Zoological Section as the result of the present year's work; the anatomical department of this section has, however, made a fair start under the direction of the late member, Mr. A. G. Burchardt. W. B. Lewis's Report of the Geological Section, with accompanying plates, shows there has been some activity in this department. A. F. Buxton's Entomological Report consists of a complete list of the Lepidoptera which have been noticed within eight miles of the School Close. Under Mr. Kitchener's, the President's, guidance, some good work has been done in the Botanical Section, though the workers seem to be few. Appended to the report of this section is an abstract of two papers by Mr. Kitchener on a Pelerian form of *Linaria vulgaris*. On the whole, the Report of this Society's work for 1872, is one of which there is no reason to be ashamed, and we hope that each year will add to the number of those who take an active part in the work. From many scientific societies it is not advisable nor often expedient to exclude non-workers, but in such societies connected with schools, it should be insisted on that every member be an active worker: only thus can they completely serve the purpose for which they are established.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Dr. Sanderson's Experiments and Archebiosis

THE letter by Dr. Sanderson, in last week's NATURE, contains an interesting and important confirmation of my experiments, which I was very glad to see. There are two or three points, however, which seem to require some comment.

In the first place the flasks and retorts after exposure to the heat were kept only from three to six days, before they were submitted to examination in order to ascertain whether fermentation had or had not taken place. But in cases in which fluids are exposed to heat for a long time, or are exposed to higher temperatures, the process of fermentation is almost invariably delayed and also modified in intensity. It must not therefore be supposed that fermentation would not have taken place at all in certain of Dr. Sanderson's flasks, simply because it had not occurred within four, five, or six days.

Secondly, Dr. Sanderson thinks his present experiments enable him to say that the particular fluid with which he experimented is not prone to undergo fermentation within six days, after it has been heated to a temperature of 100·92° C. I would ask Dr. Sanderson, however, whether he has been careful to observe the precise temperature attained by an infusion boiling rapidly in a flask from which the steam can find exit only through a capillary orifice—as in the experiments which we performed together?

Thirdly, I think it very desirable that Dr. Sanderson should state definitely to the scientific world what precise meaning he wishes to convey by his emphasized use of the word "*chance*" in the concluding paragraph of his letter. There seems a little ambiguity in his use of the word, which is the more to be regretted, since it occurs in the statement of an inference—where freedom from all possibility of misconception is so eminently desirable. H. CHARLTON BASTIAN

University College, June 23

Spectrum of Nitrogen

IN a letter to NATURE (April 17th), Mr. Stearn throws some doubts on the accuracy of my experiments regarding the spectrum of nitrogen. I shall take the earliest opportunity of repeating and completing my experiments, and hope then to bring the question to a satisfactory close. As, however, some time may elapse before I can resume work, I wish to say now a few words in answer to Mr. Stearn's letter.

Before all, I wish to state clearly in what way the correctness of the opinion I profess with regard to the band-spectrum of nitrogen would be affected by an error introduced into my experiments. The unexpected result of an experiment of mine, together with a remark which Plucker makes in one of his papers, suggested to me the idea that the so-called band-spectrum of nitrogen might be that of the oxides of nitrogen. I was confirmed in this idea soon afterwards by a remark of Angström in his recent paper on double spectra (*Comptes Rendus*, August 17, 1871, but which was omitted in the English translation), by which he calls attention to the close resemblance of this band spectrum with the spectrum of metallic oxides. I have described in my paper the experiment just mentioned. A rather narrow tube showed, when exhausted, the lines of nitrogen; as soon as the air entered the bands appeared. The remark of Plucker alludes to the fact that a tube filled with oxides of nitrogen showed the bands with unusual brilliancy. In order to test the accuracy of this opinion I intended to make a crucial experiment by taking care to remove every trace of oxygen. I used for this purpose, at the suggestion of Dr. Balfour Stewart, small pieces of sodium heated in the vacuum tubes. The sodium was fused several times in succession in order to free it from impurities. When the nitrogen was thus treated it always showed a line spectrum, the lines of which seemed to coincide with those of the known line spectrum of nitrogen when measured with the instrument at my disposal. It seems now that I have been too hasty in assuming that this apparent coincidence was a real one. While passing through London a few weeks ago, Dr. Huggins was kind enough to allow me the use of his spectroscope in order to compare, under his supervision, the spectrum of my tubes with the real line spectrum of nitrogen. I then found that, although my tube shows a line which is very near the principal double line of nitrogen, the spectrum is not that of nitrogen. I am at present unable to say what is the origin of this spectrum; but I do not think that its formation can be brought forward as a proof that the band spectrum is not due to oxides of nitrogen. On the contrary, it rather shows that an impurity which has no effect on the spectrum of air, will have one when all the oxygen is removed, and that a change has therefore probably taken place in the conducting power of the gas which gives out the spectrum.

I do not quite see the real object of Mr. Stearn's letter. If he merely wishes to say that the proof brought forward by me is insufficient, and that the question must still remain an open one, I confess I have nothing to say against it. If he, however, wishes to convey the idea that nitrogen has really a double spectrum, I do not think his argument is a correct one.

I will not trespasss any longer upon your space, but I may, I think, fairly ask your readers to suspend their judgment until I have completed my experiments.

Heidelberg, May 30 ARTHUR SCHUSTER

Ground Ivy

WITH respect to the question started in the number for June 12 of this journal as to the Ground Ivy, it may be said that in *Glechoma*, as also in *Origanum vulgare*, *Thymus serpyllum* and *vulgaris*, and *Mentha vulgaris*, specimens having flowers with small corolla and undeveloped anthers are very common, I think as common as specimens having flowers with large corolla and the two sexes developed. Also of *Mentha aquatica* and *Prunella vulgaris* specimens with smaller corolla and only pistils developed are found, but much more rarely than those of the other form.

I have attempted in my work to give an explanation of the origin of the second form of the above-mentioned Labiatæ, as follows:—

The species named are distinguished from our other Labiatæ by the coincidence of the following three peculiarities:—

1. By an abundance of honey, and in consequence of that by an abundance of insects visiting and cross-fertilising them.*

2. In the hermaphrodite flowers, by a stigma so far overtopping the anthers and developed so long after the anthers that self-fertilisation is impossible, or nearly so.

3. By a great variability in the size of the corolla in the hermaphrodite flowers of different specimens.

Now when the flowers on different stems of the same species differ in the size of their corolla, it is evident *a priori*, and ascertained by direct observation, that generally those with the largest corolla are the first seen and visited by insects flying near them, those with the smallest corolla the last. The latter, always the flowers last visited, are fertilised exclusively by the pollen of previously-visited flowers, consequently produce their pollen in vain; and since the non-production of useless organs is always an advantage to every organic being, varieties of the smallest

* For instance, I found *Thymus serpyllum* visited by 7 species of Apidæ, 3 species of *Sphegidæ*, 14 species of Diptera, and 6 species of Lepidoptera; *Glechoma* visited by 21 species of Apidæ, 8 species of Diptera, and 3 species of Lepidoptera.