ing to polar globules, thrown off from the germ-cells before they develop into embryos; third, the 'residual nuclei' of the germogens set free, as the final event in the history of infusorigens. The infusorigen is a group of cells, consisting, at one period, of a peripheral layer of cells partially enveloping a large central cell. Its development from a single cell by a process of cleavage, and the epibolic growth of its peripheral layer, give ground for believing that it passes through a gastrula stage. In diphygenic individuals the germ-cells are different for the two kinds of embryos. The first to appear, one or two at a time, are the large germ-cells of the infusoriform embryos. After these embryos escape, there remain in the parent-body small cells, which multiply until they fill the greater portion of the axial cell, and eventually give rise to vermiform embryos. The difference between developmental division (cleavage) and multiplicative division of cells is here brought in striking contrast. No definite evidence of fecundation has been obtained, but it perhaps occurs with one form of embryo. In the development of the vermiform embryo, karyokinesis splits the germ-cell into two unequal parts. Then follows a three, and next a four celled stage, in which three cells form a cap over the fourth. This leads to a gastrula, in which a single entoderm-cell is enveloped by a small number of ectoderm-cells. The blastopore closes, and the multiplication of cells at this pole soon leads to the pyriform embryo, of which the pointed end is the blastoporal region; while the rounded end corresponds to the future cephalic pole. In this stage the first germ-cell appears at the hind end of the entoderm; the second germ-cell, at the anterior end; and from these two arise the other germ-cells. There is, therefore, a triploblastic stage, if we regard the two germ-cells as representing the mesoderm.

It may be added, that important errors of van Beneden have been corrected by Whitman, whose article is one of unusual interest and merit. As to the relationship of the dicyemids, Whitman says, "I see no good reason for doubting the general opinion that they are plathelminths, degraded by parasitism. Whether they, and their allies the Orthonectidae, have descended from ancestors represented now by such forms as Dinophilus, or from the Trematoda, is a question which further investigations must decide."

C. S. MINOT.

TEMPERATURE AND ICE OF THE BAVARIAN LAKES.

AFTER an account of temperature observations on Swiss lakes by earlier observers, as Brunner and Fischer, Simony and Forel, A. Geistbeck (*Ausland*, 1882, 961, 1006) gives a detailed tabulated statement of his observations during 1881 on sixteen Bavarian lakes, showing the following results. As to variation with depth, the first six metres are almost constant; between six and eighteen metres there is a rapid cooling; then, to fifty metres, a slow decrease; and, below this, an almost constant temperature of a little less than 5° C. Daily variation is distinct to six metres, but ends at eighteen. Annual variation is reduced to from 0.2° to 0.9° at the bottom of the deeper lakes. Two groups are noted. The warm lakes, with an average temperature of 7.3° to 17°, are less than one hundred metres deep, their bottom temperature is below 5°, and they have a decided annual variation through their entire depth. The cold lakes, Königs, Starnberger, Walchen, and Achen, are from 115 to 196 met. deep, and, below fifty metres, are always cooler than 5°, with an average temperature of 5.2° to 5.6°: these have, therefore, a great volume of cold water even in midsummer, and a slow and small annual temperature range. The cause of this difference is seen partly in the depth of the lakes, and further in the relation of lake-surface to drainage-area, which, in the cold lakes, averages 1 to 10, and, in the warm, 1 to 30. Exceptions, here and elsewhere, to the rule of depth, are Barm (31.5 met. deep), Gosau (63), and Toplitz (105), which belong under the cold group; for, in spite of their moderate depth, they are well protected by steep shores from warming by sun and wind. On the other hand, Geneva (334) and Gmundener (190) approach the warm group, presumably on account of their large drainage-area. Certain small mountain-lakes, fed mostly by springs, show a relatively low summer and high winter temperature. Form of the bottom, and nearness to entering-streams, have strong control over the water's warmth. The lacustrine flora and fauna are determined chiefly by temperature and light. Reeds and algae are common along shallow shores, but all rooted plants end at a depth of twelve metres. The littoral molluscan and crustacean fauna disappears at twenty metres. In deeper water there is a special 'pelagic' fauna. (In this connection, see Forel, La faune pélagique des lacs d'eau douce, — *Arch. sc. phys. nat.*, viii. 1882, 230.)

The lake temperatures fall quickly in the autumn by circulation, but rise slowly in the spring by conduction and wave action. In winter a temperature lower than that of maximum density penetrates to a considerable depth: less than 3° has been found at forty metres. Ice forms first on the shallows along the shore, and spreads outward. The high lakes freeze every year, sometimes as early as October or September; the larger lower lakes, at later dates and more seldom. Walchen has frozen over only three times in this century; Constance, seven times since 1277; Gmundener, five times in the last four hundred years. In the severe winter of 1879–80 Tegern closed on Dec. 21; Zurich, in the middle of January; Walchen, on Feb. 3; and Constance and Gmundener, on the 6th. Changes of temperature produce long cracks in the ice, so characteristic as to have local names — *lehnen*, *schübe*, *wunen*, *frageln* — on the different lakes. Further description is given of the thickness and color of the ice, and certain peculiarities in the freezing of some of the lakes.

W. M. DAVIS.

LETTERS TO THE EDITOR.

Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

Zoölogical 'regions.'

MY attention has been drawn, by a notice in one of the last numbers of SCIENCE, to what seems to me to be sources of error. I refer to the determination of zoölogical regions by percentage calculations, and the idea that regions should have a certain amount of numerical equivalence. This seems to be an artificial and hence fallacious method of dealing with the subject, engendered by the lack of a proper conception of the matter under consideration. No definition or description of a 'region,' or synonymous word, can be found in any of the leading works on zoogeography; but, if we put two and two together, an idea can be formed which will, I hope, help solve some mooted questions.

Regions are known to differ in the kinds of animals occupying them, as well as in location. All, or all but one, are geographically very distinct; and all are well

separated in regard to animal distribution. This latter point goes far toward being the sole cause of regions. Any large mass of land separated from the rest of the world will, in the course of time, become inhabited by a peculiar set of animals, and obtain a comparative balance or stability of life. Thus a number of species are evolved which forms a sort of compound whole, — the life of a region. So a region may be defined as the area occupied by a peculiar grouping of animals which are isolated from the life of the rest of the world; the word 'peculiar' referring to the animals as a whole, and the isolation as of a limited and not absolute degree.

Accepting the above definition, the world can be conceived of as divided into regions, which, if the land and sea remained at rest, would be permanent, but constantly growing more and more distinct. But the land is not permanent. While the main mass is a fixture, minor changes occur, which join and separate the continents. As soon as two lands are joined, unless some other very powerful barrier exists, the life of the two at once begins to blend. The more potent kinds survive, while the weaker die out. The first, together with the life modified by the new conditions (new species evolved), in the course of time form a single region. On the other hand, if a land become divided into two, the reverse takes place, and two regions are formed. To me the palearctic and nearctic regions seem to offer illustrations of both these processes; the connection for life having been made and broken between the old and new worlds, probably by means of Asia, more than once. At present it is broken; and the nearctic and palearctic regions are formed or forming from a previous circumpolar region. With the tentative definition given here, the two are regions, since they do not form a group, and are separated. No lack of percentage differences can make the life of the two regions closely related: a change in one does not necessitate a change in the other. This also answers the circumpolar question: the resemblance in zones is due, first, to the imperfect obliteration of the old circumpolar region; and, secondly, to the fact that some of the forms which inhabited it have been driven down into the southward-pointing peninsulas, where the conditions of their life are easier. According to this definition, Madagascar should be regarded as the remains of a fading region, rather than a part of the Ethiopian. The resemblance between Africa and India is due to a southward migration which occurred not so long ago, very likely on account of the ice age, from a northern central point.

The above crude suggestions would seem sufficient to show that regions are more than numerical relations, and have an evolution of their own.

J. AMORY JEFFRIES.

Panther Creek coal-basin.

I have just read your review of the Panther Creek atlas, in SCIENCE, No. 11, and my attention has been directed to what I consider a very just and proper criticism of two special features of the atlas sheets: 1°. The discordant scales of the mine (800′=1″) and topographical (1600′ = 1″) sheets. 2°. The use of the magnetic instead of the true meridian. As a geological critic, I should be disposed to boldly condemn what you have referred to as merely misfortunes. After an association of nine years with Professor Lesley on the Pennsylvania state survey, I am convinced, that, in the successful conduct of such a survey, it is quite impracticable to attempt to attain a purely technical and systematic standard of work. All that can be done is to approach as near as possible to such a standard, while meeting the practical demands for geological results, to aid in the economical exploitation of our mineral resources. This latter is what has popularized the work of the Pennsylvania survey, and accounts for its uninterrupted continuance with liberal appropriations for a state survey, since 1874.

The published results of the survey so far relate mostly to topographical, geotectonic, and stratigraphical geology in their economical bearings, with the exception of two volumes on paleobotany. Had any other plan than that of Professor Lesley's, which he has so efficiently carried out, been instituted, the survey would never have been so liberally supported by our state legislature, and probably would have been discontinued several years ago. The important thing in a state survey is to do the best we can. If we attempt too much, we fail in all.

In regard to the discordant scales and magnetic meridian, I would say: 1°. That the publication committee of the board of commissioners has never before authorized the printing of *general maps* on a scale larger than 1600′=1″. This scale was found quite too small for the anthracite-mine sheets, and it was only after the most careful consideration on the part of the committee that a scale of 800′=1″ was adopted for the mine sheets. The smaller scale was unfortunately adhered to for the topographical sheets, on account of the cost of publication. 2°. In the Panther Creek basin, the magnetic meridian of 1869 is always used in all surveys; and the block-lines referred to this meridian on the atlas sheets have been similarly placed on all the large working mine maps. In this form the sheets are of much greater practical value for ready reference. Had the publication of this atlas been delayed until the completion of the astronomical determinations of the survey in this locality, we should probably not have obtained an additional appropriation to continue the survey, which we now feel assured of receiving.

CHARLES A. ASHBURNER,
Geologist in charge.

Philadelphia, April 21, 1883.

Crayfish.

In August, 1882, while in Fairmount Park, Philadelphia, I found a crayfish in a brook emptying into the Wissahickon Creek. It had its under parts covered with young crayfish about one-eighth of an inch long.

Professor Huxley says that the English species, Astacus fluviatilis, lays eggs in May and June, and the young leave the female in a few days; but the young staid ten days with the female after I found them. There seems to be a difference in their habits in this respect. Last Friday, April 6, I found a female crayfish with young ones clinging to it, which I caught; and a friend now has it in a tank. Do crayfish lay eggs both early in the spring and late in the summer?

RICHARD M. ABBOTT.

Trenton, N.J.

[The writer of the above is eleven years of age. — ED.]

Marking geodetic stations.

The writer of the article in SCIENCE of April 13, 1883, p. 269, in referring to the method of marking the geodetic stations in the N. Y. state survey, makes the statement that the U. S. coast survey stations are indicated 'by no surface-mark whatever,' trusting entirely to the underground-mark for the preservation of the station. The writer has, doubtless, been misled by visiting a station from which the surface-marks have been removed by curious or malicious persons. In the coast survey the greatest stress is