

take the trouble to re-arrange them in some order more intelligible to the English reader than that of the alphabetical sequence of the common French names.

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. Bastian's Turnip-Cheese Experiments

FROM Dr. Bastian's letter in last week's NATURE I learn that my last communication has afforded him satisfaction. The gratification which I feel at this expression of his approval is mixed with some surprise; for however confirmatory my experiments may be of his, so far as relates to the bare fact that boiling is insufficient to destroy the germinating power of the turnip-cheese liquid, they certainly do not tell in favour of the inference which he is understood to draw from that fact.

The experiments which Dr. Bastian was kind enough to show me last December were regarded by him as unequivocal instances of spontaneous generation. He will remember that at that time I stated to him, both orally and in writing, that the significance of the results in their relation to the doctrine of heterogenesis, appeared to me to be doubtful, and that I thought it probable that they would be interpreted by different persons in opposite senses, according to their preconceived opinions. I expressed myself in a similar manner at a discussion which took place on the subject last winter at the Royal Society. It was for the purpose of clearing up this doubt that I made the experiments recorded in my last communication. I did not expect to prove that the production of Bacteria in Dr. Bastian's experiments was *not* spontaneous, but merely to determine whether the fact afforded any support to the opposite conclusion.

Having first shown that living organisms increase and multiply in the liquid in question, when boiled at the ordinary temperature, under circumstances which absolutely preclude the introduction of living matter from without, I prove that under otherwise similar conditions this result is not obtained when the liquid is subjected to ebullition at a slightly higher temperature. I show further that the liquid even when heated to 102°·5 C. suffers no impairment of its power of supporting the life of Bacteria, for by inoculating it with a drop of ordinary distilled water it at once becomes pregnant. Hence I conclude, not that spontaneous generation is impossible, but that the particular experiment in question is not an instance of it, and that no argument founded on it in favour of the doctrine is of the slightest value.

It is unnecessary for me to occupy your space by at any length adverting to the side questions raised by Dr. Bastian in the other paragraphs of his letter.

In examining the liquids within a few days after heating rather than later, I followed his own method.

I made no attempt to determine the temperature of ebullition in flasks with capillary orifices, because I know of no method by which it could be done accurately. Besides, it was not required for my purpose.

I employed the word "chance" in its ordinary sense. In the sentence to which Dr. Bastian refers I explained that, although there may be a limit of temperature at which a liquid, before possessing the power of breeding Bacteria, is deprived of that power, experiments such as mine are insufficient to define that limit. As regards the turnip-cheese liquid it has been shown that between the temperatures of 100° and 102° C., the probability of pregnancy diminishes rapidly as the temperature increases. It is not as yet possible to say at what point the probability vanishes.

University College, June 30 J. BURDON SANDERSON

The Zodiacal Light

CONTRARY to Mr. Hall's experience of astronomical books (see NATURE, vol. viii. p. 7), in neither Herschel's "Outlines of Astronomy," Humboldt's "Cosmos," nor Guillemin's "Heavens," can I find any hint of a permanent difference between the brightness of the zodiacal light east of the sun and west of it, though Arago's "Popular Astronomy" says that according to Cassini, "it is generally less lively and less extended in the morning than in the evening." But even if Cassini was correct, this is no positive proof of any difference between the two "branches" of the zodiacal light at the same time, seeing that he lived in the tem-

perate zone, and probably did not observe it in both morning and evening at the same time of year. Mr. Hall's situation in Jamaica is favourable for investigating this point, and I should not wonder if he finds the fact different from what he supposes. But even the books that consider the zodiacal light to surround the sun in the shape of a lens, acknowledge that it may extend further one way than another, and further at one time than another.

Sunderland, June 7

T. W. BACKHOUSE

AT about half-past one in the morning of June 5, the sky was clear, but the stars were not very brilliant, on account of the diffused light, and consequently the Eastern branch of the Zodiacal Light was very faint; as I was endeavouring to trace its course, a strong beam of light appeared so suddenly as to have quite a startling effect; it was not shot out like the rays of the Aurora Borealis, but gathered strength throughout its whole course, which lay through Aquarius, over the stars α and β Capricorni, through Sagittarius, across the Milky Way, and through Scorpio, passing to the N. of Antares; its visible length was therefore upwards of 100°, and as I was about to make accurate observations, it suddenly disappeared, having lasted somewhat less than one minute.

Its course was therefore nearly parallel to the Ecliptic, and about 6° to the N. of it; its breadth was from 3° to 4°; its brilliancy was equal to that of the brightest part of the Milky Way, through which it passed, and therefore allowed me to judge very accurately; and it had no colour.

Now Humboldt says in his "Cosmos,"* "I have occasionally been astonished, in the tropical climates of South America, to observe the variable intensity of the Zodiacal Light," and he considered the variation to be due to atmospheric changes, as I myself have hitherto done; but in the case above no ordinary atmospheric changes could have produced the effect observed.

It occurred June 4d. 18h. 40m. Greenwich mean time, and it would be very interesting to know whether the magnetic instruments were affected at any part of the earth.

Jamaica, June 1873

MAXWELL HALL

Meteorological Influence of Trap Rocks

THE thermometer in a mine, or coal-pit, rises, according to Herschel, 1° for every 90 feet of descent, or 58° per mile; and, according to Clerk Maxwell, the rate of increase in this country is 1° for every 50 feet of descent. These results are obtained in passing through a very small portion of the superficial crust of the earth; such, for example, as a part of the coal formation, which possesses a very low degree of conductivity. We can hardly, indeed, conceive a worse conductor than a crust consisting of alternating strata of freestone, shale, till, coal, limestone, &c. But these strata are very frequently perforated by comparatively homogeneous intrusions in the form of trap dykes, which not only possess greater conductivity, but which, from the analogy presented by volcanoes, very probably extend down to the molten matter subjacent to the external crust of the earth. Such trap dykes may be compared to an iron poker thrust through the superficial strata having its lower end in a state of fusion, and its upper end kept cool by radiation into the atmosphere. Through any continuous dyke, if this view be correct, there will therefore be a more rapid escape of heat; and when such igneous rocks occupy spaces of many square miles of the earth's surface, one would, at first sight, expect them to play a very important part in affecting the meteorological conditions of the district in which they are found. They might be expected, by the large amount of heat which they conducted freely to the earth's surface, to stimulate the growth of plants; and by the radiation of the liberated heat into the atmosphere, they ought to become—especially during night—the generators of storms, by causing a constant ascent of rarefied air. It is quite true, however, that the meteorological effects of such an agent must, as in the case of volcanoes, be observed by the far grander cycle of disturbances initiated by the solar heat; and that its agricultural efficiency may be, to a large extent, negated by differences of chemical constitution, acidity, and exposure. Still, however, the influence is there, and ought, in one way or other, to make itself sensible.

Do any of your readers possess information bearing upon this question? Such, for example, as experiments on the conductivity of the different kinds of trap as compared with the stratified rocks, or observations of the temperature of the air, especially during night, above trap-rocks as compared with that

* Our's transl., vol. i. p. 371.