beasts will exterminate the kea, but I do not see that this will be any gain, because, in coming in contact with the kea, they cannot avoid finding wounded and dying sheep, which they would attack, and this, with the slaughtering of sheep on the out-stations, would be to them the best possible training for sheep-killing, if any is necessary; and any larger beast or any bird that would attack the kea would have no hesitation in killing sheep also. As to the question of extirpating these beasts where they may become a pest, the following example from Wood's "Natural History" is worth noticing: A number of rats established themselves by a fishpond, devouring the fish and doing much other mischief, so that the owner was much pleased when a colony of weasels came and, having killed or driven away the rats, settled in their place. For a time all went well, but presently, other food failing them, the weasels began to kill rabbits, poultry, etc., so that the owner became as desirous to destroy them as he had been the rats, but he failed to do so, and the weasels remained in triumphant possession.

Mr. T. Bent, M.L.A., of Victoria, is of opinion, as the result of information gathered on a visit to India, that the mungoos will become as great a pest in these colonies as the rabbit. This result would equally attend the naturalization of either ichneumons or weasels, as the former are practically but tropical weasels and probably resemble them in nature and habits much more than I have stated.

The importation of these beasts should therefore be stopped and those already at liberty destroyed, at whatever cost; if this be done without delay, I do not think it is now too late to extirpate them.

For further information on the *Mustelidæ* and *Viverridæ* see Wood's "Natural History" and the "Encyclopædia Britannica," Arts. Ermine, Ferret, Fur, Ichneumon, Mammalia, and those on northern countries.

ART. XIX.—*A short Description of a few Experiments bearing on the Question of Spontaneous Generation.* By DR. J. HUDSON.

[*Read before the Nelson Philosophical Society, 3rd November*, 1884]

THE few remarks contained in this short paper, are written with the object of eliciting discussion.

The question of spontaneous generation is not by any means a new one, nor is it confined to the learned, for I have frequently heard ignorant hospital patients account for the parasites with which they were infested, by saying that they bred them, meaning that they arose spontaneously, and that no amount of care would prevent their development. Practically, the

belief in spontaneous generation is pretty general; when organisms appear in situations where they are not expected, and under ciruumstances which, to a superficial observer, would appear calculated to exclude them, the easiest mode of accounting for their presence is to assume that "they came of themselves"; and, by way of parenthesis, I will say here that, when we wish to account for or explain observed facts, we are always right to take the simplest theory, provided such theory does not clash with any other known facts.

The possibility of organisms having a spontaneous origin has been narrowed and narrowed by successive observers, until it is only the *Bacteria* which at the present day are presumed, under certain conditions, to arise spontaneously. These *Bacteria* are exceedingly minute rods and spheres that invariably appear wherever decomposition of animal or vegetable matter is going on. For instance, suppose we take any organic infusion such as can easily be made by soaking a piece of meat, or hay, or turnip in water for an hour or so; we strain off the clear liquid and set it on one side. If we examine it after a few days (a week or ten days in winter, a couple of days in summer), we shall find that the clear liquid has become turbid, and that it begins to smell offensive,—in common parlance it has turned bad, or decomposed! Why has it decomposed? Modern science tells us that organic matter cannot decompose without the presence and help of *Bacteria* of some kind or other; that a complex organic infusion, provided we rigidly exclude germs, will remain as stable as a solution of sulphate of copper. We have a practical application of the principle in the various tinned meats. The germs in or on the meats or fish are first killed by exposure to a high temperature, and then the tins are hermetically sealed so as to exclude air, or rather germs; for it is not the air that does the harm. How can we prove this?

I have here three glass tubes, which I will call Nos. 1, 2, and 3. Some months ago I put into each of these tubes some chopped hay and water. No. 1 I left untouched in a sheltered place but exposed to air and light; No. 2 I boiled; into No. 3 I inserted a cotton wool cork, quite permeable to air, but which has been found to act as a sieve to *Bacteria* and their germs, and then I boiled it for about five minutes. The steam issued freely from the cotton wool, demonstrating its perfect permeability to vapour and air. After four days I looked at the tubes. No. 1 appeared clear, No. 2 distinctly turbid, No. 3 clear. A drop of No. 2 placed under the microscope showed numerous rod-like *Bacteria*. The earlier appearance of turbidity in No. 2 is easily explained by the boiling having made the infusion quicker (and so prepared the fluid for the reception and growth of germs) than the cold water did. At the end of a week I examined the tubes a second time. Nos.

1 and 2 were very turbid, and both showed under the microscope numerous *Bacteria*, but No. 1 (the unboiled specimen) showed in addition some free swimming Infusorians, derived (they or their spores) in all probability from the hay, but possibly from the water. The boiling of No. 2 had killed these higher organisms. No. 3 was still clear. After several months (it was in July that I put up these specimens, examine them now) Nos. 1 and 2 are positively filthy, they are far advanced in decomposition, and owing to evaporation are drying up. No. 3 has diminished fully half its bulk owing to the same cause; but what remains is a clear fluid, and if we examine a drop of it under the microscope, I feel well assured will not show any living organisms. Now this is very remarkable. What is this exemption from decomposition and the associated development of organisms due to? It is not due to the previous destruction of organisms by boiling, else No. 2 would have escaped. Neither is it due to the exclusion of air, for air has been freely admitted; therefore we can only conclude that the exemption is owing to the cotton wool plug having caught and retained the germs which are ever present in the atmosphere.

I have also a specimen of a small quantity of a highly putrescible animal fluid, which has remained clear and unaltered by means of the same simple precautions.

We may here for a few minutes consider what is the nature of putrefaction; putrefaction expresses the chemical change which organic matter undergoes when exposed to air, dust, etc. If we take a solution of an inorganic salt such as nitrate of potash and set it on one side and examine it after a long interval, we should find it was nitrate of potash still; similarly if we take a number of neutral salts, taking care to select only those that would not chemically react on each other, we should still find that even after a very long time they would still be unaltered; but with an organic fluid the case would be different; a solution of albumen such as the serum of blood would very soon putrefy, that is, it would undergo a chemical change, and as this change is accompanied with an offensive smell, it is called putrefaction. The process is identical with fermentation, in fact only a variety, and fermentation is a chemical change induced in an organic fluid by means of the growth in that fluid of certain definite minute organisms; thus the alcoholic fermentation is caused by the yeast plant (*Torula cerevisiæ*) the butyric fermentation by the growth of *Bacillus subtilis*; this organism grows best where there is but very little oxygen, and I may state here in parenthesis that the well known but at the same time unpleasant symptom of heartburn is caused by this particular fermentation; the lactic fermentation, or the souring of milk, by the *Bacterium lactis*, and the numerous fermentations and decompositions of organic fluids by the different species of *Bacterium*,

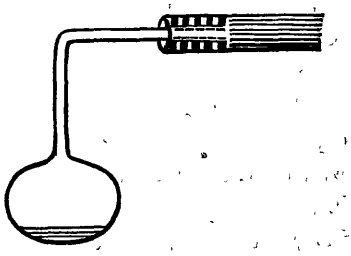
Torula, *Bacillus*, and *Micrococci*. It will be well now to examine briefly some typical experiments of the leading heterogenists—Pouchet, Jeffreys Wyman, and Bastian.

Pouchet in one of his experiments introduces a flask into a vessel containing a decoction of barley which had been kept boiling for six hours; the flask being completely filled with this fluid was brought to the surface and corked, and then the circumference of the cork was surrounded by varnish. On the sixth day a deposit of yeast was seen, and the flask burst on the seventh.

Here an impure vessel, cork etc., were used, and the heat was not applied to them for a sufficient length of time. Further, the fluid cannot have been boiling when the vessel was introduced and corked, otherwise, if he had done it efficiently, the operator would have scalded his fingers. It is, moreover, noteworthy that Pouchet only got organisms in one experiment of this kind.

I may here say that Pasteur has completely upset all of Pouchet's experiments and deductions.

In Jeffreys Wyman's experiments such fluids as sugar gelatine and hay infusion, or flesh sugar and gelatine, were put into a flask, the neck of which was drawn out and bent at right angles, the extremity of the neck was inserted into an iron tube and cemented there with plaster of Paris. The iron tube was filled with wires, leaving only very narrow passage-ways between them. The general relation between the quantity of fluid and the capacity of the flask was about 20 to 500. The flasks were then boiled for periods varying between a quarter and two hours, while at the same time the iron tube containing the wires was heated to redness. On withdrawing the lamp from the flask, the air which entered passed over these heated iron wires. When cold the flasks were sealed with the blowpipe. Fourteen vessels were prepared in this way, and in ten of these, when opened after the lapse of various periods of time, *Vibriones* and *Bacteria* were found. The other four remained barren.



Curiously enough, Pasteur had made numerous experiments almost exactly similar and had obtained no organisms. Why this different result? Pasteur's flasks were half filled with fluid, while Wyman's contained only from $\frac{1}{20}$–$\frac{1}{30}$ part, so that we can easily understand that in Wyman's experiments a portion of the air and of the walls of the flask were never heated to within many degrees of the boiling point. Another source of fallacy is to be found in the fact that the air which passes through the iron tube with its contained wires is only exposed to dry heat, and it is within

the experience of every one that very high degrees of dry heat can be sustained by living bodies with impunity; so that spores entering with the air through the iron tube need not necessarily be destroyed.

Any one who reads Dr. Bastian's book on the "Beginnings of Life" will lay it down feeling convinced of the possibility of spontaneous generation, and yet on close examination it is full of glaringly false facts; for instance, he states: "A closed flask containing a very strong infusion of hay, to which had been added $\frac{1}{20}$th part of carbolic acid, was opened twelve days after it had been hermetically sealed." Bastian states that this flask contained organisms of a peculiar form. Such a statement as this—that a saturated solution of carbolic acid can permit the growth of organisms—is opposed to all experience and experiment. It is usually found that so small a proportion of carbolic acid as the $\frac{1}{250}$th part effectually prevents the growth of organisms in organic infusions freely exposed to the air. The supposed organisms were probably granular deposits produced by the action of the strong acid on the glass or the hay infusion compounds. Many of Bastian's experiments were made with cheese; now, the spores of *Bacillus subtilis*, which abound in cheese, have peculiarly resisting powers, and, being contained in a badly conducting material like cheese, might easily escape the boiling point.

With regard to Bastian's experiments Dr. Roberts says: Dr. Bastian's process does not insure that the entire contents of the flask are effectively exposed to the boiling heat.

Professor Huxley states in "Nature," that he had seen Dr. Bastian's experiments and preparations, and expressed his belief that the organisms which Dr. Bastian got out of his tubes were exactly those which he put into them; that in fact he had used impure materials, and that what he imagined to be the gradual development of life and organization was the simple result of the settling of these solid impurities. For instance, he relates how on one occasion Dr. Bastian showed him a specimen of a fungus which had developed spontaneously, which Huxley recognized as a fragment of the leaf of a *Sphagnum*, and that it was so he ultimately with great difficulty convinced Dr. Bastian.

Drs. Bendon Sanderson, Ray Lankester, and Mr. Hartley have also tested Dr. Bastian's experiments, and found them wanting.

In concluding this short and imperfect paper, I would here remark that the old doctrine "*Omne vivum ab ovo*," appears to me to be the only one scientifically tenable.

P.S.—On tube No. 3 being opened, no organisms were found, and the odour of the solution resembled fresh hay, so forming a marked contrast to the other tubes, which had a very stale musty smell.