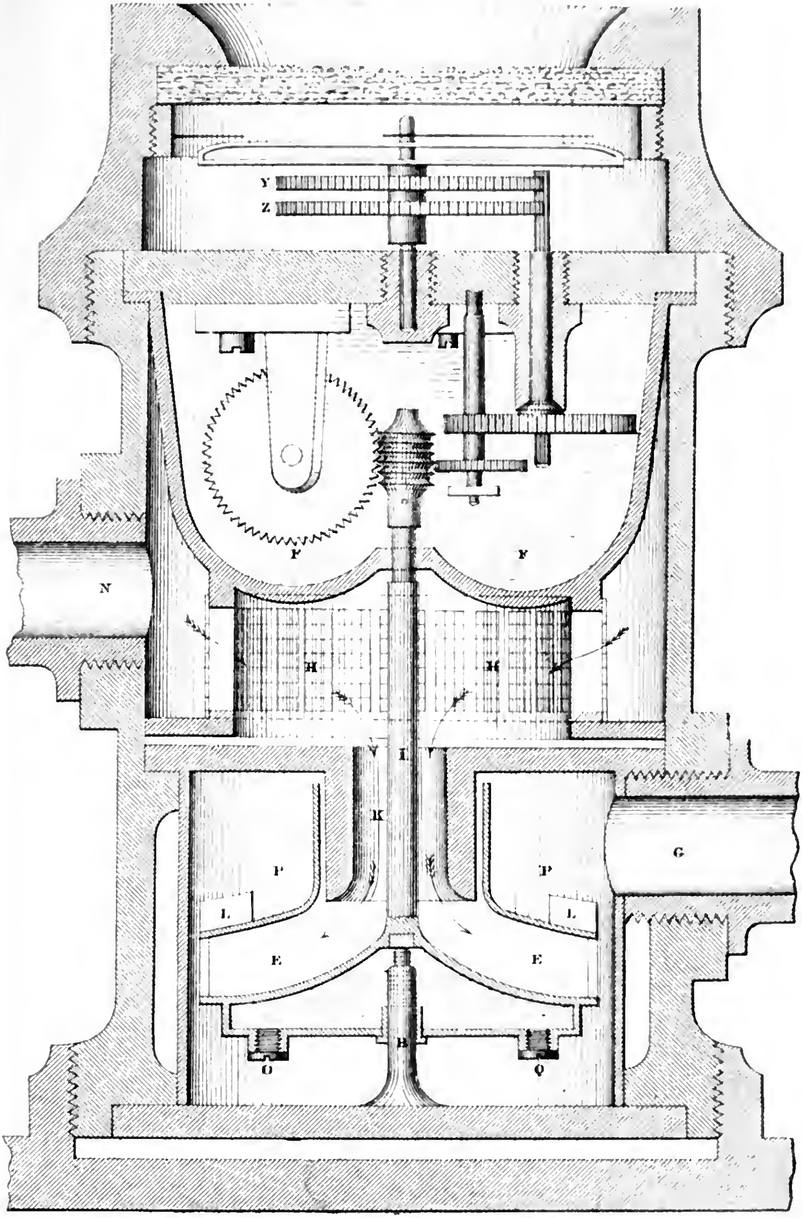


Scale, Full size.

IMPROVED WATER METER.

Fig. 7.

Transverse Section of Small Spiral Meter



Scale, Full size.

0 1 2 3 *Inches*

(Excerpt Proc. Inst. M. E. 1854.)

WATER METER.

Fig. 1. *Section of Meter and Filtering Chamber.*

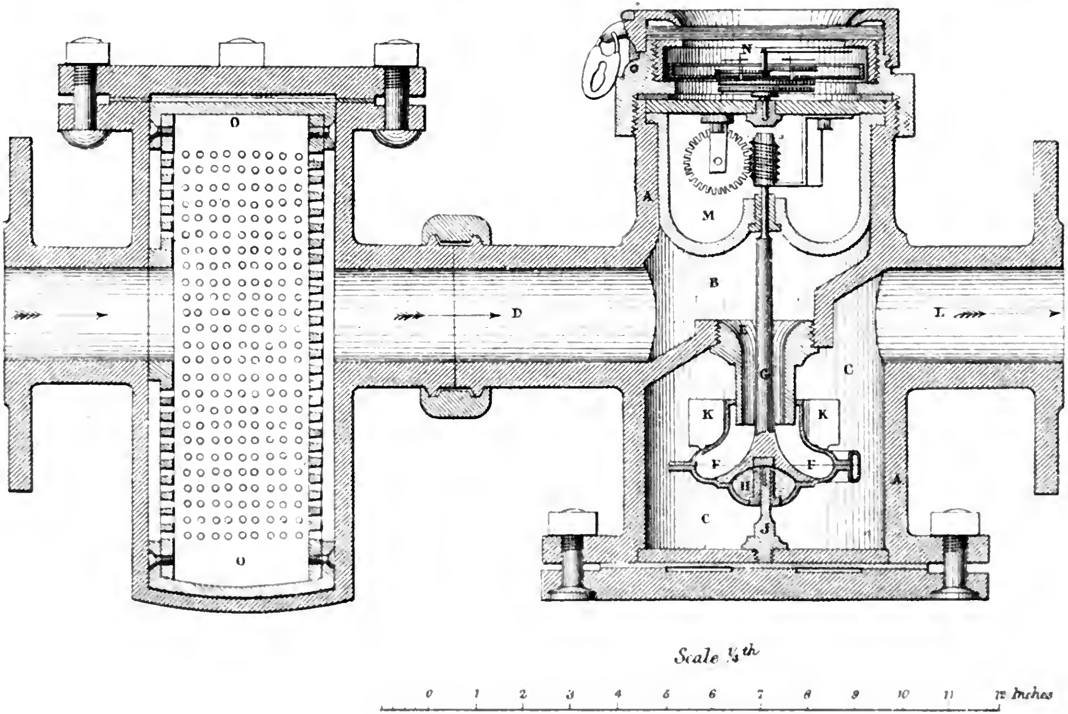


Fig. 2. *Perspective View of Drum.*

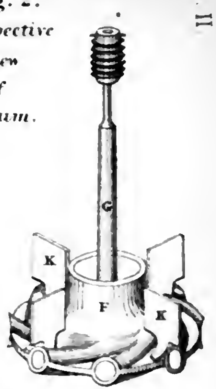
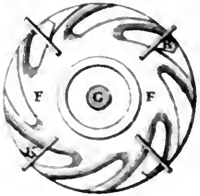


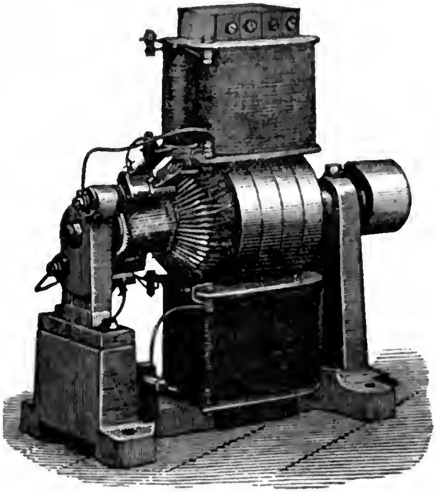
Fig. 3 *Plan of Drum*



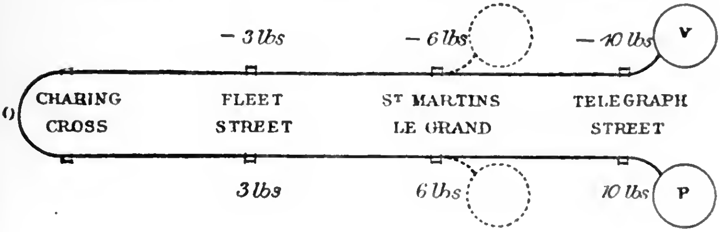
(*Excerpt. Proc. Inst. M.E. 1856*)

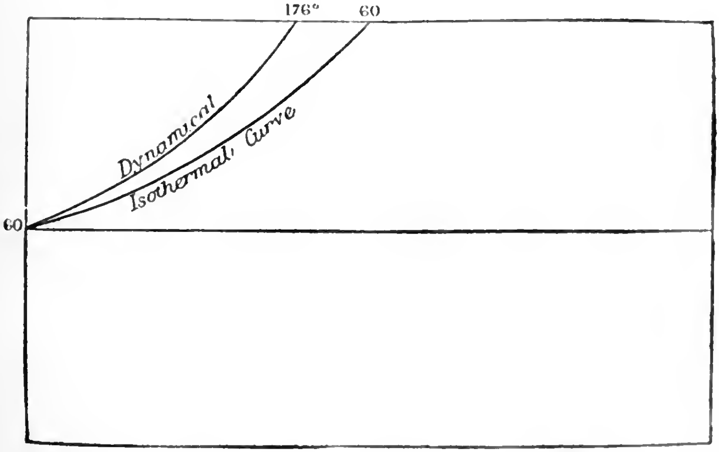
[illegible] & Hammond, Lith. Broadway, Westminster.

DYNAMO MACHINE.



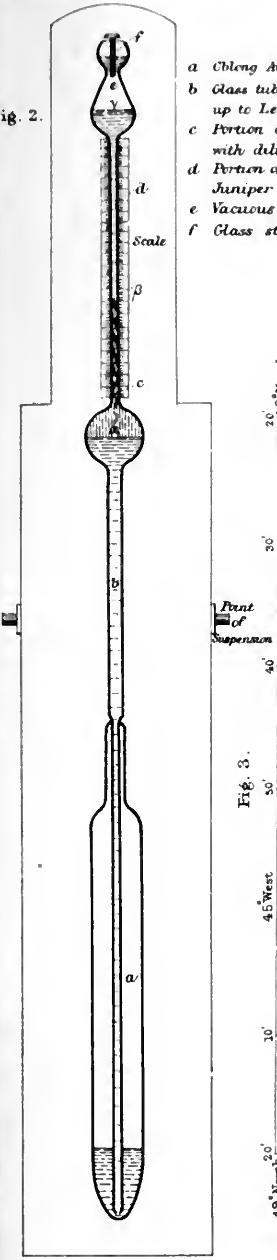
PNEUMATIC TRANSMISSION CIRCUIT SYSTEM.





First attempt to construct a Bathometer

Fig. 2.



- a Oblong Air bulb of glass.
- b Glass tube filled with mercury up to Level α
- c Portion of Glass tube filled with diluted alcohol to Level β
- d Portion of tube filled with Juniper Oil to Level γ
- e Vacuous space above Oil
- f Glass stopper

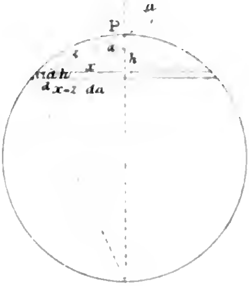
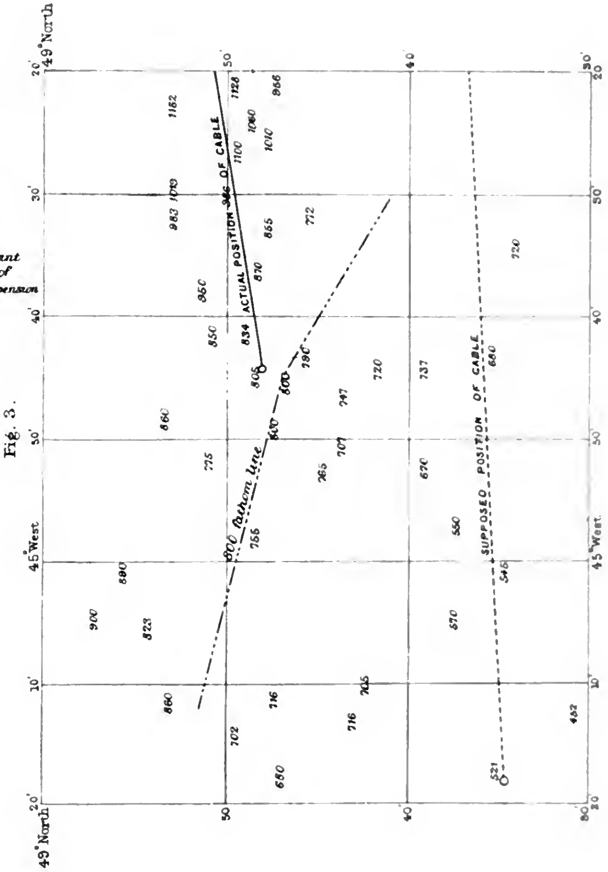


Fig 1.

Fig. 3.

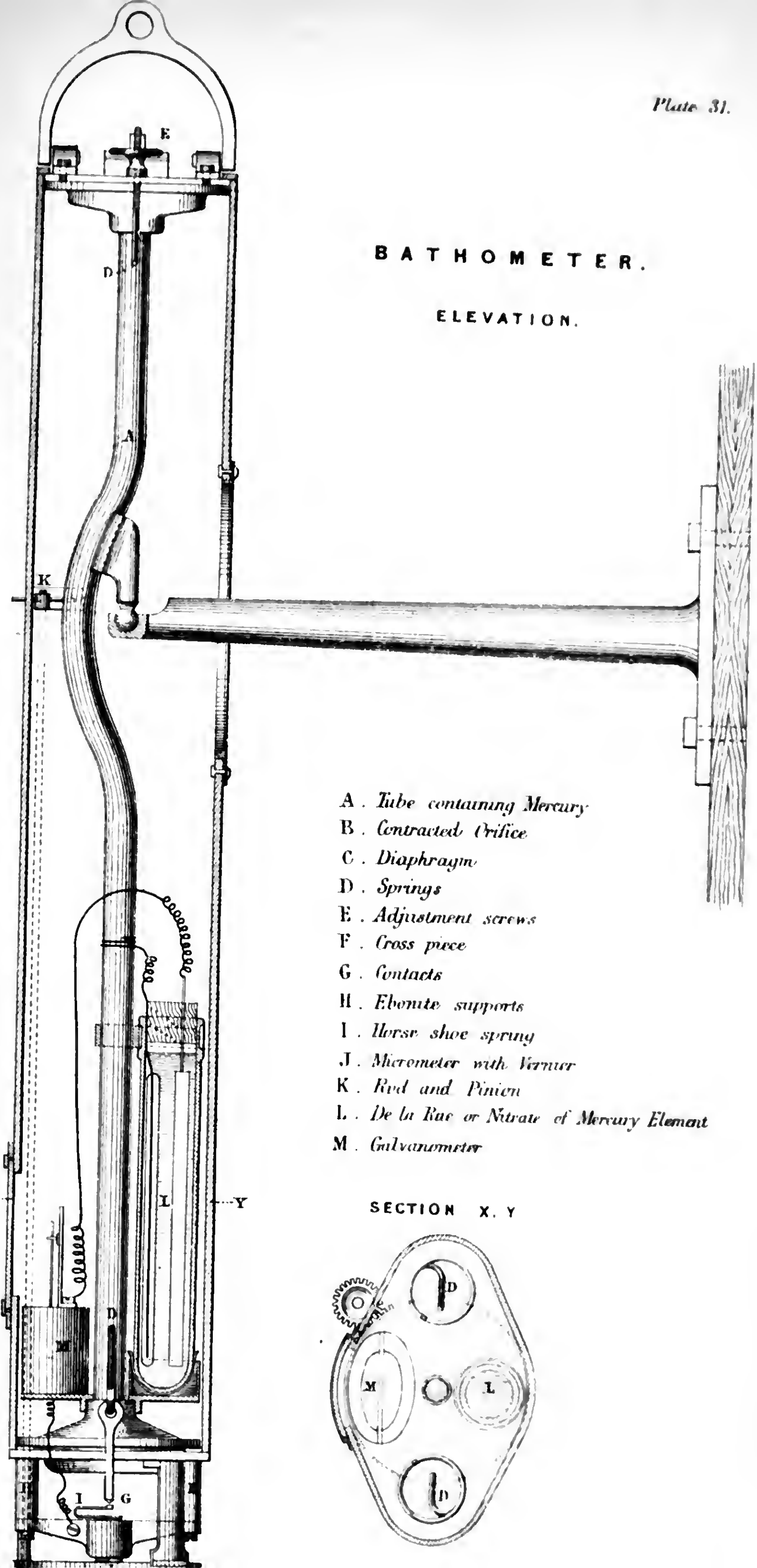


(Excerpt Phil. Trans. 1876.)

Robt J. Cook & Hammond, Lith Broadway, Westminster, S.W.

BATHOMETER.

ELEVATION.

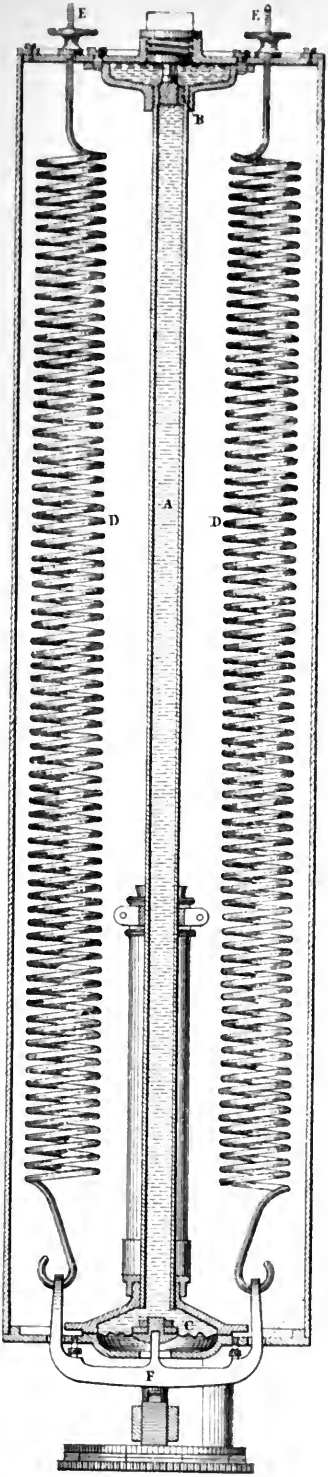


A . *Tube containing Mercury*
B . *Contracted Orifice*
C . *Diaphragm*
D . *Springs*
E . *Adjustment screws*
F . *Cross piece*
G . *Contacts*
H . *Ebonite supports*
I . *Horse shoe spring*
J . *Micrometer with Vernier*
K . *Rod and Pinion*
L . *De la Rue or Nitrate of Mercury Element*
M . *Galvanometer*

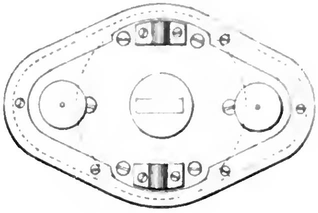
SECTION X. Y

BATHOMETER.

LONGITUDINAL SECTION

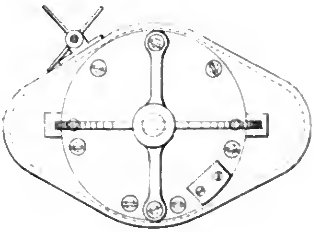


PLAN.



A . *Tube containg Mercury*
B . *Contracted Orifice*
C *Diaphragm*
D . *Springs*
E *Adjustment screws*
F . *Cross piece*

PLAN.

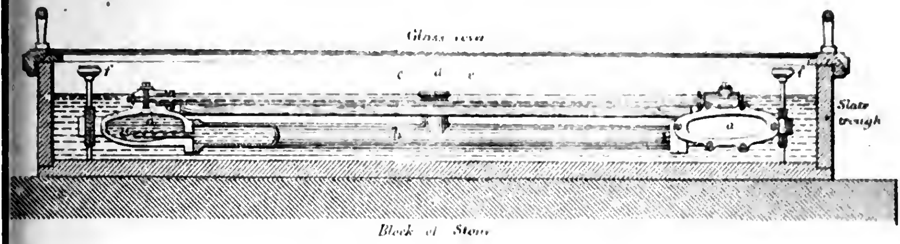


(Excerpt. Phil. Trans. 1876.)

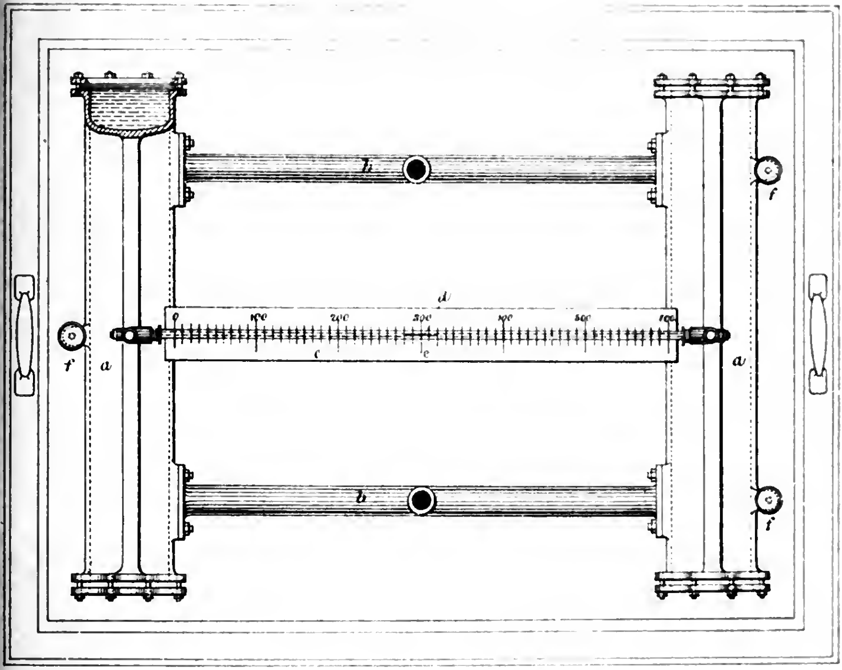
Robt J. [illegible] & Hammond, Lith [illegible]way, Westminster, [illegible]

HORIZONTAL ATTRACTION METER.

Elevation & Section



Plan & Section



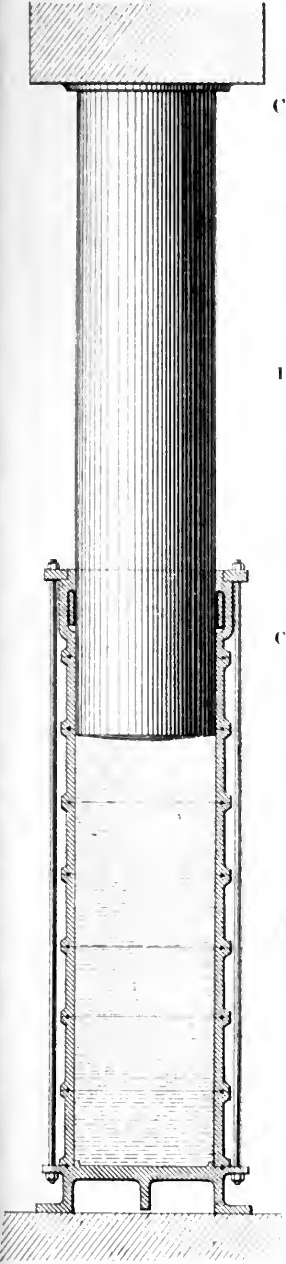
a. Reservoirs filled with mercury to Level a, and alcohol above that Level
b. Cross tubes connecting reservoirs below.
c. Glass tube connecting reservoirs above
d. Index Air bubble
e. Scale
f. Levelling screws.

cerpt Phil. Trans. 1876.

HIGH PRESSURE VESSELS.

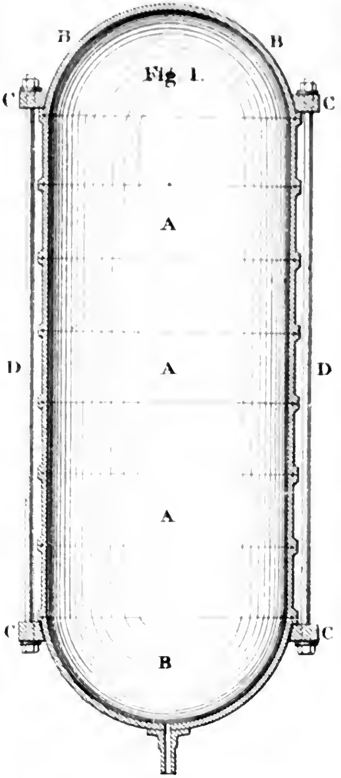
Hydraulic Cylinder

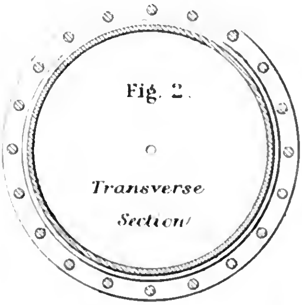
Fig. 5.



Air Vessel for Compressed Air Tramway Locomotive.

Longitudinal Section

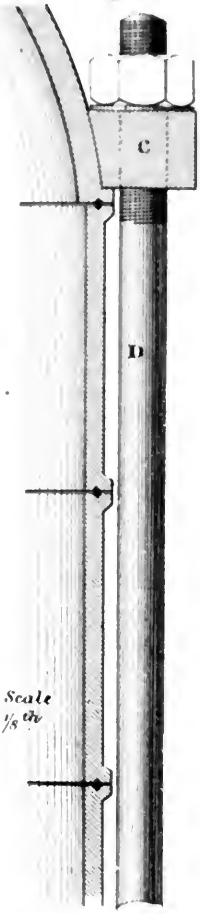


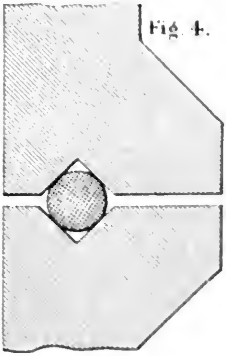


Scale 1/30th.

Ins. 12 6 0 1 2 3 4 5 Feet

Fig. 3.





Full size

(Excerpt Proc. Inst. M.E. 1878.)

HIGH PRESSURE VESSELS.

Marine Boiler

Fig. 6. Longitudinal Section

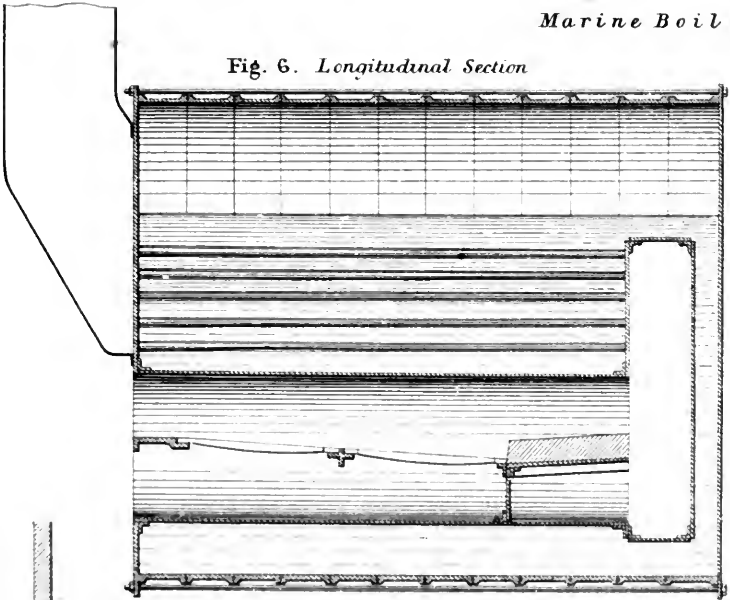


Fig. 7. Transverse Section.

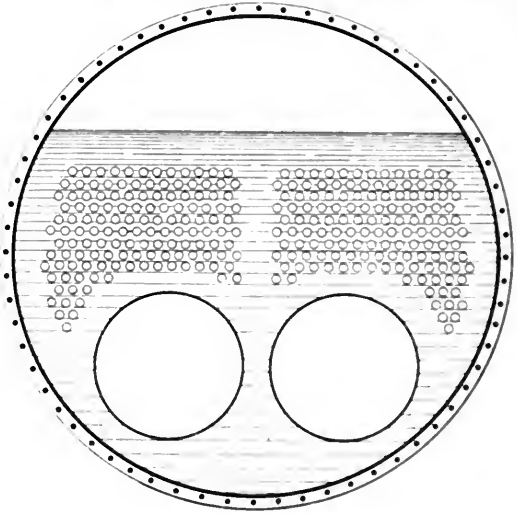
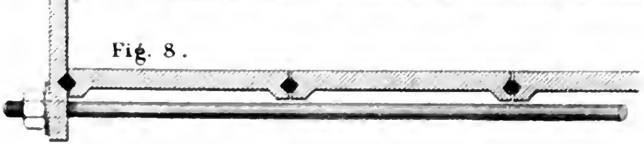
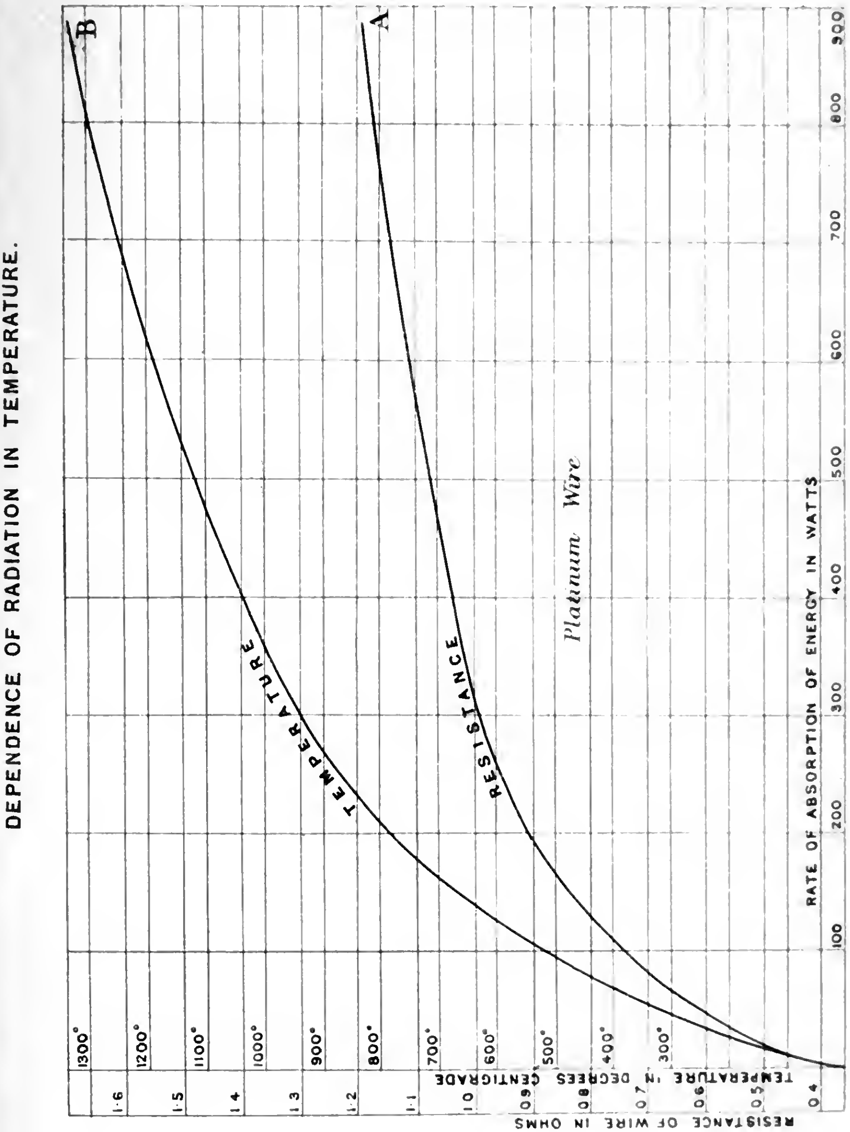


Fig. 8.



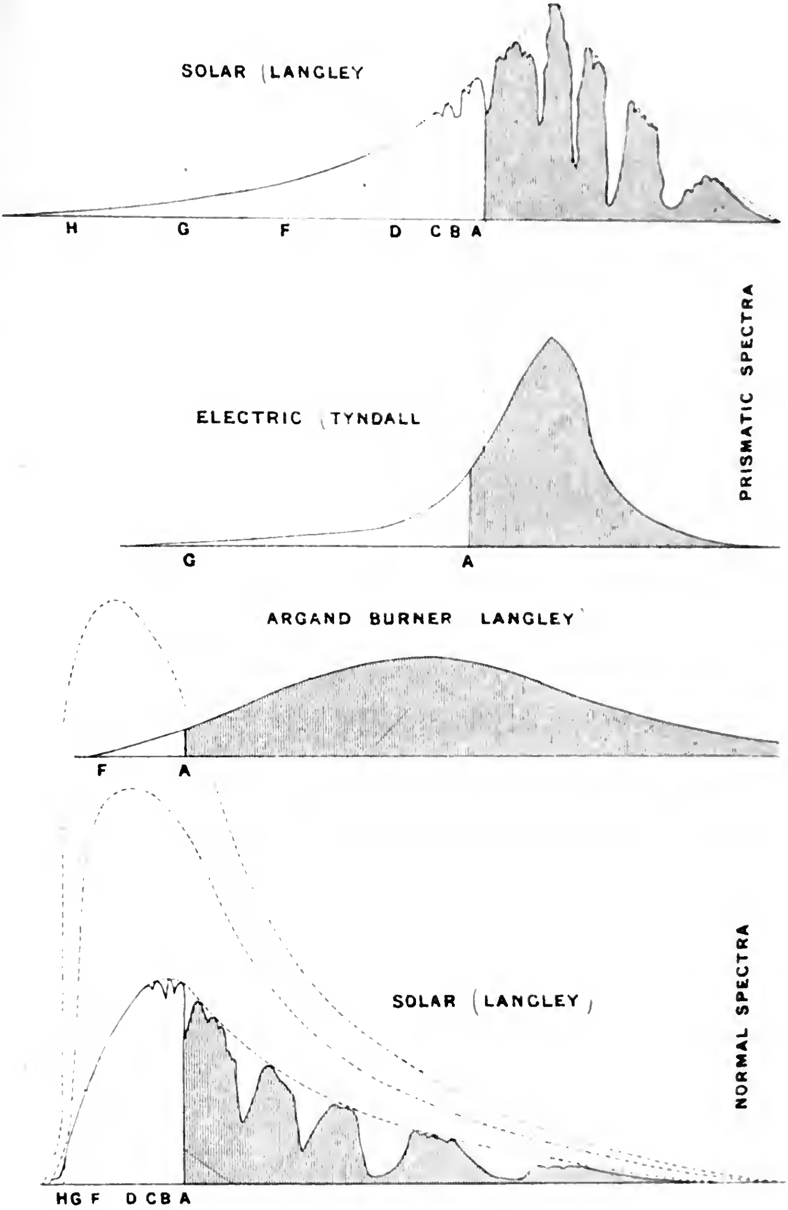
Scale 1/48th



DEPENDENCE OF RADIATION IN TEMPERATURE.

(Excerpt. Proc. R. S. Vol. XXXV.)

QUESTIONS INVOLVED IN SOLAR PHYSICS.



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