

too small for a bacterial growth. It was not at all obvious how bacteria could have remained in one set of tubes and not in the other, unless the radium salt itself acted as a shield, so to speak, for any spores which may originally have become mixed with the salt, perhaps during its manufacture, and when embedded in it could resist even the severe process of sterilisation to which it was submitted.

On heating the culture and re-sterilising the medium, the bacterial-like forms completely disappeared; but only temporarily, for after some days they were again visible when examined in a microscopic slide. Nay, more, they disappeared in the slides when these were exposed to diffused daylight for some hours, but re-appeared again after a few days when kept in the dark. Thus it seems quite conclusive that whatever they may be, their presence is at any rate due to the spontaneous action of the radium salt upon the culture medium, and not alone to the influence of anything which previously existed therein.

When washed they are found to be soluble in warm water, and however much they may resemble microbes,

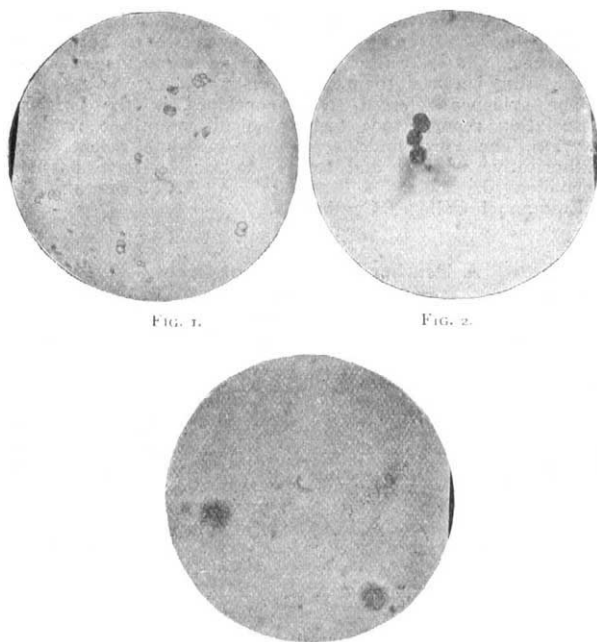


FIG. 1.

FIG. 2.

they cannot for this reason be identified with them, as also for the fact that they do not give subcultures as bacteria should.

Prof. Sims Woodhead has very kindly opened some of the test-tubes and examined them from the bacteriological point of view. His observations fully confirm my own. He assures me that they are not bacteria, and suggests that they might possibly be crystals. They are, at any rate, not contaminations.

I have tried to identify them with many crystalline bodies, and the nearest approximation to this form appears to be that of the crystals of calcium carbonate, but these are many times larger, and, in fact, of a different order of magnitude altogether, being visible under comparatively low powers; and are, moreover, insoluble in water.

A careful and prolonged examination of their structure, behaviour, and development leaves little doubt in my mind that they are highly organised bodies, although not bacteria.

Unfortunately the quantity is so very minute that a chemical analysis of their composition is extremely difficult. The amount of salt in the first instance is so small, and the number of aggregates, or whatever they may be, thus produced perhaps still smaller.

The most effective method of studying their properties,

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from the physicist's point of view, is that of long and, so far as possible, continual observation, a method similar to that which the astronomer is bound to adopt in his study of bodies over which he has not the control to deal with as he pleases.

From the accompanying photographs it will be observed that they are not all of the same size; they range from about 0.3μ to the minutest specks; they are mostly, if not altogether, all of the same shape, and show distinct signs of growth; the larger ones appear to have sprung from smaller forms, and these in turn from still smaller ones, and they have all probably arisen in some way from the invisible particles of radium.

Fig. 2 distinctly shows the existence of nuclei in the larger and more highly developed forms, whilst Fig. 3 reveals, though indistinctly, what is their most remarkable property of all, and that is their subdivision when a certain size is reached. They do not grow beyond this size, but subdivide.

These photographs, together with the numerous results of eye observations, which indicate that a continuous growth and development take place, followed by segregation, leave little doubt that whilst on the one hand they cannot be said to be bacteria, they cannot be regarded as crystals either in the sense of being merely aggregates of symmetrically arranged groups of molecules, which crystals are supposed to be. The stoppage of growth at a particular stage of development is a clear indication of a continuous adjustment of internal to external relations, and thus suggests vitality.

They are clearly something more than mere aggregates in so far as they are not merely capable of growth, but also of subdivision, possibly of reproduction, and certainly of decay.

The subcultures do show, however slightly, some indication of growth after four or five weeks, although that growth is, I understand, too small for a bacterial subculture. Moreover, when examined in the polariscope they have not been found to yield the characteristic figures and changes of colour which crystals generally give.

Thus for these reasons I have been led to regard them as colloidal rather than as crystalline bodies, and probably more of the nature of "dynamical aggregates" than of "static aggregates," of which crystals are composed.

There appears to be a tendency amongst text-book writers to classify minute bodies which are not bacteria as crystals, but really without sufficient reason, and as these bodies cannot be identified with microbes, on the one hand, nor with crystals on the other, I have ventured, for convenience, in order to distinguish them from either of these, to give them a new name, *Radiobes*, which might, on the whole, be more appropriate as indicating their resemblance to microbes, as well as their distinct nature and origin.

Some slightly radio-active bodies appear also to produce these effects after many weeks.

A more detailed account of these experiments will be published shortly. This note merely contains some of the principal points so far observed.

I have to thank Mr. W. Mitchell, who sterilised the tubes, for the assistance he has rendered in these experiments.

JOHN BUTLER BURKE.

Cavendish Laboratory, Cambridge, May 10.

The Consolidation of the Earth.

THERE are several points in Dr. See's last letter (*NATURE*, May 11) calling for remark from the geological point of view.

(1) The effect of (hydrostatic) pressure at depths tends not to liquefaction (as in the case of the ice of a glacier) but to promote crystallisation, the condition of the greatest density of mineral matter, as I showed years ago in my little work on metamorphism in discussing the relation of the crystalline to the vitreous states. It is here that the importance of "solid-liquid critical state" comes in.

(2) We have no right to assume the existence at any stage of the history of our planet of a mere molten ball radiating heat directly into cold space, since in that "pre-oceanic stage" it was surrounded by a non-conduct-

ing mantle or "jacket" of such enormous density and altitude as to contain (as its main constituents) (a) the greater part of the water of the present hydrosphere in the vapour state; (b) the CO_2 locked up in the limestones and other carbonates of the lithosphere, as well as that represented by the coal and the living vegetation of the globe; (c) the hydrocarbons possibly represented by Archæan graphite, together with (d) the halogens (if atomic evolution had reached that stage), including the Cl_2 of the 73 per cent. of the NaCl of the salts of the present ocean. It is conceivable that a vast convection system existed, as the outer zones of the primordial atmosphere underwent cooling with consequent condensation, and descended towards the molten globe; but there could scarcely be contact generally between such cooler portions and the heated molten mass. The conditions would be rather such as are partly illustrated by what a student of physics is familiar with as the "spheroidal state" of a liquid floating on a cushion of steam above a hot plate of metal. Under the enormous pressure prevailing at the surface of the globe in that pre-oceanic stage of its history great quantities of superheated steam and other gases must have been mechanically included, and in some cases, perhaps, occluded, in the hot crust in the inceptive stages of its development by congelation; and in such circumstances, as I suggested seventeen years ago, superheated water in traces would probably enter into the composition of such silicates as *hornblende* and *mica*, the two most characteristic of the minerals of the heavier metals of the Archæan gneisses and schists. A year or two later that hypothesis received demonstration from the splendid work of de Kroustchoff (see *NATURE*, vol. xliii. p. 545, also *Bulletin de l'Académie des Sciences de St. Petersburg*, tome xiii., "Über künstliche Hornblende," by K. von Chrustschoff). So, I take it, we can understand how such a crust could float on a magma of molten rock material, just as air-charged fragments of pumice or of charcoal float on water, yet sink quickly to the bottom under the exhausted receiver of an air-pump; or as even a coil of platinum foil (sp. gr. 21.5) can be made to float in water inside a good air pump, as it is pontooned by innumerable bubbles of distended atmospheric gases previously condensed upon its surface; or, again, as masses of lava slag of large dimensions are seen to float for a time upon the vast lake of liquid rock material in the crater of Kilauea. With tidal action in the magma greater when the moon was nearer the earth than at present, such a thin crust would easily undergo disruption, while portions of it would float off and be engulfed in the magma. This view, which I propounded some seventeen years ago, had been anticipated partly by Zöllner, and was adopted by the distinguished American geologist, Dr. A. C. Lawson, to explain the phenomena presented by the enormous inclusions of more basic rock masses in the gneiss of the Rainy Lake region, which excited great interest among our leading British geologists at the International Geological Congress in London in 1888, though it seems at the time to have been very imperfectly perceived by most of them. So far the evidence we have goes to support Dr. See's contention that the descent of such masses into the magma would be arrested long before they even approached the centre of the sphere; but one feels great difficulty in following his argument based on "Laplace's law," for reasons given in my former letter (*NATURE*, May 4).

By a slip I wrote, it appears, "impossibility" for possibility in the top line of p. 8 in my last letter.

Bishop's Stortford, May 17.

A. IRVING.

The Spirit-level as a Seismoscope.

A MISCONCEPTION seems to prevail among seismologists as to the behaviour of a spirit-level. A displacement of the bubble is regarded as conclusive evidence of the tilting of the instrument. It should be pointed out, however, that this is far from being the case. For a second cause, equally effective in producing displacement of the bubble, is a horizontal acceleration of the instrument in the direction of the tube. The position of the bubble should be taken as indicating, not the normal statical vertical, but

the dynamical residual vertical obtained by subtracting the acceleration of the instrument (as a vector) from that of gravity. (I disregard, in this statement, the slight lag due to viscosity.)

A couple of simple experiments, serving to emphasise this, may be suggested. A spirit-level is suspended in a horizontal position by two equal strings attached one to each end. In one case the strings hang vertically from two hooks; in the other case they are attached both to one hook. If the level is set swinging in the plane of the strings, then in the first case the bubble will be found to have an oscillatory movement relatively to the tube, the tube having linear acceleration but no tilting movement. In the second case the tube has both movements, but their effects exactly neutralise each other, and the bubble remains stationary in the tube. The expert waiter (may it be added?) who hurries about with plates of soup has a very effective empirical knowledge of this last case of compensation.

The motion of the bubble of a level has been brought forward as evidence in favour of the undulatory character of the disturbance producing the motion; but if the above suggestions are to be accepted, the motion might as reasonably be urged as evidence of a horizontal disturbance; the truth being that the instrument is sensitive to both disturbances, and is quite ineffective as a means of discriminating between them.

The evidence referred to is contained in the British Association report, 1902 (seismological committee report, p. 72). The view finds acceptance in some recent and authoritative works,¹ and seems, so far, to have passed unchallenged.

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A Feather-like Form of Frost.

THE accompanying photograph shows a form of frost not, I believe, usually seen except at a comparatively high altitude and unsheltered position. This photograph was



FIG. 1.—Frost "feathers" on windward side of rock.

taken on April 22 near the summit of Carnedd Llewelyn, N. Wales (3484 feet above sea-level). These delicate frost "feathers" appear gradually to grow outwards from the rock face on the windward side, and the delicacy of their form is, no doubt, modified in some degree with the varying rate of the wind and the temperature. I have found, in the same district, these "feathers" 9 inches from root to tip; those shown are about 6 inches long. They form a comparatively solid mass where they touch, but the tips keep distinct, and the whole mass is in reality very brittle, and easily breaks up into small pieces.

H. M. WARNER.

44 Highbury Park, N., May 16.

¹ Dutton, "Earthquakes in the Light of the New Seismology," p. 137; Davison, "A Study of Recent Earthquakes," p. 280.