The Principles of Protistology.

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"We may also here observe, that in our enquiries after any truth, and more especially in regard to the generation of small living creatures, which cannot be examined by the naked eye, we ought not to rely on any tales that are told on these subjects, but on our own experience, and even that not lightly, but by long and unwearied trials and experiments, whereby to come at the truth."

Anton van Leeuwenhoek.

Preface.

Some years ago, I began to study the so-called "unicellular" organisms — or Protista, as I prefer to call them — in the hope that I might be able to obtain from them some insight into many problems of biology. I thought then that these organisms — on

account of their peculiar organization — were likely to yield very valuable information regarding many obscure biological phenomena. Now that I have a first-hand knowledge of a large number of Protista, I still hold this view. But I know that they are not going to furnish us with the sort of information which most biologists imagine. They are not going to reveal vital phenomena in a simplified form.

More than one attempt has already been made to weave the knowledge derived from the Protista into the fabric of general biology. To my mind, all these attempts have failed, and they have failed because they have been founded upon an entirely false interpretation of the Protista.

The evolution theory and the cell theory, formulated as they were in the middle of last century, have had a paralysing effect upon the study of the Protista. These theories have forced men to see the Protista from an entirely subjective point of view, and have prevented Protistology from throwing any light upon biological problems in general. So long as the Protista are "primitive unicellular organisms", so long will their biological significance remain unrecognised. The point of view from which they are generally regarded is objectionable, but not sufficiently objective.

Biology is only just beginning to shake itself free from the fetters of last century. Already there is a tendency becoming evident to study things as they *are*, in preference to speculating upon things which *must be*, or which *ought* to have happened. Yet in spite of this, the Protista have been neglected — so far as general biology is concerned. A very great deal is known about them now — far more, indeed, than any one man can ever hope to learn — and yet for general biology they are still where they were fifty years ago, when hardly anything definite was known about them.

The great importance of the Protista — to my mind — lies in the fact that they are a group of living beings which are organized upon quite a *different principle* from that of other organisms. It may therefore not unreasonably be hoped that a study of them will afford many highly important biological facts for comparison with those derived from the study of the Metazoa and Metaphyta. The Protista offer us, in other words, a new point of view for looking at the phenomena of life.

I have often met with two statements when I have attempted to discuss certain of my views with others. These are: first, that they contain nothing that is really new — that they are simply what

everybody really knows; secondly, that they are merely eccentric — perversions of established truths. Now I have never discussed the whole of my views with anybody. I would therefore ask anybody who adopts either of these positions to read the whole of this paper before he pronounces judgment upon any one part of it. If this is done, I think that my position towards accepted beliefs regarding the Protista will be found to be logically sound. It is not a hastily assumed position, nor an outcome of a natural tendency to iconoclasm, but a position which I have been led to occupy from my own fairly extensive first-hand knowledge of the Protista and of general cytology, supplemented by fairly wide reading.

Many matters dealt with in the following pages could obviously have been expanded to a much greater length. But it has been my aim to treat only of the essential, and consequently I have in many cases sacrificed detail for the sake of brevity. I hope to follow up this analysis with many more detailed analyses of the vital phenomena presented by the Protista. The present paper in largely analytic and destructive: but it is so of necessity for it is useless to attempt to build upon a rotten foundation.

The Protista.

It is necessary, before proceeding any further, to explain what I mean by "Protista". I use the term to designate all those organisms which are generally described as "unicellular" — whether they be regarded as animals, plants, or intermediate forms. I do not use the term in its narrower sense, as introduced by HAECKEL (1866). By Protista I mean therefore all those organisms which are now called Protozoa, Protophyta, and Protista in the narrower sense. This is the meaning which SCHAUDINN gave to the name Protista when he founded the "Archiv für Protistenkunde" in 1902, as a "Sammelstelle für alle Forschungen zur Naturgeschichte der Einzelligen". With the constitution of the group Protista in this sense I hope to deal elsewhere.[1])

I use the word Protistology to designate the science of the Protista. This is the English equivalent of one of the three terms (Protistologie, Protistik, Protistenkunde) introduced by HAECKEL (1866).

I must point out that I use the word Protista merely as a

[1]) I hope soon to publish a paper supplemeutary to the present one, in which I shall discuss the constitution, classification and probable relation to other organisms, of the group Protista.

label for the group. I use the word because it is already in existence, and it seems to me undesirable to introduce a completely new nomenclature. It must be understood that when I speak of Protista, Protozoa, and Protophyta, I do not attach any significance to the literal meaning of these names: I do not mean to imply that the organisms are in any way "primitive" or "first" forms of life.

I must point out also that the recognition of a group Protista in the sense here given does not in the slightest degree increase the difficulty of classifying organisms — an objection which was more than once [1]) urged against Haeckel's system. It was urged that whereas certain organisms had a doubtful position among the animals or the plants, by introducing a third group for them the difficulty was in reality increased, as it became necessary to decide whether the doubtful forms were animals, plants, or protists. This objection obviously does not apply to the group Protista as defined above. The only difficulty is to decide whether a given organism possesses the so-called "unicellular" type of organization, or not. The distinction between the Protista and the Metazoa and Metaphyta is that which already exists between the Protozoa and Metazoa on the one hand, and the Protophyta and Metaphyta on the other.

The Protist Individual.

An absolutely fundamental point which must be recognized at the outset of our analysis is this: one whole protist individual is a complete individual in exactly the same sense that one whole metazoan individual is a complete individual. Amoeba is an entire organism in just the same sense that man is an entire organism. As far as the concept "individual" can be analysed — and I believe that it is at present unanalysable [2]) — it is clear that a protist is no more homologous with one cell in a metazoon than it is homologous with one organ (e. g. the brain or liver) of the latter. Only the cytologist blinded by what he sees through the microscope could ever believe in such a preposterous proposition. Fallopius, Wolff, v. Baer, and the older biologists were prevented from falling into such an error owing to the imperfections of early microscopes. To the man who has not been led astray by the cell theory, this proposition is self-evident.

[1]) Cf. for instance Saville Kent (1880), Vol. 1.

[2]) Cf. Herbert Spencer (1864). "There is, indeed, ... no definition of individuality that is unobjectionable. All that we can do is to make the best practicable compromise." (Vol. I p. 206.)

The idea that a protist is the homologue of one cell in the body of a metazoon is an outcome of the general belief in the cell theory. I shall therefore leave a discussion of this matter until I have analysed the cell theory itself in relation to the Protista. (See next section, p. 274) I would emphasize the fact here, however, that a protist behaves as a whole organism, and not as a part of one, and a metazoon behaves as a whole organism in just the same way, and not as a "colony" or "state" of separate individuals.

The failure to recognize the fact that a protist is a complete organism has led — by way of the cell theory — to a very curious interpretation of the value of a protist individual. It has led to the adoption of a view which is very prevalent — a view which is well expressed by CALKINS (1909) as follows: — "Students of the Protozoa and biologists generally (e. g. BÜTSCHLI, WEISMANN, etc.) early called attention to the fact that not the single cell of a protozoön, but the entire succession of cells that may be formed from the period of one conjugation to that of the next, should be compared with the metazoön .... If we could take such an entire succession of cells thus formed from the repeated divisions of a fertilized protozoön, and if at any given period could combine them in one mass of cells, we would have the analogue of a metazoön and would find that the protoplasm represented by the aggregate of cells would manifest the same successive periods of vitality as those of youth, adolescence and old age in metazoa" (p. 103).

CALKINS, it will be observed, calls this idea a fact, and he is by no means alone in his belief that it is one. Now I must point out that the idea is absolutely erroneous — it is a doubly false analogy.

In the first place, the body of a protozoon it not the homologue of a single cell in the body of a metazoon,[1]) and hence the succession of individuals formed from one conjugation to the next is not comparable with a metazoan body any more than a swarm of bees is comparable with an elephant. In the second place, even supposing that this false analogy were correct, it is quite obvious that there is no real parallel between the succession of protozoan "cells" and the body of a metazoon. Let us suppose, for example, that a protozoon undergoes ten successive divisions between one conjugation and the next. The "single-celled" zygote, by repeated divisions, gives rise to 1024 "cells" as a final result. These "cells",

[1]) See next section, p. 274 et seq.

we will suppose, adhere together, so as to resemble the metazoon with which they are to be compared. After the tenth division, the cells are ready to conjugate. That is to say, we now have an organism consisting of 1024 "cells", each of which is a gamete. It may be justly asked, Has anybody ever seen a metazoon which is in the slightest degree like this? Has anybody ever found a metazoon which is composed of nothing but coherent gametes?

It is, to me, almost incredible that a view such as this should have found wide acceptance; yet such appears to be the case. I can only express a hope that future writers will pause and think before they perpetuate this unfortunate misconception.

To my mind, Ehrenberg (1838) — in spite of his incorrect interpretations in matters of detail — was far nearer to the truth when he saw Protista as "vollkommene Organismen", than any more modern biologist, who regards them as analogues of parts of multicellular beings.

The Protista and the Cell Theory.

Before I can proceed any further, it is necessary to consider the Protista in their relation to the cell theory. This leads me to an analysis of the cell theory itself, for without such an analysis it is impossible to interpret the Protista satisfactorily.

Now the cell theory is almost universally accepted, so that many biologists regard it as a statement of fact rather than a theory. Yet neither the botanist nor the zoologist, as a rule, takes the trouble to analyse the cell theory. It is therefore not surprising to find that the cell "theory" is really partly fact and partly hypothesis. As Sedgwick (1894) has very truly said: "There is a want of precision about the cell-phantom, as there is also about the layer-phantom, which makes it very difficult to lay either of them. Neither of these theories can be stated in so many words in a manner satisfactory to every one. The result is that it is not easy to bring either of them to book."

As a statement of what is generally understood by the "cell theory", I will follow Sedgwick and consider what is taught to the student of biology. "We tell him", says Sedgwick (1894, p. 89), "that the cell is the unit of structure, that an organism may consist of a single cell, or of several cells in association with one another: we draw the most fundamental distinction between the two kinds of organism, and we divide the animal [1]) kingdom into two great

[1]) And, of course, the vegetable kingdom also.

groups to receive them ... We tell him that the various structures present in a protozoon are all parts of one cell, whereas in a metazoon the various parts are composed of groups of cells which differ from one another in structure."

I think this is a fair and objective statement of what most people understand by "the cell theory". I have more than once been told by biologists that such is not a fair statement of their belief[1]): but I have never succeeded in obtaining any other definite (not subjective) statement from them. I say this in order that I may not be hastily accused of tilting against a windmill. For the same reason, I will add the two quotations (from two excellent treatises on the Protozoa) which now follow[2]):

1. „So ist die Zelle der Elementarorganismus, das Individuum auf der niedersten Individualitätsstufe ... Die einfachsten Organismen, die einfachsten Tiere (Protozoa) und die einfachsten Pflanzen (Protophyta) sind weiter nichts als selbständig und unabhängig lebende Zellen ... Die höheren Organismen (Metazoen und Metaphyten) sind Zellenstaaten ..." (Lang, 1901, p. 1).

2. „In ihrem gesamten Aufbau entsprechen die typischen Protozoen nur einer jener Einheiten, aus denen sich der Körper der vielzelligen Tiere wie aus vielen Bausteinen aufbaut, sie bestehen aus einer einzigen Zelle" (Doflein, 1910, p. 3).

Every teacher of biology knows how difficult it is to define the cell to his pupils in such a way that they learn exactly what constitutes "a cell". It is a most remarkable fact that although this must be known to almost every biologist, yet hardly any attempt has been made either to define or to analyse this difficulty.[3]) Now an analysis is not only possible, but quite simple — so far as its general terms are concerned.

The first and most obvious point which analysis brings to light is this. The word "cell" is used for a heterogeneous assemblage of

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[1]) Bourne (1895) says he is "quite certain that the picture which he (Sedgwick) draws of the teaching given to every student of biology is a travesty of the truth" (p. 147).

[2]) The "cell theory" is also formulated in similar and very definite terms by Virchow (1858).

[3]) Sedgwick (1898) in his Text-book has made an attempt to compass the cell theory in his account of the Protozoa. Sachs (1892, 1895), also, has tried to obviate certain of its difficulties by means of his "energid" hypothesis. Neither of these attempts can be regarded, I think, as an entirely satisfactory solution of the difficulties.

things which are not really homologous with one another. These things fall into three main groups. The name "cell" has been given to:

(1) a whole organism (e. g. a protist individual);

(2) a part of an organism (e. g. a liver "cell");

(3) a potential whole organism (a fertilized egg).

Only those who are blinded by the cell theory could see any real similarity in these three things — apart, that is, from their obvious structural resemblances in certain features.

The word "cell" has become so firmly established in biology, that it would be almost impossible to displace it completely. What, then, are we to do, if we wish to retain our present terminology but to escape our difficulties? I think I have arrived at a simple solution of the difficulty — a solution which will meet all present needs, and one which will at the same time avoid all the errors involved in the cell theory. It is as follows:

When we consider living beings as a whole, it is evident that they are all composed of a substance which we call protoplasm (v. Mohl). Now the investigation of an immense number of organisms has brought to light a most important fact, namely, that the protoplasm of a living organism always consists of two elements, a nucleus (or nuclei) and cytoplasm.[1]) The former may possess the most varied forms.

Now in a very large number of multinucleate organisms the cytoplasm is subdivided into a number of definite compartments, each of which encloses a nucleus. These cytoplasmic subdivisions with their enclosed nuclei we may call — following the ordinary usage — c e l l s: and we may say that the organisms themselves display a c e l l u l a r s t r u c t u r e. Very many organisms, however, are uninucleate, binucleate or multinucleate, but show no subdivision of the cytoplasm into compartments containing the nuclei. These organisms may therefore justly be called — when compared with the former group — n o n - c e l l u l a r. It is obviously incorrect to call them unicellular, for the cells of cellular animals and plants are subdivisions of w h o l e organisms. Upon this basis, therefore, we may define Metazoa and Metaphyta as organisms possessing a cellular structure: we may define the Protista, on the other hand, as organisms possessing a non-cellular structure.

[1]) Concerning the Metazoa and Metaphyta probably nobody would question this statement. Regarding the Protista, I will so far anticipate a part of what I have to say in a supplementary paper, as to state that I believe there is no real evidence that any non-nucleate forms exist.

This is no cell "theory": it is a statement of fact, and therefore greatly to be preferred to any theory. It is a simple solution of every difficulty connected with the word „cell". There can be no longer any difficulty in deciding whether a multinucleate *Actinosphaerium* or *Opalina* should be regarded as consisting really of one cell or many cells: they consist of no cells — they are constructed upon a non-cellular principle. Similarly, with regard to a binucleate *Paramecium*, or *Trypanosoma*, or an *Arcella* with its complex nuclear arrangements — they are all simply non-cellular organisms. The difficulty which has been experienced with regard to syncytia and plasmodia also vanishes. There can be no doubt as to whether the plasmodium of a Mycetozoon should be regarded as one cell or many cells: it is a non-cellular structure. This must be obvious to anyone who will think of the things themselves in concreto, and not in abstracto — that is to say, in terms dictated by the cell theory.

The cell theory thus vanishes and can be replaced by facts as soon as we give a definite and objective meaning to the word "cell". That the Metazoa and Metaphyta are organisms composed of cells in much the same way that a house is built of bricks, is not a theory but a fact. Anybody who can use a microscope can easily convince himself that this is so. The theory came in when it was assumed that all living beings are composed of cells — that the Protista consist each of a single cell, the Metazoa and Metaphyta of many cells — that the "unicellular" organism is the analogue of one cell in the body of a "multicellular" organism. For this is what is implied when we use the words "unicellular" and "multicellular". This may perhaps be called a theory, but it is more accurately called a misconception. Yet it is what every student of biology is taught.

The essential difference between the structure of Protista and that of other organisms is properly and objectively expressed when we describe these as cellular, those as non-cellular. The concept "cell", derived from a study of cellular organisms, is a fairly simple one. It is quite clear that the correct antithesis, in the present case, is between cells and not-cells, and not between many cells and one cell — as has hitherto been universally assumed.

An exactly comparable error has been committed in the case of worms.[1]) The Nematoda have been described[2]) as "unisegmental

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[1]) I am indebted to Prof. Sedgwick for kindly calling my attention to this point.

[2]) See Rolleston (1888).

Vermes", as opposed to the Chaetopoda, or "multisegmental Vermes". It is obvious that, as far as segments are concerned, the difference between these two groups is correctly expressed by saying that Chaetopoda are segmented and Nematoda are *not* segmented; and not correctly stated by saying that the former are multisegmented, the latter unisegmented. A segment is a *part* of an organism — an entire organism may be segmented or not segmented. It cannot consist of one segment. Similarly, organisms may be called cellular or non-cellular — they cannot properly be contrasted as "multicellular" and "unicellular".

There are many points which still require elucidation before this analysis can be regarded as complete. We have seen already (p. 276) that there are three main categories into which the structures now called "cells" may be separated. We have seen that, as regards two of these, a correct antithesis may be drawn between them in a very simple manner. We may correctly call the structural units of which a Metazoon is composed "cells", and we may call the whole body of a Protist "non-cellular". The third type of thing which is called a "cell" — namely, the fertilized egg — still remains to be considered. Are we to call this a cell — following the customary usage — or not a cell? And how is it with the blastomeres of a developing egg? Are they cells or not cells?

These questions are easily answered. The fertilized egg, before it undergoes cleavage, is not a cell any more than a Protist individual is a cell. It shows no cellular differentiation, but it is a (potentially) complete organism. The blastomeres, on the other hand, are properly called cells. They are parts of a whole. After the first cleavage, the organism as a whole has acquired a cellular structure. It matters not whether there are two blastomeres constituting the whole organism, or two thousand. They are cells in just the same sense that liver-cells and brain-cells are cells.

The fertilized egg itself is not a cell. We speak of it as unsegmented before development, and call its development segmentation. Now the segments into which it divides itself are cells. Therefore, when not segmented it is not cellular. The non-segmented egg cannot correctly be called one cell any more than it can be called one segment. Cells, like segments, are *parts* of organisms — not whole organisms.

It might perhaps be urged that the first two blastomeres of a developing egg (e. g. of a sea-urchin) can be separated and can then give rise, each of them, to a whole larva: that the early blasto-

meres are, therefore, potential whole organisms just as much as is the egg itself: and that therefore if the egg itself is not a cell, neither are the blastomeres; or conversely, if the blastomeres are cells, so also is the egg. This is, in reality, no contradiction of what I have already said. The blastomeres are, under ordinary conditions, parts of a whole and not whole organisms. When artificially separated they may each produce a whole larva. But this does not indicate that they must be regarded as individual organisms. I can cut a worm into two pieces, and each part will in time produce a whole organism. Yet I suppose nobody will maintain that the original worm should therefore be regarded as really two organisms and not one.

It might be asked — If the fertilized egg is not a cell, what is it? To say that it is not a cell is a mere negation, and does not tell as what it is and what we are to call it if we are not to call it a cell. I would suggest that the egg be called an egg; or, — if this sounds too unscientific — an ovum. To call it an "egg-cell" is unnecessary and undesirable, for it leads to a confusion of ideas.

Similarly, with regard to the Protista, it might be asked, If an *Amoeba* is not a cell, what is it? If its body cannot be called a cell, what can it be called? It is obvious that an *Amoeba* can be called an *Amoeba*, and its body can be called its body (or soma, or the equivalent in any other language which may be preferred). *Amoeba* has a body in exactly the same sense that man has a body. It is only the cell dogma which makes a man believe that a protist has a body which is the equivalent of one small fraction of his own.

Another question which might be asked is the following: Is the gamete of a metazoon a cell or not a cell? The answer to this question is also obvious. Gametes may be correctly called cells. They are parts of an organism, and not whole organisms — although they become detached from the parent organism when fully formed. In the same way, a leucocyte is a detached cell [1]) — a part of an organism and not a whole organism. In spite of its morphological resemblance to an *Amoeba* at one stage in its life, the leucocyte is properly called a cell, whilst *Amoeba* is properly called a non-cellular organism. This correctly defines the difference between the two.

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[1]) Unconnected, that is to say, by any nervous or protoplasmic union. Of course a leucocyte is still in what may be called "chemical connexion" with the organism to which it belongs.

That there is a real difference, in spite of certain superficial resemblances, I think nobody will deny.

It might be objected, with apparent justice, that an unfertilized egg — which I call a cell — is sometimes able to develop parthenogenetically. The parthenogenetic egg before it begins to develop is therefore a potential whole organism, just as is the fertilized egg — which I call non-cellular. According to my view, therefore, the very same egg is first of all called a cell, and then not a cell — though its structure remains apparently unchanged. Now this again is no real contradiction. Everybody will admit that there is a moment when the gamete ceases to be a gamete and becomes a new individual organism. Yet it is impossible to determine the exact point at which this change occurs. If I say that the gamete (a cell) becomes a potential whole organism with a non-cellular structure, I am only expressing the same fact in different words. No additional difficulty to that which already exists has been introduced.

It is quite obvious that all the confusion in the cell theory has arisen from too wide an application of the word "cell". When we think of the things themselves to which the name has been applied, it becomes clear that a great error has been committed in making the term "cell" too elastic. This has led to a complete confusion of ideas. When the same name can be used for several quite different things — when we can call a whole organism and the thousandth part of an organism by the same name, it becomes obvious that the almost universal misconceptions which now exist regarding the cell were almost inevitable. The cell theory was a great generalization, but a generalization which was merely verbal. Now unless we introduce a completely new terminology — which would be almost impossible at the present moment — it is evident that the remedy for the difficulty lies in restricting the application of the word "cell". This will have to be done before any really clear comparison can be made between the vital phenomena of the Protista and those of other organisms.

The incorrect notion that Protista are "unicellular" was not established in biology without a struggle. The earlier workers upon this group were not all deceived completely by the cell theory — as their followers have been. Claparède and Lachmann (1858), for example, began their "Études" with the following remarks: "On serait tenté de croire que la théorie de l'unicellularité des infusoires n'a plus aujourd'hui qu'un intérêt historique, comme celle de la polygastricité ... La théorie de l'unicellularité des infusoires n'a pas

besoin d'être combattue ici plus en détail. L'ouvrage que le lecteur a sous ses yeux n'est qu'une longue protestation contre elle. Chacune de nos pages est un nouveau coupe de hache porté à sa base" (p. 14). It is most interesting to note how more modern biologists, convinced of the truth of the cell theory, and yet finding that it does not exactly fit the Protista, have endeavoured to square their belief with the facts by various subjective subterfuges.

It was v. SIEBOLD (1845) [1]) who really introduced the idea that Protista consist of single cells, homologous with single cells in the body of a metazoon. He was anticipated to some extent by MARTIN BARRY (1843) and RICHARD OWEN (1843). [2]) It is interesting to note, however, that the latter considered that the Ciliata could not be properly regarded as simple cells — an account of their high degree of structural differentiation. The ideas of v. SIEBOLD were shared by KÖLLIKER and others, and very soon came to occupy a prominent position in biology. HUXLEY (1853), nevertheless, clearly saw that there was something wrong in the application of the cell theory to the Protista when he wrote: "It is true, indeed, that the difficulty with regard to these organisms has been evaded by calling them "unicellular" — by supposing them to be merely enlarged and modified simple cells; but does not the phrase an "unicellular organism" involve a contradiction for the cell-theory? In the terms of the cell-theory, is not the cell supposed to be an anatomical and physiological unity, capable of performing one function only — the life of the organism being the life of the separate cells of which it is composed? and is not a cell with different organs and functions something totally different from what we mean by a cell among the higher animals?" (Vol. 1 p. 265).

Neither HUXLEY nor anybody else, however, appears to have seen the real truth about the Protista — that they are non-cellular. Accordingly, we find in the literature some most curious subjective attempts to interpret Protista in terms of cells. Here are some instances:

PERTY (1852) regarded the Protista as really "composed of an aggregation of separate cells, none of which have attained their complete development, but remain indistinguishably united with each other". [3])

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[1]) Cf. v. SIEBOLD (1849).

[2]) See BÜTSCHLI, Vol. 3 for the history of this matter.

[3]) I quote from SAVILLE KENT, Vol. 1 p. 22.

Similarly, we find Gegenbaur (1859) advocating the view that Infusoria should be regarded as cell-complexes rather than as simple cells, though he emphasizes the point that v. Siebold's generalization should be accepted "mit größter Vorsicht" (p. 43).

Stein (1867) also, considered that the Ciliata could not really be regarded as consisting of single cells, but were rather forms greatly modified from original cells. „Die ausgebildeten Infusionsthiere aber wird man immer Anstand nehmen müssen als einzellige Organismen zu bezeichnen, denn sie sind nicht bloß einfach fortgewachsene Zellen, sondern der ursprüngliche Zellenbau hat einer wesentlich andern Organisation Platz gemacht, die der Zelle als solcher durchaus fremd ist" (p. 22).

Carnoy (1884) is content to say of the Protista that "ces petites créatures représentent, en effet, l'epanouissement idéal de la cellule, son plus haut degré de différentiation et de complication" (p. 101). He does not recognize the fact that they are such ideally developed cells that they are not real cells at all.

Finally, we see this kind of obscurantism at its zenith when we find Hickson (1903) saying of a Ciliate that it is "strictly speaking, not unicellular, but bicellular or tricellular, etc., according to the number of micronuclei which it possesses" (p. 394), and when we are told by Prowazek (1910) that: "Die Protozoenzelle ... ist in einem gewissen Sinne ein einzelliges Metazoon" (p. 1). Perhaps no better instances than these could be found of a case in which a theory in capable of taking precedence of facts.

Attempts to bring the Protista into line with the cell theory have also been made by R. Hertwig (1902), Gurwitsch (1904) and Doflein (1909). All these workers have realized fully that this is a difficult matter, but they have failed to see why it is so. Their chief difficulty has really been owing to the fact that they assumed the fundamental truth of the cell theory. The inadequacy of this theory had already been pointed out by Whitman (1893) and Sedgwick (1894), though upon different grounds.

So far, I have chiefly confined my attention to pointing out that a Protist is not the homologue of one cell in the body of a metazoon. There is another side to this question, however, which I have not discussed. It is this: Is a metazoon to be interpreted as a colony of "elementary organisms" (i. e. its cells) or not? So far, I have assumed that this question must be answered in the negative. Recent research has shown beyond a doubt, I think, that a metazoon cannot be interpreted as merely a cell colony. It has been truly said by

DRIESCH (1907): "All attempts to conceive the organism as a mere aggregate of cells have proved to be wrong. It is the whole which uses the cells .... or that may not use them" (Vol. 1 p. 28).

DRIESCH's own work upon experimental morphogenesis has furnished most conclusive proof of the truth of this statement. MORGAN's (1898)[1]) work upon regeneration in *Planaria* also furnishes good evidence that the cells are of secondary importance — the organism acts as a whole and is to some extent independent of its cells. MORGAN has shown that when parts of a planarian are cut off, the animal is capable of "regenerating" itself without producing new tissues by cell division. It remodels itself to the correct shape, using only those cells which are already in existence. A number of similar regeneration experiments might easily be quoted. In the Protista, also, regeneration always takes place without the formation of cells. The regeneration phenomena in the Protozoa are now well known from the studies of HOFER, GRUBER, BALBIANI, VERWORN, PROWAZEK, and many other workers.[2]) It will be unnecessary to quote experiments in detail. F. LILLIE's (1902) work upon the egg of *Chaetopterus* has shown also most clearly that differentiation and development — though to some extent abnormal — may take place without any cells being formed. He has shown that the egg of this worm may be made to develop "parthenogenetically" into a ciliated and otherwise differentiated "larva", without undergoing cleavage. "The process of cell-division, as such, is necessary neither to growth, differentiation, nor the earliest correlations; but it is accessory, in Metazoa, to all three as a localizing factor, often from the earliest stages" (p. 494).

I think there can now be little doubt of the truth of DRIESCH's remark given above. In the case of plants, WILSON (1906, p. 393) says: "it has been conclusively shown by HOFMEISTER, DE BARY and SACHS that the growth of the mass is the primary factor[3]); for the characteristic mode of growth is often shown by the growing mass before it splits up into cells, and the form of cell-division adapts itself to that of the mass: "Die Pflanze bildet Zellen, nicht die Zelle bildet Pflanzen" (DE BARY). I think HUXLEY (1853) clearly recognized the truth of the view so happily expressed by DE BARY in these words when he wrote of cells that they "are not

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[1]) Cf. also MORGAN (1901).

[2]) Cf. DOFLEIN (1909) and WILSON (1906) for references.

[3]) i. e. in development.

instruments, but indications — that they are no more the producers of the vital phenomena than the shells scattered in orderly lines along the sea-beach are the instruments by which the gravitative force of the moon acts upon the ocean. Like these, the cells mark only where the vital tides have been, and how they have acted" (Vol. 1 p. 277).

It may be urged that to restrict the application of the word cell in the way which I have done in preceding pages, is undesirable. I believe this is not so, for a restriction of the word to one definite class of things greatly clarifies our ideas regarding living beings. Moreover, there is a historic justification for the restriction which I propose. The word cell was introduced by Hooke (1665) originally for the structures which I have called cells.[1]) The "cells" of the cell theory of Schleiden and Schwann were also the structures to which I restrict the word "cell". It is true that Schwann had the idea that the cell was a kind of individual[2]) but that does not affect the fact that "cells" to him were the subdivisions of the animals and plants which are properly called cellular. Schwann did not extend the word "cell" to the Protista. That was a later constituent of the cell "theory".

How is "a cell" to be defined? This is a question which naturally arises, and which still remains to be answered.

The usually accepted definition of a cell[3]) is that originated by Leydig and M. Schultze — namely, that "a cell is a mass of protoplasm containing a nucleus". It is quite clear that this definition as it stands includes not only cells properly so called, but also many Protista and the fertilized egg. When a definition is so wide that it can include several quite different things, it is obvious that the generalization which it permits us to make is merely verbal. It makes the really heterogeneous appear homogeneous. The fault lies with the definition, and not with the things themselves. If we add to the Leydig-Schultze definition that the cell is a part of an organism and not a whole organism, we shall have a correct definition. We must also state that a cell is bounded by a membrane or cell wall of some sort. The cell must be defined in terms of the organism, and not the organism in terms

[1]) Hooke, of course, saw only the cell wall: but he used the word cell to designate the subdivisions into which the tissue was divided,

[2]) "For Schwann, the organism is a beehive, its actions and forces resulting from the separate but harmonious action of all its parts" (Huxley 1853, p. 254).

[3]) Cf. Wilson (1906), p. 19.

of the cell. If this had only been universally realized fifty years ago, the existing misconceptions about the cell would never have arisen.

VIRCHOW's aphorism "omnis cellula e cellula" is obviously inaccurate. The nucleus, certainly — so far as is known — always arises from a pre-existing nuclear structure. But to say that the cell always arises from a pre-existing cell is true only in the same sense that it is true to say that the liver always arises from a pre-existing liver. The fertilized egg is non-cellular, but the organism acquires a cellular structure during development. The cells may appear after the first nuclear division (e. g. in *Echinus*) or not until later (e. g. in *Peripatus*). The time of their appearance in ontogeny is variable.

Several words — such as "cytology", "cytoplasm", etc. — which are now firmly established, offer certain difficulties. None of these difficulties is really insurmountable. "Cytology" may be understood to apply to the study of cells of cellular organisms — as it actually does in practice at the present moment: or it may be interpreted as a mere label for a science, and not taken literally as the study of cells properly so called. Similarly, the word "cytoplasm" may be used as a label for that part of the protoplasm which is not a part of the nucleus — its literal meaning being put aside.

I believe I have now made it quite clear that the truth about the cell theory is this: the various structures called cells certainly exist — but the cell theory is a myth.

It may be contended that my analysis and destruction of the cell theory is a mere matter of words — that everybody really knows the limits of the cell theory, and nobody is really under any delusion regarding the "cell" and its significance except myself. I believe, however, that I have succeeded in showing that the cell theory is itself a verbal generalization only. It is not the organisms which are wrong, but the cell theory. The word "cell" has been extended to too many different things, and has consequently made organisms appear as they are not.

The cell theory must be abolished. It has had its value in directing attention to the minute structure of organisms, especially to their nuclei. Now that is has forced men to regard things as they should be and not as they are, it has not only ceased to be of value but has become positively harmful. Its harmful effects are especially well seen in the case of the Protista. To mention only a few of its consequences, it has made Protista appear as

"simple, elementary organisms", analogous with parts of other forms: and it has made the Metazoa and Metaphyta appear as colonies of elementary organisms, and not as whole organisms. It has also given rise to the erroneous idea [1]) that a succession of protist individuals is the analogue of a metazoon. In addition to this, it has made men think that the Protista are going to yield information about the fundamental phenomena of life more readily than the Metazoa and Metaphyta. The real truth is that the Protista are not simpler than other organisms — they are merely differently organized. A correct interpretation of the Protista can never be reached until the cell theory has disappeared. [2])

Although the cell theory is to a large extent accountable for the erroneous views which now obtain regarding the Protista, it is by no means the only obstacle which has to be surmounted. A second and most important obstacle is the belief in the existence of "higher" and "lower" organisms. In the following section, I will attempt to cope with this difficulty.

On "higher" and "lower" Organisms.

It is almost impossible to read any work dealing with the Protista from a general point of view, without finding them referred to as "lower" organisms. The meaning of this expression is never discussed, [3]) but it is universally assumed that certain forms are "higher" than others, and that the Protista are the "lowest" living

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[1]) See p. 273.

[2]) Prof. J. B. Farmer has kindly called my attention to the fact that organisms have already been compared as "cellular" and "non-cellular" by Sachs (Lectures on the Physiology of Plants, Eng. trans. 1887). I cannot find any evidence, however, that Sachs applied "non-cellular" to the Protista in the sense in which I have used the word, or that he recognized its significance in regard to these organisms.

[3]) I had already proceeded a considerable way in my analysis when a remarkable paper by Franz (1911) made its appearance. Franz has given a most excellent analysis — as far as it goes — of the "higher and lower" idea. I would refer the reader to the original. I must point out, however, that Franz's paper has in no way influenced what I have to say in the following pages — in spite of many similarities which the reader will notice. My own analysis was almost complete before the publication of Franz's paper, and has been worked out quite independently. With Franz's conclusions I am in complete agreement. I have made use of his paper only in so far as it has enabled me to omit a discussion of many points with which he has dealt quite adequately, and which I should otherwise have been forced to treat in detail in the present analysis.

beings. There can be little doubt that most people, when they use the word "higher", mean "more nearly perfect" or "better" in a not clearly defined sense. As this idea has a most important bearing upon the interpretation of the Protista, it becomes necessary to find out whether "lower" has any really objective meaning. I shall attempt, therefore, to analyse the expression "a lower organism" in order to show whether the Protista should be called "lower organisms" or not.

The belief that certain organisms are in some way "higher" than others appears to be as old as biology itself. It certainly has found expression in countless biological writings since the time of ARISTOTLE.[1]) Not merely among modern writers, but among the older ones also, do we find this belief. Among the old writers the belief was usually founded upon the supposed existence of a "scala Naturae" — with man at its top: among the moderns the belief is usually supposed to be founded upon the evolution theory, and finds expression in the construction of a phylogenetic tree — singularly enough, also with man at the top.

In the first place, I will attempt to discover what a modern biologist understands by a "higher" or "lower" organism: but I must point out that I believe most biologists, when they use the words

[1]) The following passages in ARISTOTLE are interesting in this connexion: "Nature proceeds little by little from things lifeless to animal life in such a way that it is impossible to determine the exact line of demarcation, nor on which side thereof an intermediate form shall lie. Thus, next after lifeless things in the upward scale comes the plant, and of plants one will differ from another as to its amount of apparent vitality; and, in a word, the whole genus of plants, whilst it is devoid of life as compared with an animal, is endowed with life as compared with other corporeal entities. Indeed, as we just remarked, there is observed in plants a continuous scale of ascent towards the animal.... And so throughout the entire animal scale there is a graduated differentiation in amount of vitality and in capacity for motion" (Historia Animalium, 588 b). "Plants, again, inasmuch as they are without locomotion, present no great variety in their heterogeneous parts.... Animals, however, that not only live but feel, present a greater multiformity of parts, and this diversity is greater in some animals than in others, being most varied in those to whose share has fallen not mere life but life of high degree. Now such an animal is man. For of all living beings with which we are acquainted man alone partakes of the divine, or at any rate partakes of it in a fuller measure than the rest" (De Partibus Animalium, 656 a). "We therefore must not recoil with childish aversion from the examination of the humbler (*ἀτιμοτέρων*) animals" (ibid. 645 a). There are also other passages containing similar expressions. We sometimes find, for instance, that certain organisms are called "more perfect" than others. I would refer the reader to the works themselves.

"higher" and "lower" with regard to organisms, do not possess any clear idea of what they mean by these terms. It is therefore not surprising to find that there are several quite distinct ideas included in the expression "a lower organism". These ideas appear to me to fall into three main groups. I will consider each of these separately.

(1) The phylogenetic category. By "higher" is often meant "more highly evolved" — that is, separated by a greater distance from the original forms of life. For example, mammals are said to be "higher" than fishes because they are supposed to have appeared later in the history of the earth. The degree of separation of forms in time cannot be a real criterion for determining whether one form is "higher" than another: for existing mammals are called "higher" than existing fishes. It is obvious that if the fishes which we now know only as fossils gave rise to the fishes and mammals now living, then the living forms are all separated in time by exactly the same distance from the fossil forms. The degree of separation can therefore be measured only in terms of the amount of structural change which has taken place during the time occupied in descent from a common ancestor. This leads us to the second category (see below).

The difficulty involved in using the terms "higher" and "lower" in a phylogenetic sense is obvious. One has only to look at the ancestral trees constructed by different people for the same group of organisms to see that the "highest" forms owe their position to the predilections of the constructor of the tree. "Higher" means simply nearer to the top of the ancestral tree — itself, in most cases, constructed on an entirely subjective basis — and therefore does not mean "better" in any sense, though this meaning is often superadded. It is obvious that "higher" in a phylogenetic sense is used in quite a special sense, and depends for its meaning upon the particular theory of evolution which the user of the word adopts: that is, it is largely subjective.

(2) The morphological category. By "higher" may be meant "more highly organized" — that is, displaying a greater degree of structural complexity. For instance, a fern appears morphologically more complex than a yeast, and is therefore "higher".

It is manifest that "higher" used in the sense "structurally more complex" is used in quite a special sense, and is different from "higher" used in a phylogenetic sense. It is also obvious that "higher" cannot be used in this sense to mean "more nearly perfect". A gutless parasitic nematode would usually — from the morpho-

logical point of view — be called "lower" than a nematode possessing a well-developed gut. Yet the gutless animal is no less nearly perfect than the other: in a sense, it is more nearly perfect, for it is able to dispense with the necessity of making and using an intestine. In spite of this, there is certainly a very large subjective element in "higher" used in the sense "structurally more complex", and it is generally believed that "higher" in this sense implies something "better" in a sense not clearly defined. The fallacy appears to me to be derived from the use of the word "high" in two different senses. "More highly differentiated" = simply "more differentiated": but it is then supposed that "more highly differentiated" = "higher" = "better" in some way.

(3) The anthropomorphic category. By "higher" is meant "more like man". This category includes a very great deal of what is meant by "higher" among animals. The "highest" animals are simply those which most resemble man. Man is considered to be the most nearly perfect of all animals. Those which are least like him are the "lowest", because they are the least near perfection. The fish is "higher" than the worm; the reptile „higher" than the fish; the ape "higher" than the reptile; man "higher" than the ape or any other animal. The development of the brain — man's chief attribute — is therefore one of the chief criteria in deciding whether an animal is "high" or "low".

It is unnecessary to emphasize the fact that the anthropomorphic point of view is purely subjective, and has no value in objective science. It must be pointed out, however, that anthropomorphism cannot account for the application of the word "higher" to plants. A rose tree is said to be "higher" than a mushroom but it is no more like a man.

Now although biologists use the words "higher" and "lower" in a sense which is phylogenetic, morphological, or anthropomorphic (or a combination of these), they do not usually realize this clearly. It is apparent, also, that in whatever sense the word "higher" is used, it has certain subjective ideas associated with it. The three categories into which I have separated the ideas involved in the words "higher" and "lower" are, moreover, largely interwoven, and frequently in the form of a complex vicious circle. For instance, an organism is often "higher" (= "more highly evolved") because it is "more highly differentiated", and it is "higher" (= "more highly differentiated") because it is "more highly evolved" — evolution being

usually supposed to have occurred from the structurally simple to the structurally complex.

When we try to analyse the idea "lower" in regard to organisms, it very soon becomes apparent that "higher" and "lower" are figures of speech which do not always appear to correspond with anything objective in the organisms themselves. Phylogeny and morphology, or a mixture of ideas derived from both, will not account for the prevalent idea that certain organisms are "higher" (= "better", "more nearly perfect"), than others. The biologist as a rule probably believes that the phyletic relations and the degree of structural differentiation of an organism furnish him with sufficient justification for using the words "higher" and "lower": and although this is really not the case, I believe most biologists have also, at the back of their minds, an idea that the "higher" organisms are in some way "better", "more nearly perfect", than "lower" organisms. This is not based upon any scientific knowledge. The belief appears to exist quite apart from any biological training. The man who knows nothing of the supposed phyletic relations existing between organisms, still regards certain of them as "better" in some way than others. He regards a rose-tree as "better" than a moss, a horse as "better" than an earthworm, animals — on the whole — as "better" than plants. This is a b e l i e f which has existed from a very early age. The b e l i e f existed certainly long before the evolution theory and long before modern morphology. It has been grafted on to these, and they have in turn come to be regarded as the justifications for the belief. Man, in the words of the Psalmist, is "a little lower than the angels", — and everything else is "lower" than man in exactly the same sense.

The basis of this belief is largely anthropomorphic. Animals are "better" than plants because man is an animal. The animals which are most like man are the "best", the "highest". Yet anthropomorphism does not give us by any means a complete solution of the problem. It obviously does not apply to plants.

There are no scientific grounds to justify the belief that certain organisms are more nearly perfect than others. Apart from anthropomorphic conceptions, we have no idea what a perfect organism should be. If we consider organisms as they a r e, objectively, it is apparent that all are equally "high" or equally "low". We cannot say that this species is nearer perfection than that.

Perfection can only be predicated of an organism when it is a d j u s t e d t o i t s e n v i r o n m e n t in such a way that no improve-

ment upon this adjustment is possible. The most nearly perfect organisms — the "highest" — are therefore those which are most nicely adjusted to their environment — those whose adaptations are best suited to their mode of life. There is no reason to suppose that man is better adapted to his environment than *Amoeba*. The one, therefore, is as near perfection as the other. No organism is perfectly adapted to its environment — not excepting man. They are all just sufficiently adapted to their mode of life to enable them to exist. They would not be here if they were not.

The only organisms which can be called "lower" in the sense that they are less adapted to their environment, are degenerate individuals in a species — that is, those individuals which fall below the minimum degree of organization necessary for the preservation of the species.[1]) Parasites and socalled "degenerate" species are not less nearly perfect than other species — they are merely adapted to their different environment in a different way. The kind, and also the degree, of differentiation displayed by an organism is correlated with its mode of life. It does not really permit us to measure one organism against another. To call one organism "higher" than another because it appears more differentiated is meaningless.

If "higher" means "more nearly perfect", therefore, it can only be used dogmatically. When we consider the organic world objectively — apart from anthropomorphic prejudices — we have no criterion save adaption for judging of what is good and what bad, what perfect and what imperfect. "Higher" and "lower" used in this sense are therefore a matter of belief.

The belief in the existence of "higher" and "lower" organisms (= "better" and "worse" organisms) existed long before morphological and phylogenetic speculations acquired the important position which they now occupy in biology. It existed long before the Darwinian epoch, and still exists in the mind of the man ignorant of morphology and the (supposed) phyletic relations existing between organisms. The appeal to morphology, phylogeny, and anthropomorphism (e. g. cerebral development) is only a pretext for a belief already present in the mind — an attempt to find an objective justification for something quite subjective. The idea that certain organisms are

[1]) Species on their way to extinction — owing to a change in environment with which they are unable to cope successfully — might also be called "lower". Thus, we might call the lion a "lower animal", as he appears to be doomed to extinction on account of a change in environment (man) to which he cannot react in such a way as to preserve his species.

"lower" than others must therefore have a psychological explanation.

I have already pointed out that anthropomorphism cannot explain completely the belief in the existence of "higher" and "lower" organisms. And there are certainly many other factors in this very complicated psychological problem. Let us try to discover some of them.

To some extent, the aesthetic sense may be accountable for the belief that some organisms are "better" than others — i. e. better = more beautiful. Thus, it may be that a rose is better than a moss because it is more beautiful. Yet this obviously cannot give us a complete explanation any more than anthropomorphism.

Another factor which is of some importance is the old belief in abiogenesis. Old beliefs die hard. Even when they are dead, their footprints can often be clearly traced upon the sands of the human mind. The doctrine of abiogenesis is dead, but its vestiges are still recognizable in the belief in "higher" and "lower" organisms. There was a time when it was natural to suppose that many organisms arose spontaneously from non-living matter. In those days, worms, fungi, protozoa, and certain other organisms — born apparently from non-living matter — were no doubt with justice regarded as inferior to men and trees. These were more vital because incapable of being generated spontaneously from the non-living. Now we know better, but we cannot altogether escape from the clutches of the old belief. It is no uncommon thing to find it stated in popular works — and also tacitly assumed in works which are not popular — that the little mass of living slime which we call *Amoeba* is not so distantly related to the inorganic slime in which it moves, as is a human being.[1]) It is easier, apparently, to most people to suppose that very small organisms are nearer to the inorganic world than are large organisms. Yet the gap between the *Amoeba* and non-living matter is no less than the gap between man and non-living matter. The gap is, in reality, the same — that between the living and the non-living.

I think I have made it clear, from what I have already said, that we are dealing with a complex psychological problem, and one which has as yet not been solved. I think it will also be clear

[1]) Cf. Saville Kent (1880), who refers to Protozoa as "mere specks of animate jelly" (Vol. 1 p. 16): and see also an exactly similar remark ("mere specks of animated jelly") in Calkins (1909, preface) — also many similar expressions in many other works.

that anthropomorphism, aesthetics, and the obsolete belief in abiogenesis cannot completely account for the idea — which appears to be universal — that certain organisms are "higher" (= "better" in some way) than others. As we have already seen, a justification for this view cannot be found in morphology or phylogeny. What is the basis of this belief?

I think I can attempt an answer to this question. The solution of the problem lies in the literal meaning of the words "higher" and "lower" — that is, in the concept of size. Large things are to man better than small. This is a deep-rooted belief in the human mind. A few illustrations will serve to show this clearly.

When we wish to express approval of anything, we usually employ words which are primarily indicative of large size — e. g. we make use of adjectives such as great, grand, magnificent, superior, etc. In the same way, we express our disapproval by words which primarily have reference to small size — e. g. little, small, low, inferior. Compare, for instance, the following: "little-minded" and "great-minded"; "high principles" and "low principles"; "to make much of a person" and "to belittle a person"; "superior quality" and "inferior quality", etc. etc. Numerous other instances at once suggest themselves. It will, I think, be unnecessary to insist upon this point. It is, indeed, difficult to express our approval of anything without employing words which primarily refer to large size.

Ideas of what is good or perfect are largely linked up with ideas of large size. Large things are better than small — man sees more of them and therefore "thinks more" of them. This is very well seen in the old (and still existent) anthropomorphic conceptions of God. Man regards himself as the most nearly perfect being on the earth. God, an absolutely perfect being, was therefore conceived as a man on a very large scale. This idea probably underlies the idea of God which the majority of mankind possess at the present day. Even when God is conceived as an "infinite being", this probably takes the form, in the human mind, of a being of infinitely large size. (Compare here the opening words of the Magnificat: "My soul doth magnify the Lord.")

We see this connexion between size and goodness expressed in many other ways. Gods and saints are frequently depicted as large men. The sculptor also makes his statue of a hero of "heroic size" — that is, larger than other men, to show the hero's "greatness". Witness, moreover, the disappointment which is commonly felt when we first meet a man, whom — from his achievements — we have grown

to regard as a "great" man, and discover that he is physically smaller and inferior to ourselves.

Large organisms are, to man, better than small organisms. He regards them as more nearly perfect. This is the real psychological explanation of the existence of "higher" and "lower" organisms in biology. "Higher" organisms are primarily those which are literally high — that is, of large size. The "highest" animals and plants are the largest, the "lowest" the smallest. Small organisms are regarded as less nearly perfect than large. Hence the wonder usually excited when minute organisms are seen through a microscope for the first time. In Leeuwenhoek's [1]) day microscopic animals were "contemptible little creatures". Linnaeus, as is well known, for a long time refused to recognize the Protozoa. His attitude towards them is well seen in the name *Chaos* which he finally bestowed upon *Amoeba* — "nomineque specifico, infausto satis, gentem innumeris speciebus affluentem in tenebras damnat." [2]) This attitude of Linnaeus is practically the same as that adopted by nearly all biologists at the present day. It is the outcome of an inherent tendency in man to regard small organisms as in some way worse than large.

When organisms exceed man in size, the anthropomorphic idea of his own superiority comes in. A tree may be larger than a man, and is "higher" than a mushroom: but it is not "higher" than man, because it cannot do what he can do. Man's chief specialization being in the development of his brain, we accordingly find that he generally judges other animals by the degree of approximation of their intelligence to his own. Thus, he refuses to call an elephant "higher" than himself though it is larger, because he considers it less intelligent. Similarly, he regards a dog as "higher" than a pig — though it may be smaller — because its degree of intelligence appears to approximate more closely to his own than does that of the pig.

The connexion between the size of an organism and its "highness" or "lowness" is well brought out in the following passage from Jennings [3]) (1906), who says that he "is thoroughly convinced, after

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[1]) "When, therefore, we see these wonderful properties in so small and, to us, so contemptible a creature; and ... etc." (Letter from Leeuwenhoek to A. Magliabechi, dated 16 Oct. 1699. Vol. II p. 88).

[2]) O. F. Müller (1773).

[3]) It is a very curious fact that Jennings uses the terms "higher" and "lower" freely, and never attempts to analyse them. It is all the more curious because

long study of the behavior of this organism, that if *Amoeba* were a large animal, so as to come within the everyday experience of human beings, its behavior would at once call forth the attribution to it of states of pleasure and pain, of hunger, desire, and the like, on precisely the same basis as we attribute these things to the dog" (p. 336). That is to say, if it were larger, it would be a "higher" organism.

We have already seen that organisms which are structurally more differentiated are "higher" than those which are structurally less differentiated. It therefore happens that of two organisms of approximately equal size but displaying different degrees of differentiation, the more differentiated would usually be called the "higher". The explanation of this is similar to the one already given in regard to size. A morphologically complex organism is "higher" than a morphologically simple because in it we see more things. Just as man tends to regard large things as better than small, so also he tends to regard many things as better than few. An organism with a complex life-cycle is regarded as "higher" than one with a simple life-cycle, merely, I believe, because we see it doing more things; and we always tend to assume that an organism which can do many things is in some way "better" than an organism which can apparently do but few. This probably explains, in part, why animals are "better" than plants as living beings. For we see animals generally engaged in discharging many vital functions — moving, eating, respiring, excreting, etc. — whereas these activities are not obvious to us in the case of a plant.

It will hardly be necessary to point out that when we obtain the impression that large or complex organisms are "higher" or "better" than others, it is something quite subjective — something which is psychological and not capable of being treated from the point of view of objective biology.

We have now reached this point in our analysis. The belief in the existence in "higher" and "lower" organisms is found to be based upon several different ideas, the most important of which is that large things are, ipso facto, better in some way than small. We can perhaps go one step further, and attempt to answer the next question which naturally arises — Why does man think that large organisms are better than small? This inquiry is really out-

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Jennings' own work largely serves to demonstrate that there is no essential objective difference between "lower" organisms and others.

side the scope of the present paper, but a few remarks will not be out of place.

I think the answer to this question must be somewhat as follows. Fear is at the bottom of this belief. Man is, and probably always has been, afraid of animals larger than himself. They are more powerful than he by virtue of their greater size. He therefore respects them more than smaller and less powerful creatures which he can himself control. The large animals are thus better than the small animals, on the whole. The fear of large and powerful animals has been transferred to plants and inanimate objects, and has been transformed into the emotions of awe, reverence, respect. Large mountains certainly excite in many people a feeling of awe — in my own case, so does large and powerful machinery, especially when in motion. This feeling itself is closely akin to fear, and also contains a certain element of admiration. But I do not wish to develop this theme any further at present.

I think it will now be apparent to anybody who has followed my analysis up to this point, that the expression "a lower organism" has not a simple significance, but rather represents a tangle of ideas which are chiefly of subjective origin. The terms "higher" and "lower" as applied to organisms have nothing to recommend them: on the contrary, they lead to great confusion of ideas and consequently to many erroneous conclusions. In biology, these words have been as productive of evil as the word "progress" has in the formulation of an evolution theory.

By calling the Protista "lower organisms", biologists have been led to suppose that they are really simpler forms, nearer to the earliest forms of life which appeared upon the earth. They therefore believe that the study of these forms is likely to elucidate many problems connected with the vital phenomena of man and the animals more closely resembling man: they believe that the Protista are going to reveal vital phenomena in a more elementary form, and hence in a way which is more easily understood. This is a fallacy, although it is the foundation of a great deal of work which has been done upon the Protista — especially from a physiological point of view.

In concluding this section, I may be allowed to express a hope that the adjectives "higher" and "lower" will soon cease to be applied to organisms. When this happens, biology will gain. To call Protista "lower" organisms is unnecessary and misleading: it leads only to obscurantism.

The Protista and the Evolution Theory.

In the two immediately preceding sections, I have attempted to analyse the expressions "unicellular" and "lower" as applied to the Protista. In the present section, I shall attempt to extend my analysis to the expression "primitive" as applied to these organisms.

At the outset, I would point out that the Protista at present occupy an absolutely false position in the theory of organic evolution. This is due almost entirely to the ignorance which prevailed regarding these organisms when current ideas concerning evolution were still in the melting pot — before they had solidified into their present form. Yet although we now possess much detailed knowledge of the Protista, this ignorance of former times is still a constituent of the evolution theory. The theory of organic evolution will soon have to be recast.

We encounter the statement that the Protista (or a certain section of them) are "primitive" organisms so frequently in biological literature that it seems hardly necessary to cite instances. HAECKEL's writings are full of such expressions. In fact, the Protista are so generally called "primitive" that nobody appears to question their right to this title.[1]) I have already raised objections to the expression in the case of the Bacteria (DOBELL, 1911), and I will now enter into the matter more fully.

The expression "a protist is a primitive organism" appears to me to be based on three different ideas. These may be stated thus: (1) A protist is a simple organism. (2) In evolution, simple organisms always precede complex. (3) A simple organism now living is more like the earliest forms of life than a complex organism now living. Hence a protist is a primitive organism.[2])

I will endeavour to analyse these ideas further. First of all, what is meant by a "simple" organism? Undoubtedly, I think "simple" is understood primarily as meaning "structurally simple" — i. e. relatively but little differentiated. If this were the only meaning attached to the word it might be justifiable to use it in this connexion.

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[1]) As concrete instances, I cite the following: HAECKEL (1878) speaks of "die urwüchsige Einfachheit im Körperbau und in den Lebenserscheinungen dieser unvollkommenen Urwesen" (i. e. Protista). "As the name Protozoa indicates, they are primitive animals" (CALKINS 1901, p. 1). "A Protozoon is a primitive animal organism" (CALKINS 1909, p. 17). The spacing is mine. It is also quite often stated that certain forms are "more primitive" than others.

[2]) An erroneous conclusion drawn from three unjustifiable assumptions.

Nevertheless, although we may grant that a protist is morphologically less complex than a metazoon — as a general rule — there can be no doubt that very many protists are extraordinarily complex, if we consider all the structural changes which occur throughout the whole life-cycle. It is significant, also, that the Protista about which least is definitely known are generally the "simplest": for instance, the Bacteria, about whose structure and life-history great diversity of opinion prevails, are generally regarded as the simplest of all organisms. As a matter of fact, I believe, they are not "simple" in any sense of the word (cf. Dobell 1911). They are merely very small, and hence have come to be regarded as "lower" organisms.[1]) I think nobody who has worked upon the Protozoa for any length of time really believes that they are morphologically very simple — though most protozoologists continue to call them so. Others who have not studied the Protozoa and who wish to call them "structurally simple" should glance at Doflein's text-book before they do so (see Doflein 1910). "The *Amoeba*" is frequently taken as the type of utter simplicity in organisms. Yet we still know very little about the life-cycles of the *Amoebae* — but enough is known for it to be stated that their life-cycles are complex, and that apparently there are dozens of species of *Amoeba*, each with a different life-cycle. Is any one of these really simple? I think nobody who has tried to work out one such life-cycle would answer in the affirmative.

Now it appears to have been quite overlooked that apparent structural simplicity may be correlated with physiological complexity. From a physiological point of view, the Protista are very complex. Jennings' (1906) excellent book furnishes abundant confirmation of the truth of this statement.[2]) Further, are we justified in saying that it is "simpler" to move a flagellum or pseudopodium without the aid of muscles, nerves, etc. than a leg with these structures? Is it "simpler" to digest and breathe and excrete with the same structurally homogeneous part of a body than with separate, structurally heterogeneous parts? To put it very crudely — is it "simpler" to digest one's dinner with one's feet than in one's stomach? It is

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[1]) Cf. preceding section.

[2]) "Unicellular organisms react to all classes of stimuli to which higher animals react" (p. 261). "Action is as spontaneous in the Protozoa as in man" (p. 261). "The behaviour of the Protozoa appears to be no more and no less machine-like than that of the Metazoa; similar principles govern both" (p. 263), etc. etc.

obviously only "simpler" when this is what we mean by "simpler" — that is, when we beg the question. It is certainly not easier to comprehend. This brings me to the real crux of the whole matter. The word "simple" has more than one meaning. It may be justifiable to say that Protista are simple (= morphologically but little differentiated) — though this might be controverted: but it certainly is not true that Protista are simple (= easy to understand). In the latter sense they are no simpler than any other living beings. It is this double use of the word "simple" which has prevented the Protista from being seen in a proper light: it is this word which has — in company with "lower" and "unicellular" — given rise to the universal belief that the Protista display vital phenomena in a more elementary form than other organisms.

So much for the "simplicity" of the Protista. Let us now consider the supposition that the "simple" organisms now living are comparable with the original forms of life upon the earth.

Organic evolution is generally conceived as having taken place, in the main, from the morphologically simple to the morphologically complex — from the less differentiated to the more differentiated. This idea has then, apparently, been transferred to the forms of life now existing: so that the morphologically simple forms are called "primitive". (It should be noted, in passing, that the idea that organic evolution goes from simple to complex is, in part, derived from a consideration of simple and complex forms now extant). It is not at all obvious, however, why a conception of the distribution of forms in time should be applied to the forms distributed at the present moment in space.

This may be expressed more clearly thus: Let A, B, C ... Z represent the living animals, arranged in order of structural complexity, from the simplest (A) to the most complex (Z). Let α, β, $\gamma \ldots \omega$ represent the succession of animals from the most primitive and simple (α) to the most complexly organized (ω) now living — arranged in order of their sequence in time. Now it is obvious that Z corresponds with ω. But it is far from obvious that A, B, C ... Y correspond with α, β, $\gamma \ldots \psi$. Indeed, it is highly improbable that they would correspond. Yet this is the assumption — an entirely unjustifiable one — which is made when existent forms (e. g. the Protista) are called "primitive".

To proceed a little further in my analysis, let me take a concrete case for criticism. "I take it for granted", said Gaskell (1910), "that we all believe in Evolution and that an upward progress can

be traced from the Protozoa to Man." Since no one present ventured to differ from Gaskell on this point, I may perhaps also take it for granted that this extraordinary statement finds at least some acceptance among biologists. [1])

First, let me ask the question, Why should it be supposed that the Protozoa as they now exist should be the ancestors of man as he now exists? Obviously they are not. If by Protozoa are meant animals like what we now call Protozoa (i. e. really not Protozoa), I would remark that there is really no foundation for the belief that these hypothetical organisms ever existed. The only Protozoa which we know to have existed at an early epoch are Radiolaria and Foraminifera — for the most part exactly like those now existing. There is no more reason to suppose that these organisms, with their complex and peculiar structures and life-histories, are the beginnings of man than that man is the beginning of them.

It is well-known that the popular saying "man is descended from an ape" is an inaccurate statement. By "ape" is meant a definite kind of being now extant. The biologist therefore prefers to say that "man is descended from an ape-like ancestor" — meaning thereby that man and ape had a common ancestor. The subjective element in a statement of this kind becomes obvious if we reverse this and state — a thing which probably no biologist would dream of stating — that "the ape is descended from a man-like ancestor." Yet if man and ape had a common ancestor — not an ape and not a man — why should it have been more ape-like than man-like? Similarly, with regard to the Protozoa, very few biologists would, I believe, object to such an expression as "the *Amoeba*-like ancestor of man." Yet how many of them would countenance calling this hypothetical creature "the man-like ancestor of *Amoeba*?" [2])

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[1]) This remark was made by Gaskell during a discussion on the origin of Vertebrates. It may therefore seem that to have controverted this statement would have been irrelevant to the discussion. This is not really so. If everybody had realized the fallacies in this statement, there would probably have been no discussion at all.

[2]) There is a passage in Franz's paper (p. 35) which is almost exactly parallel to this paragraph. I must point out that the above was written by me before I had read Franz's paper, and I have therefore let it stand exactly as it was written, as it is an integral part of my argument. I was astonished to find the same idea expressed quite independently by Franz and in almost the same words.

Why should it always be taken for granted that by "Evolution" is meant "an upward progress from Protozoa to Man"? This is only one hypothesis of organic evolution. That evolution of some sort has taken place in living beings I regard as certain. But that evolution of the Haeckelian "Amoeba to Man" type has not occurred I regard as equally certain. We can certainly believe in evolution without believing in this dogma.

There is absolutely no reason to suppose that any real *Amoeba* now extant is man's ancestor. And among the Protista whose life-histories are to any extent known there is no organism which corresponds with the creature of the myth. Concerning this fabulous "amoeba" we know nothing — save that its correct systematic position is probably in the group which contains the centaur, the phoenix, and the hippogriff. That happy, simple organism which just grows and divides and is called "a protozoon" and is regarded as representing the beginnings of life on the earth, [1]) will have to go back some day to the place from which it came — the dominion of dreams.

A belief in this hypothetical "amoeba" has led to a totally erroneous interpretation of the Protista. One constantly finds traces of this in biological writings. "The Protista", says Verworn (1897), "... seem to have been created by Nature for the physiologists, for, besides their great capacity of resistance, of all living things they have the invaluable advantage of standing nearest to the first and simplest forms of life; hence they show in the simplest and most primitive form many vital phenomena that by special adaptation have developed to complexity in the cells of the cell-community" (p. 51). I know not whether the Protista were created by Nature for the physiologists: but is seems to me that this physiologist has himself created these curiously simple and primitive forms for Nature.

It is not necessary for me to enter here into a general discussion of the theory of organic evolution — save in so far as the Protista are directly concerned. For those to whom evolution means "an upward progress from the Protozoa to Man" it may seem that

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[1]) "Unicellular organisms .... Each individual grows to a certain size, and then divides into two parts, which are exactly alike in size and structure .... If protected from a violent death, they would live on indefinitely, and would only reduce the size of their overgrown bodies by division. Each individual of any such unicellular species living on the earth to-day is far older than mankind, and is almost as old as life itself" (Weismann, Vol. 1 p. 72).

to controvert such a statement is to deny the evolution theory. Yet such is not the case by any means. It is, however, quite evident that those who hold this anthropomorphic belief really deny the evolution theory themselves in the case of the Protozoa. To suppose that the Protozoa which now exist are essentially the same as the first forms of life, is to suppose that while man has been evolving, the Protozoa have remained unchanged — that is to say, they have undergone practically no evolution. It is, to say the least of it, highly improbable that man alone of all animals has attained his present form by continuous evolution: that the ape has undergone less evolution than man, the cold-blooded vertebrates less than the warm-blooded vertebrates, the invertebrates less than the vertebrates, Protozoa less than Metazoa — each animal having undergone an amount of evolution which is directly proportional to its degree of resemblance to man.

The only alternative to this view — for those who speak of evolution from "Protozoa to Man" — is, it seems to me, to adopt the hypothesis of abiogenesis. If it be supposed that such forms as the Protista are still arising from non-living matter, it might be justifiable to regard them as therefore nearer than other organisms to the first forms of life. Such a belief is to my mind quite untenable.

Undoubtedly one of the strongest supports for the "Protozoa to Man" hypothesis has been the "recapitulation theory". It is supposed that when the egg undergoes segmentation in ontogeny, it repeats the processes which occurred in phylogeny when the Metazoa arose from "unicellular" ancestors. The recapitulation "theory" — the "fundamental biogenetic law" — has had to be so modified on account of the facts of development, that it really has been almost explained away. Apart from this, however, I must point out that the belief that early stages in ontogeny correspond with early stages in the "Protozoa to Man" phylogeny, is really based on nothing more than a false analogy. My analysis of the cell theory in previous pages will permit me to state this precisely.

A metazoan egg undergoing segmentation is a non-cellular organism undergoing differentiation by forming cells. Before segmentation, the egg is a whole organism: after segmentation it is the same whole organism, but more differentiated. After segmentation, the organism is not a colony of individuals each of the same value as the original egg. A protozoon undergoing division, on the other hand, is one organism dividing into two: it is one whole organism becoming two whole organisms of the same value as the original

whole organism. If segmentation were really analogous to the divisions of a protozoon, it would produce a cluster of eggs and not a differentiated organism. This is a fact which is so obvious, that it is quite surprising that the use of the word "cell" should have prevented it from being realized. There is no real analogy between an egg dividing into two blastomeres and a protist dividing into two protists.

To a certain extent, a fertilized metazoan egg is comparable with a protist individual. The latter is a whole organism with a non-cellular structure: the former also is a (potential) whole organism with a non-cellular structure. When the protist individual divides into two, it produces two whole organisms: when the egg divides into two blastomeres it remains the same organism — it does not produce two whole organisms. With division, the analogy vanishes. By calling the protist, the egg, and the blastomeres all "cells", an artificial verbal analogy is established. Yet even then it is astonishing that anybody can believe that one individual (egg) can divide into two individuals (blastomeres) and still remain one individual. This is a "two in one" mystery no less incomprehensible than the "Three in One" mystery. Possibly it is believed for the same reason — "certum est quia incredibile est".

It is often stated that a *Volvox* colony is the analogue of a blastula. This is simply a false analogy, due to the cell theory. A *Volvox* colony is an assemblage of individual organisms, each highly specialized.[1]) It is a colony, and like other colonies may be composed of a few individuals or of a very large number (up to about 20 000). A blastula, on the other hand, is one whole organism with a cellular structure. It is, to me, almost incredible that anybody could advocate the view that the Metazoa have arisen from aggregated Protozoa. When a protozoon divides into two, each daughter individual is still a protozoon, similar to the original form. When subsequent divisions occur, and the individuals remain connected so as to form a colony, they are still Protozoa. To suppose that a colony of protist individuals — each a complete

[1]) Volvox is, of course, regarded by many people as not only analogous to a blastula, but also as an organism intermediate between a protozoon and a metazoon. Doflein, for example, refers to it as "this primitive metazoon" (1909, p. 443). This erroneous idea appears to have arisen from the fact that many of the individuals in a colony are, apparently, unable to reproduce new colonies — i. e. are sterile. But it is no more justifiable to call them "somatic cells" because of this, than it would be to call the workers in a hive of bees "somatic cells".

organism — by adhering together could give rise to an organism of a different order, is as extraordinary as to suppose that a swarm of bees could unite to form a dog.

If the Metazoa have arisen from protist-like forms — which is far from proved — it is far more natural to suppose that they did so by developing an internal cellular structure, and not by the aggregation of individuals to form a colony.[1]) The aggregation idea is one of the results of the unfortunate application of the cell theory. The most that the early development of a metazoon can be held to show is the way in which non-cellular ancestral forms became cellular. This, however, is mere hypothesis. It must not be forgotten that there are no known adult animals which correspond with the two-cell, four-cell, eight-cell, etc. and blastula stages seen in ontogeny[2]): and the fact that there is a non-cellular stage resembling somewhat a certain stage in the life of a non-cellular organism now extant does not necessarily furnish any support for the recapitulation "theory".

Beyond emphasizing the point that early stages in ontogeny are only comparable with what we see in the Protozoa by means of a false analogy, I do not wish to enter into a discussion of the recapitulation hypothesis. This hypothesis is, I think, a matter for individual belief. The evolution theory was an induction from a large number of facts. The recapitulation hypothesis is a deduction from the evolution theory. It applies to a certain class of the facts, and cannot be directly proved or disproved. Some people prefer the hypothesis — others, among them myself, the facts. Recapitulation is, at best, a hypothesis: it has no claim to the title "fundamental biogenetic l a w".[3])

I trust that enough has now been said to show that the Protista can only be called "primitive organisms" by making the grossest and most unjustifiable assumptions. I think it will become quite clear to anybody who will devote serious attention to the matter that the Protista occupy a false position in current theories of

[1]) Cf. also Saville Kent (Vol. 1), and Sedgwick (1888).

[2]) I have referred throughout to holoblastic, "alecithal" eggs only. Although by no means all eggs are of this class, the others are "explained" by the recapitulationists as modified by "caenogenesis", and I may therefore — with them — disregard them.

[3]) Prof. Sedgwick has called my attention to the fact that he has already expressed the same views as these, in almost the same words, in an article on "The Influence of Darwin on the study of Animal Embryology", published in "Darwin and Modern Science", Cambridge 1909.

organic evolution. This is based almost entirely, I believe, upon the assumptions that they are "simple" in some undefined way, and that organic evolution has proceeded from simple to complex — from less differentiation to greater differentiation. Why this should be obvious, I do not know. To me it is far from obvious. (It may also be pointed out that in inorganic evolution (e. g. the chemical elements) it seems to be equally obvious that evolution is from complexity to simplicity.) By making assumptions, and arguing in a circle, we can of course arrive at the conclusion that Protista are "primitive" forms.[1]) But the Protista themselves, as they now exist, furnish us with no foundations for such a belief.

The Interpretation of the Protista.

It has been very truly said by PROWAZEK (1910) that "die Protistenkunde ist auf dem besten Wege, eine selbständige Wissenschaft zu werden" (p. 1). This is unfortunately so true at this moment that if Protistology proceeds much farther along its present path it is likely before long to become completely independent of the Protista in concreto. This is well seen in the adjectives which are customarily applied to the Protista — which are called "primitive, lower, simple, unicellular", organisms. In the preceding part of this paper I have attempted to show that these titles bear only a distant and subjective relation to the objective phenomena presented by the Protista. The present and final section will be devoted chiefly to summarizing to some extent the results of what has been said in preceding sections, and also to indicating what I hold to be the correct interpretation of these organisms.

It is not without interest in this connexion to consider how the fathers of Protistology — uninfluenced by the evolution theory and the cell theory — interpreted the Protista. LEEUWENHOEK, who discovered the Protista (in the seventeenth century), was far from being impressed by their simplicity. His method of reasoning is curious and interesting. When he saw the "tails" of Infusoria (and spermatozoa) moving like the tails of rats and mice, he drew the conclusion that they must be operated also by means of muscles, tendons, and joints. Hence he expresses his wonder and admiration

[1]) "Placed as they (i. e. the Protozoa) are at the lowest limit of animal life, they must ever be closely connected with problems concerning its origin" (CALKINS 1901, p. 4).

of organisms which possess such a multiplicity of structures in so small a body. This seemed quite a natural conclusion to Leeuwenhoek. [1])

Leeuwenhoek's followers, however, were by no means unanimous in their opinions. O. F. Müller interpreted Infusoria as composed of homogenous masses of a gelatinous substance [2]) devoid of differentiation for the most part. His interpretation appears to have been shared by Cuvier, Lamarck, Treviranus, Oken, and many others.

At a later date, Ehrenberg reverted to the earlier interpretation of Leeuwenhoek. He regarded the Protista as highly differentiated organisms, endowed with stomachs, gonads, and other organs. The Protista were, for him, no less complex than other animals. His views, after enjoying a brief and glorious celebrity, were almost completely overthrown by Dujardin.

Just as we may regard Ehrenberg as the spiritual descendant in Germany of the great Dutchman Leeuwenhoek, so may we regard Félix Dujardin as spiritual successor in France to the great Danish naturalist O. F. Müller. For the ideas of Dujardin were essentially the same as those of Müller. He regarded the Protista as organisms composed of a homogeneous gelatinous substance — named by him "sarcode" — which he believed to be similar to the undifferentiated substance present in very young animal embryos.

The Müller-Dujardin interpretation of the Protista was that which prevailed when the cell theory and the evolution theory took a firm hold upon biology. It is the incorporation of these three which has produced the modern interpretation of the Protista. Nevertheless, I believe it is true that the cell theory and the evolution theory contain the Protista in a false and disguised form: and the Leeuwenhoek-Ehrenberg interpretation — though quite wrong in matters of detail — is far nearer to the truth than most modern views.

The modern interpretation is well seen in the following quotation: "Here in these mere specks of animated jelly, which rarely measure more than the hundredth part of an inch, we find, in their simplest forms, the manifold processes of the living organism"

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[1]) It is interesting to compare this with the views which many biologists now hold. For example, a well-known zoologist with whom I once discussed the organization of Bacteria said to me "It's no good telling me they are complex. Even if you can show that they are highly differentiated, it is none the less obvious that things of that size m u s t b e simpler than other organisms".

[2]) "Substantia gelatinosa", "mera gelatina", O. F. M.

(CALKINS 1909, preface).[1]) The truth, however, is that the Protista are very small — but they are not simple. In them, we do not see vital processes in a more elementary form than in other organisms: we see them rather in a more complex form — due to what may be called the "multum in parvo" principle on which all Protista are organized. Physiologists who attempt to analyse vital phenomena by means of the highly differentiated organisms, rather than by means of the Protista, are right. This was clearly recognized by one of our greatest physiologists, MICHAEL FOSTER, when he said — "It is not for me, who in my rash youth had wild dreams of building up a new physiology by beginning with the study of the amoeba, and working upwards, to say one word against the experimental investigation of the lower forms of life. But experience and reflection have shown me that, after all, the physiological world is wise in spending its strength on the study of the higher animals. And for the simple reason that in these, everything being so much more highly differentiated, the clues of the tangles come, so to speak, much more often to the surface, and may be picked up much more readily. Taking again, as an instance, the molecular processes which give rise to the movements of animals, and which appear under such forms as that of amoeboid movement, and that of the contraction of a striated muscle, I venture to think that the very apparent simplicity of the former is an obstacle to our getting a real grasp of its inner nature, and that by our studies of the complex muscle, we are drawing nearer to such a grasp than we could ever have done by observations confined to the phenomena of the amoeba itself. And so in many other instances. The study of the lower forms of life is, in reality, more difficult than that of the higher forms; and the latter naturally comes first."

All attempts to interpret the Protista as elementary or simple organisms have failed. Even those who are loudest in their assertions that they display vital phenomena in their simplest terms do not demonstrate that this is the case. Does it never strike these biologists as peculiar that in almost all discussions of important biological phenomena — such as heredity, variation, sex — the Protista are hardly ever mentioned, or are only considered in a

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[1]) See also the opinion of LAMARCK: "Ces animalcules .... offrent ce qu'il y a de plus simple dans la règne animale, c'est-à-dire, les plus faibles ébauches de l'organisation" (p. 369). "Les monades sont les plus petits, les plus imparfaits et les plus simples de tous les animaux connus" (p. 371) etc.

very brief and parenthetic manner? Yet this is not because they are but little investigated. Thirty years ago relatively very little was known definitely about the Protista: but now a very great deal is known. And still they are "elementary, unicellular, primitive, lower" organisms, displaying life at its simplest!

I will now, in conclusion, summarize as briefly as possible the chief conclusions at which I have arrived in this paper.

First, I think it is desirable that all the organisms which are now miscalled "unicellular", should be distinguished from the multicellular animals and plants. This immense group of living beings may be conveniently called *the Protista* — a name which must be regarded, however, as a mere label, with no more subjective significance attached to it.

Secondly, a protist individual is not the homologue of a single cell in the body of a multicellular animal or plant; but it is homologous with a whole multicellular organism. The protist is a *non-cellular* but complete organism.

Thirdly, the Protista are not properly called "simple", "lower", "unicellular", or "primitive". These are terms which have arisen chiefly through misconceptions involved in the cell theory and the theory of organic evolution. All these adjectives are quite arbitrarily and unjustifiably applied to the Protista, which differ from the Metazoa and Metaphyta in that they are *differently organized* (non-cellular as opposed to cellular).

Finally, Protistology — the study of Protista — when correctly appreciated in this light is one of the most important, but one of the most neglected branches of biology. Since the Protista furnish us with a group of living beings which are organized quite differently from all others, an analysis of their vital phenomena will afford us a large mass of knowledge *for comparison* with that derived from cellular animals and plants — upon the vital phenomena of which, almost all biological generalizations are based.

Literature References.

ARISTOTLE: Historia Animalium, translated by D'Arcy Thompson (Oxford 1910). De Partibus Animalium, translated by W. Ogle (Oxford 1911).

BOURNE, G. C. (1895): A criticism of the cell-theory; being an answer to Mr. SEDGWICK's article on the inadequacy of the cellular theory of development. Quart. Journ. Micr. Sci. Vol. 38 p. 137.

BÜTSCHLI, O. (1880—1889): Protozoa. in: BRONN's Klassen u. Ordn. Leipzig.

CALKINS, G. N. (1901): The Protozoa. New York.

— (1909): Protozoology. New York.

CARNOY, J. B. (1884): La biologie cellulaire. Lierre.

CLAPARÈDE, E. et LACHMANN, J. (1858): Études sur les Infusoires et les Rhizopodes. Genève.

DOBELL, C. C. (1911): Contributions to the cytology of the Bacteria. Quart. Journ. Micr. Sci. Vol. 53 p. 395.

DOFLEIN, F. (1909): Lehrbuch der Protozoenkunde. Jena.

DRIESCH, H. (1907): The Science and Philosophy of the Organism. (Gifford Lectures, Aberdeen.) Vol. 1.

DUJARDIN, F. (1841): Histoire naturelle des Zoophytes. Infusoires. Paris.

EHRENBERG, D. C. G. (1838): Die Infusionstierchen als vollkommene Organismen. Leipzig.

FOSTER, M. (1895): Review of VERWORN's Allg. Physiol. in: Nature Vol. 51 p. 530.

FRANZ, V. (1911): Was ist ein „höherer" Organismus? Biol. Centralbl. Bd. 31 p. 1.

GASKELL, W. H. (1910): Discussion on the origin of Vertebrates. Linnean Soc. London.

GEGENBAUR, C. (1859): Grundzüge der vergleichenden Anatomie. Leipzig.

GURWITSCH, A. (1904): Morphologie und Biologie der Zelle. Jena.

HAECKEL, E. (1866): Generelle Morphologie. Berlin.

— (1878): Das Protistenreich. Leipzig.

HERTWIG, R. (1902): Die Protozoen und die Zelltheorie. Arch. Protistenk. Bd. 1 p. 1.

HICKSON, S. J. (1903): Article Infusoria in LANKESTER's Treatise on Zoology part 1. London.

HUXLEY, T. H. (1853): The Cell-theory. Scient. Mem. of T. H. HUXLEY Vol. 1.

JENNINGS, H. S. (1906): Behavior of the lower organisms. New York.

KENT, W. SAVILLE (1880—1882): A manual of the Infusoria. London.

DE LAMARCK (1835): Histoire naturelle des animaux sans vertèbres. I. Infusoires. 2me ed. edited by Deshayes and Milne Edwards. Paris.

LANG, A. (1901): Lehrbuch der vergleichenden Anatomie der wirbellosen Tiere. Protozoa. Jena.

LEEUWENHOEK, A. VAN: The quotations are from an English translation of L.'s works by S. HOOLE. London (2 vols.) 1798 and 1807.

LILLIE, F. R. (1902): Differentiation without cleavage in the egg of the annelid Chaetopterus pergamentaceus. Arch. Entwicklungsmech. Bd. 14 p. 477.

MORGAN, T. H. (1898): Experimental studies of the regeneration of Planaria maculata. Arch. Entwicklungsmech. Bd. 7 p. 364.

— (1901): Regeneration. New York.

Müller, O. F. (1773): Vermium terrestrium et fluviatilium, seu ... etc. succincta historia. Havniae et Lipsiae.

Prowazek, S. (1910): Einführung in die Physiologie der Einzelligen (Protozoen). Leipzig u. Berlin.

Rolleston, G. (1888): Forms of animal life. 2nd ed. edited by W. Hatchett Jackson, Oxford.

Sachs, J. (1892, 1895): Physiologische Notizen. II. Beiträge zur Zellentheorie, and IX. Weitere Betrachtungen über Energiden und Zellen. in: Flora Bd. 75 u. 76.

Sedgwick, A. (1888): A monograph of the development of Peripatus capensis. Studies Morph. Lab. Cambridge Vol. 4.

— (1894): On the inadequacy of the cellular theory of development, and on the early development of nerves, particularly of the third nerve and of the sympathetic in Elasmobranchii. Quart. Journ. Micr. Sci. Vol. 37 p. 87.

— (1895): Further remarks on the cell-theory, with a reply to Mr. Bourne. Quart. Journ. Micr. Sci. Vol. 38 p. 331.

— (1898): A student's text-book of Zoology, Vol. 1. London.

Siebold, C. T. v. (1849): Über einzellige Pflanzen und Tiere. Zeitschr. wiss. Zool. Bd. 1 p. 270.

Spencer, H. (1864): The principles of biology. London.

Stein, F. (1867): Der Organismus der Infusionstiere. II. Abteilung. Leipzig.

Verworn, M. (1897): General Physiology. 2nd ed. English translation by F. S. Lee, London 1899.

Virchow, R. (1859): Cellular Pathology. English translation of 2nd ed. by F. Chance, London 1860.

Weismann, A. (1883): Essay on Heredity. English translation, Oxford 1891—92.

Whitman, C. O. (1893): The inadequacy of the cell-theory of development. Biol. Lectures Wood's Holl. Vol. 2 p. 105.

Wilson, E. B. (1906): The cell in development and inheritance. 2nd ed. New York.