from him in my interpretation of the results of some of these investigations.* I am quite content to accept the conclusion which is alone derivable from this long chain of evidence. I am even prepared to grant to Professor Huxley, for the sake of argument, that *Bacteria* may be "suspended in the atmosphere in myriads." The evidence thus referred to, if true in all respects, would have been very valuable if it had been brought against the doctrine that none of the minute living things of infusions derived their origin from atmospheric germs, though it may and does fall utterly powerless before the doctrine which is alone urged, that *some* of the Living things met with in infusions appear to be produced independently of pre-existing living matter. If it could be proved that the air contained five hundred times as many germs as can now be shown to exist therein, this discovery would still be quite compatible with the truth of the other doctrine that under the influence of certain conditions some Living things, appearing as minutest visible specks, do arise *de novo* in solutions.

Whether such an occurrence can or cannot now take place is a question which is not at all dependent upon the prevalence or paucity of germs in the atmosphere. I may also remind Prof. Huxley of a fact which he seems to have forgotten, and that is, that the atmosphere is not the only source of germs. These may be present in the water or in the materials dissolved therein. Seeing, therefore, that in certain experiments which constitute the corner-stones of his edifice of proof, and which are brought forward, I suppose, as being capable of influencing our judgment upon this great question, the materials which were dissolved and the water employed were merely boiled for fifteen minutes, we must look upon this as an admission by Prof. Huxley that in his opinion the exposure of the solution for such a time to a temperature of 100° C. was an adequate precaution to ensure the destruction of all pre-existing living things that may have been contained therein. This is a most important admission—tacit though it be—in the face of other evidence which can be mentioned, and if Prof. Huxley does not really believe this, how is it possible for us to understand what either his argument or science gains from the citation of the following experiments?

Having boiled portions of "Pasteur's solution" for fifteen minutes, in three separate flasks, he placed in the neck of one of them, whilst ebullition was continuing, a large plug of cotton wool, left another with the mouth of the flask open, whilst into the third, when cool, he placed some *Bacteria* taken from a solution of hay. "In a couple of days of ordinary warm weather," he says, "the contents of this [latter] flask will be milky from the enormous multiplication of *Bacteria*. The other flask open and exposed to the air will, sooner or later, become milky with *Bacteria*, and patches of mould may appear in it; while the liquid in the flask, the neck of which is plugged with cotton wool, will remain clear for an indefinite time." And then Prof. Huxley adds:—"I have sought in vain for any explanation of these facts, except the obvious one, that the air contains germs competent to give rise to *Bacteria*," and similar to those with which one of the solutions was purposely inoculated. Now, with reference to these statements, the possibility at once suggests itself, that had a different solution been used in the case where the neck of the flask was plugged with cotton-wool a very different result might have been obtained. In order to throw light upon this subject I have performed the following experiments:—

Immediately after reading Prof. Huxley's address, I procured a piece of cooked meat, made an effusion of the same, and after filtration put it into a flask. It was then boiled for fifteen minutes, after a large plug of cotton wool, $1\frac{1}{4}$ " in length, had been pushed into its neck. After this time the plug was rendered tighter by pushing in more wool. Another flask was prepared in

* Neither time nor space will permit of my mentioning these various points on which I am inclined to differ from him. When Prof. Huxley says, however, after a tragical metaphor, "It must be admitted that the experiments and arguments of Spallanzani furnish a complete and a crushing reply to those of Needham," I will only say that I cannot agree with him, and will remind him that, in this case at least, he is not supported by Pasteur, whose logic is so invincible. Pasteur says (*Ann. de Chim. et de Phys.*, 1861, p. 9):—"Un examen impartial des observations contradictoires de Spallanzani et de Needham sur le point le plus délicat du sujet, va nous montrer en effet, contrairement à l'opinion généralement admise que Needham ne pouvait en toute justice abandonner sa doctrine en présence des travaux de Spallanzani."

I would also call Prof. Huxley's attention, as an impartial historian, to some communications made by M. Victor Meunier to the French Academy (*Comp. Rend.*, 1865), from which he will see with reference to the vessels with bent necks, that it is possible to perform these experiments with an "entire success" of a different kind from that to which he alludes. Others besides myself have also performed such experiments with results similar to those of M. Meunier. Much seems to depend upon the nature of the solution employed.

a similar way, only in this, a strong filtered infusion made from undressed meat was placed. At the expiration of the fourth day (Monday morning, Sept. 17th) the weaker solution, still quite clear, was opened, and on microscopical examination of two or three drops of the fluid a multitude of minute motionless particles of various sizes were seen, others in active movement, and two or three *Bacteria* about $\frac{1}{8000}$ " in diameter. The flask containing the stronger solution was opened at the expiration of forty-two hours. The fluid still appeared quite clear, and on microscopical examination of a few drops of the fluid many tolerably active *Bacteria* were found varying between $\frac{1}{10000}$ "— $\frac{1}{1000}$ " in length, besides a multitude of particles, some moving and others motionless.

These results seem to me what might have been expected after what I have made known concerning putrefaction *in vacuo*. It could scarcely be expected that mere filtration of air should be able to prevent putrefaction when it has been already shown that this will take place in the absence of air.

What conclusion, then, is now deducible from Prof. Huxley's three comparative experiments? Certainly nothing that has any value for the support of his argument.

[A strong point made by Prof. Huxley is the supposed fact that the possibility of preserving meat is a fatal reply to the experiments of myself and others. I shall show next week, that the actual facts strengthen my point of view, and that "perfectly good" cases of meat which I have examined have contained *Bacteria* and *Leptothrix* filaments.]

H. CHARLTON BASTIAN

LETTERS TO THE EDITOR

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

English Physiology

THE present state of physiology in this country ought to be a matter of regret. Though foremost in many things, Britain is far behind Continental countries in the field of physiological science. We can boast of a few distinguished physiologists, as John Hunter, Sir Charles Bell, and Dr. John Reid; and famous microscopists, as Carpenter and Beale; but a very small number of English names can be cited compared with the host of Continental physiologists, past and present, as Magendie, Müller, Von Beidol, Von Baer, Bédard, Bernard, Brown-Sequard, Du Bois Reymond, Helmholtz, &c. This discrepancy arises not from want of talent, but from lack of opportunity. The mental qualities required by a physiologist, as observation and memory, are developed separately at different periods of life. Hence there are only a limited number of years during which any such branch of learning can be cultivated with fresh ardour, and during which the power termed originality can be brought into play. The Continental schools make use of these precious years by affording those who are naturally inclined to cultivate any one branch of science, full scope for repeating the observations of their predecessors, and for endeavouring to add to the existing stock of knowledge. By having various laboratories and certain paid appointments connected with their universities, they allow young men to devote their whole time and energy to the study of individual subjects, as physiology. Those who set themselves to work of this kind do not look forward to the practice of the medical profession, but purpose to live and work as physiologists. These young men are known by their labours to be specialists, and are proposed by the senatus with which they are connected for a vacant professorship when it occurs. This is the only method of securing original and extensive work in any one scientific branch, as Physiology. It would be well, therefore, if the approaching Royal Commission of Inquiry into the State of Science in this country would not overlook Physiology, but would make some arrangements whereby Great Britain might no longer be stigmatised by her Continental neighbours as "having no Physiologist."

Dr. Stricker, in the two articles he has already communicated to NATURE on "The Medical Schools of England and Germany," has not referred to the Edinburgh University, which possesses the best furnished Physiological Laboratory to be found in Great Britain, and one equal to most of those met with in Germany. The plan recently introduced into this Scottish University of having salaried assistants to the professors re-

semples that which exists in the German Universities. This arrangement not only allows opportunity for carrying on original research, but also enables the professors to impart a most beneficial impulse to younger men.

In this country it has hitherto succeeded best, and appears to be most in consistence with our national constitution, to place the pecuniary assistance afforded to scientific men under the control of learned bodies as the Universities and the Royal Society. It is to be hoped, however, that our Government will, ere long, recognise the duty of advancing physiologists by aiding them with grants of money. Every medical school in Britain ought to have a physiological laboratory, well furnished with instruments—electrical, chemical, and physical; and with the view of instituting and supporting these it will be necessary to supply certain sums of money annually from the public exchequer. In addition to money given to Medical Schools for the purpose of buying instruments, two or more grants might be set aside as prizes to be bestowed annually on the schools furnishing the best physiological work, and to be distributed by the governing bodies of the said schools among the workers. The awarding of such prizes might very well be entrusted to the Royal Society.

I trust that this plea on behalf of physiology will not pass unnoticed by you.

Birkenhead

P. M. BRAIDWOOD

Mirage

MIRAGE is not, in my experience, an uncommon phenomenon. I saw it this summer on the flats at the mouth of the Dee (Wirral side). It may often be seen, with a bright sun and still air, after heavy rain, on Hartford Bridge Flats, and other level and gravelly heaths in the Bagshot Sand district, where the fir trees may be seen floating in water, or forming promontories jutting out into a lake—a phenomenon similar to, though far less striking than, what I have seen on the Plain of Aan, in Provence.

The most curious mirage-effects I ever saw were on the Wash during hot summer weather. The mirage is there known as the "looming of the land," and when it is about it is impossible at moments to distinguish the sand and weed-banks from the sea, while the distortion, both perpendicular and horizontal, of ship's masts, &c., is ludicrous. In one case I saw a herd of seals on a sand-bank transformed into a row of long-legged monsters, wading in water, or rather rooted by their long legs to the legs of a similar row of monsters below them, which was their distorted reflection in wet mud. I had some difficulty, at first, in making out what they were.

Eversley Vicarage, Sept. 16

C. KINGSLEY

Astronomical Science

IN your number for August 5th, a letter referring to Astrology, signed "C. J. Robinson" ends as follows:—"Astronomical Science is hardly likely for the sake of sentiment to treasure up the discarded swaddling clothes which for so many centuries impeded its onward progress." Surely such language indicates a sad confusion of ideas on the subject, since it is most unquestionable that the belief of antiquity in Astrology—far from retarding—greatly promoted the study of Astronomy. In fact, the names of Ptolemy and Kepler show that the greatest of ancient and the greatest of modern astrologers were at the same time the greatest Astronomers of their era, and the brilliant discoveries of the latter in both sciences suffice to dispose of the "swaddling clothes" theory without citing the instance of Napier, who, it is well known, invented that most admirable scientific expedient and indispensable handmaid to Astronomy, Logarithms, to shorten and facilitate his astrological calculations.

I have not seen Moore's Almanac referred to by Mr. Robinson, but any one by consulting an Ephemeris may verify the following curious facts. War against Prussia was declared by the French Emperor on the 15th July. The preceding lunar change was a total eclipse of the Moon on the 12th, in 20° 15' of Capricorn, when the Sun and Moon had (substantially) the same declination as Herschel, Saturn, and Jupiter. Between noon on the 14th and noon on the 15th, Mars came to the opposition of Saturn retrograde. On the 15th, Herschel was in conjunction with the Sun, the planet having at the same time the exact parallel of declination of Saturn and Jupiter. So exceptional and extraordinary did these planetary positions and relationships appear to me that more than two years

ago I made two crosses at the middle of July in my Ephemeris, and outlined a hand in the margin that I might not omit to note when the time came whether anything unusual occurred. Now the eclipse on the 12th took place on the Ascendant in the Revolutionary Figure of the Emperor Napoleon in square to Mars and opposition to Herschel, and according to the old astrologers "an eclipse of the Moon in Capricorn in evil aspect to Mars causes military disasters," whilst modern astrologers credit aspects of Herschel with producing events of a strange and unexpected character. Again the same figure presents the Moon in conjunction with Saturn retrograde on the place arrived at by Herschel by direction, whilst the Ascendant falls on the place attained by Saturn, the whole presenting, according to the canons of Astrology, a rare combination of evil portents. Probably it is the preceding data taken in conjunction with the primary directions (also of evil import) which have furnished the ground for the predictions of misfortune to the French Emperor to which Mr. Robinson alludes.

T. S. PRIDEAUX

7, Eardley Crescent, West Brompton

[We insert this as a specimen of a kind of letter which it should be impossible to write in the nineteenth century.—ED.]

Insects upon a Swallow

DURING the month of August, at Meran, in the Tyrol, a swallow sitting upon a stone at the side of a public thoroughfare let me take it up without showing the least fear, or even moving. The cause of its indifference was immediately apparent; two large insects of a dark slate colour were running about the bird upon the outside of its feathers, their power of adhesion being considerable. While trying to remove them, one got upon my hand and was lost, being thrown some distance by the second of two hasty but vigorous shakes. The other fell to the ground after hanging by a thread, similar to, but much stronger than, a spider's single thread. The form of the insects was quadrilateral, the head being at one of the angles, the measurement between the opposite angles being about $\frac{1}{4}$ inch; the strength of the skin was so great that the insect required three crushing rubs by a lady's foot against the road before its activity was destroyed. The bird seemed conscious of release from its parasites, and struggled to get away, and then was only just able to flutter languidly to a tree about forty yards distant. The toughness of the insect, its activity and power of clinging, fully account for the inability of the bird to free itself.

I have seen an account *somewhere* of a bird, whether a swallow or lark I forget, similarly troubled, and showing the same fearlessness of capture.

G. H. H.

Birkenhead, Sept. 8

NOTES

PROFESSOR HUXLEY's presidential address is not his only outcome at Liverpool which it is our duty to chronicle—a duty which we perform with gratitude to him for his plain speaking. At the unveiling of Mr. Gladstone's statue on the 14th inst., Mr. Huxley, after referring to the Compulsory Education measure, which promises in time to rid us of our worse than Eastern degradation, as one of Mr. Gladstone's greatest achievements, added that if he might presume to give advice to a man so eminent as Mr. Gladstone—if he might ask him to raise to a still higher point the lustre which would hereafter surround his name in the annals of the country, it was that he should recollect there was more than one sort of learning, and that the one sort which was more particularly competent to cause the development of the great interests of the country, was that learning which we were in the habit of calling Science. That Mr. Gladstone was profoundly acquainted with literature, that he was an acute and elegant scholar, they all knew, but he suspected that the full importance for the practical interests of the country of developing what was known as Science was not quite so clear to the Prime Minister as it might be. But, seeing the great faculty of development which his past career had shown, he had no doubt that such a man would by-and-by see that if this great country was to become what it should be, he must not only put the