born zwischen „beipflichten und „bestätigen" macht, kann ich nicht als berechtigt anerkennen.

Die vorstehend erörterten Fragen habe ich eingehender behandeln müssen, weil den Zoologen meine Arbeiten über die Ortsbewegung der Bacillariaceen kaum bekannt sein dürften. Das Lauterborn'sche Werk dagegen, dessen vorwiegender Inhalt, die Kernteilung der Diatomeen, sicherlich das größeste Interesse verdient, den Vorteil hat, unter der stolzen Flagge des Heidelberger Zoologischen Instituts zu segeln.

In meinen Arbeiten über die Ortsbewegung sind nach und nach eine größere Zahl von Einzelbeobachtungen und Schlußfolgerungen mitgeteilt worden. Neue Untersuchungen werden vielleicht manche derselben als irrig erweisen oder modifizieren. Jede begründete Berichtigung, die einen tieferen Einblick in das Wesen der Ortsbewegung gewährt, wird willkommen sein. Immer aber wird billigerweise vorausgesetzt werden müssen, dass, wer fremde Arbeiten mit vermeintlich überlegener Sachkenntnis aburteilen will, sie zuvor nach Sinn und Wortlaut aufmerksam prüfe und dass dabei der Anteil nicht verkümmert werde, den andere an der Lösung solcher Fragen zu beanspruchen haben. [43]

Untersuchungen an Biflagellaten[1]).

Ingleside Lee, London S. E., June 30, 1896.

Dear Prof. **Hartog**.

I am unfortunately not in a state of health sufficiently vigorous to give myself to an exhaustive criticism of the interesting monograph Die Polytomeen by Raoul Francé in Pringsheim's Jahrbücher, vol. 26, 1894, p. 295 et seq., which would, in other circumstances, have been no serious tax upon me.

The ground on which Bütschli first and Francé lately have attacked the researches of the late Dr. Drysdale and myself has always been the assumption that our biflagellate organism was identical with *Polytoma uvella* Ehr. and that if our observations on this organism were inconsistent with those of other observers on *Polytoma uvella*, the whole of our results were discredited in consequence.

As it is, I should like you to remember that whilst we gave ourselves jointly and persistently to the study of the phenomena of the life-histories of a given group of septic organisms, we saw very early in our work so much that was complex that we avoided all attempts not only at classification, but even at nomenclature: and to

1) Auf Wunsch des Herrn Prof. Hartog bringen wir den an ihn gerichteten Brief des Herrn Dallinger über seine in Gemeinschaft mit Herrn Drysdale ausgeführten Untersuchungen zum Abdruck. Die Red.

this hour I am in no way involved in this matter. I have never professed to adopt, or to criticise when adopted, any designations of the seven septic organisms on which the work was done. Nor can I of necessity recognize any assumed identity now. One thing for example peculiar to the „biflagellate" form with which France assumes he is dealing, was, and still is, the comparative difficulty with which it is obtained. In our monograph of its life-history (read in 1874) we make the following remarks viz „Prolonged work with infusions has led us to make observations concerning them which, although without explanation . . . . seem to us of sufficient importance for note. Our first maceration was a cod's head; it was freely exposed to the air, but excluded from the light. For two months nothing at all remarkable presented itself. Abundance of *Bacteria termo* and *B. lineola* and amoebae were found. But at the exspiration of the twelfth week the form to be described (the „biflagellate") appeared — survived for three months and two weeks to the almost complete exclusion eventually of other forms — and then was supplanted by other monads, some of which have been described by us in former papers.

„This maceration was made from ordinary water supplied by the company on the Cheshire side of the Mersey. The same year, in the same place another cod's head, and the head of a salmon were macerated in separate vessels. It was later in the year and the production of vital forms was slower; yet in the course of four months the same phenomena as those described above took place; the only difference being that the form that we are about to describe (the „biflagellate") did not persist so long".

„In the autumn of the same year another cod's head maceration was made in Liverpool from the ordinary water supplied to the town. This up to the spring of the following year showed no trace of the form in question . . . . while a maceration . . . . made in april 1873 under the same circumstances . . . . was found in april 1874 to swarm with the peculiar form in question".

It must be remembered that our work was not casual or transitory, but persistent, extending without break over years; and even when we had completed our study of a given form, in persueing the study of others akin to it, we were constantly seeing the former in its various stages again, and so through a long period were provided with opportunities for the revision of our previous work.

Moreover during our many years of joint work in the study of these organisms we followed a definite, and as we believed then, and still believe, an essential method, from which we never deviated.

1. We persued independent observations, mostly in the same room, and with precisely similar instruments, on the same organism from the same maceration; and for this purpose my colleague lived with

me for many months in each year, or else arranged a residence close by for himself and family. By this means we came independently upon such phenomena as presented themselves, and either was able to confirm the observation of the other.

2. Our (to us) most important determination was to observe the entire series of cyclic changes in each organism in the living form only; in no case under any circumstances to rely on post mortem evidence, or make deductions from dried „prepared" or stained specimens.

This resolution was formed after nearly twelve months of careful experiment and comparison. Many of the organisms were not more than the 1—4000th of an inch in length, and in every speck of the ripe maceration presented themselves in such great numbers, that, apart altogether from shrinkage and distortion, wholly changing the relation and aspect of delicate phenomena, the aggregation was so constant in drying as to make (to us) reliable results impossible. The consequence was that no part of our work was done, and no one of our conclusions was drawn from dead or „prepared" specimens.

3. Nothing was ever admitted by us jointly, as established, which had not been seen by both; first independently, and then together: and this applied not only to the cyclic changes themselves, but to their sequence in each of the organisms; and then drawings were made which were never approved until they were satisfactory to both.

4. To the matter of measurements we attached much importance, as our experience had taught us this might almost be considered of specific importance. Hence for each of the seven forms studied we made alternately in the course of our studies fifty measurements each (at different times).

5. We did not allow ourselves to be satisfied with discontinuous observations so far as the final results were concerned. The hinderences were of course many. At times certain morphological features presented themselves persistently for many days; these were possibly followed by others equally persistent; but our principle was never to infer a connection between even consecutive phenomena: but to actually observe the sequences of development from beginning to end. This was only possible with some such „continuous growing stage" as we devised and used; and it was this that made the work so prolonged, and made two observers indispensible.

6. From the dates of the earlier papers it will of course be well known that the observations were made with achromatic object-glasses. They were however of the very highest quality, and were designated the 1—12 th, 1—16 th, 1—25 th, 1—35 th and 1—50 th. Those who have cared to follow my work for the last twenty years, will know that I was amongst the earliest to point out, and endeavour to have

corrected the essential defects of these lenses. Their power was always too great for their numerical aperture. But this could only be got over in one way, viz the employment of a homogeneous medium which would make large numerical aperture possible; and the use of some new „optical metal" or glass which should have higher refractive and dispersive indices. At the suggestion of Mr. Ware Stephenson Prof. Abbe gave a practical form to a homogeneous system of lenses, enormously benefitting microscopy; and by the combined work knowledge and ingenuity of Prof. Abbe, Dr. Schott and others new optical media were made, which gave a new value to and formed an epoch in the manufacture and use of the modern microscopes. How deeply we entered into and appreciated these optical improvements as aids to investigation it is enough for me to appeal to the Proc. of the Royal Microscopical Society, or to my edition of the late W. B. Carpenter's „The „Revelations of the Microscope" to establish.

For „critical images" and final certainty on any delicate question of the nature of a microscopical image these apochromatic lenses of Abbe are indispensible.

Nevertheless, not only before, but after the introduction of these, the over-amplification of the series of achromatic lenses I have described above was equally indispensible. The minute organisms studied, always in an active state, could never have had the details of their life-cycles made ont with immersion lenses of any kind — water or oil. We required great enlargement; but with dry lenses; for in following the constantly moving organism, so as to keep it continuously in sight, we were obliged to keep the mechanical stage in constant action, not only in rectangular but in diagonal movement; of which the beautiful stage of Powell and Lealand has for nearly half a century allowed.

But by such necessary motion with the delicate „continuous growing stage" employed in our work, it becomes clear to the practical worker, that at length, in spite of any amount of care, the water employed for „immersion" between the front lens and the cover-glass would ultemately pass the edge and mingle with the fluid in which the living organism was being studied; and so destroy the observation. This would be still more inevitable if the immersion fluid were oil.

Clearly then dry object-glasses with great magnifying power, illuminated by a suitable condenser were essential to our method, and even to-day must be employed on such observations.

But this by no means prevented the use of the new apochromatic objectives; they became a valuable suppliment, and admirable mode of testing the work we had hitherto done: and after the homogeneous objectives were introduced, and again after the apochromatic object-glasses were in our hands, we went over every important point — not

of course consecutively as with the dry lenses — but in isolated detail and confirmed our preceeding work making new drawings.

We found the enlarged numerical aperture, and the correction of the spectra not corrected by the achromatic objective-glasses of inestimable value; but we found that the use of a suitable condenser with N. A from 1·0 to 1·5 absolutely indispensible for the best results.

It will thus be seen that the details of the work in question, and consequently the details of the life-cycle of this „biflagellate" form have been gone over several times since they were first done, and with lenses and entire optical systems wholly different from those employed at the beginning.

As I have practically followed the improvements in the optical apparatus of the microscope for thirty years, and have during all that time cooperated with some of the best manipulators in the world, I naturally attach importance to apparatus, and method. Moreover I find myself greatly doubting whether the „moist chamber" employed by Francé would in my hands have been satisfactory for doing the above work. But its use is probably made more efficient by the statement of Francé on p. 297 viz that „all the researches here presented were carried out . . . . . partly and indeed mostly on living material, partly however also on preserved and stained material". This of course leaves us in doubt as to how far essential points were inferred, not only as phenomena, but as sequences from post mortem and stained specimens; but even more, it leaves us in doubt as to how far the absence of certain phenomena, which in my experimental experience, would be most certainly absent from, or indiscoverable in „preserved and stained material" of the „biflagellate" monad, is to be attributed to the use of such material.

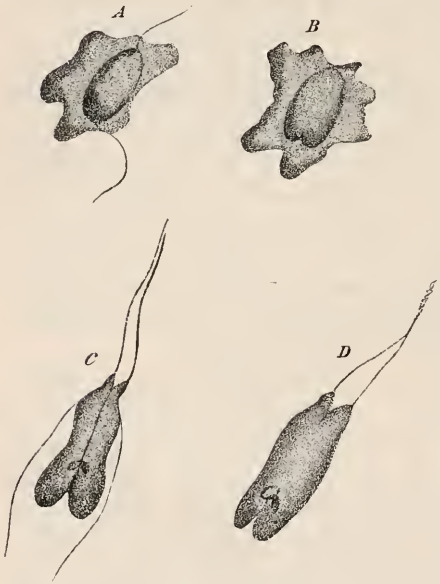
That there are many biological subjects and even very minute biological subjects that will tolerate the process of drying without much prejudice to the discovery of accurate results I do not for an instant doubt; but this does not apply to the septic organisms. Myself and colleague obtained the strongest evidence as I have already hinted, from experience and experiment, that errors, negative and positive, are inevitable to a study of these minute forms in any way dependent on drying and staining. But I venture to think that the errors resulting from its employment are rather multiplied than diminished when the work of observation on the life development of such forms is done „partly . . . . . on living material, partly . . . . . on preserved and stained material".

Observations up to a certain limit on the life-cycles of even the minutest of these and similar organisms are not really difficult, if we possess two or three good microscopes, with lenses of the best corrections worked with a suitable condenser, and a well constructed piece

of apparatus for keeping the organisms alive continuously under close observation; and the combination of these things is by no means rare. But the difficulty begins when we have to follow the organisms into the more obscure, and relatively less common phases of their life-cycles, always supposing that our observations are made upon the living and active organism. It becomes a task impossible, in my judgement, save to joint observation. Over and over again given observations fail from many causes, and there is no way open but to begin again.

You may have noted that we expressed this opinion many years ago; its importance was indeed stated in our very first paper[1]); where the need of continuous observation is enforced by an example.

Now in the accompanying rough sketch taken from my folio, A represents what we called a „cercomonad" in the earliest condition prior to the act of fusion. B represents the same organism when the act of fusion (of two) is almost complete.



Let it be supposed that we obtain material from a maceration containing this organism in great abundance, and in some approved way dry on a slide a lesser or larger group of these forms, and then stain them. The process of shrinkage and distortion, and the inevitable changes will make it difficult indeed to discover that they are not alike, that a whole series of changes has occurred in B, which have never existed in A, and that they are morphologically and physiologically entirely different. But by following A into fusion with another (never a matter that can be done without difficulty and enduring patience) and seeing the blending forms reach the condition of B, we become provided with unmistakable evidence.

Again in the 1878 paper in the Proceedings of the Royal Society „On the life-history of a minute septic organism", we are provided with another instance. In the sketch C represents a state of fission: but D gives a drawing of a condition of fusion in the same organism. The manner in which the intermediate stages took place would never have been (in our hands) discovered by means of dried and stained

1) Monthly Microsc. Journ., Vol. X, p. 55.

specimens and this affirmation is made, not empirically but after experiment and endeavour. There are details that must have been lost by the death, drying and staining of the specimens, while the delicate flow of sequences must have been lost.

I daresay you may have noted that in my later papers I have shown that the delicate use of a staining agent is possible upon the living form; it increases the death rate on the stage but it makes some observations on the living organism easier.

I venture then with deference to question whether the methods employed by Francé are a fair test of the work done so long ago on the „biflagellate" monad. May I not venture to ask you to consider what appears to me to be the impossibility of finding on a dried and stained specimen the spore-condition — that is to say the burst sac — shown in plates XXIV, XXVI, XLI, LXXXV and CIV, amongst others of our joint papers. Francé is quite right when he says that such observations are not (by his method) or are only with great difficulty within the reach of our optical expedients. Even under the very best conditions such apparatus must be of the very highest quality, and employed by those who by long experience know how to use them in such a way as to bring into operation their finest and most perfectly corrected endowments.

Of the accuracy of the joint observations of myself and colleague upon the organism we have simply for the sake of our own convenience called the „biflagellate" I have not the remotest doubt. They were not only made with great care and patience at the first, but they have been more than once repeated and confirmed. But whether it is identical with the *Polytoma* of Francé's paper I do not venture to decide. I do however, with all deference question the competence and suitability of his methods as modes of criticism of the joint work of Dr. Drysdale and myself. [37]

Very sincerely yours **W. H. Dallinger.**

Otto Ammon, Der Abänderungsspielraum.

Ein Beitrag zur Theorie der natürlichen Auslese. Sonderabdruck aus: Naturwissensch. Wochenschrift. Berlin. F. Dümmler. 1896.

Wer die Wirksamkeit des Selektionsprinzips auf die Umbildung der Organismen anerkennt, muss zugleich zugeben, dass Abänderungen, welche einen bestimmten Grad nicht überschreiten, für das Gedeihen eines Lebewesens indifferent bleiben und deswegen von der Selektion nicht beeinflusst werden. Es muss also einen „Abänderungsspielraum" geben, innerhalb dessen Grenzen die Variation frei bleibt. Werden nun die gemessenen Größen eines variablen Gegenstandes und die Frequenzzahl jeder dieser Größen in ein Koordinaten-System eingetragen, so dass die Abscissen den Größen, die Ordinaten der Frequenzzahl entspricht, dann ergibt sich daraus eine Kurve.