

like Dr. Huggins, possessing such an instrument as he did, should fail to have seen the bright lines at first, nor why, believing as he did in the aid afforded by the Indian observations to an observer searching for these lines, he should yet have left it to Mr. Lockyer to make this discovery.

BALFOUR STEWART

Dr. Bastian's Experiments on the Beginning of Life

IN the issue of NATURE for January 9, Dr. Burdon Sanderson has recorded some experiments on the behaviour of certain organic mixtures boiled for five or ten minutes in flasks which were hermetically sealed during ebullition. He found, as Dr. Bastian had done, that Bacteria appeared in these sealed vessels—not always, but frequently. Dr. Sanderson is, however, careful not to endorse the conclusions which Dr. Bastian has drawn from these experiments. This method of experimenting appears to me to involve two serious sources of error which invalidate them in so far as they are used to support the theory of spontaneous generation.

The first source of error is the possibility of the introduction of atmospheric germs at the moment of sealing.

Those who are practised in the sealing of flasks during ebullition are aware that the sealing can only take place just as ebullition is about to cease; otherwise the vessel bursts, or the imprisoned steam opens a path for itself through the softened glass where the flame is being applied. There is thus at the moment of sealing, a risk of some reflux of air into the flask and a consequent vitiating of the experiment. Perhaps it may be thought that because air thus introduced has to pass through the flame applied to the tube through the red-hot tube itself, and enters a flask whose contents are not far below the boiling temperature, any germs contained in it must be destroyed; but that is a very hazardous assumption. Momentary contact (or approximate contact) with a flame or a heated surface is by no means so destructive as at first appears, and experience has taught me to suspect that the contents of a boiling flask are not speedily deprived of vitality. This source of error is probably not a frequent occurrence in Dr. Bastian's experiments, but I am inclined to think that it was a frequent one in Dr. Wyman's experiments, and that it seriously vitiates all those experiments in which air passed through a heated tube was used. At any rate the exactness of experiments so conducted is very much at the mercy of the care and dexterity of the operator, and hence, probably, their contradictory results in different hands.

In repeating Dr. Bastian's experiments I have avoided this source of error by inserting a tight plug of cotton wool in the neck of the flask before beginning to boil. In this way any chance germs introduced by an accidental reflux of air during sealing are prevented from passing into the flask.

The second source of error is much more important. It is this:—Dr. Bastian's process does not insure that the entire contents of the flask are effectively exposed to the boiling heat. Herein lies, I believe, the chief cause of the inconstant and contradictory results obtained by him. It is beyond doubt that Dr. Bastian is perfectly correct in his statement that the experiments made by Pasteur with "Pasteur's solution," and by Lister with urine, yield different results when made with other solutions and mixtures. The contrast is most striking. In my own experiments I have found that filtered infusions of any animal or vegetable substances (and I have tried a very great variety) can be invariably preserved unchanged when boiled for five or ten minutes in a flask plugged with cotton wool; but if milk be treated in the same way, or if a few fragments of a green vegetable be added to the infusion, or if alkaline albuminous solutions or mixtures, containing cheese, be treated in the same way, they almost invariably breed Bacteria in abundance. What is the cause of this difference? For some time it appeared to me difficult to account for, but I came to the conclusion at length that it was simply due to the fact, that with the more complex organic mixtures every particle of the material within the flask does not really attain the boiling heat. These more complex mixtures generally froth excessively in boiling, and spurt about particles which adhere to the glass, and probably some of these escape the full effect of the heat. What first led me to this conclusion was the behaviour of milk. Milk boiled for ten or even twenty or thirty minutes, in a plugged flask, almost invariably curdled and produced Bacteria in a few days; but when the milk was put into a long-necked flask, plugged with cotton wool, and hermetically sealed, and the flask boiled in a good-sized can of

water for twenty or thirty minutes, then the milk remained permanently unchanged, and produced no Bacteria. I possess specimens of milk treated in this way which have remained unchanged for many months, though exposed to warmth, to light, and free access of air, that is to say, to air filtered through a good plug of cotton wool. I obtained similar results with the other organic mixtures which could not be kept unchanged by simple boiling over the flame. Highly putrescent mixtures, containing blood-serum, egg-albumen, fragments of meat and vegetables, remained perfectly barren after the flask containing them had been immersed in a water-bath kept at a boiling heat for twenty or thirty minutes.

The essential conditions of the experiment are, first, the effective exposure of the whole contents of the flask to a boiling heat; secondly, the absolute prevention of any fresh entrance of extraneous solid or liquid particles; and the conclusion I have come to is that if these conditions are rigidly observed, the flasks remain barren; if they do not remain barren it is simply because one or other of these conditions has not been observed.

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The unreasonable

I UNRESERVEDLY accept Prof. Clifford's disavowal of the meaning I attributed to his words concerning Kant's Antinomies, in his Address (*Macmillan's Magazine*, Oct. 1872). At the same time I cannot allow that the misprision was wholly due to my "exuberant imagination." He said, "The opinion . . . is set forth by Kant . . . in the form of his famous doctrine of the antinomies," &c. This ought to mean that the "doctrine of the antinomies" is one form of that "opinion;" and the opinion being, "that at the basis of the natural order there is something which we can know to be unreasonable," I was fully justified by the mere words of the Address in the inference (which he disclaims) that he intended to identify the doctrine of the antinomies (the Antithetic, in fact) with that of the unreasonable basis of the natural order. How was I to know that the "something" was either (? which) "the transcendental object" or the world of *noumena*?

I premise, then, that it is the Antithetic which "is set forth by Kant in his famous [but little understood] doctrine of the Antinomies," and not "the opinion that at the basis of the natural order there is something which we can know to be unreasonable." Prof. Clifford, however, meant to signalise the latter; and he asserts, and by sundry extracts from the *K. v. V.* attempts to substantiate the assertion, that "the transcendental object [which lies at the basis of the natural order] is unreasonable, or evades the processes of human thought."

Now Kant, so far from proving (or asserting) that, takes pains to show that it is reasonable, though it persistently seems to be the reverse! According to Kant, the thing *per se* illusorily appears to be the object of experience; and this illusion is inevitable, and no criticism can dispel it. (Kant compares it to the seeming magnitude of the horizontal moon.) But criticism can and does explain it, so that, though it persists as a spectre haunting the reason, it is wholly and strictly amenable to the processes of reason, or in Prof. Clifford's sense, reasonable.

Of Prof. Clifford's quotations, (a) and (b) are irrelevant to his second position; the former does not directly touch "the processes of human thought;" the latter does not touch "the transcendental object!" His third position is equally unsupported by the extract, "Man [not Mann] kann aber," &c., which may be thus rendered:—"But conversely we can also deduce from this antinomy a real, not indeed a dogmatical, but a critical and doctrinal advantage, namely, of indirectly showing the transcendental ideality of phenomena (*Erscheinungen*)." The method is by showing that the antithesis is contrary, as distinguished from contradictory, and by invalidating both the alternatives, whence it follows that the subject of them is not an existing totality. The antinomies are thus used, not as Prof. Clifford vainly imagines, to prove that the transcendental object is unreasonable, but that the postulate of its being a *noumenon*, or thing *per se*, or true basis of the natural order, is untrue, both alternatives being false.

Prof. Clifford is, as I said, really attacking Hamilton. I do not care where he got the doctrine from, nor what he does with it. If it amuses him to set up these absurd nine-pins and then bowl them over, with flourish of trumpets, I have no wish to interfere with him, only he had better mind his H's and K's, and not impute this stuff to Kant. Once for all: in Hamilton's