Mr. Preece.

1st Experiments.

d (yards)	50	100	200	300	...
Calculated value of deflection	35	22·3	11·4	6·6	...
Observed deflection	35	23·0	11·5	6·5	..

2nd Experiments.

d (yards)	100	200	300	400	800
Calculated value of deflection	16	8·2	4·7	2·9	0·7
Observed deflection	16	8·0	5·25	4·0	2
				Approximate.	

3rd Experiments.

d (yards)	100	200	300	400	800
Calculated value of deflection	17	8·7	5·0	3·1	0·75
Observed deflection	17	8·5	5·5	4·0	1·5
				Approximate.	

D = 440 yards.
Values of *d* approximate only.

Mr. Evershed.

Mr. S. Evershed: A fortnight ago we had, what is, I am sorry to say, the rare, pleasure of listening during the evening to Dr. Lodge's interesting account of his explorations in that sort of fairy-land which he has made his own—the region of the invisible, intangible ether. Dr. Lodge has the happiest manner of getting his own ideas into other people's heads, and I must confess that I find nothing more instructive than an evening spent under those circumstances.

Dr. Lodge has given us in his paper a kind of general sketch of the principles of telegraphy by magnetic induction. To-night

Mr. Evershed.

I want you rather to come out of the fairy-land into which he led us, and look at the subject before us from a business point of view.

If wireless telegraphy of any kind ever comes into practical use, it will not be to replace or displace the ordinary wire telegraph, but it will be for use in such cases as are met with fairly frequently where a wire or cable cannot possibly be laid. Therefore it comes down to this: There may be some such places where communication is desirable: how much must people be prepared to pay for it? All electrical engineering problems ultimately come down to questions of cost, and in preparing my paper I endeavoured to deal with the subject from that point of view, rather than attempt any detailed description of apparatus.

In order to arrive at the cost of an induction telegraph we must consider the three items of which every telegraph of the kind will be made up—first of all, the arrangement of the circuits; secondly, the transmitting devices, and the manner in which we are to obtain the primary power, and the amount of power which is feasible under any given conditions; and, thirdly, the receiving devices. I need not tell electrical engineers that the most important thing to know about receiving devices is how much power it takes to work them. There is only a very limited amount of power that can possibly be received in a secondary circuit at a distance, and that amount of power must suffice for working some sort of indicator; that is to say, an indicator must be provided which will respond to the exceedingly minute amount of power available. Now I need not keep you long over the question of the circuits, because this part of the subject is fully dealt with both in Dr. Lodge's paper and in my own paper. I am very glad to see that Dr. Lodge supports the view which I have from the first adopted, namely, that the circuits should be laid in the plane of the earth, with their magnetic axes vertical. The first time I worked out the details of an induction telegraph was in 1892, in connection with communication with light-vessels; and the idea I had was to communicate with all the lightships—and there are numbers of them—round the coast of Kent by means of a single circuit

Mr. Evershed.

enclosing the whole area of the county. I found, as you would naturally expect when you remember that the area of the secondary circuits is limited by the size of the light-vessels, that the cost was prohibitive, unless we could employ an immense amount of primary power. I gave up that scheme, and then devised an arrangement for communicating with the North Sand Head light-vessel by means of a cable on the bottom of the sea. That arrangement, as most of you know, was a total failure, in consequence of the enormous absorption by the necessary armouring on the cable, by the sea, and by the iron of the ship. Of those, the iron of the ship was the most important. I notice that Dr. Lodge rather suggested that the sea would not have much influence, but it did have a very considerable influence. Even at as low a frequency as 16 periods per second the sea caused the absorption of from 30 to 40 per cent. of the induction; but, even supposing the sea had been absent, the iron of the ship was alone sufficient to prevent any signalling at such a frequency as 400 ∿ per second. I was able by means of the vibrating rectangle of my relay to measure the total absorption due to the combined effects of armoured cable, sea water, and iron hull of ship, and it actually amounted to 97 per cent. at 16 ∿ per second.

Now, when we come to telegraphy on land, the matter is rather different, because, so far as we know, the earth at moderate depths is a very poor conductor, and, although there is a great deal of it, the fact that its specific conductivity is very small is in our favour. If it is only low enough, telegraphy can be carried on by means of magnetic induction.

Before we lay circuits, as Dr. Lodge has described, we must endeavour to ascertain what arrangement will give us the best value for our money. Clearly the circuits ought to be circular, but as a rule we shall find that an impossible condition, and we must be content with rectangular circuits, and make them of as large an area as possible. It is also clear that the cost of that circuit will depend upon the weight of copper which you put into it, and the length of the pole line on which it is run. And for that reason you will find in my formula 4^a that I have given

Mr. Evershed. you the maximum power which is available in the receiving circuit, in terms of the frequency, the length of the side of a square circuit (I took a square because you can get something approaching that in practice), and the *power* wasted in the primary.

I notice that Dr. Lodge is hesitating between taking the E.M.F. in the primary circuit, or the primary current, as the essential factor. He is apparently not quite clear which to use in his equations. I met with exactly the same difficulty when I first began working at the subject, and it is only within the last two or three years that I have seen clearly that the cost is pretty sure to depend upon the power. The price of the dynamo does not depend very much on the E.M.F. or current it has to give; it depends upon their product. The price of a battery or dynamo for working an induction telegraph will depend upon the power, and therefore the corresponding factor in my formula is watts. Then we have the volume of copper. I only left in volume because it is a little more convenient for calculating the resistance than if you put in weight. It is quite easy to substitute the weight of copper. Formula 4^a, then, tells us what is the greatest amount of electrical power you can possibly convert into mechanical power to work a receiving instrument when you have adopted every possible means for bringing the current into step with E.M.F., either by means of condensers or other means; and probably the condenser system which Dr. Lodge described to us will prove to be the best for the purpose. It is physically impossible to get more than that power. I have worked out a numerical example in my paper, and, as you see, the amount of mechanical power which is available in the secondary circuit is only 0·34 ergs per second. We are accustomed to dealing with horse-power and watts, but when we have to reckon power in ergs per second it is not easy to realise how small it is, and how exceedingly delicate the receiving instruments must be.

To turn for a moment to the transmitting devices, you can use interrupters (as I believe Mr. Preece has invariably done); but, although they are admirably adapted for giving a readily audible tone in the telephone, they give trouble from sparking. Mr. Preece gets rid of sparking to some extent by means of a condenser, but it cannot be entirely eliminated by this means. Another transmitting device is the one which Dr. Lodge has described to us, namely, an alternator fitted with a condenser, in order to bring the current into step with the E.M.F., and so bring "power-factor" up to unity. The use of condensers for that purpose is fairly familiar to those of us who have been working with alternate currents. I do not think I need say any more about transmitting arrangements, because, to my mind, if the difficulty of a suitable indicator can be overcome, there is very little difficulty in providing the proper transmitting apparatus.

Mr. Evershed.

Now I come to receivers. Since the function of the receiving instrument is to detect an alternating current, it is probable that some form of vibrator will always be used, like the diaphragm of a telephone, the rectangle which vibrates in my relay, or an apparatus of that kind. The "buzzer" used by the War Office is another instance of a vibratory receiver.

The complete theory of the vibratory relay is given in my paper, and, as you will see by referring to Fig. 2, the rectangle behaves precisely in accordance with theory, the discrepancies being well within the limits of observational errors. In the case of the telephone we are unable at the present moment to measure the amplitude of the diaphragm, and, therefore, unable to measure the mechanical power required to make an audible note. However, it is easy to measure the minimum ampere-turns on the coils necessary to produce an audible note. Mr. Oswald Cox and I made a determination of this kind at Woodfield Works some years ago, and I had the great advantage of having two experts from the Post Office to assist me on that occasion, namely, Mr. Gavey and Mr. Kempe; and we all four made a trial of what we could hear, and, curiously enough, we all agreed. The human ear no doubt varies from time to time, but under the same conditions four average men do not differ very much in what they can hear. I have compared the result which we got with that given by Lord Rayleigh for a similar type of telephone. Lord Rayleigh says that with 1·6 milliampere-turns, alternating

Mr. Evershed. current, in the coil of his telephone there is a readily audible note. He says you can sometimes just hear with about half that amount. Now Mr. Gavey, Mr. Kemp, Mr. Cox, and myself were able to hear 1·4 milliampere-turns; or, rather, Mr. Gavey thought that was about the least amount which was necessary for signalling. But we could all hear a faint note when the ampere-turns were reduced to about 0·7 milliampere turns; so we do not differ appreciably from Lord Rayleigh on that point. At least 1·4 to 1·6 milliampere-turns may be taken to be necessary in order to produce easily audible signals in a Gower-Bell telephone. The Gower-Bell pattern, however, is very imperfect when considered as a means for converting electrical into mechanical energy. As a motor the Gower-Bell is not good. It is like some of the early alternating-current dynamos and motors of the "inductor" type.

At the time the tests I have just described took place, Mr Cox devised a species of "moving-coil" telephone, which has since been re-invented and developed by Dr. Lodge. The idea rose very naturally in Mr. Cox's mind from working with the relay. Seeing the rectangle moving in a magnetic field, it naturally occurred to him that he might attach the coil of the telephone to the diaphragm and allow it to move in a strong magnetic field, and we devised one or two forms of that type. I feel sure the "moving-coil" telephone will ultimately displace the telephones now used, not only for motor work—that is to say, for induction telegraphy—but also for speech. It is clear, from what we all heard of Dr. Lodge's telephones, that they are at least equal to the ordinary patterns as speech instruments, and electrically they are infinitely superior.

But I do not lay very much stress on matters relating to the design of telephones, because I cannot help thinking that we shall have great trouble in working at anything like a frequency of 400 per second over great distances, in consequence of the absorption of induction waves by the earth. I look more towards a system working at very much lower frequency—something below 50 periods, for example. The only low-frequency indicator I have been able to devise is my relay for calling attention, and

Mr. Evershed.

at present it is only available for that purpose—for ringing a bell to call attention. But I cannot help thinking that something on those lines will ultimately be made and adapted to give Morse signals, and will enable us to signal to great distances without any fear of absorption. The two "call" relays which were made for the North Sand Head lightship experiments were arranged to work at 16 periods a second. There is no particular virtue in 16 periods a second, but it happened to be a convenient value. A rectangular wire of a convenient length and material had that frequency, and, it being a very suitable frequency for the purpose, we stuck to it. The equations given in my paper enable one to calculate exactly how much power is required to maintain the vibrations of the rectangle at any amplitude, and I think I have included every electrical and mechanical detail which is necessary in order to clearly understand what a relay of this type will do.

As Dr. Lodge mentions in his paper, those two relays have now been in use for some months at Lavernock and Flat Holm, on Mr. Preece's wireless telegraph there, and I was pleased to hear from Mr. Preece at the last meeting that they are working satisfactorily. Dr. Lodge was kind enough to say in his paper that he admired my system of call, but he then proceeds to say that I have obtained the result at the expenditure of a good deal of power. Well, of course, "a good deal of power" is a little indefinite. I have looked through Dr. Lodge's paper in order to find out what he would consider a moderate amount of power, and I find in his own transmission between the College at Liverpool and his house he is now using, so far as I can make out, from 500 to 900 watts in the primary circuit; so I take it he does not consider that a very considerable expenditure of power. Now on the Lavernock–Flat Holm telegraph we are only using about 120 watts for the call—much less, in fact, than Dr. Lodge uses himself.

Professor AYRTON: Did you say 120 watts?

Mr. S. EVERSHED: Yes, 120 watts. The current is 8 amperes, and the resistance of the circuit is less than 2 ohms. Through Mr. Preece's kindness I am able to show you one of these relays at work. I have only been able to rig up an apparatus which will just show you the manner in which it works. I cannot, of

Mr. Evershed.

course, show you its actual operation by means of an alternator three miles away.

[One of the relays in use at Lavernock was placed on the table and connected to a secondary coil. Three means by which the two rectangles could be brought into contact and a bell rung were shown to the meeting. In the first, the continuous current produced by slowly moving a powerful magnet across the secondary coil was of sufficient strength to force the rectangles together without vibration. In the second, a feebly magnetised piece of clock spring was set in vibration close to the coil. The spring having been tuned to vibrate in exact synchronism with the rectangles of the relay, it was shown that an almost invisible vibration of the spring generated an alternate E.M.F. in the coil, and the current flowing through the rectangles set them vibrating and rapidly brought them to a sufficient amplitude to close the bell circuit. The third method was analagous to that in actual use at Lavernock. Exact synchronism was obtained by allowing a small magnet set spinning near the coil to gradually come to rest; thus generating a current of gradually diminishing frequency. The little magnet was mounted on an axle fitted with a small fly-wheel to ensure a sufficiently gradual reduction of speed. It was shown that the relay responded (and rang the bell) when the speed had fallen to that corresponding to the frequency of the relay.]

Dr. Lodge describes in his paper and showed us upon the screen several examples of "call" apparatus based on the same principle as my relay, but he uses tuning-forks as the receiving vibrators. I began with tuning-forks, but very soon gave them up, on account of the large amount of power they need to work them. I have since found that Lord Rayleigh measured the power taken by a small tuning-fork when it was giving a just audible note. He found it required no less than 42 ergs per second to maintain its vibrations. Now the rectangles in the relay which I have shown to-night reach a visible amplitude when the power is only one-thousandth of an erg per second. That shows the enormous difference in the molecular, air, and other frictional resistances opposing the motion of the fork and the vibrating rectangle.

Mr. Evershed.

I should like just here to make a digression, because it so happens that I can give you a little information which will clear up a doubtful point. There have been many questions raised as to whether in Mr. Preece's system of telegraphy the effect is due to induction or due to leakage. Well, it must have occurred to everyone, Why doesn't someone measure the leakage, and so settle the matter once for all?

The truth appears to be that it is by no means easy to measure the leakage when an alternate current or an interrupted current is used. In the spring of this year I went down to Lavernock to assist in setting the call apparatus to work there, and Mr. Gavey and I spent three or four happy days there doing nothing but experimenting. Among other things, we measured the leakage in the secondary when a continuous current was flowing in the primary. We found that when 1 ampere was flowing into the sea through the earth-plates at Flat Holm, we got a potential difference on the terminal earth-plates at Lavernock of 0·4 of a millivolt—quite enough to make the telephone, in use there for signalling, howl. So much for the leakage with a continuous current. Now by means of the relay I was able to measure the leakage with an alternate current at 16 periods a second. It ought, you would think, to have borne the same proportion to the primary current that had been noted with continuous currents. Not at all! It was ever so much less, only amounting to from one-sixth to one-seventh the amount of leakage with continuous currents. That appears to be due to the fact that when we were using the alternator at Flat Holm an alternating potential difference was set up between the Lavernock earth-plates, and the electrolytic effects on the plates being incapable of complete reversal at even so low a frequency as 16 periods per second, prevented the current in the Lavernock line from rising to the value reached with a continuous current. But if the leakage at 16 periods per second is only one-sixth or one-seventh of the value observed with a continuous current, how much less must it be with a frequency of 400 periods per second! It appears to me quite possible that at 400 periods a second the leakage is reduced to a very insignificant amount, and that the

Mr. Evershed. greater part of the current received in the telephone for Morse code signalling at such a high frequency as 400 periods is really due to magnetic induction. The matter should be cleared up by further experiments, and no doubt experiments on the electrolytic effects of earth-plates can be carried out in a laboratory.

The final matter I have to speak about is absorption. Dr. Lodge takes rather an airy view of absorption ; he does not think it will be serious. Well, perhaps the wish is father to the thought. I, on the contrary, having gone through a bitter experience, and having had a beautiful scheme to which I had devoted an immense amount of time rendered completely useless by reason of the enormous absorption, take a very different view of the matter. There is no doubt in my mind that absorption of the waves by the conduction of the earth will play an important part in diminishing the effect at a distance in circuits separated by many miles. It appears to me that the absorption should be measured, and I do not see how we can prophesy or design any induction telegraph whatever until that point has been definitely cleared up. It is no use saying, "Here is an indicator which will work "with only one-thousandth of an erg per second," or whatever it may be. I do not know how much Dr. Lodge's magnifying telephones take, and I should like him to tell us how many ergs per second must be converted into mechanical work in the first of his series of telephones in order to make an audible signal in the final telephone of the series; or, if he cannot tell us the power, tell us the current, or the volts, or something—something to give us a clue to the amount of current or power which is necessary.

But even when we have that information we cannot prophesy what Dr. Lodge's telephones will do on any given circuits, because it may be that with the relatively high frequency he uses the whole of the energy leaving the primary circuit will be absorbed before it reaches the secondary. Therefore it appears to me we really must, in the first place, measure that absorption effect once for all and get to the bottom of it. I am preparing plans for doing it myself. It is rather a difficult job; it is a matter which cannot possibly be undertaken without the co-operation of the Post Office, and that co-operation, I need hardly say, has been very cordially offered to me by Mr. Preece. With his aid I hope to clear up the existing uncertainty as to absorption by the earth. But it must be cleared up decisively; it must be a definite measurement of absorption. Qualitative results in a question of this kind are absolutely of no use whatever; we must have a quantitive result. We must know exactly what the absorption is, and how it varies with distance, with frequency, and with different underlying geological formations. When that uncertainty has been removed, and we know how much to allow for absorption, we shall be able to predetermine the best form of apparatus for signalling by magnetic induction, with the same confidence that we now feel in designing a plant for power transmission. Mr. Evershed.

Mr. W. H. PREECE: I thought, Sir, that I would save the time of the meeting by putting on record the work that we have done in the Post Office in establishing the so-called "wireless "telegraphy." A paper has been distributed, I hope amongst you all, in which you will find on this particular branch of the subject a reference made to what we have done during the past 16 years. Now I want you to observe that there is one very great difference between the work that we have been doing and the work that has been brought before you by Professor Oliver Lodge and by Mr. Evershed. The work that we have done during the past 16 years has been purely and absolutely experimental. We have started from a very small beginning, and we have pushed on steadily annually. I think, if a record of the work, and of the innumerable experiments made from 1884 to 1892, were in print and were shown to you, you would be simply astonished that so much work has been voluntarily, unrewardedly, and cheerfully done. To save the time of the meeting, I thought that I would illustrate the principle and explain the diagrams as rapidly as I could. The system was not patented, as it might have been. On the diagram on the left-hand side the connections are shown. I may say at once that that apparatus is extremely simple. I have read of the apparatus in use at Lavernock being spoken of as cumbrous. There is no doubt in the earlier experiment in 1892 it was cumbrous; but that term applies no Mr. Preece.

Mr. Preece. longer, for we have replaced the very heavy alternator by 50 ordinary common dry cells. A little motor is shown at A. The motor and the interruptor are on the same spindle. When I put on the switch the current goes through the motor; the motor has interrupted the current at a frequency of 400. *Here* I have a simple Morse key, an ordinary telegraphic key used when a message is transmitted. On the table I have a primary coil, and through *that* coil currents are sent when I depress *this* key. *Here* I have a telephone fixed with a trumpet-shaped mouth, so that I hope the sounds will be heard over the room. Now, when I bring *this* secondary coil over the primary one, the currents induced in the secondary coil as I come down increase. As I take the coil away you will observe how rapidly the effect diminishes. I can make that sound disappear by simply turning the one coil so as to place it at right angles to the other. That simply illustrates the principle. Now *here* we have a rectangle forming the primary coil, and *here* is a movable coil of 50 turns. I brought it close up to the table so as to make use of the same secondary coil. At the other end of the room there is another rectangle. These two coils represent Lavernock and Flat Holm, and the sounds that you will hear by the telephones may be taken as an indication of the sounds heard across these 3½ miles separating those two places, although probably they are twice as strong. Now we will send the signals through that coil, and see if it is possible to make them audible. If anyone at the other end of the room will only listen to one of those telephones, they will tell me whether they hear signals or not.

[*Several members listened at the telephones, and stated that the signals were distinctly heard.*]

They are perfectly audible at that end of the room. Well, here you have an illustration of a simple telegraphic apparatus that has been devised by sheer plodding experiment. It has taken a good many years to reach this present condition. It has been made a real practical telegraph by the calling apparatus that Mr. Evershed has explained to you to-night. It is as practical and as simple and cheap and as effective as any form of

Mr. Preece.

telegraph apparatus that exists. If it is in practical operation only between Lavernock and Flat Holm, it is because nobody has asked for it anywhere else. Professor Lodge, in his paper, mentioned that Mr. Stevenson, in Scotland, had proposed to use this system in the extreme north of the Shetlands, at a place called Muckle Flugga; but, as a matter of fact, no experiment was ever tried there. The experiment that was referred to was an experiment tried near Edinburgh, in a place called Mayfield. The experiment itself was not an experiment of Mr. Stevenson's, but one which was carried out under my instructions by my assistants in Edinburgh, to prove to Mr. Stevenson that his views were wrong. I am not quite sure that they succeeded in doing that. You cannot convince a man against his will. But, at any rate, no connection has yet been made between the Shetlands and Muckle Flugga. I have no more to add, except this—that I do not propose to discuss Professor Oliver Lodge's paper; it contains nothing really new except the call. I said what I wished to say about it the other night. I appreciate it very much, and I think he has brought to bear upon the subject his well-known style, with which we are all so well acquainted. But I venture to say to him that there are a good many statements which require a little modification. I will not point them out to him now, but I am quite sure when I do so by letter there is nobody who will be more ready to correct them than he.

Prof. Fleming.

Professor J. A. **Fleming**: The two communications which have been brought before us by Dr. Lodge and Mr. Evershed raise many interesting points for discussion. Dr. Lodge opens his paper with a rough classification of methods of space telegraphy. The time has hardly yet arrived for an accurate classification of these methods. If we leave out of account some methods in an embryonic condition, such as those of Zickler—who is said to employ ultra-violet light—and neglect those methods which depend upon conduction through soil or water—which are certainly not space telegraphy at all—we may say that at the present time we are undoubtedly in the possession at least of two practical methods for communication through space depending on electrical means. The first, which is our chief subject of discussion now, Prof. Fleming.

may be described, perhaps, as a closed-coil system, and is that devised by Mr. Preece and elaborated by Dr. Lodge. This method consists virtually in the construction of a gigantic air-core alternating-current transformer of which the coils are separated from one another by a distance, perhaps, of many miles. Dr. Lodge ingeniously takes advantage of the principle of resonance to exalt the effect in the secondary circuit. We have for a long time been familiar with a similar effect in the case of large alternating-current transformers used with a condenser in the secondary circuit. We have often discussed here the effects produced when a long concentric cable having a certain electrostatic capacity is switched on to the secondary circuit of a transformer and thereby exalts the electro-motive force in that circuit. In this closed-circuit system of space telegraphy the energy associated with the primary circuit never leaves it in the form of a true wave. We merely use the secondary circuit to detect and measure at a distance the time variation of the magnetic force due to the primary circuit. The other method, which may be called the open-circuit method, to which Dr. Lodge also briefly alludes, has been made known to us chiefly by the interesting experiments of Signor Marconi. This latter experimentalist uses a primary wire, open or circuit, consisting of a straight vertical rod, in which an alternating current is set up by means of an induction coil and spark balls. At a distance he places a similar secondary circuit in which there is a Branly detector, also the two circuits are connected together at one end through the earth. Dr. Lodge suggests that a part of the effect in the secondary circuit in this case may be due to electric jerks, as he says, taking place through the soil, and that "the "coherer in the receiving circuit is a detector of electric jerks "transmitted by the earth or by uninsulated conductors such as "gas pipes." I am not sure that I understand exactly what is meant by an electric jerk. I have tried a good many experiments with Branly detectors, consisting of tubes or boxes partly filled with metallic filings, and it is convenient to call these arrangements discontinuous conductors. A Branly detector of this class is a detector of electro-motive force in the circuit in which it is

Prof. Fleming.

placed, and when that electro-motive force reaches a certain critical value the discontinuous material passes suddenly from a condition of almost perfect non-conductivity to a condition of very high metallic conductivity. If a detector of this kind is put in series with a long wire, and if electric force is created in that region, either by an electro-magnetic wave passing through it, or any other way, then the view I take of the matter is that the wire integrates the electric force parallel to itself into electro-motive force. If the wire has a sufficient length, then, since the electro-motive force is the line integral along it of the electric force, a value of electro-motive force in the circuit may be reached which breaks down the non-conductivity of the coherer. Therefore, if the electrical force is small, but if the wire or rod in circuit with the detector is made sufficiently long, we may be able to make evident the presence of the electric force in the region by its action on the discontinuous conductor or Branly tube. Hence, in the case of the open-circuit system, what we virtually do is to integrate the electric force into electro-motive force and detect this last. In the case of the closed-circuit method, what we do is to integrate by the secondary circuit the time variation of the magnetic force due to the primary circuit, and estimate that as electro-motive force by its action on a suitable receiver.

It is essential in both those systems that there should be a certain relative position between the receiving and transmitting circuits. In the case of the open-coil system, it is obviously necessary that this secondary circuit should be parallel to the primary circuit, and unless that is the case no signalling can be carried on over any great distance. It can also be shown that in the open-circuit system a similar condition holds good.

I have before me a box containing a battery, bell, relay, and Branly tube detector, connected up as usual; and on the other side of the room we have an induction coil and a pair of parallel rods, with spark gap, to transmit waves across the room. The Branly detector is inserted in the gap between two other long rods. At present the rods of receiver and transmitter are approximately parallel to one another. The experiment I want to show you is that to cause the bell to ring it is necessary that the rods

Prof. Fleming. of the receiver apparatus, or secondary circuit, must be parallel to the rods of the transmitter apparatus, or primary circuit. If we turn the receiver rods so as to be at right angles to the transmitter rods, the effect on the receiver is practically nothing, or, at least, is very much diminished. [*Experiment shown.*]

This experiment shows, I think, the necessity for the parallelism of the primary and secondary rods. In the early days of Marconi's experiments, we heard a good deal about tuning these "wings," as the appended rods or wires were often called, which are necessarily connected to the detector; and it was suggested that in this case also a true resonance effect existed. That, however, I believe, is not the case; the true function of the long rods or wires, as used in Marconi's system, is not explained by simply asserting the effect to be due to resonance.

Then, as to the functions of the earth in the open-circuit system of space telegraphy, we are in the presence of some interesting questions. Dr. Lodge seems to suggest conduction through the earth. I think that there is room for much more experimental work before we can speak on that subject with perfect confidence. Mr. Evershed strikes a truer note when he says that much room yet exists for inquiry. I tried some experiments in the last summer of the following kind:—If we connect the transmitter rods of the induction coil with the rods of the receiver apparatus by a wire which may be continuous or may be broken into lengths, when that wire is laid upon the ground, however much it may be earthed, we find that the wave from each spark at the transmitter travels along the wire and affects the detector. It naturally occurred to me to try these experiments with the wire immersed in water. The apparatus was therefore set up by the side of a lake, and a wire laid in the water one-eighth of a mile long; this wire, as before, connected the receiver and transmitter rods. On making sparks at the transmitter, we obtained, however, no effect at the receiver. It has been suggested several times, I believe, that an arrangement of this kind might be utilised for signalling through bare wires laid in water, even through the wire covering of submarine cables, using a Branly detector as relay. This suggestion, I find, has even found its way into patent specifications; but I am afraid it was put there by enthusiastic patentees, who rushed to the Patent Office first, and the laboratory, if at all, afterwards; because it certainly will not work. There is no possibility of transmitting signals along wires laid in that way through water, because of the exceedingly large absorption of the wave-energy that appears to take place in water. The above experiment confirms, therefore, the experience of others that both the conductivity and the power absorption of ether-wave-energy by water is too great to allow Hertz waves to be transmitted any great distance through it. Prof. Fleming.

There are many other points in connection with the ether-wave system of wireless telegraphy which it would be of great interest to discuss; but, as the subject before us is more particularly at the present time the magnetic induction telegraphy, I will not extend these remarks, but simply conclude by congratulating Dr. Lodge on the success he has achieved in advancing those methods of space telegraphy which the newspaper articles persistently call "wireless telegraphy," though in reality only telegraphy with less wire than we are generally accustomed to use.

General C. E. WEBBER: At this hour of the evening, and with so many wishing to speak, it has occurred to me to remind the Institution of one fact only in the past which I think is deserving of interest. It affects the memory of one of our Past-Presidents (Mr. Preece has spoken of the history of this question); and I should like to remind the meeting of the fact that on the 23rd March, 1882, when Professor Dolbear read his interesting paper in this Institution on what he called the "New Telephone," Mr. Willoughby Smith spoke these words: "I was prepared to "have shown that by placing a flat spiral of fine silk-covered wire "in the centre of this room, the spiral being connected to a suitable "transmitter and battery, that every person present possessing a "telephone would have been able to have heard whatever sound "was influencing the transmitter, although the telephones were "not in any way attached to the circuit." I believe that was the date from which so many of us started (I, in my own small way) General Webber.

General Webber. to investigate the interesting subject which has been brought before you this evening.

Mr. Whitehead. Mr. C. S. WHITEHEAD: The question has been raised by Dr. Lodge whether certain equations with which I opened a paper bearing on this subject do apply to frequencies so low as 300. As the equations have some importance in connection with this question, I am very glad of the opportunity of a short reply. I understood from Dr. Lodge that it was not to the mode of proof he objected, but to the very first equation with which I opened the paper being applied to the subject of induction telegraphy when the frequency was so low. What I am going to say refers only to this point. It was not so stated in the paper that I wrote, but, as a matter of fact, the equations with which I began were, with a certain assumption, to which I shall refer presently, strictly derived from Maxwell's equations. Hence Dr. Lodge's objection must refer either to the assumption or to Maxwell's equations. I will begin with the assumption. Maxwell, as is well known, supposed that the total current in a conductor is made up of the conduction current and the polarisation current. The assumption I made was that the polarisation current might be neglected. That is a question which is very easily calculated, and the result is, provided the frequency is not comparable with that of light, no serious error can possibly be introduced. The frequency I was considering was only about 300, as I have said. Hence I am confident that no error of any importance can arise on this ground. The next point is, Do Maxwell's equations apply? This is a matter entirely of opinion. I may perhaps be allowed to cite one or two authorities which bear me out in thinking that they do apply. Professor J. J. Thomson, for example, in his book, "Recent Researches," has this sentence: "We shall begin "by writing down the general equations which we shall require "in discussing the transmission of electrical disturbances through "a field in which both insulators and conductors are present." And again, in his "Elements of Electricity and Magnetism:" "We owe to Maxwell a theory, now in its main features "universally accepted, by which we are able to completely "determine the electrical conditions not merely in the conductors, "but in every part of the field." Dr. Hertz also, in his book, "Electric Waves," in the English translation, says: "Maxwell's "theory is Maxwell's system of equation." Mr. Whitehead.

Professor J. J. Thomson, Mr. Oliver Heaviside, and Dr. Hertz, in many papers on various electrical questions, all start by using Maxwell's equations, without one hint, as far as I remember, that they are not everywhere applicable. Professor J. J. Thomson, after the two sentences I have quoted, starts with Maxwell's equations; and not another sentence is there, or one word of warning, to the effect that these equations are not really true, not merely for this or that space, or for this or that frequency, but they are the general equations we must use in all parts of the field. So far as I can see, if my equations have to go, Maxwell's equations must go with them. It is not the hour, nor am I the man to defend Maxwell's theory; but this, perhaps, I am permitted to say: If Maxwell's equations are not true, where are the equations we must use instead of them?

Sir HENRY MANCE: I should like to make one suggestion of a practical nature, which, although simple, may be of service to those who are engaged in working out the problem of wireless telegraphy. As I understand, the method proposed to indicate the signals is to cause the distant receiving apparatus to respond to the intermittent transmission of magnetic waves for the long or short periods required by the Morse alphabet; this being so, it will be obvious that, as in all telegraphic apparatus, there is a certain amount of inertia to be overcome, and I think that greater sensitiveness would be obtained if the receiver is caused to give out a steady continuous note, in which the slightest alteration would easily be detected by any telegraphist. Better to explain my meaning, I might mention that many years ago I had to deal with a land line on which communications regularly failed every night, until oil insulators were introduced. It was found that long after signals had ceased by ordinary Morse, and when no current whatever could be detected on the galvanometer, there was still sufficient arriving to affect the humming tone set up by a make-and-break electro-magnet belonging to an electric bell which happened to be in the local circuit, but from which Sir Henry Mance.

Sir Henry Mance. the bell portion had been removed, thus allowing the spring to vibrate freely when the relay circuit was closed, which it happened to be at that time.

I suggest that, instead of endeavouring to produce an intermittent action on the receiver, which in its normal position is at rest, it would be better to have the receiver in constant action—that is to say, vibrating at a high rate, so as to produce a certain note—and then make the signals by acting on the receiver in some way so as slightly to alter the tone emitted.

Mr. Sennett. Mr. A. R. SENNETT: There certainly can be no doubt as to the amount of interest Dr. Lodge's paper has aroused in the minds of all of us who have heard it. We do not know what the future may have in store for induction telegraphy, but one thing we know—that a necessity exists for connecting our lightships with the mainland, either by its means or otherwise. Dr. Lodge has referred to this; and it seems that Mr. Evershed was led to study the question of wireless telegraphy, in a measure, from considering the necessities in this relation. I was therefore very much disappointed to hear him throw a very considerable damper upon that on the score of expense. But, Sir, if there is anything that the English people have reason to be proud of, and are proud of, it is their maritime supremacy; therefore I think we ought not to be daunted by electrical troubles, and certainly not deterred in our efforts by financial difficulties: we ought to go ahead in our experiments until the problem has been solved, and the matter has become a *fait accompli*.

I was very much impressed and surprised by the figure that Dr. Lodge gave us as to the absorption taking place in sea water. Mr. Evershed said he understood Dr. Lodge to say that this was only small. I understood him to say it was no less than 70 or 75 per cent. This being so, we must direct our efforts accordingly. I remember, some years ago, Mr. Wimshurst explaining to me his system of connecting lightships. I thought it very ingenious and practical, and it would certainly overcome the whole of the difficulties arising from absorption. The principal difficulty hitherto experienced has been that of cable abrasion. I will sketch this simple device. What Mr. Wimshurst suggested was simply to put an induction shackle, or swivel, into the cable; this Mr. Sennett. consisted of two disc-shaped chambers. D represents a disc. In that disc is a coil of wire which can be efficiently and entirely insulated—wholly embedded, in fact, in some dielectric, as, for example, a suitable resinous body. E is the other chamber, or half, of the swivel, embedded in which is a corresponding coil—primary or secondary coil, whichever you like to consider it. *Here* is the chain going down. I cannot myself imagine why that should not act, and I certainly think the Trinity House authorities should try such a simple contrivance.

I cannot help thinking that this invention, in conjunction with suitable and sensitive instruments, should solve this problem of communication with lightships, progress in regard to which has not, I venture to suggest, up to the present reflected greatly upon the inventive ability of our country.

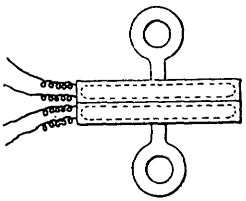

FIG. A.

I was very much interested in Dr. Lodge's paper, because his system essentially turns upon isochronism; and I was particularly impressed because he made the almost startling statement that the sensibility of his system was increased a *million-fold* by the proper tuning of the condensers. I happened to be present at the reading of the paper by Mr. Stevenson in Edinburgh in 1892, to which Professor Lodge has referred. Mr. Stevenson certainly described to us what he actually did—the wires and circuits he used, and everything else in his experiments at the Isle of Skye. I cannot quite assimilate that with Mr. Preece's remarks.

Mr. PREECE: It was near Edinburgh—Murrayfield.

Mr. SENNETT: I ventured to impress upon Section G of the British Association at this meeting my views as to the great advantage to be derived from isochronism, and I said that with regard to the connection of lightships it seemed to me that it was not necessary to employ electricity at all for the actual transmission. Having regard to the enhanced velocity of sound in water, its constancy, and the homogeneity of the medium, I advocated a system consisting essentially of the transmission of mechanical

Mr. Sennett. vibration directly through the water, in conjunction with delicate and sympathetic receiving apparatus, so devised that it could be tuned in accurate unison with the periodicity of the mechanical vibrations thus transmitted.

Such experiments are rather expensive for outsiders to make, but I did what I could in this direction, and was very pleased with the results, and even more strongly impressed with the great value of synchronism and accurate tuning.

I will sketch the instruments I made and used. No electrical impulses passed through the water at all, although electricity was used in the receivers. Mechanical, instead of electrical, waves were used; these may be produced, for example, by means of an electrically driven siren emitting a certain note in the water. A bell, indeed, may be used, for it is simply surprising how far one can hear even slight noises produced under water; and I think the day may come when we shall be able to communicate in this manner from ship to ship in motion at great distances; and I think that such transmission will prove so efficient that, apart from its utility in increasing the sensitiveness of the system, the tuning will be found a necessity for isolating the particular and predetermined note employed.

For the receiver I took a membranous diaphragm similar to those used by Professor Thompson in his telephone. This I put in the mouth of a Helmholtz resonator. On the back of this diaphragm I put a Hughes microphone. This microphone had its battery in the ordinary way, but included in the circuit was an induction coil. The secondary coil of this induction coil was connected to a telephone, and this, again, was mounted in front of a diaphragm similar to the first. I found that it was advisable to put the microphone diaphragm at an angle of about 60°, for this had a kind of damping action on the jumper, as we may call it. My diaphragms were not fitted as in ordinary telephones, but they were mounted more after the fashion of an ordinary kettle-drum. D D represents the diaphragm in the microphone, R the resonator, B the local battery, C the induction coil. The vibratory period of the resonance cavity was known, and could be adjusted to the greatest possible nicety. The fundamental note of this tympanum—as I prefer to call it—was tuned to the resonance of *this* chamber. Mr. Sennett.

It seemed to me to be very essential that a relay, or relays, should be made use of; indeed, seeing that no current or electrical influence was sent through the water at all, the receiver itself—or, rather, that portion of it working in conjunction with its own battery—may be looked upon as a relay.

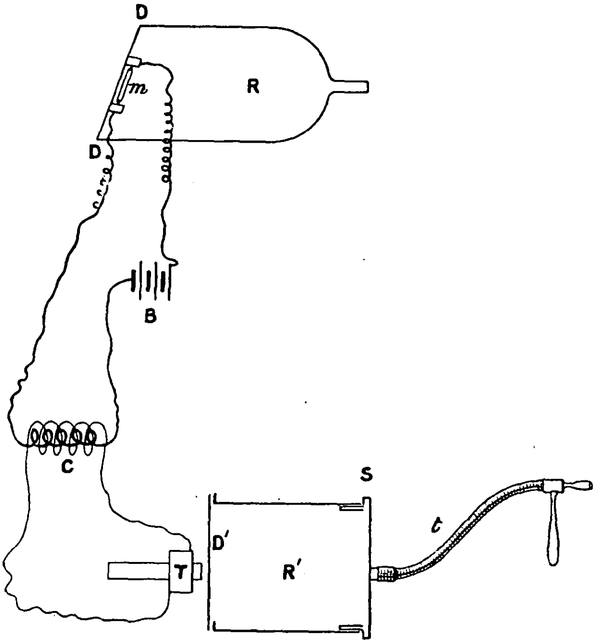

Fig. B.

The receiver proper was very similar. The tympanum, D′, was very accurately tuned to the chamber R′, and was made to form its base instead of covering its mouth. For this chamber I employed an ordinary Helmholtz resonator, which is provided with a slide (at *s*). By pushing this slide in or out the combination could be tuned to the greatest possible sensitivity, with the result that a feeble current of isochronous periodicity circulating in T was made to produce an appreciable sound in the tube *t*. I used a

Mr Sennett. conveniently shaped ear-piece of glass, and I consider that for shipboard use the receivers should be duplicated, and such an ear-piece used in each ear. I feel convinced that, if one were to put on a lightship an arrangement of this kind, and transmit from the nearest harbour—it might be 20 or 30 miles away—the vibrations from a big bell or a powerful siren, one would hear distinctly, and be able to signal by the Morse code. Such a system would lend itself to use between ships in motion; and possibly, by fitting the diaphragm, or an acoustic lens, in a revolving frame, one could, in a fog, by revolving the frame over a divided quadrant, find out the exact direction from which this sound emanated—a matter ot great importance.

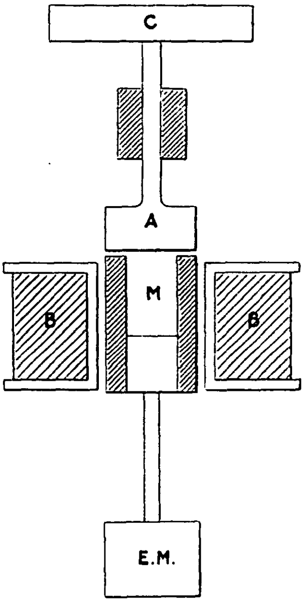

Fig. C.

In common with everybody else—as I should imagine, but speaking for myself—in making these experiments I got very tired and worried over the employment of tuning-forks, and the system with which I am now experimenting is simply this: I do away with all vibrating parts, and use instead a revolving commutator divided up into the requisite number of contact pieces. This is mounted on a vertical spindle, and the bottom of the spindle carries a round disc of soft iron, which forms the armature of an annular electro-magnet, M. This magnet is mounted in its turn on a spindle, and is driven at an approximately uniform rate, but at a speed, say, 50 per cent. faster than it is desired that the commutator shall revolve. The commutator is held back by one of Professor Hughes's very ingenious governors. On account of the armature travelling more quickly than the governor, its brake is always applied to a certain amount, and the instant the commutator drum has a tendency to accelerate in the least the brake goes on tighter, and an uniform velocity is thus obtained, and Mr. Sennett. at the same time all the difficulties attendant upon tuning-forks, reeds, vibrators, mercury cups, and other paraphernalia are obviated.

There are other matters I should like to have said a word upon, but at this late hour I will only just refer to one. Professor Lodge touched upon, and demonstrated to us with his remarkably loud-speaking relay micro-telephones, the characteristic sound which it is well known that every telephone possesses, and which detracts so much from the efficiency of the instruments. I think we might appropriately call this the telephonic "nose-timbre." If we want to know why this sound exists, and want to be put in the way of eliminating it, we have only to consider the construc- of the tympanum of the human ear.

I should like here to point out the difference in the way such instruments should be treated, according to whether it is desired to aggrandise a sound—say, for example, the fundamental note of a musical tone—or to obliterate other and confusing sounds,—in other words, whether it is wished that the instrument should have a powerful and sympathetic fundamental note of its own, or be capable of receiving with almost equal facility a number of sounds of varying pitch.

Our ears are specially constructed for the latter purpose. First of all, in the human ear the diaphragm is oval, not circular; secondly, it is not a "free" diaphragm, for there is glued, as it were, to the back of the tympanum a bone. What is the effect of this? It is to prevent the diaphragm having a pronounced intrinsic fundamental note, for it cannot possess more than two radii of the same length, instead of its being equi-radial as is an ordinary telephone diaphragm. The consequence is, it practically has no fundamental note. Conversely, if the ear could speak, it would naturally have no note of its own—it would possess no characteristic *klangfarbe.*

On the other hand, when you desire to reinforce a given tone, you must deviate from Nature's receiver, and call to your aid all the effects of resonance you possibly can; consequently, I place my diaphragm actually in a resonance cavity, and find its sensibility to be thereby enormously increased.

Mr. Sennett. Therefore I consider I have found that by adopting something of the construction of the human ear one is able to take away very largely from the characteristic sound with which we are familiar in telephones and phonographs.

In conclusion, it would be redundant in me to eulogise Mr. Preece for the amount of energy he has put into the subject. May I ask him to extend this hospitality in labour—I sincerely trust he will—to the system Dr. Lodge has developed? and I am inclined to think that, if Dr. Lodge's system were employed, and the utmost attention given to the tuning of the receiving instruments, such as I have described, whatever kind of prophecy we might indulge in as to the future of induction telegraphy, we should be excused any reasonable degree of rashness. I have for a number of years been convinced of the necessity of efficient tuning and the enormous value of isochronous working, and the great sensibility of Mr. Evershed's reed-call is but an exemplification of this.

Prof. Ayrton. Professor W. E. AYRTON: The only point I wish to refer to at the moment is in connection with the remarks made by Mr. Whitehead. Possibly those present may not have appreciated the importance of the investigation to which he referred. Some three years ago Mr. Evershed brought to my notice the fact that he had made such an extraordinary sensitive relay call as he has shown us to-night, although this is the first time I have ever seen it in its complete form. He stated that for the purposes of his experiment she was able to reduce *to scale* the sizes of the coils that he proposed to place at the bottom of the sea and round the lightship, but what he was not able to do was to settle the area and thickness of a copper sheet that ought to be inserted between the primary and the secondary coils so as to correctly imitate the action of the sea; and he naturally added that until he could ascertain that fact he was unable to carry out the complete experiments which would enable him to predict what would happen when a trial was made to ring a bell on the lightship by the action of a coil placed at the bottom of the sea.

I, therefore, entered into a correspondence with Mr. Oliver

Heaviside on the subject. Mr. Heaviside expressed much interest in the problem, and was good enough to write his views to me. Without going fully into the matter, he said that he anticipated that the absorption of the sea would be small, and that the experiment of signalling inductively through it was well worth trying. Mr. Evershed has told you what the result was; and he has mentioned that there were three causes why the result was a failure, any one of which alone he considers would have been fatal—viz., the sheathing of the cable, the absorption by the sea, and that by the iron of the lightship. Now two of those causes we can dismiss, because we can remove them. As Dr. Lodge pointed out the other night, it is possible to employ a wooden lightship. Also, you can imagine that no iron nor steel sheathing were used with the cable. But what we cannot do is to float a lightship away from the land without having sea under it. The question appeared to me, therefore, to be of great interest; and Mr. Whitehead, who was then one of my students, or had just left me—I forget which—was good enough to attack this problem mathematically at my suggestion. He brought me his results and showed them to me before his paper was read before the Physical Society; so that I am responsible, at any rate for the method used in attacking the problem, although not, perhaps, for the accuracy of all his numerical calculations. Now, if that calculation is wrong *in principle*, I should be extremely glad if Dr. Lodge would point out why it is wrong. In other words, if the Maxwellian equations do not hold in the space between two such coils as Mr. Preece has been using in this room to-night, I should like to know why they do not, and, secondly, what equations do hold. Dr. Lodge told you last time he was here that the discussion which followed the reading of Mr. Whitehead's paper was very briefly reported in the *Proceedings of the Physical Society*, so that it was impossible to say what the discussion really had been. Now this discussion, as far as I remember, consisted not only of remarks made by various people, but particularly of a written communication from Mr. Oliver Heaviside. And in this communication, as far as I remember, he objected neither to **Prof. Ayrton.**

Prof. Ayrton.

the mathematics nor to the fundamental equations. Was not that so, Mr. Whitehead ?

Mr. WHITEHEAD : Yes.

Professor AYRTON : Mr. Oliver Heaviside did not object at all to the equations ?

Mr. WHITEHEAD : No.

Professor AYRTON : And the only point he objected to was the value Mr. Whitehead had employed for the conductivity of sea water. There was, however, no doubt about that value, since it was experimentally measured for the purpose. Mr. Oliver Heaviside kindly suggested another method of attacking the problem, but that is another matter. Up to the time, therefore, when Dr. Lodge gave us his most interesting paper, a fortnight ago, I was under the impression that the equations used by Mr. Whitehead were correct. Now it is all-important to ascertain whether they are, or are not, correct. Is it a fact, or is it not a fact, that in 20 metres of sea water, with a frequency of 300 or 400, whatever it was Mr. Whitehead assumed, there is an absorption of something like 79 per cent.—that the field, in fact, is diminished by 79 per cent. in consequence of the conductivity and permeability of the sea water ? For, if that be the case, then trying to work Mr. Evershed's call *through* considerable thicknesses of sea water seems to be nearly hopeless. The only other point I want to put to Dr. Lodge is to emphasise the question Mr. Evershed has asked, viz., What is the actual amount of power that must be received by the coil attached to Dr. Lodge's first telephone of his train of telephones in order that the last one may act as a call by giving out a loud sound ? Mr. Evershed tells us—and I have no doubt what he tells us is absolutely correct—that if he gives to his instrument 1-1,000th of an erg per second—he did not tell us what that was in watts, but it is something extremely small——

Mr. EVERSHED : Ten thousand million relays can be worked by one watt.

Professor AYRTON : Ten thousand million of his relays can be worked by one watt, or about six hundred thousand million of his relays by the power taken by that glow lamp on the President's table! Will Dr. Lodge, in his reply, therefore, kindly tell us what is the minimum power that must reach the first of his train of telephones? since then we shall be in a position to judge which of the two systems of call is the more sensitive. Prof. Ayrton.

As, doubtless, many others desire to speak this evening, Dr. Lodge will, I feel sure, excuse me if I do not take up time by enlarging on the pleasure and delight we have all experienced in listening to his paper, and on our expression of thanks for his kindness in giving it to us.

Dr. OLIVER LODGE: I am much obliged for the opportunity of saying a few words at this stage, to remove some misapprehensions. Mr. Evershed's paper was an important communication. He has worked at this subject for a long time, but he appears to keep his work dark, and I had not known of it when I spoke at the British Association meeting at Bristol this autumn. I am very glad that he has now brought it forward, and told us what he has done. I did not quite understand his reference to a fairyland, but he rather spoke at first as if I had suggested that space telegraphy would replace wire telegraphy. I never intended to convey that suggestion. I have always said, when asked, that wherever you can lay a wire, you had better do so; because you generally want to talk to one particular person, and there is nothing like having a wire straight from sender to receiver. There are cases, perhaps—such as newspaper intelligence—where you want to shout it all over the country simultaneously, for which space telegraphy may be suitable; but perhaps Mr. Preece would hardly like us to do that by magnetic induction telegraphy, because it would be liable to disturb the telephones—although by the way, I am not sure that he cares whether they are disturbed or not, except the trunk lines. With regard to the power required for the Evershed "call," I stated (quite truly) that his call takes a good deal of power at the sending end to work it. To this Mr. Evershed retorts, very fairly, that my signalling, as reported in my paper, takes a good deal more. I admit that, so far as I have hitherto had it working at Liverpool, it has taken more power at the sending end to work a telephone at the distant station than with the much longer base Dr. Lodge.

Dr. Lodge. wire at Lavernock he has had to apply in order to work his call at Flat Holm, or *vice versâ*. I do not admit that I needed, or got, more power at the receiving end. Moreover, much less sending power is now sufficient; and with the receiving apparatus which I showed you last time I feel convinced, from the result of my experiments in the laboratory, that the power that is used at Lavernock for giving the audible telephone signals—which is quite a small power, and is quite insufficient and unsuitable to work Mr. Evershed's call*—will be amply sufficient and suitable to work the call that I propose, by aid of the magnifying arrangement which you saw at work last time. I should like to be able to answer the question at once, how many ergs are required to work the first of my magnifying telephones, but I have not got the data to hand at the moment. I will take care, however, to be able to answer that next time. It is evidently a question that demands an answer, but I must say that certainly the power will be exceedingly minute. It is very much less than is wanted to make an audible sound in an ordinary telephone, for instance, and that is pretty small. I prove this by using a pair of induction coils such as happen to be here: one can get a true zero or a graduated effect by tipping them more or less at right angles to each other. I supply a gentle current to one of them from an alternator whose field magnets are not excited, and I connect the other coil either with the first telephone of the magnifying series, or with an ordinary Bell or Ader or Collier telephone. Suppose then that I arrange this ordinary telephone so that I can hear it about as loud as they have it at Lavernock: I can then tip the secondary coil up and up so that the sound shall get less and less, until you cease to hear it altogether. I then tip it up a little more, but not up to zero, and I switch it on to the series of magnifying telephones. In the last of the series I can still hear the signals easily, even loudly, humming out into

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* *Note added* 31*st December*, 1898.—Near the end of Mr. Evershed's paper, he speaks of the power available for a telephone at high frequency being greater than that for his call at low frequency; but this is a mistake, if it refers to the actual Lavernock conditions: the self-induction part of the impedance is several times as great as the simple resistance at high frequency.

the room. If I continue to tilt the coil, it continues to act on the magnifying telephone more and more feebly until I get it actually into the neutral position: then the sound suddenly stops; but give one of the coils the least tilt and it is heard again. Dr. Lodge.

I would also say, concerning the speed of signalling with a tuning-fork arrangement, that, though I was afraid it might be rather slow—I was afraid that a tuning-fork receiver would take a little time to work up its swings—yet I find that, using 300 or 400 alternations a second it takes appreciably no time to work up its swings, and however fast the key is worked the series of magnifying telephones respond and the signals come out quite clear and sharp. You cannot tap the dots quickly enough to blend them indistinguishably into one another, so long as they are at all reasonably strong. If they are too weak this is what happens: When the signals are purposely made altogether inaudible, you hear in the magnifying telephone nothing but the usual growl of the microphone; but when you tilt the secondary coil down sufficiently you begin to hear the musical notes of the signals coming; as you tip it up again the signals begin to fail, merging into the growl, and there just comes a point when you cannot detect the signals. You can hear the noise is there, but you cannot signal properly. I do not know that one can really give the exact number of ergs needed for a signal, because it varies considerably with the conditions of the adjustment of the telephone, but in any case it must be extraordinarily small. I propose to modify the telephone from what I showed last time here, by making the upper prong carry the screw, and the other prong carry the other carbon; *i.e.*, the top carbon of the microphone is to be carried by one prong, and the bottom carbon by the other prong. When the fork vibrates properly the microphone will be put into action, but any slight mechanical jarring of the fork as a whole will not be so likely to affect it; thus to some extent imitating an ingenious feature of Mr. Evershed's two rectangles, which are admirably arranged so that a mere mechanical disturbance throws both rectangles up and down together, while the proper electrical disturbance you want to hear vibrates them in opposite phase and so brings them into

Dr Lodge momentary contact. Mr. Evershed's rectangles are thrown into a state of very visible motion, but in my call I do not want any perceptible motion : I only want motion enough to work a microphone—excessively smaller than anything visible, or than anything needed to work any possible kind of discontinuous relay.*

There are some things I should like to have said with reference to Professor Fleming's interesting remarks with regard to the other, the Hertzian, method of space telegraphy, but I think I will not speak of them now. Perhaps they hardly belong to the present subject, which I take to be limited to the magnetic system. I would just say, however, that his remarks about the inadequacy and uselessness of a partially earthed wire I do not wholly agree with. Dr. Muirhead and I have made experiments on an earthed wire, sending what I call an electrical jerk, or, if he prefers it, a very sudden rise of potential, through a wire not insulated, or very imperfectly insulated, or through gas pipes, and I have been surprised at what a considerable distance a coherer at the other end can feel some trace of effect. If the wire is very clean, as possibly it was in Dr. Fleming's interesting experiments in Regent's Park, no doubt a perceptible disturbance does not go far ; but an ordinary coat of oxide insulates to some extent. What one means by a bare wire is a wire roughly and imperfectly insulated—not insulated with gutta-percha, anyway—and with such a wire, I believe, by the method of jerks and a coherer, a good deal more can be done than his remarks would lead you to think.

Before I forget it, I wanted to thank Mr. Niblett last time for lending us the batteries which were used for the experiments. Mr. Niblett was good enough to send over to this hall a number of his convenient portable secondary batteries, and I quite omitted to thank him for doing so.

Professor Ayrton says you cannot have a lightship except on the sea, which is true ; but you need not put the coil that you are going to use for signalling to that lightship at the bottom of the sea. That is by no means the best place to put the coil. You can

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* *Note added later.*—Nevertheless, the motion is sufficient to give an appropriate back E.M.F., as shall be shown in a later reply (page 930).

keep it in the dry, and thereby do away a good deal with the absorptive effects of sea water; though, of course, if the ship be made of iron, or of any other metal, it will screen itself more than any sea water can, even though the receiving cable be outside the ship. Dr. Lodge.

Now we come to Mr. Whitehead, and I am particularly anxious to remove an impression into which Mr. Whitehead seems to have fallen. I only read his Physical Society paper* in the train coming down last time, and it reminded me of a question —which I fancy had been half raised in my mind before—about the calculation of the opacity of a conductor. I am afraid I may have expressed myself hastily, as if I desired to say that his equations were wrong; I did not speak guardedly enough. Mr. Whitehead is a mathematician, and you had far better take his equations and his results as correct until somebody substitutes something else for them; but I do find that this calculation is made by different people in what seems to me different ways, and, if I may be pardoned for indicating the kind of thing I mean, I will write down the equations which Maxwell uses where he shows us how to tackle this problem of electro-magnetic waves passing through a conducting medium.

[*Here an abbreviation of "Maxwell," vol. ii., section* 798, *was written on the board.*]

That is not quite the same as Mr. Whitehead's calculation, nor is it quite the same as what Professor J. J. Thomson uses for a partially similar case. My feeling was that this theory of Maxwell's could not possibly apply to all frequencies. The case treated by Maxwell is a slab with a source of light on one side and a detector on the other, and he is attacking the problem of the transmission of light through a partial conductor, and calculating the absorption of energy to be expected. He makes it depend on the ratio of p, the frequency constant, to q, the wave-length constant; and p/q equals the velocity of light. Now this velocity is nearly the same for all frequencies; so at once you get the logarithmic decrement of the

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* *Proc. Phys. Soc.*, 1896-97, or *Phil. Mag.*, August, 1897.

Dr. Lodge. disturbance the same for all frequencies. Hence that slab will be equally opaque whether those vibrations are the rapid vibrations of light or the slower vibrations of Hertz waves, or any other electro-magnetic vibrations. But we know experimentally, as well as by common sense, that a conductor is not equally opaque to all these waves, and that long waves can go through where short waves are absorbed; because, if a conductor wipes out a certain amount of energy at each swing, then, if there are three pulsations in its thickness, there will be a certain percentage wiped out If there are 30 pulsations in its thickness, there will be a great deal more wiped out; and if there is only half a pulsation, then there will be but a small fraction of amplitude destroyed. The opacity must therefore depend on the wave-length as compared with the thickness. Now Mr. Whitehead, instead of getting Maxwell's result, gets something much more reasonable: he finds that the opacity depends on the square root of the frequency; treating the matter as straightforward diffusion. But then Professor J. J. Thomson, who ten years ago treated of a case very like this—viz., a circular oscillator on one side of a liquid layer and a circular resonator detector on the other side, where, however, the circuits are comparable in size to the wave-length —gets a thing like this:—

[*Quotation from "Proc. Roy. Soc.," vol.* 45, *p.* 268.]

Thus you see that, whereas Mr. Whitehead gets an opacity proportional to the square root of the frequency, and Maxwell gets something independent of the frequency, Professor J. J. Thomson, for a case almost identical with the problem before us, though with far more rapid vibrations, gets something apparently proportional to the frequency; hence I do not feel that we have got to the bottom of this problem; though I by no means maintain or suggest that these investigations differ irreconcilably, or that there may not be a way to reconcile them all.*

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* *Note added* 31*st December*, 1898.—In the discussion on Mr. Whitehead's paper at the Physical Society, as reported in their *Proceedings*, appears a brief statement from Mr. Oliver Heaviside, which represents all that was published of a "note" of three or four pages sent by him to the Physical Society, a copy o

Our present problem of two coils on dry land is, however, not the same as any of those hitherto treated. It is no question of a conducting slab interposed between the two coils, but of a conducting mass in their neighbourhood. The problem is not one of opacity, but is more like the problem of a Hughes induction balance disturbed by the neighbourhood of a conductor. Last week I wrote to Professor J. J. Thomson suggesting to him this problem—not the same problem that Mr. Whitehead attacked, with one coil buried in the sea, where I admit the absorption may be very considerable. (Indeed, in every case I think the conductivity of the earth may give us trouble—I said so in the paper. I regard the matter as one of importance, but one that can certainly be solved by mathematicians.) The problem I put was this: One horizontal coil is elevated on posts at a small height above the earth at one latitude; and another horizontal coil, also at a certain height above the earth treated as a badly conducting sphere, is at another latitude: find what is the damping power of the earth's conductivity on the mutual induction between coil and coil. Just as I came into the room, I received a letter from Professor J. J. Thomson to say that he had worked it out, and giving in general form the solution; it is rather complicated, and he has not yet reduced it to arithmetic. Dr. Lodge.

Professor AYRTON: This problem or the other one?

Dr. LODGE: This problem.

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which he has to-day kindly sent me. It contains a complete solution of the problem—at least, when the wave-length is large compared with the source—including both Maxwell's and Mr. Whitehead's as special cases. Its essence will be found in Mr. Heaviside's "Electrical Papers," vol. ii., page 422; see also "Electro-magnetic Theory," page 452. There are two cases—one where inductive capacity dominates, the other where conductivity dominates. On looking at Maxwell again, I see that he quite realises the two cases, and treats them separately; though, as it seems to me, he applies the wrong one to gold leaf, evidently thinking that from the high frequency of light it would be the one applicable. Mr. Heaviside treats the two cases together—*i.e.*, for all frequencies and all conductivities—in a general manner. His paper is dated November, 1887; so all this was done before the era of Hertz.

I propose to communicate a note to the Physical Society on the matter, because I reckon that the hitherto extremely discordant experiments on the opacity of gold leaf are more nearly accordant with the more general theory, and may possibly lead to interesting results or suggestions concerning the specific inductive capacity of gold.

Professor AYRTON: And he does not know what it will come to?

Dr. Lodge. Dr. LODGE: No; he has no figures as yet: everything is in series and spherical harmonics, and even the form of the solution may depend on what conductivity you assume for the earth; but by postponing the discussion he will no doubt have finished it, and perhaps he will communicate it either directly or through myself.

I thank you, gentlemen, for letting me say what I have had to say so far.

On the motion of Mr. W. H. PREECE, the discussion was adjourned to Thursday, January 12th.