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Source: *Science*, New Series, Vol. 139, No. 3550 (Jan. 11, 1963), pp. 81-88
Published by: American Association for the Advancement of Science
Stable URL: http://www.jstor.org/stable/1710098

11 January 1963, Volume 139, Number 3550 SCIENCE

Biology and the Nature of Science

Unification of the sciences can be most meaningfully sought through study of the phenomena of life.

George Gaylord Simpson

Day by day the researcher, the teacher, and the student think in terms of immediate goals and tasks: the project, the lecture, the assignment. All of us should occasionally back off a bit and ask some questions from a wider perspective. What is science anyway? What do scientists hope to accomplish? How does a particular science articulate with science as a whole?

Escape from the Greeks

There is a whole library of attempts to define science. The literature is so prolix and in part so contradictory that I cannot analyze it and should perhaps hesitate to add to it. The element of confusion is well illustrated by a recent statement that science is "thinking about the world in the Greek way." That is in fact an important thing that science is *not*. It has often been argued that the Greek sense of order was a necessary condition for the rise of science. Necessary, perhaps; sufficient, definitely no. The actual origin of science in the modern sense involved a revolt against thinking in the Greek way.

The Greek way, which became traditional in medieval Europe, was well expressed by Plato, for example, when he said in *The Republic*, "We shall let the heavenly bodies alone, if it is our design to become really acquainted with astronomy." In other words the essence of things was believed to reside in a philosophical ideal, and observation of real phenomena was considered not only unnecessary but also positively wrong. Some five centuries after Plato, Ptolemy again formalized the Greek way and helped to embed it in Western thought for another 1500 years when he said that the goal of astronomy was "to demonstrate that all heavenly phenomena are produced by uniform circular motion." Now, that is not physically true, and Ptolemy knew that it was not. He was explicit that his intention was not to explore physical reality. The early astronomers' only gestures toward reality were attempts to "save the appearances," that is, to try to eliminate obvious contradictions without abandoning their a priori philosophical ideals, such as that of uniform circular motion. "Saving the appearances" was a euphemism for saving the philosophical postulates. Facts were not to be explained, but to be explained away.

Science was born when a few thinkers decided that appearances were something not to be saved but to be respected. Those hardy souls—Copernicus, Galileo, and Kepler were among them—eventually abandoned the Greek way of deciding how things ought to be and gave us our way of observing how in fact things *are*. Definitions of science may differ in other respects, but to have any validity they must include this point: the basis of science is observation. This may be expressed and applied in different ways. Francis Bacon, a close contemporary of Kepler, had a concept of science as gathering all possible observations and then deriving from them generalizations and laws by induction, in accordance with an elaborate system rooted in Scholastic logic. It has often been pointed out that Bacon's system does not really work and has not been followed by any successful scientist (Bacon himself was not one). Nevertheless his respect for observation and his operational approach to science are among the points in which his influence has been profound and beneficial.

The Definition of Science

Thinking of science in terms of methods came more and more into vogue with the scientific triumphs of the 19th century and science's great acceleration into the 20th. Indeed one still, although now less frequently, hears of teaching *the* scientific method, as if science were a set routine applicable to any subject. That tendency reached a climax with Karl Pearson and others a generation or so ago. The scientific method was sometimes formalized as involving six successive operations.

1) A problem is stated.
2) Observations relevant to the problem are collected.
3) A hypothetical solution of the problem consistent with the observations is formulated.
4) Predictions of other observable phenomena are deduced from the hypothesis.
5) Occurrence or nonoccurrence of the predicted phenomena is observed.
6) The hypothesis is accepted, modified, or rejected in accordance with the degree of fulfillment of the predictions.

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There is no question that such a cut-and-dried method does work in particular instances, or that each of the six operations is essential in various phases of scientific research. Nevertheless, the formulation fails as an overall characterization of science. It is not a definition; it says nothing of the goals or the nature of science. Its implication of a general routine that automatically solves any scientific problem is false. It quite ignores the most difficult, most creative, and most important elements of scientific endeavor. How does one discern a problem, or decide what kinds of questions are to be asked? How does one determine what observations are relevant? And especially, what kind of hypothetical solutions are acceptable and where do they come from? Perhaps the most cogent objection of all is that important basic research has seldom really followed the "method" just as it is stated.

In our own days James Conant has strongly criticized that kind of formulation and has proposed a new definition of science and another characterization of its methods. His definition is: "An interconnected series of concepts and conceptual schemes that have developed as a result of experimentation and observation and are fruitful of further experimentation and observation." He characterizes scientific method as comprising: "(1) speculative general ideas, (2) deductive reasoning, and (3) experimentation." Like all brief statements on any subject, these are ambiguous and incomplete outside of the expanded context given them by the author. The definition, taken by itself, does not define. If reread, it will be found to apply perfectly to the work of Picasso, for example, and although Picasso's work is certainly creative and great it is with equal certainty not science. Of course Conant's point is to emphasize the dynamic, ongoing nature of science. That is a characteristic of the most important scientific investigations, but dynamism is not confined to science and does not characterize all of science.

Conant may also be too hard on his predecessors. His summary of scientific method is freer, more impressionistic, than the earlier formulation and to that extent more nearly covers the varied gambits of research. It is not, however, contradictory, and in some respects it is less complete or explicit. It starts with the formulation of the hypothesis, the third step of the earlier summary, and its other two steps are essentially the fourth and fifth. The sixth step is not eliminated but is simply taken for granted and not stated.

The main virtue of Conant's formulation is its recognition of the role of speculation, intuition, or just plain hunch in finding a hypothesis. It ignores the fact that some observation inevitably precedes the speculation, and both formulations fail to note that observation (whether of nature or of an experiment) always is the first step in any scientific investigation. No one ever had a hunch that was not *about* something —in the case of science, about possible relationships among facts already known.

Conant makes the essential point that the aim of science is to seek and verify general ideas, relationships, and interconnections among phenomena. Obviously science has nothing to do and cannot exist if phenomena have not, in fact, been observed, but there science begins, not ends. It follows that although the observation of facts or memorization of data is a necessary basis and accomplishment of science, that in itself is not science. Science, truly to be such, must center not on descriptions and names but on principles—that is, generalizations, theories, relationships, interconnections, explanations about and among the facts.

A second point often left implicit but requiring meticulous attention is that the materials of science are literally material. The observations of science are of material, physically or objectively observable phenomena. Its relationships are material, natural relationships. This is not to say that science necessarily denies the existence of nonmaterial or supernatural relationships, but only that, whether or not they exist, they are not the business of science. This requires, if you like, a measure of self-discipline among scientists, a recognition that their methods do not work properly in the absence of this restriction.

The third feature that distinguishes science from other fields of thought and of activity is that it is self-testing by the same kinds of observations from which it arises and to which it applies. It is, to use a currently popular but perhaps overworked bit of jargon, a cybernetic system with a feedback that in spite of oscillations keeps its orientation as nearly as may be toward reality. That is the point of the deductive phase stressed by both Pearson and Conant as well as by almost all other modern writers on scientific method, although in fact formal deduction is not invariably involved in scientific self-correction. On that more must be said, but here we have reached a point where another attempt to define science is in order.

One way to approach definition is to consider science as a process of questioning and answering. The questions are, by definition, scientific if they are about relationships among observed phenomena. The proposed answers must, again by definition, be in natural terms and testable in some material way. On that basis, a definition of science as a whole would be: Science is an exploration of the material universe that seeks natural, orderly relationships among observed phenomena and that is self-testing. We may well add, but not as part of a definition, that the best answers are theories that apply to a wide range of phenomena, that are subject to extensive tests and that are suggestive of further questions. It is such theories that contribute most to the ongoing aspect of science so properly stressed by Conant. Nevertheless, most scientific endeavor has more limited objectives, and some endeavor, even though scientific by definition, has no evident sequel.

The Straying Physical Sciences

It is noteworthy that almost all studies of the philosophy and methods of science have referred primarily to the physical sciences. That is in part because the physical sciences do have a primacy—not, I insist, logically but historically. The first sciences, as we now strictly define science, were physical sciences. That was at a time when scientists considered themselves to be also, or even primarily, philosophers, and indeed "natural philosophy" was long synonymous with "physics." The tradition has persisted. It has been reinforced by the reductionist half-truth (of which more later) that all phenomena are ultimately explicable in strictly physical terms. Another factor has been the prestige accruing from the thorough and more obvious impingement of the physical sciences on daily life through technology. It is also possible that more of the most brilliant and thoughtful minds have gone into the physical sciences; I prefer not to think so, but I suspect there is some truth in that.

The point here is that most considerations of the history, methods, and nature of science have been heavily biased by concentration on physical science and not on science as a whole. That has been notably true of concepts of scientific laws, of predictability, of the testing of hypotheses, and of causality. Francis Bacon warned, "Though there are many things in nature which are singular and unmatched, yet it [the human understanding] devises for them parallels and conjugates and relatives which do not exist." Nineteenth-century physicists did not heed his warning. They refused to consider the unique object or event and assumed that all phenomena could be reduced to supposedly invariable and universal laws such as the gas laws or the law of gravitation. It followed that, once a law was known, its consequences could be fully predicted. The consequences deduced from a hypothesis became predictions as to what would happen if an experiment were performed, and that is the pertinent test embodied in Pearson's, and still in Conant's (and many others'), descriptions of scientific method. It further followed—or the physical scientists thought it did—that when a law successfully predicted an event, the law explained the event as a result and specified its causes.

Here we in the 20th century have seen something curious and indeed almost comic happen. Physicists have found that some, at least, of their laws are not invariable; that their predictions are statistical and not precise; that some observations cannot in fact be made; and that absolute confirmation by testing of a hypothesis therefore cannot be obtained. Many have gone further and concluded that causality is meaningless and even that order in nature—the last *scientific* relic of our Greek heritage—has disappeared. That is, of course, the so-called scientific revolution wrought by quantum theory and the principle of indeterminacy. The physicists' reactions to this (even in my very limited knowledge of them) ran the gamut from reason to hysteria. Some—Bridgman is a sad example—found science coming apart in their hands, further scientific knowledge impossible, and the universe and existence itself left wholly meaningless. Others, such as Jeans, also accepted the whole idea of orderlessness and acausality but, with almost mystical glee, likened the release from physical law to release from prison. Still others, with Schrödinger, have had what seems both the most mature and the most scientific reaction: they have concluded that the physicists have failed somewhere and that there must be some rational way to get over the difficulty.

The aspect that I spoke of as almost comic is this: well before the "revolution" life scientists had observed that laws, in the rigid 19th-century conception of physics, do not apply to many phenomena in nature. Further, they knew that prediction (not the only way of testing hypotheses) is commonly statistical and no less scientific or confirmatory of a hypothesis for all that. They knew that this is no contradiction of the orderliness of nature, and they discerned that only an unnecessarily restricted concept of causality is affected. The "revolution" was a revolution only for those who had insisted that everything must be explained ultimately in terms of classical physics—and where were there ever any real grounds for such a narrow view of science? It is true that understanding of statistical law and polymodal causality had crept over the life scientists gradually, so that the impact of these concepts was not seen as revolutionary. It is also true that not many biologists are given to exploring the philosophical implications of their science. There was therefore little really clear discussion of causality in biology before that by Ernst Mayr in 1961.

Self-Testing in Science

A fundamental, though not a sufficient, criterion of the self-testability of science is repeatability. Norman Campbell's definition of science as "the study of those judgments concerning which universal agreement can be obtained" emphasizes this point. That is indeed not so much a definition of science as of its field and its connection with reality. Campbell's meaning is that the data of science are observations that can be repeated by any normal person. That is as true of, say, the observation of a fossil tooth under a microscope as it is of the height of mercury in a tube in Torricelli's famous experiment, or of more recent observations of protein separation by chromatography and electrophoresis. Illusion, even to the point of hallucination, is always a possibility, but it is one that can be eliminated for all practical purposes by repetition of observations, especially by different observers and different methods. It is also true that unique events occur, but evidence on them is acceptable if there is confidence that anyone in a position to observe them would have observed them.

In what used to be called the exact sciences, which have turned out not to be so exact, it was formerly assumed that uniform phenomena had absolute constants measurable to any degree of accuracy. As a very simple example, the length and period of a pendulum were assumed to have an infinitely exact and determinable value. It now appears that this is not necessarily true, and that is one of the discoveries that so upset the physical scientists. But in the actual practice of observation it has always been evident that infinitely exact measurement is impossible. All that repetition and instrumental refinement can do is to generate a degree of confidence that a measurement (at any given time and under given conditions) lies within a certain range. Inference from the observation takes into account the size of the range and the degree of confidence. The conclusion that even in principle the range cannot be infinitely small and confidence infinitely great makes no difference operationally, at least.

It is further true that with many phenomena the whole point of observation is not an exact measurement or determination of occurrence but establishment (again to some degree of confidence) of a probability. The classical example is the tossing of a coin, and here the biologists' point is that we do *not* expect the probability of throwing heads to be exactly one-half. As modern scientists and not ancient Greeks, we are examining real, objective coins and not the Platonic idea of a coin. By repeated observation of a real coin, we can establish a high degree of confidence that the probability is in a certain range. If the range is large, it is likely to include the probability of one-half, but if the range is made small it is likely to exclude that a priori ideal. Analogous phenomena are very common in biology. For example, we do not expect an expanding population of flies to spread according to an exact law. We expect only to achieve confidence that the rate will be within a certain range of probability, or to construct a frequency distribution of rates. Discovery that Boyle's "law" has the same probabilistic nature neither surprises nor upsets us. We would expect it, because the molecules of gas, like the flies, are real individuals which, however alike they are in other respects, have had different histories. The Greeks could, but a scientist cannot, be concerned with the ideal gas of classical physics. Perhaps the revolution in physics was only the final severing of the umbilical cord from ancient Greece.

The most widespread and conclusive process of self-testing in science is testing by multiplication of relevant observations. In the natural sciences it is impossible to prove anything in the absolute sense of, for example, a proof in mathematics. Multiplication of observations can only increase our confidence within a narrowing range of probability. If confidence becomes sufficiently great and the range is encompassed by the hypothesis, we begin to call the hypothesis a theory, and we accept it and go on from there. The test, is of course, whether the range of probability is in fact within the scope of the hypothesis—in other words, whether the observations are consistent with the hypothesis.

A key word in the expression "multiplication of relevant observations" is *relevant*. The simplest definition is that relevant observations are those that could *disprove* the hypothesis, for disproof is often possible even though absolute proof is not. The more observations fail to disprove a hypothesis, the greater the confidence in it. Prediction in the classical sense is a special case of that general procedure. From the hypothesis consequences are deduced such that their failure to occur would disprove the hypothesis. Of course their occurrence would not *prove* anything; it would only increase confidence. That this is in fact a special case and not the touchstone of scientific theory is easy to demonstrate. Again, examples are more familiar to biological than to physical scientists, although they occur in both fields. The most striking example is the most important of all biological theories: that of organic evolution. Although some quite limited predictions can be deduced from the theory, the theory was not in fact established by prediction and is not sufficiently tested by it. An enormous number of observations enormously varied in kind are all consistent with this theory, and many of them are consistent with no other theory that has been proposed. We therefore can and, if we are rational, must have an extremely high degree of confidence in the theory—higher than legitimate confidence in many things we call "facts" in daily life. That kind of nonpredictive testing most commonly occurs in fields that have a temporal or historical element, such as evolution among the biological sciences or the time-linked processes in geology among the physical sciences. In fact a neglected historical component also affects many physical laws, as in the example of the histories of the individual molecules in a gas.

Science and Reality

In discussing the nature and basic procedures of science I have been quite free in using such expressions as "reality," "phenomena," "the material universe," and so on. Philosophers have long since pointed out, and philosophical scientists are still worried by, the fact that the very existence of such a thing as objective reality is uncertain. I have already referred to Bridgman's despairing conclusion that "the very concept of existence becomes meaningless." In *The Scientific Outlook* Bertrand Russell has discussed this matter more optimistically if equally inconclusively. He points out (in more and different words) that what we call observing a phenomenon is in fact only sensing certain events that occur to and within ourselves. For example, when we think we have seen something, we know only the event that light quanta of certain energies and patterns impinged on our retinas and produced other events in our nervous system. The object we think we saw "remains veiled in mystery." Russell asks finally, "Are circumstances ever such as to enable us, from a set of known events [for example, those in our nervous system] to infer that some other event [for example, the material existence of what we think we see] has occurred, is occurring, or will occur?" He concludes, "I do not know of any clear answer. . . . Until an answer is forthcoming, one way or another, the question must remain an open one, and our faith in the external world must be merely animal faith."

Now, some feel that this is nonsense and that sensible people will not waste time on it. Whether or not there *really* is an external world, we certainly have to act as if there were, so we may as well ignore the question. Indeed I shall not here spend much time on it, but it has bothered many scientists, so it does seem worth while to point out that there *is* an answer. In fact there are several. Russell himself has provided one, apparently unwittingly, although it is dangerous to assume that he is ever unwitting. His example of what he calls the "known events" includes light from the sun bouncing off a man named Jones and then entering the eye. "Jones himself" may still be "wrapped in mystery," as Russell says, but evidently *something* happened out there. The faith required is not that "out there" exists, but that what happens "in here" contains some information about it. Such an answer obviously does not supply a philosophical absolute, but it should satisfy a scientist's more modest demand for reasonable confidence.

Norman Campbell has pointed out that the fact that others demonstrably receive the same sensations as we do from the same stimuli is evidence that the outer world does exist. That is the basis for his remark, quoted earlier, on the obtainability of universal agreement in [observational aspects of] science. It is also evidence that the stimuli are structured—that is, do convey information. Again a philosopher may quibble and say that the reactions of others have no bearing if the others are not really there, but a scientist will gain another degree of confidence.

Still another consideration seems to me the most interesting of all, and yet I have never seen it clearly expressed elsewhere. It is, in a sense, a validation of the "animal faith" given by Russell (after Santayana) in the passage quoted earlier, as sole basis for assuming that we really can obtain knowledge of the outer world. The fact is that man originated by a slow process of evolution guided by natural selection. At every stage in this long progression our ancestors necessarily had adaptive reactions to the world around them. As behavior and sense organs became more complex, perception of sensations from those organs obviously maintained a realistic relationship to the environment. To put it crudely but graphically, the monkey who did not have a realistic perception of the tree branch he jumped for was soon a dead monkey—and therefore did not become one of our ancestors. Our perceptions do give true, even though not complete, representations of the outer world because that was and is a biological necessity, built into us by natural selection. If it were not so, we would not be here! We do now reach perceptions for which our ancestors had no need, for example of x-rays or electrical potentials, but we do so by translating them into modalities that are evolution-tested.

Biological Nature of Science

That is one of the several senses in which science itself, as a whole, is fundamentally biological. A second sense in which that is true is involved in another point that has lately been bothering the physicists. The point is that whenever a scientist observes anything he is himself part of the system in which the observing takes place. He therefore should not assume that what he observes would be exactly the same if he were not observing it. But he cannot very well observe what happens when he is not observing! Therefore, the argument runs (but personally I do not run with it), there is no such thing as objective knowledge, and the goals of science are wholly delusive. Some atomic physicists say this does not matter as far as the man-sized world is concerned but matters only when you get down to their invisible, but all too obviously not imaginary, objects of study. Yet I really do not see why size matters in principle. In either case the system actually observed contains something alive—to wit (as a minimum), the observer. Surely it would never occur to anyone but an atomic physicist that because a system includes something alive it cannot be properly studied!

To suppose that study, to be objective, should exclude the observer is as unrealistic as Plato. Science is *man's* exploration of his universe, and to exclude himself even in principle is certainly not objective realism—unless you insist that his inclusion is subjective by definition, but that would be merely playing with words. And to say that we cannot learn anything materially factual about a situation if we ourselves are in it is utter and nonsensical negation of the very meaning of learning. The essential in objectivity is not the pretense of eliminating ourselves from a situation in which we are objectively present. It is that the situation should not be interpreted in terms of ourselves but that our roles should be interpreted realistically in terms of the situation. To a biologist the discovery (to call it such) that every system observed includes the observer has quite a simple meaning. It merely means that all systems in science have a biological component.

There is another, related sense in which all science is partly biological. It is all carried on by human beings, a species of animal. It is in fact a part of animal behavior, and an increasingly important part of the species-specific behavior of *Homo sapiens*. From the functional point of view, it is a means of adapting to the environment. It is now, especially through its operating arm, technology, the principal means of biological adaptation for civilized man. It is an evolutionary specialization that arose from more primitive, prescientific means of cultural adaptation, which in turn had arisen from still more primitive, prehuman behavioral adaptation. I recently had occasion to point out to some ethnologists that culture in general is biological adaptation and that they could resolve some of their squabbles and find the common theoretical basis that eludes them if they would just study culture from this point of view. The suggestion was not well received, but it is true just the same. Some thought I was being a racist and some thought I was being a social Darwinian, both quite rightly pejorative epithets in ethnological circles. Of course I was being neither one. I was just being a biologist drawing attention to the really quite obvious fact that culture is a *biological* phenomenon. That is true, in heightened degree, of the special aspect of culture we call science.

Flight from Teleology

As Gillispie has admirably shown in his book *The Edge of Objectivity*, the rise of science, in the strictest modern sense of the word, centered around increasing insistence on objectivity. It now seems clear that in some instances that insistence went too far. I have noted that some scientists reached the unnecessary and, in the last analysis, absurd position that complete objectivity would exclude the observer. Since exclusion of the observer is obviously impossible in the practice of science, scientists who held that view, as we have seen, tended either to fall into despair or to revert to various more or less covert forms of idealism. I have here maintained that this was an unnecessary casualty and that the concept of objectivity essential to science is saved by recognition that scientific objectivity has a biological component. A related casualty that was almost inevitable in the struggle to develop modern science involves the concept of teleology.

The doctrine of final cause, of the end's determining the means, was another essential element in Greek thought, which was anthropomorphic in a truly primitive way. This doctrine was probably an inevitable outcome of introspective and deductive philosophy. Rational human actions are largely explicable by their purpose, by the results they are expected to produce. It therefore seemed logical to conclude that the orderly intricacy of the world at large was in a similar way purposeful and governed by a foreseen end. Such concepts were particularly important to Aristotle, and through his works they came to be held as almost axiomatic in the western European milieu in which science finally arose. The broadly philosophical position was that things exist, or events occur, as prerequisites of their results, and that the result, as final cause, is the real principle of explanation. In more popular form, this view led to the belief that nature exists only for and in relation to man, considered as the ultimate purpose of creation or the overriding final cause.

As physical science became more objective, it was apparent that teleology, even if not rejected as a philosophy, had to be ignored as a means of scientific explanation. The scientist, as such, asked "What?" or "How?" about phenomena such as gravity or gas pressure, not "Why?" or "What for?" Description of how things fall, in terms of masses, distances, and gravitational constants, is clearly scientific, but the question, "What do things fall for?" seems unscientific. It elicits no objectively testable answers. It was thus inevitable that the strictest scientific attitude should endeavor to exclude any form of teleology, and in the physical sciences there seemed to be no great difficulty in excluding it. One could, at least, readily evade teleology by ascribing physical laws to a first rather than a final cause, although even here the usual philosophical or theological belief continued to be that natural laws exist in order to make the world a suitable habitat for man.

In the biological sciences the elimination or even the brushing aside of crude teleology was incomparably more difficult, and that is a principal reason why a fully scientific biology lagged so far behind a scientific physics. It is not necessary or perhaps even possible to see any immediate, inherent purpose in a stone's falling, but it is quite inevitable that an animal's seeking its food should be interpreted in terms of purpose or, at least, of an end served. All organisms are clearly adapted to live where and how they in fact live, and adapted in the most extraordinary, thoroughgoing, and complex ways. In fact they plainly have the adaptations in order to live as they do. The question, then, is how those key words *in order to* are to be interpreted. Until a century or so ago it occurred to very few naturalists to interpret them in any but the classical teleological way. For example, to Cuvier, high priest of natural history in the early 19th century, the validity of fully Aristotelian teleology seemed self-evident, and it was the heart of his theoretical system. Cuvier went all the way to a man-centered teleological conception of the universe. He could think of no better reason for the existence of fishes—which he considered poor things, even to the watery, unromantic nature of their *amours*—than that they provide food for man. That was also the period in England of Paley's *Natural Theology* and, later, of the Bridgewater *Treatises* "on the power, wisdom, and goodness of God, as manifested in the creation"—that is to say, on Christian teleology as a necessary and sufficient explanation of nature, and most particularly of animate nature.

The facts of adaptation *are* facts, and the purposeful aspect of organisms is incontrovertible. Even if the explanation offered by Aristotelian, and much later by what was then orthodox Christian, teleology were true, that would definitely be an article of faith and not of objectively testable science. Thus it was necessary either to conclude that there is no scientific explanation of organic adaptation or to provide an acceptable, testable hypothesis that was scientific. Before Darwin most biologists accepted the first alternative, which (although few of them realized this fact) meant quite simply that there could be no such thing as a fully scientific biology. It was Darwin, more than any other one person, who supplied the second alternative. In *The Origin of Species* he made no entirely clear distinction between establishing the fact that evolution has occurred and proposing a theory as to how natural processes could produce organic adaptation. He has therefore been accused of unnecessarily confusing two issues that should have been kept quite separate, but that was not really the case. Evolution itself becomes a nonscientific issue if the explanation of adaptation in the course of evolution is left in the field of metaphysics, philosophy, and theology. Darwin really went to the heart of the matter with unerring insight. Explanation of adaptation was the key point, and Darwin demonstrated, at the very least, that a natural, objective explanation of adaptation is a rational possibility and a legitimate scientific goal. That, at long last, made biology a true and complete science.

Darwin fully respected the appearances and made no attempt to save them by explaining them away. The hand of man, for example, *is* made for grasping. Darwin said so, and then provided a natural scientific explanation for the fact. He thus did not ignore the teleological aspects of nature but brought them into the domain of science. Some of Darwin's contemporaries and immediate successors recognized that fact by redefining teleology as the study of adaptation and by pointing out that Darwin had substituted a scientific teleology for a philosophical or theological one. The redefinition did not take. The older meanings of the word *teleology* were ineradicable, and they brought a certain scientific (although not necessarily philosophical) disrepute to the whole subject.

The physical scientists had earlier, and more completely, evaded the issues of classical teleology. By the end of the 19th century, if not before, it had become for them virtually a dogma that a scientist simply *must not* ask, "What for?" Physical scientists considered the question as applied to natural phenomena either completely meaningless or, at best, unanswerable in scientific terms. Such was the priority and primacy of the physical sciences that this position even came to be widely considered a necessary qualification of truly scientific endeavor, part of the definition of science. That led in turn to a very curious development that was at its height in the 1920's and is still exerting a strong but now more clandestine effect. Many biologists threw out the baby with the bath water. In seeking to get rid of nonscientific teleology they decided to throw out all the quite real and scientific problems that teleology had attempted to solve.

That took several different forms. One form in evolutionary studies was the mutationist belief that organisms do not become adapted to a way of life but simply adopt the way of life that their characteristics, originating at random, make possible. Another form was behaviorism, which also, in essence, sought to eliminate adaptation as a scientific problem by refusing to consider behavior as motivated, as goal-directed, or even as serving needs (and hence in some sense having purpose) in the organism as a whole. Behaviorism strove to be primarily descriptive, and what explanatory element was admitted was meant to be confined to consideration of the physiological substrates and concomitants of the behavior described. It is that latter aspect that still influences a considerable segment of opinion in biology, confining biological explanation to the physicist's question, "How?" and eschewing "What for?" This attitude, still strongly held in some quarters, involves the idea that scientific explanation must be reductionist, reducing all phenomena ultimately to the physical and the chemical. In application to biology, that leads to the quite extraordinary proposition that living organisms should be studied as test-tube reactions and that their being alive should enter into the matter as little as possible. As behaviorism omits the psyche from psychology, so this form of reductionism omits the bios from biology.

Explanation in Biology

Those tendencies were unquestionably salutary in some respects. They have helped to eliminate the last vestiges of pre-Darwinian teleology from biology. They have also helped to counteract vitalistic, metaphysical, and mystical ideas which, whatever one may think of them in their own sphere, are completely stultifying as principles of scientific explanation. Here, however, the reductionist tendency has been two-edged. By seeming to negate the very possibility of scientific explanation of purposive aspects of life, it has encouraged some biologists, who insist that such aspects nevertheless exist, to seek explanations quite outside the legitimate field of science. Naming of names is perhaps invidious, but to show that I am here setting up no straw man I will just mention Teilhard de Chardin in Europe and Sinnott in the United States.

The reaction went much too far. It went so far as to falsify the very nature of biology and of science through supine acceptance of a dictum that all science is in essence physical science. In fact, the life sciences are not only much more complicated than the physical sciences, they are also much broader in significance, and they penetrate much farther into the exploration of the universe that *is* science than do the physical sciences. They require and embrace the data and *all* the explanatory principles of the physical sciences and then go far beyond that to embody many other data and additional explanatory principles that are no less—that are, in a sense, even more—scientific.

This can be expressed, as Mayr, Pittendrigh, and others have expressed it, in terms of kinds of scientific explanations and kinds of questions that elicit them. "How?" is the typical question in the physical sciences. There it is often the only meaningful or allowable one. It must also always be asked in biology, and the answers can often be put in terms of the physical sciences. That is one kind of scientific explanation, a reductionist one as applied to biological problems: "How is heredity transmitted?" "How do muscles contract?" and so on through the whole enormous gamut of modern biophysics and biochemistry. But biology can and must go on from there. Here, "What for?"—the dreadful teleological question—not only is legitimate but also must eventually be asked about every vital phenomenon. In organisms, but not (in the same sense) in any nonliving matter, adaptation *does* occur. Heredity and muscle contraction do serve functions that are *useful* to organisms. They are not explained, in this aspect, by such answers to "How?" as that heredity is transmitted by DNA or that energy is released in the Krebs cycle.

In biology, then, a second kind of explanation must be added to the first or reductionist explanation made in terms of physical, chemical, and mechanical principles. This second form of explanation, which can be called compositionist in contrast with reductionist, is in terms of the adaptive usefulness of structures and processes to the whole organism and to the species of which it is a part, and still further, in terms of ecological function in the communities in which the species occurs. It is still scientifically meaningful to say that, for instance, a lion has its thoroughgoing adaptations to predation *because* they maintain the life of the lion, the continuity of its species, and the economy of its communities.

Such statements exclude the grosser, man-centered forms of teleology, but they still do not necessarily exclude a more impersonal philosophical teleology. A further question is necessary: "How does the lion happen to have these adaptive characteristics?" or, more generally and more colloquially, "How come?" This is another question that is usually inappropriate and does not necessarily elicit scientific answers as regards strictly physical phenomena. In biology it is both appropriate and necessary, and Darwin showed that it can here elicit truly scientific answers, which embody those that go before. The fact that the lion's characteristics are adaptive for lions has caused them to be favored by natural selection, and this in turn has caused them to be embodied in the DNA code of lion heredity. That statement, which of course summarizes a large body of more detailed information and principle, combines answers to all three questions: not only "How?" and "What for?" but also "How come?" Always in biology but not invariably in the physical sciences, a full explanation ultimately involves a historical—that is, an evolutionary—factor.

Here I should briefly clarify a point of possible confusion. Insistence that the study of organisms requires principles additional to those of the physical sciences does not imply a dualistic or vitalistic view of nature. Life, or the particular manifestation of it that we call mind, is not thereby necessarily considered as nonphysical or nonmaterial. It is just that living things have been affected for upward of 2 billion years by historical processes that are in themselves completely material but that do not affect nonliving matter, or at least do not affect it in the same way. Matter that was affected by these processes became, for that reason, living, and matter not so affected remained nonliving. The results of those processes are systems different in kind from any nonliving systems and almost incomparably more complicated. They are not for that reason necessarily any less material or less physical in nature. The point is that *all* known material processes and explanatory principles apply to organisms, while only a limited number of them apply to nonliving systems. And that leads to another point, my final one.

Unity of the Sciences

When science was arising, Francis Bacon insisted that all its branches should be incorporated into one body of fundamental knowledge. Bacon placed this in an Aristotelian framework really inappropriate for modern science; he wrote it at a time when one mind could grasp the essentials, at least, of all the sciences; and he was not himself a practicing scientist. Of course nowadays, as regards detailed knowledge and adequate research ability, there is no such thing as a general scientist, a general biologist, or even a general entomologist. In the practice and teaching of science, specialization and the accompanying fragmentation of the sciences have become absolutely necessary. Yet this practical necessity has not eliminated the force and value of the conception that the universe and all its individual phenomena form one grand unit and that there is such a thing as science, not just a great number of special and separate sciences.

Bacon further maintained that the unity of nature would be demonstrated and the sciences would be incorporated into one general body by a fundamental doctrine, a *Prima Philosophia*, uniting what is common to all the sciences. Despite the great change in philosophical outlook, that has become a traditional approach to the unification of the sciences. In our own days, Einstein and others have sought unification of scientific concepts in the form of principles of increasing generality. The goal is a connected body of theory that might ultimately be *completely* general in the sense of applying to *all* material phenomena.

The goal is certainly a worthy one, and the search for it has been fruitful. Nevertheless, the tendency to think of it as *the* goal of science or *the* basis for unification of the sciences has been unfortunate. It is essentially a search for a least common denominator in science.

It necessarily and purposely omits much the greatest part of science, hence can only falsify the nature of science and can hardly be the best basis for unifying the sciences. I suggest that both the characterization of science as a whole and the unification of the various sciences can be most meaningfully sought in quite the opposite direction, not through principles that apply to all phenomena but through phenomena to which all principles apply. Even in this necessarily summary discussion, I have, I believe, sufficiently indicated what those latter phenomena are: they are the phenomena of life.

Biology, then, is the science that stands at the center of all science. It is the science most directly aimed at science's major goal and most definitive of that goal. And it is here, in the field where all the principles of all the sciences are embodied, that science can truly become unified.

Divergent Reactions to the Threat of War

A peace and a shelter group were studied to examine their different responses to the Berlin crisis.

Paul Ekman, Lester Cohen, Rudolf Moos, Walter Raine, Mary Schlesinger, George Stone

Different proposals for dealing with the threat of war had been offered and discussed but generally aroused little enthusiasm prior to the Berlin crisis. With the intensification of international tension during the summer and early fall of 1961 there was a rapid growth of interest in civil defense measures and a proliferation of groups concerned with peace. The desirability of fallout shelters became a focus of conflict between proponents of these different approaches, and controversy was widespread in Congress, among scientists, and at a community level. Within one homogeneous community these divergent viewpoints were expressed in the nearly simultaneous formation of two groups, one organized to build a fallout shelter, the other to oppose shelters. We studied these groups in order to understand the factors which had led them to adopt such different reactions to the threat of war.

The two groups that we studied were formed within the same suburban upper middle-class community, about 20 miles from San Francisco (*1*). This is a community of about 8000 people who live in new, single-family dwellings, most of them built by a single developer in a contemporary architectural style. The first to form was the Organization for Atomic Survival in Suburbia (OASIS). Its members, who live fairly close to each other within the community, planned to build a private fallout shelter to accommodate a maximum of 100 people. A number of them were also active in promoting a program for construction of community fallout shelters in the public schools. Members of the second group, People for Peace, were originally brought together by their shared opposition to community shelters, but they described themselves as advocates of a "positive" program for peace, not just opponents of shelters.

People for Peace had 28 members and OASIS had 26 at the time of the study. A member was defined as anyone who attended more than one meeting. There were equal numbers of men and women in OASIS; there were twice as many women as men in People for Peace. Demographic data were similar for members of the two groups: most were in their mid-thirties, had more than one child, had at least finished college, and were earning between $10,000 and $15,000 a year. The fact that the two groups were demographically similar, and came from a single small homogeneous community, enhances the significance of our comparison but also limits the extent to which our findings can be considered representative of other groups with similar purposes.

Less than a month after they had formed, these two groups were separately approached by a member of our research team and asked to participate in a research project. The six members of the research team had not worked together before, nor had any of us studied problems in the area of peace and war. We were, and remain, divided in our beliefs regarding civil defense and peace groups. These differences were purposely made explicit, and measurement techniques were arrived at jointly in an attempt to counteract the influence of any one bias. It was not possible, however, to compromise on the appropriate areas of inquiry. Instead, the domains of behavior sampled reflected our diverse hypotheses, stemming from the differing value orientations of the members of the research team. The tests covered attitudes about war and peace, more general opinions, personal characteristics, background and life history, and game and risk-taking behavior. Most of the tests were specifically devised for the study, although some parts were borrowed from other studies (*2*).

A member of the research team observed each meeting of the two groups from October 1961 to February 1962. In the second week of January 1962,

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