organisms in the blood as epi-phenomena in the course of certain diseases can scarcely be explained except by the supposition that archebiosis or heterogenesis (one or both) have taken place in their altered blood, or in blood and tissues simultaneously.

This the present writer long ago pointed out, and he strongly insisted upon it in a paper published about eighteen months ago,¹ but which does not seem to have reached Dr. Lewis before the printing of his present work. Attention was there specially called to the fact that organisms speedily appeared in the blood of previously healthy animals or of human beings suddenly killed, in such situations and under such conditions as to make it almost impossible to account for their presence except by the occurrence of one or other of the processes above mentioned, giving rise *in situ* to a new birth of such microphytes. Organisms can, in fact, be made to appear at will (as Lewis and Cunningham, as well as Sanderson, had shown) in localised parts of previously healthy organisms by lowering the nutrition in certain ways of such parts of the body, *i.e.*, by either tying the artery supplying the part with nutrient fluid, or by subjecting the part to the influence of some germless chemical irritant. On the other hand, when the nutrient processes throughout the body are checked by the death of the animal, the production of microphytes, which was before local, now, as the writer has several times pointed out, becomes general.

Let the germ-theorists look to these facts and give us a better explanation if they can; because in the cases above referred to, organisms appear in tissues which they themselves have proclaimed to be germless, and in blood which they have declared to be free from all antecedent signs of microphytes.

The facts of the latter order have been distinctly confirmed by Dr. Lewis. He says: "Rats were obtained, killed by means of chloroform, and set aside from three to twenty-four hours or longer, according as the temperature of the atmosphere was high or low. The result proved that almost invariably bacilli were to be found in the blood, in the spleen, and in other organs."

It appears, however, and the fact is one of considerable significance, that when death takes place in certain modes (as by poisoning with carbonic acid or carbonic oxide), organisms have a still greater tendency to appear in the blood and that they manifest themselves with surprising rapidity.

A man who was sent to seek for rats, having found, "That he could procure more than could be accommodated in the cage which he had brought with him, he obtained a large earthen vessel, transferred twenty-seven rats into it, and tied a piece of cloth over the mouth of the vessel. As may be supposed, the rats had perished before he got home—all except one. . . . I examined the blood and the spleen of twenty of these rats within about six to eight hours of their having been caught, and found in each case that there were innumerable bacilli present, in every way morphologically identical with *Bacillus anthracis*.² In some of the cases the number was astonishing. They were present chiefly in the form of rods, but here and there some were seen to have grown to such

a length as to cover two fields of the microscope. . . . This experience tends to give support to the statement made by M. Signol before the French Academy, to the effect that motionless bacilli identical with those found in charbon, will be found in sixteen hours, or less, after death, in the blood of animals which have been asphyxiated by means of a charcoal fire."

Dr. Lewis shows that these organisms which make their appearance within the bodies of animals so soon after death are not only morphologically indistinguishable from *Bacillus anthracis*, but that they 'go on, under suitable conditions, to the so-called "spore" formation in precisely the same manner. The characters of these organisms under different conditions are well shown in Pl. I.

But if mere modes of dying influence the quickness with which such organisms appear in the body after death, it is not inconsistent to suppose that they may in certain cases—that is, in association with certain morbid processes—be much more prone than in others to show themselves as epi-phenomena. And this seems to correspond with what actually occurs; in many contagious diseases, as above stated, such organisms seem to be absent, in a few they show themselves, and that by far the most frequently in cases where death is already pretty closely at hand.

Referring to the bacilli met with in malignant pustule (charbon), septicæmia, and the so-called "typhoid fever," in the pig, horse, and other animals, Dr. Lewis says: "It may be confidently stated that they are never to be detected in the earlier stages of the disease, but only at a brief period before and after a fatal termination. To my knowledge they have never been found in the blood of animals which have subsequently recovered; they have always been recognised only as one of the concomitants of impending dissolution. This is undoubtedly the case so far as the two diseases first cited are concerned."

Those who are the warmest advocates of the germ-theory of disease—a doctrine resting on sufficiently unstable foundations—are not always cautious or discreet in the way they speak of others who lean to a belief that the organisms met with in association with disease are mere epi-phenomena, often produced within the body by a process of heterogenesis. Yet the latter interpretation, so far as present knowledge goes, seems to the writer essential for the explanation of our power to determine, at will, the presence of microphytes in the germless tissues or germless blood of previously healthy animals.

H. CHARLTON BASTIAN

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Antiquity of Orchids

I HAVE been struck by a most cogent remark of Mr. Wallace's in his review of Mr. Allen's "The Colour Sense" (NATURE, vol. xix. p. 501), viz., "But surely in orchids the perianth

¹ "On the Conditions Favouring Fermentation, &c.," *Journal of Linnean Soc. (Zool.)*, vol. xiv. pp. 89-93.

² That is, the bacillus met with in association with charbon or splenic fever.

more highly specialised than in any existing flowers whatever; and if we take into account the world-wide distribution of these plants, their intense richness in genera and species, and their wonderful complexity of structure, we must consider them as among the most ancient instead of the most recent of flowers."

Without venturing any opinion as to the geological age to which the development of this wonderful order of plants must be traced back, it seems to me that there are some classes of facts concerning our native European species which support the conclusion that their existence in their present specific characters must date from a very remote time.

1. It is an important fact that out of fifty species of orchids enumerated in Garcke's "Flora des deutschen Reiches" (exclusive of the Bavarian Alps, which possess two or three more species), not less than forty-one occur in the British Isles (besides *Neotinea intacta* and *Spiranthes romanoviana*, not found in Germany), a proportion considerably exceeding that of phanerogams generally. Now, as it seems scarcely credible that orchids should possess means of transportation across the sea in preference to other plants, we must conclude that they inhabited the British Isles before their separation from the Continent, which involves that they occupied stations near the present coasts of Germany or France previous to a great deal of plants that reached these coasts only subsequently to the formation of the Channel. These conclusions are rather strengthened by the fact that several orchids are by no means frequent in Germany, and very rare and local in Britain, which proves that their occurrence is not to be accounted for by favourable present conditions, and even renders it probable enough that some of the species found in Germany, but not in Britain, may in the latter country have become extinct in times not very long ago.

2. Notwithstanding the light so plentifully thrown on the significance of the floral peculiarities of our orchids by Mr. Darwin's admirable investigations, there remain some species whose relations to insects, although evidently of a most specialised nature, are yet very little understood. Such seems to be the case with *Himantoglossum* and with *Ophrys*. It is therefore to be suspected that the adaptations of these species may point to insects no longer existing in our countries. However, I should not insist on this point were it not somewhat connected with the following:—

3. It has been observed by Mr. Darwin that "the frequency with which throughout the world members of various orchideaceous tribes fail to have their flowers fertilised, though they are excellently constructed for cross-fertilisation, is a remarkable fact." And further on Mr. Darwin alludes to the unknown causes which lead to the destruction of seeds or seedlings, forming a check to the multiplication of orchids. Indeed, with many of our native species, though abundantly fertilised, multiplication by seeds is evidently but a rare exception to the general rule of propagation by side-bulbs, *i.e.*, mere individual persistence. Thus the wonderful contrivances for cross-fertilisation point back to different conditions of life in the past, under which their function must have been much more active and important than it is now.

There may, I think, be found analogous cases in very different quarters of the vegetable kingdom; for instance, the frequent reduction of the peristome of mosses to mere rudiments is probably connected with actual preponderance of vegetative propagation over propagation by spores. In the orchids, too, there are already perceptible some traces of a regressive change of the apparatus for cross-fertilisation (for instance, in *Ophrys apifera*), as will be inevitable in the course of ages, whenever specialised structures are no longer sustained by active function, leading to their reproduction under the agency of natural selection. Perhaps such regressive change goes on more slowly in cases of merely vegetative propagation.

Finally, I may allude to the fact of our native orchids belonging to very different groups of the order, and this enhances the argument for antiquity, based on their geographical distribution.

Freiburg im Breisgau, May 10

D. WETTERHAN

Barometric Pressure and Temperature in India

In order to satisfy myself as to whether some of Mr. Broun's conclusions (NATURE, vol. xix. p. 6) held good when inland stations were included in the comparison, I recently examined in detail the pressure and temperature oscillations at fifty-one stations in the Indian peninsula, computed from the means given

in Mr. Blanford's "Indian Meteorologist's *Vade Mecum*." As these stations represent every part of the country, the results afford a basis for deduction of sufficient extent to be reliable.

An inspection of these shows (1) that the range of pressure-oscillation corresponding to 1° F. varies very much at different places, its extreme limits being 0.002 in. at Leh, and 0.032 at Vizagapatam, and its mean value for all the stations 0.017 in.

2. That on arranging the stations in order of elevation, it is plain that this variation is partly related to the height above sea-level, being least at Leh (11,538 feet), the most elevated station, and increasing from thence downwards at the mean rate of about 0.002 in. for every thousand feet of descent. The numerous deviations from this rule, however, especially at the lower elevations, make it evident that other factors operate besides height, such as distance from the coast and latitude. Though no rigorous comparison with these has as yet been made, a mere inspection of the results is enough to show that proximity to the sea, and, therefore, *ceteris paribus*, greater humidity is associated with a greater barometric oscillation corresponding to 1° F.¹

3. That when the element of horizontal space alone is considered, the pressure oscillation increases with the temperature oscillation, but not very regularly, and that while this relation holds pretty generally in the plains, it disappears altogether at the more elevated stations, or, when the element of vertical space is introduced.

Now while the pressure ranges generally decrease pretty regularly as we ascend, the temperature-range remains about the same at different heights. The decrease, therefore, in the pressure-oscillation for 1° F., noticed in the case of elevated stations, must plainly be solely due to the contraction which takes place in the pressure-range.

The diminished mass of atmosphere above the more lofty stations, doubtless in a great measure accounts for this diminished amplitude of the oscillation. Another important factor, however, in this result, must not be overlooked, *viz.*, the double annual oscillation of monthly mean pressure which takes place at stations of 6,000 feet and upwards in height, apparently throughout India, the effect of which is sensibly to diminish the absolute annual range of pressure-oscillation at these heights.

In fact, the curves representing the mean monthly barometric pressure in the winter at the hills, and on the plains respectively, may be taken to approximate more or less in character to those in the adjoining figure.

Pressure Curve (the critical points are marked with asterisks).

Now as the pressure on the plains is made up of the pressure at a height of 6,000 feet + the weight of the stratum of air between, it is evident that the latter must reach its maximum somewhat later than December. Moreover, as the winter depression at the highest stations is considered to be caused by the inrush at their level of the saturated anti-monsoon at this season,² the direct effect of the cold of this season in increasing the density, and hence the pressure of the atmosphere, will only be felt in the stratum of air between them and the plains. It is im-

¹ This relation appears to be independent of that due to elevation, with which it might be thought identical, owing to the general decrease in the heights as we travel seawards. The following stations which have nearly the same elevation, but are at widely different distances from the sea, will show this very clearly:—

	Height above sea in feet.	Bar oscillation for 1° F.
Lucknow	369	0.017
Sibsagar	332	0.028
Difference	37	0.011

Now if the approximate rule for difference of elevation already mentioned, *viz.*, a decrease of 0.002 inch in the bar-oscillation for 1° F. for every 1,000 feet of ascent be applied in this case, the decrease at Lucknow should evidently be inappreciable. As it is, however, it is of very considerable magnitude.

² Vide "Indian Meteorologist's *Vade Mecum*," by H. F. Blanford, p. 75.