sopher to deny that we can penetrate the veil of appearance; but for such a one, the words "universe," "eternal," "ultimate," are unmeaning, or at best indicative of problems, not words to be lightly used in positive propositions. Mr. Wrench's phenomenalism is, in short, a very thinly-disguised materialism.

As philosophy, then, the book has no great merit. Nor can it be said greatly to extend or clarify our psychological knowledge. Mr. Wrench's fundamental classification—that of the instincts as self-preservative, reproductive, gregarious—is familiar, but it should not be accepted as final without strict examination. His notion of "sub-instinct," a specific form of one of the main instincts, as, *e.g.*, patriotism is a specific gregariousness, is not without value, but it is scarcely conducive to clearness to apply this same term to the objective social custom which results from the interaction and mutual modification of the "forms the instincts take in the thought of the individual." Mr. Wrench's main practical inference from his analysis of human nature is that our present system of education should be inverted, and science given the predominant place, for, he says, "the process of abstraction is essentially gregarious." The intellectual fallacy in educational theory has been so often exposed that it is unnecessary to do more than notice this remarkable version of it.

In statement Mr. Wrench is clear and concise, and such purely scientific exposition as he gives in the course of his work is admirable.

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 intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

The Need of a Great Reference Library of Natural Science in London.

ANYONE desiring to see the new books in various branches of science who has had the use of the great libraries in Oxford or in Cambridge, and finds himself transferred to London as his habitation, must be astonished, as I have been, to find that there is no great scientific library in London, and that access to all the incomplete libraries of the various scientific societies does not enable him, even when he takes the large amount of trouble necessary to inquire at all of them, to see the important and necessary new books in various branches of work.

The deficiency is in regard to new "books" rather than in regard to periodicals. It must be noted that of late years, not only scientific periodicals, but large and costly separate scientific books or special memoirs, often expensively illustrated, have appeared, and are appearing, in increasing number. I could name several books in prehistoric archaeology, in comparative anatomy, and other subjects, which I have been unable to find in London within six months or a year of their publication, and others which are not likely to be purchased by any of our societies. The smaller societies devoted to special subjects have neither money nor house-room for a first-rate library. The larger societies neglect special subjects, on the theory that they are provided for by the special societies. The Royal Society has by no means such a library as might be expected in view of its age and dignity. It has insufficient funds and space, and, whilst aiming at completeness in periodicals and the publications of scientific societies, is a "broken reed" for one who leans on it as a help in the matter of books. It is true that the Linnean, the Zoological, the Geological, and the Chemical Societies, and the Society of Antiquaries have in their libraries many splendid books, and annually purchase a limited number of new books; but if their libraries are taken all together, in conjunction with that of the Royal Society, the Royal Medical Society, and the London Library, they do not

constitute that thing which is so necessary to the mature student of modern science, namely, a complete, or nearly complete, library of scientific publications, where the newest books may be seen and consulted as soon as published.

We are so behindhand in this matter that it is not possible in London even to see a new book from France or Germany with a view to its purchase. We ought to have in London a professedly *complete* library of modern scientific publications accessible to all mature students (whether on payment of subscription or otherwise), provided with a big reading-room where all the newest books can be seen and read. Such a library should not lend its books, but have them always ready for consultation. It should have a staff of really competent librarians able to help the reader to find what he wants, and it should be open until ten or eleven o'clock in the evening, and as late on Saturdays and all public holidays as on other days, for it is precisely at those hours when libraries are universally shut that a great number of eager students would find their only chance of using them.

It has been often suggested that such a library as I desire might be formed by the union and cooperation for this purpose of our various scientific societies, and I believe that might be so if a practical scheme were formulated. It would not be necessary for every society to give up its existing library, but it would be necessary for each society to contribute largely in money and books in order to constitute and maintain the new combined or central "consulting" library. Probably if the Government could be persuaded to give for this purpose the buildings formerly assigned to the University of London, and now occupied by the various examining bodies connected with the Civil Service and the Army, the National Scientific Reference Library could be at once constituted. In view of the urgent public necessity for such a library, the Government might be expected to provide a subsidy of two or three thousand pounds a year, and the scientific societies might contribute so much a head for their members and place their existing libraries at the service of the new institution without giving up their special rights to borrow certain books.

In order to make any further in the matter, it is clearly necessary to form, in the first place, an estimate of the minimum size of such a library and its reading-room, and of the annual expenditure necessary for the purchase of books, as well as for librarians, attendants, heating, and lighting.

I should be glad to receive any suggestions from those who feel the need of such a library. It seems to me that the essential points to be aimed at are:—(1) completeness, so that any and every book of scientific quality shall be on the table as soon as published; (2) accessibility of the library to readers until a late hour of the evening and on holidays and half-holidays, as well as on ordinary days.

The value of such a library to every kind of worker in science would be immense. It should be open to everyone on payment of a moderate annual subscription. It may be objected to any new library (such as I propose) that the library and reading-room of the British Museum supply the want. They do not, since books are not obtained there without delay. Many foreign books are not obtained there at all.

E. RAY LANKESTER.

Vapour-density and Smell.

IN a letter to NATURE of May 13 I made a statement to which Dr. Perman very naturally takes exception (May 27, p. 369). He cites ammonia, hydrocyanic acid, and hydrofluoric acid as instances of volatile bodies lighter than air, yet odorous. In considering the physiology of olfaction, however, certain conditions which might lead to misconception must be ruled out. In the first place, a very minute addition of impurity suffices to give odour to an otherwise odourless substance. Formalin was the substance of which I was writing. My judgment, based on sensory experience, absolutely declines to accept the somewhat fatty scent which I recognise with my nose close to a dish of formalin as a property of the vapour which irritates my conjunctiva when far beyond the range of

smell. The chief drawback to the ordinary commercial method of preparing formaldehyde is, I am told, the impossibility of preventing polymerisation. In the same way, as Dr. Perman himself points out, hydrofluoric acid at ordinary temperatures "consists mostly of molecules H_2F_2 ." Hydrocyanic acid, again, shows a great tendency to polymerisation and to decomposition in the presence of water. The possibility of ionisation in the presence of the film of moisture on the surface of the olfactory membrane and of the moist air in the nasal chambers must also be taken into account. It is also possible that certain gases produce an olfactory effect after the incorporation of water in their molecules.

In the second place, a distinction must be drawn between indirect olfaction due to chemical action and olfaction which can be accounted for only as the result of the vibration of olfactory hairs. I, personally, should hesitate to describe the effect upon my nervous system, through my olfactory membrane, of pure ammonia, as a sensation of smell. It seems to stand in an entirely different category from the smelling of musk. To make such a distinction recalls to mind the fact that olfaction is the successor of chemical stimulation, chemiotaxis. The sense of smell may be based upon the older and coarser mode of action of olfactory bodies as well as upon the more modern and refined.

Either of the three substances which Dr. Perman has cited as odorous is capable of producing a change in the constitution of protoplasm such as cannot, we suppose, be produced by the minimal amount of human effluvium which enables a dog to track his master, or even by the minimal quantity of drifting particles which are capable of appealing to a man's far less sensitive nose. It can be demonstrated experimentally that one part of mercaptan in 50,000,000,000 of air gives a recognisable odour to the mixture. Chemical action in such a case seems to be out of the question.

Although we cannot conceive the way in which so minute a quantity of matter plays upon the instrument which originates nerve-impulses, we picture the olfactory hairs as answering to some change in the vibrations of the molecules of air, or of the atoms within their molecules, due to the influence of the olfactory particles. Such evidence as is at present available, if we make allowance for the sources of error to which I have alluded, points to the conclusion that to produce this molecular or intra-molecular change the added gas must be heavier than air. That olfactivity is not proportional to density is sufficiently evidenced by the aggressive scent of sulphuretted hydrogen and of many other substances which are comparatively light. In my letter of May 13 I suggested that the inability of flies to distinguish between pure water and water containing formaldehyde seems to point to the same conclusion.

ALEX. HILL.

The Germ-layer Theory.

THE most important criticism in the review on May 13 of "The Origin of Vertebrates," by Dr. W. H. Gaskell, is based on a dogmatic view as to the fundamental distinctness of the germ layers and their predetermination for the formation of certain organs. It is evident that your reviewer regards this as a settled fact. It is therefore only fair to point out that this is by no means the opinion of all morphologists. Indeed, Morgan, Hertwig, Braem, Driesch, Conklin, Jenkinson, and many others grant little phylogenetic value to the germinal layers.

The germ-layer theory requires the supposition that there is a prelocalisation in the egg of the various substances necessary for the formation of the different organs, and that these substances in its segmentation pass into definite segments which form the germ layers. Now this supposition is directly contradicted—or at least made exceedingly improbable—by the results of the experimental separation of the first two, four, eight, or sixteen cells formed in the development of many animals. Further, some of the facts of regeneration and budding show that the ectoderm is on occasion quite capable of forming endoderm and mesoderm. The anomalies also which exist in the formation of the layers in vertebrates are patent to every student, while research on cell-lineages in the

invertebrates has shown most diverse histories. So far as an independent observer can judge, the trend of modern research is to show that embryology gives no sure evidence of the homology of the germ layers.

J. STANLEY GARDINER.

Cambridge, May 22.

PERHAPS the reviewer should have made it plainer that the difficulty he stated at the top of p. 303 is not admitted by those morphologists who have ceased to believe that the germ layers afford any criterion of homology. He simply expressed his conviction, which he shares with many, that it does count for something which layer a structure develops from. He said that he was not prepared to follow Dr. Gaskell in throwing the germ-layer theory overboard, and that this made criticism difficult, a discussion of the author's dismissal of the theory being impossible in an article which appreciation of the book discussed had already expanded far beyond the limits prescribed.

THE REVIEWER.

Gaskell's "Origin of Vertebrates."

IN the review of my book on the "Origin of Vertebrates," which appeared in NATURE of May 13, the reviewer, discussing my theory that the vertebrate central nervous system represents the conjoint central nervous system and alimentary canal of an arthropod, says "this view lands us in difficulties which seem to us as insuperable as those of the reversal hypothesis seem to the author." He then proceeds to say, "we want to know, for instance, where the arthropod's mesenteron has gone?" This is the "only one of the most obvious difficulties" of which he makes mention. I wish he had mentioned more, as I am most anxious to have all the difficulties of my theory pointed out and fully discussed.

He will find in my paper in the *Quarterly Journal of Microscopical Science*, vol. xxxi., that I look upon the peculiar tissue which fills up the space between the brain and the cranial wall in Ammocetes as the remains of the corresponding tissue which surrounds the brain of such animals as *Limulus*; in other words, this tissue represents the mass of generative glands and so-called liver-tissues in these animals. This so-called liver, together with its duct or ducts leading into the gut, constitutes the mesenteron, and the most distinct remnant of such mesenteron in Ammocetes is the tube, called by me the old liver-tube, which leads from the fourth ventricle to terminate on the surface of the brain at the conus post-commissuralis, as is shown in a series of sections reproduced in that paper. In my book I have discussed this vestige of the arthropod's mesenteron on pp. 209, 210, 211, chapter v., but have not re-published the series of sections given in my former paper. In the summary of chapter v. I have not mentioned this question of the vestiges of the arthropod's liver, as it was not especially concerned with the subject-matter of chapter v.; possibly that is the reason why it has failed to attract the notice of the reviewer.

The reviewer says that "the tubular appearance of the vertebrate central nervous system appears to some an unimportant architectural consequence of the mode of development from a medullary groove," and also in reply to my argument "that the extraordinary resemblance between the structure and arrangement of the central nervous systems of vertebrates and arthropods is against the view of their phyletic distinctness," he asserts that, "given segmentation in two distinct types, we naturally expect similarity in the general plan of innervation." But the whole point is that the tube is not a simple tube such as would be formed by the coming together of medullary folds, but one, which invariably possesses a ventral diverticulum, the tube of the infundibulum, situated in exactly the position of the arthropod oesophagus, on the view of the phyletic relationship between the central nervous systems of the arthropod and the vertebrate.

The reviewer seems to think that I lay too much stress on Ammocetes and ignore Amphioxus and the tunicates, and also that I am inclined to flit a little from type to type, making use of arachnids, *Peripatus*, and annelids when the Palæostraca are insufficient. I thought I had made it clear in my book that my object was to find out,