

hydrogen. Later (June 1867) Mr. Johnstone Stoney showed, in an important paper on the "Constitution of the Sun and Stars," that hydrogen was to be expected. I acknowledge that I ought to have found the lines.

γ "When the position of the lines was known, Huggins saw them instantly with the same spectroscope which he had previously used in vain." Of course in this remark I refer only to my own experience; I do not wish to be thought to imply that such assistance was either needed or received in the case of any other observer. Prof. Stewart asks why I did not see the lines sooner after receiving the news from India. This question awakens painful memories. At the time the Indian reports reached me I was watching by the bed of the dying, and a few days afterwards I suffered so severe a bereavement that I was unable to resume work in the observatory until the beginning of December, when I saw the lines. WILLIAM HUGGINS

The Beginnings of Life

IT seems advisable for me not to pass without comment the communication made by Dr. Wm. Roberts, of Manchester, in last week's NATURE.

Dr. Roberts calls attention to what he considers two possible sources of error in my experiments. The first is the "possibility of the introduction of atmospheric germs at the moment of sealing the vessels," owing, as he says, to the fact "that the sealing can only take place just as ebullition is about to cease," and to the consequent risk of some "reflux of air into the flask." After Dr. Roberts has made a series of experiments similar to those which have been recently cited in these columns (see NATURE, Feb. 6, p. 275 with *Erratum* in Feb. 13, p. 296), he may perhaps be a little less apprehensive as to this source of contamination. It is, however, not the fact that flasks cannot be sealed during ebullition, and this I shall be very happy to demonstrate to Dr. Roberts. Moreover, if he will refer to Dr. Sanderson's letter, Dr. Roberts will find that in speaking of the sealing of the flasks in the blow-pipe flame, he says care was taken "to continue the ebullition to the last." And in several series of experiments M. Pasteur also made use of flasks which had been sealed during ebullition—believing that in so doing he was experimenting with vessels from which all living germs had been excluded. Speaking of the preparation of such flasks, Pasteur says (*Ann. de Chim. et de Physique*, 1862, p. 74): "je ferme l'extrémité effilée pendant l'ébullition. Le vide se trouve fait dans les ballons." No one has hitherto questioned M. Pasteur's skill as an experimenter.

The second alleged source of error is, according to Dr. Roberts, much more important. My mode of experimentation, he says, "does not insure that the entire contents of the flask are effectively exposed to the boiling heat." Although Dr. Roberts confirms my statement that many fluids treated in the manner I have described do soon swarm with living things, he seems to think their appearance may be due to the fact that several of the mixtures "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." I feel quite sure that in my experiments no portion of the inner surface of the glass has escaped the scathing action of the boiling fluid. The vessel has generally been more than three-fourths full before the process of heating has been commenced, so that when ebullition occurs the fluid has always swept over the previously uncovered inner surface and, as Dr. Sanderson testifies, "during the boiling some of the liquid was frequently ejected from the almost capillary orifice of the retort." The inner surface of the vessel was, in fact, always thoroughly and repeatedly washed with the boiling fluid, nearly half of which has been spurted away in order that I might effect this object.

Dr. Roberts says:—"Dr. Sanderson is, however, careful not to endorse the conclusions which Dr. Bastian has drawn from these experiments." But this is scarcely a fair statement, since Dr. Sanderson had near the beginning of his letter announced his intention of taking no part in the controversy. Dr. Sanderson's opinions, however, on the elements of the question have already been set forth (see Thirteenth Report of Medical Officer of Privy Council, p. 59). Whilst not believing in the danger of atmospheric contamination by Bacteria germs, he does believe, in common with other biologists, that immersion in boiling liquids is a ready means of destroying them. If Dr. Sanderson had not thought that the conditions of the experiments were such as to be exclusive of the intervention of living germs, why should he have previously doubted "my statements of fact" in

respect to them? Does Dr. Roberts consider Dr. Sanderson so much of a tyro in these matters as to suppose that he would doubt the well-known fact that living germs will always rapidly multiply in suitable fluids? If not, then the only other source of doubt that could have arisen, must have been as to the possibility of the appearance of swarms of living things in hermetically sealed vessels in which all pre-existing organisms had been killed. And if Dr. Roberts wishes ample proof that such has been the view also entertained by others I need only refer him to the last few pages of a curious article (purporting to be a review of my work "The Beginnings of Life"), which appeared in a recent number of a journal (*Quart. Journ. of Micros. Science*) of which Mr. Ray Lankester is one of the editors. It is perhaps fortunate for the reviewer's reputation and for his fame as a scientific experimenter that his name does not appear, or that his unsuccessful experiments, destined to upset my views, were not published before the advent of Dr. Sanderson's letter.

In some respects the actual results of the experiments performed by Dr. Roberts differ from those of other experimenters. Thus he has found that filtered infusions of any animal or vegetable substances can be "invariably preserved unchanged when boiled for five or ten minutes in a flask plugged with cotton wool." The results obtained by M. Victor Meunier and by myself have been different, and we have both shown that they are apt to vary according to the strength and nature of the infusions employed. Dr. Roberts says he has also found that many "highly putrescent mixtures" 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," although several of the same mixtures "could not be kept unchanged by simple boiling over the flame," and the sealing of the flask during ebullition. If, after what I have already said concerning the latter mode of experimentation, anything is to be deduced from these facts, it would perhaps be that the partial vacuum within the flask is more favourable to the initiation of putrefactive changes in some boiled fluids than their contact with filtered air. This is what I have always thought, and evidence pointing that way may be found in *Appendix C* of my "Beginnings of Life." Certainly one cannot assent to the conclusion which Dr. Roberts would draw from such experiments, based upon the supposition that the boiling of the sealed flasks in water is a protective measure. Dr. Roberts' results are here again somewhat different from others which have long become matters of history. Need I say that this was essentially the method of experimentation introduced more than a century ago by Needham, and that his results were confirmed by his adversary, Spallanzani, who says: "L'ébullition d'une demi-heure ne fut pas un obstacle à la naissance des animalcules du dernier ordre qui peuplèrent toujours, plus ou moins, tous les vases exposés à son action pendant tout ce temps-là." Does Dr. Roberts forget that Dr. Wyman boiled his flasks for two hours and yet obtained positive results? that he boiled others in a Papin's digester under a pressure of two and five atmospheres respectively, and still obtained living organisms from his flasks. Must I also remind him of the numerous experiments by Prof. Cantoni, of Pavia, in which the hermetically sealed flasks were heated in a Papin's digester to temperatures ranging from 100°—117° C.; and to several of the experiments that I have myself recorded in which undoubtedly—living organisms were obtained from flasks that had been heated in fluids raised to temperatures varying from 130°—153° C. (e.g. such experiments as are recorded in "The Beginnings of Life," vol. i. pp. 441, 443, 447, and 463).

When will those who do me the honour of referring to my experiments look all round and cease to argue from one half of the facts?

H. CHARLTON BASTIAN

University College, Feb. 24

Himalayan Ferns

DURING the years 1861–66 I took every available opportunity to collect ferns in the Sevalik and Himalaya ranges. There being at that period no published work on the ferns of British India, and one subsequently published in Madras not having come under my notice, my specimens, several hundred in number, and all well dried, remain unclassified.

Would this collection be of any scientific value, and if so, to what society could I present it? I opine it would be worse than useless to offer it to the herbarium of the British Museum, as there it might remain untouched for the next fifty years; whilst at Kew I presume Hooker's superior collection would render my poor mite useless.