DO LIFE PROCESSES TRANSCEND PHYSICS AND CHEMISTRY?

One of the general symposia developed for the annual meeting of the American Association for the Advancement of Science held on December 30, 1967, in the Caspary Auditorium of Rockefeller University, in New York City.

In the attempt to relate religion to the sciences, philosophers and theologians and other scholars are apt to be bothered by what is called "reductionism," a common form of which is the supposition that explanations of phenomena of practically any kind—including human social, psychological, and even (according to some) religious phenomena—can be reduced to an explanation in terms of physics. When distinguished scientists challenge this reductionistic view, it has an interest even greater than when it is challenged by humanists. While the editor of *Zygon* is not disturbed by reductionism, many others are; hence the importance of publishing this symposium, which may not be the last word, but which is nevertheless on the new frontier of the age-old debate. Michael Polanyi gave an early version of his now famous answer to this question at the Center for Advanced Study in Theology and the Sciences on May 31, 1967. A more detailed version will be found published in *Science,* 160:1309–12 (June 21, 1968). But the version printed here and the significantly revealing commentary by other distinguished scientists and philosophers of science may have a special value, not only for those concerned with relating religion and the sciences, but for all who are concerned to understand the nature of scientific explanation in general and its explanation of life in particular. The text printed here is a modestly edited transcription of the audio tape recording of the informal panel discussion, and the reader may feel that he is attending an exploratory conversation of experts contemplating frontier questions about the nature of scientific explanation and its relevance for questions of life and religion. —EDITOR

Panel: *Gerald Holton,* Chairman; *Michael Polanyi, Ernest Nagel, John R. Platt, and Barry Commoner*

GERALD HOLTON

Ladies and gentlemen: I have been asked by the program committee

Gerald Holton, who was chairman of the discussion, is professor of physics at Harvard University, and has distinguished himself also as an educator and as a philosopher and historian of science. Among his other outstanding avocational contributions is the fact that during his term as editor of the American Academy of Arts and Sciences (1957–62) he transformed its *Proceedings* into the quarterly journal *Daedalus* which speaks so eloquently as a voice of the academic world on wide-ranging human problems, with a circulation of over 60,000. He identifies other panelists seriatim in the text.

of the American Association for the Advancement of Science to chair this session, and I do so gladly because I expect it will be both educational and enjoyable. The question before us is as old as science. But so are many other lively topics of the day. The quest of Thales for primordial materials is still being carried on in the most modern laboratories in the guise of the search for the elementary particle that will explain all matter. In the George Sarton Memorial Lecture, given two or three days ago at this conference, Dr. Cyril Smith, of M.I.T., gave a very challenging talk on "The revival of qualities, corpuscles, and phlogiston in the modern science of materials." The point I wish to hint at is that underneath some of today's more challenging laboratory work there are long-range and perhaps unresolvable thematic preoccupations.

Here is where the physical sciences on the one hand and philosophy on the other are so different. In the physical sciences, you know that within five years, on the average, the specific problem will somehow be answered. In philosophy, on the other hand, you know that you have a good question if it is worth talking about forever. And the persistence of larger questions in science below the flood of quickly solved special problems shows that science has a philosophical basis.

Today we are turning again to an ancient and perhaps unanswerable large question. I shall try to get out of the way of the panel as quickly as I can, and confine my function to that of an environmental engineer, turning the thermostat up or down as the need arises, and to introduce the members of the panel.

Dr. Michael Polanyi has had a long and distinguished career in both physical and social science. He was born in Budapest in 1891 and graduated from the University of Budapest in 1913 as Doctor of Medicine. From there he went to study chemistry at the Technische Hochschule in Baden, Germany, became an army medical officer in 1914, was struck down with diphtheria shortly after joining, and, while convalescing, wrote a thesis in physical chemistry for his Ph.D. at the University of Budapest.

Dr. Polanyi taught at the university there in 1919, and in 1920 he began teaching at the Kaiser Wilhelm Institut in Berlin. In 1929 he was appointed a life member of the Institut, but resigned four years later and accepted the chair of physical chemistry at Victoria University at Manchester, England. In 1948 he exchanged his chair in physical chemistry for a chair in the social sciences. Today we are reminded of that remarkable symbolic occasion by seeing him bring to bear his competence in both fields on one topic. He began a series of visiting professorships and fellowships in 1950 and has taught at various universities, including California, Chicago, Duke, Wesleyan, and Yale, and he has been awarded many honorary degrees.

MICHAEL POLANYI

Ladies and gentlemen, I shall talk for quite a while about a subject which might seem far-fetched, namely, machines. But you will see shortly that this leads up to our main question.

Let me introduce the subject by suggesting that if all men were exterminated, which is not so difficult to imagine today as it used to be, the laws of inanimate nature would not be affected, but the production of machines would stop. Not until men rose again could machines be formed once more. Some animals can produce tools, but only men can construct machines. Machines are human artifacts consisting of inanimate material.

Now, the Oxford dictionary which one usually invokes at this point describes a machine as an apparatus for applying mechanical power, consisting of a number of interrelated parts, each with a definite function. It might be, for example, a machine for sewing or painting. Let us assume that the power is built into the machine and disregard the fact that we have to supply it with power from time to time. We can then say that the manufacture of machines consists in cutting suitably shaped parts and fitting them together, so that their joint mechanical action will serve a human purpose. The structure and working of machines are thus shaped by men, even while their material and the forces that operate in them obey the laws of inanimate nature.

In constructing a machine and supplying it with power, we harness as it were the laws of nature at work in its material and in its driving force and make them serve our purpose. But this harness is not unbreakable. The structure of the machine and its working can break down. Nothing is more well-known. But this will not affect in the least the forces of inanimate nature on which the operation of the machine relies. It merely releases these forces from the restrictions the machine imposed on them before it broke down. So the machine as a whole works under the control of two distinct principles. The higher principle is that of the machine's design. This higher principle harnesses the lower one, which consists in the physicochemical processes on which the machine relies for its working.

We commonly form such a two-leveled structure, as I shall call it, in conducting an experiment. But there's a difference between constructing a machine and rigging up an experiment. The experimenter imposes restrictions on nature in order to observe its behavior under these restrictions, while the construction of a machine restricts nature in order to harness its working. We may borrow a term from physics and describe both these useful restrictions of nature as the imposing of boundary conditions on the laws of physics and chemistry.

Let me enlarge on this. I have exemplified two types of boundaries. In the machine we are interested in the effects of the boundary conditions, while in the experimental setting we are interested in the natural processes controlled by the boundaries. Both types of interest are common. When a saucepan bounds a soup that we are cooking, we are interested in the soup, not in the saucepan; likewise, when we observe a reaction in a test tube, we are studying the reaction, and not the test tube. The reverse is true, for example, for a game of chess: The strategy of the player imposes boundaries on the several moves which follow the laws of chess, but our interest lies in the boundaries, that is, in the strategy, not in the several moves. Similarly, when a sculptor shapes a stone, or a painter composes a painting, our interest lies in the boundaries imposed on the material, not in the material itself. I would distinguish these two types of boundaries by saying that the first represents the test-tube type of boundary, while the second represents the machine type. We'll see that this is useful.

All communications have machine types of boundaries, and these boundaries form a whole hierarchy of consecutive levels. A vocabulary sets boundary conditions to the utterance of the voice, a grammar harnesses words to form sentences, and the sentences are shaped into a text which conveys a communication. These are the consecutive levels. At each level we are interested in the boundaries imposed rather than in the principles harnessed. Communications will prove of particular interest to our main problem, to which I'll now return.

From machines, we pass to living beings. We arrive there by remembering that animals move about mechanically and that they have internal organs which perform functions as the parts of a machine do, functions which sustain the life of the organism in the way that machines serve the interests of their users. For centuries past, the workings of life have been likened to the workings of machines, and physiologists have been seeking to interpret the organism as a complex network of mechanisms. Any single part of the organism is puzzling to physiology and meaningless to pathology until the way it benefits the organism is discovered. We may add that any description of such a system in terms of its physicochemical topography would be quite meaningless but for the fact that the description covertly recalls the system's physiological interpretation. Similarly, the topography of a machine is meaningless until we guess how it works and for what purpose.

In this light, the organism is shown to be, like a machine, a system under dual control. Its structure serves as a boundary condition, harnessing the physicochemical processes by which its organs perform their functions. Thus, morphogenesis, the process by which the structure of living beings develops, can be likened to the shaping of a machine which will act as a boundary for the laws of inanimate nature. As these laws serve the machine, so they also serve the developed organism.

Let me emphasize here the fact that the boundary condition is always extraneous to the process which it delimits. In Galileo's experiments of balls rolling down a slope, the angle of the slope was not derived from the laws of mechanics but was chosen by Galileo. This choice of slope was extraneous to the laws of mechanics, as the shape and manufacture of test tubes is extraneous to the laws of chemistry. The same holds for machine-like boundaries. Their structure cannot be derived from the forces which they harness. Nor can a vocabulary determine the content of a text. And so on. Therefore, if the structure of living things is a set of boundary conditions, this structure is extraneous to the laws of physics and chemistry governing the forces which the organism is harnessing. Under this supposition, the morphology of living things transcends the laws of physics and chemistry.

But before being satisfied with this argument, we should admit that the analogy between machines and functioning organs is weakened by the fact that the organs are not shaped artificially as the parts of a machine are. It is an advantage, therefore, to find that the morphogenetic process is explained in principle by the genetic transmission of information stored in a chemical compound, the famous DNA interpreted in this sense by Watson and Crick.

The informations stored in DNA, which control morphogenesis, can be shown to be boundary conditions like those imposed on a material by shaping it into a machine. That's why I talked so much about machines. For, just as the information contained in a printed page is conveyed in a distinctive arrangement of letters which is not due to any physical interaction between the letters, so the information content of a DNA molecule inheres in an ordering of its constituents which is not due to any physical interaction between them. It is a boundary condition, and as such, it is extraneous to the chemical forces composing the molecule, just as if their pattern were artificial, as that of a machine is.

Its ordering has, in fact, a negentropy, which will give some additional precision to what I have just submitted to you. This negentropy is due to the fact that the chemical forces in the molecule permit any alternative isomeric sequence to be formed with an equal or virtually equal probability. This means that the sequence of substituents which bears the molecule's information is virtually extraneous to the molecule's chemical forces. In other words, the information-bearing function of a sequence of substituents is a boundary condition harnessing the molecule's chemical forces, and as such, it is extraneous to the laws of chemistry. Analogously, the arrangement of printed letters is extraneous to the chemistry of the paper and ink forming the printed page.

According to the theory of Watson and Crick, the negentropy—the negative entropy—content of DNA is transmitted to the offspring in the negentropy of its bodily structure. It follows then, by the same analysis that I have applied to the DNA molecule, to the machine, and to a number of other cases, that the distinctive improbability, or negentropy, of a living structure is extraneous to the laws of inanimate matter at work in the organism.

The structure of living things is a boundary condition which harnesses the physicochemical forces of the organism and as such, the structure of the organism transcends the laws of physics and chemistry. This is what I meant by writing a short time ago a paper entitled "Life Transcends Physics and Chemistry." But that formulation is too loose, and I want to amend it in the sense I have just suggested. Thank you.

CHAIRMAN HOLTON

Thank you, Professor Polanyi. Our next presentation is that by Professor Ernest Nagel, who is University Professor at Columbia University. Professor Nagel was born in Czechoslovakia and obtained his secondary education in New York City, where he also obtained his B.A. degree at C.C.N.Y. and his Ph.D. degree at Columbia University.

He has taught at C.C.N.Y. and Rockefeller University and has been a visiting professor at many other institutions.

He has been the editor at various times of the *Journal of Philosophy*, the *Journal of Symbolic Logic*, and the *Philosophy of Science*. He is the former president of the American Philosophical Association, the Symbolic Logic Association, and the Philosophy of Science Association. In fact, to many of us, he is both the younger and the elder statesman of the philosophy of science. Among his publications are the books *Logic of Measurement*, *Introduction to Logic and Scientific Method* with Morris Cohen, *Sovereign Reason*, *Logic without Metaphysics*, and the handbook in the field, *The Structure of Science*. Professor Nagel.

Ernest Nagel

Thank you, Professor Holton. The question that I'd like to discuss this afternoon is not, as stated, entirely determinate. The question, "Do life processes transcend physics and chemistry?" could be construed in a number of different ways. I propose to interpret it in the way in which Professor Polanyi himself interpreted it in the paper which is in a way the basis for our discussion. Can the science of biology explain life in our age by the workings of physical and chemical laws? Now my own position on this question is, I suppose, a bit ambiguous, and I would like to indicate just why.

But before I do so, it seems to me that a few preliminary words are in order, to explain how I think a discussion of this sort might be most profitably conducted. When someone claims, as I think Professor Polanyi and perhaps some of my other colleagues on this panel claim, that life or the behavior of living things cannot be explained by or reduced to physicochemical laws or theories, one would like to know in what sense the "can" or "cannot" should be construed.

Presumably—and I hope it is a fair presumption—the question is not whether we do have the intellectual capacity to give such explanations. For this is a question that I think is essentially empirical, in the sense that only an appeal to the facts, many of which are not available to us, could decide this. Certainly one would have to agree that there is a tremendous amount that we don't presently know about biological process in terms of physicochemical laws. We cannot decide whether human beings have the capacity to explain the behavior of living things. Now the question of whether, if we do have the capacity, we will some day succeed in offering such explanations is, in a very identifiable sense, an empirical question. It is a question of whether, at some time in the future, we might succeed in doing something which, because of a lack of knowledge or lack of effort, we have not yet succeeded in doing.

I take it that the question which is really before us is one which suggests that the "cannot," in the claim that biological laws cannot be explained, is a statement of an impossibility. This is not a sheer physical impossibility but a logical impossibility. It thus involves, as all logical questions do, an analysis of meanings or ideas. I think Professor Polanyi, in his preliminary remarks has indicated that in his view the reduction of biology to physics is a logical impossibility.

Now, what is required to establish a logical impossibility? There are many instances of such impossibilities in the history of thought. For example, it is logically impossible to trisect an angle using only a compass and a straightedge. It is logically impossible to derive the Euclidian parallel postulate from the remaining assumptions of Euclid. Or, to take a perhaps trivial illustration, it is logically impossible to determine the longitude and latitude of a ship from knowing the age of the captain.

But clearly, if the question is to be settled or to be discussed in a profitable manner, one would have to put one's cards on the table and indicate what assumptions are being made about the character of physics or chemistry. This would lead one to say either that reduction is impossible or that reduction is possible, in the sense of a logical "can" or "cannot."

Let me review very briefly the conditions for giving an explanation. I'm oversimplifying the whole subject, which is a very complicated one, but at least the following requirement seems to me fairly well recognized. One must assume some set of laws or some theory about a domain of inquiry. For example, to explain planetary behavior, one can use Kepler's laws. One can also explain that behavior in terms of Newtonian theory; and similarly for other domains.

But laws and theories by themselves are not sufficient; one must have an additional statement of initial and boundary conditions. Professor Polanyi has made it quite clear, although for reasons which I'm not sure that I fully grasp, he makes much of the point that the initial and boundary conditions are never derivable from the theoretical or lawlike assumptions that are being made. But in any case, if the explanatory premises for some phenomenon contain only laws or theories but no boundary conditions, then it's clear that such premises do not succeed in explaining either biological phenomena in terms of physical and chemical laws, or even purely physical phenomena in terms of laws and theories of the physical sciences themselves.

The third requirement is that the facts to be explained must be carefully formulated in statements. One has an explanation when such statements are logically derivable from the theoretical or lawlike assumptions that one makes, supplemented by the various boundary conditions that one introduces.

And finally, as a kind of special case, in those domains where the facts to be explained involve the use of notions that do not appear in the theory to which the reduction is to be effected, then those notions must in some sense be "defined" in terms of the basic ideas of the theory to which the reduction is to be made.

With these preliminary remarks out of the way about what I think are desirable ground rules for carrying on the discussion, let me state very briefly and dogmatically my own views on the particular issue before us. I believe all of us agree that the development of modern science has been in good part the reduction of laws with an initially limited scope to much more comprehensive principles. Moreover, the original domain of application of these principles has been frequently quite different from the domains to which the laws explained by those principles are applicable. Classical illustrations of this include the reduction of physical optics to Maxwell's electromagnetic theory and the reduction of the laws of heat or thermodynamics to the kinetic theory of matter. More recently, many if not all the laws of chemistry have been reduced to, or explained in terms of, quantum theory.

Now, in all these illustrations, I've tried very carefully to indicate what the theory is to which the reduction has been made. No one today would claim that one could explain the laws of chemistry through Newtonian theory. This would be patently a mistake, an incorrect claim. The burden of my own position is this: If you ask, "Does life transcend physics and chemistry?" or "Can you explain the laws of living organisms in terms of physics and chemistry?" you must state just what theory of physics and chemistry you have in mind when you seek to offer an explanation.

It may very well be that we have not yet explained, in terms of current physical theories, the totality of biological phenomena. Such a total explanation may now be impossible, but this would certainly not settle the question of whether, using an amended theory of physics or chemistry, such reductions might be made. And so my plea is that the question has to be considered relative to a particular theory of physics and chemistry. The all-out attempt to establish either the possibility or the impossibility of explaining biological laws in terms of physics or chemistry is not really a well-defined or profitable one. On the other hand, I do not believe that any a priori proof can be given to the effect that a reduction of biological laws to the theory of some domain of physical inquiry can be given. This issue seems to me to be an empirical one, to be settled, if at all—and perhaps never completely—by the progress of inquiry. Since I believe that no one's crystal ball is so clear that he can read the future, this is likely to remain an unresolved problem for some time.

Let me make two concluding remarks. The problem we are discussing is in good measure generated by the fact that living organisms possess a hierarchical organization, and that laws found to hold on different levels are not in general the same. The issue, as I understand it, is whether laws which are operative at one level of organization can be explained, in the sense which I suggested, in terms of laws which operate on the most elementary level, namely, those which involve the behavior of elementary physical particles. However, contrary to what is often maintained, the mere fact that there are various levels of biological organization, that one can distinguish between lower and higher levels, does not seem to me to preclude the possibility of explaining the laws at a higher level by laws at a lower one. In particular, the question is not settled in the negative by the fact that terms occur in laws at a higher level which do not occur in laws at a lower one. For it may be possible to designate the sufficient conditions for the occurrence of properties on higher levels of organization in terms of properties designated by predicates employed in laws on lower levels.

The final point I want to make is to emphasize the importance of keeping apart two questions which are sometimes confounded. One question is whether we can give a physicochemical explanation of the laws concerning living organisms as these organisms are constituted at a given time (e.g., at present). The other and quite different question is whether the laws concerning the historical or evolutionary development of living systems can be explained in physicochemical terms. It's certainly conceivable that an affirmative answer is the correct one to the first question, but not to the second. But it is easy to conclude, though mistakenly, that biological laws cannot be explained in terms of physics and chemistry, if one fails to distinguish the question whether the laws of biological evolution are reducible to physicochemical ones from the question whether the laws formulating the behavior of organisms at a particular stage of historical development can be understood in physicochemical terms.

As you see, my own position is undogmatic. On the one hand, I think that a good many biological laws can be explained in terms of present-day physicochemical theories. Others are at present not so explicable, and no one can be sure whether they will eventually be so explained. One can make guesses as to what the future will bring, but the supposition that one can establish conclusively the impossibility or the inevitability of reducing biology to physico-chemistry seems to me entirely mistaken.

CHAIRMAN HOLTON

Thank you, Professor Nagel. The next presentation is by Professor John R. Platt. He was in the field of physics and spectroscopy for many years at the University of Chicago, and is now Research Biophysicist and Associate Director of the Mental Health Research Institute of the University of Michigan. His current work in biophysics is on color and perception. He is also a general educator, the author of a large number of essays, some of which have been collected in books on scientific creation and its social aspects. His books, such as *The Excitement of Science,* 1962; *New Views of the Nature of Man,* 1965; and *The Step to Man,* 1966, have had a very wide readership. Professor Platt.

JOHN PLATT

What I want to talk about is not exactly, "Does life *transcend* physics and chemistry?" I'm afraid of these Latin words. I suspect that there's no one in this audience who has used the word "transcend" in these scientific meetings except in discussing the subject of this conference. It's a word reserved for preachers and philosophers; and the nice thing about the effect of such a word in stimulating philosophical discussion is that each philosopher can misunderstand it in his own way.

What I would therefore like to discuss instead, phrased in a more familiar way, is "Does life *go beyond* physics and chemistry?" I won't keep you in suspense. My answer is "yes." Now you can all relax while I explain my reasons for this answer. My reasons fall into three categories.

The first category is that of ordinary analysis from the "objective" point of view. What I mean by objective is the kind of experiences that all of us have in looking at the objects of scientific study, for example in looking at or studying this atom, this crystal, this dog, this flower, or this subject in a psychological experiment. Even within this quite objective and scientific-positivistic frame of reference, we see in many cases that there are "emergent" properties which appear with the increase in size or complexity of an organism or of an object. These emergent properties are what some people call "systems properties."

In physics one of the most beautiful examples of an emergent property is the example of gravity. Gravity is essentially not predictable from atomic and nuclear physics. It is a tiny, tiny correction to all the atomic equations because the size of the gravitational force is about 10^{-38} times the size of the electromagnetic forces in the atom. The result is that if our physicists had always been operating in a space ship, doing their atomic experiments, and they had never had a big planet around to pull apples down around their heads, they might have gone on for centuries without being able to predict gravity! Or perhaps, if they had found this tiny 10^{-38} correction somewhere in their experiments, they might have easily supposed that there could be positive and negative gravity as there is positive and negative electricity. And it would have taken them quite a lot of experimentation to establish that this was false and straighten out the theory. In short, we see here a case where the minor corrections at one level of size or organization become the major phenomena at another level. The minor correction at the atomic level becomes major at the planetary level because the strong atomic forces all cancel each other out; gravity, however, is cumulative, and so it becomes the big and important thing at the planetary level.

The same sort of thing happens with any kind of increased complexity as well as with increased size. In general, a system is not easily predictable from the properties of its subsystems. It is true that we always try to understand a complex system in terms of its subsystems. This is rational, this is what we mean by "explanation," in our generation, as Professor Holton has emphasized, so we should always adopt this reductionist program. It's good tactics in science, once you see a complex thing, to try to take it apart, to try to reduce it, to try to understand it one bit at a time, and then to put it together and see what other properties it has that were not contained in the subsystems. And so, for example, I would say that a traffic jam has systems properties which the individual cars do not have. The traffic jam is the result of the interaction of individual cars.

Or, to give an example which is a favorite of Donald MacKay, the English physicist and philosopher: MacKay emphasizes that one can understand the principles of a neon-gas-discharge tube, one can understand exactly how the filaments work, how the current goes through, how the atoms are broken down into electrons and positive ions going in opposite directions and maintaining a beautiful blue or red discharge tube. And one can understand all of this from physics and chemistry terms without realizing that what it says is "Joe's Bar and Grill." There's no contradiction between "Joe's Bar and Grill" and the physical chemistry of the discharge tube, but the point is that "Joe's Bar and Grill" is a systems property of the whole arrangement and that it relates to us, to our social organization, and to our social design of this machine, as Dr. Polanyi might put it.

To give you another example, in biology one sees a great growth of complexity in comparison with physics and chemistry. Physics and chemistry have traditionally been concerned with the behavior of atoms or particles such as planets. Now, it's true that a planet in our opinion is not just a "point particle," but that is the way its motion in the skies was interpreted in the building up of astronomy. It was a "particle" which had just two or three coordinates, and we, with our enormous brains, with a hundred million cells in our eyes, could easily look at the properties of these two or three coordinates of a planet.

If you ask a physicist what is the most complicated array of experiments he can imagine doing on a crystal or what is the most complicated set of properties he thinks are important in the physics of a crystal, sometimes you'll get an answer like ten experiments, or thirty properties, or perhaps thirty experiments, or a hundred properties, or some such number. But, while physics operates with properties and experiments which might be of the order of less than 10^3—that is, one thousand—chemistry goes beyond this to consider things like the structures of enzymes, the structures of large, conjugated chain molecules, the directing effects of one reaction on another. And the existence of the 10^6, a million or so, synthetic molecules of chemistry would suggest that perhaps our chemical questions might be numbered in something like the order of 10^7 or ten million.

By comparison with these questions, biology is fantastically complicated. The number of bases in the DNA chain that determines your heredity, the number of bases that exist in every cell of your body and specify the shape of your nose, the color blindness of your eyes, perhaps the age at which you will get gray hair—this number of bases is of the order of 10^9, or something like six times 10^9 (that is, one to six billion). Any one of those bases could be missing or changed, and it could be a lethal change, so we need each of those 10^9 bits of information to specify our individual genetic make-up.

In the case of the human brain, we get up to something like 10^{11} neurons and something like 10^{14} (a hundred trillion) synapses. These enormous numbers in biology might be rather redundant, but nevertheless you see that compared with physics and chemistry we're in a fantastically complicated world. It would be very surprising if the properties of this world did not show new emergent aspects which the study of the individual components or subsystems could not show.

To give a specific example, I believe that one of these days, we will understand the biochemistry of neurons, possibly the biochemistry of memory and learning. I believe that one day we may understand the synaptic connections between neurons. But when we do, when we have understood the whole of the neural organization, I think it will still not be possible to tell by looking at my neurons whether I really love to chase girls with pretty red dresses. That enthusiasm is a systems property of the whole system—and, in fact, is an evolutionary property which I am glad to say that I possess, thanks to a long line of ancestors who liked the same thing.

The second category or domain which I want to emphasize, in considering how life may go beyond physics and chemistry, *is that of experimental and logical unpredictability.*

The first aspect of this unpredictability is what I would call "complexity unpredictability" or the "complexity indeterminacy" of the human brain. Each of our brains contains 10^{11} neurons with 10^{14} synaptic junctions between these neurons; but if you want to look at my brain and find out how it works, and whether it works in a deterministic way or not, you have only 10^8 cells in your eye. How can 10^8 cells detect the state of 10^{11} neurons or 10^{14} synapses?

It is, I would say, logically impossible, and certainly operationally impossible, for one of us to determine the precise initial state of the brain of another one. And therefore, even though we might be sure that each corner of this brain works in a deterministic way, we cannot "prepare the initial state of this whole system" or measure the "final state of this whole system," so we cannot determine whether the whole brain works in a deterministic way. This is a kind of indeterminacy that I call complexity indeterminacy. It is very different from any of the problems of physics and chemistry, which deal with particles and molecules whose numbers are small in comparison with the numbers involved in describing our perceptual apparatus. In considering the human brain, we are studying something which is the same size as our perceptual apparatus; in fact, it's bigger, because our perceptual apparatus is only a subsection of the brain as a whole. This is a kind of problem which has never occurred in objective science before.

The second aspect of unpredictability is the logical invalidity of self-prediction; this also has been emphasized by Dr. McKay. If I say to the baseball which is travelling through space, "You will drop by two feet after another twenty feet," the baseball still drops by two feet after another twenty feet. But if I say to you, "You will drop the spoon after two seconds," it makes a great difference whether you have heard me say this or not. The behavior is no longer independent of whether or not the prediction is made. The communication of a prediction from one person to another, about the second person's behavior, has spoiled the value of the prediction. The second person now knows about it, and may follow it or not, as the case may be, just to spoil or to confirm it. It is no longer prediction in the scientific sense. And so scientific predictability of my actions or yours comes into a logical sphere which is outside the sphere of physics and chemistry: Such actions are not external objects which we study in a detached way.

The third category of life beyond physics involves the matter of subjectivity. Here it is helpful to draw a diagram of the organism and of the external world. My own scientific area of study concerns the problems of perception and the problems of how a system, such as a complex organism, can know its relationship to its environment. This is the "biophysics of perception," if you want to call it that. One of our present models for a system such as the human brain is a "sensory-motor decision system." What this means is that signals from the environment come in to the organism. In the organism, they are sensed, then they are converted by a complex network into motor outputs which go back into the environment and change the environment in such a way as to change the inputs again. This process is called reafferent stimulation. It has now been shown by Von Holst, by Held, by Gibson, and by numerous others, that we cannot perceive anything without manipulating it. Our perception stops whenever an image is "stabilized" on our retina, for example. The result is that perception involves action, and action is perception, in a sense. That is to say, there is no action without awareness and there is no awareness without action, in a sensory-motor decision system which is operating in a cybernetic way around a feedback loop into the environment.

Under these circumstances, you can see that the world is divided into two parts, but that the two parts are inseparable. It is the external part of the world, the external half-world, which is the world of physics. This is the world where we, say, take the spoon and then drop it, letting go so we can see how it falls. The internal part of the world is, instead, the world of cybernetics. It is the world of *choosing* to take the spoon, choosing to drop it. I don't know why you'd want to drop a spoon, but if you did want to drop it, you'd do it for reasons of your own which would have to do with the world of choosing.

The result is that the world of physics and chemistry is only half a world. It's the world "out there." It is the world without values, without love, without death, without vomiting. It's the world without cybernetic choices, which are involved in the human manipulation.

So it is being found out today, that even in an abstract field like mathematics, *we* are the ones who choose the problems to be solved and who decide whether the answer is a proof for us or is not. A computing machine cannot choose the mathematical problems, and it cannot make proofs unless *we* are in fact convinced by its proofs. This is easy to see because, as everyone knows, it has often happened that one mathematician was not convinced by the proofs that convinced a hundred colleagues, and he turned out in the next century to be the one who was right. He had a feeling that some premise was missing. This same thing happens with a computing machine demonstrating proofs: If I'm not convinced by it, it is not a proof for me. So, subjectively, *we* are the ones who set the problems, we are the ones who do the science, we are the ones who interpret it and choose how to use it for our own human purposes.

In this subjective aspect of science—I would like to make the illustration personal for Dr. Nagel, not to single him out but simply to make it graphic—I would say that life transcends physics and chemistry because Professor Nagel transcends anything he can say with his tongue or think with his brain about philosophy or physics.

CHAIRMAN HOLTON

The next speaker is Barry Commoner, who was born in New York in 1917, got an A.B. at Columbia University in 1937, and a Ph.D. at Harvard in 1941. He also has picked up various hardware of the honorary kind: LL.D.'s, D.Sc.'s, and so forth. He is chairman of the Department of Botany and director of the Center for the Biology of Natural Systems at Washington University in St. Louis.

His research is in the chemistry of living cells, the role of free radicals in biological systems, and the chemical basis for replication. But he is also very well known to many of us because of his continued concern about the social aspects of science. He won the Newcomb Cleveland Prize of the A.A.A.S. and is most recently the author of *Science and Survival,* a book published in 1966. Professor Commoner.

BARRY COMMONER

I think that, in a symposium of this kind, it's a good idea for the speaker to announce in advance what his subject is, because it obviously can vary. Now, fortunately, I don't need to talk about philosophy because we have had a large and detailed presentation of most of the philosophical questions that are involved. I'll simply say that I am going to take the position that life does go beyond physics and chemistry, and the philosophical basis for this position will be all of those things said by the preceding speakers that seem to fit in with that idea.

Now that I've gotten that out of the way (I'd like to talk philosophy, but I've been given so little time), I want to use some data. The point that I want to make is that I think most of you must be troubled. You must be troubled by the fact that at least two and a half speakers have come out against the notion that one could create life chemically, and yet we all know from reading the *New York Times* and other journals that life has been created chemically from things taken off the shelf. (It didn't say which shelf.) You must wonder what Kornberg would say if he were here. What would Lederberg say? What would Crick say? Well, I'm going to tell you what they would say. And the reason that I'm going to do so is that the work that they have done, in my view, establishes clearly on empirical grounds that life transcends chemistry. I want to try to prove that, using the data of molecular genetics.

I'm going to seize on a particular aspect of biology which I think is fundamental, the property of life which involves inheritance, self-duplication, replication. It is, of course, the fundamental property of all living things.

I think that in many ways what has been going on in biology for several years is a continuous narrowing down of the obvious complexity of living things and living systems in a search for an explanation on a simpler level. We see a vast array of organisms in nature, and we look for some classification scheme that will explain how they got there. We see a complex structure in a single organism and we look for the origin of the separate cells in order to explain the organism in simpler terms. We see a vast array of chemical properties in a single cell, and we look for a molecular explanation of that.

One box after another in this set of Chinese boxes has been opened. Each time, the people who have looked for a simple explanation have been disappointed in one sense, because inside each box was another complex question. You open up the tissue box and you have the cell; you open up the cell box and you have the vast array of molecular reactions. And now what's happened is that the last box has been opened. Hopefully, it contains a simple answer to all of biology. And that's what I want to talk about.

Now let me speak of the source of the marvelous capability of living systems to produce more of themselves and to produce this entire nest of Chinese boxes. This is what we mean by self-duplication, and I assert that it occurs nowhere in non-living systems—that it's a unique property of life. For this reason at least, we should define it in terms of where it exists.

So I'll speak of self-duplication first as something which is defined by the way in which living things do it. Although self-duplication can occur only in a suitable system—for example, with the necessary nutrients around, and so on—the inheritable characteristics of the progeny are generally not influenced by the environment. The specificity, the biological uniqueness of an organism, is derived only from the specificity of its parents. This is what we mean by self-duplication. It means that an acorn absorbing only air and water from its surroundings gives rise to an oak and that the oak produces another acorn. And that my children, who were raised—it seemed at times—on peanut butter and hamburgers, turned out to be Commoners.

There's a considerable body of evidence at this time which shows that this unique property involves certain features which can be explained in chemical terms. For example, the color of a rose is inherited, and we can express that feature of the rose in chemical terms by describing the pigment which has the red properties. Moreover, we can describe the origin of that pigment by showing that there are certain other chemical constituents—enzymes—which catalyze changes that lead to the synthesis of the pigment.

So, in many features of life, the uniqueness can be reduced to a chemical datum: the synthesis of a particular substance. There are some features that can't be handled that way, but I won't talk about them. I think it would be unfair if I told you I was going to present the view of Crick, Kornberg, *et al.*, and then gave them an insuperable difficulty, like explaining how the DNA code determines the pattern of fingerprints on your hand, which is, after all, inherited. We know no chemical explanation for that.

But leaving that aside, taking the fact that there are certain genetic features which are chemically determined, we can now translate a biological property—self-duplication—into chemical terms. This is, after all, as my colleagues have pointed out, the essence of the question, "Can we reduce biology to chemistry?" You all know, I'm sure, that there have been various ways of describing the way in which this translation occurs. Let me speak, then, of the way in which one achieves the biochemical specificity—the presence or absence of a pigment, let's say—from a series of chemical events.

Several types of molecules are involved. We have, for example, clear evidence that nucleic acids contain a long sequence of individual nucleotides. There is pretty good evidence in most cases that the sequence is specific and that, in proteins, there are certain sequences of amino acids. We have a pretty good idea that this specificity is something which is imprinted during the biological processes involved in cell development and replication.

Now the theory that I want to talk about asserts that the biochemical features that are inherited are derived from the properties of proteins; that these in turn are derived from the sequence of amino acids in a given protein; and that this sequence is derived from the sequence of nucleotides in a DNA molecule. There are well-known chemical mechanisms for this transmission. If what I have just said is true, then I would have to agree that this aspect of life, a very fundamental one, is explicable in terms of physics and chemistry.

Here I disagree with a statement made by Professor Polanyi. I think that the molecular biologists have given us a sound molecular explanation of how the nucleotide sequence could arise in a DNA molecule from forces derived, not from that molecule, but from another one. Therefore, I don't agree with one of the reasons that he gave for supporting the position that we hold in common.

One important feature of this scheme is Crick's proposal, which is usually called the "central dogma." (The term was supposed to be a joke, I am told, but it's a joke which has found its way into indexes.

I know one monograph that has in its index "Dogma, the," and then a series of eleven references.)

Most people think that the central dogma is what I have just described, namely, that DNA determines RNA, RNA determines protein, protein determines inheritance. These relationships are only part of the "central dogma," for Crick goes on to say that not only does DNA determine the specificity of RNA, and RNA determine the specificity of protein, but the reverse is forbidden: that is, the specificity of protein cannot determine the specificity of DNA.

It is now apparent that the central dogma is wrong. The evidence for this is as follows: DNA is synthesized by a protein–DNA polymerase. In a series of experiments in the last few years on bacteria which contain a mutant polymerase, it was found that the biochemical specificity of the polymerase influences the nucleotide sequence of the DNA.

As far as I'm concerned, that kills the central dogma, because it says that the specificity or information content of protein, which is the source of genetic specificity, is in part derived from DNA, and the specificity of DNA is in part derived from protein. This is simply the chicken-and-egg conundrum on a molecular level. It is now impossible to assert that either protein or DNA is exclusively the repository of genetic information.

I might say parenthetically that there is a reason why DNA *appears* to be the sole repository of genetic information in the sense that it will transform organisms as was shown in the famous experiments that began here in the Rockefeller Institute. In other words, you can transform an organism by infecting it with DNA, but you can't transform it by infecting it with protein. This experiment does not prove that genetic information is carried solely by DNA. In the cell, such information is redundantly incorporated into protein, but is nonredundantly incorporated into DNA. There are only one or two molecules of each genetically active segment of DNA, but there are thousands, probably tens of thousands, of replicas of each protein molecule. Clearly, if you intrude one or two molecules from the outside, you will influence the DNA population but not the protein population. If one could experimentally starve a cell and reduce the level of redundancy of protein, one might indeed observe genetic transmission with a protein.

One of the interesting developments of the last year is the discovery that the Scrapie virus does not contain nucleic acid of any kind.

It doesn't seem to contain protein, either, but as far as I'm concerned, any linear polymer may contain genetic information.

The Cold Spring Harbor Symposium of 1966 starts out with a litany reciting the genetic code but concludes with statements which contradict it: "Ambiguity"—this is a nucleotide code which indiscriminately specifies the incorporation into protein of different amino acids. Hence it is not a code at all. "Infidelity" is the tendency of the code to become drastically altered when the temperature and other supposedly non-specific conditions are changed. This is like providing a spy with a code book that produces gibberish on a warm day. "Duplicity" is the introduction into successive molecules of the same protein of two different amino acids in a place which, according to the code, should be universally occupied by only one of them. This has all the specificity that you would get from tossing a coin.

The point I'm making is that it is now clear that the origin of genetic specificity in self-duplication is not monomolecular. It does not come from DNA; it comes from the interaction of an array of molecules. Here one can apply the entire philosophical background, which John Platt very conveniently gave us.

The final thing I want to say is that I think there are serious implications of some of the data that have been put forward. We have to think about these in addition to the philosophical problems.

One of the requirements for the whole notion that there is a single molecular source of the complex features of life is that the code which translates the DNA nucleotide sequence into the amino acid sequence in proteins must be universal. If it's not universal then we are right back to the separate determination of species characteristics—which biologists have known about for a long time. This means, in other words, that if you want to make rabbits, you start with rabbits. So, universality is a very important datum and I am delighted to report that as a result of the work of Nierenberg, it has been shown conclusively, as far as I'm concerned, that the code is not universal.

Some of you may be astonished by the statement because his paper was reported, both in *Science* and in *The New York Times,* as evidence that the code *is* universal. But let's go to the data. What Nierenberg did was to set up experiments in which he took trinucleotides, which are the "words" that spell out amino acids, and artificially tested them in laying down amino acid sequences. He used test-tube systems from three different species: an amphibian, a bacterium, and a mammal. If the code is universal, then each trinucleotide should have been translated exactly the same, regardless of which species might provide the auxiliary chemical machinery.

He found that, in thirteen of the twenty amino acids, the code appeared to be universal, no matter which species was used. In seven cases the code was not universal. Now seven out of twenty seems to me pretty good evidence of non-universality. But I'll read his conclusion: "The genetic code is essentially universal."

I reject this conclusion on the grounds that the meaning of the adjective is incompatible with the effect of the adverb chosen to modify it. But even aside from that, I personally wouldn't give a passing grade to a student who reached this conclusion on that sort of data. It seems to me that he has shown that the code is *not* universal. If only three species out of the thousands available gave rise to thirty per cent non-universality, you've got a pretty loose system.

What we now know is that the simplest system which is alive, and the simplest system which is capable of replication, is the living cell. I think that this is proof that the properties of life cannot be reduced to chemistry. I'm delighted to know that in this particular year this proposition was put philosophically and, in the same year, demonstrated to be true empirically. Thank you.

CHAIRMAN HOLTON

Thank you, Mr. Commoner. Now, for the next half hour, which is all that is left for the panel before our question period, I propose that the five of us sit around this frighteningly small table, and come to blows.

I think that I should start by saying that I'm by no means prepared for this imbalance of opinion. Someone like Kornberg or Sinsheimer should be sitting here instead, to satisfy the necessity for dialectic balance. But, if I had to make a case for a strong "No" on the topic of this meeting, I would perhaps begin with an historical fact about the evolution of ideas. Recall Galileo's decision about what to call "primary" and what to call "secondary" qualities, which was his attempt to handle the problem of reduction, and to allow physics to explain both terrestrial and celestial phenomena. He threw to one side, as outside the bounds of physics and chemistry, such phenomena as those of heat, because at that time it was not possible to reduce them to measurement. But not very long thereafter, he developed the first usable thermometer, and so heat was again brought back into physics.

I have a feeling that the evolution of ideas shows that at all times, some chemists, biologists, physicists are likely to become intrigued by very difficult problems and make efforts to fit them into their science even though this means enlarging the boundaries of those studies. The one thing that makes me sure that our discussion on life processes will have to be modified next year or five years from now is that there are young people, and perhaps older people too, hard at work at this very moment to solve the problems of complexity, using as tools some modifications of present-day chemistry, biology, physics, and such studies as information theory.

The same kind of thing happened in the field of solid-state physics, where quantum mechanics, originally derived from the properties of homogeneous substances, had to be modified to deal with the much more complex problem of interfaces between impure substances. I have a feeling that the old problems of biology, insofar as they are saddled with complexity, are a challenge to the new breed of complexified and interdisciplined scientists.

The second point, very briefly, is that in the history of the evolution of ideas there appears to be a rhythm to the generation of new transforming principles. From time to time, a transforming concept such as Pauli's exclusion principle or Bohr's complementarity principle, gets injected into a field, and then the people within that field find themselves in a new kind of game. In that way, too, certain borderline fields, such as biophysics and biochemistry, can be transformed. If one were to make a bet that physics and chemistry will prove to have the capacity for encompassing these problems after all, this would be the place to make them.

Well, I have done my duty; I have not reported to you what I would say if I were giving an organized talk, but my function is to spark some disunion among these otherwise rather too unified points of view. Professor Polanyi, would you do me the favor of shooting down my remarks as a starter?

PROFESSOR POLANYI: Well, I had actually intended to answer Ernest Nagel . . .

PROFESSOR HOLTON: Oh, even better . . .

PROFESSOR POLANYI: But if I should answer to you, I would say that I am very much impressed by the fact that in physics certain reductions have been tried with great persistence for a matter of seventy years, and I don't think that any advance has been made toward reduction.

It was at the end of the last century that the kinetic theory of gases was developed into the second law of thermodynamics. The development was based on an additional assumption which was made as to the structure of a gas in an aggregate of atoms or molecules. This additional assumption was that there is an elementary disorder present. It was repulsive to many physicists when Boltzmann introduced this assumption, and they tried to get rid of it. Soon after, Max Planck was confronted in his inquiries with this situation; but, though he wanted to get rid of the assumption, he finally had to accept it. He had indeed to accept as the basis of quantum theory an even more general "impossibility" in the form of the principle of elementary disorder applying to every aspect of atomic parts.

We are in no better position today. We still have almost the whole of physics, all of thermodynamics, all of the nature of temperature and pressure and diffusion, and heaven knows what, none of which are explained in the original atomic theory of gases. All of these magnitudes are founded, not on the anatomy of particles distributed in space, but on an utterly irreducible additional principle, namely, the principle of randomness. Randomness cannot be defined in any other terms, and it is not unusual to find in physics magnitudes which resist definition in other terms.

I also want to discuss the principle of boundary conditions here for I have written about it quite a lot in past years, and nobody else even mentioned the poor thing so far. I want to take up the subject by contesting Professor Nagel's view that in this case there is something missing from the identification of the kind of knowledge and kind of principles, simply because I maintain that one cannot define something in terms of something else.

It is a very simple thing to demonstrate this because the relationship between the boundary condition and the principles which it constricts, which it controls, which it harnesses (these are the expressions I use) is represented by the fact that we have natural laws which are differential equations. Differential equations can be integrated, and then we have integration constants. I don't think anybody will contest the fact that it is logically impossible to derive an integration constant from a different equation by the integration out of which it arises. And I would add that all the cases of boundary conditions which I have mentioned have the character which I have identified in this simple mathematical illustration.

PROFESSOR NAGEL: May I comment on that?

PROFESSOR HOLTON: The rule of the game is that we must interrupt each other. It's the only rule.

PROFESSOR NAGEL: I agree wholeheartedly with the view that you cannot derive the value of the integration constants from the law itself. But it seems to me that if you make the requirement for an adequate explanation the possibility of deriving boundary conditions from the theories or laws, then you are unable to explain in physicochemical terms not only the behavior of biological systems but also the behavior of a purely physical system. Your requirements for explanation are excessive: You're defining the notion of explanation in such a way that it is logically impossible to explain anything whatsoever.

PROFESSOR COMMONER: I'm rather glad that you brought this point up because I think it is an answer to something Professor Holton proposed. I don't think that the atomistic biologists are going to win out, because it has become clear that in an area where reductionism has had a better chance to operate, namely, the relation between chemistry and physics, it has already failed.

Now, here I want to disagree with something you said in your presentation. Take, for example, the molecular properties of carbon compounds, which are based on a tetrahedral structure with four bonds of equal strength, equally oriented in space. Does the quantum-mechanical description of the properties of the carbon atom predict this situation?

What the quantum-mechanical properties of the carbon atom predict is that the atom has a valence of two. By supposing that somehow the energy state of one of the atom's electrons is elevated, it's possible to propose a new state of the carbon atom that has the valence of four. Only now it turns out that three of the bonds are arranged at right angles to each other and the fourth one is just pointing in no direction at all. Now with a little bit more wiggling with the equations, you can finally propose the actual tetrahedral bonds.

Why were all these changes in the calculations made? They were made because the chemists knew the actual structure beforehand. It's perfectly clear that the quantum-mechanical description of chemistry is an effective way to talk, to describe molecular structures. It gives us a very useful conceptual system. But it does not predict otherwise unknown molecular structures. That's why I don't think we've been able to reduce even chemistry to physics; so I think we biologists are going to be safe for a long time.

Dr. Platt: You're saying that not only does *life* transcend physics and chemistry, but chemistry transcends physics.

Professor Commoner: Right.

Professor Holton: That's what keeps chemistry interesting.

Dr. Platt: Let me make a remark which is a generalization of this. It comes back to what you said. I hope that physics is going to go on developing. I hope that next century there will be some more physics to learn, some refinement of the equations, some improvement of the ultimate approximations, down to that last 10^{-38}. But the result is that if biology is going to be explained by physics and chemistry, and if physics and chemistry are going to keep developing, it becomes an act of faith to assert that biology can be explained by today's physics and chemistry or by some ultimate physics and chemistry which you may never reach. Because, although you may now get an acceptable explanation, if physics goes on developing, either this explanation is thrown out or it's shown that the explanation was based on a false approximation.

It seems to me that each discipline at each level of complexity must justify itself in its own terms. Cells must be studied as cells, regardless of what reductionism becomes possible. Chemistry must be studied as chemistry, regardless of what reductionism is possible. Physics must be studied as physics, regardless of whether it can be explained by some Greek approach as embryological growth.

Professor Holton: Yes, I heartily agree that physics must continually undergo transformations, but I don't think that makes the physics that has been developed irrelevant. It makes parts of it uninteresting, and those parts take refuge in textbooks where only beginners ever hear about them and everyone else forgets.

Dr. Platt: But you don't want an *incomplete* physics to be explaining *all* of biology.

Professor Holton: No, of course not.

Dr. Platt: Then next year, when you get a more complete physics, some of the explanations will be wrong.

Professor Commoner: Right. I think the trouble with molecular biology is that it's a brilliant attempt to reduce biology to old-fashioned and outmoded physics. Atomism works beautifully in a certain realm of physics—in atomic physics. It's useless when you get into solid-state physics. The holistic biologist will find that he's got lots of friends among the modern physicists and chemists.

PROFESSOR NAGEL: May I intervene, though perhaps I'm really acting out of character in doing so. I'd like to break a lance on behalf of the reductionist program, though I don't subscribe to it in the dogmatic sense; I don't believe that one must be able to reduce biological phenomena to physicochemical processes.

In the first place, I would like to say that nothing in the reductionist program, as I understand it at any rate, precludes the need for studying the behavior of the organism as a highly complex system. The temporal order in which the hierarchically organized biological structures are discovered is irrelevant to the question of reducibility. The reductionist program does not say that you must first begin to explore the behavior of elementary physical particles, and study the properties of the total system only subsequently.

No, this assumption about the order of study introduces a consideration that I think is not germane to the question we are presumably discussing—namely, whether, once we have established some laws about the behavior of the total living system or some parts of it, the processes that are operative in that organism or in some of its constituent parts can be explained in terms of physiochemical properties. Let me also add that I am not qualified to challenge the claim that some of the proposed explanations of the carbon bond in terms of quantum mechanics are unsuccessful. Nevertheless, if the claim is warranted, all it shows is that quantum theory has not yet achieved what it hoped to do; it does *not* show that the proposed reduction is inherently impossible.

PROFESSOR COMMONER: My point is that the successful use of quantum mechanics in chemistry *requires* a prior understanding of chemistry itself, so that there is no way to substitute quantum mechanics for chemistry.

PROFESSOR NAGEL: Maybe so.

PROFESSOR COMMONER: What I am saying is that a system like a living cell, which is inherently complex, has properties which are not discernible in the isolated parts of the system. Therefore, the information that you derive from the parts cannot possibly lead you to the unique properties of the whole. This is a restatement of Niels Bohr's complementarity principle. I am prepared to accept the cell as a unit of action, if you like, just as Bohr accepts the quantum as a unit of action. It makes no sense to look for details in the cell and connect them to life when it's absolutely necessary to destroy life in order to describe these details.

PROFESSOR POLANYI: I would like to use this opportunity, where I fully agree with Professor Nagel, to join him in his admonition that the difficulty of carrying out calculations, for example, for the carbon bond, has nothing whatever to do with the logical possibility of explaining the carbon bond in terms of quantum mechanics.

But let me add in reply to Nagel, that I do not deny that one can have such irreducible systems as I have described in my talk in the inanimate domain. I have in fact described one such system in the case of a machine. Nothing could be more inanimate than a machine.

One can have such systems also in the atmosphere, as Professor William T. Scott of the University of Nevada, who is here in the audience, has shown in some detail. And that is quite natural, because in the atmosphere you have part of the material (that is, the moist air) which is to be treated statistically, and also another part (the drops produced by individual molecules or atoms) which is to be treated in a way that is incompatible with statistics.

I have, in fact, mentioned in the paper that originally started this discussion that there is plenty of evidence of this kind of irreducibility in the inanimate domain, owing to boundary conditions generated by the development of the universe producing stratification and other orderly patterns. The difference which I pointed out is that the quantity of information—of negentropy or whatever you call it—is vastly greater per cubic centimeter in living things than in the comparatively empty air which produces rain, or in any other inanimate system found in nature.

DR. PLATT: Isn't that what I called information? Complexity?

PROFESSOR POLANYI: Yes, information, negentropy. I'm not so sure about complexity.

DR. PLATT: You don't want to call it complexity?

PROFESSOR POLANYI: Complexity does not necessarily have information. Perhaps it does, but I'm not quite sure about it.

PROFESSOR NAGEL: I'd like to make two comments, one to Professor Commoner's remark and one to Professor Polanyi's.

Professor Commoner made the statement that you cannot discern in the parts the properties of the total system. Perhaps you cannot in one sense of "discern," but it seems to me that this is not entirely germane to the issue. What you stipulate as constituting parts of the system would have to be determined by some general theory that you assume about the system. Could you discern in the properties that are attributed to a nineteenth-century atom the fact that it has valence? No, you can't discern this, but you have to have some kind of theory about the atom.

And so it doesn't seem to me that the question of whether you can discern a property in the sense of being able to observe it, either directly or indirectly, is a germane question. The question should be whether, given a particular theory about constituent parts, you can derive from the theory relevant statements about specified properties of the system.

My second point is in reply to Professor Polanyi. Again I must apologize for being so intransigent, but there's no one here to defend the reductionist position, so I feel that I'd like at least to play devil's advocate as far as possible.

Certainly it seems to me that any man of any sense whatsoever would have to agree that the boundary conditions determine the operations of the system and that the laws of physics or chemistry are not sufficient to account for the behavior. On the other hand, the way in which the water flows down a river is in part determined by what the banks are like. Surely the principles of hydrodynamics are operative, but so are the characteristics of the banks. It seems to me that this is something that can be explained in terms even of our present knowledge of physicochemical processes.

So I'm not quite sure where you draw the line in any given problem between what can be explained in terms of physics and chemistry and what cannot. Boundary conditions have to be stipulated. But in the illustrations that you yourself give, it is by no means clear that the character of the boundary conditions cannot in another context of investigation be explained in physicochemical terms.

PROFESSOR COMMONER: Let me try to pin this disagreement down to a specific thing—the cell. Now, what do I mean by my generalization about the parts and the whole cell? A living amoeba has certain properties. Dismember it, take out of it material which diminishes the amoeba, and you find that it has lost what was its most interesting property—namely, that it was alive.

It seems to me that this immediately defines what we mean by parts and what we mean by explanation. I know that if you did the experiment right, you could explain the mass of the cell by summing up the mass of the separate parts. But what I'm saying is that nothing that you can derive from an analysis of the parts leads to an explanation of the fact that the entire system is alive.

I think, to be positive about it, that we've got to begin to worry about ways of dealing with intrinsic complexity which don't rely on the crutch of atoms.

Atomism seems to have worked very well in a rather narrow segment of the array of matter—atomic physics. However, it may well turn out that atomic physics is a special case in which atomism works and that in the rest of the universe we are confronted with a totally new problem.

That problem is, how to develop an analysis of something which retains its intrinsic complexity. Lord knows, it's not going to be easy to publish papers rapidly on this. There are many restraints on it, there are no quick answers. But in many ways, I think this is the issue and that the age of atomism, to quote Lancelot Law Whyte, is drawing to a close.

PROFESSOR HOLTON: Would you be happy if this were granted, and the three categories of the childhood game, "animal, vegetable, and mineral," were now enlarged to include negentropy? Is that then a basis of explanation? In other words, where are you dissatisfied with atomism and therefore with mineral alone? At what point would you say that you have a basis for a simple explanation that does encompass life? Are the ideas from information theory and cybernetics, as Platt remarked, such a basis? Or, are the ideas of the generalized machine of Professor Polanyi? Where would you yourself feel happy to stop?

PROFESSOR COMMONER: The whole concept of information content arises from electronics, and I'm not sure that it applies to biology.

I think that that which is alive is a system which is capable of automatically generating a replica of itself.

This is an old idea—that life is derived from life—which leaves open the historical question of where the first living organism came from.

DR. PLATT: Your definition, though, is only possible in a suitable environment.

PROFESSOR COMMONER: Right.

Dr. Platt: And so it is a system's property of organism plus environment, and not even of organism alone.

Professor Commoner: Except that the *specificity* of the organism is derived solely from the preceding organism.

Dr. Platt: In the same environment.

Professor Commoner: For example, if you change the nature of the food, you still get the same organism. There may, of course, be questions about mutagens in the environment and so on, but I don't think that's fundamental.

I think that what we mean by self-duplication is that the specificity which is inherited is totally derived from the preceding living organism. This is a definition which will stand up pretty well. Anything that can do that is alive; anything that can't do that is dead. Kornberg's virus was dead. The bacterium that made the DNA that was used was alive. You can get into trivial questions, such as spores, which aren't able to do very much, or an organism which is no longer capable of replication. But these are kinds of logical traps. I would simply settle for the biblical notion of begetting.