Discussion.

Sir John Coode.

Sir JOHN COODE, President, said that at the end of last year Professor Elihu Thomson, the inventor of the process, had been asked to communicate a description of the system of electric welding, but owing to numerous engagements he was unable to prepare a Paper for the Institution, and thereupon Sir Frederick Bramwell had kindly undertaken to do so; he had taken an immense amount of trouble in the fulfilment of his task, and deserved the hearty thanks of the members. He would now ask Sir Frederick to give some practical illustrations showing the process in operation; and he could not help thinking that the members would be as much struck as he himself had been with the marvellous simplicity of the process. The heating of the carbons of the arc lamp had been before the world about a quarter of a century; the two carbons had been brought close together in a state of fusion, and it was remarkable that no one had before thought of putting two metals together in the same way and giving them a squeeze; for that was what, put in the simplest terms, the process amounted to.

Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL observed that as the President had already explained, the Paper had been written at his invitation; if, therefore, the time of the Institution had been occupied by an uninteresting subject the President only was to blame.

The process of welding, as ordinarily understood, depended upon a property possessed by the so-called weldable metals, which was, that while their surfaces could be brought into a sufficient degree of plasticity to form a union, their bodies were nevertheless still left sufficiently tough to bear the stress of hammering or pressure. Dr. Percy had explained the matter in the clearest possible manner, with regard to the typical weldable metals, and in speaking of non-weldable metals, he had shown what was needed to enable them to be united by fusion.[1] It would be

[1] "Metallurgy." By JOHN PERCY. "Iron and Steel." Edition 1864, p. 5. *Action of heat. Welding.*—Iron requires a very high temperature for its fusion. Its melting point has not yet been determined with certainty, but has been estimated at 1550° C. by Pouillet, which, however, is questionable. We have no difficulty in fusing it perfectly in our assay-furnaces in which platinum remains infusible. Iron has one remarkable and very important property, namely, that of continuing soft and more or less pasty through a considerable range of

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seen that as regarded the so-called non-weldable metals, Dr. Percy had pointed to the difficulty of hitting upon that which might be called the critical temperature of such metals; and also had alluded to the further difficulty that even if it could be hit upon, there was no certainty of maintaining it during the time needed for the operation. These two obstacles to the welding of the so-called non-weldable metals were, however, absolutely overcome by electric heating, because any temperature could be obtained within the power of the machine, and it could be maintained any length of time without change. He had placed upon the wall some illustrations of tire welding, which went back to a state of things long passed away. The mode in which railway tires were welded was shown, and there was also an illustration of the mode (not so well known) in which lengths of gun-tube were welded together. He had always looked upon this last as one of the best instances of welding, an instance where care was taken to make the surfaces clean at the outset and to prevent them from being injured by the access of dirt, and thus to obtain in a clean manner the welding heat and then to effect the union by endway-pressure. He had alluded to Mr. Wilde's Paper, which excited so much attention twenty-four years ago, when he showed how a dynamo excited by another dynamo could perform that which was then the marvellous feat of fusing a piece of wire 15 inches long and $\frac{1}{4}$ inch in diameter. Mr. Wilde was one of those whom the world did not sufficiently remember, as having done a very great deal towards the perfection of the electrical implements which were now so largely used. He did not know whether there was any need to enforce it, but he had said that for welding purposes the question of heating was in a sense irrespective of the electric pressure. No doubt it was the lowering of the pressure, the destruction of it, if it might be so called, which did away with energy in the electrical form and caused it to be reproduced in the form

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temperature below its melting point. It is sufficiently soft at a bright red-heat to admit of being forged with facility, as every one knows; and, at about a white-heat, it is so pasty that when two pieces at this temperature are pressed together they unite intimately and firmly. This is what occurs in the common process of *welding*. Generally metals seem to pass *quickly* from the solid to the liquid state, and so far from being pasty and cohesive at the temperature of incipient fusion, they are extremely brittle and in some cases easily pulverizable. But, admitting that there is a particular temperature at which a metal becomes pasty, its range is so limited in the case of the common metals, that it would scarcely be possible to hit upon it with any certainty in practice; or, if it were possible, its duration would be too short for the performance of the necessary manipulations in welding.

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of heat; but whether that lowering took place from 99 volts to 66, or from 66 to 33, or from 33 to zero, the effect was the same. Dr. Hopkinson, who was one of those who had devoted great attention to the subject of the change in the electrical conductivity of metals under variations in temperature, had been kind enough to allow him to put upon the wall enlargements of the diagrams of some of his experiments on this increase in resistance (*Fig. 11*). It would be observed that, whereas at 32° Fahrenheit, there was a resistance of 1, at 1823° Fahrenheit the resistance had increased to $8\frac{1}{4}$; and, no doubt, if the observation had been carried on to the temperature attained in the experiments just performed before the meeting, the resistance would have been much greater. The Paper to which he had referred was to be found in the Philosophical Transactions for 1889, and it contained a vast variety of most important matter which he recommended to the attention of members interested in these subjects. He had mentioned that it was impossible to state the exact number of amperes needed for a particular temperature. The fact was that the heat was cumulative. He was now about to show not a weld, but a heating up of a bar put in between the jaws, and heated by the passage of a comparatively low current, which would be continued until a temperature was reached at which there appeared to be a balance between the heat generated and the losses, so that the temperature was maintained. If round the heated bar there were then placed an incombustible envelope, and if it were kept there for some little time, and then withdrawn, it would be found that the temperature had enormously increased. He would instance such an experiment as proving that it was impossible to say that any particular number of amperes were needed to produce any particular temperature, and as establishing that the number of amperes depended upon other circumstances as well as on the desired temperature. With reference to the statement in his Paper that it was desirable that the large currents should be produced close to the machine, he wished to call attention to a piece of conductor of the same size as that which was bringing the electricity from the dynamo a few yards away. He would ask the members to compare its diminutive section with that of the conductors of the machine itself conveying the large current of low electromotive force. It would be seen that these were two bars of copper about 7 inches deep, and 2 inches or $2\frac{1}{2}$ inches thick. They were the conductors which were needed for the transmission, without too much heating of this extremely large current; while the current from the dynamo, before it reached the trans-

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former, was brought in by conductors, which were mere wires. The machine in which the large welding had been carried on had been so fully described in the Paper that he need not further refer to it; but there was on the table a machine which had not been so fully described. It was used for the welding of small wires, and matters of that sort. In the box, forming the stand, there was a transformer, and there were jaws to hold the piece under operation; the two jaws were urged to approach by a spring, and in this way was obtained the endway pressure which performed the welding. There was an implement for the purpose of insuring that according to the nature and dimensions of the metal it should have when fixed in the jaws a proper projection. When the switch with which the apparatus was provided was drawn into the position shown, the current was on; but as soon as the jaws had moved sufficiently near together to "upset" the softened material, they struck a detent, allowing the switch to move automatically, and to cut off the current. Reverting to the iron welds he wished to call attention to the results, shown on the table, of the bending cold of the bars that had been electrically welded. They were certainly below those of the hand-welded specimens to the extent of about one-half; while when bent hot the results of the two modes of welding were practically the same. He was referring to diagram, *Figs. 26*. He could only say that he wished he had had the wit to have the welds annealed, because he had no doubt that would have brought the cold bending to an equality. He would now show, with two pieces of Farnley iron, a weld of the character of those of which eighty had been made, as mentioned in the Paper. The next experiment would be with two pieces of wire cable. In order to obtain the results just shown, an engine of Marshall's which indicated 50 HP., a large dynamo and a small one, had been erected at the back of the meeting room. This had involved, of course, a great deal of preparation; but it would not be all bestowed on this single meeting, as he had the permission of the President to say that the apparatus would be allowed to remain during the week, and that on each day it would be in operation between the hours of two and four o'clock.

Mr. Preece.

Mr. W. H. Preece observed that during the past twelve years he had devoted so much attention to the connection existing between the passage of electricity through matter and the generation of heat therefrom, that he was sure the Institution would pardon his giving to some extent the substance of numerous papers on the subject which he had submitted to the Royal Society. It was desirable, in the first instance, to point out that the trans-

Mr. Preece. ference of what was called electricity through matter, whether gas, liquid or solid, was always accompanied by an expenditure of work. If a spark passed through a gas, the gas was dissociated into its constituent atoms; if a current passed through a liquid, the liquid also had its molecules broken up into their constituent atoms; and if electricity passed through a solid, inasmuch as there was nothing there to break up, the electricity generated a certain motion of the molecules which gave what was called heat. It was nearly half a century ago, in 1841, that Joule—one of the greatest experimenters and one of the most wonderful men that the century had produced—developed the law that determined the generation of heat, and that law had been proved ever since to be actually true, and it underlay the fundamental facts of the system of electric welding and of all practical applications of heat derived from electricity. But Joule's law, that the heat developed per second varied directly as the square of the current and as the resistance of the conductor, or $H = C^2R$, assumed that the conductor conveying the electricity was surrounded by something that was not itself a conductor of heat, and that all the heat generated in the conductor was utilized, or, as some said, wasted, in the conductor. Another great genius, happily still living, Sir William Thomson, about ten years afterwards said that the development of heat in a conductor could not possibly be taken into consideration without also considering the effect of the dissipation of heat from the surface of the conductor; that whenever a conductor conveyed a current of electricity there was not only the generation of heat in the conductor but a radiation and convection, or, as it was called by the more general term, the emissivity of heat from the surface; and that when the heat generated in the conductor was equivalent to the heat emitted from the surface, the temperature of the wire became constant. He wished to point that out because in every heat question it was essential to draw a broad distinction between the heat generated and the temperature of the body that conveyed the heat. His attention to the subject began with the improvement of lightning protectors to preserve sub-marine cables, which were based upon the notion that a very fine wire, say of platinum, would be fused by a lightning flash before it was possible that the strength of the current derived from the lightning flash could injure the cable; and therefore it was desirable to find the dimensions of the fusible conductor which could not under any circumstances be fused by such currents as were used for telegraphy, but would be fused the moment the current acquired such strength that it might injure the cable. This was

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the principle of "cut-out" so largely used in electric lighting. In carrying out that inquiry he showed that the law that determined the fusing of a wire bare and exposed in air (a condition assumed in all inquiries and in all papers written on the subject) simply depended upon the strength of the current that flowed, or rather the quantity of electricity that passed. It followed a distinct law based upon the diameter of the wire, which was expressed thus:— $C = a\,d^{\frac{3}{2}}$; C being the current, a a constant called the fusing constant, and d the diameter. In fact, by determining by experiment the current that would fuse a wire of any material an inch in diameter, the current that would fuse any other wire could be determined by simply taking the square root of the cube of the diameter. An iron rod, an inch in diameter, through which a current flowed, would certainly be fused when the current reached 3,148 amperes. There was nothing in nature more certain than this; and to ascertain how much current would fuse a wire 2 inches in diameter it was only needful to take the cube of 2, and the square root of that, which was 2·8. That rule had been shown to be absolutely exact. Based on that rule he had made a series of very careful experiments from which had been determined what were called the fusing constants of metals. Assuming that a rod of iron 1 inch in diameter would fuse with 3,148 amperes, 1 inch of copper would fuse with 10,244 amperes, and the other metals in proportion.

Fusing Constants.

——	Diameter in Inches.	——	Diameter in Inches.
	Amperes.		Amperes.
Copper	10,244	Platinoid	4,750
Silver	7,688	Iron	3,148
Aluminium . . .	7,585	Tin	1,642
German Silver . .	5,230	Lead	1,379
Platinum	5,172	Alloy (Lead 2 parts, Tin 1 part) . . .	1,318

The point which he wished to urge was that those who argued on Joule's law alone argued on a false assumption, and that it was necessary to take into consideration the question of the surface exposed to radiation and convection, and the environment of the conductor. Mr. J. T. Bottomley, Professor Dewar and others had

Mr. Preece. lately worked in that field, and they were now getting clearly to understand the relation existing between surface temperature, and emissivity. But the curious fact had come out in his enquiries, that in some circumstances the question of time might be altogether neglected. A wire conveying a current took time to acquire a constant temperature, and the time it required depended upon its dimensions. A wire so small that it might be called hair-like cooled and heated with such rapidity, that it actually emitted sounds; and a thermo-telephone, which he had submitted to the Royal Society in 1878, reproduced the voice by the rapid heating and cooling of a fine platinum wire. The law was so exact, and it gave such concordant and satisfactory results, that there was now not the slightest difficulty in determining the temperature that any wire could attain from any current; and *vice versâ*, by measuring the temperature which a round cylinder acquired when carrying electricity, the current could be ascertained. But like all laws, that one was liable to be broken, and certain departures had to be taken into consideration. It was, of course, scarcely applicable to the apparatus exhibited. One of the most serious allowances that had to be made was the cooling effect of the ends. In the apparatus there were short lengths of iron bars, probably about 3 inches long, inserted between heavy jaws, the ends being attached to immense masses of metal. The result was that those masses of metal had to acquire heat, and they reduced the temperature of the end, so that instead of a bar of iron taking about 3,000 amperes to fuse it, the probability was that it would take nearer 10,000. On the other hand, if instead of being 3 inches the bar of iron were 3 feet long, it would probably be found that in the centre of that bar fusion would be effected by about 3,000 amperes. It would be noticed, in watching any experiments that might be made, that the bright heat was in the centre, and the black part of the bar was at the end. Again, allowance had to be made for the influence of the environment of the wire. Many engineers were apt to consider the effect of lagging in steam-pipes, and there were certain forms of lagging that maintained the temperature of the steam pretty constant, and reduced the waste heat very greatly. On the other hand, certain forms of lagging had exactly the opposite effect. It was a curious thing that if the environment of the wire had a deleterious influence on conductors, heating would be seriously felt in submarine telegraphs. Submarine telegraphs conveyed currents for a long time, while heavy street mains conveyed powerful currents for electric lighting purposes, and if heat had an accumulative effect in conductors surrounded by insulators, sooner or later damage might be occasioned; but as a matter of fact, the presence of gutta-percha, india-rubber, asphalt, and nearly all materials used for insulating conductors had the reverse effect, for they tended to cool the wire. In 1882 he was engaged with Mr. Robert Sabine in some interesting experiments, which were unfortunately brought to an end by that gentleman's untimely death. It was found that the emissivity of an asphalt surface was seven times that of a copper surface, and india-rubber and gutta-percha were greatly in excess of bright copper. The result was that, with most of the materials that were used to insulate the current, the wire was cooled. There were some things in which the reverse effect occurred, one of them being asbestos, the emissivity of which was very small. It was an insulator of heat; and one of the prettiest experiments that the Author had shown was that of covering a glowing wire with a sheet of asbestos, the result of which was that the emissivity was checked, the temperature rose, and the wire was fused. There was a departure from that law, and a curious one, with which electricians were not yet thoroughly acquainted, but they were becoming more familiar with it every day, namely, the practical effect of alternate currents of great frequency, such as were being used at Deptford to be transmitted to London for electric lighting purposes, and which formed the basis of nearly all the great systems of electric lighting, not only in America, but in England. It was a remarkable fact that when alternate currents of electricity were transmitted through iron wires, there was not only the heat due to the passage of the currents of electricity, but there was a kind of churning action in the iron itself due to the incessant rapid reversals of magnetism. It was a kind of viscous friction amongst the molecules of iron, and heat was generated, which varied almost directly with the number of the alternations of the current per second, and also with the strength of the current. The result was that with alternate currents the generation of heat was greater than with continuous currents. A careful experiment showed that an iron wire which fused with a steady current of 122 amperes was melted with 92 amperes when the current was alternating. That effect of alternate currents on wires had received the rather awkward, but now well-known, name of hysteresis. It represented the work done on iron molecules by the reversals of magnetization. So far as present knowledge was concerned, the practical application of these laws had been most charmingly shown by the experiments the meeting had witnessed. There were one or two

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Mr. Preece. points which the Author had not made as much of as he might have done, and which were worthy of careful consideration. When iron was heated first for welding purposes, it was raised to a very high temperature indeed, probably 3,000° Fahrenheit or more, and was thus heated beyond the point where oxidation set in. In the case of steel, the blue, yellow, and orange points of oxidation were very fairly indicated, but he did not know that any one had absolutely measured the temperature of iron or steel at which scaling commenced. Whatever that temperature was, it was a point to be avoided; but in welding by hand it was not avoided. There was a fixed fiducial point in heating, the point where objects first became visible. The first appearance of red light was a marked fiducial point, like the freezing or boiling point of water, and it had been determined by many physicists to be 525° Centigrade, or 970° Fahrenheit for all substances. In welding by electricity the metal reached that point, and then gradually, as the current increased, became plastic; it got beyond the state of viscosity, it virtually became liquid, the molecules were mixed together, and before reaching the point of scaling the very temperature was reached which secured the weld. He might be permitted to give an illustration of a perfect weld. He would take a tube in which there was a cylinder of ice; if the cylinder were drawn out and broken, and he desired to weld it, he would put it back in the tube and melt the broken bar of ice; the molecules of water would then rush about amongst themselves, and become one homogeneous mass. Then, by freezing the bar again, an absolutely perfect weld would result. Very much the same thing happened in welding a rod of metal by a current of electricity. The metal was brought to a condition of fluidity, where all the molecules rushed together and became one homogeneous whole; it was then allowed to cool in air, and the result was an absolutely perfect weld. The subject of electric welding was not altogether novel. It had long been employed in the works of Messrs. Siemens, where he had seen it many years ago; indeed, one of the first to work in that direction was the late Sir William Siemens. Mr. Alexander Siemens was present, and perhaps he might be able to state something about the work which was carried on. Electricians were obliged to keep their eyes as well as their ears open since the advances in electricity were so great, and he hardly knew where they were going to stop. Electric welding had been introduced in Russia, and had attracted great attention there. He referred to the Bernardos system, in an account of which he had read that it was possible to weld iron-plates under water; that if a leak, or a crack, or an accident took place to a plate of an iron ship under water it was possible to fuse it even there. Mr. Preece.

Mr. ALEXANDER SIEMENS said that, as Mr. Preece had mentioned, there had been some practical applications of electric welding at the works with which he was connected. It was found that much time was lost in welding the wires in the ordinary way during the sheathing of cables, and in 1881 a special apparatus was constructed, in which the welding heat was produced by a continuous current of electricity. The machine used at that time was small, the current was 60 amperes and 20 volts, but occasionally during the welding it increased to 100 amperes. In the circuit of the machine a variable resistance was fitted. The firm did not weld butt-ends, but made scarf-joints, so as to be quite sure of making a strong joint. On testing afterwards it was found that the wire broke as often in another place as in the weld. The sizes of the wires were 0·073, 0·11, and 0·131 inch. About the same time —in April, 1881—a proposal was made by Mr. Atkinson, who wanted to tin the iron plates of ships as a protection against the action of sea-water. He employed a carbon handle, to which one pole of the dynamo was fastened, the other pole being attached to the iron body of the ship. Bringing a piece of tin into the arc, so to speak, he first fixed little patches of tin on the iron plate, and then he attached the tin plates. There was an application of the same sort of welding in the electric tramway on Ryde Pier. The rails on which the tramcars ran were used as conductors; the ordinary fish rails were not a good enough electrical connection, and therefore the neighbouring rails were attached to each other by a piece of iron bent in a U shape. Mr. Siemens.

Mr. R. E. CROMPTON remarked that those who had seen the apparatus shown at the Paris Exhibition must have been greatly struck with the way in which every detail had been worked out, and must have highly appreciated the mechanical skill and inventive power of Professor Thomson. The Paper and discussion so far as it had gone might convey the belief that only the alternating current could be used for attaining the great heat necessary for this mode of welding. Mr. Preece, in particular, seemed to think that the hysteresis of iron added to the heating effects, but Mr. Crompton did not agree with him. He had made such welds on the previous day, using continuous currents supplied from a few large accumulators; he took pains to measure the current and found it much higher, possibly tenfold the amount stated by Mr. Preece. As the heating was accomplished so very rapidly, the Mr. Crompton.

Mr. Crompton. question of emissivity did not much affect the matter. He believed it was of great importance that the intense heat commenced at the point where the cross-section of the circuit was smallest, namely, where the two rounded surfaces of the ends of the bars to be welded were forced into contact. The welding thus commenced at the centre of the bar, and then spread outwards. This conduced to making the welding sound; for as contact commenced in the centre and the welding commenced also in the centre, the scale which must always be formed whenever iron was heated to such a temperature was forced outwards as the weld took place. Those who had not attempted electric welding had little idea of the great difference in welding metals other than iron and steel. These two last were weldable metals in the true sense of the word, that was, they attained a pasty condition before fusing, whereas the non-weldable metals lost all cohesion just at the fusible point. Mr. Preece had made a mistake in comparing the regelation of the ends of two cylinders of ice with the welding of iron. The phenomenon of the freezing together of the two cylinders of ice was of the same class as the fusing together of two non-weldable metals; a broad distinction should be drawn between it and the phenomenon of welding.

Sir Frederick Abel. Sir Frederick Abel wished to add a word in confirmation of what Mr. Crompton had stated. He thought the action was extremely simple. There was no doubt that a scaling or oxidation of the metal must take place. It was not a question of temperature; it began at a comparatively low temperature, but as the temperature increased the tendency to the fusion of the scale increased, and if welding began at the centre the fused scale would be squeezed from between the surfaces by the two first portions that came together, and as the pressure was continued, and the surfaces were brought together, the scale would be squeezed out, and a perfect weld obtained. When Mr. Preece referred to Joule, Sir Frederick Abel wondered whether he was going to remind the Institution of Civil Engineers that Joule actually predicted in detail many years ago the practical application of electricity to welding purposes. He had given an interesting description of the way in which electric welding ought to be carried out, and the way in which it had been carried out.[1]

Mr. Mordey. Mr. W. M. Mordey said that he wished to be allowed to point

[1] Memoirs of the Literary and Philosophical Society of Manchester. 2nd Series. Vol. xiv. (1857), p. 49. "On the Fusion of Metals by Voltaic Electricity."

Mr. Mordey.

out some of the differences between direct currents and alternate currents for welding. When Professor Elihu Thomson first published his account of electric welding eighteen months or two years ago, his dynamo was a direct-current machine. It was interesting to note that he had abandoned the use of that class of machines and now employed an alternate-current machine to get the large currents that he required. The first apparatus that he used was a low-tension direct-current dynamo, and the welder was placed immediately over the machine. He now was able to adopt the much more convenient plan of having the engine and dynamo at a distance from the place where the welding was done. With low tension direct working that was not possible, even separation of a few feet being objectionable on account of the enormous conductors necessary. By using alternate currents quite a small conductor sufficed to convey the high-tension primary current to the welder, where, by means of a transformer, it was conveniently changed to a very low tension current. There were several advantages in connection with the use of alternate currents. He did not agree with Mr. Crompton as to the advantages of using a direct current for the purpose. Except for very small welds he thought that a direct current was not anything like as useful as an alternate current. Mr. Preece had said that magnetic hysteresis caused the heating of the iron, but he thought this was not exactly the case; in the case of welding copper and other non-magnetic metals, hysteresis had not to be considered at all, and even with iron the metal soon reached the temperature at which it was non-magnetic, and therefore not subject to hysteresis. Besides hysteresis, alternate currents set up an action which was absent with direct currents. Hysteresis was a magnetic molecular effect; it was molecular friction accompanying the change of magnetic condition. The other, or as it had been called, virtual resistance effect was due to the induction of the current on itself, and the result was a tendency to drive the current to the surface of the conductor, and to cause the conductor to oppose a higher resistance to alternating than to direct currents. Alternating currents in passing through conductors were denser near the surface than in the inner portion, and with large conductors and rapid alternations, the inner portions were scarcely traversed at all by the current. This effect, which was not understood a few years ago, was the explanation of some of the difficulties experienced in electric lighting, when it had been attempted to work with large alternating currents. He suggested that it would very probably have a bearing on electric welding and could be made use of to advantage, as a large conductor

Mr. Mordey. could, he thought, be more readily heated by an alternating current than by a direct current. The former would act first locally on the outer parts of such a conductor, and, on these becoming heated, the increased temperature would cause the current to pass inward. He had worked out some cases from formulas given by Lord Rayleigh in 1886, in the "Philosophical Magazine,"[1] and more recently by Sir William Thomson.[2] In the case under consideration, if the alternator gave about 250 or 260 alternations per second, which was a very usual rate in America, there was a virtual resistance effect set up which made the resistance of the conductor much greater than it would be in the case of a continuous current. That circumstance was attended by advantages and also by disadvantages. The advantages were in the actual welding, the disadvantages in the secondary conductors of the transformer and in the connections; but as the dynamo gave a small current only, that effect was not felt there. With a bar ½ inch in diameter, with the periodicity mentioned, the resistance was practically the same for a steady current and for an alternate current. Mr. Crompton could weld with a direct current as well up to that size as with an alternate current, but he could not as conveniently get a current generated and conveyed to the welder. At ¾ inch in diameter the resistance increased 7½ per cent. At 3 inches in diameter the resistance increased four times, so that with a conductor 3 inches in diameter a tube with ½ inch radial depth was practically as good a conductor for carrying an alternate current of that periodicity as a solid cylinder. Reference had been made to the use of rounded ends. If they were used they probably served a good purpose in forcing out the oxide and getting a sound weld throughout. If flat surfaces were put together the current started on the outer side, and it was not until the outside was rendered nearly red-hot that any current got into the inside. It was a mistake to suppose that the heat started from the middle. There was no heat at all in the middle. Probably practical welders present would tell them that to heat up a solid conductor of 3 or 4 inches diameter to a given temperature on the outside would not require a much longer time than to heat a tube 3 or 4 inches in diameter and about ½ inch radial depth. It was not until the outside of a solid conductor got hot, and so increased the resistance, that the alternate current was driven in at all. Under ordinary conditions for an electric conductor, where the rise of

[1] The Philosophical Magazine. 5th Series. Vol. xxi. (1886), p. 381.

[2] Journal of the Institution of Electrical Engineers, 1889. Vol. xviii. p. 16.

temperature was not marked, there was no current through the inside, and it was as well to bore it out and to use a tube. The difficulty in connection with the construction of transformers might be got over in one or two ways. In the transformer represented, he observed that a very large section of conductor was used (*Fig. 13*) for the secondary. It certainly was a tube, and it struck him that it was not a particularly good form of secondary conductor. A better effect would be obtained by making the secondary conductor of stranded wires insulated from one another, and electrically welding or soldering the ends on to the jaws of the welder. Those who had seen the machinery at the back of the building might be surprised at the large size of the plant, but he supposed the Author would state that it was not necessary to have such large plant in order to weld pieces of iron or other metal of the size shown. As the welding took only a minute or a minute and a half, the best way would probably be to have a small engine with a large heavy fly-wheel, which would act as an accumulator, the power being given out by the fly-wheel, with the assistance of the engine at the time the weld was made. In that way the boiler and engine would be cheaper and the efficiency higher. He was glad that Mr. Preece had referred to the Benardos process, which was brought out about the same time as the process developed by Professor Thomson. That process, he believed, had some advantages for small plates of sheet metal, &c., which he thought might perhaps be welded more easily and satisfactorily by that method than by a process which required considerable pressure. Mr. Mordey.

Mr. H. Howard, of the firm of Lloyd and Lloyd, stated that he had seen the Benardos process at work in Russia in the making of casks of thin steel sheets 3 millimetres (0·118 inch) thick. The ends of the casks were welded together. They were about 1 metre (3·281 feet) in diameter, and 1½ metre (4·921 feet) in height. He had seen one very remarkable piece of welding. A man was working inside one of the casks, the shell of which was 2 millimetres (0·079 inch) thick, with one of the Benardos carbons, welding on the man-hole flange of ½ inch steel. It was an exceedingly difficult operation, and he doubted whether it could be accomplished by any other process. He subsequently saw a large number of ends welded into wrought-iron cylinders, 9 inches in diameter and about 2 millimetres (0·079 inch) thick. They were welded at the rate of more than one per minute, and the weld was an exceedingly good one, the material being uninjured by the process. He had put one of the cylinders on the ground and driven in Mr. Howard.

Mr. Howard. the end with a sledge-hammer, the result being that it broke through the solid material, and the weld was not affected. He had submitted one of the cylinders to Mr. Kirkaldy, who at the weld obtained a tensile strain within a fraction of 92 per cent. of that of the solid metal. He did not think that the two processes should be regarded as rivals. What one could not do the other could, and he believed there was ample room for both.

Professor Kennedy. Professor A. B. W. KENNEDY said that he had lately spent a few weeks in examining the working of the process in America, and would give a few particulars as to what he had seen. In most of his experiments a machine of the type shown in *Fig. 18* was used, a 60-unit machine having an electromotive force of 300 volts, its rate of alternation being 50 double alternations per second, and its speed about 1,000 revolutions per minute. The engineers who had charge of the works at Lynn, near Boston, where most of the machines were made, had fixed by experiment a standard time during which it was convenient that the operation should last, and also a standard projection beyond the edges of the copper clips for the pieces to be welded. The projection which they made in the case of a round bar was equal to the diameter, so that for a bar 1 inch in diameter there was a clear space of 2 inches between the clips. The standard time was forty seconds for a 1-inch bar, and otherwise in proportion to the diameter. Whatever the duration of the current and the diameter of the bar, within certain limits it might be expected that the actual energy expended would be reasonably proportional to the cube of the diameter, speaking only of round bars. He did not, in his own experiments, measure the indicated HP., but he measured directly the current going to the transformer (using a watt-meter which he had previously calibrated), and he obtained the following figures. For a 1-inch bar the weld took 328,000 foot-lbs. of energy, for a $\frac{3}{4}$-inch bar 143,000 foot-lbs., for a $\frac{1}{2}$-inch bar 43,000. These quantities were very nearly in proportion to the cube of the diameter. With steel the figures were nearly the same. As the length of the part heated was made to vary as the diameter, and as the time was also made to vary as the diameter, the HP. ought to vary as the square of the diameter, and the HP. per square inch ought to be constant. He found that the HP. per square inch for a 1-inch bar was 20·0, $\frac{3}{4}$-inch 19·3, $\frac{1}{2}$-inch 20·4; in steel, 1-inch 23·0, $\frac{3}{4}$-inch 20·5, and $\frac{1}{2}$-inch 19·6. That was, as nearly as he could measure it, the mean electrical HP. going into the transformer per square inch of cross section of weld, during the actual time when the current was turned on, and he had taken tolerably

Prof. Kennedy.

frequent readings during those few seconds. In another type of machine, the one used for copper, he found that the HP. was considerably less. At the standard time for a 2-inch bar it was only 14·6 per square inch. He had found in the case of copper a discrepancy which he would not then attempt to account for, namely that the net electrical HP. was nearly constant, although the time varied considerably. He made the time vary from twenty-one to thirty seconds, but the net electrical HP. per square inch of copper was in every case almost exactly 45. The actual energy varied directly as the time, as nearly as he could measure it. At one of the largest bicycle factories in the United States, at Hartford, Connecticut, he found a welding machine being used for brazing the tubular limbs into the Y-shaped fork of the front wheel of the bicycle. The fork was a nickel steel casting, into which the two tubes were to be brazed. Each tube was put into its socket, and two or three turns of fine brass wire were wound round it. The tube was held in one clamp and the socket in the other, and the current passed through and melted the wire. No pressure was exerted, but the brazing found its way into the joints, and a very excellent brazed joint was made. The managing director of the works had told him that he liked the process so much that he proposed to alter the designs of the bicycles in order that as many parts as possible might be brazed in that manner. The great cleanness of the braze was very noticeable, the borax glaze outside being not so much spread as in the case of a number of hand brazes which he saw at the same works; and the time occupied in brazing was not more than one-half that required with a gas furnace. This machine had made about 30,000 brazes in nine months. Professor Thomson, and the engineers who had worked with him, had made a great speciality in the matter of welding (galvanized) telegraphic wire, and also copper and brass wire. At one of the largest telegraph-wire works, that of Messrs. Cooper, Hewitt & Company, Mr. Spilsbury had told him that it was his duty to send in from time to time to the Postal authorities three lengths of 50 feet each of wire for testing conductivity. On one occasion he had made one of these 50 feet lengths out of 50 separate feet welded together into one length, and had sent the three to be tested without saying anything about this. All three lengths passed the test equally well! One thing that had surprised him was that a brass wire, welded in this fashion, with a mere butt weld, should stand drawing without the least harm. He had taken five little pieces of wire 4 inches long, and had them welded into one length and had the burrs removed; they were at once carried to the drawing plate and

Prof. Kennedy. passed in his presence six times through (the wire was of 0·125 inch diameter to begin with, and finished with a diameter of 0·03 inch), and there was not the slightest trace of the existence of the four welds. At the works of Messrs. Roebling, Sons & Company, at Trenton, he found that it was a habit to piece up all kinds of odds and ends of old wire; and a welder which was going apparently day and night had made at the time of his visit about 194,000 welds. He had found with some interest that wagons in America possess such a thing as a "fifth wheel"! It was the circular ring serving as a guide for the bogie frame of the front axle, called a perch plate. At the works of the New York Fifth Wheel Company these rings were made of channel section, about $2\frac{1}{2}$ inches by 1 inch, and $\frac{3}{8}$ inch thick. They were bent round cold to a slightly larger diameter than was intended, so that they made a complete ring. The two ends were held in clamps, the current was passed through, and they were simply pushed together during the process as in the case of straight bars, and as far as he could see the weld was as perfect as any of the others. From the time of putting in to the time of taking out, including some hammering, about four minutes elapsed; then the burr had to be ground off on an emery wheel, which took three minutes more, so that the whole operation took about seven minutes. The smith told him that in welding the rings as he had formerly done he had first to flatten down the two sides and open them out, then scarf-weld them, and then hammer up the edges again, which occupied thirty-five or forty minutes; so that the saving in time was very great, and there was certainly no comparison between the quality of welds after they were made. Whether the process would be applied to the welding up of angle-iron frames, or other parts of constructive ironwork, was perhaps questionable; but it was obviously a process by which such welding was possible, and engineers would be glad to get welded angle-iron frames instead of the built-up frames which they were now compelled to use, and which had all kinds of weak places round them. The welding of chains, he believed, had not been carried very far, but what he had seen was so satisfactory as to make him hope that the electric welding process would be used for that purpose. He believed that the idea was not to use two half links shaped like U's entirely separate and then to put them together and weld them (although he had had some links welded in that way very well), but to make the links in a C shape, so that there was only one opening, and to hold the two bends of the C in the clamps. When the current was put on, the resistance on the open side increased as it heated, so that the other side heated up also. Thus by the time the current Prof. Kennedy. had been kept on long enough to weld the one side, the other side was hot enough to upset, and when the pressure had been put on, the link looked as if it had been welded on both sides. The Author had alluded to the welding of wire cables. This he had seen carried out several times, and had brought some specimens back with him. Of course, the effect of the welding in such a case was simply to make a short piece of cable (perhaps equal in length to two diameters) perfectly solid. In the case he had tested, the weld was about 78 per cent. of the strength of the actual unwelded cable, which was an enormous percentage for any wire cable, and for a cable of the interlocked type it was specially satisfactory.

Mr. Henry Davey regretted that he only had a superficial Mr. Davey. knowledge of the subject of electricity. In the text-books he

Fig. 29. *Fig. 30.*

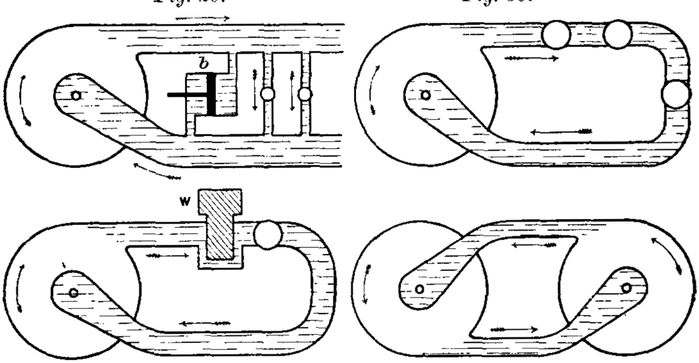

Fig. 31. *Fig. 32.*

found a constant reference to hydraulic analogy; and the statement was made that a current of electricity behaved in much the same way as a current of water. The books, however, did not go much into detail, but only dealt with the subject in a very general way. In speaking of a hydraulic engine, the first engine that naturally occurred was that of reciprocating machinery. He thought, however, that if attention was directed to hydraulic analogy in a more complete way, and machines were sought out which had a more perfect analogy, it would be of more assistance. By a little ingenuity there could be got from the centrifugal pump and the turbine almost a complete analogy as to the behaviour of electrical currents, taking the centrifugal

Mr. Davey. pump as the dynamo, and the turbine as the motor. He had in a diagram which he had placed on the wall, a centrifugal pump, which was analogous to a self-regulating dynamo. A self-regulating dynamo had a shunt, and it could be employed to supply lamps in parallel. If another lamp was added there was a greater demand for the current, and the self-regulating dynamo supplied that current. To complete the analogy with the centrifugal pump he would put in as a shunt a hydraulic cylinder (*Fig. 29*), with a piston, the rod of which was attached to a mechanism which would expand or contract the mouth of the pump. Immediately the pressure was increased on one side and diminished on the other momentarily, the shunt came into action; it opened the mouth of the pump, and the increase of the engine-power gave the increased current. That, he thought, was a more complete analogy than he had seen in the text-books. *Fig. 30* illustrated a series dynamo, and *Fig. 31* a series dynamo supplying an accumulator. Supposing the speed of the centrifugal pump (*Fig. 31*) to be reduced below what was necessary to keep up the pressure in the accumulator there was a reversed polarity. The last diagram (*Fig. 32*) showed the centrifugal pump giving motion to a turbine, or the coupling of a dynamo to a motor.

Mr. Matheson. Mr. EWING MATHESON observed that the Author had spoken of putting the pieces of metal to be welded a second time into the machine, and it appeared that the time occupied by the second process had a considerable effect in regard to the total result as compared with hand-welding. He had seen a great many pieces welded, but it was very seldom that they were put a second time into the machine. It was not essential to the process of welding. If it was required to re-heat them for any manipulation, they could be re-heated at will; but it was not necessary for the primary purposes of welding. If, therefore, the time occupied for the second operation were deducted from the total amount, which was given as one hundred and thirty-five seconds, the comparison with hand-welding would be considerably altered. Taking the gross power while the current was on at 33, and the average power when the engine was running light and produced no current, at 9, instead of giving an average of $23\frac{1}{2}$ HP., only 18 HP. would be required if the piece was only once heated. That showed very considerable difference in comparing the cost and the time occupied between hand-welding and electric-welding. Several references had been made to the difference between the hand-welding of iron and electric-welding. Both had their advantages; but one of the most remarkable things in connection with the machine described was that it welded what could not be welded at all by the hand-process, namely, hard and so-called unweldable steel. To engineers, steel-makers, and tool-makers, that was a distinct advantage. He had seen some very hard Mushet steel (the hardest tool steel) welded to soft or Bessemer steel and to iron in a way that allowed the tool steel to be used right up to the end, instead of having to be thrown away when it was too short to hold in a machine. He had also seen lead welded—a curious operation, impossible by any other means, and one which he believed, in certain manufactures, was very valuable. Mr. Matheson.

Mr. B. Blount asked whether there were any data respecting the composition of the bars before and after welding. The smaller angle through which an electrically-welded bar might be bent cold might be due to some change which had occurred during the drastic operation of electric-welding; and he should be glad to know if any analyses were available which would throw light upon that point. Mr. Blount.

Mr. W. W. Beaumont had made a number of experiments with the same machine with $1\frac{1}{8}$-inch Farnley iron, and with other sizes; and from what he saw with regard to the passage of the current, the discrepancy between what Mr. Crompton and Mr. Mordey had said was only apparent. When the ends of the pieces of iron were rounded, the centre parts, or the parts that touched first, became hot first, like the two pointed ends of a pair of carbons in an arc lamp. It might be true that the passage of the continuous current was as Mr. Crompton had stated, and the alternating current as Mr. Mordey had said; but as a matter of fact, those parts first became hot which were first in contact with each other. But then the bars were not rounded, they were flat. When the bar was sheared off, the shearing was at a slight angle, and the two pieces were placed in the clamps so that one angle became the complement of the other, and contact took place over a considerable area immediately after the current was switched on. With regard to the power taken in performing 1-inch welds, he had found, from experiment, that the maximum HP. was 22 or 23, and the average, taking the whole time it was being made, not much more than 12. That would reduce the cost given in the Paper as compared with the cost of welding the same iron by hand. He had noticed that the electricians welded some of the bars at one operation, but when they wanted to be very careful to get an excellent weld, or one with a good finish, though perhaps not stronger than the others, they took two heats. He had also observed that they almost invariably Mr. Beaumont.

Mr. Beaumont. left the iron where it was welded rather larger in section than the other part of the bar. That might, perhaps, explain the fact that in some cases the bending did not take place to so large an angle as in the case of some of the hand-made welds. It would be interesting if, when tests were made of the strength of electric welds, the specimens could be turned, so that the sections should be the same throughout. Mr. Mordey had mentioned the use of a small engine and heavy fly-wheel. From the figures which Mr. Beaumont had given, it appeared that a smaller engine might be used, when only one machine was needed, more economically than a large engine. Although welding by electricity might not take the place of ordinary welding for common work, the process would, no doubt, be extremely valuable in many cases where welding was now impossible, or where things had to be made out of a solid piece but could be more cheaply made in parts; and also in the work of brazing.

Mr. Mordey. Mr. W. M. MORDEY said that the difference of tendency between an alternate and a direct current was this: with a direct current the part heated first was the centre; with an alternate current it was the outside; with an alternate current the heat grew inwards; with a direct current, outwards.

Mr. de Segundo. Mr. E. C. DE SEGUNDO stated that no details had been given of the increase of cost of electric welding over that of ordinary welding by a smith's fire. It was of course not fair to judge by the temporary plant exhibited at the Institution. But, even in the case of a welding machine in constant use, it was not easily seen how, generally, electric welding could compete with ordinary welding when the cost was taken into joint consideration with the time saved. Neither in the tests mentioned in the Paper, nor in those detailed by Professor Kennedy, could a saving in time of more than 50 per cent. be claimed in ordinary work, whereas the cost must be many times as great. For in the case of the smith's fire the work was brought into direct contact with the heat of the fire, and the heat was to a great extent localized by the blast, and by well-known measures adopted by smiths; but in electric welding even under the most advantageous conditions, namely, where power could be obtained cheaply, the first cost, as also the maintenance of the plant, was and must be necessarily larger than that of an ordinary forge, and the power was frittered down by two efficiencies, that of the dynamo and that of the transformer. In cases were steam-power was used, a considerable increase of loss was unavoidable, since heat being the lowest known form of energy, the transformation to a higher form and then down again to the lower could not but result in waste. Further, it must be evident that, at least for the present, the range of applicability of electric welding must be limited, for, with the machine exhibited it was certainly only possible to do work of a very straightforward character. For instance, should it be required to weld channel or other forms of iron, as related by Professor Kennedy, it was not conceivable that the same clamps would be used as for round bars; otherwise it was difficult to see how injury to the material from local heating could be avoided. If not, special clamps, or universally adjustable clamps, or special machines for each class of work, must be provided; but the first arrangement would involve a loss of time, the second would be difficult of realization owing to the contacts necessary for such large currents, and the third would involve a heavy outlay. It was, of course, extremely difficult to calculate the electrical energy required to weld iron, but the following gave some idea, however rough, of the required amount. Assuming the bar to be welded to be 1 square inch in section, and that the metal for 2 inches on each side of the weld was raised in temperature 2,000° Fahrenheit on the average, the expenditure of heat, supposing for simplicity in calculation there were no radiation and constant specific heat, would be about 250 thermal units. Now the Author's practical illustration showed clearly the enormous amount of heat lost by radiation and conduction, hence at least 50 per cent. might safely be added to this value. Of course the duration of the "heat" should be taken into account, as this would greatly affect the amount of heat radiated. Assuming, however, $250 + \frac{250}{2} = 375$ thermal units to be the amount required, this multiplied by the average counter-efficiency of the transformer, say $\frac{1}{0 \cdot 6}$ of the dynamo, say $\frac{1}{0 \cdot 85}$ of the engine, boiler, and loss in transmission by belting, &c., say $\frac{1}{0 \cdot 07}$, would correspond to 10,600 thermal units liberated in the furnace, or to the combustion of something like 1 lb. of coal, such as was used in a smith's forge. This, it need hardly be said, although a low estimate, was probably in excess of what was burnt at a forge for so small an operation as the welding of two bars 1·13 inch in diameter. The cost would increase as the work got larger, and probably a limit would soon be reached. Besides it was not probable that in the case of large bars, the heat would be anything like as uniformly distributed throughout the material as if the bars had been heated in a smith's fire. There could, however, be little doubt that for certain special branches of industry electric welding would find a useful application, but it would not be prudent to predict for it any wide extension in purely mechanical or civil engineering work.

Mr. de Segundo.

Sir Frederick Bramwell.

Sir F. BRAMWELL said he had very little to reply to. Not unnaturally, the speakers had been principally electricians, and they appeared to him to have shown sufficient diversity in the opinions they expressed for one opinion to neutralize the other; the acid and alkali making a neutral salt. Mr. Preece had given the result of experiments made by him to ascertain the number of amperes needed to immediately fuse a wire, so immediately as to represent the result of a flash of lightning upon a conductor, for it was with reference to conductors that he had instituted the experiments. It appeared to Sir Frederick Bramwell that that had nothing to do with the Paper. He had not been dealing with the instantaneous effects of electricity, but with the practical welding question, and therefore the question of what number of amperes passing through any given diameter of wire of any given metal would cause instant deflagration, without the question of the loss of heat energy by dissipation coming into account, had nothing to do with the subject before the meeting. The experiments were, no doubt, interesting in themselves, but they were not germane to the matter. On the question of time being an element in the attainment of any needed temperature, there was one point in his Paper, that of the extra resistance offered by the metal as the temperature rose, the importance of which appeared to have been overlooked; to this point he should have to revert. Mr. Crompton had said that time had nothing to do with the matter. He could not understand how a gentleman of his large experience in electrical science and in its applications could have made that statement, or indeed how any one who had seen no more than the experiments carried out at the meeting could have come to that conclusion. It must have been obvious to every one that the temperature was a growing quantity, and that it continued to grow until the sources of dissipation of heat operated to such an extent that the dissipation was equal to the supply, and then it remained stationary. If a given conductor—the thing to be welded between the points that were to weld it—destroyed the well-known 33,000 foot-lbs. of electrical energy in any given time, say a minute, the utmost that could be got out of that destruction, and that which would inevitably be got out of it, was 41 or 42 heat-units. These heat-units would be obtained if the current were continued, say a minute; if continued two minutes, the number of heat-units would be doubled. There would, he agreed, be a certain amount of loss, but the product of heat-units would be doubled; and how, therefore, it could be said that the heating, depending as it did upon the destruction

Sir Frederick Bramwell.

of electrical energy, and the conversion in the conductor which destroyed that energy into heat was instantaneous and independent of time, he could not imagine. Mr. Mordey had referred to some extremely interesting experiments, but he did not appear to agree with Mr. Crompton or with Mr. Preece. His suggestion was that in alternating currents, the heat was communicated, to begin with, on the outside of the material, and that it would heat up a tube more rapidly than it would heat up a solid bar. Some of them no doubt were aware of a curious instance, in the person of one of their Past Presidents, of the effect of an alternating current upon a bar, the outside having been made so hot that his fingers were burned by laying hold of it, but afterwards, when thrown down and picked up by a labourer, it was found to be quite cold. The heat, in that case, like beauty, was only skin deep. This expression was corroborative of Mr. Mordey's views. But although this might be abstractedly true of the result of using an alternating current, it certainly did not obtain in the practical work of the machine, and Sir Frederick Bramwell was inclined to believe that Mr. Mordey, like the other speakers, had neglected the statement in the Paper to which he had before alluded, that of the effect of the increase of the resistance with the rise of temperature. He desired to be permitted to quote the passage: "This increase of resistance to the passage of the current, as the temperature increases, is of great utility in electric welding. Consider the two ends of bars to be welded, mere ordinary rough surfaces, the first contact is made upon numerous points, through these the current passes, and they become rapidly heated, and offer more resistance. As endway pressure is applied, the surfaces in contact become of larger and larger area. The greater current seeks those parts which, although in contact, are at a lower temperature, and this goes on until contact, and uniform temperature, are obtained all over." If more value had been attached to that statement it would have been seen why it was practically a matter of indifference whether the heat began at the outside or in the middle or anywhere else. It might be illustrated by the passage of water. Suppose there were a closed vessel out of which proceeded a number of tubes, some a $\frac{1}{4}$-inch, some $\frac{1}{2}$-inch, and some 1 inch diameter, and that the box was supplied with a uniform pressure, and that it was desired to send water through the tubes. They all knew what the rate of flow would be through those respective diameters, how the inch tubes would prevail over the $\frac{1}{2}$-inch, and the $\frac{1}{2}$-inch over the $\frac{1}{4}$-inch tubes. But suppose while the water was going through the

Sir Frederick Bramwell. larger tubes with great rapidity there was deposited from it some matter inside the 1-inch and ½-inch tubes, choking them up? It appeared to him that the time would soon arrive for the other tubes to take up the running, because on the hypothesis those tubes that were the best at the beginning would be the worst afterwards. In that way, from the incident of the operation which he had suggested, they would obtain a practical uniformity at last of the currents through all the various parts of the water-conductor which he had thought of as a comparison. Similarly with electricity. Any one who had watched the two bars in contact would have seen that the glow began where the contact took place, whether in the middle or at the edge; that the points of contact then became plastic, that they were compressed by the screw, that more surfaces were brought in contact, and that those surfaces being cooler, the current sought them, heating them up next in turn, and that this operation went on until the whole surface was heated. Mr. Preece's suggestion was that the greater heat at the centre as compared with that at the edges was due to the enormous rapidity with which the heat was conducted away by the holding jaws. Although no doubt there was conduction there, he thought Mr. Preece would find that it was not, as he had put it, because the heated part was cooled down by contact with the jaws, but that the central part remote from the jaws was the hotter because it was the part of worst conduction from the very outset of the current, and became worse as the operation went on—worse to begin with because of the few points of contact, worse afterwards because of the heating up of that particular place; therefore it necessarily remained the hottest. With regard to the other speakers—Mr. Siemens, Sir Frederick Abel, and others—he could only thank them for the kind manner in which they had spoken, and for the information they had given. Professor Kennedy's statement also as to what he had seen in the United States, where the machines were in actual commercial use, was of great value to the meeting. One speaker had said that he was surprised at the amount of plant and power used for the experiments. He had already adverted to the cause, but he trusted he might be allowed to repeat that as much electrical horse-power must be destroyed as would give the needed amount of heat-units in a given time; it was a necessity, and it could not be helped, unless they were prepared to spend more time in the heating up; but this meant waste of the workman's time, and waste of the production of the machine, and waste of power, as the cooling losses were continued for a longer period. As a matter of fact, there was utilized a very

Sir Frederick Bramwell.

large amount of the heat derived from the destruction of the electrical energy—of the electrical horse-power—the defect in utilizing the fuel was not at this point, but was at an earlier stage, that of obtaining in the steam-engine anything like the value of the heat-units residing in the coal consumed. If they only had the horse-power in the steam-engine for anything like the theoretical value, there would be no furnace so economical in rendering back the heat to the iron; the loss would be found to be much less than with any known mode of applying heat; but the difficulty was at the outset, the first conversion of the fuel, the turning of its latent energy into motive power; the loss was there. It remained to be seen how far, on a balance of advantages, the greater consumption of fuel (if it were greater) and the size of the plant, would outweigh the obvious conveniences connected with the certainty, the cleanliness, and the rapidity of the operation. As to the way in which the bars had shown signs of cracking when bent cold after passing through only 60°, as compared with the scarf-welds at 114°, he had already said that he thought it could be cured by annealing, but he had not as yet suggested the cause of the difference; he now desired to express his belief that it was due to the extreme localization of the heat during the welding operation. With a scarf-weld the whole length of the bar was heated up as far as the length of the scarf, and there was a considerable extension of heat beyond that; but with the electrically heated butt-weld the heat was much localized, and it appeared to him that that might result in a difficulty in the cold bending—a difficulty of no great importance, and one which might be almost certainly overcome by annealing. He thought he was justified in saying this because when the electrically-welded bars were bent hot they bent neither better nor worse than the hand-welded bars, and this difference between the hot and cold states could only be due to the fact that in the re-heating the heat was diffused through a considerable length. He therefore thought that if these bars had been allowed to cool down, and had then been tested, they would have bent as well cold as the hand-welded bars. Although he had not been able in person to make the experiment, or even to superintend it, he had been told, and he had every reason to believe it was the fact, that in cases where the bars had been annealed they had given bending results as good as hand-welded bars.

[CORRESPONDENCE.