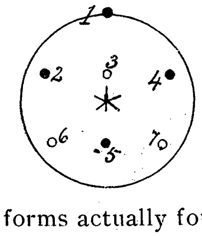
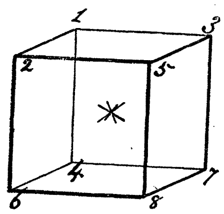
remaining three at the corners of a lower triangle alternate with the first. These positions are here assumed only for the sake of the symmetry that then appears with reference to the position 1. A very small change of positions will suffice to make the same symmetrical arrangement with reference to any of the other positions 2, 3, 4, 5, 6, 7; and it is assumed that so much motion may take place that only one form of the molecule MAB_6 is possible. Under such conditions the prediction of isomeric forms is largely guess work; but the following, which include most of the forms actually found, seem probable:



$$M^{vii} (2) \left\{\begin{array}{l} A_2 B_5 \\ AB_5A. \end{array}\right. \qquad M^{vii} (3) \left\{\begin{array}{l} B_4A_3 \\ BA_3B_3 \\ AB_4A_2. \end{array}\right.$$

5. Oktavalent atoms,—Cube.



$$M^{viii} (3) \left\{\begin{array}{l} A_2 B_6 \\ AB_5AB \\ AB_6A. \end{array}\right.$$

$$M^{viii} (3) \left\{\begin{array}{l} A_3B_5 \\ A_2 B_5 A \\ AB_3ABAB. \end{array}\right.$$

$$M^{viii} (7) \left\{\begin{array}{l} A_4B_4 \\ A_3BAB_3 \\ AB_3A_3B \\ A_2 B_2 AB_2 A \\ A_2 B_3ABA \\ A_3B_4A \\ A_2 B_4A_2. \end{array}\right.$$

$$M^{viii} (3) \left\{\begin{array}{l} ABC_6 \\ AC_3BC_3 \\ AB_6B. \end{array}\right.$$

$$M^{viii} (22) \left\{\begin{array}{l} A_2 B_2 C_4 \\ A_2 BCBC_3 \\ A_2 BC_2 BC_2 \\ A_2 BC_3 BC \\ A_2 BC_4B \\ A_2 CB_2 C_3 \\ A_2 C_2 BCBC \\ A_2 C_2 BC_2 B \\ A_2 C_4B_2 \\ ABC_2 ABC_2 \\ AC_3 ABCBB \\ AC_3 ACB_2 \\ AB_2 CAC_3 \\ AC_3 AB_2 C \\ AC_2 BAC_2 B \\ ABCBAC_3 \\ ACB_2 AC_3 \\ ACBCABC_2 \\ ABC_2 BC_2 A \\ ABC_3 BCA \\ AB_2 C_4A \\ ABC_4BA. \end{array}\right.$$

$$M^{viii} (7) \left\{\begin{array}{l} AB_2 C_5 \\ ABC_2 BC_3 \\ ABC_3 BC_2 \\ ABC_4BC \\ ABC_5B \\ AC_3 B_2 C_2 \\ AC_5B_2 . \end{array}\right.$$

$$M^{viii} (13) \left\{\begin{array}{l} AB_3 C_4 \\ AB_2 CBC_3 \\ AB_2 C_2 BC \\ AB_2 C_3 BC \\ AB_2 C_4B \\ ABC_2 B_2 C_2 \\ ABC_2 BCBC \\ ABC_3 B_2 C \\ AC_3 B_3 C \\ AC_3 B_2 CB \\ AC_2 B_2 C_2 B \\ AC_2 BCBCB \\ AC_2 BC_2 B_2 . \end{array}\right.$$

$$M^{viii} (24) \left\{\begin{array}{l} A_2 B_3 C_3 \\ A_2 B_2 CBC_2 \\ A_2 BC_3 B_2 \\ A_2 CBC_2 B_2 \\ A_2 B_2 C_2 BC \\ A_2 C_2 B_2 BC \\ A_2 BCBCBC \\ A_2 CBCB_2 C \\ A_2 BC_2 B_2 C \\ A_2 CB_2 CBC \\ AB_3 AC_3 \\ ACB_2 ACBC \\ ACB_2 ABC_2 \\ ABCBACBC \\ ABCBABC_2 \\ AC_2 BAB_2 C \\ ACB_2 AC_2 B \\ AC_2 BACB_2 \\ ABC_2 AB_2 C \\ AB_2 CACB_2 \\ AB_3 C_3 A \\ AB_2 CBC_2 A \\ AB_2 C_2 BCA \\ AB_2 C_3 BA. \end{array}\right.$$

The possible isomers of more complex molecules of this class are very numerous. But there is no advantage in writing them out since they are not found in practice.

When divalent or trivalent atoms or groups replace two or three monovalent atoms in the preceding formulæ, the possible forms are in all probability restricted to those in which the replaced atoms are adjacent to one another. The molecule $P^v NO$, for example, is of the form $M^v A_2 B_3$, but only one of the three isomeric forms given in the table is in this case possible.

If there were examples of still higher valence there would be no regular arrangement possible until twelve atoms were arranged about the central atom, and these twelve would crowd one another or be necessarily so much closer to one another than to the central atom that the form would be unstable. The same cause of instability would of course prevent the formation of molecules in which more than twelve atoms are grouped about one. We have then a plausible reason why valence should not exceed the number eight.

A possible explanation of the fact that elements having odd valence remain odd and those having even valence remain even may be found in the supposition that atoms whose valence is even are symmetrical while those having odd valence are asymmetrical,—that is to say, the latter have their centre of attractive force for other atoms not coincident with their centre of gravity; and on the farther supposition that in order to form a stable compound the atoms must form a more or less symmetrical arrangement about the central atom. An atom having odd valence will then attract one other atom and form with it a symmetrical molecule. A farther addition of one atom destroys the symmetry. The addition of a third restores it. So that if symmetry is a necessary condition of stability the valence must increase by twos. On this hypothesis the stereochemical structure of molecules whose central atom has odd valence will be entirely different from what is represented in the preceding diagrams. The anomaly of chlorine, whose most stable oxygen-acid (per-chloric acid) is the one whose molecule appears most irregular in the above diagrams, disappears, and the arrangement required for the molecule of perchloric acid is perfectly regular. The arrangements for the odd valences, in fact, become the same as for the even valences of the next higher order with the omission of one point whose place is supposed to be (in part) occupied by the asymmetry of the central atom. In some cases the number of isomers possible on this theory differs from the number given in the preceding table. Such cases may decide which (if either) of the views here presented is preferable.

BOOK-REVIEWS.

Investigations on Microscopic Foams and on Protoplasm: Experiments and Observations directed towards a solution of the question of the physical conditions of the phenomena of life. By O. Bütschli, Professor of Zoology in the University of Heidelberg. Authorized Translation, by E. A. Minchin, B.A. (Oxon.), Fellow of Merton College, Oxford. London, Adam and Charles Black, 1894. 1 vol., 8vo, xvi, 379 p., with xii plates.

THIS is an attempt to determine the character of protoplasm, by analogical reasoning from the microscopical appearance and behavior, in water, of drops of oil containing soluble substances, such as salt or sugar, which, by their attraction for the water, cause it to enter into the oil and produce a solution which fills and expands the cavities previously occupied by the salt or other substance, thus converting the oil into a fine froth.

Although the author formally divides his work into only two parts, viz., I. that relating to the structure of Oil Foams and II. that relating to the structure of Protoplasm, it may be more conveniently considered as consisting of four divisions,—1. treating of the author's experiments with microscopic foams, 2. recounting his observations upon living protoplasm, 3. reviewing all known theories of protoplasmic structures, and 4. giving Professor Bütschli's own conclusions as to the physical nature of protoplasm.

In the words of the translator, "protoplasm is conceived of in this work as having the structure of a froth or foam in which minute droplets of a watery liquid take the place of air in the bubbles of an ordinary foam." The author applies to this formation a term which means pretty nearly "honeycomb structure," but which Mr. Minchin finds it more convenient to translate as *alveolus*, using as the related adjective the word *alveolar*.

The method of experimentation and comparison made use of by the author of this work is of course not wholly new, and this is not his first presentation of the subject. As far back as 1878 Professor Bütschli broached the general idea worked out in the present book, and although he has modified his views from time to time to meet the criticisms of fellow-investigators, or to accommodate them to facts brought to light by his own observations, this may be regarded as in the main a final defence of his original thesis,—namely, that protoplasm, while generally presenting an alveolar arrangement, is in itself structureless, and that its manifestations of activity are explainable on purely physical grounds.

The aims of his experiments have been, first, to produce artificial foams of the fineness of those believed to exist in protoplasm and comparable with the latter; second, to induce in his artificial plastides the phenomena of streaming, locomotion, and growth by intussusception; and, third, to show that the structure of living protoplasm and its so-called vital movements are similar in character to those produced by his methods. In pursuance of his first object he has undoubtedly found a simple method of making oil froths with a meshwork as fine as modern optical appliances can render visible, and to this extent he has matched the reticular appearances brought to light during recent years in vegetable and animal protoplasm. But, unfortunately, the microscope has not yet reached that degree of perfection, even with its latest apochromatic objectives and compensating oculars, which will enable us to speak with entire positiveness of the real form of any of the smallest structures we are able to discern, and so, with reference both to these microscopical foams and to the reticulated protoplasm of living organisms, we cannot at present depend entirely on sight and wholly dispense with imagination. In his alveolar oil-drops our author properly assumes the right to argue from the larger cavities down to the very smallest, and to assume from his employment of a structureless colloid that the boundary walls and the interconnecting threads are nothing but films of oil in different degrees of tension. But in the case of protoplasm we may fairly question whether he is thus far justified in carrying his analogy beyond the bound of mere superficial resemblance.

In pursuance of his second aim, Professor Bütschli has found that when the exchange takes place between the contents of the oil vacuoles and the surrounding water, a streaming movement is set up, the currents of which, both within and without the drop, may be made visible by mixing India ink or other coloring matter with the fluids. He has also observed that at this time the drop, which is compressed to a thin layer between glasses for microscopical examination, is likely to "creep somewhat rapidly backwards and forwards under the cover glass," a movement which, he claims, is like that of a simple amoeba. The progression is in a line with the streaming motion already referred to;—that is to say, there is a point upon the surface of the drop at which the interchange between its contents and its environing liquid is most active, towards which point the internal currents converge, and from which the external currents diverge, thus creating an anterior pole to the drop, and the author himself expresses the belief that "the creeping progressive movements are without doubt in connection with such streamings." In strongly pressed drops several centres of extension currents may arise, and, as the oil then spreads in several directions at once, there is created the appearance of "pseudopodium-like processes," but "such drops show, as was to be expected, no actual forward movement as a whole." The author remarks that "not infrequently a drop of the kind just described is observed to run towards one of the strips of cover-glass employed as supports," which plainly suggests that all of his "creeping" movements may be the result of capillary attraction, when they are not produced by the mere pressure of his compressorium.

As to "growth," Professor Bütschli is not able to present any proof beyond the simple swelling of the drops during the first imbibition of water and solution of the salt crystals entangled in the oil, by which the conversion of the oil into froth is accomplished. In fact all of the processes described are but temporary and are strictly confined to the period necessary to bring about a state of physical equilibrum in each case. Even the streaming movements are known to have continued only from twenty-four hours to a maximum of six days, apparently in proportion to the freedom allowed the drop to carry on the process of interchange, or to the quantity of matter concerned in the operation. In any case, the author admits that "the streams gradually become weaker and weaker, and finally cease," and that "the duration of the extension currents described is, for the most part, relatively short." In this respect, at least, the parallel between oil drops and protoplasm is lacking, for, according to the latest and most generally accepted belief, the one essential characteristic of protoplasm is its never ceasing activity, and it is this very attribute which now needs explanation and to which biologists are devoting the greatest attention. Professor Bütschli's experiments certainly cast no more light on this problem than do the achievements of chemists who have approached close to the synthetic construction of "the physical *basis*" without actually initiating in it the vital processes. As to the apparent similarity between the so-called creeping of his oil drops, and the purposeful and continuous migrations of the protozoa, Professor Bütschli finally admits that his explanation "seems at present feasible only for amoeboid movement in the strict sense, while other modifications of it, especially the formation of the fine pseudopodia of numerous Sarkodina, obtain no explanation." His theory of the simplest amoeboid movement is, however, itself so novel and extraordinary that we cannot but think it needs confirmation quite as much as does the phenomenon which it is intended to illustrate. Our author's belief is, that "by the bursting of some of the superficial alveoli, enchylema is poured out upon the free surface of the protoplasmic body, where it produces a local dimunition of surface tension, and in this way sets up an extension centre together with forward movement." If this theory is ever established upon a basis of actual observation it is difficult to see why the process may not be witnessed as easily in living forms as in artificial foams. On purely speculative grounds there are strong objections to the hypothesis, and some of these have made so powerful an impression upon Professor Bütschli that he is constrained at the last moment to append a frank confession that they necessitate the admission that the explanation of amoeboid movement brought forward by him "is inadequate." Even on this point, therefore, his experiments have not furnished a tenable theory applicable to free protoplasmic forms. Add this to the author's other admission that in his oil foams "nothing was ever observed of a rotational streaming such as occurs so commonly in vegetable cells," and we are left in wonder as to what possible application his experiments can have to biological problems of any kind.

We have given so much attention to Professor Bütschli's main contention that our space does not permit of a review of his less important arguments. It must therefore suffice to say that his criticisms of the various theories of protoplasmic structure are able and interesting.

The objects of his animadversions are, however, principally those who, like Velten, Brücke and Heitzmann, have held to the necessity of an organization in protoplasm made up of more solid and more fluid parts, the more solid constituting the active reticular structure in which resides the power of contractility, the more fluid being the passive contents of the living meshwork. All that needs to be said on this point is that the theory of Heitzmann is a fair attempt to account for actually observed phenomena in natural organisms, while the speculations of Bütschli do not appear to explain satisfactorily the behavior of even his own creations.

LETTERS TO THE EDITOR.

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

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The Editor will be glad to publish any queries consonant with the character of the journal.

Earth Worms.

THE earth worm notes and comments which have been published in recent numbers of *Science* have been of considerable interest to the writer, and this opportunity is taken to offer a few notes of observation, made at various times, bearing on the question of the cause of the appearance of large numbers of earthworms in rainy weather.

While it is not uncommon to read or hear of what are apparently well authenticated instances of "showers" of frogs, tadpoles, and fish, it is very rare to hear of any one who has seen earth-worm showers. Yet among the unobservant, the commonly received explanation of the occurrence of these animals in the large numbers that appear on our city and village sidewalks and pavements, during rain storms, is that they "rain down."

The habitat of the aquatic and amphibious animals makes it possible to accept as true the accounts of falls of such forms, because they might be taken up into the air with the water in which they live by some sudden strong uprush of air, as a tornado, but it is difficult to imagine conditions under which the burrowing earthworm could be raised to a position from which it could "rain down."

The worms which appear during rain can be satisfactorily accounted for, if a reason sufficient to bring them out of the ground can be found. Each square foot of loamy soil has, among other inhabitants, one or more earthworms living in it, so that the grass borders of streets and walks, even in large cities, harbor myriads of these animals and the soft earth in less thickly settled communities hides numbers of them beneath its surface.

Granted, then, the presence of the worms near at hand to sidewalks and pavements, cause adequate to bring them out of the earth during the rain and to make them wander about must be found. An explanation readily suggests itself, from the fact that the burrows of the animals must be full of water when it is raining, and it would seem that they would have to come to the surface or drown, and for a long time the writer was satisfied that this theory explained the appearance of the worms.

The following facts, noted at different times, tend to show that the explanation is not sufficient:

A number of years ago, while preparing some earthworms for use in a zoölogy class, I was washing them in a tank of running water, the source of supply being a small faucet tapping the village mains, the ultimate source being a small river. In the tank, which was of galvanized iron, were several crayfish, a few specimens of Onodonta and other mussels, and some snails. During the washing process, several of the earthworms slipped away into the tank, and they were left there to serve for food for the crayfish. There was no sand or earth in the tank, except a small quantity of sediment which had accumulated from the water and its inhabitants, and this was not over an eighth of an inch deep in the deepest place. Within a short time, all of the animals, except one very large specimen of a species of unio, were taken from the tank, and wishing to keep the unio alive, the water was not shut off, but left running in a small stream through the winter and the following summer. The next fall, having to use the tank for another purpose, the unio was removed and the tank cleaned. When the water was allowed to run out, in the bottom of the tank was found a large and active earthworm. The room was a private one, of which I carried the only key, and there had been no earthworms in it since the previous year, and the opening through which the water entered the tank was only sufficient to admit a slender stream of water, just sufficient to keep up circulation in the tank. The worm was carefully examined to make sure of the identity of the species, and it was permitted to escape. The sediment in the bottom of the tank was largely vegetable in its origin, and was of such character as to furnish abundant food for an earthworm, but was even at the end of the year hardly as deep as the worm was thick. The tank was about a foot deep, and the worm had lived about a year in that depth of water.

A second case came to my notice while collecting crayfish in a small river in this vicinity. The water in the part of the stream where the collecting was done was a little less than knee deep and the stream about forty feet wide. The crayfish hide under water-logged slabs and pieces of bark from the mills above, and to catch them the wood has to be moved. Under a slab in the middle of the stream was found a live and active earthworm, which was not buried in the mud, but lying immediately on the surface of it under the slab. There had been no rain for several days, and it was not probable that the worm had been washed into the stream from the bank. These instances, together with the fact that these animals are frequently abundant in soil that is saturated with water, and the observations and records of similar and more numerous cases of the same sort, noted by Darwin in his work on the "Formation of Vegetable Mould," tend to make it plain that the earthworm is not driven out of its burrow because it fears water.

From these considerations it is probable that still other causes to explain the phenomenon must be sought, but it is not my purpose to offer any theory in explanation. The following facts, however, may suggest a line of approach, along which investigation may be made by those disposed to attempt to work out the problem. If a light tapping be made on the surface of ground inhabited by earthworms, they will come to the top of their burrows, and if the tapping is kept up they will finally crawl entirely out of the ground. The birds are well aware of this fact, and robins, in particular, make use of their knowledge of