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A MYCOLOGIST VIEWS THE INDIVIDUAL*†

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Modern taxonomic procedures differ greatly from those in use when man began to classify his plants systematically. The first steps toward systematization were made by 'plant namers'. Unfortunately we still have 'plant namers' who are unable, or unwilling, to distinguish their procedures from true taxonomic evaluations. The earliest 'plant namers' did more than merely recognize 'photographers' flash images of plants; indeed, some of them observed similarities common to all living organisms. The next steps were directed toward plant describing, and they experienced difficulty in distinguishing clearly between some plants and some animals. However, these describers saw morphological characteristics, and attempted to evaluate them. They were keen observers, and as a result we have profited greatly from their serious efforts to record such characters. They became aware that living organisms represented an association of characteristics, and that in many instances different organisms existed simply because of differences in these characters. Due to the lack of any concept of evolution, they failed to realize the intricate relationships that actually exist among organisms. They had no concept of phylogeny. To them a biological entity was simply a fixed association of characteristics which recurred as frequently as the Creator saw fit to create additional organisms having this same association. In other words, their individual was cut from a static pattern. The static concept precluded any understanding of the significance of the multitude of variations which are now universally accepted in the natural order of things. To them, the thing in itself was more important than the thing in the process of becoming. Until this concept was discarded, no progress toward a recognition of relationships between more or less closely related individuals was possible. This temporal association of characters actually constituted their interpretations of biological entities. When we realize that these relationships of associated characters are not constant it becomes clear that their problem in the classification of plants was very complicated indeed. If we accept their concept of a biological entity we are confronted with a significant problem. Let us admit that the taxonomist is confronted with the complex problems arising from a static, temporary association of characters, and then let us introduce the number of variations which actually appear. Our problem is greatly increased, but still may seem both inconsistent and unimportant to the casual observer. Obviously, a study of the static association of a combination of unvarying characteristics at a given time is much simpler than making an attempt to recognize and/or to organize a complex of variable characteristics into an accurate delimitation of a species. If this were possible and practical, our

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problem would be more easily solved. However, when we consider the numerous phases in the life history of a given individual in the recently propounded interpretation of a biological entity, we are almost forced to conclude that our efforts will be futile. The complications are greatly increased due to the various manifestations of characteristics in some of the very complex life histories. Our considerations become even more complicated when we examine the gradual change from one form of an entity to another form, particularly when the resultant structure is quite different from the structure from which it arises. For clarity, let us apply it to the human species; it simply involves the recognition of the seven (or more) stages of man in the Shakespearian sense. In other words, progressive and successive stages in the aging of plant or animal entities simply represent visible stages in the changing lives of members of a given species. If the subject under observation is seen frequently enough, the observer is not acutely aware of the radical changes which have taken place, but when infrequently seen, these changes are so obvious that they are easily recognizable. Consider meeting an old friend after twenty years. Changes that have taken place are much more noticeable after such a long span of years than they would be if the contacts were made at more frequent intervals. I am amazed frequently when a young, slightly wrinkled, grey-headed person reminds me that he or she had a Botany class with me in the late twenties or at some other time in the antediluvian period of my career in Arkansas. It is surprising, indeed, that young people get old so early in life, and undergo such radical changes when I haven't changed myself. Then, a creak in a joint, or a muscular twinge, reminds me, that I too, have changed, and that no one organism is a constant in any given biological picture. Was the barefoot boy who wandered over the hills in southeastern Indiana from 1907-1912 a different biological entity than the person now standing before you? To be sure both morphological and physiological changes have taken place, but there has been no loss or gain in taxonomic position in the animal kingdom. Nor has there been any change in the artificial name that was given early in this century for social convenience. Doubtless, the alarmingly few persons yet remaining who were early associates of our subject, experience prolonged difficulty in bridging the gap of years, and associating the two (?) or one (?) biological entities. Or can we say that the same biological entity actually still exists 40-45 years later? This brief incursion into the problem of aging seems necessary to establish a mutuality in thinking as we continue our consideration of the problem. It should help us in the establishment of a taxonomic position.

Doubtlessly, physiological changes in a plant or animal are actually much greater than the more measurable morphological ones. (This paper is not concerned with any argument about the relative effects of physiological or morphological changes as the results of such changes become visible, but let it be said that any change in structure is later than the process which induced it.)

If we now venture to adopt the word 'individual' to designate our biological entity, we automatically transfer our speculations from biological entities to individuals. We are immediately confronted with the necessity of interpreting a series of separate structures in the life histories of practically every organism we recognize. These difficulties are multiplied when we remember that certain of these separate structures can exist independently in many species. Furthermore, if we fail to make our observations at exactly the right time, any relationship to any other part of the life history may be overlooked for long periods of time. This actually occurs in the study of the taxonomy of many species of fungi. In fact, the mycologists have a "dumping ground" in which they arrange apparently individual species systematically although they realize they are dealing with polymorphism. These are the 'species' which are placed in the Fungi Imperfecti, and no definite conclusions concerning the complete taxonomic picture can be made until additional information is available about them. This has resulted in a sort of excusable, intelligent confusion. However, these organisms grow and prosper despite the fact that we are dealing with an incomplete cycle; they are apparently unaware of the academic problems involved, and show no concern about them. These facts show conclusively that they are considered biological entities in a taxonomic sense since we assign them their apparently logical position as we attempt to categorize our knowledge while we have it. When additional facts are determined, these fungi are reclassified and placed in the group where they apparently belong at that time. However, they are not completely deprived of their original binomial designation since it is permanently recorded as a synonym. To those persons who have had no experience with taxonomy let it be said, that an extensive series of synonyms simply indicates the taxonomic work that has been done on a given species. Any such list of synonyms is practically meaningless unless considered in this manner. Furthermore, when it is found that some other workers had previously described the plant and given a name to it, the first name because of its priority is given the duty of denoting the species, and all other names are placed in the list of synonyms. Such procedures in naming plants have led to many honest differences of opinions among workers who are more interested in proper taxonomic procedures than in advertising themselves by attaching their names as authorities for different species. It is obvious that some agreement is necessary on the procedures essential to the naming of plants. There are many difficulties to be sure. The efforts which are necessary to check the voluminous literature of taxonomy are tremendous. Such effort is necessary but when it is completely done, it may require a paragraphic check of many periodicals, since new species are frequently described separately in practically any journal which may be in practically any language. Some of the earlier journals are not available in many libraries simply because a sufficient number were not produced for each library to have a copy even if all of them were preserved. Even if such a number were available few workers could read all of the languages involved even if they had the time. In an effort to simplify procedures mycologists accept 1821 as the beginning of modern taxonomic work. All fungi known and described at that time, and included by Dr. Elias Fries in his classical work, are credited to him. For that reason, many species have *Fr.* written after them. One wonders how the late doctor would feel if he knew the tremendous responsibility he has posthumously assumed. In any event, these procedures are practical and offer a starting point for more highly organized procedures.

In returning to our original thought we must agree that any definition of the term individual or biological entity is indefensible unless polymorphism is recognized. Further, we have found that our group of characteristics which we have postulated to represent the individual species varies perceptibly or imperceptibly with the passage of the slightest increment of time. (If we are unwilling to accept this, how can we explain the gross changes which are the recognizable totals of the smaller changes, and which frequently become particularly obvious in later developmental stages?) In fact, these are recognized stages in the life cycle. Consequently, we may assume that any organism may be considered a succession of individuals, and applying the interpretation rigidly we must assume that the number of individuals actually approaches infinity. To most of you this may seem unimportant, but do not forget our taxonomist, who, in his every day procedures is expected to formulate descriptive delimitations of groups of individuals which participate in a given evolutionary series. He must arrange them in a logical and harmonious system. His problem is difficult, indeed, since he must make his descriptions broad enough to include all variations, but narrow enough to designate the species.

Regrettably, too many taxonomists spend too much time during their creative years searching for differences rather than in establishing similarities. Certainly a search for, and recognition of similarities is more constructive than efforts directed toward the detection of obscure differences. This latter activity has resulted in an increased number of new species which we split from recognizable ones. No additional species should be accepted unless it is a real contribution to an organized taxonomic knowledge. True relationships become clearer when thinking emphasizes similarities rather than differences. To be sure the search for positive characters should also bring out the negative ones; the converse of this statement should also be true. In any case the procedure should provide a sound basis for constructive meditation. Our manner of approach seems to be a matter of emphasis, and, regardless of the manner of approach, the end should be the description which gives the most accurate delimitation of the biological entity, species, individual, etc., of the organism being studied.

If we are ready to accept a reasonable synonymy between *biological entity* and *individual,* yet admit temporarily for the purpose of discussion that the former is somewhat more restrictive, certain additional speculations are in order. It may be more appropriate to limit the term "biological entity" to the sum total of the characteristics of a given organism as it exhibits these characteristics at a given time. It will be seen then that the individual may be considered a succession of biological entities if this be true. Should our distinctions of individuals, or biological entities, be based solely upon morphological characters? Or should we consider physiological characters only? Or perhaps we should integrate the two? In general, the use of physiological characters as seen in the classification of bacteria is obviously impractical in the case of the higher and larger organisms. These latter organisms cannot be reduced to test tube size, nor can they be studied microscopically. In fact, it is not possible to make a detailed macroscopic examination of our larger plants 'in toto' at one time, since only a part of the plant can be seen in one field of study. On the other hand, we are able to examine the actual parts of a large plant, whereas in the study of bacteria we do not actually see their parts, but we see only the results of their processes as these processes affect certain substances.

Furthermore, direct protoplasmic observations are questionable at all levels. This is true regardless of the evolutionary position of the subject being studied. In order to observe the details of cells, protoplasm, or the functioning of protoplasm, light of some intensity must be used. It is generally known that light causes certain common chemical reactions. Is it illogical to raise a question about the possible effects of light, and to wonder if such stimulation may not cause profound changes in the quality, and/or, degree of protoplasmic activity? At least, the possibility cannot be ignored. Applying this possibility specifically to bacteria as we characteristically study them, the validity of many of our observations of their morphology and physiology can be questioned. Fortunately, taxonomic conclusions are rarely based on such directly observable characters in the group. Most of the taxonomic studies are simply interpretations of macroscopic observations of completed activities; and are not interpretations of the organisms themselves. They are really derived from the observed effects of completed or progressing physiological processes taking place in the matrix upon or in which the organism is grown. This is taxonomy based upon the effects of organisms rather than upon the organisms themselves. Can these be constant? Is it not generally believed that the age of protoplasm profoundly affects the ability of the cell to react to stimuli? Is it not possible, then, that bacteria undergo a series of changes which are masked and not observable? If our present speculations regarding such unicellular organisms are true, we have similar problems concerning progressive changes in both men and bacteria. The observation of the characteristics of such reactions is much more difficult in bacteria than in man since we observe the composite effects, and cannot remove our subject from the experimental matrix and examine it in its successive stages as we can do with man. Hence, our problem in man is simpler for that reason as well as for the reason that man can actively participate in our experiment by giving us descriptions of reactions, etc. Studies of this kind require the most careful objective study that it is possible for a taxonomist to make.

Are the successive physiological changes which we have considered in man essentially different from the commonly observed polymorphism which occurs in many forms in primitive biological orders? It may be inadvisable to complicate our problem by comparing morphological characters and physiological processes, but a short investigation may be of value. Since the work of Klebs, we have known that environmental factors exert profound effects on the protoplasm of organisms. Klebs found that changes in environment affect protoplasm, and that the structure of some organisms varies in relation to the degree of the effects of environmental factors. Sometimes the resultant new structures exhibited much different functions than the ones which were characteristic of the organisms from which they arose. This, then, is induced variation which must be recognized taxonomically, and interpreted both functionally and structurally. This complication is frequently clearly shown in the stages in the life cycle of many organisms. Clearly, then, taxonomic characters may fluctuate with a fluctuating environment in some cases. If this be true, significant physical and physiological changes may be due to environment. Too, as long as such fluctuations in environmental factors prevail, correspondingly, functions in organisms may occur within a given range. These greatly complicate the work of the taxonomist who must take them into consideration. If we base our opinion on observations of man as he passes through his successive developmental stages, it would seem that man varies less than the lower forms having distinct phases which are not easily associated with the original type from which they have arisen. This precarious opinion may be proved false by investigating some of the more constant species of Algae and Fungi in which pronounced variations apparently do not occur. Perhaps they really do occur and are actually greater than they appear to be,—we may have no instruments with which we can measure them. We must admit the possibility of differences of the same biological stature in the cells of unicellular or multicellular organisms as in the cells of man. However, the sum total of these variations may be different when considered as a whole.

A few specific examples should be timely:

1. *Bacteria* characteristically undergo the process of fission. So as a result, two *biological entities* result from one parent. During the process, the parent disappears, and we believe that each of the two 'daughter cells' contains one-half of the protoplasm, and the hereditary potentialities of the organism. Can we really call these new *entities* individuals, or are they half individuals which have resulted from a redistribution of the possessions of their parents? At the outset, these new entities seem to be just that, but as soon as they start their separate existence the picture changes, or does it? If we apply known principles of Physics, we come to the conclusion that any descendant in a given line, where fission is characteristic, contains a part (although it may be very minute) of the original protoplasmic mass of the first cell in that line. It seems then, that vegetative reproduction of this kind does not permit the complete disappearance of the orig-

inal individual. It must be remembered that we are considering simple fission, and *not* the complex situation we encounter in the development of gametes. One wonders just how different the concept really is, in the latter case.

Let us now consider another happening in the lives of certain species of bacteria. In certain species, spores are formed when conditions cause their development. Despite the significant differences of opinions regarding the mechanics of spore formation, changes do take place in the protoplasm, and a resting cell, or spore, results. Apparently significant changes in the activities of the protoplasmic mass have occurred during the transition to the spore. It is possible to recognize the spore microscopically, and to measure the life processes. These measurements indicate that such processes are proceeding on a much reduced scale. Morphologically the spore is the same unit of structure, excepting, of course, the minor (?) changes that have taken place. The chief differences seem to be physiological. If this latter assumption is granted, we introduce a great number of possible variations into the equation. This emphasizes the increasing complexity of interpreting these happenings, particularly when the intricate relationships of the physiology of an organism to the infinite number of environmental factors is considered. Many such reactions cannot be seen or measured, and perhaps they actually do cause tremendous variations in organisms which superficially seem static. Now if the proper environment is provided, our spore, as such, disappears, and the bacterial cell again apparently assumes the identical activities which characterized it when we began our consideration of it. To be sure, it apparently changes little in a morphological sense; in fact, the nature of the covering membrane is modified and the water content of the protoplasm changes to a greater or lesser degree. Despite these relatively slight visible changes, the resumption of the full physiological potential must be very significant to the organism itself. Should we consider this newly restored individual new, or is it simply a rejuvenated stage of the organism we first saw? If you consider it new, then newness certainly does not have an extensive biological basis. If it is the same original individual the taxonomist must make his description inclusive enough to include both active and static conditions, particularly in a physiological sense. Systems of taxonomy, based on physiological considerations only, present many difficulties because of the unmeasureables involved, and as we learn more about adaptive enzymes, physiological characters become more inconstant. Despite these speculations our question about the spore and its relationship to the vegetative cell still remains. What is the biological entity, or individual? Do we actually have enough information for the taxonomist to do a good job?

2. Let us now consider Oscillatoria, one of the very interesting Blue Green Algae. Historically, these algae have presented many difficult taxonomic problems because of their close relationship to the Bacteria. The position of some species is somewhat uncertain because of the similarities which exist. These algae, like the bacteria, lack complicated structures,

and they do not reproduce by sexual methods. There are exceptions to be sure regarding complicated structures, yet none of them reproduce sexually. One of the more highly developed forms has been chosen as an example. Oscillatoria, as we usually see it, is composed of a number of closely compacted cells in a linear filament and covered by a definite sheath. With an increase in number the filament becomes unwieldy, and some of the cells simply slip out of the sheath. These cells may be single, in pairs or in groups, but in any event a new plant which is similar to the original one develops. Remember now that such new plants may arise from one cell or more than one cell. Apparently, each single cell contains the entire development potential which characterized the original group of cells. In sequence, the new filament develops a new sheath, and additional cells arise by the process of fission. When a number of such cells become associated the filament swings back and forth in a characteristic oscillating movement. In other words, this coordinated activity indicates the functioning of a number of cells as one individual, but it is apparently not necessary to have more than one cell to carry on all the other functions characteristic of the plant. However, one cell cannot be detected in any oscillating movement, nor can the part each cell plays in such oscillation be observed microscopically. What is the individual,—one cell, two cells, or many cells? Extreme difficulty is encountered in recognizing one cell as the plant since it assumes a spherical shape, and exhibits no characteristics which can not be easily applied to other species.

3. Another interesting example is the common bread mold fungus *Rhizopus nigricans*. In this species, polymorphism again complicates our interpretations. This species is heterothallic (i.e. having two distinct thalli, designated + and –). These thalli are essentially identical in appearance but physiologically very distinct. Zygospores are formed only when the two strains are brought into close proximity, but let it be clearly understood that either of the two strains is capable of independent living when they are completely removed from each other. Each one continues to perpetuate the particular strain to which it belongs as long as it has suitable substrata within the range of its physical requirements. Plus and minus distinctions are based upon physiological attributes with no definitely associated variation in form or structure. They cannot be distinguished from each other until they are brought sufficiently close for protoplasmic fusion to take place. In this case we have a fusion of gametangia rather than a fusion of gametes as the first evidence of sexual happenings. Indeed, the fusion of gametes is inferred, but has never been seen. This fusion results in the formation of a zygospore, and if we accept the usual definition of sexual reproduction this zygospore contains the characteristics of both parent entities, or individuals. The details of the germination of zygospores in this species are essentially obscure, but we do find a segregation into progeny of definite *plusness* or *minusness*. The zygospore has been reported to form a hypha upon germination, and, when it does, additional developments must take place before the 'sex' strains are differentiated.

If, on the other hand, as has been reported, sporangia are formed, three possible conditions are recognizable, viz.—the sporangiospores could be all plus, all minus or some proportion of each. Additional work is indicated before all the facts are known.

A more common form of reproduction in this species is completely asexual. Specialized hyphae assume an upright position and globular sporangia develop on their tips. The spores in these sporangia are usually plus or minus depending upon that characteristic of the parent mycelium. These sporangia look alike although they may differ physiologically. In this species our taxonomist must write descriptions which include all details of these polymorphic forms. Fortunately, these various forms are distinct enough for our taxonomist to assign this fungus to its proper position although some parts are lacking. For reasons of brevity, we will omit the homothallic forms except to say that two apparently different physiological entities exist in such close association that the two are commonly called one individual.

4. In the Uredinales, or rust fungi, considerable difficulty is encountered. In some of the species we can easily observe significant morphological and physiological differences, yet, in many species we can 'tie' them together in an interpretation of one individual composed of several separate biological entities. We encounter extreme host specialization in some of the heteroecious rusts, and certain phases of their life histories are spent on widely different hosts such as white pine and gooseberry, etc. The host plants are parasitized by distinct entities which are so characteristic that they were considered individuals until they were recognized as stages in the progressive development of an individual. *Puccinia graminis,* the organism causing Black Stem Rust of cereals, has its O (pycnial) and I (aecial) stages on the common barberry (*Berberis vulgaris*), and its II (Uredinial) and III (Telial) stages on the wheat plant. The O–I stages are usually closely associated in rusts and so are the II, III stages. There are no more similarities between the O–I stages and the II–III than there would be if they were widely removed from each other, yet they are stages in the life history of the same plant. Such physiological specialization and extremely highly developed morphological differentiation, as we see here, represent the highest development of polymorphism found in the Plant Kingdom. The taxonomist must recognize a multiple number of spore stages which occur frequently on fundamentally different hosts, and he must make due allowance for these complexities in his interpretation of the species. Likewise, he should have all spore stages to be absolutely certain in his identification of the species. This is difficult indeed since some of these spore forms occur at one season of the year and others at another season. These seasons may be separated by several months; obviously, the simultaneous assembly of all necessary details is difficult, if not impossible. In other rusts, some of the component stages do not occur regularly, or at least, have never been seen. In still others, the missing forms may appear quite unexpectedly. Obviously, logical taxonomic work in such a

group involves an extremely careful study of the materials at hand. In such cases can we hope to establish a concept of an individual? What are the true relationships of the successive and/or component entities of the developing individual? Each entity has been given a name, and sometimes each spore stage has been given a name. Synonymy in the group is appalling. The late J. C. Arthur studied these rust fungi during the first part of this century and brought partial order out of this natural chaos which had been created by nature itself. It is not surprising that students of this group resort frequently to a *Host Index* to aid them in their work.

5. It has been convincingly shown that modern taxonomy is a complicated study and requires a balance not ordinarily required in many types of biological studies. When we consider certain convenient characters such as color and size, we find that they are frequently so variable that they are unreliable to use, and extremely difficult to evaluate. In the case of *Amanita muscaria* (The Fly Agaric) gradations in color from orange-yellow to brick red have been recorded. This seems to depend upon geographical factors, but the finding of such apparently different organisms in separate localities has led to the description of a number of separate species rather than the recognition of them as members of a single species, despite the variability of the culture characteristic. Such wide variation in color is not uncommon in mushrooms as well as in other plants. Obviously, therefore, it is not good taxonomic procedure to overemphasize such a variable character. Let the taxonomist consider such relative characters intelligently.

6. One more example of a biological structure should throw additional light on the complexities of our problem. Certain species of Algae and Fungi produce zoospores. These are able to move and, at times, represent the only motile structure found in the species. In some cases they arise by internal protoplasmic fragmentation followed by the extrusion of these fragments into the surrounding aqueous medium. In some cases these zoospores give rise to plants similar to those from which they were formed. In other species two of them fuse to form a new individual. In still other species, a number of them coalesce and form a relatively large naked protoplasmic mass in which the identities of the individual spores are completely lost. In addition to this loss of motility, identity, and fusion with similar structures, the resulting matrix (plasmodium) behaves quite differently than at any other time. In some of the Myxomycetes the mass simply undergoes a series of changes, and eventually gives rise to beautifully colored fructifications in which a mass of dry, powdery spores is formed. The spores are scattered by various methods, and when suitable conditions occur the life history starts over again. Some of the foregoing stages are capable of a completely independent existence, and would not be recognized as a stage of the plant if it were not observed as one of the stages in the polymorphic development. Here again we have a relatively large assortment of separate entities which may confuse the most experienced taxonomist unless he is acquainted with the vagaries of the group. The usual procedure is to designate this heterogeneous group of spore stages with one name, and recognize

that the species is composed of a number of separate and distinct biological forms more or less closely related.

The foregoing examples have been cited at random to show the complexity of taxonomic work in the lower plants. Many other equally illustrative examples could be mentioned. The recognition of the relationship of apparently distinct stages is necessary in the study of individual species. Such taxonomic work is extremely difficult, and unless it is approached carefully chaos results. Intelligent considerations of the problems of an individual in its many manifestations can contribute a sound basis upon which to organize the multitude of plants we have.