The animals, however, prove after all to be hermaphrodites. Since the last careful study of *Apus cancriformis*, as a whole, by Zaddach in 1841 (the works of Ray Lankester and others deal only with special points), new methods of research have been introduced into our laboratories which reveal details not easily discoverable by the older methods. Zaddach's figures of the ovaries and testes of Apus are thus naturally somewhat deficient—as deficient, indeed, as the best work we can do to-day will, we hope, be found to be fifty years hence.

As already announced in a preliminary note,[1] published in the current number of the *Jenaische Zeitschrift für Naturwissenschaft* (Band xxv., N.F. xviii.), a small species of Apus kindly handed me by Prof. Kükenthal, and presumably *Lepidurus glacialis*, Kroyer, proved on examination to be hermaphrodite. The specimens were found in East Spitzbergen during the expedition sent thither by the Bremen Geographical Society in 1889, under the conduct of Prof. Kükenthal. The species seemed to be new, as it did not agree with any of the descriptions of *Lepidurus glacialis*; not only was the whole animal much smaller, but its caudal plate was much smaller and not notched at the tip, and, most important of all, it possessed well-developed second antennæ, which have till now never been found in *Lepidurus glacialis* (Huxley's "Anatomy of the Invertebrata," p. 243). The new species, however, proved to be identical with the *L.* "*glacialis*" brought back from West Spitzbergen by Prof. Nathorst, specimens of which were kindly sent me by Prof. Leché, of Stockholm. It thus at first seemed likely that there were two species of Lepidurus in the Arctic regions—a *Lepidurus glacialis* and a *Lepidurus spitzbergensis*. I am now, however, inclined to look upon *L. spitzbergensis* as a stunted variety of *L. glacialis*, or, rather, as a precociously ripe young stage. My reasons for considering it merely a variety of *L. glacialis* are as follows:—

(1) I have succeeded in finding very distinct second antennæ in a large specimen of *L. glacialis*, from Greenland, kindly sent me by the Rev. Canon Norman, so that this supposed difference does not exist. (2) On examination of the genital tube, the sperm-forming centres are found in identically the same place in the two species, viz. at the posterior end of the genital tube, both, in this respect, differing from the other Apodidæ I have as yet had at my disposal to examine. (3) The other two differences—the small size of *L. spitzbergensis* and the undeveloped state of its caudal plate—are, on this supposition, easily accounted for.

We thus have, instead of two Arctic species of Lepidurus, a fully-developed *L glacialis*, Kroyer, presumably in the warmer regions, and a small, precociously developed variety from the colder and more northerly regions—*L. glacialis* var. *spitzbergensis*.

Whether the variety is permanent or not, I have no means of deciding. It is interesting to find that Packard's measurements for *L. glacialis* make it smaller than *L. spitzbergensis* ("Monograph of the North American Phyllopoda," 1883), which shows that *L. glacialis* may be dwarfed by the unfavourable environment. As Packard's drawings are (judging from the development of the caudal plate) of the fully-developed animal, this leads one to think that perhaps, in rather longer summers, the Spitzbergen variety may develop into the typical *L. glacialis* without any great increase of size.

In my preliminary notice announcing the hermaphroditism of *L. spitzbergensis*, knowing how much the reproduction of the Apodidæ had been discussed, I ventured to assert that in all probability the other species of the genus would also prove on closer examination to be hermaphrodite. As above stated, I found the sperm-forming centres in *L. glacialis* in identically the same position as in the Spitzbergen variety. By the kindness of Prof. Möbius, the Director of the new Berlin Museum, and of the Rev. Canon Norman, I have also been able to examine *Apus cancriformis* and *Lepidurus productus*. In both these the sperm-forming centres were found scattered here and there among the rich branchings of the segmental diverticula of the genital tube. They occur either at the tips of such branches, where the eggs ordinarily develop, or as slight lateral bulgings of the same. In all cases the spermatogenesis is the same, the epithelium breaking up into sperm cells; these escape into the lumen of the tube, and are found in considerable numbers near the genital aperture, where the epithelial lining of the tube is hardly demonstrable, the walls of the tube consisting of a fibrous membrane, in the folds of which the sperm-cells lurk. The eggs are then fertilized as they stretch this membrane in passing out into the egg pouch. The whole richly-branched reproductive organ, with the eggs developing at the tips of the branches, and with here and there a testis, strongly reminds one of a monœcious plant, self-contained, and able to dispense with pollen from without.

I reserve the drawings and the more detailed description of the reproductive organs of the different species for a short comparative study of the Apodidæ which I hope soon to have ready for the press. By way of caution, however, I should here add that small yellowish sacs filled with minute cells occur here and there among the developing eggs. These must not be mistaken for the testes. They are the loci of discharged eggs, and the minute cells are the epithelium cells dislodged by the shrinking of the membrane of the genital tube, which is stretched some 100-fold by the ripening eggs.

The origin of this secondary hermaphroditism is not far to seek; it is clearly a protection against isolation, as in the case of the Cirripedia and certain parasitic Isopoda. The manner of life of all these animals is such that they are always in danger of being cut off from their kind; they would thus die out unless able to reproduce either parthenogenetically or by means of self-fertilization.

Some species of Cirripedia, as is well known, have dwarf males, the last remains of the original separation of the sexes. As already mentioned, small males of *Apus cancriformis* are sometimes found. Twelve finds of *A. cancriformis* and *L. productus*, recorded by Gerstaecker, give 4458 "females" (*i.e.* hermaphrodites) to 378 males; while sixteen finds, numbering 10,000 individuals, did not contain a single male. I have found no record of a male *L. glacialis*, and none of the twenty odd specimens of the Spitzbergen variety I have as yet examined have been males. It is probable that throughout the whole genus self-fertilization is taking the place of cross-fertilization, but that some species have gone further than others in dispensing with males. Two species, for instance, *L. couesii*, Packard, and *L. macrurus*, Lilljeborg, are reported to have more males than "females (?)," but the finds in these cases seem hardly large enough to allow us to judge; it may have been purely accidental that more males than "females" were caught.

The males of the Apodidæ, with the doubtful exception of *L. productus*, seem to be smaller than the hermaphrodites, otherwise there is no very pronounced sexual dimorphism as there is among the Cirripedia. We are perhaps justified in concluding from this that the hermaphroditism of the Cirripedia is of much older date than that of the Apodidæ. No comparison is here, however, possible, since the two have nothing further in common beyond the fact that they are both hermaphrodite, and that this hermaphroditism is in both cases an adaptation against extermination through too wide dispersion of the individuals.

Jena, January 30. H. BERNARD.

[1] In this note, by an oversight, I stated that Schaeffer concluded the animals to be parthenogenetic.

Stereoscopic Astronomy.

THE note on this subject in NATURE of January 22 (p. 269), regarding the perception of stereoscopic effect on examining properly-arranged photographs of Jupiter, recalls an observation which I published in one of a series of articles on physiological optics that appeared in the *American Journal of Science* in 1881 and 1882.

By taking advantage of the moon's librations, Mr. Lewis M. Rutherfurd, of New York, produced more than twenty years ago an excellent stereograph of this heavenly body. In examining this I found it possible to observe not merely the general convexity or concavity, according to the mode of stereoscopic combination, but also the inequalities upon the lunar surface. In an American text-book I have found the statement that Mr. Warren De la Rue had succeeded in obtaining a stereograph of the sun, from which, by stereoscopic vision, the ridges of the faculæ could be perceived in sharp relief. On application to Mr. De la Rue for a copy of this stereograph, I was disappointed to learn that the negative had, unfortunately, been destroyed, and hence no copies were attainable.

My own observations may be given by quoting from the article published in the *American Journal of Science* for May 1882. "On the stereograph of the moon, to which reference has been made, the elevation of mountain ranges and solitary peaks, and even the inequalities of the supposed dead sea bottoms can be clearly seen. The crater Copernicus, and the lunar Apennines, stand forth particularly boldly, and the ridge that divides the bed of the heart-shaped Sea of Serenity can be easily traced. Anyone who has undertaken the preparation of a stereograph with the pencil or pen, knows how very difficult it is to avoid the production of roughness in the combined image at places where smoothness is desired. No two impressions from the same type can be taken that will not present some inequalities when stereoscopically examined, and no two groups of type representing the same sentence can be so accurately adjusted as not to betray imperfection when subjected to this searching test."

For this statement regarding the moon, I was subsequently criticized by an English writer, well known in astronomical circles, who considered it to be extravagant. The test furnished by the photographs of Jupiter is probably even more delicate than that afforded by photographs of the moon's minor inequalities of surface. The observation of "W. J. H." is certainly very interesting. By experiments made in 1882 I found that a plane binocular image became noticeably convex or concave when the pair of diagrams under examination were so disposed as to produce an angular retinal displacement of only 47" (*Philosophical Magazine*, October 1882). By comparing the photographs of celestial objects whose distance is known, it may be possible yet to show that the minimum displacement measured in 1882 is really not quite a minimum. W. LE CONTE STEVENS.

22 Universitätsstrasse, Strassburg, Germany, February 4.

Notable Palæolithic Implement.

DURING the last five or six years I have lived at Dunstable, and many persons in the neighbourhood now know that I notice old things a little. The consequence is that various objects are now and again presented to me for purchase. These things are mostly no good—common fossils, pieces of "petrified water," shells, coins of the Georges, &c., but at times something worth notice comes to hand.

Late last autumn a number of stones of no value were brought to me; amongst them was a good, flattish, sub-triangular, Palæolithic flint implement which had been picked up in 1830 by a farmer named William Gutteridge on Dallow Farm, near Luton—the late Mr. Gutteridge's own land. The implement had been preserved by the farmer as a curious natural stone, and he had affixed a label to it with locality and date. The person of whom I secured the stone knew nothing of stone implements. I soon ascertained the name and date to be correct from a relative of the late William Gutteridge. In 1830 the Gutteridges had held Dallow Farm for over 150 years.

Dallow Farm is in the valley of the Lea, and three-quarters of a mile west of the river at Luton. The ground is, I think, about 50 feet above the Lea, and from 400 to 450 feet above the Ordnance datum, but the heights on the large-scale Ordnance map are here insufficient. I have never found a Palæolithic implement at Luton, but I have picked up a few drift flakes there, and found a good number of Palæolithic implements a few miles off.

The Dallow Farm Palæolithic tool was found by Mr. Gutteridge seventeen years before M. Boucher de Perthes published his discoveries in France (1847), and eleven or twelve years before he began to notice such objects.

The famous Gray's Inn implement was found in 1690; Mr. Frere's discoveries were made at Hoxne in 1800; the Dallow Farm implement comes next in 1830; and the Godalming implement (Evans, "Stone Implements," p. 529) about 1842.

Dunstable. WORTHINGTON G. SMITH.

Stereom.

AMONG wants long felt, at least by animal morphologists, is some word that shall express for Invertebrata the idea that the word *bone* expresses for Vertebrata. Words such as *skeleton*, *shell*, *test*, and *carapace* express the whole structure, not the substance of which it is made. Words such as *nacre* and *stereoplasm* express some particular form of hard substance strictly defined from a physical or morphological stand-point. *Sclerenchyma* is the only word that has yet been used in anything like the required sense; but that is confined to corals, and, from its affinity with *cœnenchyma* and the like, it is well that it should be so. Driven back on cumbrous periphrases, I therefore venture to suggest the adoption of the word *Stereom* (στερέωμα, that which has been made solid). This word was used by Aristotle ("De Anim. Part.," ii. 9) for the hard as opposed to the soft tissues of the body, and may, for the purposes of modern science, be thus defined: any hard calcareous tissue forming skeletal structures in Metazoa Invertebrata, and in Protozoa. F. A. BATHER.

February 9.

Destruction of Fish by Frost.

IN regard to Prof. Bonney's letter of January 26 (p. 295), I would ask whether the fish were not killed by want of air due to the stagnancy of the water in the canal?

The moat here abounds in fish, and several holes were kept open for their sakes during the frost. The first partial thaw set our land-drains running. Where one of these began to pour a little water into the moat, though no fish had been visible since summer, now the largest pike and carp were seen crowding to the aperture, seeming to be gasping for air, and seeking the fresh flow. When the frost departed, scarce half-a-dozen fish—all small—were found dead. It would seem, therefore, that a very slight flow of fresh water would suffice to save fish from death. But this can seldom be wanting in any natural body of water, for few are even the tarns into which no brook runs. So such a cause of destruction can seldom have acted on a scale visible to a geological eye. E. HILL.

The Rectory, Cockfield, Suffolk.

A DEDUCTION FROM THE GASEOUS THEORY OF SOLUTION.[1]

BEFORE passing on, let me briefly recapitulate the chief points in Van't Hoff's gaseous theory of solution and the experimental laws on which it it is based.

(1) In every simple solution the dissolved substance may be regarded as distributed throughout the whole bulk of the solution. Its total volume is therefore that of the solution, the solvent playing the part of so much space; and its specific volume is the volume of that quantity of the solution which contains 1 gramme of the substance. To avoid confusion, it is best to speak of this as the *specific solution volume* (v) of the substance. It is obviously in inverse ratio to the concentration.

(2) In every simple solution the dissolved substance exerts a definite *osmotic pressure* (p). This is normally independent of the nature of the solvent. It varies inversely as the specific solution volume (or directly as the concentration), and directly as the absolute temperature (T). We may then write for solutions, as we do for gases, the equation $p \cdot v = r \cdot \mathrm{T}$, where p and v have their specialized meanings, and r is a constant for each soluble substance.

(3) The *molecular solution volume* of all dissolved substances is the same if they are compared at the same temperature and osmotic pressure. If m be the molecular weight, $m \cdot v = \mathrm{V}$ is the molecular solution volume; and we can now write, as we do for gases, $p \cdot \mathrm{V} = \mathrm{R} \cdot \mathrm{T}$, where R is the same constant for all substances.

(4) This constant R has the same value when the formula is applied to the dissolved state as when it is applied to the gaseous state itself.

(5) The gaseous laws, as I have stated them, are not absolutely true for dissolved matter in all circumstances. Dissociation often occurs, as it may occur in the process of vaporization, thus causing apparent exceptions. But apart from this there are and must be variations from the laws in the case of solutions of great concentration, just as there are in the case of gases and vapours of great concentration—for instance, in the neighbourhood of the critical point.

I wish now to ask your attention more particularly to

[1] Part of an address delivered by Prof. Orme Masson as President of Section B of the Australasian Association for the Advancement of Science, January 1891.