Zygon | *Journal of* RELIGION AND SCIENCE VOL. 11, NO. 4

REDUCTIONISM: A REVIEW OF THE EPISTEMOLOGICAL ISSUES AND THEIR RELEVANCE TO BIOLOGY AND THE PROBLEM OF CONSCIOUSNESS

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Scientists are not given to analyzing, any more than other human beings, the philosophical presuppositions of their activities, for it has been remarked, with some justification, that the average scientist knows no more about what he is doing than the average centipede knows how it walks. This may be an unnecessarily severe stricture on scientists, certainly on those who would interest themselves in a paper of this kind; but it is extraordinarily easy for the scientist—say, a molecular biologist, who employs methodologically reductionist concepts (see below) in order to pursue his particular kind of research —to carry over that attitude into a more general philosophical posi-

A. R. Peacocke, dean and tutor, Clare College, Cambridge University, presented this paper at a meeting ("The Problem of Consciousness") of the Science and Religion Forum at van Mildert College, Durham University, England, April 10–12, 1975. He writes, "This paper is greatly indebted to the discussions and papers circulated in Oxford during 1971–72 among a group of scientists, theologians, and philosophers, including Rev. A. E. Dyson, Dr. R. S. Harwood, Dr. J. D. Lambert, Mr. J. R. Lucas, Dr. D. W. Millard, Dr. A. Miller, Prof. W. D. M. Paton, Mr. A. J. Robins, Mr. Howard Robinson, and Dr. J. A. Russell; I hope they will forgive me if I have misunderstood their views."

[*Zygon*, vol. 11, no. 4 (December 1976).]

tion. Then the procedure of analyzing a biological organism, or part of it, by means of physical and/or chemical[1] techniques becomes a philosophical belief that a biological organism is "nothing but" a physicochemical system.

Occasionally, such a view is explicitly urged as, for example, by Francis Crick with respect to biology: "The ultimate aim of the modern movement in biology is in fact to explain *all* biology in terms of physics and chemistry."[2] This is a fairly blunt statement of an extreme reductionist position, but some form of this view is very naturally espoused by scientists accustomed to investigating complex (e.g., biological) systems by taking them apart in order to see how the component units interlock temporally and spatially.

The Relevance of the Question of Reductionism

Whether explicitly or only implicitly embraced, a reductionist interpretation of the relations among the different sciences can limit and determine any understanding of the nature of man in general and of his consciousness in particular. It is therefore appropriate that we begin our deliberations on the problem of consciousness by trying to clarify this matter of reductionism. For if it is true that chemistry is nothing but physics, the biochemistry of cells nothing but chemistry, the biology of organisms nothing but the biochemistry of cells, and ecology and sociology nothing but biology, then the branch from this main line—the statement that consciousness is nothing but neurophysiological events which are nothing but biochemistry, etc., and so down the chain to physics again—becomes the more plausible and, indeed, attractive to the point of being compelling. I think this possibly accounts for the popularity of reductionist views among scientists, in spite of the obvious vulnerability[3] of the kind of reductionist circle which the view generates in its crudest forms. Physiological processes are merely forms of applied biochemistry, which is merely applied chemistry, which is merely applied physics, which is merely the application of mathematical truth, which is merely the result of the laws of logic and of forms of thought, which are merely the product of social, cultural, and linguistic influences, which are merely the expression of psychological mechanisms, which are merely physiological processes. . . .

All-embracing chains of reduction, such as history–psychology–biology–physics-and-chemistry, have a seductive simplicity which obscures the weaknesses of many of the links. Thus, to take the first of these "links," even if we had a causal predictive account of the psychology of all particular individuals, the intersection between psychological events and events in the physical world would still often have the character of accidents since the predictability of both psychological and physical event is relative to its own previous variables.[4] The intersection of causal events is an "accident" relative to one's understanding of the individual psychology and of the nexus of physical events. This unpredictable character of "accidents" which involve human beings and their encounters with the physical (and biological) worlds is amplified in human history (e.g., *this* head of state killed at *this* point of time, before *that* meeting, had an irreversible, profound influence on human history). Thus the proposed all-embracing reductive chain lacks even the first link, that between history and psychology.[5] Clearly, in such series of reductive links, more is being affirmed than simply prescriptions for research strategies, and this "more" is indeed often not what scientists would wish to sponsor. Careful distinctions between different kinds of reductionism seem to be required, if only to ascertain what kind of talk about reducing mental events to physicochemical ones is licensed by the general nature of the relationships among the sciences.

The question is, moreover, not without pressing concern in such a human situation as the relation of a clinician to his patient.[6] Conventional medical education is "reductionistic" in the sense of attempting to reduce the account of pathological events to physicochemical terms. Medical doctors are trained to believe that there is an "explanation" in such terms, which is only deficient or not available because of our lack of knowledge and of adequate research. From such attitudes it is very easy to step into the kind of more general, philosophical reductionism with which we are here concerned. In practice, there can be visualized a spectrum of disease—at one end, purely physical illnesses which could be described in terms of, for example, virulent organisms meeting susceptible hosts with predictable responses and adequately describable in physicochemical terms; at the other end, neurological and psychiatric illnesses in which our understanding of the chemical and electrophysiological events underlying (however obscurely) mental processes is very elementary or nonexistent. What the clinician knows is that, even if he had a full, physicochemical account of the processes involved in these latter illnesses, it would only be a part (and that a small one in many cases) of coping with the complex interrelation of factors which contribute to the illness and which mar the total integration of a human being.

The clinician becomes acutely aware of this when he has to apply his scientific training, with its reductionistic implications, to individuals with whom he has professional and personal relationships. This personal nature of the doctor-patient relationship may lead to a lack of scientific objectivity, but often the effect of the doctor's personality has a more positive influence on the patient's recovery of health than does the exclusive application of detailed scientific knowledge. The patient responds to the will of the doctor, explicit or not, in a way in which the contents of a test tube do not respond to the will of a chemist.

Certainly, the doctor finds himself at two or more levels in the hierarchy of explanatory schemes and has to act as if human individuals have more value than their constituent elements; and this attitude, superficially at least, is not readily derivable from a reductionist philosophy. The medical doctor often faces moral choices, asking at one and the same time, "What shall I do for him [the patient]?" and "How shall I manipulate this [human] physicochemical mechanism?" The manipulative approach of the second question has been so successful as to engender a world view according to which "how?" seems to be the only question to ask; but the question of what a doctor *should* be doing is not readily answered in a manipulative-mechanistic framework, which cannot therefore be a full and adequate account of the case.

But even this complex personal dilemma of the clinician can be placed within a broader context. For the very institution within which doctor and patient interact can shape the whole situation. It has been found that some institutions, for example, mental hospitals, themselves often disturb behavior.[7] Thus the effects of social organization and systems on human self-understanding and action have to be allowed for; and this, too, implies a hierarchy of complex levels within the person, a hierarchy which the clinician has to recognize.

With this kind of experience in view, it is not surprising that, in the argument about the problem of consciousness, the choice between reductionism and antireductionism is continually raised at many different stages. The position adopted concerning the relation of mind and body, of mental events to physicochemical events, of psychological regularities to physicochemical laws, will be related inevitably to the position adopted with respect to various forms of reductionism. Thus a *materialist* reductionism involves the belief that there is only one kind of "substance," namely, matter, and that the mental aspect of matter, evolved at a certain stage on earth, is but an aspect of matter and not a type of substance in its own right; a *property* reductionism (almost indistinguishable from the foregoing, in the light of the long-continued discussion of what constitutes "substance") attributes the emergence of mental properties to the advent of certain aggregations of material properties, and mental properties are then properties of material bodies which cease to exist when the latter are dispersed and disorganized; a *nomological* reductionism describes, in principle, the behavior of a conscious being solely by reference to physicochemical laws, so that the conduct of the organism as a whole is therefore summable from that of its parts, using only physicochemical laws—and this amounts to a ban on emergent laws at the level of mind. Clearly, a number of interrelated concepts are involved here, including both the problem of emergence (of new things, new properties, new laws) and the characterization of and distinction between mental and physical states (to which H. M. Robinson refers in his paper).[8]

The choice of attitudes with respect to reductionism has many repercussions in areas of particular interest to those concerned with the interface between science and religion, particularly Christian theology. In the following, I attempt to report the distinctions among different meanings of reductionism which have been distinguished in the literature. Unfortunately, these distinctions are not always heeded, so that much ambiguity and confusion often occur when scientists, at least, discuss these matters. It soon transpires that one form of reductionism which has been commended by some philosophers of science is epistemological (or linguistic) reductionism, according to which the theories, laws, and statements in one field of science (e.g., biology) are but special cases of theories, etc., in some other branch of science (e.g., physics-and-chemistry, in this instance). It is to be distinguished from nonautonomy (= reduction) of higher-level processes to lower-level processes. Recent considerations of the kind of hierarchies that prevail in actual systems, in theories, and in the relations between the sciences have important implications for the whole question of epistemological reductionism; and the greater part of the following survey is concerned with this aspect of the matter (with special reference to Morton Beckner's contribution to the conference on "Problems of Reduction in Biology" in Bellagio, Italy, in 1972, and to other papers given on that occasion).[9]

This survey (covering the views, inter alia, of Crick, Francisco J. Ayala, Theodosius Dobzhansky, W. H. Thorpe, Ernest Nagel, J. R. Lucas, Beckner, C. G. Hempel, K. F. Schaffner, P. Medawar, J. J. C. Smart, and Michael Polanyi) leads me to the view that an epistemological antireductionist position (theory autonomy, "weak organicism") is defensible both with respect to the general relation of biology to physics-and-chemistry and to the particular relation of consciousness to neurophysiological events. A precise and, I think, helpful application of the adjective "emergent" to consciousness can be made; and this, in its turn, provides a more fertile basis for the science-religion dialogue than is allowed by crude forms of either ontological reductionism or supernaturalism.

Types of Reductionism

Reference has already been made to one classification of reductionism (viz., materialist, property, and nomological reductionism) according to the sphere of operation of the reduction (viz., substance, property, and law, respectively). This was useful in relation to the mind-body problem, but more general distinctions have been made along the following lines.[10]

METHODOLOGICAL REDUCTIONISM

Even before the dramatic successes of "molecular biology" in recent decades (though in this respect it was operating in much the same way as had biochemistry since the beginning of the century), the progress of the natural sciences could already be attributed, in part, to their analytical propensity to break down unintelligible, complex wholes into experimentally and theoretically more manageable lower levels of organization of component units for the purposes of exploration. Briefly, one builds up from the micro level to the macro level. This prescription for research has been vindicated throughout a wide range of the sciences and is scarcely a matter of controversy. In particular, even the most holistic of biologists would not deny the value of the unraveling of the molecular basis of heredity in DNA (deoxyribonucleic acid) or the protein-structural basis of immunological response. Indeed, this strategy is an inevitable consequence of the natural world consisting of a hierarchy of organized systems at multiple levels, one system (e.g., biological macromolecules and living organisms) constituting the interacting units from which more complex systems are assembled at the next level (i.e., in single living cells and in ecosystems of populations of organisms in their environment, respectively). Such relationships, which emerge empirically, necessitate an analytical strategy of a methodologically reductionist kind. So each "science" becomes a relatively autonomous interlocking network of theories, descriptions, concepts, experimental techniques, fields of observations, and (one should honestly add) also of individual scientists and their network of personal relationships. There is nothing wrong in this, and it is widely accepted as the way in which a science progresses, even though the relationship of the hierarchy of *systems* which are studied and the hierarchy of *theories* may not be well understood, as Beckner has argued, and has been grist to the mill of arguments about reductionism.

Controversy on methodological reductionism begins only when exclusive claims are made on behalf of the analytical reductionist prescription for research that this is the only fruitful and legitimate mode of investigation, in biology in particular. Then biologists such as Dobzhansky, who was not intellectually comfortable with a reductionist methodology and explanations, have to urge the claims of a contrasting methodology, which can be broadly called "compositionist." The compositionist approach recognizes that in biology there are several hierarchically superimposed levels of integration of structures and functions. For example, the use of adaptations, of structures and processes, to the whole organism for survival and reproduction in an environment has to be the focus of attention at a level of combination of many factors. A trait of the whole organism can only be described and known as "adaptive" in relation to this total composition. (It is, in fact, a trait which enhances the probability of survival and reproduction in this milieu.) Dobzhansky argued that the biologist does not really have to choose between a reductionist and compositionist approach: Both are equally necessary; they are complementary to each other; each is incomplete without the other; and, indeed, at the present state of knowledge, each is incomplete by itself.[11]

Such a view recognizes that the various levels of integration of structure and function (e.g., along the series—ecosystems, populations, individuals, cells and organelles, macromolecules) necessitate methods, both intellectual and experimental, which are specific to those levels, namely (in the same order as in the parenthesis above), community ecology, zoo and plant geography; population ecology, population genetics; morphology, physiology, behavior studies, developmental genetics; cytology, cell physiology; biochemistry, biophysics. Understanding of both the pattern and the component units of any one level is required.

This eirenic approach rejects the extreme, methodological-reductionist view that complete knowledge of components will automatically reveal the patterns which they compose. The very complexity of organismic patterns of structure and function should deter one from adopting this extreme position, and few will be found to do so. But some reductionists, while recognizing the complementarity of reductionist and compositionist methodologies, nevertheless do so only as a kind of concession to the present incompleteness of our knowledge and still believe, however vaguely, that the expectation is that all the sciences, in particular biology, will be reduced to physics and chemistry. In what sense this view has been propounded and what its validity is can emerge only if we examine other, more metaphysical forms of reductionism. These raise much broader questions of the kind, "Is the relation between theories at different levels purely linguistic, for example, a relation between biological statements and physicochemical statements?" and "What do we count as explanations, and do they operate in the same way at different levels of complexity?" Clearly, the issue of reductionism in this more metaphysical mode implicates many major problems in the philosophy of science. In what follows, I shall try to isolate the issue of reductionism as far as possible but with the warning that, in the long run, it is a Pandora's box in the philosophy of science.

ONTOLOGICAL REDUCTIONISM

Put crudely, ontological reductionism consists in the view that complex wholes (in particular, and in our focus of interest, biological organisms) are "nothing but" their component parts. It is a statement about what certain entities *are:* hence the designation of this form of reductionism as "ontological," that is, concerned with being. Everything, however, then turns on how the "nothing but" in this (too) brief statement of ontological reductionism is elaborated and unpacked. In the literature on this subject, the term "ontological reductionism" has been used in two ways, which must now be discussed (again with the relation of biology to physics and chemistry especially in view). The first, *A*, implies that the laws of physics and chemistry apply to all biological processes at the atomic and molecular levels and excludes all "vitalist" views which suggest that some entity or substance is added to atoms and molecules to constitute them as living organisms. The second, *B*, asserts that biological organisms are "nothing but" atoms and molecules and implies that a physicochemical account of their atomic and molecular processes is all there is to be said about them. Let us consider each position more fully.

Ontological Reductionism A. In this form, upon which there would be wide agreement, the ontologically reductionist position simply asserts that complex wholes are indeed made up of units that obey their own particular laws which are not abrogated by their assembly into these larger wholes—that (e.g.) physicochemical entities and processes underlie all living phenomena. Ontological reductionism so defined, Ayala can then go on to say that this term "implies that the laws of physics and chemistry fully apply to all biological processes at the level of atoms and molecules";[12] and he reemphasizes the remark of Dobzhansky to the effect that "most biologists . . . are reductionists [ontologically] to the extent that we see life as a highly complex, highly special and highly improbable pattern of physical and chemical processes."[13] Or one could instance C. N. Hinshelwood: "One might in very general terms regard a mass of living matter as a macromolecular, polyfunctional free radical system, of low entropy in virtue of its order, with low activation energy for various reactions in virtue of its centres, and possessing a degree of permanence in virtue of a relatively rigid structure."[14] This view is distinguished particularly by what it excludes—namely, that certain entities other than physicochemical ones operate and are present in biological organisms and so constitute them as "living" organizations of matter. This latter view (denoted as "vitalism") is almost totally rejected by biological scientists even when they take up positions opposed to other forms of reductionism. This leaves open the possibility that one can be both antireductionist and antivitalist, as will be shown. It is important to note that what is excluded by this view is the existence of any ontological entities (be they ethereal soul stuff, élan vital, life force, or entelechy), the addition of which to the organization of atoms and molecules then constitutes them as living. This is tantamount to asserting that no extra "substance" is added to atoms and molecules when they adopt the complex organization which is characterized as "living." If that is what is being rejected, then this form of ontological reductionism must, and indeed does, command almost universal assent. For the biological sciences have scored their most spectacular successes by acting on nonvitalist assumptions. For these reasons, form *A* of ontological reductionism is acceptable even to many who are antireductionist, in the sense that they deny some of the metaphysical forms of reductionism yet to be described. This having been made clear, it is important not to go on to draw a further, wrong conclusion.

Ontological Reductionism B. Because complex wholes are made up of constituent units which obey their own laws at their own level (e.g., biological organisms are made up of molecules which obey physicochemical laws), this need not mean that merely instancing the component units (molecules, etc.) of a complex whole (living organism) entails there being nothing else to be said. It is indeed true that the answer to "What else is there [e.g., other than atoms or molecules in a living organism]?" is "no-*thing* at all," but this need not mean that describing the molecular constituents and their properties is all there is to be said (i.e., that there is *nothing more*).[15] This view certainly has strength and the attraction of simplicity and so can often appear to render other views, if not demonstrably false, at least redundant.

This extreme and rather broad form of ontological reductionism, which is here denoted as *B* and which has been dubbed colloquially as "nothing buttery" by D. M. Mackay,[16] is opposed by those who wish to assert, as I do, that, even while recognizing that the constituent units of a complex whole (such as atoms and molecules in a living organism) obey their relevant laws at their own level, there is indeed much more to be said. It may be true that even the Archbishop of Canterbury is

59 percent water, but so also are General Amin and the latest Nobel laureate. There is something more to be said, even if one does not want to say that there is some special entity present in living organisms.

At the beginning of this paper, I quoted Crick's remark, "The ultimate aim of the modern movement in biology is in fact to explain *all* biology in terms of physics and chemistry," as a typically strongly reductionist statement. Indeed, it would amount to an avowal of ontological reductionism *B* if, as it seems superficially, the "physics and chemistry" in terms of which he claims biology is to be explained are the laws of physics and chemistry of atoms and molecules and their processes only as we know them at present and only as they are applied to atomic and molecular processes as such. So interpreted, this remark has often been regarded as a classic statement of ontological reductionism *B* and has been opposed by many authors.[17] However, if by "physics and chemistry" Crick meant—and the research activities of molecular biologists would be consistent with this—a much expanded form of these sciences, then what he is asserting becomes more obscure. Is the expansion of physics and chemistry going to absorb into its conceptual schemes more purely biological concepts such as adaptation, immune response, and so on? If so, the designation of that according to which biology is to be explained as "physics and chemistry" becomes a purely semantic operation and a misleading one at that. However, even this statement by Crick, which has often been taken as typical of ontological reductionism *B*, is explicitly about explanation and not ontology as such, although it is a natural conclusion and often also a hidden implication that biological organisms *are* "nothing but atoms and molecules" if they are fully explained in terms of the sciences of atoms and molecules. So this nest of ambiguities leads us inevitably into the consideration of epistemological reductionism.

In brief and with reference to biology, ontological reductionism *A*, which asserts that the laws of physics and chemistry fully apply to all biological processes at the level of atoms and molecules, seems scarcely to be in dispute; but ontological reductionism *B*, which, in asserting that biological organisms are "nothing but" atoms and molecules, seems to be implying that a physicochemical account of their atomic and molecular processes is all there is to be said, is widely regarded as inadequate, and is disputed. Reductionism *B*, which is often expressed ambiguously, is concerned with explanation. This leads naturally to a consideration of the "something more" that has to be said.

EPISTEMOLOGICAL REDUCTIONISM

The philosophical debate on reductionism has centered principally on "whether the theories and experimental laws formulated in one field of science can be shown to be special cases of theories and laws formulated in some other branch of science," and, "if such is the case, the former branch of science is said to have been reduced to the latter."[18] This is an epistemological—even linguistic—kind of reduction and, not surprisingly, touches on important problems in the philosophy of science as well as, more generally, in metaphysics. The literature on this subject is widespread and is engendered not only by philosophers of science but also by scientists (especially biologists) concerned with the relation of their science to others and by scientists with wider philosophical and theological concerns. Some of this literature is cited below, but fortunately a valuable volume has recently appeared, giving an account of a symposium on this theme;[19] what follows is much indebted to the papers published in that volume, and it should be consulted for appropriate elaboration of the brief account given here.

Wholes, Parts, and Relations. Discussions of reductionism are often associated with the theme of the relation between wholes and parts. For example, Thorpe quotes C. D. Broad's contention that in order to explain the behavior of any whole in terms of its structure and components "we need to know the law or laws according to which the behaviour of the separate parts is compounded when they are acting together."[20] The laws that explain the behavior of the whole, it is implied, cannot be derived from the laws that explain the behavior of the parts, acting separately. At least this is what defines, it is being suggested, an organic whole or unity to which epistemological reduction is inapplicable. The meaning and usage of the terms "whole," "parts," "sum," and "organic unity" have been carefully analyzed by Nagel.[21] The fundamental issue of relevance here is whether the analysis of "organic unities," of organic wholes, necessarily involves the adoption of irreducible laws for such systems and whether their organization is of such a kind as to preclude a simple summation of their parts to yield the whole, that is, an "additive" analysis. Such an additive analysis appears to be one which accounts for the properties of a system in terms of assumptions about its constituents, taken in isolation from the system; and "a 'non-additive' analysis seems to be one which formulates the characteristics of a system in terms of relations between certain of its parts as functioning elements in the system."[22] Nagel concludes that, although there is no doubt that there are functional wholes whose constituent parts are "internally" related (in the sense that these constituents stand to each other in relations of mutual causal interdependence), "some functional wholes certainly can be analyzed in that manner [from the additive point of view] while in the case of others (for example, living organisms) no fully satisfactory analysis of that type has yet been achieved. Accordingly the mere fact that a system is a structure of dynamically interrelated parts does not suffice, by itself, to prove that the laws of such a system cannot be reduced to some theory developed initially for certain assumed constituents of the system."[23] This conclusion, modest though it is, shows that the issue of the relation of wholes to parts, whether "additive" or "nonadditive," is not one which can be settled in a wholesale and a priori fashion. Each system needs to be examined on its own merits in this regard, even each biological system and level of inquiry (and indeed Thorpe discriminates in precisely this way).[24]

In this and other discussions of the relation of constituent parts to the "whole" to which they belong, the reality of relations, as entirely on a par with qualities, is taken for granted. There is indeed no logical warrant for making any distinction between the status of qualities and of relations.[25] Once this is allowed, the question of the ontological reductionist *B*—"What else is there [other than the constituent elements]?"—is evidently only a partial question and may need to be supplemented by "How are its constituents combined?" We are then concerned with internal structure and the relation of different elements to one another. The focus of interest of the biologist, for example, is the structure and the continuously functioning processes of life, although he inevitably also needs to know what kinds of atoms and molecules are present (but not, of course, which particular ones are there at any given moment).

Hierarchies of Systems, Theories, and Sciences: Theoretical Models for Resolving Some of the Issues of Reductionism. The expansion of our knowledge of the natural world which has occurred particularly in this century has shown it more and more to consist of a hierarchy of systems, and this is particularly true of the various levels of organization to be observed in living organisms. The sequence of increasing complexity to be found in the living world (atom, molecule, macromolecule, subcellular organelle, cell, multicellular functioning organ, whole living organism, population of organisms, ecosystem) represents a series of levels of organization of matter in which each successive member of the series is a "whole" constituted of "parts" preceding it in the series (for convenience, let each member of this series be called "higher" than the one preceding it in the list above). The higher members are "wholes," in Nagel's sense of being "pattern[s] or configuration[s] formed by elements standing to each other in certain relations" and, more to the point, being "organized systems of dynamically interrelated parts."[26] The issue, with respect to the biological level, is whether biological organisms and systems are indeed "organic unities," which require nonadditive analysis in Nagel's sense.[27] To resolve this issue, it is necessary to distinguish carefully the hierarchy of systems from the hierarchy of the theories of the sciences concerned with the systems.

The *hierarchy of natural systems* has been described frequently and needs no further elaboration, especially as it is not in dispute among scientists.[28] The concept of a set of hierarchically organized systems has also been thoroughly investigated from a more abstract viewpoint, and the conditions for a set of part-whole relations to constitute a "perfect hierarchy" have been formulated by Beckner.[29] After affirming that, with some inadequacy of fit (e.g., not all the tissues of an organism are composed of cells), biological organisms may be regarded as cases of such hierarchies, Beckner continues,

> The hierarchy model has historically provided a large part of the framework of discussion in the philosophy of biology. It is involved in a wide range of connected ideas: levels of organisation, sequences of boundaries . . . , autonomy at one level with respect to lower ones, a temporal order in the arrival of the higher levels on the cosmic scene, the emergence of higher-level entities, etc. The existence of hierarchical systems is certainly connected with the hierarchical arrangement of theories. But I do think we lack a detailed philosophical account of the connections, in part because the relations between higher and lower-level theories are not too well understood.[30]

He goes on to argue that our view of what constitutes a *hierarchy of theories* is influenced not only by notions of scope and generality but also by the empirical fact that there are very many hierarchically organized systems, and these provide us with a way of arranging sciences (and their associated theories) on a scale: An i-level theory of an i-level science is then one that is largely concerned with i-level systems; and the i-level science is comprised of theories, T_i (and also experimental laws, L_i).[31]

The reducibility of a theory at a higher level, T_h, to a theory at a lower level, T_l (i.e., lower i), has been a central concern of philosophers of science over recent decades.[32] If a theory T_h or experimental law L_h within the science applicable to level L_h can be shown, in some sense (see below), to be a special case of a theory (and laws) T_l formulated within the science applicable to a lower level L_l, then T_h is said to have been reduced to T_l. Possible samples of such pairs, T_h and T_l, might be, respectively: (much of chemistry)-(physics); (geometrical optics)-(physical optics); (gas laws)-(laws of molecular motion); (classical thermodynamics)-(statistics of molecular motion); (Mendelian genetics)-(DNA structure, coding, and transcription). More precisely, Nagel formulates reduction as the *explicability* of all the laws of T_h in the theory T_l, and the logical conditions for this which he has formulated have commanded wide assent. They are (1) the condition of connectability—that all the concepts or terms of T_h can be systematically connected with some of the terms of T_l (by synthetic-identity statements, explicit definitions, or some other semantic relation)—and (2) the condition of derivability—that each law L_h is deducible from T_l, together with statements which connect the respective concepts or terms of the two levels L_h and L_l and a description of relevant boundary conditions in the vocabulary of T_l. It should be noted that, according to this definition, reduction is distinctively linguistic, for it is the deduction of one set of empirically confirmable statements from another such set—and not the derivation of the properties of one subject matter from the properties of another—because the "nature" of things (especially the elementary constituents of things) is not accessible to direct inspection.[33] Nagel regards it as "hopelessly and irresolvably speculative" to try to make reduction the deduction of properties or "natures" from other properties or "natures," that is, to attempt ontological reductions of type *B*. Hempel regards this linguistic turn to the originally ontological character of the reductionist question as an oversimplification since it construes reduction as a strictly deductive relation between statements (or principles) in two theories.[34] The "oversimplification" to which he objects arises, in the case of the mechanistic program for the reduction of biology (as T_h) to physicochemistry (as T_l), in the claim that, in applying the condition of connectability, all biological concepts and terms can be extensionally characterized in physicochemical concepts and terms. This has to be insisted upon, in this program, for otherwise the "connecting principles" (cf. Nagel) would merely constitute additional biological laws if they expressed only the necessary or sufficient physicochemical conditions for biological concepts, so that the reduction would then be incomplete at stage 1, connectability. However, since connecting principles which are presently available do not begin to suffice to characterize all concepts and terms of biology (T_h) in physicochemistry (T_l), the program of mechanistic reduction appears to be untenably oversimplistic in Hempel's view.

Lucas has also drawn attention to the question of whether physicochemical reduction can be as "complete" (in the technical, logical sense) as some mechanists have traditionally believed. For the claim that such physicochemical reduction not only can explain everything but does not even allow that anything could be in any way different from what this reduction says it should be does not, Lucas argues, seem to work out in practice. He says that this claim is wrong because only rather restricted theories are complete. Yet "incomplete theories can be compatible without being the same in all their consequences. And so we can accept the possibility of physical and chemical explanations without thereby excluding that of biological explanations that are essentially different."[35]

The foregoing represents only a small, although central, sample of the discussions by philosophers of science about the question of reduction of theories in science. This not inconsiderable activity[36] has contributed valuably to our understanding of science and also has implications for interpreting its history as well as for assessing the actual success of attempts at reduction.[37]

Similar issues have arisen when practicing scientists have reflected on the *hierarchy of the sciences.*[38] Thus Medawar notes that as one goes along the series physics-chemistry-biology-ecology/sociology each represents a subclass within the possible interaction of the units of the preceding level, and "the sciences become richer and richer in their empirical content and new concepts emerge at each level which simply do not appear in the preceding science."[39] Corresponding to each level in the hierarchy of systems, the appropriate science employs concepts which are peculiar to it and indeed have little meaning for levels lower down (or even higher up in some cases): "As new forms of matter, non-living and living, emerge in the universe, new categories of description of their form and properties . . . are necessary and these categories will be other than those of the physics and chemistry appropriate to the subnuclear, atomic and molecular levels."[40] Medawar sees an analogy between the relation of the foregoing sequence of the sciences and the relation between, successively, topology and then projective, affine, and Euclidean geometry. In each case the group of operations defining the preceding in the series has as a subgroup the group of operations defining the next, and the concepts of the geometries become progressively richer and more particular. Moreover, every statement true earlier (or lower)[41] in the series (whether of geometries or of sciences) is true in the later (or higher), but these statements are usually not the focus of interest for the practitioners of a higher-level science because they do not constitute their distinctive problem. For such reasons, sociologists have insisted on the distinctiveness of sociological concepts from biological, and biologists the distinctiveness of their concepts in relation to physics and chemistry.

Apparently, what many practicing scientists are concerned to emphasize is this distinctiveness of the concepts of their own science, of which they are only too well aware when they try to communicate with scientists of other disciplines. This distinctiveness has been more precisely delineated by Beckner as *theory autonomy,* the autonomy of higher-level theories (T_h) with respect to lower-level theories (T_l), in the sense that the higher are not epistemologically reducible (according to Nagel's criteria) to the lower.[42] This analysis stresses theory autonomy as a relation between parts of scientific languages and is carefully distinguished by Beckner from *process autonomy*, which "has nothing to do with the languages we choose to describe [processes] but rather with some sort of causal independence."[43] He defines a process P_h, at level L_h in a hierarchy, as being autonomous with respect to processes in a "lower" level L_l if, and only if, the laws of P_h are not fully determined by the laws of processes (of a different kind) at level L_l.[44]

Beckner argues that much of the confusion in the discussion of reductionism has resulted from a failure to distinguish adequately between, on the one hand, the hierarchy of sciences, with their associated theories, concepts, descriptions, etc., and the logical relationships among them (i.e., the question of autonomy of theories) and, on the other hand, the hierarchy of actual systems and the real relations (causal, spatial, temporal, part-whole, identity) among the events, processes, etc., occurring at each level in this hierarchy (i.e., the question of the autonomy of processes). Causal connections have to be more carefully defined and analyzed for hierarchical systems than they do for pairs of systems which have no parts in common.

With these rather sharp distinctions, Beckner has little difficulty in showing that the autonomy of higher-level processes does not follow from theory autonomy, for the irreducibility of a higher-level theory (T_h) to a lower-level theory (T_l) may be due to differences in their conceptual structure and not to lack of determination of processes in the higher level (L_h) by processes in the lower level (L_l). More formally, let superscripts Aut = "is autonomous" and Red = "is reducible" (à la Nagel), T_h = a "higher-level" theory, and P_h = a "higher-level" process, so that $T_h^{Aut} = -T_h^{Red}$, by definition. So, if $T_h^{Red} \rightarrow -P_h^{Aut}$, as all could agree, then it follows that $- -P_h^{Aut} \rightarrow -T_h^{Red}$; that is, $P_h^{Aut} \rightarrow T_h^{Aut}$, but not that $-T_h^{Red} \rightarrow - -P_h^{Aut}$; that is, not that $T_h^{Aut} \rightarrow P_h^{Aut}$.

Beckner's paper casts much light on the reductionism question, for example, when he delineates the various confusions which result when the special character of hierarchical systems is overlooked.[45] His distinction between process and theory autonomy also serves to distinguish two forms of reductionism and antireductionism concerned, respectively, with processes and theory.[46] It may be helpful to set out these distinctions (see table 1).

TABLE 1

	Type of Reductionism	Type of Antireductionism
Higher-level processes: P_h	? Ontological B (not autonomous): $-P_h^{Aut}$	Strong organicism* (autonomous): P_h^{Aut}
Higher-level theories: T_h	Epistemological (reducible): $T_h^{Red} = -T_h^{Aut}$	Weak organicism* (autonomous = not reducible): $T_h^{Aut} = -T_h^{Red}$

*Morton Beckner's terms (see n. 9 below).

Reduction of theories can certainly be called *epistemological,* for it is about "the logical relations that hold between theories, descriptions, conceptual schemes and other instances of language."[47] But the autonomy of processes is about what "we may as well call real relations (causal, identity, spatial, temporal, part-whole, etc.) between the events and other phenomena that our languages describe."[48] So this question of the autonomy of processes possibly comes more within the area I have designated as ontological B or "nothing buttery," the doubt arising because of the ambiguity of reference, with respect to theories or processes, of much of the discussion of ontological reductionism. The view which supports the autonomy of higher-level processes Beckner calls "strong organicism" (table 1, upper row) and some "antireductionism" is of this kind. Beckner has little difficulty in proving that the strong organicist is driven by the logic of his case to postulate a nonmaterial control entity; that is, he is impelled to vitalism.[49] This is not the case with the epistemological antireductionist or "weak organicist."

Reduction of Biology to Physics and Chemistry

In the light of these analyses of reductionism, where does biology stand in relation to physics and chemistry? We have seen that an attitude of methodological reductionism toward biology is permissible, indeed often necessary for research, but needs always to be balanced by compositionist methodologies. We have seen that an ontological reduction, type A, of biology is generally agreed upon. The argument among biologists concerned with this question then centers chiefly on epistemological reductionism and ontological reductionism B, and the distinction, outlined in table 1, between theory and process autonomy is here of special value.

The biological scientist is certainly keenly conscious not only that biological organization is a hierarchy of parts making wholes at different levels but also that dynamic processes are themselves interlocked dynamically in space and time in more complex networks. Thus processes at the molecular level, such as enzymatic reactions, are part of a network of interlocked reactions of this kind in a metabolic web, itself distributed spatially over a structurally hierarchical framework of organelles, which are themselves interconnected by structures and by chemical messengers in a larger whole (the cell), which is itself—and so on. Hence regarding the hierarchy of systems as simply a static assembly of building blocks constituting different kinds of parts at different levels, so that within each "part," so conceived, particular processes go on and are then just added together to make the next level, does not correspond to the dynamic complexity which the biologist observes in living organisms. He therefore is bound to stress the special concepts he has to employ to describe and understand such complexities. He finds that, at each new level of biological organization, new kinds of interlocking relationships emerge, and these require both new concepts to order them and render them coherent and distinctive experimental techniques and designs of experiment. So he strongly supports the autonomy of biological concepts; that is, he is epistemologically antireductionist, in the sense of the labeling of table 1. It is this autonomy of biological theory in relation to physics and chemistry which numerous biologists have been concerned to emphasize. There are, indeed, distinctive ideas in biology which simply cannot be envisaged or translated into the conceptual terms of physics and chemistry.[50]

In staking out this position for biology, some authors have moved from stressing the autonomy of biological theory to basing their arguments on the fact that biological organisms evidence new, complex relationships between their constituent parts, and it is these relationships per se which, being logically distinct, are then assigned an autonomy of a kind which moves their position closer (although often obscurely so) to the upper row of table 1, that is, to "strong organicism." We have seen that the kind of antireductionist position which argues for theory autonomy is not at all logically committed to process autonomy. Yet some authors, in arguing for the former, slide gently into the latter through an emphasis on the new relationships which biological organisms manifest.

To some philosophers, it is almost trivial and obvious that, as Smart puts it, "new qualities emerge when samples are put together to form a complex,"[51] as when the pieces which constitute a radio set are connected in the proper sequence and then receive signals. Yet this point, regarded by such philosophers as a very modest one, is often the one which, for example, biologists want minimally to affirm when told that their subject is nothing but "physics and chemistry." One form of antireductionist argument which makes this kind of stress on relationships has been developed by Polanyi, who says that there are "boundary conditions" which characterize machines in relation to their components, and he then transfers the same argument to the relation of biology to physics and chemistry.[52] His initial point is that for machines (say, a steam engine) the principles ("boundary conditions") which determine the spatial relationships of the constituent units are those of mechanical engineering and are distinct from the properties of the separate units, described in physicochemical terms. Further kinds of discription are required to explicate the relationships between parts peculiar even to that level of organization of matter which is a machine.[53] This initial stage of Polanyi's argument is vulnerable to the criticism that the concepts of mechanical engineering are reducible to physics and chemistry, at least in principle; for given the parts with their physicochemical properties *and the relationships between these parts,* the operation of the machine can be deduced. It would then seem plausible to argue that mechanical engineering is indeed reducible to physics and chemistry. However, the italicized phrase is crucial to his argument, for the differentiating characteristic of the concepts of mechanical engineering is that they are concerned with these relationships between parts and are, to that extent, distinct from those of physics and chemistry. Whether or not this is sufficient to establish theory autonomy and so lead to the epistemological antireductionism of the kind outlined in table 1 is the question at issue.

Polanyi would regard himself as an antireductionist, yet we find Schaffner making almost the same point, although he clearly sees himself as a reductionist.[54] He defines the "biological principle of reduction" as that, "given an organism composed out of chemical constituents, the present behaviour of that organism is a function [causal or theoretical?] of the constituents as they are characterisable in isolation *plus* the topological causal interstructure of the chemical constituents. (The environment must, of course, in certain conditions, be specified.)"[55] Yet the crux of the question is his "plus"—not to mention the "environment," which must be taken to include that larger part of biology concerned with the interaction of living creatures with one another and with the physical world, in fact in ecosystems. For it is the very nature, character, and existence of what Schaffner calls the "topological causal interstructure" to denote the spatial arrangements and causal signal influences. This is what requires biology to be a distinctive science needing concepts of a quite different kind from those of (say) chemistry, which has to understand molecular behavior in relation to atomic arrangements.

However, this form of the stress on the relationships of the parts (which Nagel also emphasized in his discussion of wholes and parts)[56] has not always impressed philosophers such as Smart or even Beckner. There is a real problem here, it seems, about the logical character of biological theory and to what extent biological theories can be regarded as autonomous (i.e., not reducible in Nagel's sense), simply and only on the grounds that they have to concern themselves with the special kinds of biological interrelationships between units. This is not a question that can readily be settled for all biological theory in toto but is a matter for investigation in each case.

Polanyi's argument has more force and is certainly less superficially vulnerable to the foregoing criticism when he applies it not to a machine but to a biological system—for example, a living cell with its complex configuration in space and time, with its flow of constantly changing substances both within and across the cell membrane, and with its possession of an individual "life cycle." He has based a similar argument on certain molecular structures with biological specificity.[57] The chemical structure of DNA, the covalent (—) and hydrogen bonds (represented by a dot) which link them, thereby enabling specific bases to be paired (A·T, G·C), are describable in terms of the categories of physics and chemistry and are studied on this basis by physical biochemists and biophysicists. Within the double helical structure, there are sequences of base pairs. For example,

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A — G — C — A — G — T —. . .
·   ·   ·   ·   ·   ·     .
T — C — G — T — C — A —. . .
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All such sequences of these four base-pair "units" are equally permissible physicochemically (within certain limits which do not affect the argument) and can fit equally well into the structure. Yet, in the nuclei of any particular cell of a given organism, within its DNA double helices there are particular specific sequences which perform a unique set of coding functions (for the construction, say, of a particular group of enzymes). *This* particular base sequence in *this* DNA has a "meaning" (i.e., a defined readout via the code) only when the DNA has been assembled in that organism and can have its biochemical function as a genetic "blueprint" for the production of, for example, specific proteins in due order only when it functions in the milieu of the whole organism. Chemical processes are, indeed, the means whereby bases are incorporated into chains of DNA, but the sequence in which the bases are assembled in the DNA is a function and property of the whole organism. So this is not an argument for process autonomy. But, since no "laws" or regularities of physics or chemistry describing the nature and stability of the chemical bonds in the DNA as such can specify the actual *sequence* of base pairs in any particular case, this analysis supports the kind of epistemological antireductionism which affirms theory autonomy.

Thus there does seem to be a prima facie case for arguing that some biological concepts, and so theories, are autonomous, not reducible in the strict sense; certainly, biological concepts and theories are distinctive of the biological level in the natural hierarchy of systems, as many have argued.[58] Because of this distinctiveness, it is not likely that all biological theory is going to prove to be reducible to physics and chemistry, judging from the difficulties of reducing even chemistry to physics.[59] Moreover, we do not yet have any overarching biological theories of sufficient power to be analyzable in this way. What the conceptual schemes will be which can deal with such "topological causal interstructures" of the many different kinds discovered and yet to be discovered in biological organisms is still an open question, as is their autonomy or otherwise with respect to lower-level theories. Perhaps the clues will be found in systems theory, as some have urged;[60] or in the science of computers, as Ted Bastin surmises, "because computing systems comprise the most sophisticated combinatorial structures available to us for explanatory purposes and modern molecular biology has become extremely combinatorial in character";[61] or from automata-theoretic models such as "potentially infinite machines: push down storage automata," as D. Berlinski has elaborated upon.[62] If so, it is likely that they will be autonomous. These new conceptual schemes will have to integrate into a single framework the multilevel mode of operation of a biological organism, of which *ex hypothesi* the molecular level described by physics and chemistry would be only one, and will also have to include, in some way as yet unknown, the purposeful exploratory activity of at least the higher organisms. There is no special reason why the molecular should be selected out arbitrarily as that at which alone explanation is necessary to understand all the other levels, for, as Bastin rightly comments, "As far as one can judge at all, the cell cannot be understood in its behavior as the basis of events at the molecular level. One would judge this because the control processes of detailed cell physiology seem to proliferate endlessly in the sense that the more one understands a given chain of reactions and their associated background dynamics, the larger is the number of ancillary, trigger and other, processes which it seems necessary to call in to achieve completeness of explanation and a self-contained causal scheme."[63]

Emergence: Toward a Nonreductionist Account of Consciousness

In the case of hierarchically organized systems, a definition of emergence could be that *i*-level phenomena are "emergent" with respect to lower-level theories when, and only when, the *i*-level theories are not reducible epistemologically (in Nagel's sense already quoted) to the theories of the lower levels.[64] "Emergence" here is synonymous with "theory autonomy," for this definition is relativized to a set of theories but does inadequate justice to the different, nonmutually reducible descriptions under which (like events) any level in a hierarchical system may be subsumed. This underlies, as mentioned earlier, the confusions which occur in discussions involving hierarchies of systems and of theories.[65]

With this widening of the range of discourse, emergence can be regarded as the corollary of the nonreductionistic view of the relation between the sciences developed above. There are qualities, properties, behavior, activities, and characteristics which are distinctive of higher levels in the hierarchy of systems.[66] In particular, as we have seen, there is a case for the autonomy of much distinctively biological theory, and in this sense biology is not reducible to physics and chemistry. But the argument applies all the way up the scale of the hierarchy of the sciences and in particular to consciousness. Psychology, on this brief, is not necessarily reducible to neurophysiology and can have autonomous concepts distinctive of itself. To say this is not to deny that neurophysiological processes occur in the brain, indeed that molecular and atomic ones do so as well, but it is to deny the easy giveaway fallacy of "Consciousness is nothing but the physics and chemistry of the brain" and of all the consequences that are sometimes tritely drawn from this. For, from this approach, consciousness could be regarded as a genuinely emergent feature at that level in the hierarchy of complexity which we call the human brain in the human body and, judging from the work on animal consciousness described by Tim Appleton, also at lower levels in evolution.[67] That is, consciousness is a kind of property and activity which emerges when certain complex structures are evolved. In the same sense as I have argued above that biology need not be reducible to physics and chemistry, so these new structures and relationships between "parts" (now nerves and sense organs as well as ions and macromolecules) exhibit phenomena and behave in ways which could merit unique descriptions, requiring distinctive, autonomous concepts not reducible to those used in the level "below" (neurophysiology and biochemistry). If this were so, consciousness would be not so much the property of a new thing or entity, the "mind," but a genuinely emergent activity and property not epistemologically reducible in the sense already elaborated. So the biologist who adopts a theory-autonomous antireductionism can view consciousness as an emergent; and this allows the use to men both of ordinary and personal language and of poetic and religious (and other) languages to describe and to communicate their states of consciousness. Nevertheless, such a biologist will also stress the continuity in the hierarchical sequence which culminates in conscious (and, indeed, self-conscious) brains in bodies.

It seems to me that this theory-autonomous, antireductionist, and emergentist view of consciousness leaves it as an open philosophical question whether the "mind" (mental states) is or is not identical, contingently or necessarily, with the "brain in the body" (physical states). For such an antireductionist understanding of consciousness as an emergent does not attempt to take into account or to analyze the nature and content of consciousness itself—what it is to be experiencing or having a mental state. Indeed, superficially, it does seem difficult to see how, in principle, the language used to describe our experienced mental states could ever be reduced (in the strict Nagelian sense) to the language of neurophysiology and biochemistry. A permanent gulf seems to be fixed, and this uniqueness may perhaps be associated with the unique position of (at least) human self-consciousness as being the tool by which all the lower levels in the hierarchy of systems are analyzed. It is to this dichotomy in experience that Robinson draws attention in his paper and there, in fact, argues against a materialist account of the mind.[68]

Reductionism and Religion and Science

This paper has constituted an introduction to the first public meeting of the Science and Religion Forum on the problem of consciousness. As such, it may not appear at first to be too directly connected with the central interest of the forum. However, I hope that it will have become clear that the question of reductionism and biology is not irrelevant to that interest. For there are many scientists who, rightly adopting a methodologically reductionist attitude with respect to the relation of biology to physics-and-chemistry, too readily proceed to assume that biological theories as well as processes cannot be autonomous and so extend this account to consciousness in both animals and man. It is then but a small step to that totally reductionist view of human personality which is the antithesis of the sense of significance of persons that has inspired religion in general and is the keystone of Christianity in particular, with its belief in a creating, immanent personal power and presence in the cosmos who became incarnate in a human person in history. Assessment of the problem of consciousness will always be the touchstone of a general philosophical position and so of our attitude to the claims of Christianity. Since this assessment itself turns on the general scientific world view, I trust this paper will have served both to introduce the particular theme of this occasion and to inaugurate the forum itself.

NOTES

1. Henceforward frequently shortened to "physicochemical."
2. Francis H. C. Crick, *Of Molecules and Man* (Seattle: University of Washington Press, 1966), p. 10.
3. W. D. M. Paton, Oxford discussions, 1971–72.
4. R. S. Cohen, "Causation in History," in *Physics, Logic and History*, ed. W. Yourgrau and A. D. Breck (New York: Plenum Press, 1970), pp. 231–45.
5. J. D. Lambert, Oxford discussions, 1971–72.
6. J. A. Russell, Oxford discussions, 1971–72.
7. D. W. Millard, Oxford discussions, 1971–72.
8. H. M. Robinson, "The Mind-Body Problem in Contemporary Philosophy," in this issue.
9. Morton Beckner, "Reduction, Hierarchies and Organicism," in *Studies in the Philosophy of Biology: Reduction and Related Problems*, ed. Francisco J. Ayala and Theodosius Dobzhansky (London: Macmillan Press, 1974), pp. 163–76.
10. See Francisco J. Ayala, introduction to Ayala and Dobzhansky (n. 9 above), pp. vii–xvi.
11. Theodosius Dobzhansky, "On Cartesian and Darwinian Aspects of Biology" in *Philosophy, Science and Method*, ed. S. Morgenbesser, P. Suppes, and M. White (New York: Macmillan Co., 1969), pp. 165–78.
12. Ayala (n. 10 above), p. viii.
13. Theodosius Dobzhansky, "Introductory Remarks," in Ayala and Dobzhansky (n. 9 above), p. 1.
14. C. N. Hinshelwood, *The Structure of Physical Chemistry* (Oxford: Clarendon Press, 1951), p. 449.
15. Note that the ambiguity of "nothing" has to be guarded against at this point. Sometimes "thing" is used generally (as in "something," "anything," "everything," "nothing") of any possible entity that could be referred to by a referring expression within some context of discourse. But sometimes "thing" means a visible and tangible object.
16. D. M. MacKay, *The Clockwork Image* (London: Inter-Varsity Press, 1974), pp. 42–44.
17. Theodosius Dobzhansky, *The Biology of Ultimate Concern* (New York: New American Library, 1967); Ian Barbour, *Issues in Science and Religion* (London: SCM Press, 1966), esp. pp. 324–37; J. C. Eccles, *Facing Reality* (London: Longman, 1970); Arthur Koestler and J. R. Smythies, eds., *Beyond Reductionism* (London: Hutchinson, 1969); A. R. Peacocke, *Science and the Christian Experiment* (London: Oxford University Press, 1971); Marjorie Grene, ed., *Interpretations of Life and Mind* (London: Routledge & Kegan Paul, 1971); William H. Thorpe, *Animal Nature and Human Nature* (London: Methuen, 1974).
18. Ayala (n. 10 above), p. ix.
19. Ayala and Dobzhansky, (n. 9 above).
20. As quoted by W. H. Thorpe, "Reductionism in Biology," in ibid. p. 111.
21. Ernest Nagel, "Wholes, Sums and Organic Unities," *Philosophical Studies* 3 (1952): 17–32. His treatment may be summarized as follows: The word "whole" is used to refer to something with a spatial extension, to some temporal period, to any class of elements,

to a property of an object or process, to a pattern of relations, to a process, to any concrete object, or to any system those spatial parts stand to one another in various relations of dynamical dependence. Corresponding to each of these meanings, the reference of the word "parts" can be explicated, and this suffices to indicate at once not only the ambiguity of these words themselves but also of the word "sum," which is being attributed, or not attributed, to the relation of the "wholes" to the "parts," and so also the ambiguity of the word "addition," which is the process whereby wholes are putatively derived from parts. "Organic wholes" or "organic unities" are those systems which exhibit a mode of organization that is often claimed to be incapable of analysis in terms of an "additive point of view" (p. 26). Two kinds of this supposed "addition" have furthermore to be distinguished: "The question whether a given system can be *overtly constructed* in a piecemeal fashion by a seriatim juxtaposition of parts, and the question whether the system can be *analyzed in terms of a theory* concerning its assumed constituents and their interrelations. . . . However, this difference between systems does not correspond to the intended distinction between functional and summative wholes; and our inability to construct effectively a system out of its parts, which in some cases may only be a consequence of temporary technological limitations, cannot be taken as evidence for deciding the second of the above two questions" (p. 28).

22. Ibid., p. 29.
23. Ibid., p. 30.
24. Thorpe (n. 20 above), pp. 109–38.
25. J. R. Lucas, Oxford discussions, 1971–72.
26. Nagel (n. 21 above), pp. 24–25.
27. Ibid., pp. 29–30.
28. Peacocke (n. 17 above), chaps. 1–3; Thorpe (n. 20 above), pp. 111 ff.; H. H. Pattee, "The Problem of Biological Hierarchy," in *Towards a Theoretical Biology*, ed. C. H. Waddington (Edinburgh: Edinburgh University Press), 3:117–36.
29. Beckner (n. 9 above).
30. Ibid., pp. 164–65.
31. Ibid., p. 166. Here i enumerates, from 0 to n, a series of hierarchical levels L_i, to each of which is assigned a part P_i; every P_i (except P_n) is part of exactly one part at each level above i and is (except for L_0) exhaustively composed of parts at each level below i. This is Beckner's definition of a perfect hierarchy i. The larger i, the "higher" the level; h (and l) will denote higher (and lower) in this sense.
32. Ernest Nagel, *The Structure of Science* (New York: Harcourt Brace & Co., 1961), chap. 11.
33. Ibid., p. 364.
34. C. G. Hempel, "Reduction: Ontological and Linguistic Facets," in Morgenbesser et al. (n. 11 above), pp. 179–99.
35. Lucas (n. 25 above). The relevant passage in his contribution is as follows: "Only rather restricted theories are complete. As soon as a theory is at all rich, propositions can be expressed in it which are neither necessitated nor disallowed by the axioms (i.e., which neither can be proved from the axioms nor will lead to an inconsistency if added to the axioms). This means that although the theory will be able to make many detailed predictions about the course of events, granted some set of initial conditions, it will not be able to divide all conceivable descriptions of states of affairs into just two classes, those which must occur, granted those initial conditions, and those which cannot, in view of them, possibly occur. There is some range of uncertainty so far as any particular theory is concerned, and therefore some room for further theories and further modes of explanation. If it had been otherwise—if the relevant theories were complete—then the mechanist could have maintained that even if the biologist found biological explanations more explanatory, it could only be a matter of personal taste, for either the biological explanations accounted for exactly the same phenomena as the mechanist's explanations or else they were wrong. But incomplete theories can be compatible without being the same in all their consequences. And so we can accept the possibility of physical and chemical explanations without thereby excluding that of biological explanations that are essentially different. The incompleteness theorems of mathematical

logic give a further bonus. Gödel not only gave a formal proof that any consistent theory rich enough to include elementary number theory must contain a well-formed formula which was consistent with the axioms but not provable from them; he also argued, but this time of necessity informally, that this well-formed formula was in fact true. He could explain why it was true, although *not* within the confines of the formal theory he was considering—if he had been able to explain within elementary number theory why it was true, he would have proved it, and the formula would have been a provable one just like all the theorems. But the Gödelian formula, although it cannot be formally proved within the theory, can nevertheless be informally shown to be true. So that beyond the formal proofs of the theory, there are others which we find cogent, although they cannot be expressed within the formalism of that particular theory. In a similar fashion we may expect that, besides the explanations offered by any one scientific theory, there will be insights from outside that theory, which we find quite convincing and entirely explanatory, and which can explain some things which the original theory could not. Thus not only are explanations of one, in some sense more basic, type compatible with explanations of a more sophisticated type, but they positively require them."

36. Conveniently grouped and summarized into four paradigms by K. F. Schaffner in "Approaches to Reduction," *Philosophy of Science* 34 (1967): 137–47, where he elaborates a general reduction paradigm yielding the earlier ones as special cases.

37. If, like one group of authors (Karl R. Popper, Paul K. Feyerabend, and Thomas R. Kuhn), one considers that a complete reduction of a *Th* to a *Tl* is not likely, then in the development of sciences new theories will tend to be seen as making cataclysmic breaks with the old; whereas, if one thinks reduction is possible, then old theories may be regarded as reduced by the new ones which replace them. (E.g., Schaffner [ibid.]; cf. also the stretching of reduction to deal with cases where there is an abrupt contradiction [and not a deductive relation] between the old and the superseding new theory by relating them both to the observations they both explain [J. G. Kemeny and P. Oppenheim, "On Reduction," *Philosophical Studies* 8 (1956): 6–19].) The latter position represents some widening of the scope of what is meant by reduction, although the central idea is always that of explaining a theory *Th* in terms of a theory *Tl* from a different branch of science, corresponding (usually) to a different level in the hierarchy of systems which the sciences study. E.g., from this more historical perspective, Popper ("Scientific Reduction and the Essential Incompleteness of All Science," in Ayala and Dobzhansky [n. 9 above], pp. 259–84) asserts that, while in all the sciences the attempt to make reductions is justified on methodological grounds and must continue because we learn so much that is fruitful even from unsuccessful attempts and while nothing is as great a success in science as a successful reduction, yet hardly any major reduction in science has ever been completely successful, for there is, he argues, almost always an unresolved residue left by even the most successful attempts at reduction. There is an unresolved residue even in the reduction of chemistry to physics since the heavier elements have an evolutionary history and their coming into existence is rare in the cosmos, so that cosmological considerations enter in addition to those of quantum physics—and a theory of evolution is even more indispensable in biology. The "deductions" involved in reducing one branch of science to another are not, in practice, strict, for they involve all sorts of approximations, simplifications, and idealizations.

38. C. F. A. Pantin, *The Relations between the Sciences* (Cambridge: Cambridge University Press, 1968); P. W. Anderson, "More Is Different: Broken Symmetry and the Nature of the Hierarchical Structure of Science," *Science* 77 (1972): 393–96; Peacocke (n. 17 above), chap. 2; Peter Medawar, "A Geometric Model of Reduction and Emergence," in Ayala and Dobzhansky (n. 9 above), pp. 57–63; and L. L. Whyte, A. G. Wilson, and D. Wilson, eds., *Hierarchical Structures* (New York: Elsevier Publishing Co., 1969).

39. Medawar, p. 61.

40. Peacocke (n. 17 above), pp. 89–90.

41. Using the metaphors of the subsection entitled "Hierarchies of Systems, Theories, and Sciences" rather than those of Medawar himself.

42. Beckner (n. 9 above), p. 170, and Nagel (n. 21 above).

43. Beckner, p. 170.
44. Ibid.
45. The confusions to which Beckner (pp. 168–70) draws attention, partly as a result of current analyses, inter alia, of the description of events, are (1) saying that, because a theory T_0 appropriate to the zeroth level provides some basis ("cash value") for regarding every part P_0 at this level L_0 as of a certain kind K_0, then the set of wholes w contains only parts of kind K_0 (this ignores the distinction between being exhaustively composed of parts K_i and being composed *only* of parts K_i); (2) saying that, because every part P_i at level i in a hierarchy is composed of parts P_{i-1}, of the level below (i-1), any theory T_i must be reducible to a theory T_{i-1}; but alternative descriptions of the same thing, whether or not they belong to theories at different levels, need have no logical connection, and neither description entails the other; (3) the fallacy of concluding from the premise that since a j-level phenomenon is of kind K_j it is not of kind K_i either, where $i < j$; the fact of hierarchical organization is sufficient ground for holding that events at the j-level, in addition to their status as being of kind K_j, are also events of each kind K_i, where $i < j$; (4) if a j-level *theory* is autonomous with respect to i-level *theory* ($i < j$), then there are j-level *processes* (events, effects, phenomena) that are autonomous with respect to i-level *processes.* (See the subsection entitled "Hierarchies of Systems, Theories, and Sciences" above, where this is discussed more fully.)
46. The label "reduction" is applied only to theories, in the sense used by Nagel (n. 32 above), so the opposite of theory autonomy is theory reduction. The word "reduction" is not applied to processes, so the opposite of process autonomy is not usually called "process reduction," but the description is simply negatived.
47. Beckner (n. 9 above), p. 174.
48. Ibid.
49. Ibid.
50. Medawar (n. 38 above) cities "heredity," "infection," "immunity," "sexuality," and "fear" (p. 57), but many others were also pointed out in the conference on problems of reduction in biology by other authors who develop this emphasis in their own particular ways. Ernest Boesiger ("Evolutionary Theories after Lamarck and Darwin," in Ayala and Dobzhansky [n. 9 above]) stresses that the patterns of explanation of the adaptive or "purposeful" character of organisms which invoke use and disuse or natural selection are distinctive of biology and are "organismic" (p. 42). Gerald M. Edelman ("The Problem of Molecular Recognition by a Selective System," in ibid., pp. 45–48) has described how, methodologically, the current selective theory of antibody formation emerged only when attention was focused on two levels in the system, that of the cell and that of antibodies at the molecular level; the latter alone failed as an explanation. G. Ledyard Stebbins ("Adaptive Shifts and Evolutionary Novelty: A Compositionist Approach," in ibid., pp. 285–306) sees the study of organic evolution as polarized around two widely distant forms: evolution in the broad sense ("a succession of events that took place over billions of years of time, and gave rise successively to living matter . . . and finally to man . . ." [p. 285]), which can be studied only by a compositionistic activity drawing on information from a wide variety of sources (systematics, paleontology, population genetics), and evolution at the level of populations, which looks for changes that can be observed by a scientist in a much shorter time through quantitative, experimental methods. Theodosius Dobzhansky ("Chance and Creativity in Evolution," in ibid., pp. 307–38) shows that evolutionary theory requires concepts that play no part outside biology ("Mutation, sexual recombination and natural selection are linked together in a system which makes biological evolution a creative process" [p. 336]). Henryk Skolimowski ("Problems of Rationality in Biology," in ibid., pp. 221–22) emphasizes that evolved man is cognitive, comprehending evolution; and this itself is life enhancing and involved in evolution, so that ideas such as "feedback," "information," "environment," and "past experience" become normative, to be made sense of only in a system that admits values and norms. This necessitates an "evolutionary rationality" introducing "open-ended concepts," "growth concepts," and "normative concepts."
51. J. J. C. Smart, *Philosophy and Scientific Realism* (London: Routledge & Kegan Paul, 1963), chap. 3.

52. Michael Polanyi, *Personal Knowledge* (London: Routledge & Kegan Paul, 1958); *The Study of Man* (London: Routledge & Kegan Paul, 1959); *The Tacit Dimension* (London: Routledge & Kegan Paul, 1967); "Life Transcending Physics and Chemistry," *Chemical and Engineering News* (August 21, 1967), pp. 54–66; "Life's Irreducible Structure," *Science* 160 (1968): 1308–12.

53. Much is sometimes made of the distinction that machines are artifacts and biological organisms are not. Clearly, machines are designed by man to have certain relationships and interactions among their components, so that we can predict how they will behave, within limits determined partly by our lack of knowledge of what we need to know about the components and their properties over a period of time and under stress. The labyrinthine organization of biological organisms is only gradually becoming apparent to us, and we are profoundly ignorant of it at most levels—only a small, though central, part has been unveiled by the molecular biology of the last few decades. We are largely ignorant of the organization of biological systems; we are unable to predict any but the smallest fraction of their behavior; and we have to admit that we are ignorant even of how we ought to think about them, what conceptual tools are needed, at different levels (see below). But this relative difference in our knowledge need not of itself invalidate Polanyi's arguments, even if they are vulnerable on other counts, as discussed in this paper. It is interesting to note that even systems composed of molecules, etc., obeying the essentially deterministic laws of classical physics can be shown to have behavior which is predictable only within limits: E.g., in meteorology, it is becoming clear that "no conceivable improvement of the observing network can increase this period [of weather prediction] to longer than a value lying somewhere between ten days and three weeks. . . . The atmosphere is a physical system which is demonstrably unpredictable in practice beyond a certain time, even though it is composed of a finite collection of objects whose interactions are governed by the laws of physics" (R. S. Harwood, Oxford discussions, 1971–72).

54. K. F. Schaffner, "Anti-Reductionism and Molecular Biology," *Science* 157 (1967): 644–47, and "The Watson-Crick Model and Reductionism," *British Journal for the Philosophy of Science* 20 (1969): 325–48.

55. Schaffner, "Watson-Crick Model," pp. 345–46 (my italics).

56. See n. 21 above.

57. Polanyi, "Life Transcending Physics and Chemistry" and "Life's Irreducible Structure" (n. 52 above).

58. See n. 50 above.

59. Popper (n. 37 above).

60. Ludwig von Bertalanffy, "Chance or Law," in Koestler and Smythies (n. 17 above), pp. 56–76. Other relevant references are to be found in this same article.

61. Ted Bastin, private communication, 1972; see also his "Timeless Order," in *Biogenesis, Evolution and Homeostasis*, ed. A. Locker (New York: Springer-Verlag, 1973), pp. 137–45.

62. D. Berlinski, "Philosophical Aspects of Molecular Biology," *Journal of Philosophy* 69 (1972): 319–35.

63. Bastin, private communication (n. 61 above).

64. See Beckner (n. 9 above), p. 166.

65. See n. 45 above.

66. It seems to be the case that the cosmos has in fact evidenced a succession in time wherein more and more complex arrangements of matter have appeared with these distinctive qualities, properties, etc., at least in the terrestrial corner of the universe that we can observe in detail. In this sense, the cosmic process displays emergent qualities the significance of which, I think, is pivotal for its interpretation but which it is not appropriate for me to develop here (see my *Science and the Christian Experiment* [n. 17 above], chap. 3, esp. pp. 102–8). "Emergence" has been used in this paper with reference only to the relation of higher-level to lower-level phenomena.

67. Tim Appleton, "Consciousness in Animals," in this issue.

68. Robinson (n. 8 above).