DISCUSSION ON "THE AUDION; A NEW RECEIVER FOR WIRELESS TELEGRAPHY," NEW YORK, OCTOBER 26, 1906.

Michael I. Pupin: I have had some experience in the constructing of detectors of electrical waves. I always call them detectors, because, as a friend of mine said the other day, there are so many various detectors now that one is tempted to believe that anything will do to detect an electrical wave. In the course of the development of the art of telephony, any schoolboy could make a telephone that would receive electrical impulses; so in wireless telegraph work, the number of detectors seems to be increasing indefinitely—good, bad, and indifferent detectors appearing indiscriminately. If there must be a new name for each new detector—a new name for everything that comes up in the course of the development of the electrical art—pretty soon the science of electrotechnics will be a maze of new names; and the learning of the names will be much more difficult than the learning of the facts connected with the art. For that reason I am opposed to new names. Although Dr. De Forest is very enthusiastic about the elegance of the name audion, I must say that I am not very much impressed by it. It is a mongrel. It is a Latin word with a Greek ending. If he had said *acouion* or *acousticon* it might have been better, but more difficult to pronounce.

This is certainly a new wave detector used in actual wireless telegraphy. The physics of the thing is old. It was Hittorf, who, over fifty years ago, in 1850, I believe, discovered that in a vacuum tube in which even very high electric tensions would produce no perceptible discharge, the heating of the cathode facilitated the passage of electricity to such an extent that a small electromotive force, a few volts, would produce perceptible current. I think that was the first observation of the kind. After that the number of men who engaged in this branch of fascinating research became legion. The literature of the subject is well given in J. J. Thomson's book on "Discharge of Electricity through Gases." I am much interested in the subject, because one of my colleagues, Professor Tufts, has made some interesting investigations in that field, particularly in regard to the passage of electricity through hot gases containing a spray of salts at ordinary pressures. Dr. De Forest devotes some attention to this subject in the first part of his paper; but for reasons which are evident from the paper, he abandons this form of, I won't say audion, but wave detector. He abandons it, because it is variable, and resorts to the vacuum tube detector described in the paper. I think that is a clever step indeed, because one can see at a glance that this wave phenomenon which he first observed in connection with the passage of electricity through a hot gas at ordinary pressures, through flames of arc lamps, etc., might exist also in vacuum tubes, and that one would expect here a very much greater steadiness and reliability, and that, of course, is one of the highest

desiderata in any technical work, particularly in wireless telegraphy. In this Dr. De Forest seems to have been successful.

I should have been very glad if Dr. De Forest had given a brief historical account of the subject, and then a brief statement of the physical theory of the whole matter underlying his invention. I think the paper would have been very much more easily understood by those who are not well acquainted with this part of the electrical science. I think we all feel that the matter is somewhat outside of our ordinary lines of work; it is a new subject, a subject that has been so far mostly in the hands of physicists and not electrical engineers. I hope that Dr. De Forest will, in the final publication of this paper, contribute a brief history and a brief statement of the physical theory of the subject for the benefit of the members of the American Institute of Electrical Engineers. As Dr. De Forest states frankly, it is difficult to explain the phenomena that one meets with when one tries to make a wave detector from a vacuum tube with a hot electrode.

What we have there is the hot electrode from which negative ions recede, and then we have the cooler plate, which is negative with respect to the hot electrode. When external electrical tension is applied to these electrodes, we have a considerable leakage current. We call it a leakage current for want of a better name; it is an electrical current. This electrical current is steady, according to Dr. De Forest, and this, in fact, is the most remarkable point in his paper; it is steady under all conditions, no matter whether electrical waves strike the oscillating circuit of which the tube is a part, or not. It is steady as far as a milliammeter can tell. It is not steady as far as the telephone can tell. Now, if the effect of the hertzian waves is to produce a unidirectional current, or rather if the effect of the wave is to rectify the hertzian waves—to let either the positive or negative parts of the wave pass through unhindered—why should we not be expected to perceive this in the milliammeter? We do not perceive it, and therefore we cannot suppose that these waves are rectified. The only other thing we can guess as probably happening is that the oscillation passes through the whole circuit; but on account of some effect upon the gaseous part of the circuit—the resistance or the diminution or increase of some of the other reactions—the original current is strengthened very much at certain intervals during one-half of the waves, and weakened during the other half, or strengthened during each side of the wave. This, as Dr. De Forest points out, is also impossible to believe, because the oscillations being so rapid, could not very well pass through the winding of the telephone in the first place, and in the second place could not be expected to produce a large magnetic effect upon the diaphragm; because magnetization and demagnetization of the permanent magnet in the telephone would not follow these rapid electrical oscillations. That seems to me to be one of the most difficult points to understand in the paper. That is the stumbling

block in our understanding the *modus operandi* of the whole thing. Why does it operate? I have no explanation to offer. It would be presumptuous on my part to offer one, even if I had one to offer. If Dr. De Forest cannot explain it, I certainly cannot. But I have one suggestion to offer, and here it is: for quite a number of years, I have employed a telephone for detecting faint sounds, faint differences of potential, and faint currents. I find that the telephone is one of the most tricky instruments one can use. It will lead one to draw dangerous conclusions. The most misleading point in the detection of faint electromotive forces by the telephone is to distinguish between electromagnetic effects and electrostatic effects. The sensitiveness of the telephone has been estimated variously from 10^{-13} to 10^{-10} amperes. Now that is a very high degree of sensitiveness. It would be if it were correct. It is not correct. The telephone is not so sensitive as that, and I believe that those who made these determinations did not measure the right thing. If they had measured the current when they were determining the sensitiveness of the instrument, they would have found that the thing measured was not the current passing through the telephone coils, but in all probability the current which went through the body, all along the floor—the leakage current—which affected the magnetic force in the telephone which produced the sound. I have been misled that way quite a number of times. Whenever I see any one using the telephone in making observations I always look askance, and ask myself: has not this man been deceived in his calculations? I do not want to imply that Dr. De Forest might have been deceived in his measurements; I only want to say that when it is said the sound is heard, the question arises in my mind what produced that sound—is it the variation of the current which goes through the winding of the telephone, or is it the variation simply of the potential in the whole room, in his body, and in the windings of the telephone?

Percy H. Thomas: Apparently we have come across another example of apparatus illustrating the new theory in electrical science, an apparatus depending for its operation on the activity of corpuscles or ions. The audion probably will not introduce any fundamentally new principles when its operation is fully explained. The thing that interests us particularly at the present time is what is the nature of the ions and corpuscles. On this, scientists are not agreed, but there are a good many characteristics that are commonly accepted which will assist greatly in understanding such types of apparatus as the audion.

We may consider that electricity is either "corpuscles" or is connected directly with corpuscles. By corpuscles, or "electrons" as they are sometimes called, I mean those very small particles, approximately the 1-1000 part of hydrogen atoms with which we are all familiar. These corpuscles, then, when at rest, are static electricity, and as such are attracted by an electrostatic or "static" charge; that is, are sensitive to an electrostatic field but

are not affected by electromagnetic influences. When, however, these corpuscles or particles are moved rapidly (and they move extremely rapidly, anywhere from comparatively slow velocities to nearly the velocity of light) they are the equivalent of electrical currents, and as such are subject to the influence of magnets, and themselves produce magnetism.

These corpuscles, figuratively speaking, may be looked upon as comets, or planets, or suns in space, for when they are free in a vacuum they move about under the influence of the various forces, electrostatic or electromagnetic, as may act on them, either in straight lines or curved lines, as the resultant force may require; when distributed through space filled with air or other gases, their free movements are impeded and they bump against one another and come in contact with the molecules of the gases, and consequently are unable to go steadily in the direction in which they may be attracted; and further, they often attach themselves to some atom or molecule or aggregate of molecules which again limits their motion.

As Dr. Pupin has stated, where you expect to utilize the motion of corpuscles, there is usually a great advantage in putting the apparatus in a vacuum. Like sodium or chlorine and many other materials corpuscles do not ordinarily exist in a free state. They are usually closely associated with molecules, in which condition they do not manifest their characteristic qualities. If you want to use them you must separate or isolate them. There are many ways of freeing them from matter. Corpuscles are frequently separated from air or other gases as, for example, by radiations from X-ray tubes; the waves that come from the X-ray tube, or the Crookes tube, by some mysterious process set free corpuscles from the gas molecules; or as stated in the paper the operation may be accomplished by letting ultraviolet light fall on certain metals. Corpuscles can be produced by the arc or a flame, as in the original audion, and in many other ways. They can be separated from solids and liquids, as in the Cooper Hewitt lamp, where they are produced in great quantities, in all probability from the electrodes themselves. The various starting methods used in Cooper Hewitt apparatus serve in different ways to initiate a freeing of corpuscles. This process is much easier to continue than to start. There are a number of well-known types of apparatus which are illustrations of the action of corpuscles.

First, the Geissler tube, in which by a strong electrostatic force, (that is by a high potential) corpuscles are either detached from the electrodes or separated from the residual gas which is purposely left in the tube so that there is an agitation of the gas atoms in the vapor space which causes them to emit light. Atoms of different substances give different colors of light.

Secondly, the Crookes tube, in which practically the same phenomenon exists, except that the residual gases are extracted so that the corpuscles though forcibly driven through the tube as

before, give no light. They meet no obstruction and produce no visible effect, unless there is some fluorescent matter in the tube upon which they may impinge and cause it to give light.

Thirdly, Crookes tubes may be used for the purpose of getting waves, electromagnetic waves of some form or other, as in case of the common X-ray tube. Here the useful waves are produced by the impact of the corpuscles on an electrode of some sort.

Fourthly, the ordinary electric arc is presumably the forcing of a large number of these corpuscles from one electrode to another having them come in sufficient force and numbers to crowd back the atmospheric pressure and keep the gas molecules to one side. A large number naturally escape to the surrounding air, which is then said to be ionized.

Fifthly, the Moore vacuum tube which is in many ways similar to the Geissler tube.

Sixthly, the Cooper Hewitt mercury vapor apparatus, since it passes current through a vacuum must, according to this theory, be also a corpuscle operated device. Here there is a drawing of a large number of corpuscles from the negative electrode which in the case of the lamp excites the vapor in the tube, so that it will give light of its characteristic color. In the Cooper Hewitt type of apparatus there is this difference, however—that by virtue of the perfect vacuum and the large quantity of current or corpuscles, very little electromotive force is required to force them from the electrode; and there are other important differences. In the Crookes tube where the number of corpuscles in motion is very much smaller, a great deal of electrostatic force is necessary to separate them from the solid electrode.

Now in the audion we have a means of producing corpuscles, and we have a vacuum which allows more or less freedom of movement. We have a further means of controlling and directing these ions, that is, the additional battery electromotive force called *B*. The operation of the device depends upon the effect of the waves which come in on the transmission circuit which, in some way or other we do not now understand, so affect the action of the corpuscles which have previously been separated as to make a sound in the telephone.

I have spoken of corpuscles in connection with the audion as though they alone were the important factor while the paper speaks of ions. An ion is generally taken to be an atom to which is attached a corpuscle, or from which a corpuscle has been abstracted, and the determination as to whether the corpuscle acts entirely free of the atom is not finally settled.

The explanation which is suggested for the curious relay action of the audion, that it depends upon a virtual change of dielectric capacity in the vacuum space, is certainly an ingenious one and might turn out to be the correct one. There is one question I would like to ask Dr. De Forest. Do I understand that he presumes the action depends on the ionization of residual gases within the vacuum, or is the vacuum so perfect that the ions or electrons come from the electrodes themselves?

Lee De Forest: I think it is due to the ionization of the residual gases; the gases still exist in the lamp, because the vacuum is only that which obtains in all incandescent lamps.

Sewall Cabot: Dr. De Forest says: "The audion to a greater extent than any other responder is self-tuned." Does this condition of self-tuning refer to change in the electrostatic capacity of the audion, thus altering the oscillation pitch to which the closed circuit having capacity and inductance is resonant? Or does it simply refer to tuning to the spark frequency on the assumption that with certain adjustments the audion will become most strongly responsive to a definite frequency of spark which is within the audible range of frequencies?

Lee De Forest: Both effects are really present. Where the tuning of the audion is regulated by changing the distance between the two electrodes, as in the last case described, where the two wings are hinged, and drawn to or taken from the filament by a magnet, we have there the change of capacity. In the other case, where the distance between these and the heating current or the filament remains unchanged, we merely vary the potential across the gap. It is rather difficult to explain this tuning effect on the ground of merely changing the capacity. This may be varied by the variation of potential difference but, as I have said, even though the wave frequencies of the two transmitters are as nearly the same as it is possible to make them, one spark frequency being 125 per second and the other 60 per second, the change of the battery *B* from 12 to 14 volts makes a great difference between the response of the audion to *A* or *B*. I think this selectivity is due rather to the integrat-ing effect of the energy received during a brief unit of time. I won't attempt at this time to enter into a greater explanation than that. I think both effects are present.

Sewall Cabot: Is it true that the capacity of the audion can be changed through relatively large values so as to produce a considerable change of resonant frequency in a closed circuit when it is across the condenser? Thus connected, the capacity of the audion would presumably be very small with regard to the capacity used to bring the closed resonant circuit to the point of resonance if sharp tuning is obtained.

Lee De Forest: Yes; the capacity of the audion measured in that way is very small; a very small condenser put in shunt across it silences the response. Presumably, then, a considerable separation of the wings will not relatively change the tuning of the closed resonant circuit. The principal effect in tuning is with regard to the spark frequency.

J. B. Taylor: While I do not consider myself an expert on wireless telegraphy, it happens that I have made experiments on apparatus similar to that under discussion. I have operated a mercury-arc, with a single cathode and two anodes, from a storage-battery. The number of cells was no more than sufficient to maintain the arc, so that it was in a somewhat sensitive

condition. In another part of the room was a 25,000-volt step-up transformer, and it was noted, after a spark from the transformer, that the mercury-arc had gone out. This was at first supposed to be only a coincidence; but the experiment was repeated a number of times with the same result, the arc going out immediately on the striking of the high-tension spark. Plainly, we had an arc detector for wireless signals similar to the audion. A note was made of the matter as something to be looked into in more detail at some future convenient time. I merely cite this to show that there is much resemblance between the mercury-arc, in fact any arc, and what in the audion I regard as an arc-stream between the incandescent filament and the plates.

The author's explanation of the *modus operandi* of the audion seems to me to be too complicated to stand much chance of being correct. When something new comes up for explanation we should take the simplest view of it first. Instead of immediately calling in the aid of ions, corpuscles, and other things of which we can have little physical conception, we should leave these as a court of last resort, to be called in only when simpler theories fail to account for the observed facts.

As described in the paper, there are many points in common between the audion and mercury-vapor apparatus. This resemblance, and some slight acquaintance with the mercury-arc induces me to offer an explanation with less of ions and more of the science of acoustics.

An organ-pipe may have the stream of air so directed that the pipe just fails to speak. Another pipe by sounding may cause it to speak, or even snapping the fingers may give the same result. The sound from another source carries just enough energy to upset the balance, to make a stable condition unstable. Similarly in the device under consideration this evening, the arc-stream is just on the point of vibrating and needs only a slight impulse to trip it. Here is an arc-stream in a very sensitive state ready to make a noise in the telephone, ready to vibrate or to start a new current-stream from another portion of the anode. The high-frequency wireless impulse need have only sufficient energy to make the stable condition of the arc-stream unstable. There is a short interval of time before the current in the arc becomes steady again, and it is in this interval that the telephone receiver gives its sound. I have not had the pleasure of listening to the audion; but I do know that a mercury-arc stream has a very definite vibrational period, as evidenced by the musical sounds heard in a telephone receiver.

This acoustical theory also may explain the sensibility to spark frequency. It being apparent that great sensibility would be obtained if the recurring impulse (as determined by spark frequency) coincide with the natural period of oscillation of the arc-stream itself.

The point is made that the reading of a direct-current meter

shows no change, at the same time the telephone receiver gives evidence of fluctuating current. Dr. De Forest does not give the type, nor the sensibility of the direct-current instrument used. The current may increase or decrease, and the instrument not show it. If the sounds are due to vibrations of the arc-stream the resulting current would be very much like a direct current with the addition of a small alternating current, and a direct-current instrument will not show this so long as the alternating currents are relatively of small value.

I do not understand the analogy drawn between the hysteresis loss in iron; that is, put through a complete magnetic cycle, and what is termed, hysteresis loss in the audion. In the case of iron we supply a certain amount of energy, and a less portion of this same energy is returned, the difference being lost. In the audion I cannot see that there is stored energy in any form, so that it seems improper to speak of hysteresis losses in this case, merely because ascending and descending curves do not coincide.

Dr. De Forest makes a distinction between the relay action of the device as used by him, and the rectifying action of a similar piece of apparatus in the hands of others. This distinction seems quite proper, as the amount of energy available at the end of any commercial wireless transmission must be altogether too small to give any direct evidence of its existence to our senses.

Edward P. Thompson: A former speaker has alluded to the great importance of knowing the complete causes of the click in the telephone. Suspecting that an audible spark is produced in the telephone, sometimes, by the leakage of the oscillations to the diaphragm, I used a telephone casing without any magnet or diaphragm, providing the smallest possible spark-gap inside, by two wires almost in contact. By using this spark-phone instead of the telephone, but at short wireless telegraphic distances only, the same click was heard, and it was loud, because the sparks were produced in a small confined space close to the ear.

The probable conclusion is that the click in the telephone, although usually produced by electromagnetic action, may also be the direct sound of a very small and even invisible spark. Caution should be taken to experiment at short distances only, for it is obvious that the so-called spark-phone consists of a hertzian spark-gap detector in a confined space, with a listening opening to be placed close to one's ear.

Frederick K. Vreeland: While listening to this paper I was impressed by the striking similarity between some of the phenomena Dr. De Forest has pointed out, and certain actions that occur in electrolytic cells. We know it is quite the fashion now to point out the analogy between the behavior of electrons or ions in a gas and the behavior of the ions in an electrolytic solution; and although these analogies will not always bear the test of strict scientific scrutiny, they are sometimes very suggestive. Dr. De Forest has pointed out that in this audion there is a production of ions in the neighborhood of the hot filament, and has

also shown that these ions are concentrated mainly in the vicinity of that filament. The ions in this case are produced by the heating of the filament. But if we take an electrolytic cell—for example, a pair of platinum electrodes immersed in acid—and connect a source of electromotive force across it, we shall also get a collection of ions on the anode and a collection of opposite ions on the cathode. The electrodes are said to be polarized. The distribution of potential in the electrolytic cell is somewhat analogous to that in the vacuum tube of the audion, and we can reproduce some of the phenomena that Dr. De Forest has called our attention to. For example, Dr. Pupin discovered years ago that if we take such a cell with very small electrodes, connect a battery in series with it and polarize it, that that combination of cell and battery is capable of rectifying an alternating current. The current will flow through it in one

FIG. 1.

direction, but it will not flow through it in the other. Now, that is a rather close analogue to the arrangement of Professor Fleming, to which Dr. De Forest has called attention—his “oscillation valve,” as Prof. Fleming called it. As you will remember, he took one of these hot-filament tubes and connected it in series with a source of electromotive force and a galvanometer, and he got a rectification. He explains that phenomenon if I remember rightly, by the fact that the negative corpuscles, being so much smaller and more mobile and having a higher velocity, are more easily set in motion than the positive electrons which are gathered at the other terminal, and consequently the current will flow more readily in one direction than in the other.

Now, regarding the “polariphone” receiver, if Dr. Pupin had been in the position I was in when I coined that name, I think

he would have found it awkward to say "An electrolytic wave detector which operates by anodic depolarization." Polariphone is not quite so beautiful as audion, but it suggests the fact that the thing works by polarization and that the result is a sound. In the polariphone cell there is a very minute anode, and usually a larger cathode, and the cell is polarized by a dry battery. As you increase the voltage across the cell, at first you get no current, but when you reach a certain point a current will begin to flow; very minutely, it is true, but still it is a current, and that is at a point below the voltage ordinarily known as the decomposition voltage of the electrolyte, which is about 1.7 volts. The current commences at a point below that and gradually increases, the curve running almost parallel to the voltage axis, showing a small increase of current for a large increase of electromotive force, and finally reaching a critical point *b* where the curve turns over and you get a large increase of current for a small increase of electromotive force. If you polarize the cell to about that point it is sensitive to electrical oscillations, just as the audion is. These oscillations pass through the cell, depolarize it, and as a result of the depolarization, the counter electromotive force is diminished and a large current flows from the battery and gives an impulse to the telephone. Comparing this with Dr. Pupin's rectifier, we find a very similar relation to that which appears in the comparison of Dr. De Forest's audion with Professor Fleming's oscillation valve. It is vastly more sensitive and it performs a function which is not rectification—it is rather a relay effect, such as Dr. De Forest ascribes to the audion.

As to the question why the current does not affect the ammeter. I ask Dr. De Forest if it is absolutely certain that the current which affects the telephone is not a unidirectional pulsating current? The reason I ask this is that in the polariphone receiver you can get by suitable adjustments a similar phenomenon—you can get a very distinct and even loud signal without any increase of current which will affect the instruments ordinarily used for measuring these currents. We all know the telephone is an exceedingly sensitive instrument, even discounting the figure that Dr. Pupin repudiates. Instead of saying that it is sensitive to 10^{-13} ampere, suppose we say it is only sensitive to the millionth of an ampere. That is a very conservative estimate. Telephones are certainly sensitive to that current. A current of a millionth of an ampere would not affect any of the instruments that one would be likely to use in measuring such an effect.

Lee Deforest: I think that statement is correct.

Frederick K. Vreeland: Is it not possible you did have a unidirectional pulsating current sufficient to affect the telephone and not sufficient to affect the instrument you were using?

Lee De Forest: That is conceivable with extremely faint impulses, but at the same time I got signals which could be described as tremendously loud in the telephone, and no change

of deflection, so I had no reason to suppose that as these signals became weaker in the telephone they would become unidirectional. There was no reason to suppose they should, when they are weak, give an effect entirely lacking when they are strong.

Frederick K. Vreeland: Then you incline, as I understand you, to the belief that the currents in the telephone were really alternating currents and did reverse.

Lee De Forest: Yes.

Frederick K. Vreeland: Then as Dr. Pupin says, that is an interesting point.

I do not set forth these analogies as strictly scientific parallels. I do not know just how far we could push them into a refined analysis of the question, but they are certainly suggestive, and I think they tend to crystallize our ideas as to the relation between the ionic phenomena in gases and those in electrolytes.

Lee De Forest: Dr. Pupin's opening remarks may serve as an argument why the study of Greek and Latin should be thoroughly introduced into our engineering schools. My knowledge of Greek is almost nil; I knew, however, that "aud" was of Latin and "ion" of Greek derivation. But they are both expressive. Where we use a term one hundred times a day, it is necessary to have something brief; we could not expect the wireless telegraph operators to use a long technical description of the apparatus in speaking of it, and when several types are in use it is necessary clearly and briefly to distinguish them.

As to why the milliammeter or sensitive direct-current instrument would not show any deflection at the time the telephone responded, we know that the sound we hear in the telephone reproduces of course the spark frequency. These impulses if they are all in one direction, and sufficiently close together and are of sufficient intensity, must produce an effect on the sensitive direct-current instruments. The only reason why they do not produce an effect must be that they are alternately in one direction and the other. With an oscillograph we would find out just what their curve is. I hope some day to do this. They are undoubtedly alternating, rather than pulsating. I hope to have the privilege of showing the audion in operation to some of the members of the Institute, and letting them hear the signals. I would like Dr. Pupin especially to hear them. The intensity of the sounds is at times so great that he will surely admit that they are caused by an electromagnetic current flowing through the telephone, and not by electrostatic effects in the telephone between the core and the diaphragm, although the latter effect undoubtedly exists.

As Mr. Vreeland says, there are a number of analogies between the audion and the polariphone. They both employ ions or corpuscles, as the case may be, and the forms of the saturation curves, current and voltage curves, etc., are sometimes similar in both cases. I believe, however, that the analogies are not sufficiently close even to be discussed by lawyers in patent litigation!