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 olfactive 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 olfactive 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 olfactive 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 intramolecular 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 Ammocœtes 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 Ammocœtes 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 œsophagus, 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 Ammocœtes 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,