from the conjugant, and only in the first days of the conjugating fit is there an approach to a very slight secondary mode at the conjugating type value. This may correspond in the first days to a very small percentage of individuals in a "conjugating mood" among the non-conjugants. The skewness, however, as measured by Dr. Pearl's numbers, although slight, is the *other* way, showing that the non-conjugant population might best be conceived as a distribution *wanting* a portion about the conjugant type, and not as a population with an addition on that side, as it must be in the case of a mixture of non-conjugants and potential conjugants. Taking the variability of (*a*) all populations in which there were no conjugants, (*b*) populations of non-conjugants in which conjugants appear, and (*c*) populations entirely consisting of conjugants, we have the three numbers 8·7, 8·6, and 7·9, which suffice to show that non-conjugants in a conjugating population are practically identical in variability with non-conjugants in a non-conjugating population, *i.e.* the potential conjugate is a very small proportion of the population, conjugation taking place rapidly after the conjugating phase is reached.

I am sorry to have to reply to Mr. Lister in this fashion. I fear, to use his own phrase, he will still "go away unedified" from "the biometric side of the church." But the time has come when vague insinuations based on no complete study of biometry must be replaced by some attempt to understand before criticism is passed. Above all, in a case like the present, a total disregard of the contents of Dr. Pearl's memoir and a suggestion that he has made errors and overlooked difficulties, which he has actually dealt with at every turn, is not to the credit of the critic. A man who has spent years in studying Paramæcia, and made thousands of measurements after much consideration of the difficulties, may reasonably expect a different type of criticism from another who clearly has attempted no such series of measurements, and whose authority for *ex cathedra* utterances may therefore be well called into question. Dr. Pearl's full paper is now in type, and I do not think his reputation will suffer when the paper is tested against the *a priori* criticisms which Mr. Lister has passed upon it. KARL PEARSON.

Biometric Laboratory, University College, London, October 12.

Radium and Geology.

IN NATURE of October 11 (p. 585) two letters appear on this subject, in reply to which a few words may perhaps usefully be said. Mr. Fisher's principal point is that if the earth's internal heat is maintained by radium, there is no room left for that shrinkage of the globe by cooling which some geological theories require. I think that the difficulty is only apparent. The duration of radium, it is generally agreed, is limited to a few thousand years. The supply must be in some way maintained, or there could be no radium on the earth now. Writers on radioactivity are generally agreed that the radium supply is kept up by the spontaneous change of uranium into radium.

Since radium is found in ordinary rocks, we must, on the received theory, suppose that uranium also exists in these rocks. It may be objected that uranium is never entered as one of the constituents found by chemical analysis. But, since the quantity to be expected is only of the order of 1/1000th of 1 per cent., this is not surprising. It might be possible, by very special methods, to detect uranium in granite, but I think in any case we may feel confident that it is there.

Everything depends on this initial supply of uranium. It gradually passes into radium, and, after that, into some inert form. The supply of uranium cannot last for ever. Its gradual diminution must involve the cooling and shrinkage of the globe.

It may perhaps be thought in these circumstances illegitimate to equate the escape of heat per second from the earth to the supply generated by radium in that time. There is reason, however, to feel pretty sure that thermal equilibrium is practically established in a time small in comparison with the duration of uranium, so that the rate of change in the amount of the latter can have no appreciable influence on the distribution of temperature in the globe at any moment.

Mr. Palmer suggests that if the earth's internal heat is due to radium, the moon ought to be internally hot too, and its volcanoes should be active. I discussed the question of the moon's internal heat in my first paper (Proc. Roy. Soc., A, vol. lxxvii., p. 472). I quote from that paper:—"It has generally been supposed that the lunar volcanoes are extinct. But that view seems to rest chiefly on an *a priori* conviction that the moon has no internal heat. As Prof. W. H. Pickering has pointed out, all those observers who have made a special study of the moon have believed in the reality of changes occurring there."

Even if there were good reason to be sure that the lunar volcanoes were extinct, that would still be inconclusive. For it is believed by many geologists that volcanic action is due to the penetration of surface water to the hot interior of the globe. Thus volcanic inertness may be due, not to the absence of internal heat, but to the absence of surface water. R. J. STRUTT.

The Rusting of Iron.

IN my remarks on "The Rusting of Iron," published in NATURE of September 27, I directed attention to the fact that pure hydrogen peroxide solution was rapidly decomposed by cast-iron, the latter becoming covered with rust. This, I stated, "was, no doubt, due to catalytic action."

In his friendly criticism of my remarks, Dr. Gerald T. Moody writes, in NATURE of October 4, "that the metal becomes covered with rust in a few minutes, is not, however, to be referred to catalytic action, as Mr. Friend suggests, but is a consequence of the formation of acids by the oxidation of some of the impurities present in the iron, and of the subsequent electrolytic action."

That acids are formed in the above manner may be regarded as certain. These attack the iron, forming minute quantities of salts, which are decomposed by the oxygen of the peroxide, yielding rust, and liberating the acid, which can now attack more iron. In this way a small quantity of acid may be instrumental in oxidising a large quantity of iron. In other words, the acid is a catalyser, and the reaction is analogous to the rusting of pure iron in the presence of carbonic acid, oxygen, and water. The particular acid or acids which will cause this catalytic action must depend, of course, on the sample of iron used.

For the same reason "the intensity of action will be determined by the amount of acid formed on the surface of each particular sample of metal, when in contact with the peroxide." It is thus unnecessary to assume an electrolytic action, as Dr. Moody suggests. This is supported by the fact that the same result may be obtained by employing pure iron, and commercial hydrogen peroxide, which invariably contains hydrochloric acid and other impurities, as Dr. Moody has himself pointed out.

Würzburg, October 9. J. NEWTON FRIEND.

Optical Illusions.

IN your issue for September 27 a description of some optical illusions furnished by revolving fans recalled to my mind a very powerful illusion which I noticed some time ago, but for which I have not been able to furnish a satisfactory explanation.

A thaumatrope card (*i.e.* a card having a cage pictured on its one side and a bird on its other side) was mounted so as to turn round a vertical median axis at a speed of about two revolutions a second.

When an observer, viewing the rotating card from a distance of 5 feet or more, shuts one of his eyes, the card appears instantly to reverse its direction of rotation. (At the same time the axis of rotation appears to tilt a little away from the vertical.) On reopening the closed eye the illusion vanishes, and the card again appears to assume its true direction of rotation.

I showed the illusion to several friends, who all agreed as to its striking perfection. DOUGLAS CARNEGIE.

Newcastle-on-Tyne, October 10.