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THE OPERATIONAL IMPERATIVE: SENSE AND NONSENSE IN OPERATIONISM

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Abstract

Several important terms in biology have recently been criticized for not being "operational." In this paper the course of operationism in physics, psychology and genetics is sketched to show what effect this particular view on the meaning of scientific terms had on these disciplines. Then the biological species concept and the concept of homology are examined to see in what respects they are or are not "operational." One of the primary conclusions of this investigation is that few terms in science are completely operational or completely nonoperational. Some scientific terms, especially theoretical terms, are a good deal less operational than others; but, far from being regrettable, this situation is essential if theoretical terms are to fulfill their systematizing function and if scientific theories are to be capable of growth.

Biologists frequently assert that the need in biology for operational definitions, concepts and procedures is imperative. However, what these biologists mean by "operational" ranges from admirable good sense to utter nonsense. At one end of the spectrum, a concept is operational if in some instances there are ways of discovering if the concept is applicable. At the other end, the meaning of the concept is supposedly equivalent to the set of operations used to test its applicability. Surreptitious vacillation between these two extremes has been characteristic of the movement termed "operationism"[1] since its very inception. The purpose of this paper is first, to describe the role played by operationism in physics and psychology, and then to examine various biologists' notions of operationism to see exactly how appropriate they are for actual biological practice.

OPERATIONISM IN PHYSICS

In 1927 P. W. Bridgman in his famous treatise *The Logic of Modern Physics* suggested that a fruitful way of looking at Einstein's special theory of relativity was to regard the theory as substituting "operational definitions" of concepts like length and non-local simultaneity for definitions in terms of properties:

> We may illustrate by considering the concept of length: what do we mean by the length of an object? We evidently know what we mean by length if we can tell what the length of any and every object is, and for the physicist nothing more is required. To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*

Bridgman's thesis is both radical and poorly expressed. Synonymy is a relation which holds between linguistic entities. Two words or two statements can be synonymous, but a concept cannot be synonymous with a set of operations. Bridgman might have expressed himself better had he said that a concept such as length denotes a set of operations, not a property of the objects being measured. And what is more, the meaning of the concept is this set of operations. Bridgman's thesis is radical because usually the intension of a concept is considered to be its meaning.

One of the main reasons for trying to formulate operational definitions is to insure the objectivity of science. If a scien-

[1] The terms "operationism" and "operationalism" are used interchangeably to refer to the thesis made popular by P. W. Bridgman. Bridgman himself abhorred the terms.

tific concept is synonymous with a set of operations, and if these operations are such that they can be performed publicly by any qualified person, then the intersubjectivity and repeatability so important to the objectivity of science are guaranteed. If a physicist could restrict himself to just those operations connoted by the term—meter readings, manipulations of various instruments, and the like—then operationism *would* insure intersubjectivity, repeatability and objectivity; but from the start Bridgman had to admit the necessity of paper-and-pencil operations, mental operations, verbal operations—in short, symbolic operations. The introduction of such notions *does not automatically exclude* intersubjectivity, repeatability, and objectivity; but it certainly lessens the guarantee initially connoted by the term "operational definition."

Another reason for wanting operational definitions is to promote clarity and precision. If a scientific concept is synonymous with a *uniquely specified* set of operations, and if these operations in turn can be performed unequivocally to obtain precise results, then the concept itself will be clear and precise. For example, the common notion of length is far from precise. Even Newtonian physicists assumed all sorts of things about length which, independent of their acceptance of Newtonian theory, they had no unquestionable right to assume. For instance, they assumed that the results of measuring length by laying measuring rods end to end would be the same as those of light triangulation. This did not always turn out to be the case. If the meaning of a concept is completely specified by the results of a unique set of operations, then when two physicists use this concept each knows precisely what the other means. As Bridgman (1927) put it:

> In *principle* the operations by which length is measured should be *uniquely* specified. If we have more than one set of operations, we have more than one concept, and strictly there should be a separate name to correspond to each different set of operations.

Physicists, including Einstein,[2] soon realized that the cost of this way of obtaining precision was too high. The most obvious objection to the strict one to one correspondence between every set of operations and a separate concept is that it multiplies concepts beyond comprehension. There are too many ways of measuring length. No physicist could reserve a special concept of length for each. These concepts must be somehow integrated. Bridgman sees only a practical justification for treating two operationally different concepts as the same concept:

> Strictly speaking, length when measured in this way by light beams should be called by another name, since the operations are different. The practical justification for retaining the same name is that within our present experimental limits a numerical difference between the results of the two sorts of operations has not been detected.

Two operations are said to define the same concept if the results are the same, otherwise not. For example, measuring rods come in various colors. To test whether measuring length with a yellow rod is the same operation as measuring with a brown rod, a physicist must compare the results to see if they are the same within the limits of experimental error. But perhaps the color of the object being measured matters. Or perhaps there is a significant relation between the colors of the rod and the object *ad infinitum*.

Obviously, the procedure suggested by Bridgman is not and can not be the one actually used in physics. There is a more fundamental justification for considering various operationally different concepts the same concept, namely, physical theory. According to Newtonian theory, certain circumstances (e.g., the movement of the measuring rod from place to place) were

[2] Historically Einstein was affected by the work of Ernst Mach, who tried to formulate operational definitions of all the concepts of Newtonian mechanics and succeeded only in making hash of the theory (Mach, 1960), but nothing in the special theory of relativity requires operationism. See Lindsay (1961), Bridgman (1961) and Grünbaum (1961).

thought to be irrelevant to the measurement of length. As it turned out, Newton was mistaken. According to relativity theory, some of these circumstances must be taken into account; but *some* circumstances are still thought to be irrelevant (e.g., the color of the measuring rod). In fact, the whole notion of a *set* of operations defining a concept presupposes some way of deciding when the *same* operation is being repeated and when two operations are *different* operations. In actual practice, such decisions are made in the context of an interplay between theory and the outcome of experimentation and observation. And with the intrusion of theory comes the possibility of other than observational error.

Two of the underlying themes of operationism have been the essential superfluity of scientific theory and the desirability of making science "safe." If all scientific terms are given operational definitions, then all theoretical terms would be replaceable by observation terms and none but observational error would be possible (Hempel, 1965). Bridgman (1927) expressed himself as follows:

> It is evident that if we adopt this point of view toward concepts, namely that the proper definition of a concept is not in terms of its properties but in terms of actual operations, we need run no danger of having to revise our attitude towards nature. For if experience is always described in terms of experience, there must always be correspondence between experience and our description of it, and we never need be embarrassed, as we were in attempting to find in nature the prototype of Newton's absolute time.

Bridgman hoped to spare us the need for any future Einsteins. This is not possible. The only reason that operationism initially looked attractive to physicists was that physics had a strong theoretical foundation. The lack of such a foundation will have serious consequences for operationism in psychology.

At this point an operationist might object, as did Bridgman (1959, 1961) that operationism is not a full-fledged philosophy of science. It is merely an attitude, a point of view. The unavoidable reply to this objection is that *the operational point of view depends for what success it does have on the very element of science which it is designed to eliminate*. Operationism was intended as a cathartic to purge physics of all non-empirical wastes, but it proved to be so strong that the viscera were eliminated as well. Physics as a theoretical science is possible only on a more liberal notion of operationism; but any notion of "operational definition" sufficient to permit theoretical terms fails to accomplish the end which served initially as the primary motivation for operationism—the desire to make physics strictly and rigidly empirical.

OPERATIONISM IN PSYCHOLOGY

Because of the strong theoretical foundations of physics, extreme versions of operationism were never very influential in physics, and more moderate versions were able to function with some success. The poverty of operationism was not so immediately apparent in psychology. Traditional psychology was fragmented into the introspectionists, physiologists and Freudians. The method of the introspectionists was to analyze consciousness itself, by examining either the verbal reports of a subject or one's own feelings and perceptions. The results were seldom either uniform or comparable. The numerous schools of introspectionist psychology were in constant, unresolvable conflict, with precision, clarity, objectivity, intersubjectivity and repeatability appearing notable for their absence. Freud built a theoretical structure on such data and turned his attention to therapy. As illuminating as Freudian theory may be for the therapist, it is hardly more than a metaphor. No part can be taken literally, and it seems to be suspiciously insulated against disproof.[3] The physiolo-

[3] To discover the latitude that is given the term "operationism," see Ellis (1956) in which "operational definitions" are provided for some of the basic terms in psychoanalytic theory. In addition Else Frenkel-Brunswick (1961) claims that in defining such concepts as the unconscious and instinct, "Freud pursues an essentially operational course."

gists tried the opposite tack from that of the introspectionists and Freudians. They concentrated on understanding the central nervous system; but the complexities of their subject matter proved to be beyond their capabilities, and little progress was made.

It was in the midst of such factionalism that John B. Watson (1913) introduced behaviorism. He called for retrenchment. In his own work with rats, neither reconstructions of the subject's consciousness nor knowledge of its physiology played a part. The subject was viewed as a black box. All that mattered was input and output. Watson maintained that, in order for psychology to become an objective, exact science, psychologists had to turn their attention to that aspect of animals, including man, which is open to intersubjective, repeatable observation—behavior. Anticipating Bridgman by more than a decade, Watson called for "operational definitions" of the traditional mentalistic terms of psychology. Perhaps each of us has direct access to his own conscious states, but in all other instances the only thing we have to go on is behavior.[4] Thus, such mentalistic terms as "thirst" and "intelligence," if they are to be acceptable scientific terms, must be operationally defined by objective indices like time-lapsed-since-drinking and intelligence tests. One's own feeling of thirst or common sense notions of intelligence cease to be relevant to the psychological concepts of thirst and intelligence. In short, Watson wanted to purge science of Bridgman's "mental operations."

Periodically, in their enthusiasm, behavioral psychologists went even further and denied the actual existence of mind or consciousness. Although the major objections raised by the behaviorists to the existence of minds concerned only the *substantial* minds of classical philosophers like Descartes, at times they seemed to be arguing against self-awareness itself, as if the only way to decide whether you were angry was by looking in the mirror. Certainly *sometimes* our behavior leads us to re-evaluate our introspective estimations of our feelings, but these instances are the exceptions. Behaviorists were tempted to make them the rule. Conversely, what of those cases in which we all know that we are thinking, but there is no apparent behavior? In his original (1913) paper, Watson suggested that perhaps the so-called higher thought processes were really faint reinstatements of the original muscular act, imperceptable movements in the larynx!

The complete denial of mind was unnecessary bravado on the part of the behaviorists. They needed only to establish that any reference to the results of introspection was unnecessary in the science of psychology (Bergmann, 1961). Knowledge of behavior and the antecedent conditions were sufficient. The advances of behavioral psychology in the last fifty years have been impressive; but the major stumbling block remains—symbolic operations. This was precisely the obstacle confronting Bridgman in physics. It is one thing to define mass in terms of balances, but the moon, which cannot be put in a balance, also has mass. Fairly complicated mathematical manipulations are necessary. Similarly, it is one thing to define fear in terms of trembling, facial expressions and the like; but the subject can also say, "I'm afraid." To avoid the apparent introspective character of such utterances, behaviorists had to treat them as meaningless noises. Any meaning eventually associated with them had to be derived from behavioral studies. In short, behaviorists were faced with the task of giving a behavioral account of language (Skinner, 1957). Even the most evangelical

[4] This argument presupposes the underdeveloped state of physiology. Physiological evidence fulfills all the requirements for acceptable scientific data. To protect themselves against any possible future advances in physiology, some behaviorists (e.g., B. F. Skinner, 1953) have contended that even if physiological data were available, it would be redundant. Behavioral studies alone would be sufficient. For a survey of the present state of physiological psychology, see Stellar and Sprague (1966).

behaviorist cannot claim that such attempts have been very successful.

Disregarding the controversial issue of a behavioral analysis of symbolic operations, let us recall the difficulties which confronted operationism in physics. If a term is to be defined by a set of operations, what is to determine the membership of the set? When are two operations the same operation? Which circumstances are relevant and which irrelevant? For example, there are numerous ways to determine what is commonly thought of as intelligence—in depth interviews, I.Q. tests, and so on.[5] Each of these is in an obvious sense a different kind of test. The associated concepts, hence, should be different. But even if we restrict ourselves to a single kind of test, say the Stanford-Binet, there are problems. The questions on the test are under constant revision. New versions of the "same" test are periodically issued. Further, is a particular version of the Stanford-Binet test given in the morning on a sunny day in June to a group of fifteen in a small room the same test when it is given in the late afternoon of a somber day in January to a group of over a hundred in a huge auditorium? A tentative answer to the question of what is to count as the same test can be given by comparing results. If, by and large, the same people do well on two different types of I.Q. test, then there is some reason to claim that these tests are measuring the same thing. Further, if results from giving the same test in the morning and the afternoon do not vary appreciably, then this consideration can be considered irrelevant, and so on.

Though correlation in results is necessary to establish what is to count as the same test and to integrate several different types of tests to form a more general concept, it is not sufficient. For example, if a psychologist were to find a close correlation in the results of the Stanford-Binet intelligence test, the Kuhlmann-Anderson intelligence test and a palmar sweating test, it is unlikely that he would integrate them into a single concept. When the actual practice of behavioral psychologists is studied, one discovers that all sorts of considerations enter in which are not justified on strict operational grounds. For example, when I.Q. tests are formulated, the questions must be selected, but on what grounds? How do we know antecedently which factors are relevant and which irrelevant? In point of historical fact, one consideration which psychologists thought was irrelevant is sex. Any question which distinguished between the sexes was eliminated. We know that certain attributes are correlated with sex in people; for example certain diseases. What right has an operationist to assume in advance that sex is an irrelevant consideration in forming the concept associated with the cluster of attributes commonly termed "intelligence"?

Far from arguing that such operationally unwarranted decisions should be eliminated from psychology, I believe that such considerations are *necessary* in order for operationism to get off the ground. In physics these decisions were made on the basis of a highly articulated theory. Einstein did not *discover* that length when measured by different means gave different results. Rather, the theory he devised *required* that the results of certain types of measurement not agree. In psychology there was no such theory. About all psychologists had to go on was the accumulated knowledge of everyday experience, some rudimentary psychological theories and a little knowledge of physiology. They were put in the position of developing a theory and formulating definitions simultaneously. In these circumstances, attempts at operational definitions are more often a hindrance than a help. It is one thing to argue for or against

[5] In my discussion of what is commonly thought of as intelligence, I do not intend to imply that there is a single, unitary, tight cluster of attributes which can be termed "intelligence." It may be the case that under careful analysis, several largely independent factors may emerge. The point is that if a scientist is going to use something as informal as common everyday beliefs about intelligence to guide both the formulation and interpretation of his experiments, he should do so explicitly.

operational definitions as the ideal in a finished science; it is another to show the advantages and shortcomings of operational definitions in the process known as science. Operationism does not provide a principle of meaning adequate for an acceptable reconstruction of science as a finished product, but might it not function fruitfully in the logic of discovery? In an attempt to provide a partial answer to this question, we turn to the history of the gene concept in biology.

OPERATIONISM AND THE GENE CONCEPT

Mendel in postulating unitary germinal "factors" to explain the phenomena of heredity was following a tradition of long standing in biology. The originality of Mendel's work lay in his discovery of the simple mathematical ratios exhibited in the all or nothing transmission of certain "differentiating characters." Mendel reasoned from the discreteness of these differentiating characters to the discreteness of the factors responsible for them. Later Mendelians maintained that these factors were responsible for all the phenomena of heredity and reasoned from their assumed discreteness to the universality of discrete unit characters. Not only were there sharply defined units of heredity, but also the characters themselves were unitary. The one gene-one character hypothesis was born. Castle (1916) questioned the existence of both units:

> The idea of fixity among living things seems to be one which the human mind is loathe to give up and which has to be constantly combatted in the advancement of biology. For centuries it was the fixity of species which dominated biological thought. When the Mendelian theory of unit-characters came in, the idea of fixity, unchangeableness, attached itself to the unit-characters. Driven from this hold, it now seizes on the single factors on which Mendelian characters depend. Simultaneously, it attaches itself to the conjectural mechanism which underlies Mendelian heredity, the chromosomes.

The controversy in genetics over the discreteness of unit characters and germinal factors and the relation between the two was motivated in a large part by the consequences which the resolution of the conflict would have for evolutionary theory. Darwin had steadfastly maintained that variation was gradual. Geneticists such as De Vries used the rediscovery of Mendel's differentiating characters to argue that variation occurred in large steps, producing new species suddenly. As interesting as the controversy between the adherents of continuous variation and those favoring discontinuous variation is in the history of genetics, the purpose of this paper is to investigate the role which operational definitions played in the formation of the gene concept. The notion of unit characters will be discussed in a later section. Did geneticists propose operational definitions of "gene"? Would it have helped if they had?

W. Johannsen introduced the new term "gene" in 1909 for the units of heredity so as to free the concept from any connotations that might be left over from earlier particulate theories of heredity. As he put it:

> The word "gene" is completely free from any hypotheses; it expresses only the evident fact that, in any case, many characteristics of the organism are specified in the gametes by means of special conditions, foundations, and determiners which are present in unique, separate, and thereby independent ways—in short, precisely what we wish to call genes.

The word "gene" as thus introduced may have been free of some hypotheses, but hardly all. In this short statement Johannsen claims that genes are unique, separate and independent determiners in the gametes. The concept of a gamete, for example, may be a good deal less hypothetical than that of a gene, but it is still neither inference-free nor theory-free. We can see gametes through a microscope, but the claim that we are seeing *gametes* goes far beyond what we actually *see*. Our use of the term presupposes theories of embryology and gross genetics which are no less theoretical than the genetic theory under consideration. Similarly, when we look through a telescope at the moon, we see round, concave indentations in its surface.

To call these depressions "craters" is to commit oneself to certain theories of astronomy (Hanson, 1958).

In actual practice, these theories which we tacitly assume are so fundamental and so commonly accepted that it never occurs to us to question them. This is what Johannsen did. He accepted certain common views of embryology and gross heredity as proven and held only the more detailed specification of the attributes of the gene in abeyance. In pointing this out, I do not intend to imply that Johannsen should have been more rigorous in making the term "gene" completely free of theory. On the contrary, Johannsen's retrenchment was fruitful primarily because it was not total. He did not call for the abandonment of the theoretical entities termed "genes." Rather he thought that it was time to decide which of the many attributes assigned to these hypothetical units were most strongly supported by the evidence and which were postulated primarily as the result of a particular theory. The danger of this position is the ease with which it can be transformed into an outright denial of theoretical entities. For example, E. M. East (1912) defended the adequacy of the Mendelian notation to describe the facts of heredity but "only" as a conceptual scheme. Mendelian factors, he maintained, were only somatic characters "poked" into the germ cells, not "biological realities." Somatic characters are concrete facts; Mendelian factors are concepts pure and simple.

> We have a right therefore to poke our characters into the germ cells and to pull them out again if by so doing we can develop—not a true conception of the mechanism of heredity—but a scheme that aids us in describing inheritance.

Perhaps struck by the extremity of his position, East does add an occasional placating qualification. For example:

> The term factor represents in a way a biological reality of whose nature we are ignorant just as a structural molecule formula represents fundamentally a reality, yet both as they are used mathematically are concepts.

Obviously East is in a muddle. Whether "factors" or genes are considered biological realities or mere concepts is not too important. What is important is whether theoretical terms are supposed to be mere summations of observed data or whether it is permissible to go beyond the facts. Those who term theoretical entities mere concepts tend to maintain that as such they must be mere summations of the observed data. Luckily, geneticists from T. H. Morgan (1914) to Watson and Crick (1953) have been willing to go beyond the evidence at hand and to hypothesize about the unknown attributes of the material basis of heredity.

By the mid-1950's the amount of genetic data being collected was rapidly becoming overwhelming. Numerous hypotheses about the nature of the gene had been brought forth. Many had been decidedly refuted, but several conflicting hypotheses, including that of Watson and Crick, still remained viable. It was at this crucial time in the development of the gene concept that L. J. Stadler (1954), in the last paper before his death, called for another period of retrenchment and explicitly advocated the use of operational definitions in genetics:

> The discussion of these difficulties and of the possibility of remedying them by more rigorous definition of experimental concepts is only an application to biology of the operational viewpoint that has become commonplace in modern physics, largely as a result of the critical studies of P. W. Bridgman. As Bridgman notes, this sort of critical reconsideration, made necessary in physics by the development of relativity, is essential in scientific thinking if the methods are to be made elastic enough to deal with any sort of facts that may develop. The essential feature of the operational viewpoint is that an object or phenomenon under experimental investigation cannot usefully be defined in terms of assumed properties beyond experimental determination but rather must be defined in terms of the actual operations that may be applied in dealing with it. . . .

It should be noticed that Stadler has departed considerably from a strict operationist position. It is all right for definitions to make reference to assumed properties as long as the properties are not beyond experimental determination. Stadler draws the line between operational and nonoperational terms on the basis of technical possibility. If we presently have a test to determine the existence or nature of a particular trait, then attributing that trait to the gene is operational. One need not perform that test for every gene in every instance. Definitions for him are not just summations of past data. Stadler spells out his position in more detail when he attempts to define "gene" operationally. He says:

> What is a gene in operational tems? In other words, how can we define the gene in such a way as to separate established fact from inference and interpretation? The definition may take into account not merely the evidence from experiments on the occurrence of mutations but also the evidence from experiments on the inheritance of genetic differences of any kind, or from any other experiments that bear on the nature of the gene. The definition may specify attributes of the gene that can be determined by recognized experimental operations, whether these are attributes already established in past experiments or attributes that might be determined in future experiments.
>
> Operationally, the gene can be defined only as the smallest segment of the gene-string that can be shown to be associated with the occurrence of a specific genetic effect. It cannot be defined as a single molecule, because we have no experimental operations that can be applied in actual cases to determine whether or not a given gene is a single molecule. It cannot be defined as an indivisible unit, because, although our definition provides that we will recognize as separate genes any determiners actually separated by crossing over or translocation, there is no experimental operation that can prove that further separation is impossible. For similar reasons, it cannot be defined as the unit of reproduction or the unit of action of the gene-string, nor can it be shown to be delimited from neighboring genes by definite boundaries.
>
> This does not mean that questions concerning the undetermined properties mentioned are meaningless questions. On the contrary, they are the all-important questions that we hope ultimately to answer by the interpretation of the experimental evidence and by the development of new experimental operations. The operational definition merely represents the properties of the actual gene, so far as they may be established from experimental evidence by present methods. The inferences from this evidence provide a tentative model of the hypothetical gene, a model that will be somewhat different in the minds of different students of the problem and will be further modified in the light of further investigation.

Although Stadler's more liberal view of operationism reveals admirable good sense, it is still faulty in several respects. First, technical possibility determines the properties of the actual gene. What determines permissible properties of the hypothetical gene? Physical possibility? If so, then laws are assumed, and these laws are not specified. Logical possibility? If so, how are we to distinguish between the logically possible and the logically impossible? Providing answers to these questions has proved to be one of the major stumbling blocks of logical empiricism and its stepchild, operationism. Second, Stadler assumes that it is possible to separate "established fact" from "inference and interpretation," that the actual gene can be clearly distinguished from the hypothetical gene. It is certainly true that some observations are a good deal more direct and free from inference and interpretation than others, but no "fact" is sufficiently brute and pristine to be infallibly insulated against the possibility of error. For example, no observation seemed more direct and free from inference than the observation that the earth did not move. The arguments of Copernicus and Galileo that it both rotated and revolved were extremely complex and required considerable interpretation. Nothing like observational confirmation was obtained for the revolution of the earth until the 19th century—long after all scientists accepted it as an indubitable fact. Like Johannsen, Stadler assumes certain highly inferential facts as established and then proceeds to select certain others which are still questionable and in need of further work. As a tactic in the logic of discovery, this procedure has often proved to be quite fruitful; but it must be recalled that Einstein made his great contribution by questioning the most widely and commonly accepted "observational facts" about space and time. And it was Einstein's theory of relativity which prompted Bridgman to set out the tenets of operationism.

It might be thought that when we turn

to those units traditionally held to be "operationally defined"—the muton, recon, cistron and operon—that operationism should at last have found a home. As early as 1940 H. J. Muller recognized that the numerous tests then used to analyze the structure of the genetic material need not be delimiting the same units. The units of crossover, breakage, mutation, function and reproduction need not coincide.

> Although it seems often to have been assumed, there is yet no empiric evidence, and only doubtful theoretical ground, for assuming that the lines of demarcation between genes, as defined on any one of these systems, would coincide with those of any of the others, or even for assuming, in the case of some given one of these systems (especially the mutational one) that such lines of demarcation are necessarily invariable, non-overlapping, well defined and absolute.

Later Benzer (1959) named and explicitly defined some of these genetic units in terms of the tests used to determine them.

> The unit of recombination will be defined as the smallest element in the one-dimensional array that is interchangeable (but not divisible) by genetic recombination. One such element will be referred to as a "recon." The unit of mutation, the "muton," will be defined as the smallest element that, when altered, can give rise to a mutant form of organism . . . a map segment, corresponding to a function which is unitary by the cis-trans test . . . will be referred to as a "cistron."

In 1960, Jacob et al. added the operon to the list of operationally defined genetic units; and still others have been forthcoming.

These genetic units are a good deal closer to the operational ideal than anything met thus far. The tests to decide the size of each of these units are clearly specified. Units determined by different tests cannot be said to be the same units unless the results coincide. But all of these units are defined in terms of what *can* happen, not what *has* happened; and they all presuppose a set of theoretical beliefs about the molecular structure of the genetic material. Neither "recon" nor "muton" is defined in terms of a single, strictly specified test. Any test or experiment which could show that genetic interchange had occurred by recombination would help determine the limits of the recon. Similarly, any test which could show that a portion of the genetic material had been altered would count toward determining the limits of the muton. Only the cistron is actually defined in terms of a specific test, the cis-trans test. The question is—are geneticists interested in those units which happen to be delimited by a particular test in and of themselves or are they interested in those units which are significant in genetic theory?

The main objective of genetics since Watson and Crick proposed their model of DNA has been the gradual deciphering of the genetic code. Certain units which were, in a loose sense, operationally defined have taken on theoretical significance. For example, if it is true that the smallest units of mutation and recombination are single nucleotide pairs, then the muton and recon have properties other than those provided by their original operational definitions. The meaning of the term "nucleotide" is not exhausted by the tests used so far to delimit mutons and recons. The cistron remains more closely tied to the test which originally specified its meaning. It is revealing that geneticists are beginning to question the importance of the cistron as a functional unit. For example, E. A. Carlson (1966) asks, "Is the cistron too rigorously defined for analysis of the functional gene?" The cis-trans test is not important in and of itself but only to the extent that it delimits a theoretically significan functional unit.

The fate of the various operationally defined units in genetics has been either to depart from their original operational definitions and become theoretical entities or to retain their operational character and decrease in importance. The goal of molecular genetics is to discover the fine structure of the genetic material. New and more powerful tests are constantly being devised. Earlier crude tests fall into obsolescence and with them the operational concepts they defined. The importance of the molecular structure of DNA and RNA remains.

At best operational definitions in genetics have proved to be helpful, temporary, half-way houses in the construction of genetic theory.

OPERATIONISM AND THE SPECIES CONCEPT

Chief among the proponents of operationism in biology have been Paul Ehrlich, R. W. Holm, Robert R. Sokal and Joseph H. Camin. As early as 1961 Ehrlich was questioning the usefulness of the biological species concept on operational grounds. A year later, he and R. W. Holm extended the list of objectionable terms to include competition, niche, community, climax, population fitness and, to some extent, even population itself. In addition, Sokal and Camin (1965) consider the concepts of phylogenetic homology, blood relationship and subspecies as well as the principles of evolutionary taxonomy to be nonoperational. It will be the purpose of this section to examine these criticisms in order to discover what the biologists mentioned find wrong with current biological terminology and what relation these criticisms have to the philosophic position traditionally known as operationism.

The concept most frequently singled out for operationist condemnation is the biological species. The biological species concept affords two difficulties to the operationist: first, it is the name of a class of classes and second, it functions as a theoretic term in evolutionary theory. The first of these difficulties presents special logical problems (Buck and Hull, 1966). The word "gene" denotes a class of individuals. The classical gene was thought of as an entity sufficiently unitary and localized to be considered an individual. The word "species" denotes a class of classes. Particular species on any definition are usually treated as classes of individual organisms.[6] The criticisms which have been brought to bear on the biological definition of species because of its status as a class term, if valid, would count equally against *any* definition of species, including the phenetic definition proposed by the critics. Since the phenetic definition is designed to be devoid of any theoretic import, the criticisms directed against the theoretical nature of evolutionary species will not be automatically reciprocal.

In his original paper, Ehrlich (1961) began by pointing out that the actual testing of the interbreeding habits of very many species in nature would be impossible and added that, "even when sympatric organisms can be studied, partial interbreeding may make a clear decision impossible. Genetic tests in the laboratory, while yielding valuable information are never definitive." Even if the biological definition were reformulated in order to consider laboratory tests conclusive, the "amount of work involved in clearly delimiting a single species would be staggering."

The situation which Ehrlich describes is a result of "species" being a class term and is connected in no way with the position traditionally known as operationism. If, in order for a class term to be "operational," the operation must be performed on all of its members or be at least performable (in the sense of being technically possible), then no significant class term in science is operational. One would think that the concept of melting point is perfectly reputable, yet the percentage of elements and compounds whose melting points have ac-

[6] Periodically, biologists (e.g., Ghiselin, 1966) have suggested treating species as individuals. The suggestion is not that a species is nothing but a collection of individuals. On the contrary, it is precisely because they are more than a collection of isolated individuals that biologists have maintained that a species might be treated fruitfully as itself an individual. For some purposes this might well be the case; for others, certainly not (See Gregg, 1950). There are significant differences between the interrelations exhibited by the members of a species and those exhibited by the members of the class of red things. Similarly, there are also important differences between these relations and those exhibited by the parts of an individual organism. For example, the cells in an organism are by and large contiguous, whereas the members of a species are in most instances spatially separated.

tually been determined is staggeringly small. Yet physicists contend that all compounds have melting points. According to thermodynamic theory, they *must* have melting points, even if we don't know what they are. The same can be said for evolutionary species in nature. Ernst Mayr (1965) has frequently emphasized that there *must* be biological species among biparental organisms, whether or not we know what they are. According to evolutionary theory, there *must* be such evolutionary units.

The magnitude of the scientific enterprise does not seem to deter other scientists. Why should it be of special concern to the taxonomist? The answer can be found, I think, in the cataloguing function of taxonomy. People concerned with cataloguing tend to forget that the purpose of a catalogue is to be *used*, not to be completed, like the librarian who complained that she could get her job done if people would just stop taking the books out!

In order for a class term to be operational even in the strict sense, the most that can be demanded is that *sometimes* the operation can be performed. The trouble with classical Newtonian absolute simultaneity was that there was *no* way to establish it because there were *no* instantaneous causal chains. It is indisputable that at least sometimes the interbreeding habits of a species, for example, *Homo sapiens,* can be known in some detail. To be sure, not all actual crosses have been observed, nor has the likelihood of all possible crosses been estimated; but this is hardly necessary. Similarly, in phenetic studies, "overall similarity" is estimated on the basis of a fantastically small percentage of those attributes that could be used, and the sample is taken from an extremely small percentage of the total population. Pheneticists should not set up standards that they themselves can never hope to meet.

The biological definition of species also makes reference to potential interbreeding, and here operational criticisms have some point. Ehrlich and Holm (1963 and 1962) say, for example:

> There are numerous definitions of biological species in the literature, but none is operational, since they all include the idea of "potential interbreeding" and this cannot, by definition, be tested. There seems to be an element of crystal-gazing in the idea of potential interbreeding.

Within a population a sufficient number of the appropriate members actually interbreed (the percentage varying with the species). In addition, several populations may be included in the same species because they are held to be potentially interbreeding with each other; on occasion two or more such distinct populations do actually meet and interbreed.

One major difficulty with the criterion of potential interbreeding is that it is a dispositional property. If the circumstances *were* right, the populations *would* interbreed. One way of testing a dispositional property is to realize it. The claim that the members of a particular class of populations are included in the same species because these populations are potentially interbreeding is partially substantiated if two of these populations happen to meet in nature and actually do interbreed. Two possible misunderstandings must be eliminated at the outset. First, the production of a few fertile hybrids is not sufficient, nor is the interbreeding of every member of one population with every member of the other necessary, for the realization of this dispositional property. Again, what is required is that a certain percentage of the appropriate members of each population actually interbreed and produce fertile offspring; the percentage varies with the species and the selection pressures. Second, the fact that potential interbreeding is a dispositional property does not preclude its being tested. To be sure, the particular pair of populations under discussion are no longer potentially interbreeding; they are actually interbreeding. But the original claim was about a class of populations. These two populations are just samples. Similarly, the claim that sugar is soluble in water can be tested by dropping a few sugar cubes in water. If they dissolve, the claim is in part substantiated. Of course, these particular sugar cubes are no longer soluble; they are dissolved. Sugar, nevertheless, remains soluble. Ehrlich is mistaken when he says that potential interbreeding, by definition, cannot be tested. It may be the case that potential interbreeding often cannot be tested, but it isn't just a matter of definition. Ehrlich probably means that *because* of the reference to potential interbreeding in the biological definition of species, this definition is nonoperational. But on this usage all terms will be operational or nonoperational "by definition."

The minimal justification for the application of a dispositional term to a class is its occasional realization under appropriate conditions, but this is not its strongest justification. Of equal importance are the law-statements concerning the mechanisms of the process. Although you and I might claim that sugar is soluble purely on the basis of past cups of coffee, physicists can back up their claim with much more. They can tell us what it is about the molecular structure of sugar and of water which makes sugar soluble in water. Similarly, even if all the crows that had ever been observed were black, a geneticist would not put much stock in the claim that all crows are black, simply because, given the mechanism of pigment formation, it is unlikely that it will always run perfectly. Albinos are too common. Ehrlich and Holm's uneasy feeling about the biologists' claims of potential interbreeding may result from the fear that too often a comparable justification in terms of the relevant physiological and ecological mechanisms is lacking. To avoid the charge of crystal-gazing, biologists must be able to provide the conditions which, if realized, would transform two potentially interbreeding populations into actually interbreeding populations. Of course, this need not be done in *every* instance; but it must be possible to do it for a representative sample.

In most cases, neither actual interbreeding nor potential interbreeding is asserted on the basis of direct observation of interbreeding habits or of the mechanisms of reproduction. Instead both are inferred from morphological data. The major thrust of Ehrlich's argument is directed against this inference and is, again, independent of operationism. Ehrlich argues that in order to warrant inferring interbreeding status from morphological similarity and difference, the morphological cluster must be tight and gaps between it and other such clusters abrupt. The more gradual the gradations between the clusters, the less warranted the inference. Biologists have long admitted the existence of a high percentage of "poor" species among plants and lower animals; but supposedly among higher animals clearly circumscribed species are the rule, and poorly defined species the exception. To test this latter contention, Ehrlich sampled Nearctic butterflies and discovered no preponderance of well-defined species. Thus, he concluded that there was reason to doubt the existence of well-defined species among other well-studied groups. If even gradation is the rule among higher animals as well as among plants and lower animals, and if even gradation precludes any inference to interbreeding status, then seldom can interbreeding status be inferred with sufficient certainty; and the biological definition is not applicable in most cases.[7]

[7] From the above considerations, Ehrlich in his original paper (1961) concluded that, "the genetic definition of species, never employed in practice, be discarded as an ideal." In his later paper with Holm (1962), the conclusion is that, "the biological species definition never has been operational and never will be." Finally, again on his own, Ehrlich (1964) says that it is pretense to think that species are defined, "by hypotheses about crossing relationships when species are actually defined phenetically. . . . No species has ever really been defined 'biologically,' and it is unlikely that one ever will be: membership or nonmembership is determined primarily on phenetic grounds." The phrasing of these various pronouncements makes it sound as if Ehrlich is confusing the definitions of the names of taxa with definitions of the category term "species." It is certainly true that the name of no particular

The basic assumption of Ehrlich's argument is that definition and inference require tight clusters and abrupt gaps. When interbreeding habits can be ascertained directly, "partial interbreeding may make a clear decision impossible"; and when interbreeding habits are being inferred, the lack of "good" species would make the inference difficult if not impossible. This paper is not the place to argue this assumption. It is enough to remark that the problems presented by even gradation are as great for the pheneticist as for the evolutionist. Any morphological gap that would justify the pheneticist distinguishing two OTU's could also be used by the evolutionist in estimating evolutionary relationship. An explanation for the seemingly higher demands made on the distinctions drawn by the evolutionists than on those of the pheneticists is that the evolutionists are claiming to reflect actual evolutionary development, whereas at times the pheneticists seem to claim that a classification constructed on any objective criterion is valid.

All of the preceding has concerned difficulties arising from "species" being a class term; but the word "species" also functions in evolutionary theory and, hence, in evolutionary taxonomy as a *theoretic term*. There is a continuum of terms in science which stretches from those at one extreme which are relatively free of theoretical import, such as, hard, slick, spot, irregular, and invagination, to those at the other which are intimately associated with a particular scientific theory, such as, electron, atom, inertial gravity, castration complex, cell, gene and species. Often this continuum is treated as if it stretched from observables to unobservables or from real things to mere abstractions. The relation is not that simple. The key property of theoretical terms is the role they play in theories. Observations mark the occasion of their application but do not fully supply their meaning. The surest sign that "species" functions in evolutionary theory as a theoretic term is that if evolutionary theory were abandoned or greatly modified, the meaning of the term "species" would be altered accordingly.

In reply to Ehrlich and Holm's operationally-oriented criticisms of current biological terminology, G. L. Webster (1963) pointed out that many physicists have found that operationism inadequately accounts for theoretic terms in physics. Ehrlich and Holm (1963) countered with the following remarks:

> We admit that our thinking, like that of virtually all present-day scientists, has been influenced by the writings of Bridgman. This does not mean that we embrace operationalism as a philosophy of science, for it does not represent a philosophy and was not proposed as such. Our attitude is well summed up by Rothstein: operational definitions are necessary but not sufficient conditions for progress. Much useful theoretical structure in biology (for example, various ideas about the origin of life, the general theory of evolution) is not amenable to direct operational analysis. However, valid concepts which are formally integrated into such a theoretical framework must be verifiable empirically. Such empirical verification requires operational definition. The interesting assessment of operationalism by Lindsay does not seem germane to this discussion. Concepts such as "species," "niche," and "community" have not been used by biologists as abstruse theoretical constructs linkable only indirectly (or perhaps not at all) with observations. It would be entirely fair to say that many biologists are under the impression that species, niches, and communities are *things observed.* If these concepts are to be useful to biologists, their operational definition is a *sine qua non.*

One is at a loss as to the spirit in which these comments were offered. In their original paper (1962), Ehrlich and Holm

species (the name of a taxon) has been defined in terms of interbreeding or potential interbreeding. The biological definition is supposed to define "species" itself (the name of a category). It is also true that in no instance has the interbreeding status of every member or even every living member of a biological species been determined. A few may have been observed actually to interbreed, but the vast majority of individual matings are not observed. Then, of course, there are all those individuals which have not actually interbred but are only potentially interbreeding with one another. Ehrlich may intend to be arguing that there is no way ever to determine whether two individuals are potentially interbreeding.

point out the, "unfortunate tendency of some nontaxonomists to treat species as entities." They then turn around and use this tendency, which they take to be mistaken, to argue against the theoretic nature of the species concept. Even if species were treated as entities or things, it still doesn't follow that they must necessarily be observable in any straightforward sense of observation. Species have as their members individual organisms which exist in the paradigmatic case of existence and can be observed in the paradigmatic case of observation, but it is the complex interrelations which these individuals have to each other that lead biologists to treat species as entities analogous to organisms (See footnote 6). Some organisms interbreed with each other, some are descended from others, still others compete for food or mates, and so on. Which of these relations are to be recognized in the definition of "species" is determined at least in part by the role the term plays in evolutionary theory. For example, Ehrlich was previously quoted as saying that the biological definition could be made more operational if it were reformulated in terms of genetic tests, but evolutionists could obtain this increase in operationism only at the expense of theoretic significance. Genetic tests are only evidence of the evolutionary status of a group. Similarly, the concept of intelligence, as defined by a particular I.Q. test, is operational; but its significance is limited. It is at best only indicative of future performance outside a test situation —and this is what is important.

Ehrlich and Holm say that they do not embrace operationism as a philosophy of science, and yet the position which they describe concerning theoretic terms is operationism in its strictest form. To be valid a concept must be verifiable empirically. From what they say elsewhere, Ehrlich and Holm cannot mean "verifiable" in the technical sense of complete confirmation. They recognize *degrees* of operational meaning. For example, they complain that, "many concepts in population biology have low information content and little or no operational meaning" (Ehrlich and Holm, 1962). The problem is *how much operational meaning is sufficient to make a scientific concept acceptable?* They say that ecologists are attempting, "to develop an operational definition of 'niche'—that is, to specify the set of physical operations which would assign to every niche a unique value . . ." (Ehrlich and Holm, 1962). Will anything less do?

Sokal and Camin (1965) are no clearer on this crucial issue. They distinguish between empirical, operational and numerical taxonomies which overlap but need not be co-extensive. They defined "operationalism" as follows:

> Operationalism in taxonomy (as in other sciences) demands that statements and hypotheses about nature be subject to meaningful questions, i.e., those than can be tested by observation and experiment. If we cannot establish objective criteria for defining the categories and operations with which we are concerned, it is impossible to engage in a meaningful scientific dialogue about them.

But their comments in other places are inconsistent with this reasonable position. Sokal (1963) dismisses the biological species concept, because, in the vast majority of the cases, the genetic criteria cannot be applied:

> Much has been made in the new systematics of the so-called biological species concept. However, many of the criteria, as a matter of fact, all of the criteria of the species concept, are of the kind that Bridgman would call nonoperational because for the vast majority of all species that have been described or are going to be described, the criteria of the genetic biological species concept are just not going to be applied.

Sokal and Camin (1965) term "blood relationship" nonoperational, not because it cannot be applied in the vast majority of cases, but because its, "meaning can be made precise only in mendelizing populations but not among species of higher taxa. . . ." And when they provide an example of an operational statement, it seems to be formulated so that it could be applied in all cases:

Thus, for example, the statement that resemblances among some OTU's are to be measured in terms of characters X, Y, Z, coded into character states i, j, k, treated by standardization of characters and correlation among OTU's is operational.

In order for a concept to be operational, must it be applicable in *all* cases, in *most* cases, or in *some* cases? The answer, in all cases, can be shown to be too strong for most scientifically significant terms. If the answer is in some cases, only the most metaphysical terms fail to fulfill this requirement. If the answer is somewhere between all and some, the question remains, "Where?"

OPERATIONISM AND OPERATIONAL HOMOLOGY

In the preceding section, Sokal and Camin provided an example of an operational statement. In their example, resemblances among some OTU's are measured in terms of characters X, Y, Z, coded into character states i, j, k. As it stands, this statement is neither operational nor non-operational, since the characters and character states have not been specified. The statement which results from their specifications will be operational only to the extent that these characters and character states are themselves operationally defined. The purpose of this section will be to examine various attempts to define "character," "homology" and "unit character" operationally.

Sokal and Sneath (1963) define a character as any feature which varies from one individual organism to another. Thus, if specimen 241 is blue and specimen 242 is red, then being colored is a character and being blue or red are its states. Colless (1967a) has suggested that color be termed a character and blue color and red color be termed attributes. Several other systems of terminology have also been suggested. Until one becomes established, I hope I will be excused for not adhering very closely to any of them. Although the decision to consider color a character and blue a character state was made on the basis of a single variation between two specimens, the terms "colored" and "blue" are not the proper names of that particular instance but the names of classes. To speak extensionally, "colored" denotes the class of colored things; and "blue" denotes the class of blue things. The class of colored things already has two members (specimens 241 and 242). The class of blue things has only one member (specimen 241). It may even turn out that specimen 241 is its only member, but it will nevertheless remain a class. It could have other members.

It follows from these considerations that the word "character" denotes a class of classes. Thus, defining "character" is logically on a par with defining "species" and the usual logical difficulties should be expected (Buck and Hull, 1966). The following diagram might help in visualizing the parallels between the two cases:

species		genus	character state		character
$\in$		$\in$	$\in$		$\in$
Homo sapiens	$\subset$	*Homo*	red	$\subset$	colored
$\in$			$\in$		
LBJ			instance 242		

Closely associated with the notion of character are the concepts of operational homology and unit character. A definition of "operational homology" must provide criteria for deciding when two instances of a character are to be classed together as instances of the *same* character. More often, however, a definition of "operational homology" is intended to provide criteria for deciding when two antecedently constructed classes should be collapsed into a single class or a single class divided into two or more classes. Sokal and Sneath's (1963) decision is to call two characters or character states the "same" whenever they are indistinguishable.

One immediate source of confusion is the type of indistinguishablity intended by Sokal and Sneath. Must the distinctions be made just by the unaided senses, or can scientific instruments, theories of measurement, formal calculi and the laws and theories of empirical science also be used?

The question is an important one. If decisions concerning operational homology are confined just to direct observations independent of any other consideration, then one might plausibly claim that operational homologies are *observed*, whereas evolutionary homologies must be inferred *a posteriori*. Pheneticists often claim that phenetic taxonomy and operational homology are in some sense basic to evolutionary taxonomy and evolutionary homology. For example, Sneath (1961:124) says:

> It is often said or implied that taxonomy should be based on phylogeny. The truth is that to all intents and purposes phylogeny is based on taxonomy. . . . After all, if I wish to know the probable ancestors of living horses, I do not go to the fossil echinoderms and assert that a sea urchin is a fossil horse; I pick a fossil which shows considerable similarity to living horses. In other words, from an intuitive estimate of their similarity—a taxonomic estimate—I deduce a phyletic relationship. If you prevent my taxonomic estimate, by blindfolding me, I would have no idea of the phylogeny of the specimens; obviously, therefore, I could never deduce their taxonomy either.

No taxonomist thinks he can observe that two instances of a character are homologous in an evolutionary sense. This he must infer from observations. If the phrase "taxonomic estimate" is expanded to include simple observation, then phenetic taxonomy is certainly basic to evolutionary taxonomy, since observation is necessary for any empirical science. No taxonomist can infer evolutionary homologies blindfolded! But on this interpretation, the claim that phenetic taxonomy is basic to evolutionary taxonomy becomes fairly unexciting. It also becomes basic to quantum physics.

As unexciting as this position might be, it can be held only if taxonomists can observe that two instances of a character are operationally homologous without recourse to any inference whatsoever. This view, in turn, gains its air of plausability from the ease with which the simplest observations of color, shape, sound and the like as properties of *physical objects* can be confused with the *sensations of the observer*. Is the specimen blue or is the observer having blue sense data? Are taxonomic characters attributes of the specimens or the taxonomist? If the term "blue" refers to a sense datum, then further analysis of this character state would result merely in smaller patches of blue. Decisions as to operational homology not only can but must be made just by the unaided senses. If two shades of blue *look* the same, then they *are* the same.[8] However, if the term "blue" is an attribute of an organism, then all sorts of additional considerations come into play. For example, the presence of a pigment and the molecular makeup of that pigment become relevant. In this case estimations of operational homology depend on inferences as much as do estimations of evolutionary homology, and the claim that phenetic taxonomy and operational homology are basic to evolutionary taxonomy and evolutionary homology becomes less obviously true. Observation becomes basic to both phenetic and evolutionary taxonomy. Both operational homology and evolutionary homology must be "deduced."

In their discussion Sokal and Sneath make it clear that they are concerned with much more than just direct observation by the unaided senses and that they are talking about the fine structure of organisms, not sense data. For example, two specimens might both *look* blue; but if the blue color in one is due to pigmentation and in the other to optical interference phenomena, then the two characters are not operationally the same. If we limit ourselves just to the blue color due to pigmentation, the pigments may have different molecular structures. Hence, additional operationally different characters must be distinguished. Further, two molecules can be identical in their structure; but if they have been produced by different biosynthetic pathways, then they are distinguishable and operationally different. But this analysis must end somewhere.

The purpose of defining "unit character"

[8] To my knowledge the only biologists who have suggested that classification is a matter of "clipping" together sense data are J. S. L. Gilmour (1940) and Donald H. Colless (1967a & 1967b).

is to establish a lower limit to such progressions. Sokal and Sneath (1963) define "unit character" theoretically as, "an attribute possessed by an organism about which one statement can be made, thus yielding a single piece of information." This definition is theoretical only in the sense that it makes reference to a formal theory—information theory. Sokal and Sneath provide some empirical content to their theoretical definition when they tentatively identify their taxonomic bits with the genetic code. They observe that the progression of physical analysis must end somewhere, even if it is at the fine structure of the genetic material. Their definition is now theoretical in the more important sense that it makes reference to an empirical theory—genetic theory. Sokal and Sneath also provide a working definition of "unit character," since taxonomists must and generally do classify in the absence of any knowledge of the molecular mechanisms which have eventuated in the phenotypic characters of the organisms under study. The difference between these two definitions is that for the working definition, the analysis need not be carried all the way to the fine structure of DNA. The lower limit is set by our present knowledge and technical ability. "The presence or absence of a bristle in an insect may be a unit character, if we know nothing of its finer structure and have no way of subdividing it."

The upshot of Sokal and Sneath's discussion of characters, operational homology and unit attributes is that they can no longer complain that a phylogentic concept of homology is, "not susceptible to direct proof but only to proof-by-inference." This is equally true of operational homology. If the concept of evolutionary homology is in some sense "nonoperational," it will have to be something other than its inferential status that makes it so.

One possible difference between estimations of operational and evolutionary homologies is the theories used in making the necessary inferences. Pheneticists may be able to limit themselves to highly confirmed and unproblematic theories, whereas evolutionists are committed to evolutionary theory. Several biologists have recently provided tentative explications of the notion of operational homology. One thing that these various explications have in common is that they are all primarily concerned with geometrical considerations. For example, William G. Inglis (1966) says:

> When a feature shown by one organism is compared with a feature of a different organism and the two are considered to be absolutely equivalent, it is because they are the same shape (approximately, but not always) in both, and occur in the same position relative to other features in both organisms and to the total outline of both organisms.

Rolf Sattler (1967) in a paper on petal inception refrains from calling the two primordia with which he is dealing petal and stamen primordia to avoid distortion in comparison. Instead he calls the first primordium to develop primodium I and the second primordium II. He then concerns himself only with their spatial arrangement. Key (1967) talks about tracing outlines of West Africa and South America in order to homologize their coastlines. What is the point of treating the determination of homologies as if it were an exercise in geometry? One answer is that the inferences licensed by geometry are safe. Ideally estimations of operational homology should be matters of direct, inference-free, theory-free observation; but if inferences must be admitted, those of the formal sciences are the safest. Maybe some reptiles don't have three-chambered hearts, but all triangles have three sides. As might be expected, none of the biologists cited actually limits himself to direct observations of shape and relative position. As Key (1967) says, "In reality, the attributes taken into consideration go far beyond the geometrical ones." Shape and patterns are just a beginning. Cellular structure, embryological development, and so on also matter. The crucial question is whether evolutionary development is also relevant. Why is it that among all scientific theories, only recourse to evolutionary theory is illicit?

One objection commonly raised against evolutionary theory is that it is "historical," since phylogeny is a unique, non-recurrent series of events. This objection stems in part from confusing phylogeny with evolutionary theory. Phylogeny takes place in time. In any description of a particular phylogenetic sequence, a specific segment of time is designated as the period during which the phylogeny occurred. Evolutionary theory, however, is also concerned with the causes and mechanisms of evolution as such. The law-like statements of evolutionary theory are not unique in claiming that whenever conditions sufficiently similar to a certain kind recur, events quite similar to another kind are likely to eventuate (See Goudge, 1961). As Ernst Mayr (1965) has said:

> To establish unequivocally the fact of evolution was after 1859 the first concern of the young science of evolutionary biology. The study of phylogeny soon became predominant, at least in zoology. Indeed, even today there still are some zoologists to whom the term "evolution" signifies little more than a determination of homologies, common ancestors, and phylogenetic trees. By far the majority of evolutionary biologists, however, have shifted their interest to a study of the causes and mechanisms of evolutionary change and to an attempt to determine the role and relative importance of various factors.

Why can embryological theory be used to determine courses of embryological development and biochemical theories to determine biosynthetic pathways, while evolutionary theory cannot be used to reconstruct phylogenetic development? It has been objected that a particular evolutionary sequence is over and done with and irrevocably in the past. The data needed to reconstruct it may well be lost. But a particular occurrence of a biosynthetic process is also over and done with and irrevocably in the past. The evidence needed to reconstruct it is even more likely to be lost. The difference between the two situations is that in the case of biosynthetic pathways, another instance of the sequence can be produced here and now. Recurrences are numerous, regular and quite similar. In the case of evolutionary sequences, additional instances of a sufficiently similar kind are hard to come by. There are evolutionary patterns that recur, but they do so irregularly and, from our point of view, too slowly. Are these sufficient grounds for dismissing the concept of evolutionary homology as nonoperational? No one seems to mind physicists talking about the half life of uranium 238, although no physicist is going to live the 4.5 billion years necessary to observe a particular sample decay. Other scientists are permitted recourse to well-confirmed scientific theories even if they concern very long, drawn-out processes. Biologists, it would seem, have a comparable right.

In summary, pheneticists criticize the concept of evolutionary homology for depending on inferences and theory when their own excellent discussion of character, operational homology and unit character are equally dependent on inference and theory. Thus, if evolutionary homology is still to be dismissed as nonoperational, or at least less operational than operational homology, pheneticists must explain why. If reference to theory as such is not at fault but only reference to evolutionary theory, then pheneticists must explain what it is about evolutionary theory which assures the nonoperational character of any concept which assumes it.

CONCLUSION

I would like to conclude this paper with a parable. So the story goes, early date farmers noticed that only half their trees produced dates. To increase their yield they began to weed out the "sterile" trees. For a while they were successful, until the last sterile male tree was cut down. Then all the other trees stopped producing. Theoretical terms are like the male trees. They are not completely operational, but they are necessary for the progress of science. Operationism is fruitful only when it is not total. Strict operationism is incompatible with theory and more moderate versions impossible without it.

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