

correctly forecasting the monsoon rains either as to time or intensity is still far from complete solution. They are so elusive that the forecasting of rainfall even for three days in this part of the world is not yet conspicuous for its infallibility.

The importance of the laws of rainfall is so incalculably great that it is not extravagant to say that their discovery is the ultimate object of, and excuse for, the millions of meteorological figures that are published annually in all civilised nations. Hence the discussion of rainfall in all its aspects is worthy of encouragement, and in this connection the centre of gravity of annual rainfall may possibly, after all, be of some value.

J. Cook.  
Edinburgh, April 18.

I AM glad to read Mr. Cook's reply to my remarks, but believe that my criticism cannot be dismissed as a mere *a priori* one, and that it goes to the root of the matter. It is true that Mr. Cook illustrated his proposal in a most exhaustive manner, and that he did not suggest that his method might be of service in comparing the rainfalls of places in quite different climatic regions. But the general reasoning in the first paragraph of my former letter cannot be both correct and incorrect. Assuming it to be correct, it follows directly that even if we confine our attention to the records for a single station we might have the same C.G. for two years which differed greatly from one another as regards the monthly distribution of rainfall. In such a case, what possible significance could attach to the position of the C.G.?

I am heartily in sympathy with Mr. Cook's feeling that the discussion of rainfall in all its aspects is worthy of attention, but note that he himself does not maintain that his method is, but only that it may possibly be, of some value. It is certainly at first sight surprising that the calculated C.G. of rainfall for a large number of places for a given year in, say, Scotland should be very nearly the same, for the monthly rainfalls as ordinarily tabulated exhibit a bewildering complexity; but if the monthly values for the various stations are expressed as percentages of the year's total, the resulting picture is usually of a very simple and symmetrical character, which would lead one to anticipate that the C.G.s for the various stations would approximate closely.

ANDREW WATT.

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Edinburgh, April 22.

#### The Fertilising Influence of Sunlight.

THE beneficial effect of heat on soil is recorded by Virgil in the following passage, to which Mr. F. B. Smith has directed my attention:—

"Often too, 'tis good  
To burn the stubbles and with crackling flames  
Consume the empty stalks; whether from thence  
The earth derives a hidden store of strength  
And fattening food, or whether 'tis that fire  
Rakes out the subtlest vice and sweats away  
Excessive damp, or whether by the heat  
New pores are opened and the choked are cleared," &c.  
("Georgics," Bk. i., lines 100 *et seq.*)

It is interesting to learn from Mr. Fletcher (April 7) that the natives of Bombay, in certain circumstances, subject their soils to heat. Mr. Fletcher regards the explanation given by Dr. Hutchinson and myself as incorrect, and suggests that the effect is due to the destruction of some toxin. This was the first hypothesis we examined, but was found to be insufficient.

(1) Toluened soil (*i.e.* soil treated with a small quantity of toluene, which is subsequently allowed to evaporate *in situ* without washing anything from the soil) is more fertile and more favourable to bacterial activity than the original untreated soil.

(2) When an aqueous extract of untreated soil is added to the toluened soil, there is a still further increase in fertility and in bacterial activity. The same result follows when a minute amount of the untreated soil itself is added instead of the aqueous extract.

(3) When a larger quantity (5 per cent.) of the untreated soil is added a similar effect is produced for a time, then the bacterial activity begins to be depressed. This action increases, and finally the depression, both in bacterial

activity and in fertility, is out of all proportion to the 5 per cent. of soil originally added.

Experiment (2) is conclusive against the hypothesis that a soluble toxin exists in the untreated soil which can be put out of action by toluene. For such a toxin should cause a decrease, and not an increase, in productiveness. Experiment (3) is equally conclusive against a relatively insoluble toxin; had this been present the depression should have shown itself at once, and should have been proportional to the amount of toxin, *i.e.* of untreated soil, added.

The *growth* of the injurious factor in experiment (3) seems to necessitate a biological hypothesis. Considering these and our other experiments in detail, Dr. Hutchinson and I see no way out of the conclusion that organisms are present in soil inhibiting the development of bacteria, and therefore of plant food. The organisms, whatever they are, must be larger than bacteria, or they would occur in the extract of experiment (2) along with the numerous bacteria there present—indeed, the beneficial effect of this extract was traced to the unweakened races of bacteria present, partial sterilisation having somewhat weakened the soil bacteria. Further, they develop more slowly than bacteria. As similar phenomena have been observed in all the soils examined, we are justified in supposing that the organisms are widely distributed, and constitute an important factor in soil fertility.

Mr. Fletcher's water-culture experiment is not germane to the point. A toxic body that occurred there would not necessarily come direct from the plant or be found in the soil. It is extraordinarily difficult to keep prolonged water cultures sterile, and until some attention is paid to the bacterial changes going on it is impossible to regard the results as proof of the presence of toxins in soils. Indeed, I know of no satisfactory evidence of their existence in normal soils.

E. J. RUSSELL.

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#### Pneumatolysis.

It is thirty-nine years this month since NATURE, over diffident initials, published my first scientific communication that ever saw print. For more than thirty of those years I have been much interested in the physics of plutonic rocks. Quite recently an event has occurred which must be almost without precedent in science. The petrologists have apparently repudiated, with unanimity, what is an axiom beyond dispute with chemists.

For some years past the petrology of plutonic rocks has been based on the new doctrine of "pneumatolysis," or the solvent powers of gases over solids.

Perhaps the last published important work on chemistry is the English version of Ostwald's "Fundamental Principles of Chemistry," 1909. Referring in that work to a certain diagram, representing the behaviour of one solid and one gas, the author writes:—

"From this point the liquid phase exists in the presence of the gaseous phase to the end of the diagram, *because solid substances do not form solutions with gases*" (pp. 186-7). Italics mine.

I believe that every chemist will assent to the above statement. If a gas is to mix with a solid, as a solution, the solid must first be vapourised; but if this be so the greater part of twentieth-century petrology breaks down, because it is everywhere relying on the truth of pneumatolysis.

From the student's point of view the situation is as paralysing as it is stupefying, and there seems nothing to be done but to put away the microscope. It is no work for students to discuss first principles.

Southwood, Torquay, April 18. ARTHUR R. HUNT.

#### Anomalous Reading of Hygrometer.

MAY not the observation referred to in NATURE of April 7 (p. 165) be a very simple case of latent heat evolution by condensation when the atmosphere is supersaturated with vapour? I think I have seen the wet bulb registering a temperature higher than the dry bulb; but this explanation seemed at the time so obvious that I made no careful verification of the apparent phenomenon.

HUGH RICHARDSON.

Bootham School, York, April 11.