record, not only the above-mentioned 43-minute oscillations, but also a series of much more rapid oscillations having a period of about 35 seconds (the latter figure being obtained by a stop-watch). The width of the basin is, at the point of observation, 2030 feet, and its mean depth about 100 feet (the tidal range is less than 2 feet). Calculation by the above formula gives, for first partial vibrations, a period of 35·8 seconds, which is very close to the 35 seconds observed; but it is to be noted that for a basin whose depth is *not* small compared with its width, the above formula is somewhat in error, and a more correct formula (involving a hyperbolical cotangent) gives 37·5 seconds. In any case the agreement is striking, and the two instances given are almost conclusive as to the explanation advanced. Simultaneous observations on opposite sides of such a basin would be quite conclusive, but these I have not yet had an opportunity to make.

(4) The following notes on the other cases referred to in (1) may be of interest in connection with observations referred to by Capt. Thomson. Quaco is only about twenty miles further up the Bay of Fundy than St. John, and yet, while expecting to find there a period similar to that at St. John, I found a period of 12½ minutes (from four separate records). This was at first puzzling, but, later, an examination of the chart showed the existence of a dangerous ledge (the Quaco Ledges) coming nearly to the surface, at a distance of eight or ten miles off shore, and forming, with a headland above Quaco and another below, an irregular basin, the dimensions of which no doubt determine the period of oscillation. A quite similar explanation applies to the oscillations at Halifax, for there a succession of banks (the Emerald, Sable Island, Le Have and Roseway) form, with the Nova Scotia coast, a large-sized bay of irregular shape.

(5) As to the external impulse that starts the oscillations, there is much uncertainty. Marked oscillations at St. John are frequently accompanied by barometric disturbances, but not always. My own observations make me believe that the oscillations and heavy ground-swell usually coexist. A notable case (for which I have to thank Mr. S. W. Kain, of St. John) occurred on September 18, 1898, when the heaviest ground-swell in several months was accompanied by marked periodic oscillations recorded on the Kelvin gauge. On the whole, I believe that the disturbance of equilibrium is due either to abrupt local variations of atmospheric pressure, or to the transmitted effect of a distant hurricane.

Those who are interested in this subject will find fuller details of the cases here discussed numerically (and also a short bibliography of the subject) in a paper by myself in the *American Journal of Science* (vol. iii., 1897). The nature of the oscillations in the Gulf of St. Lawrence are shown on curves that illustrate a tidal report, by Mr. W. Bell Dawson, in the last volume of *Transactions* of the Royal Society of Canada (see NATURE, vol. lviii. p. 260). A. WILMER DUFF.

Purdue University, Lafayette, Ind., U.S.A.

GENERAL LAW OF THE PHENOMENA OF MAGNETIC PERTURBATIONS OF SPECTRAL LINES.

IN the *Philosophical Magazine* for April 1898, I pointed out that the resolution into triplets, &c., which the spectral lines suffer in a strong magnetic field, did not appear to follow any obvious general law, but appeared to be some complex function of the wavelength. To this was added the following remark:—"It is possible, however, that the lines of any one substance may be thrown into groups for each of which $\delta\lambda$ varies as λ^2, and each of these groups might be produced by the motion of a single ion. The number of such groups in a given spectrum would then determine the number of different kinds of ions in the atom or molecule.

"Homologous relations may also exist between the groups in different spectra, but all this still remains for complete investigation."

Although this investigation is still far from complete, yet the measurements so far made uniformly go to show that the foregoing expectation is about to prove true, and that e/m or $\delta\lambda/\lambda^2$ is the same for the corresponding lines of the natural groups in the same spectrum, and, further, that this quantity remains the same for corresponding lines or groups in the homologous spectra of different substances.

Not only is the magnitude of the magnetic effect governed by the foregoing law, but the *character* also of the effect is the same for the corresponding lines; and this is very interesting, as it shows that the corresponding lines probably arise from the same origin. The theory is consequently verified by the facts when the spectral lines are considered in groups corresponding to the molecular events which produce them.

THOMAS PRESTON.

COAST-TELEGRAPHS AND SPACE-TELEGRAPHY.

THE year 1898 was an important period in the history of space-telegraphy, it was the period in which the possibility of being able to signal across wide stretches of open sea, with certainty in all weathers and at high speeds, became first generally recognised as practicable. Within the year the final report of the Royal Commission on the question of Coast-Telegraphs, published late in 1897, came into our hands; and the last few months of the year witnessed a truce to the war of "wireless-telegraphy." A wave of good feeling has now united the opponents into something like coherence, and the honours have been divided with universal approval. The result is that for the future Italy takes prominence, England eminence, while Russia, Germany and France share the luxury of many grievances.

The close of the year is very appropriately characterised by three papers, respectively communicated by Dr. Lodge,[1] Mr. W. H. Preece,[2] and Mr. S. Evershed,[3] to the Institution of Electrical Engineers, all emphasising the merits of one and the same system of space-telegraphy. The authors themselves were more or less unanimous as to the course further experiment should take, but the discusssion that followed the reading of these papers showed a certain lack of directness; many of the speakers were carried away by side issues, and a great deal of time was occupied with ill-considered suggestions and old matter. While fully recognising the value of open discussion, and of hints thrown out at random on subsidiary matters, the present writer thinks it may be useful here to indicate the limits to which the problem may be narrowed down, and to point out the very serious work that is now calling for the aid of space-telegraphy.

It is very generally admitted that space-telegraphy will replace metallic-circuit systems only under conditions where metallic circuits are impracticable. The fact that metallic circuits have been laid over the Andes, may be taken as proof that there are remarkably few land-areas that cannot be spanned by wires. For communication between *fixed* points on rough coasts, a wire suitably protected is still the right and the best thing, as is evidenced by the cable[4] laid in 1890 between Pollagill Bay on the north-west coast of Ireland, and Portdown Bay, Tory Island, and thence by duplicate underground cables to the lighthouse on the north side of the island. The great advantage of a metallic-circuit system is the consequent privacy of the messages, the simplicity of the apparatus, the speed of transmission, and the possibility the system offers for working by telephone, and in other ways avoiding the expense of skilled operators. Space-telegraphy is at present limited to comparatively short distances, and its usefulness is confined to spanning estuaries, skirting sea-boards, and for such purposes as

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[1] "Improvements in Magnetic Space-Telegraphy."

[2] "Ætheric Telegraphy."

[3] "Telegraphy by Magnetic Induction."

[4] See an important paper by Mr. H. Benest, "Coast-Telegraph Communication," read before the Balloon Society, March 18, 1892).

that to which it was applied by Mr. Preece between Oban and Mull in 1895. But, except in rare instances, it is only likely to replace submarine cables between moored vessels and the shore, *e.g.* between light-ships and the coast-guard stations. The point of failure in submarine-cable communication thus happens to coincide with the point of favour of space-telegraphy. The present object of those working at space-telegraphy should therefore be to supplement the cable-system of coast telegraphs, so that all the light-vessels and lighthouses of our coasts may be brought into communication with one another and with the life-boat stations.

Some idea of the scope of the work of completing our coast-telegraphs may be gathered from the fact [1] that the whole number of distinctive lights, including port, harbour, and pier lights, light-vessels, and lighthouses on the coasts of the United Kingdom in October 1898 was 1095, and at the beginning of that year only[2] 51 light-vessels and light-ships were in communication with the telegraphic system. Three additional lighthouses, *i.e.* those at Godrevy, the Skerries, and Walney Island, were connected to the shore during last summer; but pending [3] further results of the experiments with the system of "wireless-telegraphy," it was thought better to postpone the work of connecting more light-ships to the shore by electric cables. These lighthouses and other sea-marks are maintained out of the fund [4] derived from lighthouse tolls, which amount to over 500,000*l.*, the lighthouses, &c., being thus self-supporting. This fund has lately been augmented by a special grant; with a view to increasing the efficiency of the system.

The hard task of establishing communication with a light-ship moored in a tide-way, successively rising and falling at each tide, swinging, rolling, and pitching, and in other ways gesticulating around her uncertain anchorage, has taxed the resources of cable engineers for the last quarter of a century. The system now generally adopted consists in the use of a swivel[5] or toggle having a hollow spindle so as to allow a telegraph-cable to be passed up through the middle of it. This swivel is placed between the ship and her moorings at about the water-line. One or more chains lead down from "eyes" on the swivel to the mooring anchors; there are generally two such chains, or there may be three, spread out tripod-fashion, to terminate in mushroom anchors. A riding-chain leads upwards from the swivel, and is secured to the vessel in the ordinary manner after passing through the hawse-pipe; the cable is hauled on board through the hollow swivel, over a sheave and on to a drum, which can be turned round by hand as required, to take the twists or "turns" out of the cable as they are formed. A special veering-drum has been designed by Mr. Benest,[6] by means of which this operation of taking out "turns" is performed automatically by a special gear on the drum. Electrical communication is maintained through the axes of the drum by brush-contacts or otherwise. In rough weather the riding-chain is generally veered out, sometimes up to fifty or sixty fathoms; and after a gale it is often found in tangled masses, which are sometimes as large in girth as that of a man. The cable itself, if it has not been entangled, has meanwhile been saved from kinking by the swivel device, but it suffers a good deal from the threshing and flattening against the bottom, from friction at the swivel, and occasionally from fouling the chains. An ingenious type of veering-cable has been designed by Mr. F. C. Crawford,[7] which is built up in such a way that it is very difficult to make a kink in it. To secure this result, the stranded conductor of seven tinned copper wires is covered with india-rubber and lapped with cotton tape to make a bedding for the sheathing wires. The sheathing consists of twelve double-tinned steel wires, each coated separately with india-rubber and tape. Finally there is an outer serving of india-rubber, which encloses everything into a compact form, and, while allowing the requisite amount of flexibility,[1] prevents the sheathing wires from slipping over one another when the cable is bent. As a further protection, especially against the "threshing" action, this veering-cable is occasionally threaded with beads or ferrules of lignum vitæ, or with washers of india-rubber.

Another metallic-circuit method has been tried at Sandy Hook,[2] in which use is made of the mooring chains themselves as a means of connecting a telephone in circuit from the vessel to the cable. This method is reported upon very favourably by its designers, Messrs. Blake and Caldwell, but in their account of it the conditions are not very fully elaborated. The present writer made some experiments in this direction some years ago, and came to the conclusion that the conductivity of such chains was too variable to be trusted. His tests showed that a mooring-chain of 2-inch links and $\frac{1}{2}$-inch iron, 12 fathoms in length, after being pickled in tar, dried, and slung up in the air, had a resistance of 9 megohms when tested with 100 volts; when tested with 750 volts the resistance broke down to 400 ohms. A similar chain, dry and somewhat rusty, had a resistance of 850 meghoms. When dipped in water and again slung up, the resistance of the 12 fathoms was 25,000 ohms. Chains sagging in water were able to transmit telephone currents with a battery of a few volts. Two mooring-chains, of the same dimensions as those described above, were carefully tarred, and the contacts at the links were cleaned bright over a small area. The chains were then paid out from the shore parallel to one another, 8 feet apart, in about 2 fathoms of water in the Thames, the distant ends being hauled on to a barge. Telephoning between the barge and the shore was impossible, even with 100 volts in the circuit. All the electricity passed from chain to chain by way of the water. Brass chains had, of course, much lower resistance than the iron chains; the resistance of brass chains was practically the same, wet or dry. But it was always found that with any chain whatever in the circuit, the loose contacts caused "buzzing" [3] in the telephone. It is possible that Messrs. Blake and Caldwell used chains under great stress, or that they used some special device, but in view of his own experiments the present writer does not regard the chain system as a practicable one.

Some years ago an alternative method of connecting a light-ship to a telegraph cable was suggested by Mr. James Wimshurst. He arranged a swivel which contained two flat coils, a primary and a secondary, placed one over the other, so that one could rotate coaxially upon the other, for the purpose of avoiding kinks as the ship swings about. As a matter of fact, the danger does not depend so much upon the twist, as upon the threshing action due to the rise and fall of the cable. Mr. Wimshurst's suggestion is worthy of a trial, but it must be remembered that the swivel, in the form proposed by its designer, meets only a very small part of the difficulty.

This short review of metallic-circuit and allied direct-cable methods, serves to show that the attempts made in this regard by the Royal Commission have only resulted in partial success.[4] While admitting that the problem is now fair game for the space-telegraphers, the present

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[1] *Liverpool Mercury*, October 26, 1898.

[2] Report of the Royal Commission, September 1897.

[3] *Times* House of Commons Report, July 1, 1898.

[4] *Imperial Institute Journal*, March 1896.

[5] See Bedwell's Patent, No. 367, 1876.

[6] Patent, No. 19,646, 1895.

[7] Patent, No. 21,657, 1895.

[1] This cable is in use at Formby Light-ship, with very satisfactory results.

[2] Annual Report of the Lighthouse Board of the United States, June 30, 1895.

[3] See *Electrical Review*, vol. xxvii. p. 57 and p. 656, 1890; also May 14, 1897.

[4] *Standard*, Friday, February 1, 1895: "The Ramsgate life-boat and tug *Bradford* proceeded to the Goodwins, in response to signals of distress from the lightship; the lightship telephone having, it is understood, got out of order."

writer would point out that the resources of cable engineers in devising direct metallic-circuits are by no means exhausted. There is a singular lack of evidence on this point in the Blue Books; the Royal Commission seems to have swung about a good deal on its own small swivel, with something very like disregard for outsiders. For some reason, perhaps unknown, they failed to call as witnesses the very men whose experience would have made for success. In departing from these older methods, it must not be forgotten that we are departing from all the advantages offered by the telephone; skilled operators will have to be requisitioned, and there will be need for a delicate device for "calling-up" the operator.

The fourth Report (dated May 12, 1896) of the Royal Commission concluded with a very hopeful note on a contemplated trial of an "inductive method" suggested by Mr. Evershed. "Under this system," it says, "the cable running from the shore is laid in a circle on the bottom of the sea immediately under the light-vessel, the circle having such an area that the vessel will always be within the circumference of it. Round the deck of the vessel a number of 'turns' of insulated wire are coiled, which are in connection with a telephonic receiver on board ship. This system has not yet (May 1896) been subjected to a practical test at a light-ship."

The fifth and final Report of that same Royal Commission (September 1897), referring to the trial, observes that "the experiment was carried out in August 1896 at the Goodwin (North Sand Head) light-vessel, but after a careful trial it proved a failure. The apparatus had been tested on shore with satisfactory results, but when it was tried at the light-vessel, which is moored in ten fathoms of water, it was found almost impossible to effect communication by means of it, the electric energy being almost entirely lost in the sea. The difficulties experienced were entirely electrical."

In the following year a mathematical investigation as to the cause of this failure was undertaken by Mr. C. S. Whitehead (*Proc. Phys. Soc.*, vol. xv. pt. xi. pp. 188–200, 1897); it was communicated to the Physical Society in a paper read June 11, 1897. His theoretical results show that if his calculations are correct, the normal magnetic induction of the primary coil loses 79 per cent. of its initial value in passing to the secondary through ten fathoms of sea-water. These figures as to the absorption-factor of sea-water have been criticised by Mr. Oliver Heaviside and by Dr. Oliver Lodge. In the meantime Mr. Evershed has turned his attention to improving his apparatus. In his recent paper he regards the inductive coils as a particular case of a dynamo-electric machine, he neglects the absorption-factor, and gives an expression for the power available at the receiving station for ultimate conversion into motion at the receiving instrument. The result arrived at is that the mechanical power is independent of the number of turns into which the total volumes of copper are divided on the primary and secondary circuits, and that the two circuits should have equal volumes of copper. His assumption at the outset, that the use of two horizontal circuits implies a loss of one-half the mutual induction between them, may have to be modified. Perhaps the most important part of Mr. Evershed's paper is his description of a "call" or receiving apparatus, which enables exceedingly minute currents to be detected. From a remark in his paper, he does not appear to be aware of the work that has been done in America by Lucien Blake and Eugene Caldwell in their attempts to provide an instrument to replace the telephone. In the Annual Report of the Lighthouse Board of the United States (June 30, 1895, p. 37) will be found an account of a calling device. This Report says: "The receiving apparatus or relay for this system would be vibratory in character and tuned to a frequency of vibration to correspond with the period of the calling current. In addition to this mechanical adjustment, the electrical circuits might be adapted by the use of condensers and inductances to respond more readily to alternate currents of the same period. The action of such a system would be cumulative, *i.e.* each successive impulse of current would arrive just in time to increase the vibration in the relay until sufficient amplitude would be obtained to operate a circuit-closing device." This report also touches upon an interesting point raised by Sir Henry Mance in the recent discussion. With regard to the sensitiveness of "calls," it observes that "a careful study of all kinds of instruments in which weak alternating currents produce mechanical movement, shows that by far the most efficient are those in which the current does not produce an alternating magnetic field, as in the electrodynamometer, but operates to increase and decrease the strength of field of a permanent magnet. The mechanical motion produced by such polarised machines is always vibratory. Careful measurements in the laboratory show that this relay will work positively with a current representing 0·0001 watt, and that it can be operated with much less energy than this." This report seems to have escaped the attention of Mr. Evershed; he suggested in his recent paper that so far as he was aware the ordinary Bell telephone has been exclusively used as the receiving device in this class of experiments. There is, of course, no question as to priority; Mr. Evershed was using a vibratory indicator on this synchronous principle in 1892, the American report refers to an instrument designed in 1895. In his latest (1896) form of apparatus, Mr. Evershed duplicates the vibratory metallic rectangles; they are connected to two separate secondary circuits in such a direction that they oscillate in opposite phases in a strong magnetic field. This arrangement has the advantage that when the twin rectangles are in unison it is almost impossible to bring them into contact by shaking the instrument.

Mr. Preece's paper is a history of the experiments made by himself and his staff, since 1885, on "the electromagnetic form of ætheric telegraphy," *i.e.*, on the method of signalling between one alternate current circuit and another. This work has been more or less familiarised to us by the newspapers. He used horizontal coils of large diameter at the sending and receiving stations, but they were regarded as "impractical things" and they were replaced by straight conductors, placed in parallel planes, one at each station. Capacity and self-induction were eliminated. An interesting series of experiments is described, in which two earth-plates are buried at a distance apart in the earth; the lines of electric "flow" are traced, and the locus of a hypothetical resultant-conductor is plotted. Incidentally, Mr. Preece makes the curious statement that "we know *by Ohm's law* that the resistance of a circuit increases with its specific resistance and length, and diminishes with its sectional area." By Boyle's law this involves a misconception! Of the various arrangements of inductors for his mode of signalling, Mr. Preece prefers parallel wires connected to earth at each extremity, the wires being carried to a considerable height. The most satisfactory results were obtained over a distance of 3·3 miles across the Bristol Channel; and when it is remembered that the speed of signalling is practically as high, and that the system is as certain and as efficient as the ordinary metallic circuit system, this result must be regarded as the best so far brought to notice.

The question naturally occurs to us at this point, What then has become of the "coherer" systems? Mr. Preece[1] has recently said that the Marconi system is able to traverse a distance of twenty-five miles; but on the same occasion he mentions that there would be no difficulty in communicating by the alternate-current inductor system over a similar distance. Hence there

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[1] *The Engineer*, November 25, 1898.

is little to choose on the merit of distance. On the other hand the speed of signalling by the "Marconi" system is limited to something like twelve words a minute, and we must conclude that it is this circumstance that handicaps the method. It is also probable that "coherer" systems are at present too susceptible to mechanical and fortuitous electrical tremors for the ordeal of a telegraph office, but in the absence of evidence it is perhaps scarcely fair to draw comparisons in this regard. According to the *Times* of Monday, the 9th inst., arrangements are being made under the direction of Signor Marconi at the South Foreland lighthouse, and aboard the South Goodwin light-ship, for a series of experiments with his apparatus. It is stated that if the system is found satisfactory it will forthwith be adopted between those points —the distance between them is about three miles.

Two articles in the *Electrician* of November 12, 1897, one by Dr. Lodge[1] and the other by Mr. A. C. Brown, should be referred to as indicating the extent of the work done and the hopefulness of the votaries of "coherer" systems at that time. Later developments of the "coherer" system, particularly in the matter of syntony and the best arrangement of contacts, are dealt with by Dr. Lodge in a communication to the Physical Society of January 21, 1898, an account of which appeared in NATURE in February 1898. The possibility of individualising signals by syntony on Dr. Lodge's system is discussed in an excellent article in the *Electrical Review* of August 19, 1898, which is prefaced by some remarks that already show the weak points of "coherer" systems. Messages had then been sent by Marconi over a distance of about sixteen miles, and received in "dot and dash" on the Morse ribbon at a rate of something under twelve words a minute; but it had been found in practice that the principle of resonance could not be applied with sufficient effect to ensure that messages should be recorded only by a single selected receiver. Whether the later more elaborate methods of syntony for "coherer" systems, proposed by Dr. Lodge, have justified themselves in practice is not yet known, but it is a significant fact that Dr. Lodge's most recent paper on the subject of space-telegraphy makes a distinct departure from the "coherer" system, and contains no account of experiments in the direction of syntonised receivers used in this particular way.

In 1898, experiments as to the mysterious connection between "coherers" and photo-electric phenomena made little progress towards a practical system of telegraphy. In the hands of Prof. Minchin these experiments, at the outset, showed great promise; they gave us the "collecting wire" which, protruding into space, acts as a "feeler" for Hertz waves, but at present they remain as toys of the laboratory. The so-called "Lichtelektrische Telegraphie" of Prof. Zickler, of which a very good account is given in the *Elektrotechnische Rundshau*, No. 21, pp. 232-233, 1898, is more pretentious as a means of signalling. Prof. Zickler makes use of an old discovery, due to Hertz, that ultra-violet light is able to reduce the spark-resisting power of an air-gap. He directs a beam of light from an arc lamp, provided with a shutter, upon the extended spark-gap of an induction coil, and by opening and closing the shutter causes sparks to pass at the gap at corresponding intervals of time. These sparks affect a "coherer" circuit, and signalling is rendered possible. Such experiments are very attractive from a purely scientific standpoint, but it is not easy to see the advantage of this method as compared to the ordinary heliograph. Using the simplest form of apparatus, Prof. Zickler succeeded in signalling by this means across a space of two metres; and by the aid of an arc lamp that expended nearly two horse-power, and by the use of quartz lenses, that distance was extended to 200 metres. The speed of receiving the signals is not mentioned; the extreme limit is probably about twelve words a minute. He proposes to apply the system to lighthouses and to fortresses. We admit that such a system would be admirably suited for the purpose of warning our *foes* off dangerous parts of the British coasts; but for the purpose of warning our own or friendly ships, a surer means of communication must be adopted.

Comparing the various methods,[1] and keeping in mind that we are here concerned with a practical question for engineers, rather than with the scientific aspect of space-telegraphy, it would seem that the "coherer" systems as a whole are about to be cast aside, and that preference is to be given to alternate-current inductor systems for coast-telegraphs. If this is indeed to be the case, the problem is greatly simplified, and experiment resolves itself into the single task of finding the best design of apparatus for communicating between a moored light-ship and the shore by means of inductor coils. Dr. Lodge's paper on "Improvements in Magnetic Space Telegraphy" gives us what may be regarded as the academical aspect of that task; he describes a new receiving device for magnetic induction telegraphy, and he explains the method of putting it into practice. The principal feature of this device is the outcome of an electric resonance experiment first described in NATURE, vol. xli. p. 368, eight years ago—namely, the experiment of syntonic Leyden jars. Dr. Lodge now replaces the jars by condensers, and the "tuned circuits" take the form of horizontal coils of wire. The inducing coil is connected to an alternate-current dynamo, and the induced coil is connected to a train of telephone relays, the last of which is thus set into violent action at each received impulse. The paper includes the theory of two such circuits arranged in mutual syntony, and it is full of information and suggestions as to the proper course future experiment should take; the advantage of syntony is discussed, and the relative importance of conduction and induction is considered together with a detailed investigation of the theory of "detectors" for such a system.

The absorption-factor in the case of sea-water will probably be one of the first matters to be dealt with by experiment and theory in the present year. By the use of horizontal inductor coils, one on the light-ship and one ashore, Dr. Lodge avoids the practical difficulty; nevertheless the problem is sure to elicit his interest. Mr. Whitehead, holding tacitly to Maxwell's equations, deduces a law for the absorption-factor, and to this law Dr. Lodge takes exception. Maxwell supposed that the total current was made up of the polarisation current and the induction current; Mr. Whitehead assumes that the polarisation current may be neglected, and that provided the frequency is not comparable with that of light, no serious error can arise from that assumption. Do Maxwell's equations fail then for such a case? Dr. Lodge seemed unwilling to admit that they do fail; he suggested that Mr. Whitehead had written them down in a form that did not agree with Maxwell, and that Prof. J. J. Thomson had written them in yet another form. Mr. Whitehead now asks Dr. Lodge what is the right form? And that's how the matter stands.

ROLLO APPLEYARD.

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[1] By a slight error, Dr. Lodge there attributes an experiment on liquid "coherers" to Lord Rayleigh. This experiment was first described and shown by the present writer on March 26, 1897, at the Physical Society. A few weeks later it was *repeated* by Lord Rayleigh at the Royal Institution.

[1] For the various methods of space-telegraphy, see a paper by Dr. S. P. Thompson (*Soc. Arts Journal*, 46, pp. 453-460, 1898).

THE FISHES OF TANGANYIKA AND OTHER GREAT LAKES.

THE first part of the fifteenth volume of the *Transactions* of the Zoological Society of London, which has just been issued, is devoted to a report by Mr. G. A. Boulenger, F.R.S., on the collection of fishes made by