

of the controversy can teach us nothing definitely as to the death-point of Bacteria and their germs, though they are of interest with regard to the question of the degree of heat which suffices to check the productivity of the fluids in question.

We are now told that Mr. Lankester himself, and those with whom he sides, are agreed as to the fact that Bacteria are killed at "a temperature a little below 70° C." Of course I cannot tell to what extent Mr. Lankester is in possession of the views of Prof. Huxley and others, but if what he states is really true, the statement is of a reassuring nature; it looks like progress, and leads me to hope that the only remaining doubt may soon be solved. How long does it take for the "through-heating" of certain "possible" Bacteria germs? This is now the knotty problem which, according to Mr. Ray Lankester, seems alone to require solution before we can positively decide as to the heterogenetic origin of Bacteria. Perhaps I may help him on his way to the solution of this difficulty by calling his attention to certain experiments made in Calcutta by Dr. Timothy Lewis, in reference to the existence of living tape-worm germs in cooked meat ("Report of Sanitary Commissioners with the Government of India, 1871"). Dr. Lewis says:—"The temperature of legs of mutton which had been put into the boiler almost as soon as the water was put into it averaged 140° F. (60° C.) in the interior at the moment the water had reached the boiling point (212° F.), and after boiling for five minutes the temperature had reached 170° F. (76° C.)." Now with these facts in his possession, and with some suggestions from physicists of his acquaintance as to the mode of conduction of heat generally, Mr. Lankester may perhaps soon solve his problem, so far as this is practicable. The problem itself may be stated thus:—If the through-heating of several pounds of protoplasm in the shape of a leg of mutton, when immersed in water, takes place at such a rate as to raise the central portions of the joint to a temperature of 60° C. by the time the water has reached 100° C., and if the exposure of the leg of mutton to this heat for the space of five minutes suffices to raise its central portions from 60° to 76° C., how many seconds, minutes, or hours will it take to heat an infinitesimal part of a grain of protoplasm (all through) to the temperature of 76° C.—that is, to a degree of heat decidedly above the death-point of bacterial protoplasm as given by Mr. Ray Lankester? The Bacterium-germ in question, it must be recollected, cannot be supposed to have undergone any extreme amount of desiccation previous to its immersion in the experimental fluid, since such desiccation would have already destroyed its life, according to Dr. Sanderson.

Whilst Mr. Lankester is seeking the solution of the problem above stated, perhaps he might with advantage also reflect a little more closely upon the possible value or otherwise of some of the negative results to which he is so fond of alluding. It is perhaps scarcely necessary for me to remind Mr. Lankester that the obtaining of such negative results is always easy, and may show nothing more than the relative incapacity of the experimenter for performing careful work according to instructions. Not long ago Mr. Lankester, upon the strength of his own negative results, triumphantly announced that he was about to prove to the world the falsity of my views, and so help to justify the opinion which he at the same time expressed as to my being "the mesmerised victim of delusion," "an abnormal psychological phenomenon," and many other fine things. But unfortunately for Mr. Lankester, just about the same time Dr. Sanderson (whose opinions he so much respects) had an opportunity of satisfying himself that I could demonstrate the experimental results which Mr. Lankester failed to obtain. Dr. Sanderson helped to show, in fact, that my positive results were worth more than the many negative results obtained by other workers.

Finally, I think it necessary to add a few words concerning the views of my colleague, Dr. Sanderson, on the subject of heterogenesis, simply because I find his experiments and supposed views frequently quoted by Mr. Lankester, and others, as evidence of the erroneous nature of my conclusions.

I have been led by my experiments to believe in Heterogenesis and also in Archebiosis, but I regard the recognition of the present occurrence of Heterogenesis as of far more importance than the recognition of Archebiosis. Now the controversy between Needham and Spallanzani, and also that between Pasteur and Pouchet was as to the present occurrence or non-occurrence of heterogenesis. This was what they understood, and what the majority of people at the present day still understand, as "Spontaneous Generation." And as to the reality of this process, Dr. Sanderson has been convinced. He admits that Bacteria may appear in flasks, and other situations, where we are warranted in believing

that no bacterial matter pre-existed—which is exactly equivalent to a belief in "Spontaneous Generation," in the sense implied by Pasteur and others. In support of this statement I have only to make the following quotations from his papers and reported speeches of the last two years. Referring to experiments made in 1871, Dr. Sanderson says: "Bacteria could not be shown to be present either actually or in germ in the healthy liquids or tissues, or in the products of healthy inflammation" (*British Medical Journal*, May 11, 1872, p. 508). This statement was made with reference to man, and also to the lower animals with which he had experimented. In another part of the same communication as it stands revised in the "Transactions of the Pathological Society," for 1872, Dr. Sanderson says: "If a few drops of previously boiled and cooled dilute solution of ammonia are injected underneath the skin of a guinea-pig, a diffuse inflammation is produced, the exudation liquid of which is found, after twenty-four hours, to be charged with Bacteria." Other chemical agents will act in the same way even when every precaution against external contamination has been adopted; and as a drop of this fluid introduced with equal care into the peritoneum of another animal is always capable of exciting the phenomena of pyæmia, Dr. Sanderson has made known the very important fact that this process "can be proved to be capable of originating from inflammations produced by chemical agents under conditions which preclude the possibility of the introduction of any infecting matter from without." Again, in a speech delivered last month before the Clinical Society, and reported verbatim in the *British Medical Journal* for March 24, Dr. Sanderson insists upon the complete establishment of the truth of this latter proposition both for man and the lower animals. He says: "We must admit that the whole process of pyæmia can originate in the organism independently of external influences." But, as he also says: "In every pyæmic inflammation—whether it be a primary or a secondary one—in every form of pyæmic action, you have always the presence of septic products," that is of Bacteria. Now if Bacteria by their germs do not normally exist in the tissues of animals, and if you can determine their presence there at will under conditions which, as Dr. Sanderson says, "preclude the possibility of the introduction of any infecting matter from without," what must be the mode of origin of the Bacteria in such cases, and how can Dr. Sanderson do other than yield his assent to the doctrine of "Spontaneous Generation," or Heterogenesis, so far as the origin of Bacteria is concerned?

University College, April 6 H. CHARLTON BASTIAN

Earthquake in St. Thomas

ON the morning of the 11th instant at 4.30 A.M., a smart shock, accompanied by a rumbling noise, like that of a waggon rolling over rough pavement, travelling, as is usual here, from east to west, woke up the inhabitants of St. Thomas. It was followed within a few seconds by another shock, to the full as abrupt in its character as the first; the movement appeared to be not so much undulatory as vertical.

The concussion produced was felt still more distinctly within the harbour itself, where the jar communicated to the ships resembled, as one of the captains described it, that which might be produced by a heavy bale falling through the hatchways into the hold. Simultaneously the water of the bay, then perfectly still, assumed a turbid appearance, as though clouded by mud and sand; and a little later the surface was agitated by a strong ripple from the south, lasting some time.

On the same morning early the royal mail steamer *Corsica*, commanded by Capt. Herbert, was at anchor discharging cargo off the harbour of Dominique, about 170 miles distant from St. Thomas, S.E. The harbour is on the side of the island, and sheltered from the swell produced by the trade winds; the weather calm. Just about 5 A.M. a succession of heavy rollers broke in; they lasted for half an hour, and rendered all communication with the shore during that space impossible. No shock was felt on board the *Corsica*, but Captain Herbert caused note to be taken of the marine phenomenon, not doubting that it must have been due to an earthquake, as indeed was evidently the case.

The centre of disturbance would appear to have been in this case under the sea at some distance S.E. from St. Thomas, a direction often indicated in such occurrences. On one occasion only, that of the severe shock of November 1867, did the movement seem to have been propagated from due south, its centre

being in the deep soundings between the islands of St. Thomas and Ste. Croix.

During the same day two other slight shocks, one at about 10 A.M. the other at noon, were felt at St. Thomas; they were unaccompanied by noise.

W. G. PALGRAVE

St. Thomas, W. I., March 21

Physical Axioms

CONVINCED that the fulfilment of astronomic predictions can never demonstrate the laws of motion, and yet feeling myself quite destitute of intuitive belief in those laws, I have been led to think that in the present controversy truth may lie somewhere between the positions respectively enunciated by Mr. Spencer and his critic.

By reasoning which seems to me equally lucid, ingenious, and unanswerable, Mr. Spencer has shown that certain ultimate mechanical laws are tacitly assumed in every process of experimental verification. But I do not see that this vitiates completely the inference drawn from such verifications. The pure empiricists argue that because certain observed results coincide with the results of calculation, therefore the assumptions on which the calculation was based must be true. Now without doubt the demonstrative character of this inference vanishes entirely under Mr. Spencer's searching criticism. But it seems to me that a *high probability* remains behind. For were there any but an excessively minute error in the laws of motion, our astronomical observations could agree with the results of calculation only by a conflict of errors—a conflict which Mr. Spencer himself hints at. But there are overwhelming chances that these errors would not be so accurately adjusted throughout an immense variety of cases as exactly to compensate one another in every single instance. Hence I cannot but regard the laws of motion as hypotheses, the truth of which is shown by experiment to be overwhelmingly probable. The doctrine here assumed may be illustrated by an appeal to those old friends of probability students—the dice. If I throw double sixes ten times running I naturally conclude that the dice are loaded. This supposition almost necessarily involves the sameness of the ten throws, whereas the supposition that they were not loaded is consistent with an immense number of other results. Our minds choose the former alternative in obedience to an instinct which might with much show of propriety be formulated into an axiom. We may, however, deduce a justification for it from two ultimate intuitions of our nature—belief in uniformity of sequence and the general doctrine of chances—intuitions by which the mind apprehends respectively the ultimate law of knowledge and the ultimate law of ignorance. Belief in any special fact beyond individual experience can be rationally arrived at only by applying the former law to that knowledge which our individual experience furnishes, and the latter law to that ignorance which our individual experience has failed to enlighten.

It is the *approximate* truth of the laws of motion to which I have throughout referred. That there may be an *excessively minute* error in all physical and even all geometrical principles, Prof. Clifford has long ago shown how unphilosophical it is to deny.

F. W. FRANKLAND

Royal College of Chemistry, April 18

The Fertilisation of Fumariaceæ

Apropos of the interesting discussion on this subject which has appeared in your columns, I should much like to know whether any of your readers have observed the mode of fertilisation in *Corydalis claviculata*. Last summer I spent a considerable time in attempting to find this out, but without success. In every flower which I gathered in the mature state, I found the style broken off at the articulation immediately above the ovary, as if to prevent the possibility of fertilisation after a certain period. As the interior parts are completely concealed by the corolla, it was difficult to determine whether the separation had actually taken place on the flower, or was the result of the dissection, but I believe the former to be the case. In a large number of observations, extending over a considerable time, I never saw an insect visit the plant (this was in Westmoreland), though seeds were freely produced. Müller does not mention this species in his classical work on the subject, "Die Befruchtung der Blumen durch Insekten."

ALFRED W. BENNETT

ALLOW me to bring before the notice of readers of NATURE a small point bearing on the fact of the bright hue presented, after fertilisation, by the flowers of *Fumaria capreolata*.

Is it not possible that the pale colour may be more attractive

to the fertilising insects than a brighter one would be? May not the drawing-principle be the result of correlation between the art-manifestations of the attracting and the æsthetic susceptibilities of the attracted organism, and not depend solely on gaudiness of the flower? If this be so, we know that these susceptibilities have, at any rate sometimes, a very limited range, as is seen in the bee-orchis, where the similarity of the labellum to the body of a bee is very close, both in colour and in form, and cannot be useless, seeing that a great amount of developmental force is expended in its production. On this view also the rejection of highly-coloured poisonous caterpillars may in part be referred to the non-agreement of their hues with the orthodox colour-notions of birds. On the other hand, if mere gaudiness is aimed at, why should there be such diversity exhibited? why would not one colour answer the purpose in every instance?

The present case is capable of ready explanation on the supposition that it comes under the influence of natural selection; for, as Mr. Spencer has shown, the hue of the flower results from a diminished amount of nutritive material supplied to the coloured parts, so that the least vigorous individuals would have these most highly coloured at the time of fertilisation. But since the pale flowers are preferred by the insects, they would stand a better chance of being fertilised than would the bright ones, so that a process of selection would be set up resulting ultimately in the disappearance of the latter.

If it be established that cross-fertilisation is not the rule with the flowers of this fumitory, of course it is a fact which has nothing whatever to do with the present argument, and the explanation given by Messrs. Darwin and Müller is entirely satisfactory. I cannot but think, however, that special attention will bring to light many cases of cross-fertilised flowers becoming more highly coloured after fertilisation, the phenomenon being explained simply as a decomposition-phase in the life-history of the contents of the cells composing the coloured organs.

S. MOORE

I VENTURE to suggest the following as possibly an explanation of the fact observed by Mr. Traherne Moggridge, that the flowers of *Fumaria pallidiflora* attain their brightest colouring when the time for their fertilisation has past.

In plants with a racemose inflorescence the individual flowers do not open simultaneously, but more or less in succession. The flowers lowest in the raceme open first: by the time they have in *Fumaria pallidiflora* attained their brighter colour, those a little higher up on the rachis are just at the stage for fertilisation, and the former may serve to attract insects to the latter, just as in some plants (e.g. *Poinsettia*) we may presume that the highly-coloured bracts attract insects to the comparatively inconspicuous flowers which they surround. The flowers a little way up the raceme would serve in their turn to attract insects to those above them; and these again to those still higher; the process going on for a considerable time in *Fumaria*, as it is quite common for the pedicels in the lower part of a raceme to be bearing fruit that has attained its full size, while at the top there are flower-buds still unopen.

Quisqualis indica affords another instance of flowers assuming a more intense colour after fertilisation. Its flowers grow in short spikes; on first opening and during fertilisation, are white, very faintly tinged with pink; but subsequently turn a light reddish-orange, and finally a purplish-red.

T. COMBER

Newton-le-Willows, April 7

Power of Memory in Bees

ILLUSTRATIONS drawn from experiments or observations made upon animals lower than ourselves in the scale of life must always possess great interest. That impressions received by us in early life are more permanent than those made in after years, and that the memory of the old is less retentive in the reception of new impressions than is that of children, are circumstances universally acknowledged. On October 29, 1873, I removed a hive of bees in my garden, after it was quite dark, for a distance of 12 yards from the place in which it had stood for several months; and between its original situation and the new one there was a bushy evergreen tree, so that all sight of its former place was obstructed to a person looking from the new situation of the hive.

Notwithstanding this change, the bees, every day, flew to the locality where they formerly lived, and continued flying around the site of what had been their home, until, as night came on, they many of them sank upon the grass exhausted and chilled by the cold. Numbers, however, returned alive to their new position,