that germ-free platyfish, *Xiphophorus maculatus,* required live food for their maintenance. This caused many technical difficulties and led to inadequate growth. *Tilapia macrocephala,* on the other hand, are regularly raised on dried food, which can be sterilized easily, and preliminary tests indicate that the dried food does not lose its nutritive properties when it is autoclaved. In addition, fertile eggs are available in large numbers, for the adults reproduce during the entire year at frequent intervals. Many fish can be raised together, they require little care, and the environment can be kept reasonably uniform. What is still needed is a simple device whereby the fish can be fed and whereby gases can be exchanged aseptically.

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References and Notes

1. J. L. Reyniers, *Lobund Repts. No. 1* (1946); *No. 2* (1949).
2. E. S. Shaw and L. R. Aronson, *Bull. Am. Museum Nat. Hist.* 103, 379 (1954).
3. I wish to thank Joel Rodney and Stephen Levine for their technical assistance.
4. J. A. Baker and M. S. Ferguson, *Proc. Soc. Exptl. Biol. Med.* 51, 116 (1942).

12 February 1957

Concerning the "Cellularity" or Acellularity of the Protozoa

Most protozoologists (*1*) probably will agree with Alan Boyden's recent contention (*2*) that it is logical to consider protozoa as "cellular" organisms (that is, cells). In fact, over 40 years ago, Minchin (*3, 4*) defended this notion well, shortly after the appearance of Dobell's vigorous attack (*5*) on the cell theory and its implications concerning the nature of protozoa. However, the recent outburst of criticism directed at Dobell and Hyman (*6*) by Boyden seems rather harsh and narrow. Neglected entirely, furthermore, was consideration of the fundamental question of how, precisely, a cell may be defined; until an entirely satisfactory answer to this question is available, it may be as improper to insist dogmatically that protozoa are unicellular as to claim that they must be noncellular. It is the purpose of the present comment to show that the whole problem is considerably more complicated than Boyden has indicated.

C. G. Ehrenberg (*7*), many years ago, championed the notion that protozoa are "vollkommene Organismen." But, as a consequence of F. Dujardin's exposure (*8*) of the fallaciousness of his contemporary's *morphological* observations, many biologists came to think of the protozoa as simple and as comparable with a single metazoon only when the physiologic (or sexual) life cycle of an entire clonal population was considered. In the light of the atmosphere of the period in which Dobell wrote his trenchant essay, he deserves credit for focusing fresh attention on the fundamental truth of Ehrenberg's idea: in general a *single* protozoon is as capable of independent locomotion, feeding, growth, reproduction, regeneration, and so on, as is any *entire* metazoan organism.

Neither Dobell nor Hyman directly denied the essential homology of nuclei and various cytoplasmic structures that are possessed in common by metazoan cells and the protozoa, since they admitted that the same fundamental organization is to be found in members of both groups. In their insistence that an individual protozoon is, also, homologous with an entire multicellular organism, I think perhaps the only serious breach of logic is the use of the word *homologous* (which is employed directly in this connection, incidentally, only by Dobell). As Minchin (*4*) observed, "the view generally held that the entire organism of a Protozoon is truly homologous with a single body-cell of a Metazoon seems to me quite unassailable. . . . On the other hand, any Protist, as an organism physiologically complete in itself, is clearly analogous to the entire individual in the Metazoa—a comparison, however, which leaves the question of genetic homology quite untouched."

What is a cell? Although it would be inappropriate to offer a lengthy treatment of the question here, we must consider the matter to some extent. Dobell added to the well-known classical definition of the cell the qualification that it "is a part of an organism and not a whole organism." Thus, his insistence that protozoa are noncellular represents a stand not at all inconsistent or illogical, it seems to me, with respect to his own definition of a cell. Minchin himself, Dobell's most outstanding critic, suggested (*4*): "So long as the Protozoa are studied entirely by themselves, without reference to any other forms of life, they may be termed non-cellular in the sense that they are not composed of cells." Hyman also described a cell as "one nucleated division of an organism" (but compare *9*). One must acknowledge that adherents to the definition (be it good or poor) proposed by Dobell and Hyman are placed in an uncompromising position: protozoa are not parts of organisms and thus cannot be cells. The several workers (for example, Lwoff, *10*) on the physiology of protozoa who presumably adopted the acellularity concept may well have been accepting the spirit, only, of Dobell's interpretation in order to emphasize the striking similarities in the biochemistry of the individual protist and the entire metazoan animal.

Nearly a decade ago, J. R. Baker (*11*) criticized Dobell's ideas quite strongly, yet he offered an original definition of a cell ("a mass of protoplasm, largely or completely bounded by a membrane, and containing within it a single nucleus formed by the telophase transformation of a haploid or diploid set of anaphase chromosomes") which, on his own admission, obliges one to consider all "polyenergid" protozoa, including the ciliates, as noncellular organisms. To follow Baker, one would have to recognize both unicellular and acellular forms; the distinction would be dependent solely on the number of nuclei present.

Some biologists have suggested that certain protozoa are truly multicellular in their organization. G. S. Carter's discussion (*12*) is particularly pertinent: he calls attention to the cnidosporidian *Myxobolus,* in which several somatic cells are observable at one stage in the life cycle, endowing the organism with genuine multicellularity.

Thus, it need not be considered altogether illogical to think of the protozoa as comprising a variety of forms some of which are clearly only unicellular, others multicellular in certain stages, still others acellular throughout their lives. Personally, however, I favor rejection of the circumscribed definitions of a cell offered by both Baker and Dobell, and I consider the protozoa, as a group, to be unicellular organisms (not necessarily animals). But the dangers associated with such a generalization should always be kept in mind. A single protozoon is a whole individual—more than the equivalent of a component, dependent part (cell) of a highly integrated multicellular organism. In spite of its morphologic homology with a dissociated cell of the metazoan body, it often possesses an unparalleled degree of subcellular differentiation. Physiologically, as well as morphologically, the majority of the protozoa are independent, complex organisms, far from simple in spite of their typically microscopic size.

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References and Notes

1. Among 20 postwar publications, including text books in the field from five foreign countri which treat all the protozoa, or a particu group, or some major aspect of protozoolo only three strongly favor the acellularity co cept.
2. A. Boyden, *Science* 125, 155 (1957).
3. E. A. Minchin, *An Introduction to the St of the Protozoa* (Arnold, London, 1912).
4. ———, *Brit. Assoc. Advance. Sci. Rept.* 437 (1915); reprinted in *Am. Naturalist* 5C 106, 271 (1916).

5. C. C. Dobell, *Arch. Protistenk.* 23, 269 (1911).
6. L. H. Hyman, *The Invertebrates: Protozoa through Ctenophora* (McGraw-Hill, New York, 1940).
7. C. G. Ehrenberg, *Die Infusionsthierchen als vollkommene Organismen* (Leipzig, 1838).
8. F. Dujardin, *Histoire naturelle des Zoophytes Infusoires* (Paris, 1841).
9. Although Hyman considered the protozoa as acellular throughout her treatment of them as invertebrates, she employed the term "unicellular" in the title of a paper published 2 years later [L. H. Hyman, *Biol. Symposia* 8, 27 (1942)].
10. A. Lwoff, *Biochemistry and Physiology of Protozoa* (Academic, New York, 1951).
11. J. R. Baker, *Nature* 161, 548, 587 (1948).
12. G. S. Carter, *A General Zoology of the Invertebrates* (Sidgwick and Jackson, London, 1946).

4 February 1957

Alan Boyden's (*1*) strictures on an "unwise" attack on the cell theory by Dobell, Hyman, and Lwoff, and by us, are symptomatic of a malaise that is afflicting research on the organization of cellular animals. Comment therefore is in order.

J. R. Baker (*2*), in an exhaustive analysis of the vicissitudes of the cell theory, set forth his understanding of the essentials of the cell theory in the form of seven propositions, to which we adhere (with reservations on numbers 3 and 7). The italics are ours in the following abbreviated restatement of Baker's propositions:

1) Most organisms contain or consist of a large number of cells.

2) Cells have essentially the same nature and are units of structure.

3) Cells arise, directly or indirectly, from preexisting cells.

4) Cellular organisms consist of nothing except cells, transformed cells, and material extruded by cells and by transformed cells (except that in some cases water, with its dissolved substances, is taken directly from the environment into the coelom or other intercellular spaces).

5) Cells are *to some extent* individuals, and there are therefore two grades of individuality in most organisms: that of the cells, and that of the organisms as a whole.

6) Each cell of a many-celled organism corresponds *in certain respects* to the whole body of a simple protist.

7) Many-celled plants and animals probably originated by the adherence of protist individuals after division.

Boyden states that "because of their essential [*sic*] correspondence part for part with the cells of Metazoa, Protozoa are undoubtedly cellular," and thereby begs the question. Later, he says: "Dobell wished to emphasize that an individual protist could act as an individual metazoan, which within limits is true." What "limits"? We fail to see them. Boyden imputes to us "the denial of the essential homologies that exist in all cells." We consider that protozoa are homologous in their general structure to the cells of metazoa but that they have the autonomy of the whole organism. We contend, therefore, that it is necessary to be clear about where the homology of cell to cell ends and where the homology of cell to organism starts. The term *acellular* was coined to avoid an unwarranted extension of the cell theory to protists. We regret that a less tendentious word was not coined; *unicellular* is even less satisfactory. In order not to coin words for this situation, it seems best—and simplest—to put the emphasis on protozoa vis-à-vis metazoa, and to avoid altogether the use, for protozoa, of adjectives containing the term *cell*.

Baker sets the dispute in historical perspective by pointing out that the disagreement about the cell theory has mainly involved two extensions of the original cell theory: (i) that the cell of the many-celled organisms are homologous with the individual protists; and (ii) that this is an essential part of the cell theory. Baker's propositions 5 and 6 amount to a rejection of (i) and (ii). Samuel Butler's epigram, "a hen is an egg's way of making more eggs," is defensible. But emphasis on cell over organism leads to an untenable position: "an organism is a cell's way of making more cells." Patently, this is a fallacy of the cart-before-the-horse variety.

The crucial problem is the *something* which is responsible for the integrity of the organism: can it be homologized with any cell or part of a cell? No indeed. The central problem of embryology is that of differentiation, with its concomitant loss of totipotency (the capacity to form an organism). R. B. Goldschmidt (*3*) concurs in the opinion of Paul Weiss that "the most difficult and most neglected of all basic fields of morphogenesis [is] that of supercellular integration." Hence our italics in propositions 5 and 6.

The malaise to which we refer is a disinclination to face up to the problem of the prerequisites for cellular existence. For example, cancer is a unique disease of cellular organisms. Yet, despite this prod, we are ignorant of the essential, deep-seated coordinations among cells. The newcomer to biology might suppose that the lower metazoa would be favorite objects of investigation, for here one might expect to find these ties in their quintessential and most accessible form. The research scene is depressingly different: the workers engaged in analyzing, in chemical terms, the cellular differentiations of coelenterates and flatworms (and, for that matter, sponges) are all too few. (Boyden is not the object of this remark; he has been working on the organization of *Hydra.*) We have argued (*4*) that this neglect means a stultification of the comparative side of protozoology: how can the hormones of vertebrates and arthropods be traced with assurance to their origins, perhaps as far back as the protozoa or other protists, if information is almost wholly lacking for the lower metazoa? Boyden says that "there may have been a time when the complexity of some [*sic*] protozoans and the integration of the metazoan individual were underestimated"; alas, that time is still now—a fact that is witnessed by the near-total lack of progress on the problem of the origin of any hormone and of the mode of action of any hormone on the target cell (*5*). To dwell on the well-established homologies of cells while playing down their differentiations and interdependencies hardly seems helpful in tackling the problem of the fundamental basis of cellularity.

Furthermore, rigid adherence to 19th century cell theory has hindered medical research as well. Thus, J. H. Kellgren states (*6*):

"After the introduction of the cell doctrine by Schwann in 1819, it became customary to think of the human body as a mass of living cells. Though there is much truth in this concept, it is only half the truth, since more than half the body is composed of extracellular material. . . . Although collagen and elastic fibres were characterized histologically and, to some extent, chemically during the nineteenth century (Hollett, 1871), the personality of Virchow and his dictum, '*Omnis cellula e cellula*,' remained dominant, and the extracellular framework was regarded as so much inert stuffing that could have little bearing upon the problems of health and disease. It was not until 1933, when Klinge showed that alterations in the intercellular substances were the most striking feature of certain rheumatic diseases, that interest in these substances was reawakened, and since then the concept of collagen diseases (Klemperer, 1950) or connective tissue diseases (Kellgren, 1952) has received increasing attention, and it has even been suggested that the state of the extracellular ground substance may control cellular activity (Gersh, 1950)."

Protozoa are organisms first, with some homologies to cells.

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References

1. A. Boyden, *Science* 125, 155 (1957).
2. J. R. Baker, *Quart. J. Microscop. Sci.* 89, 103 (1948).
3. R. B. Goldschmidt, *Theoretical Genetics* (Univ. of California Press, Berkeley, 1955).
4. S. H. Hutner, "Introduction," *Biochemistry and Physiology of Protozoa* (Academic, New York, 1955), vol. 2.
5. D. Hechter, *Vitamins and Hormones* 13, 293 (1955).
6. J. H. Kellgren, *Lectures on the Scientific Basis of Medicine* (Athlone, London; De Graff, New York, 1955), No. 3, p. 78.

20 February 1957

In answering the question "Are there any 'acellular animals'?" in the negative, Alan Boyden (*1*) refers to the cell theory as one of the greatest generalizations of biology. Certainly it has been, and is, of the utmost importance in the very extensive areas where it is relevant. But any successful generalization carries within itself the potential capacity to become a dogma. When that happens, it may well serve as a Procrustean bed into which unhappy facts, if they do not fit properly, must be forced by violence. I wish to call attention to one fact of organization that can be fitted into the cell theory only by such violence.

A large myxomycete plasmodium may contain millions of nuclei imbedded in a common matrix. At its advancing margin, such a plasmodium is essentially naked, bordered only by an ectoplasmic layer from which delicate pseudopodia may extend. The remaining portions, and particularly the veins that are usually its most conspicuous elements, are surrounded by a thick, gelatinous, enucleate sheath, within which the enclosed nucleate protoplasm circulates, and which collapses and is left behind on the substratum as the plasmodium advances. How may such a structure be forced into the cell theory? Two attempts have been made.

One, repeated many times, refers to such a mass as a single, multinucleate cell. As anyone who has cultured plasmodia knows, it may break up into two or a dozen smaller plasmodia, which may combine again in any possible degree. A large plasmodium, cultured in a Wardian case, may cover an area of half a square meter; there is good evidence that, in nature, it may be much larger. If this is to be regarded as a single cell, it must carry with it into the cell theory the concept of an enormous cell that may break up into numerous smaller, but still relatively enormous, cells that may re-fuse into larger units in a sequence that may be repeated an indefinite number of times until it is interrupted by fruiting. To call such a structure a cell seems to go far toward destroying the usefulness of the cell theory in areas where it is really important.

Another suggestion, less popular but not infrequently invoked, is that a myxomycete plasmodium is a multicellular organism in which each cell is represented by an individual nucleus and that portion of the surrounding cytoplasm that may be assumed to be under its influence at any particular instant. Examination of the flow in a plasmodial vein under a moderately high power objective demonstrates that there is no constant relationship between a nucleus and its surrounding protoplasm. This is evidenced by the relative movement of nuclei and visible extranuclear granules borne in the same stream. Such an explanation, then, is purely idealistic and quite completely divorced from observable fact.

It is, of course, recognized that the swarm-cells, which often function as gametes, the zygotes, and the spores of Myxomycetes may legitimately be regarded as cells. But this does not affect the argument for regarding the organization of plasmodia as acellular. Such recognition, it is true, carries further implications. If plasmodia are to be called acellular, what is to prevent one from taking seriously the arguments that have been advanced with respect to the lack of cell organization in mucors and numerous other fungi? Discussion of this problem goes beyond the scope of these comments, but its existence should be noted.

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Reference

1. A. Boyden, *Science* 125, 155 (1957).

4 February 1957

I am pleased that my simple question, "Are there any 'acellular animals'?" has resulted in the expression of opinions both favorable and unfavorable, because there are larger issues involved than the immediate answer to the question. My brief report and the three replies could provide an interesting study in biological thinking.

J. O. Corliss agrees with me in considering the protozoa, as a group, to be unicellular organisms. "But the dangers associated with such a generalization always should be kept in mind." My view is that the dangers of denying this generalization are far greater than those of accepting it. For this is primarily a question of homology, and if, as is generally admitted, the body of the individual protozoan corresponds, part for part, with the individual cells of metazoa, then we must refer to them all as "cellular" (or as "acellular"). The history of our terminology is such that we are required to use the term *cellular* for such protoplasmic organization, and a decent respect for our heritage of words would confirm us in this usage.

It is granted that precise definitions are desirable, but the trouble is that nature presents us with so many variations on a central theme that precise definitions are difficult. The more important capacity is to recognize the central theme —in this case, the general pattern of protoplasmic organizations commonly referred to as "cellular." Only a fundamentally different pattern of organization should be called "acellular," but there is no such difference in organization between the gametes and zygotes, between the zygotes and subsequent stages of metazoa, or between any of these cells and individual protozoa.

The matters raised by S. H. Hutner and L. Provasoli are important in themselves, but I do not agree with them in the belief that progress in the understanding of problems of differentiation and integration requires us to consider protozoa acellular or to believe that cellular stages arise out of acellular stages in metazoan ontogeny. All protoplasmic systems are organized, and there is surely some virtue in accepting this fact as we search for the new truth.

As to the points raised by G. W. Martin, they do not, in my opinion, destroy the validity of the generalization that animals are cellular. There may always be differences of opinion in regard to whether certain plasmodia are unicellular or multicellular, but they remain *cellular* for all that. If fundamentally different kinds of protoplasmic organization in animals should be found, they might become of great interest from the standpoint of primitive evolution, but the cell theory would still stand.

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13 March 1957

Magnetometer Method for Recording Gastric Motility

Although human gastric motility has been recorded for many years, all accepted methods to date, other than roentgenographic, require the subject to swallow the sensing element with its attached tube or wires issuing from the mouth or nose. These leads constitute a marked and relatively persistent noxious stimulus. One of us (M. A. W.) has searched for many years for a practical, bland technique for continuous remote recording of stomach action. With the assistance of many people (*1*) we have devised such a technique (*2*).

The subject needs only to swallow a magnet about the size of a vitamin capsule and then recline on a cot. A detector placed beneath the wooden cot about 18 inches below the subject's stomach senses the field variations resulting from the magnet's presence and motion and translates them into electric variations which are then amplified and recorded. Although a program of direct validation by motion-picture fluoroscopy plus the simultaneous use of a gastric balloon is not complete, the frequencies and relative amplitudes of the recorded waves seem to correlate well with previous results.