Bibliographical Record.

Bastian on Lowest Organisms.¹—Dr. Bastian is certainly an adroit controversialist, as well as an inventor. We remember, last year, a violent polemical discussion which was carried on between him and Professor Huxley. In the inception of the dispute the facts were all on the side of the Jermyn Street professor; but, owing to the skill of his antagonist and the peculiar style of argument into which Professor Huxley was often led, Dr. Bastian carried his lance fairly broken out of the arena.

The experiments of Pasteur and Pouchet have so long occupied the attention that it is unnecessary to quote them here. Dr. Bastian professes to have repeated with success the experiments of Pouchet, and to have entirely demolished the counter-experiments of Pasteur, Tyndall, and Huxley. We must glance at Dr. Bastian’s experiments before we enter upon the consideration of his arguments. He gives us sixty-five experimental instances, in which he alleges that he has proved what he terms the theory of Archebiosis. Of those, the following, perhaps, best illustrate his method of proceeding:

“Fluid (in vacuo) in a flask, the neck of which was hermetically sealed by means of a blow-pipe flame during ebullition.

“No. XIII.—Urine in forty-four hours showed a very slight amount of sediment. During the next two days the sediment very slightly increased, but was still small in amount. At the expiration of fifteen days, no further increase in the turbidity having taken place, the fluid was examined. The vacuum was still partially preserved, as evidenced by the rapid inbending of a portion of the neck of the flask after it had been carefully made red hot. When opened, the odour of the fluid was stale, but not foetid, and its reaction was still faintly acid. On microscopical examination bacteria and toruke were found in tolerable abundance.

“No. XXIV.—Fluid in a bent-neck flask, having eight acute flexures. Urine, in forty-eight hours, showed no change. After twelve days there was still no general turbidity, though there was a slight floculent deposit of an uncertain nature. Two days afterwards the flask was broken, when the odour of the fluid was still found to

¹ The Modes of Origin of Lowest Organisms, including a Discussion of the Experiments of M. Pasteur, and a Reply to some Statements by Professors Huxley and Tyndall. By H. CHARLTON BASTIAN, M.A., M.D., F.R.S. London and New York. 1871.
resemble that of fresh urine, and its reaction was acid. The flocculi were made up of granular aggregations, in the midst of which were a few bodies closely resembling torula, though they were somewhat doubtful in nature; neither bacteria nor vibriones could be found. The flask, having a short open neck, was then replaced in the warm bath. In sixteen hours the whole fluid had become turbid, it was also slightly foetid, and on microscopical examination it was found to be swarming with bacteria, vibriones, and leptothrix."

Against his conclusions may be cited the testimony of such an experienced observer as Dr. Sanderson, on whose researches on microzymes we commented in a past number. In a recent memoir in the 'Quarterly Journal of Microscopical Science' Dr. Sanderson gives a series of experiments which appear to contradict those of Dr. Bastian in nearly every essential respect. Dr. Sanderson shows, firstly, that neither bacteria nor fungi ever develope in solutions raised to the boiling-point, and placed in carefully cleansed and boiled vessels, which are subsequently closed; secondly, that if such solutions in such flasks be exposed to atmospheric air, no bacteria ever develope, but yeast-cells, and ultimately blue mould, do develope (whence it is inferred that the germs of fungi, but not of bacteria, are carried in the air); thirdly, that if unboiled water be used, or glass or other surface not duly cleansed be brought in contact with the above-mentioned solutions, bacteria always develope in great quantity (whence it is inferred that water and surfaces which have been or are more or less damp are the means of dissemination of bacteria). It is impossible to reconcile these statements with those of Dr. Bastian, and when two such skilful observers arrive at such diametrically opposite results, we, at least, must be pardoned for deferring our judgment.

Such, however, are the facts.

"Varius Sueronensis ait; ÕEmilius Scaurus negat, utri creditis quirites?"

The arguments and conclusions of Dr. Bastian rest upon an entirely distinct basis from his facts. He sums up, we think, the whole case in the following words:

"If fluids in vacuo (in hermetically sealed flasks) which were clear at first, have gradually become turbid, and if on microscopical examination this turbidity is found to be almost wholly due to the presence of bacteria or other organisms, then it would be sheer trifling gravely to discuss whether the organisms were living or dead, on the strength of the mere activity or languor of the movements which they may be seen to display. Can dead organizations multiply in a closed flask to such an extent as to make an originally clear fluid become quite turbid in the course of two or three days?"

"In these experiments with heated fluids in closed flasks nothing is easier than to obtain negative results. The same kinds of infusion which, if care has been taken to obtain them strong enough, will in
a few days teem with living organisms, often show no trace of living things after much longer periods; when the solutions are weak, again, those cases where only a few organisms exist in a solution which has been made the subject of experimentation, nothing is easier than, by a perfunctory examination of the fluid, to fail finding any of these sparsely distributed living organisms. Experiments, the results of which are positive, may, therefore, in the absence of sufficient care, be cited as negative; and experiments which would otherwise have been crowned with unmistakably positive results may be rendered wholly barren by the employment of infusions which have been carelessly made."

The nomenclature adopted by Dr. Bastian is very peculiar. The hideously ugly word "archebiosis" is coined to express an idea, which, when it is examined, is closely allied to that of heterogenesis. Dr. Bastian would probably not admit this fact.

The possible modes of origin of bacteria and torulae may therefore be tabulated as follows:

Modes of origin of bacteria and torulae

1. Homogenesis
   - a. Direct
   - b. Indirect
2. Heterogenesis
3. Archebiosis

We confess that we fail to see the logical distinction between these methods of origin. If the presence of organic matter is once admitted as a factor, it matters very little whether the organic matter in its individualised state is living or dead. Whether "particles of living matter" or "certain fluids containing organic matter" are the ambient medium in which organized beings are produced, is a mere question of words on which a wordy war might continue for years. The allegation by Dr. Sanderson that in solutions which have been raised to the boiling-point, and placed in carefully cleansed vessels, bacteria and fungi are not developed, is entirely destructive of Dr. Bastian’s forty-fourth experiment. If neither bacteria nor fungi develop under the conditions which Dr. Bastian asserts to be favorable to their birth and existence, the whole controversy is reduced to a dispute between Drs. Bastian and Sanderson on mere facts. Still, it must be remembered that Dr. Sanderson’s conclusions are merely negative. It is in the nature of things that they should so be, but Dr. Bastian’s conclusions are nevertheless overthrown unless some observer of equal scientific weight with Dr. Sanderson arises, who, with the same apparatus as Dr. Bastian employed, will produce results identical with those of the ingenious University College professor. This is really the only satisfactory solution of the difficulty, and until it is carried into execution we hope that Dr. Bastian will postpone the publication of his great work on the physical doctrine of life. It is true that, because his testimony is at variance with that of other observers, it need not
necessarily be wrong. There have been many instances, even in anatomy and physiology, of the opinion of one solitary observer being opposed to the unanimous voice of his contemporaries, the one man having been afterwards triumphantly proved to have been correct. Yet it is difficult for dispassionate observers when, as in a jury, investigating questions of absolute fact, not to lean towards the feeling of the majority of witnesses. If the presence of bacteria is merely due to the existence of water or damp substances—if the water is carefully boiled, and other precautions familiar to the readers of M. Pasteur's work are taken—and if, when these precautions are rigorously and formally carried out, no bacteria whatever arise, the verdict of "not proven" must be certainly returned against Dr. Bastian's conclusions.

Then follows the inquiry, what amount of antecedent probability exists in their favour? Were it not that we are investigating a strictly scientific subject, on which the mere facts have to be examined and taken at their value, we would be inclined to think that the probabilities in favour of the origin of living beings, as Professor Owen has pointed out, by a sort of heterogenesis, has much to be said in its favour. We regard the alleged refutation of the probability of heterogenesis, made by Professor Huxley at the Liverpool meeting, as entirely unsatisfactory, based, as it was, upon not a single cited original experiment.

The advocates of spontaneous generation have a right to demand a demonstration of the impossibility of their statements, instead of a mere allegation of their improbability. Dr. Bastian has certainly carried out a long series of experiments, and propounded certain distinct hypotheses, and the character both of his experiments and of his views is such as to call for a serious re-examination and discussion.

Nosology of Zanzibar—The recent work of the distinguished African traveller Captain Burton contains so many important facts which may be of value to the traveller on the coasts of Eastern Africa, that we have no hesitation in calling our readers' attention to the medical facts which we find recorded therein. The climate of Zanzibar, better than that of the hot damp eastern coast, has nevertheless many unfavorable points, which seem to preclude its ever proving to be a convenient station off which Her Majesty's ships could long cruise. Though on the island many of the white residents have escaped severe fever, the disastrous fate of Captain Owen's surveyors, the loss of life on board our cruisers, and the many deaths amongst the Mombas missionaries, even though, finding the seacoast dangerous, they built houses on the mountain slopes, prove

1 Zanzibar; its City, Island, and Coast. By Captain R. F. Burton. Svo. London. 1872.