Lenard's observations and his own experiments into account, Dr. Simpson concludes that it is not an induced effect, due to an external source; he considers that there is an actual production of electricity in the *subdivision* of large raindrops.

Dr. Simpson's conclusion has long been in my mind. Latterly, the subject has been an attractive one to me, on account of the views I have formed of the composition of water and of the chemical changes attending alteration in the size of drops, referred to in my recent communication to the Royal Society (Roy. Soc. Proc. A, vol. 103, p. 616, 1923). I was much impressed by a lecture at the Royal Institution given early in the year by Dr. Simpson (NATURE, April 14); this, together with a violent hail-thunderstorm which I experienced while yachting in August, led me to look more closely into the problem.

Assuming that water be the cause, the view I should be inclined to take is the converse of that advocated by Dr. Simpson. Granting, for the sake of argument, that changes in water can give rise to free electricity, the fusion of small drops into large would seem to be the more likely process—this being a positive change, in the sense that energy is liberated, while the division of large drops should involve a loss of energy. I assume that the small drops are richer in hydrone than the larger and that changes in composition of the water take place such as I have postulated in my recent communication.

Going further, however, can it be granted that chemical changes *in a wholly liquid circuit* ever give rise to sensible electricity—must not the circuit *be tapped by conducting electrodes* to make this obvious? We must assume that the interactions are primarily electrolytic, but is not the electrical energy, in such cases, always lowered into heat energy?

The question is of fundamental importance, and it is on this account that I make bold to be critical of a solution of a problem outside my field; yet it is one of the borderland issues which chemist and physicist should jointly consider.

Assuming that my interpretation be correct, may not the great rise in potential required to produce lightning have its origin in the coalescence or cooperation of minute drops charged by an external source?

Lenard (*Wied. Ann.*, 1892, 46, p. 584) dealt with the effect, in the first instance, in studying the electricity of waterfalls. His later laboratory experiments led him to the conclusion that it was due to the impact of separate drops upon a flat surface. The water was allowed to splash *into a zinc tray*. Both he and Dr. Simpson found it necessary to use distilled water; that from the mains gave little or no result. The air potential observed was negative, but with a solution of salt it was slightly positive. Up to a certain point, the potential increased rapidly with the length of the jet. Various liquids other than water were tried: the potential varied in sign and magnitude, but the effect was slight as compared with the water effect. Lenard seems to think that the effect has its origin in a contact difference of potential between gas and liquid. All seems to me to point to chemical interchange being at the root of the phenomena and that it is not a mere water effect.

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Earthquake Warnings.

THE recent disaster in Japan demonstrates the importance of endeavouring to ascertain if there are any premonitory indications of a coming earthquake shock which can be recognised and thus enable a warning to be given of its approach.

It seems probable that the rupture, whatever its nature may be, that gives rise to the actual vibratory shock of an earthquake is preceded by a strain or distortion of the earth's crust, which gradually increases till the stress that causes it is suddenly released. The existence of this strain should be evidenced by a progressive sag or tilt of the surface, local and minute in amount, no doubt, but probably sufficiently large to be detected.

In the Milne-Shaw seismometer the vibrations proceeding from distant earthquakes are recorded on sensitised paper on a rotating cylinder by a spot of light reflected from a mirror coupled to the boom of a horizontal pendulum. Ordinarily it is only these vibrations that are taken into consideration, but the same instrument will also indicate a slow tilt of the ground, provided that the exact position of the spot of light can be recorded and measured. In some instruments recently constructed, one of which is being installed in Uganda, this is effected by the use of a second, stationary mirror, which throws another spot of light in a fixed position on the cylinder, and traces a straight line on the record. If there is a tilt of the earth's surface it will be indicated by a variation in the distance between the mean position of the line due to the moving mirror from that of the line due to the fixed mirror, unless of course the tilt is in a direction parallel to the horizontal pendulum. Such an instrument is capable of showing a tilt of $\frac{1}{10}''$ by a movement of the indicating spot of light through 1 mm. If two instruments are employed with their horizontal pendulums at right angles to each other the direction and amount of the tilt will be exactly determined. Near the sea the rise and fall of the tide causes a slow tilt and other changes of a slow periodic character are known, but these can be allowed for and could easily be distinguished from a progressive movement indicating the approaching occurrence of an earthquake in the neighbourhood.

It seems very desirable that such instruments should be installed in localities which are known to be subject to earthquakes.

If it be found that shocks are in fact heralded by a definite tilt, it may be possible to arrange for an electric bell to attract the attention of the observer when such a tilt occurs. If he is satisfied that there is sufficient evidence of an approaching earthquake, a general alarm can be sounded. In this way a warning might be given several hours, or even days, before the shock occurred.

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Human Embryology and Evolution.

IN his reply to Prof. MacBride (NATURE, Sept. 8) Sir Arthur Keith states that in his Huxley lecture he neither affirms nor denies the doctrine of use-inheritance, but that he does deny that Lamarckism has had no part in the evolution of man. If these words were to be taken literally as expressing Sir Arthur Keith's meaning, he and I would be to a great extent in agreement, but it is obvious that the double negative was an accidental mistake, and that Sir Arthur Keith meant to deny that Lamarckism had *any* part in the evolution of man.

I have read the report of his Huxley lecture to the medical students of Charing Cross Hospital Medical School (NATURE, Aug. 18), and it seems to me difficult for an evolutionist to follow his train of thought or reasoning. He does not distinguish between the development of the individual and the evolution of the race, between ontogeny and phylogeny. He discusses the manner in which adaptations appear