ment, which was found to be 0·000208 microfarad. The area of each of the platinum plates, between which the india-rubber was placed, was 45·6 square centims. The thickness of the india-rubber was 0·106 centim., so that the calculated specific inductive capacity was 4·8. This seems too large; but an error, if there be one, in the determination of the capacity would have made the calculated resistance too small, whereas the resistance is much higher than that hitherto obtained with any of the forms of india-rubber experimented on by electricians. We found that the capacity of the electrometer quadrants was neglectable in these, and in the ebonite observations. We give the temperature and the corresponding values of $\frac{1}{t} \log \frac{E_1}{E_2}$, and of x in megohms for the points S, a, b, c, T of the curve ST.

	Temperature.	$\frac{1}{t} \log \frac{E_1}{E_2}$	x in megohms.
S	67° C.	0·0100	5391×10^6
a	75°	0·0187	2882×10^6
b	81°·6	0·0275	1960×10^6
c	86°·7	0·0390	1382×10^6
T	90°·7	0·0531	1015×10^6

II. "On the Viscosity of Dielectrics." No. I. By W. E. Ayrton and John Perry, Professors in the Imperial College of Engineering, Tokio, Japan. Communicated by Professor Sir W. Thomson, LL.D., F.R.S. Received October 2, 1877.

It is well known that india-rubber has a greater specific resistance and a less specific inductive capacity than gutta-percha. Now a popular explanation of this might be given as follows:—Imagine a portion of the india-rubber in an india-rubber condenser replaced by a metallic or other conducting substance either in a single piece or in the form of grains scattered throughout the mass of the dielectric, then the resistance of the condenser will be diminished while its capacity will be increased. As more and more metal is introduced into the dielectric the capacity will become greater and greater, so that at last, if all the india-rubber be replaced by metal, we ought to consider such a condenser as having an extremely great capacity, although on account of the conduction it would be impossible to experimentally measure the charge. If the above consideration which of course really applies to a dielectric of heterogeneous constitution, be also applicable to different homogeneous dielectrics, then we might expect to find that for all dielectrics the specific inductive capacity diminishes as the specific resistance increases. Now the experiments we have lately been engaged on concerning specific resistances enable us to prove that in the case of several well-known dielectrics this is really so, as will be seen from the following table:—

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Substance.	Specific Inductive Capacity. Air equals 1.	Authority for Capacity.	Approximate resistance per cubic centimetre in ohms after several minutes' electrification.	Temperature, Centigrade.	Authority for Resistance.
Dilute sulphuric acid	700×10^6	C. Varley	Less than 4	24	F. Jenkin.
Mica	5	F. Jenkin	84×10^{12}	20	Ayrton and Perry.
Gutta-percha	4·2	F. Jenkin	450×10^{12}	24	Standard adopted by Mr. Latimer Clark.
Shell-lac	{1·95 ? }	F. Jenkin	9000×10^{12}	28	Ayrton and Perry.
Hooper's material	3·3	Calculated from numbers given, page 67, Clark and Sabine	15000×10^{12}	24	Recent cable tests.
Ebonite	3·15	Boltzmann	28000×10^{12}	46	Ayrton and Perry.
Paraffin	1·98 2·32	Gibson and Barclay Boltzmann	34000×10^{12}	46	Ayrton and Perry.
Glass	1·90	F. Jenkin	Not yet measured with accuracy, as will be shown further on in this paper, but certainly greater than any of the above resistances.		
Air	1	..	Practically infinite.		

It will be observed that the conventional specific inductive capacity of shell-lac does not follow our law that small specific inductive capacity accompanies high specific resistance. We are inclined, however, to think that the real capacity may be far greater than this conventional one, since M. Boltzmann proved in 1874 that the true specific inductive capacities of sulphur and resin were respectively twice and one and a half times as great as the capacities for these substances given in books. Up to the present time we have not ourselves had an opportunity of finding the true specific inductive capacity of shell-lac.

In the preceding table the resistance of ebonite is given in accordance with the rule stated in our last paper, that is, the resistance given in that paper has been reduced in the ratio of 1·7 to 3·15. The specific inductive capacity of Hooper's india-rubber is usually given as 3·1; using, however, the numbers given on page 67, Clark and Sabine Electrical Tables for the capacities of a sheet of air, and of the india-rubber one square foot in area, and $\frac{1}{1000}$ inch in thickness, we find the ratio to be 3·3.

Following out the relationship shown in the above table, we may consider a highly conducting metal as a dielectric of extremely great specific inductive capacity, just as air, which has an extremely great resistance, is a dielectric having (as far as experiment has yet shown) the least specific inductive capacity. But as the resistance of the air although very great is not infinite, so it is reasonable to conceive the existence of a space more highly insulating than when filled with air, and at the same time having a less specific inductive capacity.

When we examine the phenomena of induction in this way we might expect to find a simple relation between the values of specific resistance and specific inductive capacity, but further consideration shows that the received notions of both these qualities of dielectrics are so badly defined that the first step is to establish clearness in our conceptions before endeavouring to determine in what exact way specific inductive capacity is a function of specific resistance.

As regards the capacity of an air condenser there cannot be much vagueness, for there is neither perceptible absorption nor true conduction, although further on we shall give our reasons for believing there is some absorption. But in all cases in which the dielectric is not a gas there are absorption and conduction phenomena of a complicated kind, and experiment hitherto has not accurately defined what is meant by capacity in such cases.

We are now carrying on a series of observations on induction phenomena in flint glass jars and other condensers with dielectrics having a greater and less specific conductivity than glass. It is an investigation which progresses very slowly, as some of the single experiments last nearly a month. Certain curves obtained for time risings and fallings of potential in condensers bore striking resemblances to the

curve expressing the time increase of strain in a substance subjected to a constant stress, and careful examination shows that all our results up to the present time bear so close an analogy with the stress and strain phenomena in viscous substances, that we feel that this analogy means a physical connexion. We therefore propose, even before the completion of the experiments, to show in the present paper the bearing of this analogy; since not only does it constitute an extension of Faraday's stress theory of induction, but, in addition, it forms a verification of the extension of our knowledge regarding stress and strain phenomena, as afforded by our recent experiments on the viscosity of substances. Besides this, the theory that we are about to sketch roughly, and which we hope to establish in all its details by experiment, is valuable in furnishing an easily conceived image of the internal action taking place in a dielectric.

A complete series of experiments will soon be ready for publication, showing that almost every body which can be examined exhibits a time increase of strain under a constant stress. Thus, when a small couple, much less than that which is required to produce any permanent set, is applied to twist a prism, there is a rapid production of torsion due to a property of the material measured usually by a number called the modulus of rigidity; the rapidity depending mainly on the mass which has to be moved, and also depending on the viscous yielding (or on something which is related to the viscous yielding) of the material. If we could eliminate the effect of inertia, then not an instantaneous but a very rapid production of torsion would be observed, the production of strain along the prism not being instantaneous even at the beginning, but proceeding with a velocity comparable with the velocity of electric induction. After a very short interval the torsion no longer increases rapidly, and only its amount (D) at the end of the interval is what has usually been taken into consideration by physicists and engineers. But it should be known that it is only after a long time that the production of strain ceases, and the additional torsion (d) in glass and steel fibres after some hours or days in some of our experiments became equal to ($\frac{1}{2}$D). If when the production of strain appears to have ceased the twisting couple be suddenly removed, then there is a rapid but not instantaneous reduction of strain (D), and an additional reduction (d) effected after a considerable time.

Thus for all strained substances that we have experimented on, the equilibriated state is one in which there is a free strain or a strain that is almost instantly removable, and there is also an absorbed strain which is only slowly produced, and which is only slowly removable.

Exactly in the same way, when a condenser is charged by a battery of electromotive force (V), the charge at first increases rapidly, due to a property of the condenser called its capacity (S); this rapidity

depends on the supply of electricity by the battery, and on a less or greater resistance to rapid motion offered by the viscosity (or on something related to the viscosity) of the dielectric.

If we could eliminate the effect of slow supply by the battery (and this is always eliminated in the charging of glass condensers by voltaic cells) we should still have not an instantaneous but a very rapid charging of the condenser; the rate of this charging does not at present appear to be experimentally measurable, but ballistic galvanometers measure the time integral of the current. We know from the shape of the curve which shows the increase of charge with time, and from the improbability of any want of continuity, that the rise of charge is not really instantaneous. After a very short interval when the rate of increase of charge is incomparably smaller than the mean rate during the interval, the total amount of charge (SV) is what has usually been taken into account by physicists. But this charge does, however, increase sometimes for many days to a maximum $\{(S+s)V\}$, and in certain experiments with a flint glass condenser (s) at the end of about ten days was equal to $(\frac{1}{2}S)$. If, when the charge seems to increase no longer (we must not confound with increase of charge the small flow into the condenser which continues even after a long time, and which measures true conductivity) we connect the coatings of the condenser, there is a rapid but not instantaneous loss of charge (SV), and a further loss (sV) only occurring after a long period. Thus for our condenser (exactly as for the strained viscous prism) the equilibriated state for any given difference of potentials is one in which there is a free charge, that is, a charge almost instantly removable, and an absorbed charge, that is, a charge only slowly produced and slowly removable. It has usually been considered that this absorbed charge is quite distinct from conduction, but we know that when strain is being produced in a viscous substance, and all substances seem to be viscous, some of the energy is converted into heat, therefore if, as we consider, absorption is the production of strain, it must be accompanied by the generation of heat, and the conversion of electric energy into heat is most suitably termed conduction. We cannot, therefore, have absorption without conduction, and further, we consider all true conduction as an absorption in which the whole quantity of electricity is converted into heat, or into some form of energy not electrical. Again, when strain is produced by mechanical stress, we know that the more rapid is the production of strain the greater will be the amount of heat produced by internal friction. Reasoning by analogy, therefore, we may conclude that as the rate of production of electric strain grows less and less as the interval elapsing since charging increases, therefore the rate of conversion of electric energy into heat, that is, conduction, also grows less and less, and, therefore, it is correct to say that the resistance of a dielectric does

really increase by electrification. Besides this there is, of course, some of the absorbed energy which is recoverable, and which, therefore, must not be confounded with conduction, just as the energy recoverable from a deflected beam must be distinguished from that lost through conversion into heat on account of internal friction.

True conduction in a condenser, that is, the amount flowing in after much time has elapsed, and which is entirely lost as electric energy, however difficult it may be to measure with accuracy, is perhaps the only property of dielectrics having no reference to exact time, and, therefore, enabling comparison to be made between different dielectrics; that is, when the flow into a dielectric is steady this flow may possibly be proportional to the electromotive force. We say advisedly "may possibly," because we have reasons for thinking our ideas of stress and strain will not only throw light on the questions of absorption previously referred to, but will in addition suggest an explanation of the hitherto apparent inconsistency between the laws for change of resistance by heat in conductors and in insulators. If that be so, then we think it may be possible that we shall find out that Ohm's law is always but an approximation, and that this approximation is the nearer and nearer the truth the less the resistance of the conductor, and that for insulators the current, after it has become steady, may obey some such law as that experimentally determined by Mr. C. Varley for conduction through rarefied gases. (See "Proceedings Royal Society," October 5th, 1870.)

True conduction is found to be extremely small after some weeks for the flint glass in Sir W. Thomson's electrometers, care being taken that the charge in the jar is left quite undisturbed by induction, from electrified moving neighbouring bodies for example, whereas there is much loss of charge due to conductivity as well as that due to absorption in the first few days after charging. Thus two electrometer jars in our possession when left untouched for some weeks were found, if the first measurement were made not much less than one week after charging, to retain nearly the whole of their charge, the electrometers being in constant use, but the replenisher not touched during the entire time, to avoid alteration of internal distribution. Now when the conductivity of glass is measured in any ordinary way a few hours after charging, it seems to be very great even when we are able to eliminate true absorption, which is all recoverable as electric energy. Inattention to these considerations, we think, led some of the students of Sir W. Thomson to infer that the true conductivity of flint glass was incomparably greater than it really is.

In fact, we believe that up to the present time no experiments have ever been made which determine the amount of the true conductivity of flint glass, that is, the conductivity a long time after charging. Since very hot solid glass is like pitch, a "truly viscous fluid," that is

goes on continually yielding under the action of a constant stress, we must assume, since we cannot imagine a breach of continuity in the phenomenon, that glass even when cold is also a "truly viscous fluid," although the gain of strain per day may be almost infinitely small after the first few days. Similarly, as the true conductivity is very considerable in hot glass, we may conclude that there does exist true conductivity in cold glass, although the amount will probably be so small as to make its separation from surface conduction or other extraneous loss extremely difficult.

From the curves we have obtained of the charging of condensers, and assuming that there is no discontinuity, we must assume that even the first charging is itself a very rapid absorption, and since there is viscosity, even the very first charging must be accompanied with a generation of heat, that is, true conduction. Also since it is known that gases, like all other substances, are to a certain extent viscous, we cannot believe that air and other gaseous condensers show absolutely no absorptive phenomena, in fact, sufficiently accurate experiments have not yet been made on the subject.

We conclude, therefore, that the less the specific resistance of a substance the greater is its molecular plasticity, and the more plastic the substance is the greater will be the first charge; therefore from the stress and strain analogy it follows that the less the specific resistance of a substance the greater will generally be the specific inductive capacity, the result obtained experimentally at the commencement of this paper: according to what law, however, the one increases as the other diminishes, we are not at present in a position to state.

From all that precedes it follows that when the potential of a body surrounded by a dielectric is altered by induction, a portion of the electric energy is converted into heat, the amount being greater as the dielectric is more viscous. Consequently a charged body A, perfectly surrounded by a dielectric, may be discharged without contact with any conductor, by alternately bringing near and withdrawing a distant conductor B; for the capacity of the arrangement is alternately getting greater and less, therefore the potentials must be alternately growing less and greater, and since all alteration of potential is accompanied by an alteration of strain in the viscous substance composing the dielectric, the potential energy of the system must be gradually converted into heat, whereas from the almost infinite resistance of the dielectric, if the bodies were motionless, this result could never have been attained. Consequently since the particles of any body are in rapid motion, they must all tend to acquire the same potential, even if we imagine them separated by a dielectric of very great resistance, provided the relative motions are motions of translation. But if the movements are motions of rotation only, then this equalization of potential will only take place after a very great time, if it takes place

at all. This difference may, perhaps, explain why metals conduct so much better than glass, &c.

III. "Recent Experiments on Fog-Signals." By Dr. TYNDALL, F.R.S., Professor of Natural Philosophy in the Royal Institution. Received March 14, 1878.

Our most intense coast-lights, including the six-wick lamp, the Wigham gas-light, and the electric light, being intended to aid the mariner in heavy weather, may be regarded, in a certain sense, as fog-signals. But fog, when thick, is intractable to light; the sun cannot penetrate it, much less any terrestrial source of illumination. Hence the necessity of employing sound-signals in dense fogs. Bells, gongs, horns, guns, and syrens have been used for this purpose; but it is mainly, if not wholly, explosive signals that I have now to submit to the notice of the Society. During the long, laborious, and, I venture to think, memorable series of observations conducted under the auspices of the Elder Brethren of the Trinity House at the South Foreland in 1872 and 1873, it was proved that a short $5\frac{1}{2}$-inch howitzer, firing 3 lbs. of powder, yielded a louder report than a long 18-pounder firing the same charge. Here was a hint to be acted on by the Elder Brethren. The effectiveness of the sound depended on the shape of the gun, and as it could not be assumed that in the howitzer we had hit accidentally upon the best possible shape, arrangements were made with the War Office for the construction of a gun specially calculated to produce the loudest sound attainable from the combustion of 3 lbs. of powder. To prevent the unnecessary landward waste of the sound, the gun was furnished with a parabolic muzzle, intended to project the sound over the sea, where it was most needed. The construction of this gun was based on a searching series of experiments executed at Woolwich with small models, provided with muzzles of various kinds. The gun was constructed on the principle of the revolver, its various chambers being loaded and brought in rapid succession into the firing position. The performance of the gun proved the correctness of the principles on which its construction was based.

It had been a widely spread opinion among artillerists, that a bronze gun emits a specially loud report. I doubted from the outset whether this would help us, and in a letter dated 22nd April, 1874, ventured to express myself thus:—"The report of a gun, as affecting an observer close at hand, is made up of two factors—the sound due to the shock of the air by the violently expanding gas, and the sound derived from the vibrations of the gun, which, to some extent, rings like a bell. This latter, I apprehend, will disappear at considerable distances."