

A Form of Logic Suited for Biology^{*)}

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I. Introduction: Biology versus Physics

A comparison of two sciences might appear as a rather primitive exercise. It is introduced here only because there are still many who insist on the purely empirical character of biology, i.e., the absence of a meaningful "theory" of biological phenomena. If a person enters the subject matter of biology coming from physical science, as this writer did, he cannot help but be intensely aware of the difference in character of the two sciences. It soon becomes clear that what may be called the hierarchical structure of physics has little or no counterpart in biology. By hierarchical structure we mean the possibility, which normally exists in physics, of condensing a more or less extensive area of experience into one formal statement, usually a set of differential equations from which, by mathematical methods, the description of those phenomena can be deductively derived. Nothing of the sort has ever been found in biology except in those limiting cases of physiology where the behavior of living things reduces to an application of traditional principles of physics.

Since physics deals primarily with extension, its chief tool of description is the *continuum*. Biology, on the other hand, starts with taxonomy; correspondingly, the dominant concept in biology is that of a *class*. The difference between the two terms here italicized is found indicative of the distinction between the methods of physics and those of biology. Historically, the gap between those two concepts began to narrow when in the later nineteenth century mathematicians resolved continua into *point sets* (as a rule infinite); this gave rise to a logic of such sets (set theory). Now by their definition, which goes back to Euclid, points do not have an internal structure. When in the early years of this century Whitehead and Russell succeeded in combining logic and mathematics into one edifice, modern mathematics "took off." It is almost entirely based on sets whose elements are assumed to have no internal structure. This makes modern mathematics ideally suited for dealing with the constituents of matter discovered by the physicist. It is an experimentally well-established fact that those constituents, electrons, protons, and so forth are indistinguishable; their properties are such as to make these particles rigorously identical with each other. The more refined experiments allow one to specify this identity quantitatively to many decimal places. It is a well-established principle of physics that when one forms a class of, say, electrons, all elements of that class are strictly indistinguishable; it is as a matter of principle impossible to "label" the members of such a class so as to distinguish them individually. We shall speak of classes with this property as *perfectly homogeneous classes*.

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Living things, on the other hand, are composed of many molecules. On leaving aside the limiting case of viruses for which the question of whether they are really living is not yet fully decided, apart from this, organisms can safely be assumed to be individually different from one another. This will be discussed in much more detail later on. Let us now only say that in all higher organisms this fact can be verified by direct observational inspection. If we assume the same to hold for lower, unicellular organisms, we are making a plausible generalization that is not contradicted by any known experience; in fact the observationally established adaptability of even primitive organisms makes the assumption that the elements of any class of organism are somewhat different from one another an extremely likely one.

Classes in which the elements are individually different from each other will be designated as *heterogeneous classes*. Thus, the description of biological states and processes is carried through in terms of heterogeneous classes. Ordinary logic pays little attention to the heterogeneity of classes. The basic abstract operations usually performed on classes (the junction and intersection of two classes) are of little interest in biology. There are two basic operations on heterogeneous classes which will be discussed later on; let me here just state them:

- (1) The selection of subclasses of a given class, such as to be of "greater homogeneity" than the original class. (Putting the term in quotation marks indicates merely that it is used in an intuitive manner and no quantitative measure of homogeneity has yet been defined.)
- (2) The inverse operation of selection will be designated as *embedding*; this is the construction (or demonstration of existence) of a larger class such that the class originally given is a subclass of this larger class.

It is found that a great many problems of "theoretical biology" can be tackled by means of formal operations applied to heterogeneous classes. Their clarification has, however, developed but slowly. (This is at least the impression of the present writer, who started as a theoretical physicist and later in his life became interested in applying the physicist's more abstract methods of analysis to the data of biology.) In such a clarification one is soon confronted with the well-known fact that few things in the scientist's world are more sterile and hence also more boring than sheer, abstract methodology. Methods, therefore, should only be developed in the context of a more concrete inquiry for which they constitute the tools. This author has in the course of the last quarter century written three books (Elsasser, 1958, 1966, 1975) which deal with an approach to theoretical biology that differs from others by a greater emphasis on the modes of thought of the physicist. This work is sufficiently specific to have, hopefully, avoided the pitfall of pure methodology. In the present review I have extracted and condensed those aspects of my method that deal with the adaptation of ordinary logic to the special requirements of biological science.

For nearly a century now, physicists have been imbued by a mode of thought designated as "positivism." My above-quoted three books may also be thought of as an effort at applying the physicist's positivistic mode of thought to the empirical material of biology. Here, we shall be interested in those aspects of positivism that refer to logic. Let me remark that it is certainly more than a coincidence that Aristotle, the founding

father of scientific biology, was also the founding father of logic. It should then not be too surprising that an inquiry into matters biological turns into a discussion of logic.

But one finds here one of the worst cases of intellectual confusion that have occurred in the history of human thought. The term 'positivism' was created by the Frenchman Auguste Comte (best known as the founder of sociology). It appears in the title of his work, *Course of Positive Philosophy*, which came out in the 1830s. From here Ernst Mach, who initiated the analysis of the scientific concepts used in physics, borrowed the term positivism. From this, there came also the tendency to think of this type of analysis as a form of philosophy. This arose out of an earlier stage of thought when science grew up under the tutelage of philosophy as was the case in the late Middle Ages and even later. But dragging in "philosophy" puts science into an unnecessary, only historically founded straightjacket.

Since the connection of positivism with traditional philosophy is so often reiterated in the literature, we propose here a way by which the scientist can try to dissociate himself from this connection: at first simply a different terminology. We propose the term structuralism to replace positivism. We begin by leaving everything except the terminology unchanged. We are here primarily interested in extending the method of structuralism ("positivism") from physics to biology so that it comprises all natural science. In advance of this extension we shall describe in a few words the way in which structuralist analysis is used to improve the precision of scientific thought in modern physics. This recourse to physics will give us occasion to consider the nature of structuralism and the general principles on which it is based. Only after this has been done shall we apply a similar type of reasoning to some abstract aspects of biology, which appear here in the form of a logical theory of heterogeneous classes.

II. The Structuralist Method

As just announced, we begin our presentation of structuralism in physics, the science where this mode of thought and analysis was born. We do possess a quite unusually clear exposition of the method by one of the great men of science, the astronomer Arthur Eddington (1939). The title *The Philosophy of Physical Science* together with the book's appearance at a critical juncture of political history are probably the reasons that the book has not received even a small fraction of the attention it deserves. What we readily notice is an implicit pointing to the past ("philosophy") rather than an outlook upon coming developments. Eddington speaks of the positivistic interpretation of the philosophy of science; we shall here speak of an independent foundation of physics by the use of the structuralist method, both being the same except for terminology.

The central idea of Eddington is that of an abstract structure. This is a scheme of mathematics in which certain symbols are defined, not by pointing to objects of our external experience but solely by mutual, equally abstract relationships between component symbols. Thus, for instance, in arithmetic the operations of addition and multiplication define a set of number symbols. The most ancient abstract structures known are Euclidean geometry and arithmetic (called algebra by mathematicians). For most of recorded history these two structures were considered unique. The famous philosopher Kant as late as the second half of the eighteenth century declared that space

was a "form" of human perception, given "a priori," as he called it. This is not really very different from the idea expressed by Newton a century earlier, to the effect that space and time are the "sensorium of God."

A radical revolution occurred in this mode of thought in the nineteenth century when mathematicians discovered that there was not just one geometry, the Euclidean, but many geometries differing from each other by their underlying axioms. Similarly, they found that there is not just one algebra, the one taught to children, but a vast variety of abstract algebras differing again in their axioms. Mathematics at present knows a nearly inexhaustible plethora of purely abstract constructs of this type.

Some of these abstract structures can be used to represent the physicist's observations. I presume that most of my readers are acquainted with the fact that the theory of general relativity uses four-dimensional curved spaces. Quantum mechanics in its turn uses, instead of conventional school algebra, a noncommutative form of algebra combined with certain other abstractions (Hilbert spaces and Hermitean forms) that play a role in higher mathematics.

Now the structuralist method of theoretical physics consists in this that certain properly chosen abstract structures are used as images of a body of observational data. No philosophical or theological preconception enters this process because the abstract structures are in the first place defined so as to be independent of any human experience that would go beyond constructions of pure mathematics. Eddington points out a number of implications of this method, of which I will mention here only two. First, one may try to define the aim of scientific theory within the framework of such a method. As Eddington emphasizes, the principal aim of any scientific theory of physics is not to "explain" phenomena but simply to describe them. The concept of explanation as used in physical science can have two different meanings. First, it can indicate model making that tells us how a certain natural process works; for example, one can model the operation of a geyser. Second, explanation often has a purely logical meaning. This occurs when an observed regularity is recognized as a special case of a more general law, for instance, a specific chemical reaction as the application of a law of chemistry. But by conceiving of the analysis of some natural phenomenon as the application of an abstract structure to fit a given situation, one makes clear the principal function of theory as an imaging process, that is, as a description of observed reality. Psychologically speaking, there is no possibility of an involvement of the pupil's person as there is in the less precise concept of science's providing "explanations", where assent is essential.

One benefit of the structuralist method mentioned by Eddington is the disappearance of the term "existence" from the vocabulary of the scientist. All propositions of science are essentially relational; thus, while all kinds of propositions saying that objects A, B, and C interact with each other can and will occur, no statement occurs that "A exists" except in the sense that A is subject to these interactions. Eddington describes the term "existence" as a "metaphysical" predicate and tells us that the structuralist (his positivist) method is a powerful tool to separate science from philosophy, the latter in its more extreme form of metaphysics.

One may believe, however, that it is possible to go one step farther. Implicit in the structuralist method there is the hope that the scientist (for the time being only the physicist) can liberate himself from an affliction that has beset the more thoughtful part of humanity since the beginning of time: the tyranny of words, which seems to pervade the history of human thought. There is good evidence that in much earlier times some words had magical power – one need only think of the taboo on pronouncing the name of the deity so conspicuous in the early parts of the Bible. In more modern times there occur arguments about "understanding", but this understanding is often a semiconscious process involving some unknown depths of the mind. As against this, the structuralist analysis of the laws of modern physics realizes understanding in terms of a large number of small steps whereby the elements of an abstract structure are related to numbers of observations, thus minimizing (so far only for physics) that slipperiness of concepts which seems such a spectacular characteristic of all learned discourse (even of much scientific discourse) of the past.

Such insights would be of little value if the structuralist method would forever be confined to physics. We may next define a task which this author first formulated some years ago: the structuralist method that can be taken to define scientific clarity and absence of prejudices ought to be expanded beyond physics proper, into biology. The pages to follow should be understood in this sense: they are meant to be an application of the structuralist method to matters biological, primarily to an analysis of the modifications of this method which are required to make the transition from physics to biology.

For the purposes of this task, before we can even begin to enter into any technical detail, we must focus on the overall, qualitative differences between the empirical material of biology and that of physics. There has always existed, in physics and its direct applications such as astronomy, a prevalence of mathematical methods; the material is such that it lends itself to mathematical analysis. But the relations between the biologist and the mathematician has never been very close; often they have degenerated into downright hostility. In the nineteenth century, for instance, biological journals simply refused to accept articles that contained mathematical formulas. Such incidents were not quaint exceptions but expressed the posture of the biological community during much of its history. What is it that gives the biologist an attitude so radically different from that of the physicist?

To shorten the path one has to traverse in order to arrive at a clear-cut statement of this difference, let me give at once the result: while the material of the physicist lends itself as a rule to displays of regularity that can be expressed quantitatively, the material of the biologist is characterized by a pervasive complexity and variability. We shall fasten on the term *complexity* as expressing in a condensed form the chief characteristic of the biologist's material. Needless to say, the mere term stands here for a whole history of experiences which, properly speaking, ought to fill a book; we must here simplify to keep the size of this review within reason.

One of the remarkable aspects of the complexity of living things is that its full extent has been discovered only in comparatively recent times, as history goes. The microscope was invented shortly before the year 1700, and further tools for the elucidation of biological complexity, the ultramicroscope, X-ray analysis, the electron

microscope appeared only much more recently. If, as Nordenskiöld (1946) has put it, organic life constitutes "a separate form of matter," then the chief characteristic of this form of matter can be clearly enunciated. It consists of the presence of structure within structure within structure, as a distinguished physiologist has put it. This distinguishes a living object clearly from an inanimate one, say a rock. The rock is structured on two levels, that of its microcrystallites, and again at the level of its molecules. But the living tissue is, as a general rule, structured at all levels. Whenever new instruments make accessible new domains of observation, new forms and dimensions of structure appear. This ubiquity of structure is a basic property of living things, and one that affords a rather clear-cut distinction between living and inanimate matter. We may think of this pervasive structuration as the detailed expression of the complexity which we just declared a basic property of organic life.

What, then, is the theoretical description of the complexity and this structuration? When we go back to ordinary, scientifically unrefined discourse, we realize that the members of a logical class do not yet have the property of being equivalent to each other. The class of all cows does not imply that any two cows are substitutable for each other. Instead, the description of cows depends on so many variables that the cowherd finds it always possible to tell the animals apart. This property is readily recognized as a general one for all the logical classes that appear in biology. Correspondingly, we shall say that the biologist employs heterogeneous classes. This, then, is the point at which we propose to separate biology from physics: Biology will be taken as using a logic of heterogeneous classes while physics employs homogeneous classes. The following pages are devoted to an inquiry into the properties of heterogeneous classes. We hope to show that this can be taken as a transfer to biology of the structuralist methods that have been so successful in the development of modern theoretical physics.

A main point of difference between homogeneous and heterogeneous logical classes lies in the use of mathematics within a universe of homogeneous classes. Those, for instance, who are more closely acquainted with the mathematical theory of quantum mechanics know that the entire theory of the homopolar bond centers around the equivalence and interchangeability of electrons and could not even exist without this trait.

Quantum mechanics offers us a prime example of mathematically rigorous laws of nature. What interests us here is the opposition between the idea of quantitative regularity as expressed in a "law of nature" and the complexity that we recognized as the chief characteristic of organic life. On closer view, the notion of a law of nature is found to have two implications: there is first the notion of quantities that are uniquely determined by law, and there is second the notion of quantities that vary from case to case and that we shall describe as contingent variables. (They are more familiar to the physicist in the form of initial conditions and boundary conditions.) It may not always be possible to separate clearly, in a quantitative sense, these two ingredients, namely, dynamical variables and contingent variables, but their logical distinction is clear enough. That is, the distinction between general laws and contingent effects may be dubious in individual instances, but when it comes to classes of processes the distinction can readily be made evident.

Now let me recall that according to the previous arguments physics is the study of the laws of nature whereas biology was so far only characterized as a realm of utter complexity. The following question then arises: Does this complexity lead to the need for basic conceptual innovations, or can this complexity be fully understood in terms of the intricate molecular mechanism which experience has shown to exist in all living beings? If one adheres to the latter assumption, one can be said to have adopted a physicalistic approach. The meaning of the term physicalistic is precisely that no major conceptual changes are assumed to occur on going from physics into biology. The more common term *reductionist* will here often be used interchangeably with physicalistic. "Reduction" is a rather general term for the logical operation of subsuming one regularity under a more general one. Here we are, of course, concerned with the relationship of biological order to physical law, a special case of reduction.

In the practice of biological and especially biochemical research the physicalistic approach has given rise to so formidable successes that I need not go into them at this place, assuming that my readers know a great deal already about the unraveling of the "genetic code" and other accomplishments of "molecular biology." But there remains a persistent question: Can the distinction between living and inanimate matter be exhausted entirely in terms of the familiar type of chemical mechanisms? Or what else may be necessary to account for that "particular form of matter" known as living things?

We meet here, however, an entirely novel situation as far as the progress of theoretical science is concerned. In the past, when a conceptual innovation became necessary (and it did indeed a number of times in physics), this led in all cases to quantitative predictions about the results of the "new" theory which differed from those of the "old" theory. Having made the critical measurements, the scientist could abandon the old theory and replace it by the new one. But this simple criterion does not apply to the problem that concerns us here. The fact is that there is no shred of evidence anywhere in the vast literature of biochemistry or biophysics that the laws of physics (in practice the laws of quantum mechanics) are invalid or stand in need of any modification. Any approach to theoretical biology must start from this basic fact. But at the same time this does not absolve us from the need to pursue the possibility of a substantial conceptual innovation occurring in the passage from theoretical physics to theoretical biology. In other language, any conceptual innovation must be such that it leaves the laws of quantum mechanics invariant. Of course, all molecular physics and chemistry, including all biophysics and biochemistry is assumed derivable from quantum-mechanical theory.

We have already indicated the key concept around which such a conceptual innovation will center; it is complexity, with its attendant variability. This complexity can be rendered in logic by the heterogeneity of classes, and it is the heterogeneity of classes, then, that will serve as the chief vehicle to set off theoretical biology as against theoretical physics. We shall later on have occasion to see how this heterogeneity of classes can be understood in such a way that it does not violate the laws of physics. We shall, in particular, deal with irreducible heterogeneity, that is, with heterogeneity such that no logical operations can be found which would allow one to resolve heterogeneous classes into a combination of homogeneous classes. For the time being I can just claim that this is possible; the specific theoretical arguments will appear as we

proceed. A main argument will be that the heterogeneous classes of the biologist can be thought of as subclasses of the homogeneous classes of physics which transcribe the physicist's "laws". Under these conditions, as we shall see, the question of whether heterogeneous classes can be considered the formal equivalent of some "extra" set of laws, need never arise.

My claim is then that the formalization of complexity through the irreducible heterogeneity of logical classes (as defined later) introduces an element into biology which represents conceptual innovation and so goes beyond the reductionist approach. Since at the same time this can be done without violating the laws of physics, we have a situation that, as already pointed out, seems to have no counterpart in previous "conceptual revolutions" known to the theoretical physicist.

The arguments given here have been gradually developed by this author and appear in the three books already cited that span an interval of over 20 years. I have here presented them in a novel and, I believe, clearer fashion. Still, my aim has not altered: to explore the potential for a conceptual innovation germane to biology, that conforms to the laws of physics.

III. Finiteness. Individuality

The main concept we discussed so far, that of a heterogeneous class, fits the structuralist method: it is purely abstract and free from philosophical connotations. And while homogeneous classes are the equivalent of a mathematical treatment, heterogeneous classes do not lend themselves easily to mathematical representation, the less so the more heterogeneous the classes envisaged. But even if there is little of a mathematical structure, we can still speak of a *formal treatment*. We have indicated already that such a formal treatment in terms of heterogeneous classes might be the starting point of a genuine theoretical biology; but if we ignore mathematics as a structuring element, we have little to draw upon for the ordering of the empirical data of biology.

We can proceed farther by realizing that benefit may be drawn by concentrating upon properties which enhance the consequences of heterogeneity. The results now to be presented show that it is indeed meaningful to speak of an enhancement of heterogeneity; we may take it as a heuristic program that the abstract structure of heterogeneous classes should be enhanced in the direction of bringing out heterogeneity by such logical methods or technical tricks as we can find.

Homogeneous classes yield to mathematical treatment because their members differ little or not at all from each other. Let me exemplify hydrogen atoms which are all in the same quantum state form a perfectly homogeneous class. If the individual hydrogen atoms are in different quantum states but if the chemical nature of the atoms is always that of hydrogen, we are still dealing with a homogeneous class but the homogeneity is no longer perfect. The number of members in such a class has little significance. This is so, precisely because the logical operation of formation of the class can be replaced by a mathematical description of its members, in which all the members of the class correspond to one and the same mathematical symbol. This fact is deeply embedded in the language of the physicist: he speaks of a "system" which he represents by a

mathematical symbol. He fails to specify whether his mathematical symbol represents one object of his experience (one atom) or a whole set of atoms or perhaps even an infinite set.

On closer scrutiny, the last-named alternative turns out to be of theoretical significance: Should one or should one not admit classes of *infinite* membership into the logic of description? Note that for a perfectly homogeneous class this question has no meaning for, by definition, the members of such a class are indistinguishable from each other. Hence, there is no operational meaning to the distinction between finite and infinite membership of the class. We have here a nearly classical case of the use of the term operational meaning. The early (positivistic) students of the meaning of scientific terms in physics carried out the first such analysis; on this basis others in the early years of this century constructed first the theory of relativity, then the quantum theory. We now find a subject of logic, namely the finiteness of classes, that promises to play a similar role with respect to theoretical biology as certain concepts of space, time, and causality had played earlier with respect to the structuralist reconstruction of physics proper.

It is obvious that the number of operations which may be carried out in the real world is finite; in other words an infinite set of operations, or else a class with infinite membership, are idealizations. We cannot know in advance, of course, whether a postulate excluding classes of infinite membership will have far-reaching consequences. In the event that it does, as we shall see, we shall from now on postulate that all classes used in biological descriptions have finite membership. We shall not spend time in justification of this postulate in advance of its application; we expect that the reader who continues to follow the argument will come to recognize the remarkable degree of novelty that this postulate allows us to introduce into the formal analysis of biological conditions.

The finiteness of biological classes may appear less strange when one remembers that according to the testimony of astronomers our whole physical universe is finite in its extension in space and time. This was first stated by the astronomer Hubble in the 1920s and has since come to be acknowledged as a basic astronomical fact. We do not need it here, in the sense that we can just as well think of the finiteness of classes as a matter of biological method. But even so, it may become more palatable by the reference to the astronomers' universe.

A remarkable benefit of the use of finite classes lies in the fact that this allows one to deal in a formal way with the concept of individuality. Let me lead to this discussion by some introductory words.

Owing to the historical descent of science from philosophy, many scientists have never abandoned the belief that scientific terms arise out of words of the language; and so if a word exists, they believe that there exists not just a concept but, potentially, a scientific concept. Structuralism is the sieve that more critical scientists have put in the flow of their experiences to keep out the verbalizations in which ordinary language is so rich. The structuralist method tells us that science deals only with observable relationships, where the language of ordinary intercourse deals with an unanalyzed jumble of facts, fancies, and wishful thinking. Here we find that one of the principal logical tools which

screen the language in a structuralist sense is the distinction between infinite and finite classes.

It seems impossible to define the term individuality in a universe of discourse consisting of homogeneous classes, where the distinction between a class of finite and one of infinite membership cannot be given an operational meaning. Consider, then, heterogeneous classes of finite size. Assume the number of members in the class has not been specified; it may be small. We can now define an individual as a heterogeneous class with only one member, a one class. The question of how meaningful and valuable such a definition of individuality is cannot be decided by purely formal arguments; it requires the application of the concept of heterogeneous classes to (biological) experience in order to see how well an object for which individuality is claimed can be logically separated from other objects. Such operational isolation of individuals turns out to be a matter for the empiricist, the taxonomist.

Now if one considers individuality from the viewpoint of the empirical scientist, it becomes soon clear that this property tends to enhance the difference between the physicist's way and the biologist's way of looking at nature. For the physicist, who is as a rule concerned with homogeneous classes, the individual differences between one specimen and another specimen of a class (e.g., of a class of crystals) are incidental, and the physicist tends to ignore them except when the individual features form by themselves a class (e.g., the class of semiconductor crystals containing a certain proportion of impurities dissolved). But the biologist does not think of individuality in this way. The field biologist in particular tends to think of every (higher) organism that he meets in his field work as an individual different from all other organisms of the same species.

To make clearer the benefits that accrue from finite classes consider the common method of the selection of subclasses in order to demonstrate the presence of a suspected mechanism. Given the high degree of structural complexity prevailing in living things, there may arise the case that the class has been exhausted before a decision about the presence or absence of a mechanism has been made. In other words, in a universe of finite classes there exist questions that cannot be decided operationally. This can, of course, only occur in a world of great complexity; in such a world, the running out of specimens indicates the possibility of posing unanswerable questions. In such a world, therefore, the term *irreducible complexity* takes on a precise operational meaning. It is the irreducible complexity defined in terms of an exhaustion of finite classes that makes a physicalistic biology unsatisfactory.

This is the central statement of the theoretical scheme proposed in these pages and followed up previously by the author, in his books already quoted. We shall study some of the implications of a theory of heterogeneous and finite biological classes and see in particular what the relationship of these classes is to the homogeneous and therefore in practice unlimited classes of the physicist (unlimited reproducibility of experiments).

Lest the distinction of heterogeneous as against homogeneous classes be taken as a scholastic exercise that is of little avail to the practitioner of biology, we shall next discuss some observational results that are closely related to the heterogeneity of biological classes. This will give us the first occasion, in the course of a so far rather

abstract inquiry, to connect our abstractions more closely with observation. In biology there has in the past been no clearly recognized nonphysicalistic concept which could be compared with experience. Now the heterogeneity of classes is a concept characteristic of biology; as we mentioned already, it is to be enhanced and enlarged but it will not be supplanted later by other nonphysicalistic concepts. To make what is to follow more comprehensible, let me remark that individuality is a limiting case of heterogeneity, which implies that each member of a class has characteristics that distinguish it clearly from any other member. In the practice of the naturalist the two terms are often used interchangeably; in what follows we shall now and then adopt the habit of saying individuality if we mean heterogeneity of classes. This is not likely to lead to a misconstruction of meaning. On the other hand, I believe that we are faced here with a situation where the famous statement of the philosopher Hegel applies: when quantitative differences become extreme, we are inclined to perceive them as qualitative differences. This may be taken as our guiding idea in investigating the difference between heterogeneous and homogeneous classes; we shall try to show that the observer often perceives this difference as that between the living and the inanimate.

Almost a quarter-century ago there appeared a little book entitled *Biochemical Individuality* whose author is the biochemist, Roger J. Williams (1956)²⁾. By that time Williams had acquired an extensive reputation as a biochemist, through his work on the identification and analysis of vitamins. This book is based on a collection of empirical observations, some gathered from the literature, many others obtained in Williams' own laboratory at the University of Texas. There is a simple thesis to the book. It is this: every organism can be distinguished from every other organism of the same species through measurable characteristics that are different in any two of them. (The quantitative observations are so far limited to higher organisms, but the impression this reviewer has gained is that this is a technical rather than a fundamental limitation; in other words, we have found no good argument against the assumption that heterogeneity of classes is a basic property of *all* living things, not just of higher organisms; this assumption is implicit in our later general discussion, and the existing gap should be filled out as observational techniques continue to improve.)

Williams' book is a short one (209 pages), but it contains a vast amount of information, almost all in the form of factual reports of quantitative data. It is unusual to find oneself confronted with so novel a doctrine in such a purely practical guise. There is no substitute for the reader's perusing this book (which is readily available in the trade), but we must indicate at least in outline in what the novelty of Williams' approach consists. Williams succeeds in shattering the concept of normalcy. Since any higher organism is clearly characterized by a very large number of variables, any definition of a "normal" organism must comprise the need for specifying these parameters within "reasonable" limits. But what does reasonable mean? Williams' results demonstrate with perfect clarity what it does *not* mean: the "normal" is not something in the nature of an average that can be determined observationally to be confined to within certain

²⁾ Anmerkung_evgo: *Biochemical Individuality: The Basis for the Genetotropic Concept* by Roger J. Williams Paperback: 224 pages ; Dimensions (in inches): 0.78 x 8.99 x 6.02
Publisher: McGraw-Hill/Contemporary Books; ISBN: 0879838930; 2nd edition (July 1998)
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percentage limits in a statistical sense. Such a definition would be suitable for an engineer who deals with products coming off an assembly line. The engineer could, for instance, specify that any value, x , of a variable which deviates by less than 10%, say, from an agreed-upon mean value, x_0 , is acceptable, whereas any values of x that deviate from x_0 by more than $\pm 10\%$ must be rejected. Williams' abundant data show that a statistical dispersion of the order of 10% is totally inadequate to characterize any kind of "normalcy" in biological investigations; there are innumerable parameters that will vary within a population by factors of two or three, going as high as factors eight or ten or even higher.

Williams, before he enters into the main body of his data, which deal with analyses of biochemistry, devotes a chapter to anatomical variations. By means of drawings taken from a well-known textbook of human anatomy he demonstrates the vast variety of shapes that occur, for instance, in human stomachs; one finds that the volume of the stomach may vary by a factor of eight among humans that in medical tests would be declared perfectly "normal."

Williams is, of course, mainly interested in variations of a biochemical nature, and his book is full to the bursting with examples. To save the reader a multitude of details I will confine myself to reproduce the summary of his Chapter 5 entitled "Individual Enzyme Patterns" (p. 77).

Summary. The cumulative evidence that each individual human being has a distinctive pattern of enzyme efficiencies is hard to refute on any rational basis. Furthermore, inter-individual variations in enzyme efficiencies in normal individuals, insofar as they have been determined, are not of the order of 20 to 50 percent, but are more often at least 3- to 4-fold. Differences of 10- to 50-fold (!) have been observed in a substantial number of cases even when the number of normal individuals tested was small.

Certainly these differences are far from trivial. Even to the author, who has been interested in variability for some years, the extent of the variability comes as a surprise. He therefore cannot blame his colleagues if they seem incredulous. We have included in our discussion every enzyme for which we have found substantial data, and the least inter-individual variation we know of appears to be about 2-fold.

Inter-individual differences related to metabolism come to light only when *detailed items* are compared. When two individuals of the same height and weight yield total metabolism values that are about the same, it is easy to conclude that their metabolisms are substantially identical. The evidence presented in this chapter, however, indicates that the details of metabolism in two such individuals may be very different indeed. The extent to which specific reactions may take place may vary 10-fold! This idea is admittedly difficult to accept, but it appears to be substantiated by concrete and cumulative evidence.

It would be impossible, in a report like this, to do justice to the wealth of data presented by Williams. We shall not even try to do so but refer the reader to the original work, which can be read without difficulty. Certainly, the ideology of the biomedical profession will have to undergo a radical change as Williams' data and method become more widely known. Since the data are factual, they cannot of course be ignored, but neither have they so far been recognized and taught as they so obviously deserve. The present writer, who cannot by any stretch of the imagination be thought of as a member

of the biomedical establishment, can do no more than present this impressive empirical correlate of the more abstract reasoning discussed in our pages; we hope that scientists will eventually yield to the persuasion of an overwhelming array of factual data. It appears from Williams' book that the physicalistic view of biology, according to which one would expect classes of organisms to correspond to classes of relatively uniform mechanisms that can be built up on an assembly line, is factually wrong. This result must appear to be more the beginning of a new and unaccustomed line of research than a simple fact which could be integrated into any existing point of view about the nature of organic life. For the time being we can think of the results of Williams' work as the observational counterpart of our abstract; concept of heterogeneous classes.

Let me come back once more to a point of our theoretical arguments, namely, the relationship of the heterogeneity of biological classes to their finiteness. We have above introduced the finiteness of classes by way of a postulate, that is, abstractly, without reference to empirical observations. In Williams' work there is implied, however, another possibility which he does not state with brute clarity but which will not escape the diligent reader: the vast observed differences in concentration of certain compounds as between different individuals suggest that the *metabolic patterns*, the underlying feedback cycles, may differ from one individual to the next. Such a variation of metabolism seems to be confirmed by certain observations: the concentration of a substance is found to be much less variable if tested in one and the same individual at different points in time than when one compares different individuals with each other. Given the tremendous variety of feedback loops already observed in metabolism, it must seem mainly a matter of established modes of thinking, whereby investigators are kept from testing the constancy versus variability of metabolic feedback pattern in given classes of organism. Such a task has a purely mathematical (statistical) side to it in addition to its empiricist aspects; we shall come back to the mathematical aspects a little later.

One is dealing here with a type of relationship between theory and observation that is well known in physical science but in the past has remained all but unknown in biology. For science to be at its most fruitful, it is advantageous that its theoretical and its observational components be of comparable degrees of difficulty. While this generally holds in the physical sciences, it has not usually been true in biology, where often either the theoretical aspects or the observational aspects were trivial. When one deals with the heterogeneity of biological classes that are also finite the mathematical theory (combinatorics) of such abstract structures becomes rather complicated, and so do the statistical observations on the extended samples required for such work. But there is little to indicate that they cannot be mastered by modern techniques.

The biological experience that is contained in the preceding statements, first implicitly in the discussion of heterogeneity and then explicitly by way of Williams' results, may be used as the starting point of an obvious generalization: each living thing has a measurable individuality. This cannot at the present time be proved. It may be thought of as a horizon which delimits biology. This writer finds it useful to think in such terms, because in this way we can define theoretical biology in a purely formal manner. If such an idea can be carried through in detail, we remain fully within the purview of a theory that conforms with the demands of the structuralist method. We should then be

free of the entanglements of speculative philosophy so far as biology as a separate science is concerned.

The remaining pages are given to an overview of the formal problems that are raised by such a program. The program, to say it once more, consists of the assumption that each living thing, each organism has measurable features that allow one to distinguish it from any other organism. On continuing this inquiry we shall find that in such a scheme the main question centers on the relationship between the physicist's description that uses mathematics, logically expressible in terms of homogeneous classes, and the heterogeneity and individuality of the biological description. The magnitude of this problem makes one recognize the inadequacy of any one investigator; but it seems clear that the problem is rationally posed and hence can be dealt with, ultimately, by a rational analysis, even though the technical difficulties might be very large indeed. But one main aspect of this formal approach is that it is likely to lead us into abstract problems of a rather formidable, purely mathematical complexity. This concerns answers to the question of how the universe of heterogeneous, finite classes, which is that of biology, can be quantitatively related to the universe of mathematical structures, transcribed into a logic and mathematics of homogeneous classes, as given to us by atomic and molecular physics.

Leaving these questions for a later section, let me draw attention to one very attractive feature of this formal scheme: it limits science intrinsically. According to the structuralist view, the purpose of theoretical science is to put relational order into the observed phenomena. If the principal abstract tool used is logic, then this amounts to the establishment of classes. Such classes are homogeneous in the case of physics, heterogeneous in the case of biology. The limiting case of a universe containing only different individuals as just envisaged corresponds to a bound for science that lies in the nature of science itself, not in any external convention. In our age which, as one can daily read in the newspapers, is the great age of science, it is gratifying that one can find at least one major branch of science which carries its own limitations built into it in a natural way. Using a somewhat antiquated language we might say that the order which appears in biology through the existence of classes shades off into chaos in the form of an assembly of distinguishable individuals.

In the sequel, when we speak of biological classes we shall consistently assume that these classes are heterogeneous as well as finite and that they can be broken down into a set of distinguishable individuals by suitable observations.

IV. Finite Classes and Selection

In the preceding pages we have developed a formal scheme for the representation of biological data. This turned out to be a variant of ordinary logic, namely a logic of heterogeneous, finite classes. We found empirical grounds for thinking that this is a very good way of describing a large part of all biological data, evidenced by the amazing observational results of Roger Williams. But is this really enough to characterize that "separate form of matter" which consists of living bodies? In such an estimate, the theoretician (such as this writer) does tend to exaggerate the power of specific tools, the abstractions and formal operations. We must, in addition to abstractions, have more facts drawn from experience that allow us to set living matter

off from inanimate one. This section is devoted to the enunciation of some such facts; others will be given in Section V.

As I have tried to make clear in the beginning of this article, I am approaching biology here with the mode of thought as well as with the techniques of the theoretical physicist. The outcome of the preceding inquiry was that the *logic* of the two sciences is different: physics uses homogeneous classes (which lend themselves readily to mathematical formulations), whereas the preferred tools of the biologist are heterogeneous, and finite, classes. Admitting this, we are faced with the question: Can anything specific be said about the relationship of the actual classes used by the physicist for his description and those actual classes that are used by the biologist? We shall see that a general proposition can be enunciated and that it expresses the difference between the living and the nonliving state in a more substantive manner than would be possible with the preceding, purely formal constructions.

It is significant that, while this distinction between the living and the inanimate is not a consequence of any logical formalism, it need, nevertheless, be expressed in terms of a logic of classes. In simpler words: the distinction between the living and the dead is a matter of classes, not of an individual event. This is so much a part of everyday human experience that we are usually not even aware of it. Whether a person is alive or dead at a given moment is decided by criteria that are based on *general* experience with the behavior of members of the species (e.g., the significance of heartbeat). In physics the distinction between one kind of interaction (e.g., electromagnetic) and another (say, gravitational) is much simpler because the corresponding classes are homogeneous; telling the dynamical variables apart from the contingent ones (boundary conditions, etc.) offers little difficulty.

The general tool of description of the physicist when he deals with bodies containing many atoms or molecules, is statistical mechanics. This branch of physics predates the advent of quantum mechanics by about half a century. After the discovery of quantum mechanics these two branches of physics were found to coalesce readily into a unified mode of description known as *quantum statistical mechanics*. Expositions of this subject are available in numerous textbooks. I cannot, of course, indulge in an exposition or even in a précis of the content of this particular branch of science. What I can and shall do is to extract some general conclusions that are sufficiently simple and unambiguous that they are (1) intelligible to a broader, biological audience and (2) acceptable to the overwhelming majority of the specialists.

We shall remember first that statistical mechanics, being a branch of physics, begins by dealing with homogeneous objects, that is, those which contain only one or two, or at least an extremely small variety of molecules. For such assemblies of equal molecules one replaces the study of their complicated internal dynamics by a study of statistical distribution functions. In connection with these distribution functions it is useful (and is often so done in practice) to replace all continuous distributions by *discrete* ones, that is, to replace the continuum by a set of small intervals. The general term for such intervals in multidimensional space is cells. Such a subdivision is not only applied to ordinary space but also to velocity (or momentum) space. (In general, however, when we speak in this report of "cells," we use the term in its familiar biological meaning.) Since we are interested only in objects which occupy a finite volume and have a finite

internal energy, the discretization implies that all numbers which appear in the calculus of distribution functions are finite. This fact will be very useful if presently we apply this type of reasoning to biological objects.

A standard mathematical question that arises in statistical mechanics is this: In how many ways can one distribute m objects (molecules, thought of as indistinguishable from each other) over n empty cells? The answer is given by the binomial coefficient, that is, this number is

$$\binom{n}{m} = \frac{n!}{m!(n-m)!}$$

where the *factorial function* is defined by the multiple product,

$$n! = 1 \cdot 2 \cdot 3 \cdot \dots \cdot n$$

of all integers from 1 to n . In any textbook of mathematical analysis one can find Stirling's approximation to the factorial function for large n ,

$$n! \sim (n/e)^n$$

We note here the appearance of the expression n^n which represents a rate of growth with increasing n faster than the well known exponential growth, e^n ; we shall call it *factorial growth*.

Now the mathematical details just given are not necessary for us in detail. What we must consider is the formidable magnitude of the numbers generated by factorial growth. The objects to be counted in statistical mechanics are numbers of atoms or molecules; exceedingly large numbers will appear all the time as the result of factorial growth. We can write the factorial function

$$n! = n^n \cdot e^{-n} = (10^{\log n})^n \cdot e^{-n} = (10^{n \log n})e^{-n}$$

so that factorial growth means a growth such that $n \log n$ appears in the exponent. Consider now that it stands for the number of molecules or else for the number of places available for molecules; it is clear that this kind of mathematics leads to tremendously large numbers, a fact familiar to many students of physics, since statistical mechanics is a part of the advanced curriculum in physics.

We shall now express the facts just mentioned in an alternate fashion. Let me introduce a limit that separates "ordinary" large numbers from extravagantly large ones. Let me take arbitrarily the number 10^{100} as boundary line; I call any number "immense" that is larger than 10^{100} . Conversely, if a number is smaller than the reciprocal of this number, 10^{-100} , I call it immensely small. The operational implications of this obviously somewhat arbitrary definition can readily be made clear: immense numbers of operations, however simple they may be, cannot be carried out in the real world. (Computer theorists call them "transcomputational.") To appreciate this one has only to remember that astronomers commonly give an estimate of the size of the universe by saying that it contains about 10^{80} atomic nuclei. We are fairly sure that the lifetime of the universe expressed in seconds of time is less than 10^{18} ; thus, 10^{100} may be taken as an upper bound for the total number of events (1 sec apart) that can occur in the real

world. Clearly, the precise choice of such numbers is not as important as the insight that there exist numbers so large that they no longer correspond to any set of events in the real world; if such numbers are imagined as physical events, they can represent thought experiments only.

We can now readily make contact again with the structuralist mode of thought that was introduced early in this review. The scientist imbued with the structuralist method will be set to wondering when he encounters numbers that cannot be realized operationally. He will recognize a challenge for the reconstruction of the conceptual scheme of science along structuralist lines. This is the kind of argument that will lead us to a critical evaluation of the dividing line between biology and physics. But so far in this section we have spoken only of the objects of the physicist. They form homogeneous classes because they have a homogeneous physical constitution, that is, they consist only of one kind or of a very minute variety of distinguishable molecules. But living things form heterogeneous classes because they have a heterogeneous constitution; they contain a much larger variety of different molecules as well as steric structures than one usually encounters in inorganic chemistry. If we want to find out about the difference of living and inanimate bodies, we should have to apply statistical mechanics to the heterogeneous bodies which constitute the object of the biologist's researches, as it has been so successfully applied to the homogeneous bodies of the physicist.

Here we have to admit that we are defeated before we even begin by the magnitude of such a task. None have gone before us: there is no statistical mechanics of heterogeneous objects. But we cannot afford to give up. We ask whether we cannot sketch some general ideas that may provide a lead in this vast jungle of complexity and heterogeneity. This author has proposed one such idea which he calls "the principle of finite classes." It appears in each of his above-quoted books; we proceed now to its exposition.

Before doing so it is useful to get a glimpse, in concrete and intuitive terms, of what the heterogeneity of structure of organic tissue really implies. In contradistinction to an inert inorganic body, such as a crystal, the living body metabolizes incessantly. The extraordinary complexity of the living organism is due in large part to the well-known ability of the carbon atom to form multiple bonds. If enough carbon atoms are assembled, they will form chemical structures in a great variety of ways. This variety is known to the organic chemist as the phenomenon of isomerism. To illustrate isomerism, take the very simplest case of only four atoms connected by three bonds. They can be arranged in two quite distinct ways, as shown in this diagram:



In case (a) the four atoms are all in a straight line; in case (b) there is a side branch. Now if one has a large number of carbon atoms, one can build up a tremendous variety of structures by just combining connections (a) and (b) in various ways. All these edifices are legitimate structures within organic chemistry, usually with H, O, and N atoms interspersed. Any two carbon atoms that are close neighbors (say less than one angström unit apart) can be related in three ways: by a double bond, a single bond, and

no bond at all. The application of a more quantitative analysis shows that if the number of atoms, especially carbon atoms, is thought of as growing large, the number of possible distinct chemical structures increases factorially. This is the result that we shall presently require. It is characteristic of organic tissue and would not be found to hold in any other material body. Using the specific terminology introduced a little while ago, we can say that the number of ways in which an organic body the size of, say, an ordinary cell can be realized chemically, is immense. Since the structures involved are those of ordinary chemistry, an observer with sufficiently powerful instruments should have no difficulty in principle (although the practical difficulties may be great) of ascertaining what this structure is in a concrete instance.

For any lump of matter having the size of a cell and its typical chemical constitution there exists thus an immense number of ways in which its detailed chemical structure can be realized. On the other hand, the number of cells of any given species existing in the real world cannot be immense. This is a simple result of all the estimates of the magnitude of this number (the number of cells of a given species) that one can carry out. This shows that there exists a gross disproportion in numbers between, on the one hand, the number of ways a given gross chemical constitution of a cell can in principle be realized, and, on the other hand, the number of specimens, the number of actual bodies that can appear in the world. The latter is vanishingly small, often immensely small, compared to the former. The enunciation of this disproportion will be called the principle of finite classes. Our next step will be to shed some light on its nature and implications.

In the first place, let me comment on the use of the word "principle." We do not have in mind here an axiom or a postulate but merely a very general result of a numerical estimate. Its value lies in its applicability to all organic matter. One of its chief advantages lies in its *crudeness*. We compare the size of a class, that of potential chemical constitutions, which is found immense, with a class of actual living objects, which while extravagantly large is not immense. We hence do not have to enter into any subtleties, all of which can be bypassed at this stage of the analysis. We also do not plan to write a book that would contain a more detailed "proof" of this principle. Such a book could, however, no doubt be written by a person who combines a thorough knowledge of organic physicochemistry with experience in statistical mechanics.

In order to proceed from here, we shall simply accept the "principle" as true and go on to evaluate its consequences. To repeat the chief result: of all the possible molecular patterns of a living object only a vanishingly small subclass appears in reality. If we formulate the principle in that way, we are at once led to the further question: By what "mechanism" or additional rule do organisms select the patterns that do appear in the real world? We are using here the term *pattern*, indicating a purely static case; if we would want to speak of chemical transformations, we would use the term *process* instead. The mathematical analysis used in statistical mechanics is closely similar in the two cases.

So far we have spoken of a form of quantitative analysis that pertains entirely to the chemistry and physics of highly complex "organic" compounds. This analysis involves, on closer view, the *probabilities* for the occurrence of the various chemical reactions, that is the reaction rates. But probabilities can be mathematically defined with any kind

of rigor only as limits of infinite sequences in infinite sets (infinite classes). Since we have eliminated all infinite classes in favor of finite (although possibly immense) classes, probabilities are not here mathematically defined. However, one can always define relative frequencies in any finite class: one simply compares the (finite) number of times an event A occurs in the class with the number of times another event, B, occurs. I can here only hint at these deep-lying questions known to those who have studied the foundation of probability theory. In the present context we plan to focus on the point where our propositions differ from the more usual reductionist preconceptions, and we have now arrived at this point.

If we accept the principle of finite classes but do not proceed from there to reductionist or physicalistic assumptions the latter of which imply that everything which happens in the organism is the result of physical causality combined with "randomness," then we must consider the alternative. A selection of patterns takes place among all the possible patterns. We assume this selection to be a spontaneous natural phenomenon not subject to experimental control. Only a negligibly small subclass of all the physically possible class is so selected.

Since these assumptions are somewhat novel, they deserve further discussion. In the first place, the relationship of biological regularities to physical laws is here clearly defined. The classes of biology are *subclasses* of the classes used in the description of the physicist. Furthermore, experience indicates, as reported in Section III, that biological classes are heterogeneous, whereas we know that the order of physics must be represented by homogeneous classes. This alone suggests that the heterogeneous biological classes are not the equivalent of any law of nature; hence, conventional vitalism which postulates a modification or extension of the laws of physics differs strongly from this scheme.

If we ask about the nature of those patterns that are not selected, we can say in the first place that since they are of immense number, they cannot even be explicitly enumerated. Now we are dealing here with the description of a living thing down to and comprising the molecular scale. The patterns on that scale which are compatible with the laws of physics but which have *not* been selected may be described as *molecular terata*. (Most of my readers will know that the term "teraton" is used in medicine to designate a more or less misshapen product of embryonic development.)

The most characteristic feature of this theoretical view of organic life is that in contradistinction to reductionism it postulates a specific distinction in the pattern of behavior as between living and dead objects. In such a framework, we believe that our scheme has a greater degree of what in the English language is described as *verisimilitude*. In this word, the Latin root, *versus*, meaning true, is readily recognizable. A scientific theory must have some degree of verisimilitude, resemblance to the naively perceived truth about the observed facts, something conspicuously lacking in the methods of reductionism. We can express these ideas also in another and perhaps more readily comprehensible form.

If one crosses the borderline between physics and biology, the question arises as to whether this must involve a conceptual innovation. According to the view presented here, this is most emphatically the case. It contrasts with the widespread view that in

crossing the boundary no significant conceptual innovation is required, the physicalistic view commonly described as reductionism. It does not suffice, of course, to say that such innovation is needed, one must deal with the problem by indicating in what this innovation consists. The first and most important point has already been introduced: a logical theory of classes including a scheme of heterogeneous classes is required. The classes of biology which our experience shows to be intrinsically heterogeneous appear here as subclasses, of vanishingly small extent, of the immense, homogeneous classes with which physics provides us through the application of statistical mechanics; however, the same approach gives heterogeneous classes of immense magnitude if applied to the particular structures of organic chemistry that are found in living things. In a way, therefore, the heterogeneity of biological classes is preformed in the facts of biochemistry.

It should be clear that in the presence of a major conceptual innovation I can give no more than a *program* for a novel theory, that it is too early to elaborate on the details of any such theory. My principal aim at this stage must be to bring out and exhibit as clearly as possible the logical contradictions that appear in the current practice of biology, and to show how the new formulation removes these contradictions. Also, the introduction of the new logical scheme increases greatly what I called a little while ago the verisimilitude of the theoretical scheme. It seems that in the complex field of theoretical biology a certain tradeoff is possible: simplicity of assumptions, especially retention of physical principles only, versus verisimilitude. I believe that by introducing the concept of a selection of biological classes which form a negligibly small subclass of physically possible patterns (the entire, immense class being one of terata), by doing this we have removed any logical contradiction between two simultaneous types of laws: the laws of physics and regularities of biology. Biological regularities, as we have indicated, do not derive from universal laws; since they result from a process of selection, they are *specializations* of the type of behavior derived from the laws of physics; they are therefore by definition nonuniversal.

The particular regularities underlying biology according to this scheme do not stand alone; in the next section we shall deal with a further type of phenomena that leads to a loss of verisimilitude in the usual reductionist treatment. But before doing this, let me have a glance backwards, upon a past way of dealing with the same or a closely similar problem. We are thinking now of the famous *Cartesian Method* introduced by Descartes in his *Discourse on the Method*, which appeared in 1637. In it Descartes describes his method of how to deal with complicated objects of research. Since most of the complex objects that a researcher encounters are living things or at least the products and results of organic life, the Cartesian Method has played a particularly great role in physiology. According to Descartes, one investigates a complex object by breaking it down into simpler components; one then studies these components one by one so as to grasp their functioning. At the end one puts the components together again, at least mentally, and in this way gains an idea of how the whole things works.

The notion of the Cartesian Method has survived to this day in many semipopular and semiphilosophical expositions. Nobody, however, can read Descartes' description without being reminded of the way a teenage boy takes a watch apart. This is quite natural. Historians tell us that the seventeenth century was the age when natural philosophers came to visit the workshop of the "artificer"; it was the golden age of

mechanics where God was conceived as the designer of a "clockwork universe." The benefits that accrued to biology in this period are tremendous. For example, Descartes was a somewhat younger contemporary of William Harvey. And if we look at the Cartesian Method once more, closely, we can readily perceive the point at which it differs from the assumptions made here. In order that the component pieces of a complex object may be thoroughly investigated, one requires *repetitive* experiments; in other words, the components must be *homogeneous*. In the world of real biology, heterogeneity prevails at all levels; we would have to break organic tissue down into its constituent nuclei and electrons to achieve homogeneity. This is the reason the Cartesian Method does not apply beyond a purely macroscopic level, and in this sense, but in this sense only, there is a well-defined meaning to the statement that *biology is non-Cartesian science*.

Finally, we should point out the relation of our scheme of selection to the dualistic or quasi-dualistic schemes that have been surrounding biology for a long time. Outstanding among such ideas is that of Niels Bohr regarding "generalized complementarity" which he enunciated repeatedly in the 1930s, the years after the consolidation of quantum mechanics. Bohr's idea clearly was that there might be a phenomenon analogous to the wave-particle duality of quantum mechanics but at a higher level. It is hard to see, however, how such a scheme can be reconciled with our basic idea of selection where the biologically preferred class of patterns is negligibly small, or immensely small as compared to the size of the class comprising all patterns compatible with the laws of physics. To express this numerical relationship clearly, we raise its content to the level of an abstract principle: We introduce a *principle of selection*. It asserts in essence that living things become defined only by a selection being made by nature whereby the actually occurring states are distinguished from the immense multitude of possible ones. The precise nature of the selective process is here deliberately left open. The reason for the latter caution is that it seems unlikely that its specifics can be determined without extensive recourse to empirical evidence (especially of an embryological kind). Nevertheless, it seems to make sense to state the principle in the generality given it here.¹

V. The Stability of Information

The postulate of selection just introduced is so far purely formal; it does not tell us by what criterion the selection is made. This selection has to do with the *morphology* of the organism. Since we are sure that there will be no contradiction with the laws of quantum mechanics, it is appropriate to consider in detail the idea of a conceptual innovation that does not violate quantum mechanics, for instance, in the following context.

There are frequent hints in the biological literature to the effect that the Second Law of Thermodynamics may be violated in the organism. These hints have, to my knowledge, never been further elaborated. The main reason is that the authors of such remarks are confusing two similar things: the Second Law in its generality and the application of the Second Law to information theory. The conceptual novelty to be contemplated will

¹ In another paper (Elsasser, 1981), I have replaced the term selection by the more specific one, "creative selection." Reasons for this elaboration are given or, at least, sketched in that paper.

have to do only with the application of the Second Law to information processing. The making of an assumption in this field requires in the first place that we define information. Even a layman can see that information presents itself to us as a sequence of symbols. Speech is a sequence of symbols – similarly in television transmission: a picture is resolved into a series of symbols that are sent over the air by electromagnetic waves.

Information theory deals with certain quantitative aspects that can be abstracted from such a sequence of symbols. For now we leave the connection between symbol sequences and morphology open; we shall specify it later as required. It is clear that a picture on a television screen may be thought of as a form of morphology, and we shall assume, as is customary in information theory, that there exists a process whereby this "morphological pattern" can be transformed into a sequence of symbols and eventually back again from the sequence of symbols into the morphological pattern.

To be specific, let us think of the symbols transmitted as letters of the alphabet. An important concept is that of an *error in transmission*: a letter of the message is by this error replaced by some other letter, in a random substitution. Modern information theory started when in the year 1948 the mathematician C. Shannon showed that out of any sufficiently long sequence of symbols one can construct a quantity, a number known as the *entropy*, which has the property that on substitution of new (erroneous) symbols into the message the magnitude of the entropy increases in an overwhelming number of cases. Put in terms of probability, on random substitution it is vastly more probable that the entropy increases than that it decreases.

One has to be careful to distinguish the traditional Second Law from the special application of its formalism to information sequences. Let me therefore designate the result that the entropy of an information sequence increases when errors are introduced as *Shannon's Rule*.

The formal apparatus that leads to Shannon's Rule is identical with the one which is used to derive the Second Law of Thermodynamics from statistical mechanics. But it is somewhat simplistic to think that therefore heat and information should be confounded. To give an example taken from the physicist's practice: the equation of heat conduction and the equation of diffusion are mathematically identical. But one cannot conclude from this that heat and a diffusing substance are otherwise similar. Here, we can conclude that the formalism is so well developed that one need not admit any question about the validity of Shannon's Rule. Now if we assume that any morphological feature can be translated into an information sequence, that is, a set of symbols, then the effect of Shannon's Rule as applied to random errors is the deterioration of morphological features as time goes on. This is still a very loose statement; but since by our previous postulate all admissible morphological features arise by a selection from the class of physically possible ones, neither the validity nor any assumed invalidity of Shannon's rule can lead to a violation of the laws of quantum mechanics.

In information theory the effects of random disturbances upon a message are described by the term "noise." There is ample evidence, which will be indicated in the remainder of this chapter, that in living things the effect of noise in degrading morphological order is found to be very much smaller than one would infer from Shannon's Rule. This

experience will be summarized in the *postulate of information stability*, the second of the two postulates that set off biology from the physics of the inanimate. Its full implications will gradually appear as we go on. For now we note that on assuming Shannon's Rule not to be fulfilled in organisms, whereas it is in all electronic devices, we are giving a very specific meaning to the claim that organic life constitutes "a separate form of matter," as Nordenskiöld (1946) expressed it.

There are many ways in which the stability of information against deterioration, and often the reproduction of preexisting information, appear in biology, even to the untutored observer. Ordinary (cerebral) memory is a well-known special case of this stability; so is the phenomenon of healing, including, for instance, the well known ability of lower organisms to replace lost limbs. But the most important for the biologist is plain *heredity*. It has the advantage that here the scientific concept and the concept of everyday human life do very nearly coincide, so no fanciful refinements are necessary. Since I am now dealing with a difficult task, namely, the introduction of a basic conceptual novelty, I shall confine myself here to heredity as a most conspicuous example for the discussion of information stability in organisms. This does not mean that I am unaware of other cases such as those just mentioned; it merely means that in this review in place of being comprehensive, I shall confine myself to one phenomenon, heredity, which seems broad enough.

The scientific study of heredity has a history, of course, although a limited one. This history began about 300 years ago when the invention of the microscope enabled biologists to study for the first time the cellular aspects of the process of animal reproduction: fertilization of the ovum by the sperm and early embryonic development. Even without the example of computers before their eyes these pioneers of biology could pose the problem of heredity in terms of an information sequence that is transmitted from the parents to the progeny. "Information" being by definition a sequence of symbols, the question inevitably arose: What is the nature of the symbols transmitted in heredity? In the eighteenth century two schools of thought arose. The adherents of one view, called the theory of *preformation*, claimed that a small but fully adequate "model" of the adult organism is found in every germ cell. Then there was the opposite view to the effect that the germ cell was endowed with a "potential" to regenerate the adult in the absence of a full information sequence. This became known as the theory of *epigenesis*. Of these two only the term epigenesis has survived into present-day biology. In the course of history the term has acquired various kinds of subtle meanings which I will not discuss. Here I shall take the term epigenesis in its pristine meaning, namely, indicating a process in which the handling of information violates Shannon's Rule and hence cannot be described by a computer model.

This would seem to impute upon the earlier biologists a capacity that they did not have: the capacity to make a quantitative judgment as to what a computer can and cannot do. But I believe that this is not serious because even if a person does not yet have the tools to execute a given task in detail, he may well be able to judge reasonably well the limits of the possible.

We spoke of the postulate of information stability as giving a more specific content to the "selection" introduced in Section IV. We can now say with some precision what this postulate of information stability implies. It would tell us little that is new so long as

the transmission of information in the processes of heredity can be modeled by a well-defined computer. It is when the effort at modeling by computer breaks down that the postulate of information stability enters to acquire its specific meaning. Now remember that what this postulate describes is the nature of the selection among the immense number of possible patterns that according to physics (statistical mechanics) are possible. The overwhelming majority of these are terata and are of little biological interest. Those which are selected are such that they tend to conserve the morphological information beyond what would be possible in a world of automata.

It should be apparent that in this way we attribute to the organism a quality best described by the term creativity. It should better be called recreativity since the patterns to be created follow those of preexisting patterns. Since in introducing selection we have deprived ourselves of an exhaustive recourse to physical causality, we are thrown back upon the concept of a heterogeneous class as embodying the formal aspects of the process of selection. Previously, the theory of heterogeneous classes may have appeared as a tautological reshuffling of accepted logical concepts. But used as a foundation of the postulate of information stability the heterogeneous class appears as an abstract but irreducible descriptive element of biology. It plays a role in biology similar to the role that differential equations play in physics. We do not think to ask what "spirit" induces the planet to move along the orbit prescribed for it by Newton. Similarly here, we do not ask what "vital agency" makes the selection; we merely describe what we see happening in nature.

The decisive fact, as will readily be perceived, is the one summarized by the principle of finite classes: the number of possible molecular patterns of, say, a cell, is immense and vastly exceeds the number of specimens of any appropriate biological class, so that the transition from physics to biology requires a selection of patterns. The selection that occurs at any one point in time is guided by the fact that the progeny is a member of the (heterogeneous) class of all ancestors.

(I believe that the reader will find it just as difficult as this writer has for many years to absorb the idea that "mere" membership in a heterogeneous class has results analogous to those of a "law of nature" in a more conventional sense. This indicates that the notion of universal laws, so dear to the seventeenth century, will perhaps not always remain the last word in the description of nature. But I am firmly convinced that the unwillingness to discuss conceptual innovation on the part of large numbers of reductionistically inclined biologists and biochemists is not due to a commitment to the philosophical doctrines of rationalism. The psychological connection is in the reverse direction: it lies in the erroneous belief that conceptual innovation must be connected with a change in *universal* laws that has kept the older philosophy alive. The central point of my scheme is that a quite radical conceptual innovation can occur without any violation of the laws of physics.)

Two kinds of questions now present themselves. First are those of method and general "philosophy"; I shall discuss those in Section VI. For the time being I shall concern myself in more detail with the *empirical basis* that underlies the postulate of information stability. I shall try to show that the empirical evidence points powerfully toward a postulate, such as the one sketched, as the expression of the radical conceptual innovation required to penetrate from physical science into biology.

To summarize, in order to deal with heredity at all, we first need a *model*, however crude. If it turns out to be false, we can correct it later; but a good model as a starting point is of immeasurable value in keeping one's ideas precise. The model we have in mind here is that of an electronic device in which signals circulate; for simplicity we shall speak of a computer, even if we deal only with the transmission of information, not with its modification. One quantitative effect that cannot be completely eliminated is that of noise; for instance, thermal noise is ever present. It can be reduced only by going to low absolute temperatures, a condition not of interest in biology. If in a message consisting of letter symbols we represent the action of noise by a substitution of "false" letters for the original ones, the message will become progressively less intelligible. Shannon's Rule then implies that this loss of intelligibility progresses always in the same direction. For this reason technical devices that minimize errors must be of prime importance to the engineer as well as to the biologist.

The engineer who designs computers achieves a minimum interference of noise through making the signal energy much larger than the (mean) noise energy. In ordinary computers, even miniaturized ones, signal-to-noise ratios of many millions to one are found routinely. It is apparent that at this point the comparison of organisms with computers may lead to useful results. One might expect that the organism also separates its "signal" from inevitable noise. But we run here into a severe limitation of the computer model as applied to biology. A computer has fixed design features and variable signals circulating in this fixed design. In the organism we have just metabolic activity that can only artificially be split into a stable and a variable part – there is only metabolism. Nevertheless, one will try to use a computer model when one is dealing with properties which are independent of a somewhat artificially introduced splitting into a "signal" effect and a general background. Certain observational facts concerning the metabolism of living tissue are so general that they allow us to do this.

In an earlier work (Elsasser, 1958) I have given a quotation from a then widely used college textbook of organic chemistry (Conant and Blatt, 1947, Chapter 20) bearing the name of a distinguished practitioner of the science. The passage summarizes the situation in an admirable way, and therefore I shall simply repeat it. Since I do not claim to be an organic chemist the correctness of the statement will have to be argued in any event by others than myself. The statement is:

Biochemical reactions as a rule liberate or absorb relatively small amounts of energy; a balanced or nearly balanced equilibrium is often at hand. It seems that living cells operate with reversible reactions where possible and can utilize or absorb energy in only small amounts. Thus in the oxidation of carbohydrates a complex series of changes takes place so that at no one step is anything near 100 kg-cal of energy liberated, which would result if all at once one carbon atom of a carbohydrate were oxidized by air to carbon dioxide. Apparently this necessity for reversible reactions with relatively small energy changes is a characteristic of biochemical transformations (p. 376).

Nobody with even a humble background in physical chemistry could possibly mistake the meaning of this statement. If we interpret metabolic activity in terms of the ratio of signal to noise, as one must do if one wishes to apply a computer model, then one could restate it by saying that metabolism seems always to proceed very close to the noise

level, and such a statement is quite independent of the details of any model adopted. There is then good evidence that whatever "signal" is attributed to metabolism, this signal is not properly separated from thermochemical noise. It means that such signal-processing abilities as we attribute to the living cell operate within or close to the noise level, not far above it as it should according to our understanding of information theory.

Let me now compare this biochemical result with the stability of morphological features as it can be observed in cases where heredity is of prime importance. The science of paleontology offers us occasion to observe the variation of morphological features over lengthy periods of time. Species, once they have become established, tend to undergo only relatively minor changes of their morphological features during their lifetime to extinction, which lifetime is usually of the order of several million years. Thus, the morphological features remain approximately constant over some millions or some hundreds of thousands of generations of the species. Again, this observational result is very general and applies to all kinds of fossils.

The contradiction between the biochemical result that reactions do not deviate far from equilibrium and so signals cannot be preserved from noise, and, on the other hand, the paleontological result concerning the long-term stability of morphological features-this contradiction is so glaring that I propose to describe it by a separate term. I shall speak of the paradox of heredity. There is, of course, nothing new about this paradox; it has been known for a long, long time. What is new here, if anything, is its explicit recognition as a paradox, that is the admission that such a paradox might not be capable of resolution within the physicalistic world that is built up from organic chemistry together with basic notions of computer science. We claim therefore that here is the occasion of a conceptual innovation. We have already enunciated its content: it is the postulate of information stability. This postulate defines what we usually call creativity and which more appropriately should be called *recreativity*; we spoke of this before and shall briefly come back to it in Section VI. For the time being we shall remain with the concrete aspects of, on the one hand, heredity, and, on the other, computer models that are meant to describe heredity transmission.

Let me try once more to explain, in slightly different terms, why it is that a loss of information by mixing with noise is so much more fatal in the case of morphological features than it is in the case of the molecular order-disorder relationship that underlies the Second Law, a case with which the order in a biomolecular information sequence is so often compared. In the inorganic case one has just as many molecules, but one measures only two or three "variables of state." Even when a condition of maximum entropy has been reached, one can usually, by a simple change of conditions, return to a state in which the entropy is no longer a maximum. In the case of morphology the number of parameters is as a rule large; this results in a condition where the number of possible patterns is immense in the above-defined technical sense of the term in the absence of the constraint represented by the information. Thus, information corresponds to a selection from an immense reservoir; once the information is lost, it cannot be retrieved otherwise than by actually going through this immense reservoir of variants, a procedure whose impossibility in the real world is clear enough. There exists no analog to this behavior in the physicalistic world with which we deal with the help of the Second Law.

Given the crucial importance of the preservation of information, it must long have been a challenge to the engineering mind to think up devices that serve for the protection of information sequences from deterioration. It is of significance, therefore, to see that practically nothing has been achieved in the 30 years since information theory came into existence and has occupied some of the best mathematical and engineering minds. The only method that has ever been proposed to protect information from loss is by *redundancy*, which is just a learned term for the repetition of the information. A specific device for using redundancy to maintain information has been proposed by J. von Neumann (1956). He calls it a "majority organ," and it seems worth discussing. One assumes that in place of one transmission line, computing device, or the like, one has three identical ones in parallel, all three carrying the same message. There is also inserted a device which at regular intervals compares the three messages with each other; when one of them differs from the other two, it changes the former so that all three are alike again. Quantitatively, if e is the (small) probability that one device makes an error, then the probability that this triple device makes an error is readily seen to be of order e^2 . Variants of this device with more than three parallel transmission lines or computing devices can readily be conceived.

What is impressive, however, is the extreme clumsiness of such devices. The impression is very strong if not overwhelming that this state of affairs is not due to the relative newness of the engineering art involved but comes from our human inability to extract a message from an immense reservoir of competing ones if the message has once been lost in the reservoir. In the language introduced in connection with our first postulate, the selection of a "good" morphological pattern out of an immense set of terata is hopeless once failure has occurred.

As explained in the beginning of this section, we have chosen the phenomena of heredity to illustrate the application of the postulate of information stability. There are other applications, among which ordinary (usually called cerebral) memory is most important. A recent note of the author (Elsasser, 1979) contains some suggestions as to the form this postulate is likely to take when applied to cerebral function. We are in this case in totally unexplored territory! But such history of this problem as exists indicates that here, perhaps more than anywhere else, conceptual innovation is of the essence. In the article quoted, I have shown that the postulate of information stability, based again implicitly on the principle of finite classes, seems to represent the desired type of innovation.

VI. Selected Patterns

The introduction of membership in a heterogeneous class as criterion of the behavior of living things in place of mathematically expressed "laws" is a large conceptual novelty that requires more discussion. As I hope to show, we cannot as yet say in detail just what it implies but we can readily state certain things it does not imply.

Many of my readers will have wondered why I insisted so carefully – petulantly, they may have thought – on applying the structuralist method at every step. A little reflection will now show why this was done: at this juncture we cannot fail to encounter a well-known form of philosophy, namely, Platonism. The concept of an idea, or

prototype, underlying every class will, on even superficial structuralist analysis, be found incapable of being expressed in terms of abstract relationships between observed data, the basic feature of structuralist science. Here, the scientist finds himself before a difficult choice: he can either consummate the marriage with Platonic metaphysics, whereupon the biologist seems forever committed to this hybridization of his science, or else he must explicitly dissociate biology from metaphysics by taking a strict positivistic structuralist approach. In my own writings I have resolutely taken my stand on the latter alternative. I do not claim, however, that the alternative can be uniquely expressed by such terms as "true" or "false." The distinguished British biologist B. Goodwin (1978) in a recent article has clearly indicated that he takes the opposite position. He claims that some form of metaphysics, for which he chooses that of Alfred Whitehead, is necessary to interpret the data of embryology. Here, I shall not try to persuade my reader that my structuralist method is true as against a commixture with metaphysics. I shall be satisfied with having shown how closely together lie the problems of embryonic development and the concepts of Platonism.

The structuralist method allows one to make more explicit the distinction between a scientific analysis of heterogeneous classes and the traditional reasoning of philosophers. The philosophers dealing with these questions became later split into Platonists and Aristotelians. Those who derived their thinking from Plato called themselves "realists"; they claimed that the Platonic ideas were the only real things in the world and actual objects just more or less flawed copies. The opposite party called themselves "nominalists" since they believed that heterogeneous classes are mere "names" invented by logicians to put some order into the vast variety of phenomena. Now even a modest degree of analysis indicates that neither of these two traditional philosophical attitudes is acceptable from the structuralist viewpoint: realism suffers from the fatal flaw that Platonic ideas are not operationally verifiable abstractions. But nominalism, on claiming that classes are no more than formal pigeonholes invented by logicians, runs counter to our assumption that membership in heterogeneous classes does have an operational meaning if applied, for instance, to heredity.

So we see that the problem of the operational meaning of membership in heterogeneous classes is wide open; it will no doubt require much further research, partly of a theoretical type, in a field where so far all efforts have been almost purely empirical. The status that we have claimed here for heterogeneous classes is that of a primary and irreducible type of natural order, on the same level as the more conventional "laws of nature" so familiar to everybody. It is clear that this new concept of regularity, as it was formalized in our two postulates, implies the autonomy of biology. If now we ask what the consequences of this autonomy are, the distinction of our approach from the well-established physicalistic-reductionistic one can be viewed much more clearly.

Our first password on entering biology from the side of physical science was complexity, this being the most distinguishing feature of all things living. We concluded, toward the end of Section IV, that, hence, the Cartesian Method does not apply in biology. This complexity was, so far, implicitly understood as phenomenal, that is, it refers to the anatomist's or physiologist's observation of a multitude of devices interrelated with each other in a variety of ways. The Cartesian Method had suggested that one should look for simplicity underlying the phenomenal complexity, a simplicity that can be unearthed when one breaks a complex object down into simpler (and

therefore presumably more homogeneous) small parts. Now if we say that biology is non-Cartesian science, it becomes of interest to see whether to the phenomenal complexity directly observed there corresponds also a logical complexity. Assume there are two mutually incompatible mechanisms, or models, (a) and (b), each of which could by itself produce an observed effect. Then in our world of heterogeneous finite classes, we might run out of specimens before we have formed a class which corresponds exclusively to model (a) or exclusively to model (b).

Now the ancient controversy regarding the process of hereditary transmission, the argument of preformation versus epigenesis, lends itself to an interpretation of this type, or so we may assume by way of a working hypothesis. The discovery of the genetic code has given the physicalistic interpretation of heredity a tremendous boost. But our inability to account for an information stability which by any computer model should succumb to errors but does not do so in reality – this "paradox of heredity," as we called it, resists reduction upon a simple model, preformationist or epigenetic.

Physicists had previously encountered a closely similar situation. Light was a volley of particles according to some physicists, a set of waves according to others. Later, the same dualism was found to apply to electrons. In quantum mechanics this dualism is not only declared universal but is formulated in terms of rigorous mathematics and given the name "complementarity." In the biology of heredity, the only tools of description available according to our views are heterogeneous, finite classes. We now suggest that the irreducible logical complexity of biological heredity prevents us from assigning to it a specific model, in particular, the preformationist model of the "code," while eliminating all epigenetic features. This irreducible logical complexity is formally expressed in a description by heterogeneous finite classes. Instead of proposing a clever mechanism for the resolution of the paradox, we are led to think that the tools of description are limited in such a way that the paradox appears as an integral part of the description. Here, the analogy with the way physicists did overcome their own paradoxes is quite clear. In each instance of a conceptual advance in physics, the threatening paradoxes were absorbed into a modified form of description, where they no longer seem paradoxical. I here propose that the paradoxes of biology, in particular, the most widespread of them, the paradox of heredity, can be absorbed into a form of description called holistic, which in formal terms represents a description by heterogeneous, finite classes with its built-in limitations. Let us look at this question from still another point of view.

Niels Bohr, who was the uncontested leader among the founding fathers of quantum mechanics, discussed often in his later writings the changes in thinking that have to be made on introducing the conceptual innovations of quantum-mechanical theory. He expresses them as representing a renunciation of knowledge. Let me explain in more detail what this means. In traditional, "classical" mechanics a particle has a definite orbit, a curve in space through which it moves. As we go to quantum mechanics, this orbit is replaced by a probability distribution. The price we are paying for a new quantitative scheme of description, that of quantum mechanics, is that we are losing the knowledge of specific numerical data, in this particular case the knowledge of a well-defined orbit for the particle.

Many experiences of the physicist indicate that this way of thinking about conceptual innovation can be generalized. The renunciation of knowledge just quoted expresses a psychological condition of the scientist rather than the purely abstract replacement of one set of formal relationships by another. There are other examples from the history of physics: the requirement of the model of a spherical earth is that one renounce the knowledge of an Absolute Up and an Absolute Down, so "obvious" to the uninstructed mind and adopted without qualm by Aristotle. We see that two distinct steps are involved in a change of "model" that involves major conceptual novelty. In the first place a new formal structure is required. In the second place one needs an adaptation of the language. Bohr's renunciation of knowledge is just one aspect of this, corresponding to a somewhat negative feeling; it expresses a sort of nostalgia for old, familiar modes of thought. In biology, there are concepts which are novel in the formal theory but which in practice are old and familiar; such traits are exemplified by the paradox of heredity.

The history of conceptual innovations in physics has shown that it is all but impossible to carry through such an innovation unless a novel formal structure is available in the first place. Experience then shows that once the formal structure is agreed upon, the adaptation of the language is very much easier; in fact, I do not know of any case in the history of physics where a major adaptation of the language would have occurred in the absence of a formal scheme. In physics the formal structure has always been mathematical; in biology it is, as we have seen, *logical*. Now the aim of these pages, based on the author's previous, extensive work, was to express the necessary formal structure in terms of a set of simple propositions, as simple as we could make them. This led us to the two postulates enunciated. These postulates would be meaningless unless they could be based on the principle of finite classes, which assures us that any statement of biology is a specialization and not a generalization of the content of physics; hence, no logical contradiction between biology and physics is ever possible.

But the innovation implied by our first postulate, that of holistic selection, cannot be directly compared with the innovation necessary to go from conventional, "classical" mechanics to quantum mechanics. In the last-named case the physicist finds himself confronted with two mutually exclusive alternatives, the model of corpuscles and the model of waves. A formal scheme, consisting of the mathematics of complementarity, appears now and tempers the logical contradiction to a level acceptable to the physicist: instead of a flat logical contradiction between the picture of corpuscles and that of waves, one speaks of two aspects of reality that cannot be observed simultaneously. Experimental setups designed for the observation of waves are such that they cannot reveal the presence of particles, and vice versa.

In the case of the postulate of holistic selection, no dualism is apparent, and the same holds for the postulate of information stability. The ancient antithesis of preformation versus epigenesis expresses two systems of explanation that are logically incompatible with each other. But the innovation proposed, the selection of an admissible pattern by nature itself, does not establish a mutual exclusiveness of two alternatives. This brings us back to Bohr's "generalized complementarity" mentioned toward the end of Section IV. The selection stipulated by our postulates seems not to be related to any conflict between two mutually contradictory concepts even if it can be so formulated in terms of pure abstractions. Hence, we are better off when we conceive of the selection in a more

direct sense: the selected pattern supersedes the logical division of the two models and creates in a manner of speaking a new unity, that of the organism.

Now if this is so, the question does almost at once arise of why nature goes to such great length in transmitting specific hereditary information by the use of the genetic code. Obviously, a scheme of theoretical biology which cannot make the existence of hereditary transmission by the code altogether plausible is worthless. In concluding this review, I wish to say a few words about the interpretation of the genetic code. It is usually assumed that the code transmit *the* information from one generation to the next. But what is *the* information? As we have discussed, and as is implied by the paradox of heredity, till information is subject to progressive degradation by errors, as time goes on. But depending on the item involved, this degradation can be more or less critical. Let me, as a very crude scheme, divide all information into "precise" components and "loose" components. Even a superficial view will indicate that much of the information required for morphology is loose, often very loose. Thus, the size of a man, or of an elephant for that matter, may change by 30%, say, without affecting the viability of the specimen appreciably. Similar remarks apply to a great many other morphological features, perhaps to their overwhelming majority.

But the function of the information embedded in the genetic code and used to reconstruct the sequence of amino acid residues in a peptide or protein molecule is altogether different. Here, only *discrete* alternatives are available. As is well known, errors in two or three of the members of the amino acid sequence will change the conformation of the resulting enzyme sufficiently so that the enzyme loses all or most of its specific chemical activity. The enzyme is first and foremost a *tool* of the living organism. As every engineer and every toolmaker knows, the requirements to be put on a tool are much higher than those that apply to the object shaped by the tool: the tool may have to be made of hardened steel where the objects produced may consist of soft iron. We thus can gain a new insight into the nature of the information transmitted by means of the genetic code. It would be extremely "expensive" for the organism to treat the precise information in the same way the loose information of morphology is treated; it would indeed be impossible because one could not build up a specific organic structure without having *some* specific information. Evidently nature chooses the precise information as the part to be transmitted, making it easier in this way for the "epigenetic" part of the information to be regenerated by the process of holistic selection. Clearly, any such statement is a clumsy effort to express in words of the ordinary language something that cannot be so expressed.

In this contribution I have tried to utilize the experience of the physicist which tells us that a conceptual innovation cannot in practice be carried out unless a new formal scheme has in the first place been made available. But in the transition from physics to biology the innovations are logical rather than mathematical. On raising a widespread experience made in the statistical mechanics of organic compounds to a general principle, the principle of finite classes, we succeeded in reducing biological regularity to a special case of patterns compatible with physical law rather than to a logical extension of the latter. This step, as we saw, removes any contradiction between laws of physics and biological regularities. But in order to carry through such a program, we had to recognize heterogeneous, finite classes as independent formal elements of scientific description, on a par with the laws of nature so familiar to the physicist. This

is a decisively novel point which defines a *program*; I can do no more here than to recommend this program to the consideration of my colleagues. It is, hopefully, a reasonably complete formal scheme, but the adaptation of language, as I called it a little while ago, corresponding to the application of the scheme to numerous concrete instances, will no doubt be a lengthy undertaking.

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