brium amount cannot certainly be greater than that equivalent in α activity to 2 mg. of uranium. At present the preparation has reached about one-fourth of its equilibrium value as regards actinium-X.

In ten years, of the 13·2 mg. of radium in the preparation, 0·053 mg. would have disintegrated. On the assumption that uranium is the primary parent of actinium, Rutherford has calculated that 8 per cent. of the atoms disintegrating must choose the actinium route ("Radioactive Substances," p. 523). So that, if it were formed from radium, the amount of actinium present in the preparation would be 0·0042 mg. But the active deposit from this quantity has an α activity not greater than 2 mg. of uranium. Hence the period of average life of actinium must be at least fifteen million years, the quantity in minerals must be at least 170 grams per ton of uranium, and the α activity of pure actinium in equilibrium could not be greater than 1650 times that of uranium. But a specimen of actinium, prepared and presented to me by Dr. Giesel, must have, judging from a cursory examination, a far greater activity than this, and Mme. Curie ("Radioactivité," I., p. 189) speaks of some actinium preparations as of the order of 100,000 times as active as uranium. All the researches go to show that its actual quantity in minerals is very small, and, if there were anything like 500 times as much actinium as radium in minerals, one would have expected it long ago to have been isolated and its spectrum and chemical reactions characterised. So that the experiments appear to disprove the possibility that actinium can be formed directly from radium. Similar arguments to those above may be used to show that it cannot be a primary radio-element, and its origin remains still a mystery. In the current number of the *Physikalische Zeitschrift* (p. 752) Hahn and Meitner modify my original suggestion and suppose that the branching of the uranium series takes place at uranium-X, two simultaneous β-ray changes occurring, which produce two eka-tantalums, one the known short-lived β-ray-giving product, and the other a still unknown long-lived α-ray-giving parent of actinium, also in group VA. There seems nothing improbable about this. It is almost the only other alternative remaining to be tested, and it should not be difficult to settle by experiment.

FREDERICK SODDY.

Physical Chemistry Laboratory,
University of Glasgow, August 16.

Radium and the Evolution of the Earth's Crust.

HAVING been away from home, I did not see Mr. Holmes's letter on radium and the evolution of the earth's crust, contained in NATURE of June 19, until some weeks after its publication, and thought that the interest in the subject would have so far passed as to make it not worth while referring to, what I believe to be, a considerable misapprehension of the structure of the earth as revealed by earthquakes. Later correspondence has shown that interest in the subject has not waned, and as a correct appreciation of what has been established regarding the constitution of the interior of the earth seems likely to remove some of the difficulties which have arisen out of the study of radio-activity, it may be useful to review the results obtained from the study of the transmission of earthquake waves to long distances.

To begin with, it must be distinctly understood that this line of research can tell us nothing, directly, regarding the chemical composition of the earth, nor can we distinguish between stony and metallic material; all that can be established is the rate at which two distinct forms of wave motion are transmitted, and if, at any particular depth, we find a marked change in these rates of transmission, we may say that it is caused by a change, either in chemical composition or physical state, of the material through which the waves have travelled. With this premised; the first great change takes place at, probably, about ten miles or so from the surface, and seems to correspond with the passage from the heterogeneous and fractured rocks of the outermost skin to more homogeneous material. Below this, and to a depth of about 100 miles, it is difficult to say whether any further change takes place; there are indications of change at about fifty and about one hundred miles, but it is not such as has a great effect on the rate of transmission of the simpler forms of elastic waves, and, as the differences in the time intervals concerned are not of a greater order of magnitude than the inevitable uncertainties of observation, it is difficult to be certain of the reality of the supposed alteration.

Below a depth of about 100 miles there is no evidence of any change until a depth of about 2400 miles is reached; throughout this layer there is a progressive increase of elasticity, but it is gradual and seems to be directly connected with the increase of pressure, with the result that the material, whatever it may be, develops a high degree not only of resistance to compression but also of rigidity as against stresses of short duration. At the depth mentioned, or at somewhere between 0·6 and 0·5 of the radius, measured from the surface, a very marked and remarkable change in the nature of the material, of which the earth is composed, takes place. The change is rapid, and is characterised by a small decrease in resistance to compression, accompanied by a great reduction, if not the complete disappearance, of rigidity. It is impossible to determine how this change is brought about, but it is very much what would be produced either by passage from the stony shell to the metallic core, of one hypothesis, or from the fluid or solid-fluid to the gaseous state, of another.

Whatever may be the final interpretation of the distant records of great earthquakes, the important point to be noticed is that the two great changes which they indicate in the constitution of the interior of the earth are, first, at a depth of only a few, probably not more than ten, miles, and, secondly, at about 2000 to 2400 miles from the surface. Between these depths there are suggestions of variations in composition down to a depth of 100 miles or thereabouts, but they seem to be of only minor importance, and apart from this no change in physical character, or, presumably, in chemical composition, can be detected.

R. D. OLDHAM.

8 North Street, Horsham, Sussex, August 15.

Poroscopy: the Scrutiny of Sweat-pores for Identification.

AT the recent meeting of the British Medical Association some attention was directed to a method of criminal identification which has been used at Lyons and elsewhere. A fully illustrated account of it occurs in *Les Archives d'Anthropologie criminelle* for July, from which, after careful perusal, I cannot find that there is anything in the method that does not come under the scope and practical working of dactylography. Dr. Locard has shown good reason why we should give more attention than has been usual to small patches of finger-prints, and to seek among the pores for what the ridges are too meagre to supply. Dr. James Scott, at Brighton, rightly describes "poroscopy" as founded on a study of the "impressions or orifices of the sweat ducts of the finger pulp, instead of the ridges." But pores, the openings of sweat ducts, as printed impressions, cannot be studied quite

apart from the ridges, or ridge substance, any more than the holes of which Pat's classic stockings consisted can be considered without reference to the slender remains of the fabric in which they occur.

Dr. Edmond Locard, the writer of the article, "La Poroscopie," alluding to certain landmarks in finger-print patterns (*puntos caracteristicos* they have been called), adduces in one of his illustrations some *ilots* of a single dot, each containing the opening of a single sweat-pore. The effect when printed is that of a more or less regularly shaped O or ring. Of course, if the smoked-glass method were used, what I have called the negative effect would be produced, and the pore would show up as a black dot on a white ground. In such a case as that illustrated the value of such a coincidence would be seen at once, but the value belongs much more to the system of ridges than to that of the pores. A dozen such pores might easily be found to coincide in two patterns having no real relation to each other by way of personality. I should not expect, however, after considerable experience of finger-print patterns, to find three volcanic or coral islets such as are depicted in Fig. 3 coinciding in any but two prints from the same person. But these volcanic islands are not mere pore openings. It is the sharp definition of the ridge element in them that gives character for identification.

It is difficult to conceive of many cases in actual practice where simple coincidence of pores could be made convincing to a jury. Such cases are presented with a magnification of forty-five diameters. But Dr. Locard says:—"Un jury, que trente ou quarante points caractéristiques homologues auront laissé indifférent, sera frappé par la concordance de forme, de position, et de nombre de quelques centaines de pores trouvés identiques sur les deux empreintes comparées." If the illustrations are from the exhibits in the criminal cases quoted, as one would be led to infer, the jury would seem to have been aided very greatly with outlines filled in by official pens, by which the rough places have been made smooth, and coincidences which would not strike any but police officials seeking a conviction, have been made vivid, if not always quite convincing. All this is, I trust, now quite foreign to English criminal procedure.

It is in cases where fewer than some twelve of what an English detective would in the witness-box call "characteristics" are to be found that the additional scrutiny of the pores might be useful. I agree with Dr. Locard that they remain locally fixed in position, but I have mentioned in "Dactylography" (Twentieth Century Science Series), which Dr. Locard does not seem to have read, that, their physiological activity being very variable, their shapes are constantly altering. They may be nearly closed one moment, and quite patent the next, a useful fact which makes it hopeless to forge finger-print signatures effectively with rubber stamps. This variability is most vividly shown in the illustrations to the very article now referred to, and where a finger-print pattern is doubled the pores always agree in position but rarely in shape or size. HENRY FAULDS.

36 Lichfield Street, Hanley, Stoke-on-Trent, August 13.

Calanus—a Further Record.

ON getting back to Tobermory on Saturday, we found the plankton to be in marked contrast to its condition four weeks ago (see NATURE, p. 504). The vast swarm of Calanids has gone, and there are now no signs of mackerel feeding in the bay. In fact, the change has been noticeable for some days in the seas outside, and we have not been getting lately the large plankton catches that were usual in the latter half of July. On July 14 a haul of the large surface tow-net, in the open sea off Ardnamurchan, gave such a huge catch of Calanus (about 1000 c.c.) that we promptly took a second similar haul, and had it cooked as a sort of potted "shrimp" confection for tea (sampled by ten persons, including the crew, who were much interested to try this new edible "fish"); while on August 11 a haul of the same net, taken at the same spot, gave only a small catch of some 15 c.c., containing very few Calanids, along with the usual scanty summer zoo-plankton. I have not yet seen any statistics of the mackerel fishery, but should not be surprised if this proves to be an exceptionally good year in this neighbourhood, especially in July.

I have only just received NATURE for the last few weeks, and am glad to read Mr. G. E. Bullen's further remarks (p. 531) upon swarms of Calanids and the fisheries. His excellent work—along with that of Dr. Allen—on the connection between mackerel and Calanus and sunshine in the English Channel, some years ago, is valued as the type of observational and statistical work that is required for the investigation of many fishery problems. W. A. HERDMAN.

S.Y. *Runa*, off Island of Eigg, August 12.

The Structure of X-Radiation.

IN a letter which appeared in NATURE of June 19 we described some effects obtained with various metals used as obstacles to X-radiation, which showed that the bands and haloes produced on X-ray plates up to distances of 450 cm. were neither dependent on crystalline structure (other than metallic) nor should probably be termed "diffraction" effects in the strict sense.

Further investigation, in which crystals have been entirely discarded, has led us to believe that some part of these effects at least must be referred to the structure of the primary beam.

In one of our early trials arranged to study the disposition of the spots from a thin lamina of mica, normal to the beam and covering a quarter of an inch aperture, we were surprised to find that spots were not present, but instead the photo plate, exposed at a distance of 50 cm., was entirely covered by dark parallel bands about half a centimetre in breadth and normal to a sharply defined bright cross. In later experiments without mica, and using cast-iron and other screens, both with and without apertures, a system of crossed similar bands has appeared, and a great number of trials have confirmed the result that particular metals (brass, lead, cast- and wrought-iron, &c., of various thicknesses up to 1 cm.) cannot be held responsible for these phenomena, which have even appeared when no other obstacle than a thick sheet of plate-glass has been interposed. A platinised-nickel antikathode has generally been used and 0·5 to 1 milliampere was kept constant through the bulb.

At it appears from Mr. Keene's letter in NATURE of August 14 he has used only thin sheets of metal *with apertures*, we can understand that he has obtained somewhat dissimilar effects from ours obtained without any aperture in the screen. Also with super-cooled glass plates there can scarcely be any question of crystalline structure. We have substituted special rapid plates of various makers and changed all the non-essential conditions many times. When photo plates are placed one behind the other at distances of 15 cm. (up to 100 cm.) from the source, these bands have invariably shown most clearly on the plates farthest from it. This certainly would seem to suggest masking, by secondary radiation, of an effect which properly belongs to the primary or "hardest" portion of the rays. W. F. D. CHAMBERS,
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