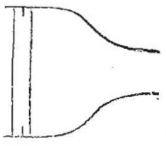
reasoning as that by which the induction of currents is deduced from the force exerted between a circuit and a magnet and the existence of contact electromotive force from the Peltier effect, it follows that a current should exist if two zinc electrodes connected by a wire are immersed in a solution of sulphate of zinc, the direction of the current being (in the solution) from the upper to the lower electrode.

"I tested this a few days ago, using a glass tube eighteen inches long, filled with a saturated solution of sulphate of copper and closed by copper caps with wires attached.



"On connecting the wires with a very delicate Thomson's astatic galvanometer belonging to Prof. Halford, a very considerable deflection was produced (200 divisions) when the tube was held vertically, the direction of the deflection being reversed when the tube was reversed.

"If the tube, after being held vertically, was placed in a horizontal position, the deflection diminished, but several minutes elapsed before the index came to zero, which it eventually did. I cannot explain the time taken. I am now preparing to test the actual loss of weight of the upper electrode.

"I have the honour to be, Sir,
"Your obedient servant,
"F. J. PIRANI,
"Lecturer on Natural Philosophy and Logic, University of Melbourne.

"P.S.—If the phenomenon has not been noticed before I shall be obliged if you will kindly communicate it to NATURE.
"F. J. P."

The Telephone

I HAVE been much interested in the communication by Dr. Röntgen on a telephonic alarum. During the past six or seven weeks, in investigating the phenomena of the telephone, chiefly as to the suggestions they offer regarding the mechanism of nervous transmission, I have frequently shown to friends the striking experiment described by Dr. Röntgen, and, amongst others, to Sir William Thomson. It has succeeded with Ut_2, Ut_3, and with numerous forks up to Ut_5, but, as stated by Dr. Röntgen, the best result was obtained with Ut_4. With those below this pitch the tone was feeble, whilst with those above it it was transient, in consequence of the difficulty of keeping the small fork going. With Ut_2, worked continuously by an electro-magnet, another fork of the same pitch sounded loudly and steadily. I have also been engaged in some endeavours to record on a moving surface the vibrations of the plate. These have been so successful as to show that it is only a question of delicate adjustment. In endeavouring to utilise one telephone by making several friends listen at once, I have found that by fixing the metal disc to a thin membrane over a small cavity filled with air, like a Koenig's capsule, and having a number of flexible leaden tubes connected with it, an ear placed at the end of each tube will hear distinctly. JOHN G. MCKENDRICK

Physiological Laboratory, University of Glasgow,
December 31, 1877

The Radiometer and its Lessons

PROF. OSBORNE REYNOLDS (vol. xviii. p. 121) appears to have done himself less than justice in the extracts he has sent you from his earlier papers, as representing his published views on the action of residual gas in radiometers. For the extracts do not suffice to constitute an explanation of this action, whereas the papers from which he makes the extracts contained what, if true, might have been an explanation of the action of residual gas, along with much else that is admittedly erroneous; and although those papers (the only ones published before mine) conclude with Prof. Reynolds's own expression of opinion that residual gas is not the cause of the force observed by Mr. Crookes.

He quotes three paragraphs. In two of these he recited the fundamental principle in the kinetic theory of gases which he sought to apply. To obtain an explanation of the phenomenon from this principle according to the method pursued by Prof. Reynolds, it was necessary for him (*a*) to establish a law connecting an excess of force perpendicular to the disc with a flow of heat in radiometers, and (*b*) to indicate agencies which could occasion a sufficient flow of heat. He quotes the passage in which he announced the result of his, as I believe, unsuccessful attempt to accomplish the former of these, but he omits the equally necessary passage in which he dealt with the latter. It will be found at page 407 of the *Proceedings* of the Royal Society, vol. xxii., and is couched in the following terms :—"It must be remembered that ϵ [which measures the outflow of heat] depends on the rate at which cold particles will come up to the hot surface, which is very slow when it depends only on the diffusion of the particles of the gas *inter se*, and the diffusion of the heat among them. It will be much increased by convection currents." If this passage, as was requisite, had been added to the extracts made by Prof. Reynolds, it would have brought his recent account of the views he had announced into conformity with my account of them.

In connection with this subject it should be observed that Prof. Osborne Reynolds has in express terms excluded from his explanation that which I believe to be the real agency which brings a sufficient supply of cold molecules up to the hot surface, for he states, in his letter to NATURE (vol. xvii., p. 27), that "it is incompatible with his explanation that the increase resulting from rarefaction in the mean length of the path of the gaseous molecules would favour the action." Now the polarisation of the gas depends on the ratio which this mean length bears to the interval between heater and cooler.

I cannot find anywhere in Prof. Osborne Reynolds's writings an explanation of the thing to be explained, viz., that the stress in a Crookes's layer is different in one direction from what it is at right angles to that direction. Let v be the component of the momenta of the molecules striking a square unit of the heater in the unit of time, resolved perpendicularly towards the heater; and let u be the corresponding normal component of their momenta from the heater, when they are thrown off. Then $u + v$ is the pressure on the heater. Now if u and v could result respectively from *unpolarised* motions in the gas, the momentum resolved parallel to the heater would be $\frac{1}{2}u + \frac{1}{2}v$ from left to right, with an equal momentum from right to left. Adding these we find $u + v$ the pressure of the gas parallel to the heater. This is equal to the normal pressure, and, therefore, under these circumstances, there would be no Crookes's force whatever. It is only when we take the polarisation of the gas into account that the momenta resolved parallel to the heater become different from $\frac{1}{2}u$ and $\frac{1}{2}v$.

Prof. Osborne Reynolds says that my views are at variance with results arrived at by Clausius and other discoverers in this branch of physics. I do not myself value appeals to authority in matters of science. But it so happens that here again it appears to be Prof. Reynolds who makes the mistake. Clausius, in his great memoir on the conduction of heat by gases, published in 1862 (*Phil. Mag.*, vol. xxiii. p. 529), warns his readers against the very error into which Prof. Reynolds seems to fall, and points out that there "are obvious limits" beyond which the laws he had discovered for the conduction of heat do not prevail, one of which limits is that the gas "must not be so expanded that the mean length of excursion of the molecules becomes so great that its higher powers cannot be neglected." Now it is just to this excepted case, to the Sprengel vacua experimented on by Mr. Crookes, that Prof. Osborne Reynolds applies the laws of conduction, and he then objects to my theory that it does not agree with the laws so misapplied. The phenomenon of Crookes's stress appears to come into existence precisely in Clausius's excepted case, viz., so soon as the ratio which the mean length of excursion of the molecules bears to the interval between heater and cooler, is such, that when multiplied by a function of the temperatures of the heater and cooler, its square is of appreciable magnitude in Clausius's equations. This may be experimentally secured either by placing the heater and cooler very close together, as in experiments upon spheroidal drops, or by excessively attenuating the gas so as to lengthen the free paths of the molecules sufficiently, as in radiometers. G. JOHNSTONE STONEY

Dublin, December 20

POSTSCRIPT, December 22.—I have just seen Prof. Schuster's letter (NATURE, vol. xvii. p. 143). Dr. Schuster will pardon me if I say that he has adopted a scarcely legitimate course in introducing into a discussion on priority his present reminiscence of one of the conversations about the radiometer which he held with his friend, Prof. Osborne Reynolds, two and a half years ago. The language in which he reports it is foreign to Prof. Reynolds's style of composition, so that we may conclude we are dealing with Dr. Schuster's words, and the words which occurred to him after he had read much else on the subject. No