special position, or referred to such special axes, as reduce the general problem to a specially simple one.

The speaker then drew the projections of three conjugate diameters of a general ellipsoid upon the board, stating that this was the best method of defining that body. He then proceeded to find the projections of the enveloping cylinders, and the shadow of the body; which he showed could as easily be done for the general ellipsoid, in a perfectly general position, as for special cases. In fact, it appeared that problems on this body gained nothing in simplicity by special methods and devices which detract from the generality of the treatment.

List of other papers.

The following additional papers were read in this section, some of them by title only: Tidal observations on soundings distant from shore, by *J. M. Batchelder*. Investigation of light variations of Sawyer's variable, by *S. C. Chandler*. Standard time-pointer and a time longitude dial; System of algebraic geometry, by *Samuel Emerson*. The calculus of direction and position, by *E. W. Hyde*. Observations on the transit of Venus made at Columbia college; Description of the new observatory at Columbia college, by *J. K. Rees*. The light variations of T. Monocerotis, by *E. F. Sawyer*. Method of observing eclipses of Jupiter's satellites, by *D. P. Todd*. Conic sections in descriptive geometry, by *J. B. Webb*. Descriptive geometry applied to the general ellipsoid, by *C. M. Woodward*. Some observations on Uranus, by *C. A. Young*.

PROCEEDINGS OF SECTION B. — PHYSICS.

PAPERS READ BEFORE SECTION B.

[*Continued*.]

The tornado at Racine, May 18, 1883.

BY P. R. HOY OF RACINE, WIS.

A CURIOUS mistake preceded the reading of this paper. There was some confusion between the abstracts of this and another paper on a tornado, which were submitted to the sectional committee; and the other paper was entered on the daily programme, but was withdrawn.

Mr. Hoy's paper began by stating that the early part of the day was pleasant, but about 6.45 in the evening two clouds of ominous appearance joined, from opposite quarters of the heavens, and at once the cyclone began. Its general direction was to the north of east. There was no rain at Racine with the storm, but there was noticed a very strong odor of ozone while the cyclone was at its height. At the start it was barely two rods wide, but when it reached Racine it had expanded to twenty rods. Its motion was rotary and oscillatory, and all *débris* was thrown to the centre of the track. When the cyclone crossed the lake it formed huge waterspouts, one central, and seven to eight accessory, whirling about the main trunk.

Prof. H. A. Rowland proceeded to discuss the paper as follows: Most observers of tornadoes just perceive that there is a whirling motion of the air, and it knocks down objects, and that is the principal thing they see. But that is very ordinary observation. Of course, a column of air in such swift rotation will tear houses down, spurt water up, and do every thing of that sort. The particular point which I observed in this paper was the description of the formation of the tornado. The phenomenon which is to be explained is the formation of the tornado, and very few have observed this. This description was very short; merely, that, over in the west or south-west, the clouds formed. Of course, to an observer from the west, one would appear north, and the other south. The point I wish to bring out is, that there was lightning passing between the two clouds. In Mr. Finley's description of six hundred tornadoes, I do not see any similar account. Many observers have seen lightning play around these clouds, but not passing between the two clouds. Mr. Finley applied to me to know whether there was any thing in the electrical theory of a tornado. Of course, any theory of the destruction being caused by electricity, houses being attracted, etc., — all that is mere nonsense. We know that the attraction of electricity is only a mere fraction of an ounce to the square inch. Before the force becomes sufficient to raise a great weight, a spark passes, and a discharge of electricity takes place. But in this case (these two clouds passing from north to south, and boiling up, having flashes of lightning playing round them), I thought there might be something in the electrical theory, as far as formation was concerned; and I calculated for the signal-service and Mr. Finley what amount of energy there was in two clouds approaching each other in this way. The rotation of the earth will cause them to come together, not in a straight line, but a little aside from each other, forming a spiral motion. The direction of the rotation of the tornado is a necessary consequence of the earth's rotation: so that it might be possible to have these electrified clouds approach each other by mutual attraction, and form a tornado at the point where they meet. I calculated the energy, and found there was sufficient for a rather small tornado in the case I took. I would not be willing to say that is the theory of all tornadoes. I say that it is only possible. There is a great deal more energy in a mass of air heated up to a considerable temperature, and rising, by force of gravitation, — a great many times more. If it were not for the electrical phenomena observed in the case, I should say there was very little probability of the electrical theory. I believe Mr. Finley will direct the signal-service observers to watch the direction of the wind. If it flows in from all directions at the point where the tornado is formed, we should determine it to be due to the rise of hot air at that point. When the ground is very hot and the air very sultry, we have two causes; and it is only by observation that we can find out its true manner. I do not lay very much stress upon the electrical theory. But it is an interesting point, to me, to notice that flashes of lightning have been observed between these two clouds, showing that they were differently electrified, and that there was some plausibility for the theory which I sent to the signal-service.

Prof. F. E. Nipher continued this discussion the next day, as follows: One matter connected with the effects of this tornado contained a point, it seems to me, of sufficient interest to call the attention of observers to the matter, in case any one should have an opportunity to observe the effect of a tornado upon water. Mr. Ferrel, I think, in his description of a tornado, states that we have a rising of the water, forming a sort of cone in the centre of the tornado; the effect being, of course, ascribed to the diminution of pressure which is known to be there. In the cyclone proper, where we have a large area, we have a storm-wave as the principal element in the case, and there is an upheaval of the water in the area of low pressure. In the tornado it seems to me very questionable whether that occurs. I base that upon this observation: A smaller wind-whirl which was observed by myself in northern Missouri, which was rather violent though not destructive, — a column of dust several hundred feet high being raised, — passed out upon a pond of water five or six feet deep, and a depression was formed in the water, extending to the bottom of the pond, — an immense cup. The water was revolving rapidly; and it was thrown into rotation with a centrifugal effect, — the same effect as when a vessel is whirled. It seems to me that this is an element which has not been considered as it should be. If the whirl is small, and you have not only a diminution of pressure in the centre, but of the whole body of the water, the friction producing a rotation of the water, if the result is sufficiently small you might get a depression instead of an elevation. I call attention to this, so that those who may be fortunate enough to see a tornado on the water may not take it for granted that it is all known.

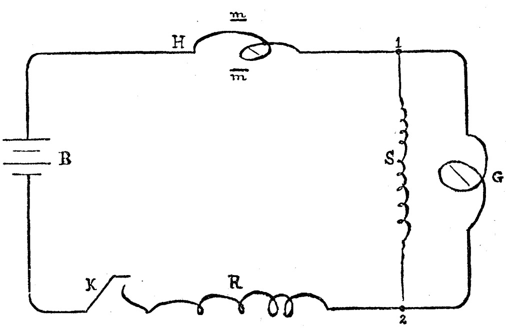
As to the remarks of Professor Rowland in regard to the possible electrical origin of a tornado, I know that he was very careful to say that he did not think any of the destructive effects could be ascribed to the action of electricity. I gathered the idea that he thought a tornado might originate in that way, — that two electrified clouds will attract each other, and come together; and he calculates the energy of the attraction which bodies can have for each other in air. It seems to me that the simple observation that was made by Mr. Hoy, together with another fact which we know, — that when the discharge passes between electrified bodies they are almost wholly discharged, — would show that when that happens the cause for that motion has disappeared. When these two clouds approach, a spark passes, and the whole thing is gone. So long as there is no spark passing, we know very well that the attraction is very much less than the maximum attraction of $\frac{15}{100}$ of an ounce on the square inch. I think, perhaps, that is a matter Professor Rowland did not consider. It does not seem to me at all likely that any such origin can be ascribed to the tornado. When it is developed, you may have a rarefied column which may be very highly rarefied, connecting the earth with the upper regions, which is precisely the reason that the lightning which was observed in the case of the Racine tornado was not accompanied by thunder.

Prof. J. T. Lovewell said it occurred to him, from his observation, that a good deal of care is necessary in order that the observer may know exactly what he sees. It was my fortune, said he, to witness a small whirl at a distance of three or four miles. I saw the funnel-shaped cloud descend toward the earth, and it looked to me as though there were a column of water. Many people who saw it spoke of it as a waterspout. It might have been water, for aught that we could have said from our point of sight. I immediately drove to the spot, and it appeared that not a drop of rain had fallen in that track. The whirl had been sufficient to overturn a few stacks of grain and hay, and a man was thrown about with his team in the road. I think, if it had struck a body of water, I should be slow to believe that it lifted any solid column of water into the air one hundred feet. It would have made a grand scattering of the water, and a great deal of it would have been thrown up into the air. I believe that a good deal of that which is commonly ascribed to columns of water rising up, and pouring down the sides in cataracts, is optical illusion. I should be slow to take the testimony of a person seeing them, unless he had his mind disabused of the common notions about these waterspouts. So far as their electrical origin is concerned, I quite agree with Professor Nipher that it is not by any means proven that electricity has any thing to do with them, except that it is a necessary adjunct, of course, to all such disturbances.

A method for the calibration of a galvanometer.

BY B. F. THOMAS OF COLUMBIA, MO.

A BATTERY of any sort is joined in circuit with a sensitive galvanoscope H, a galvanometer G, and any variable resistance R. When the circuit is closed at K, the current is so adjusted by varying R, as to give the highest desirable deflection of the galvanometer needle. The needle of H will be forced against the stops. By means of magnets m and m, the needle of H is brought back to zero. If these magnets and the galvanoscope be undisturbed, the original current strength will be indicated when the needle stands at zero, whatever changes may have been made in the circuit. If now the shunt S be connected at 1, 2, and the resistance of the shunt is made equal to that of the galvanometer (positively determined), and the needle of H brought back to zero (by increasing R, as insertion of the shunts lowers the total resistance of the circuit, and therefore increases the current strength, deflecting H), a new deflection of the galvanometer needle will be produced, the deflection being that due to a current of one-half the strength of the original current. By giving to S values equal to ∞, 3, 2, 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, etc., times the resistance of G, and bringing the needle of H to zero each time, deflection of G will result, due to currents whose strengths are as 1, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, etc. The curve is then plotted with deflections and current strengths as co-ordinates. Any desired number of points in the



curve may be obtained by giving S the proper values. The calibration may be checked by making a new adjustment for the united current, so that the deflection of G shall be about two-thirds the first deflection, and proceeding as above. Plotting the new values obtained, the curves will coincide if the work is correct. If it be found desirable, the battery may be exchanged for another during the determination.

The utilization of the sun's rays for warming and ventilating apartments.

BY E. S. MORSE OF SALEM, MASS.

MR. MORSE drew attention to this device a year ago, before the National academy of sciences. At that time he was able to offer only crude computations as to the operations of the heater, derived from its use at the museum of Salem, Mass.

The device consists mainly of a slaty surface painted black, standing vertically upon a wall, outside the building, with flues to conduct warmed air to the inside. The slates are inserted in a groove, much as one might place glass in a frame. One made within the last year was three feet wide and eight long. It was placed where it received the sun's rays as directly as practicable. Its service was to warm a room used for a library. During an entire winter the room was thus made comfortable, except on a few of the coldest days. The current of air passing through it, when the sun's rays impinged directly upon it, was raised about 30°; it discharged 3,206 feet of warmed air in an hour. This was in the morning. At 11.45 the air of the apartment was raised 29°, with 3,326 cubic feet of air discharged; at 12.45, 29° and 4,119 feet; at 1.55, 24° and 3,062 feet; at 2.45, 20° and 1,299 feet. The room measured 20×14, and was ten feet high.

The apparatus works to most advantage in a room that is ventilated by an open chimney. But some very good results have been obtained in closed rooms. One was cited, where the air in a public building was raised by such means to nearly 40° above the outside temperature. In general, a difference of 30° to 35° can thus be secured during four or five working hours of the day.

Professor Mendenhall stated that he had seen the working of the apparatus, and it proved very satisfactory. Professor Rogers gave similar testimony.

New form of selenium cell, with some remarkable electrical discoveries made by its use.

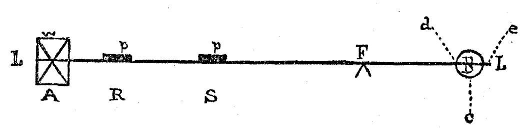
BY C. E. FRITTS, OF NEW YORK.

PROFESSOR MENDENHALL stated that in the absence of the author he was able to give only a brief summary of the paper. In the ordinary method of making selenium cells, they are constructed of a great many portions put side by side; the resistances are necessarily very high in these cells, and the light is allowed to strike in the direction of a right angle to the direction of the passage of the current. Mr. Fritts seems to have devised a different mode of operating these cells by using a very large surface, and in that way has succeeded in diminishing the resistance very greatly, which is very desirable. He has resistance as low as nine or ten ohms in the dark. The radical point of difference is, that in this case the light is allowed to strike upon the cell in the same direction as the current. He states that he has discovered many remarkable properties by means of his investigations with the instrument. When a cell of this kind breaks down, it can easily be remedied and repaired: in fact, there is no danger or difficulty of their breaking down permanently.

A method of determining the centre of gravity of a mass.

BY B. F. THOMAS OF COLUMBIA, MO.

A BAR, *L L*, is balanced on a knife-edge, *F*, so as to form a very sensitive balance. The body, *B*, of mass, *M*, is placed with a marked point in contact with a fine point, *d*; and another body of mass, *W*, is placed



at *A*, so as to nearly balance *B*. A small body of mass, *p*, is placed at *S*, to complete equilibrium. *B* is then rotated 180° horizontally, bringing its marked spot in contact with a second fixed point, *E*. Equilibrium is restored by placing *p* at *R*. The equations of moments in the two positions are, respectively, —

$$W \times AF + p \times SF = M\,(Fd + dc);$$

(*c* being centre of gravity); and

$$W \times AF + p \times RF = M\,(Fe - dc);\ (ec = dc).$$

Subtracting the first equation from the second, —

$$p \times RS = M\,(de - 2\,cd);$$

$$\therefore cd = \frac{de}{2} - \frac{p \times RS}{2\,M};$$

cd is therefore the distance from the marked spot to a vertical plane containing the centre of gravity. Taking a second marked spot in the plane thus found, the operation is repeated, with the plane horizontal. This gives a second plane through the centre of gravity. A third operation, with the intersection of the two planes in the line *de*, locates the centre of gravity.

The kinetic theory of the specific heat of solids.

BY H. T. EDDY OF CINCINNATI, OHIO.

THIS paper was based upon the well-known views of its author respecting the use to be made of the different degrees of freedom of motion among the atoms of solid bodies, in deducing a theory that will explain their diverse powers of conducting heat, and of transmitting or causing the transmission of radiant energy. The theory is based upon the conception that all bodies are constituted of equal ultimate atoms, whose combination, in different degrees of freedom, in different molecules, gives rise to the characteristic differences of elementary substances. This paper shows that the same hypothesis would cause solids, which are kept in equilibrium by radiation, to be also in thermal equilibrium when brought into contact; the equilibrium depending upon collisions of the molecules.

A kinetic theory of melting and boiling.

BY H. T. EDDY OF CINCINNATI, OHIO.

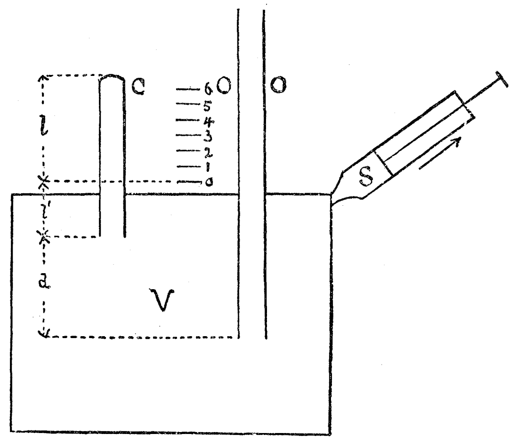
IN a solid in which the molecules are evidently held at nearly fixed mean distances by cohesive and elastic forces, there are two kinds of partially constrained freedom of motion possible for each molecule as a whole: first, a motion of its centre in a small orbit of more or less irregular shape about a mean position; and, second, a more or less irregular pendular motion of oscillation about a mean directional position. Both of these motions can be treated as vibratory motions; and the laws of force under which the motions occur, though somewhat unlike, have a general resemblance.

Two forms of apparatus for Boyle's law.

BY B. F. THOMAS OF COLUMBIA, MO.

THESE pieces are intended to enable one to adjust with accuracy and ease the mass of air to be experimented upon.

V is an iron cistern into which the open or pressure tube O, the closed tube C, and the reversible air-syringe S are screwed air-tight, and the cistern nearly filled with mercury. The syringe being connected for exhausting, and operated, air is withdrawn from C, until the mercury sinks to the bottom of the open tube, when air escapes from it, and rises through the mercury. No more air can be withdrawn from C. The mass of air remaining in C will evidently depend on the difference in depth of immersion of C and O. Let d = this difference, and let it be required to find such a value of d as will permit just enough air to remain in C to fill it from the zero of the scale, when at atmospheric pressure H. Let L = length of C from top to zero, and let l' = the length from zero to the open end of C. If now the mass of air which

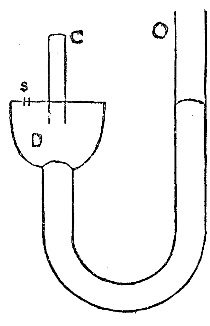


will fill the length l at H be expanded to fill the length l', the pressure H' at the bottom of C by Boyle's law is $H' = \frac{Hl}{l+l'}$.

The pressure at the open end of $O = H$. The difference in pressure at the ends of C and O is that due to a column (d) of mercury. Hence $H' = H - d$. Equating, $H - d = \frac{Hl}{l+l'}$. $\therefore d = \frac{Hl'}{l+l'}$.

On reversing the syringe, and forcing air in, the mercury will be found to rise and stand at zero in both tubes together. The demonstration is continued by forcing in more air.

A second form consists of two glass tubes connected by a strong rubber tube, and mounted on a stand with scales. The closed tube C is sealed into the screw-cover of an iron cistern D. Mercury being poured in, it will expel the air in D, and rise in an open screw-hole S in the cover. The hole being sealed by insertion of the screw, and O lowered, the air in C expands, filling C and D. On raising O, the mercury rises, and cuts off communication between C and D, preventing the return of some of the air. By making D of proper volume, the desired mass of air will remain in C. Let the volume of C above the zero = V. Let the entire volume of $C = V'$, and the volume of D above the open end of $C = V''$. Following the above steps it will be seen that a volume V' at H becomes a volume $V' + V''$ at H'; also that a volume V at H becomes a volume V' at H'. Hence the proportions $V : V' :: V' : V' + V''$, $\therefore V'' = (V' - V)\frac{V'}{V}$. The use of the rubber tube is not new: the method of adjusting the air-mass is be-



lieved to be. This form is convenient because it answers for demonstrating the law at pressures above and below one atmosphere. Of course, for the latter, the air in D must be removed through S as at first.

Natural snow-balls or snow-rollers.

BY SAMUEL HART OF HARTFORD, CONN.

The author carefully described the production and appearance of snow-balls or snow-rollers, the result of natural causes, of which a fine example was presented in Connecticut and southern Massachusetts last February. Snow had fallen, and had been covered with a frozen surface by a light subsequent rain. Upon this surface fresh snow fell, and this under the influence of wind was collected in masses of differing shapes. Some were spherical, from one to nine inches in diameter; most were rollers shaped like muffs, cylindrical, with a conical depression at each end reaching nearly to the middle. The largest observed by the author was 18 inches long, and 12 inches in diameter; but some were reported much larger. The path of formation showed that the roller had started with a small pellet, and, gaining both in length and diameter, had rolled up a long isosceles triangle of snow from its vertex. These paths were observed of a length of 25 to 30 feet, and others were reported as of 60 feet. The paths of the round balls were of nearly the same width throughout. None of these masses could be lifted without breakage. Such rollers were seen over an area of 40 miles in length: the author believed that they must have been millions in number.

Remarks on the tracings of self-registering instruments, and the value of the signal-service indications for Iowa, in June and July, 1883.

BY GUSTAVUS HINRICHS OF IOWA CITY, IO.

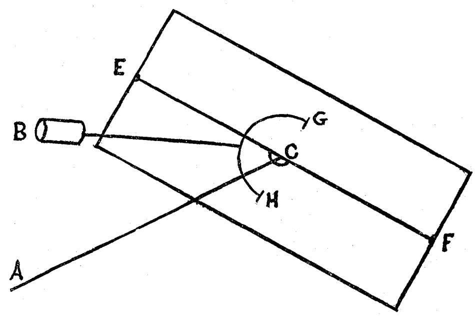
This paper was mainly a severe criticism on the work of the signal-service bureau. The author claimed that the predictions of the weather for Iowa in June had been quite untrustworthy, only 50 per cent proving correct. His views as to the value of this service were vigorously combated in a discussion which followed the reading of his paper.

A new heliostat.

BY B. F. THOMAS OF COLUMBIA, MO.

The instrument is intended to throw the reflected ray horizontally in the meridian.

A polar axis $A\ C$, driven by clockwork, is provided with a declination-arm $E\ F$, with arc, pivoted at C, and pivoted to the longer axis of the mirror at E and F. A 'horizontal axis' $B\ G\ H$, with the shaft portion B supported (by proper bearings in the standard not represented) in the same vertical plane with the polar axis, is pivoted to the mirror in its shorter axis at G and H, the points $E\ C\ F\ G\ H$ being always in one plane, and G and H so adjusted that a line through them will pass through C perpendicular to $E\ C\ F$. Moreover, the axis of the shaft portion B, extended, is perpendicular to and bisects $G\ C\ H$ at C. In order that the reflected ray be thrown horizontally in the meridian by our mirror, two conditions must be met: viz., 1°. The longer axis of the mirror must make and maintain the proper angle between itself and a line from C to the sun; or, what is



the same thing, the angle $A\ C\ E$ must equal latitude plus one-half the sun's altitude at noon. 2°. The shorter axis must move in a vertical plane, perpendicular to the meridian; or, in other words, the prime vertical. These conditions are rigidly met by the above combination.

The static telephone.

BY PROF. A. E. DOLBEAR OF COLLEGE HILL, MASS.

IN the static telephone, a ring of hard rubber is used, within which are two parallel metal-plates separated by a body of air (a non-conductor), one plate connected with the line carrying the current. The electrifying of one plate then causes attraction or repulsion of the free plate, and thus a sound in the receiver. This does away with magnetism. This system therefore requires a very large electro-motive power, and uses an induction coil of 2,000 ohms. A ground or return circuit is not present here. The equivalent is the body itself. There is no passage of electricity from plate to plate: the action is purely inductive through space. The insulation is accomplished by the intervening air-space, and by a coating of varnish, — an excellent di-electric. There is a device to discharge the induction plate in connection with this instrument, which keeps it constantly up to its full possibilities. When this instrument is fully charged, and the electrical conditions are perfect, the receiver may be entirely disconnected from the transmitter, and sounds and conversation can still be heard, even across a room.

He also called attention to the fact, that instruments that have been in use work much better than new ones, as each plate acts as a condenser.

In the discussion which followed, Dr. Dolbear was asked if the state of the atmosphere, in any way, affects the operation of the instrument. I have used these instruments, said he, on an actual line between Boston and New York, in a night when it rained over the whole length of the line, and the whole line was as badly insulated as it well could be. I have also used it on the same line under the most favorable conditions for insulation, and could not really perceive much difference. It seemed to be as loud at one time as at another.

Pres. H. A. ROWLAND. — Of course this is on an entirely different principle from our telephone. What interested me considerably was the fact, that one could hear better when the plates were charged. The explanation theoretically is very simple, and it is the same as that the Thompson electrometer is more sensitive when the jar is charged than when it is not charged; the reason being, that the attraction is proportionate to the square of the difference of the potentials, rather than the simple difference of the potentials. Therefore a small difference in the quantity, when it is large, produces a greater effect than when it is small. So the explanation is exactly the same as that the Thompson electrometer is more sensitive when you have the jar charged than when you do not. So, the higher the charge one would get, the more sensitive the instrument would be. I was especially interested in it, because it was on such an entirely different principle from the Bell instrument. I don't wish to say any thing about patent laws or decisions on this subject, for they have nothing to do with this; but, scientifically, this is an entirely different instrument from the Bell instrument, and I am especially interested on that account.

Prof. T. C. MENDENHALL. — I profess not to have quite understood the statement made by Professor Dolbear. I should like to hear your own (the president's) opinion with regard to that charge which remains in spite of the fact that the two poles of the condenser are connected by conductors. I may have misunderstood the statement; but if that is correct, I should like to know whether that can be explained or not.

President ROWLAND. — Well, I suppose we all know how retentive an electroscope is of a charge. I suppose the idea is very similar in this case. I do not suppose the plates have a difference of potential. If you should leave them for a moment, I should suppose they would soon have a little return charge. If the two plates of the condenser were together, they would have the same potential. I understood it as merely a return charge. I do not know how Professor Dolbear understands it.

Professor DOLBEAR. — The instrument itself is a most delicate electrometer when tested in this way; and when it is charged and really in good working order, the gentlest tap upon the instrument serves to show that it is in good working order, for one can apply the instrument to his ear and hear himself talk. This is the case, even when the two plates of the condenser are connected with each other through the induction coil; and so, although they may have been there for hours, or even for days, — the difference between an instrument that has not been used and one that has been charged is very appreciable.

President ROWLAND. — I suppose in that case it would be simply from the charge of the varnished surface?

Professor DOLBEAR. — Yes: I think they retain their charge for a much longer time if the surface is varnished. I do think there is a difference between the behavior of this and the charged cable. If a cable be charged for half an hour by battery, it will require half an hour to run out again, but it will be at that time quite discharged. But that is not the case with this instrument.

President ROWLAND. — I should suppose it was the charge in the varnished surface.

Prof. W. A. ANTHONY. — Professor Dolbear did not say any thing about one advantage that this telephone has over the other, that struck me when I read the descriptions of it earlier, — that, in consequence of using this electricity at such a high potential, the ordinary telegraph-lines or other instruments would have very little effect upon it: therefore the telephone is very free from induction.

Professor DOLBEAR. — My experience has been in accordance with that theory. Electro-motive force from induction from telegraph-lines is ordinarily tolerably small, although there may be at times considerable strength of current. But, the electro-motive force being so strong in my circuit, it follows that the action of such induced currents is very slight, and does not interfere with work.

Prof. C. A. YOUNG. — I would like to inquire whether you have tried any experiments in putting the end of the wire to the ear to illustrate the sensitiveness of the ear?

Professor DOLBEAR. — Yes: I have heard simply by putting the end of an insulated wire to my ear, and listening. I consider the instrument as simply the enlarged terminal of a wire, and that you are actually listening at the end of a wire.

Mr. E. GRAY. — I have made a good many experiments in another line, which I may state briefly, which may throw some light upon this, and yet I think it is very well understood. You remember, some of you, reading of such experiments made in 1874, relating to the reproducing of music on a plate by simply rapping with the finger or with some animal tissue. Now, I made this experiment, which seems to prove to my mind that the operation is as Professor Dolbear has explained it. I set my revolving disc, which was a simple disc of zinc, revolving at a steady rate, giving it a pressure with the fingers. Then I had fifty cells of battery set up, as much as I could bear, passing through them, and had some one close the circuit with a Morse key. At the same time the key was closed, my finger would be jerked forward in the direction of the rotation of the disc; and it would remain in that forward condition, showing an increase of friction, until the key would be opened, and then it would drop back; showing that from some cause there was an increase of friction, either due to molecular disturbance, or, what is probably the case, to attraction between the finger and the plate. It is necessary, to produce this experiment, that the cuticle be perfectly dry. You must rub it a long time, and have it perfectly polished; and then the cuticle becomes a dielectric, and the body is charged with one kind of electricity, and the wire or the plate with another. Later I got some fairly good results in articulation by using a small diaphragm with all the conditions as nearly right as possible; and, having a current of sufficient electro-motive force, I could actually understand words produced on the end of my finger.

President ROWLAND. — What is the difference between that and Edison's motorphone ?

Mr. GRAY. — In Edison's motorphone, when the current was thrown on, there was a decrease of friction; there was chemical action taking place on the surface. In this case there is none, and there is an increase of friction when the current is on: perhaps 'current' is a bad word to use.

President ROWLAND. — The principle is the same.

Mr. GRAY.— One is a chemical action, which causes the friction to be less at the moment of charge. In this case, however, this is purely static contact, and increases the friction in the same manner that the plates are thrown together when they are charged in this telephone. And the motion, of course, or sound, is produced by a letting-go of the finger from the plate, and not by actual vibration, in the same sense that it takes place between the two plates in this receiver of Professor Dolbear.

President ROWLAND. — You attribute it to attraction?

Mr. GRAY. — Yes: my experiments seem to prove that; I presume, because there was adhesion, there was an increase of friction during the time of the charge and the letting-go, when the circuit was open. There was really no circuit except when the charge was taken off.

Sec. F. E. NIPHER. — In regard to the case of which Professor Dolbear spoke, when it might be supposed that electricity does actually pass from the line into the ground, it seems to me that that fact, so far as it did exist, would be prejudicial to the action of the instruments; that what we want to bring about is not a current, but as great a difference of potential as possible, between the plates.

List of other papers.

The following additional paper was read in this section: — An extension of the theorem of the virial to rotary oscillation, by *H. T. Eddy*.

PROCEEDINGS OF SECTION C. — CHEMISTRY.

Report of the committee on indexing the literature of chemical elements.

THE undersigned, a committee appointed at the Montreal meeting of the American association for the advancement of science, "to devise and inaugurate a plan for the proper indexing of the literature of the chemical elements," respectfully submit the following report.

The members have conferred with each other orally and by correspondence. Several plans have been suggested, and their merits discussed. Three methods of collecting material for the indexes may be named: —

1°. Revising the Catalogue of scientific papers published by the Royal Society (8 vols. 4to).

2°. Indexing special journals by different individuals, and collating the matter.

3°. The independent plan, whereby each chemist indexes all the journals available to him with reference to a given element, in which he is presumably especially interested.

Each of these schemes is open to objections, and has its difficulties. The first would necessitate an enormous amount of clerical labor, for which volunteers would scarcely be secured; besides, data previous to 1800 could not be obtained from this catalogue.

The second involves, also, securing a large number of self-sacrificing volunteers; and both plans would require a vast amount of editorial work on the part of this committee.

The third plan seems, to a majority of the committee, the only feasible one at present. On the independent plan, seven indexes have already been compiled. The best arrangement of material has also been considered; and here again a threefold problem occurs : —

1. Chronologically.
2. Alphabetically, by authors.
3. Topically.

The committee do not venture to dictate to independent workers, but recommend the chronological arrangement, with the understanding that a topical index accompany each monograph.

The best channel of publication has also been considered by the committee. All the indexes hitherto published have been printed in the annals of the New-York academy of sciences; and the academy has generously offered, through its officers, to continue its good work. The Smithsonian institution further agrees to distribute, free of expense, all circulars and documents in furtherance of this undertaking; an offer which is of greatest importance, and for which this committee expresses sincere thanks.

Since the appointment of the committee, Mr. Webb's index to the literature of electrolysis has been published in the annals of the New-York academy of sciences; and several chemists have expressed a willingness to co-operate in the proposed undertaking. Prof. R. B. Warder of Cincinnati has promised an index to the literature of the velocity of chemical reactions; and Dr. Henry Leffmann of Philadelphia proposes to index the important element arsenic.

Your committee present to the association this brief report of progress, and respectfully desire to be continued.

H. C. BOLTON, *Chairman;* IRA REMSEN; F. W. CLARKE; A. R. LEEDS; A. A. JULIEN.