28 August 1959, Volume 130, Number 3374 SCIENCE

Explanation and Prediction in Evolutionary Theory

Satisfactory explanation of the past is possible even when prediction of the future is impossible.

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The most important lesson to be learned from evolutionary theory today is a negative one: the theory shows us what scientific explanations need not do. In particular it shows us that one cannot regard explanations as unsatisfactory when they do not contain laws, or when they are not such as to enable the event in question to have been predicted. This conclusion, which is contrary to the usual view of scientific explanation (*1*; *2*, pp. 319–352), has important consequences for research in those subjects in which serious errors are known to arise in the application of the available regularities to individual cases. These subjects include a great part of biology, psychology, anthropology, history, cosmogony, engineering, economics, and quantum physics. I shall refer to such studies as "irregular subjects"; and the thesis of this article is that scientific explanation is perfectly possible in the irregular subjects even when prediction is precluded. One consequence of this view is that the impossibility of a Newtonian revolution in the social sciences, a position which I would maintain on other grounds, is not fatal to their status as sciences (*3*). Another consequence is the reassessment of Darwin's own place in the history of science relative to Newton's.

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Darwin's Importance

We often confuse three criteria in estimating the importance of the great figures in the history of thought. The first is the indispensability of what they wrote or said, regardless of its effect, judged as a stage in the development of our present beliefs. From this point of view, to earn a place in history, a man need only be the first to discover the material or express the idea in question. The second criterion is their effect on other thinkers, and nonthinkers, which, unlike the first, requires publication and recognition (or misinterpretation). The third criterion is the extent of their personal indispensability. To judge this, we must make some estimate of the time that would have elapsed before the same contribution would have been made by others, had the individual under assessment never existed. If we introduce an index of "lucky fame" as the ratio of a man's importance on the second criterion to his importance on the third, it seems very likely that Darwin has the highest index of lucky fame in history. In fact, what is often regarded as his key contribution was formulated by Wallace before Darwin published it. Admittedly, almost the same calamity befell Newton, but only with respect to his gravitational work; his optics, dynamics, and mathematics are each enough to place him in the front rank. Moreover, Darwin's formulations were seriously faulty, and he appears to have believed in what many of his disciples regard as superstition, the inheritance of acquired characteristics and the benevolence of Natural Selection. Of course, Newton believed that some orbital irregularities he could not explain were due to the interference of angels, but he did achieve a large number of mathematically precise and scientifically illuminating deductions from his theory, which is more than can be said of Darwin. Somehow, we feel that Darwin didn't quite have the *class* that Newton had. But I want to suggest that Darwin was operating in a field of a wholly different kind and that he possessed to a very high degree exactly those merits which can benefit such a field. In place of the social scientists' favorite Myth of the Second Coming (of Newton), we should recognize the Reality of the Already-Arrived (Darwin); the paradigm of the explanatory but nonpredictive scientist.

Let us proceed by examining briefly the attempts by Darwin and others to encapsulate the principles of evolution in the form of *universal* laws and base *predictions* on them; and let us contrast their lack of success in these endeavors with the tremendous efficacy of the *explanations* they produced. During this comparison we shall try to extract the formal properties of the two key types of proposition that are associated with explanations in the irregular subjects: one type of proposition is a weaker relative of predictions, and the other type is a weaker relative of laws.

Hypothetical Probability "Predictions"

The suggestion that in evolution we see the "survival of the fittest" has some well-known difficulties. In the first place, the definition of "the fittest" is difficult even when made relative to a particular environment. It is fairly obvious that no characteristics can be identified as contributing to "fitness" in all environments. Thus, strength may increase the chance of fighting so much that it decreases the chance of survival, and intelligence may be antiadaptive in anti-intellectual societies. Furthermore, maximum specialization for a particular environment is in general incompatible, morphologically and genetically, with maximum flexibility to withstand sudden environmental changes (*4*). We are inclined to say that the organisms adopting the former line of development tend to be "fitter" *until* the change occurs, and the latter fitter *when* it occurs. Whatever we say, it is quite clear that we cannot predict which organisms will survive except in so far as we can predict the environmental changes. But we are very poorly equipped to do this with much precision since variations in the sun's output and even interstellar influences have substantial effects, quite apart from the local irregularities of geology and climate. However, these difficulties of prediction do not mean that the idea of fitness as a factor in survival loses all its *explanatory* power. It is not only true but obvious that animals which happen to be able to swim are better fitted for surviving a sudden and unprecedented inundation of their arid habitat, and in some such cases it is just this factor which explains their survival. Naturally we could have said in advance that *if* a flood occurred, they would be *likely* to survive; let us call this a hypothetical probability prediction. But hypothetical predictions do not have any value for actual prediction except in so far as the conditions mentioned in the hypothesis are predictable or experimentally producible: hence there will be cases where we can *explain why* certain animals and plants survived even when we could not have *predicted that* they would. And it is a feature of the irregular subjects that, unlike classical atomic physics, the irregularity-producing factors lie outside their range of observation and are not predictable by reference to any factors within this range (*5*).

It should be noted that these "predictions" are not easily falsified by observation, since they only assert the *likelihood* of a certain outcome. Their cash value is thus very much like that of a promissory note which says, "*If* I ever have enough money, I will *probably* pay you $100," whereas an ordinary prediction is like a check for the sum.

A second kind of difficulty with the "survival of the fittest" principle is that many organisms are killed by factors wholly unconnected with *any* characteristics they possess—for example, they happen to be sitting where a tree or a bomb falls. Of course, this is sometimes due to a habit or property they possess; but that is not always true, since even identical twins with identical habits do not always die together. This really shows that (i) even at the limits of stretching, "the fittest" refers to characteristics of an organism, and spatiotemporal location is *not* such a characteristic (in physics, the study of the "properties of matter" covers elasticity and molecular structure but not location), and (ii), location sometimes determines survival. So it is simply false to suppose that "fitness" *universally* determines survival. Of course, one could go a step further and define "the fittest" as "those which survive"; this is not stretching but breaking the concept, and this step would be fatal to all the scientific claims of the theory. We can get by with a tendency-statement instead of an exact law, because it justifies hypothetical and hence occasionally testable predictions and also explanations, but not with a tautology.

H. Graham Cannon is thus entirely mistaken when he says: "So Darwin pointed out that in the struggle for existence it will be those most fitted to survive who do in fact survive. . . . What are the fittest? Simply those that survive" (*6*). Darwin's discovery was that in the world the way it is (and has been), the fitness of the organism, in a perfectly recognizable but complex sense of "fitness" was very often the *explanation* of its survival. In a world where accidents were extremely frequent and mobility was very low, Darwin could never have supported this claim: there would not be enough correlation between the possession of observably useful characteristics and survival to make it plausible. It was partly because the opposing theory of the time was supernatural that insufficient attention was paid to the difference between Darwin's account and other possible naturalistic accounts of the history of life. If good luck in the avoidance of accidents, rather than fitness, was the dominant theme of that history, Darwinism would have been unimportant. And in it there was still the unexplained existence of variations. But Darwinism, like cosmological theories of continual creation, had the added advantage that it spread the inexplicable element *thin,* thus making it scientifically more palatable than a large lump at the beginning, whether the lump be matter or numbers of species. Darwin's success lay in his empirical, case by case by case, demonstration that recognizable fitness *was* very often associated with survival, and that the small random variations *could* lead to the development of species. He did not discover *an exact universal law* but the utility of *a particular indicator* in looking for explanations.

Survival of a Species

In this Darwin was greatly assisted by a feature of the data which constitutes a third difficulty in the attempt to sum up his account under the formula "survival of the fittest," no matter how *fittest* is defined. This is, of course, the fact that our concern is with survival for thousands of generations, not with survival to adulthood for one; and certain factors enter into, or are absent from, an explanation of the form of the *ultimate descendants* of a certain population, by comparison with an explanation of the form of *the adults* in the original population. In particular, we must add the variations in reproductive efficiency (including mating efficiency, where sexual reproduction is involved) and in parental rearing efficiency, as well as genetic variability, and subtract some considerations that affect postclimacteric survival (*7*). However, this transition to what Simpson calls the "differential reproduction of the best-adapted" does not eliminate the effect of the first and second points above; they apply with undiminished force. The theory in this form can deal with a different and more appropriate task; but it is still not capable of generating more than hypothetical probability predictions since both extensive and local catastrophes will play a large part in determining the survivors, regardless of their characteristics. And in the same way as with the simpler version of the principle, though more efficiently, we shall in retrospect still be able to *explain* many features of the record by reference to the characteristics of the surviving animals and the nature of the environmental changes. But we shall not be able to do this always; for there will still be the cases where a whole population, or that subset of it carrying certain characteristics, will be annihilated in a way that requires and justifies no reference to its adaptiveness, yet makes a substantial difference in the record of life on earth.

At this point one may wish to say that these explanations, too, are part of evolutionary biology. They are certainly part of the history of life on earth, and they are certainly naturalistic explanations. The problem is like that resolved by the great philosopher of history, Collingwood, when he laid it down that the history of man is the history of ideas; we feel that floods and earthquakes have some importance for history, but one can of course discuss their *effects* on man within Collingwood's definition. In our case, we can include such explanations as part of evolutionary biology if we wish, and admit that Darwin's theory and Mendel's additions are not involved or relevant. Or we may omit it and concede that evolutionary theory cannot alone explain the morphology and paleontology that is its field. Which decision we make is not important; but a recognition of the point, however described, is. For we cannot assess Darwin's contribution except by comparing the extent of the domain of his explanations with the domain in which we can and need appeal only to explanations of a kind that Linnaeus (or anyone else who thought the species separately created and by their nature unchanging) would have found perfectly acceptable (*8*).

Considerations Novel to Darwin

When, today, we reach the point where we are discussing a *sequence* of generations in terms of natural selection, we find ourselves faced with a fourth difficulty in any attempt to state exact laws of evolution. It is the first of those we have discussed which involves a consideration wholly novel to Darwin—the idea of random mutations (*9*). Essentially, this is a feature of the theory with a logical character which is the opposite of the catastrophes because the mutations, more or less unpredictably, *add* a new element while the natural accidents unpredictably *subtract* an old one. Again, we can sometimes be sure that the new element is a mutation after it appears—for example, nonalbinism in an albino population. That is, we can explain (in a weak sense here, though with some mutations we can go into details) the phenotypic appearance of an organism by identifying it as a mutation, although we could not have predicted it. Or to be more precise, we might have predicted it, because it does sometimes happen and we might have just had a hunch it was about to crop up. But we cannot give any *rational grounds* for supposing it to be more than a remote possibility that a particular litter from an albino strain will contain a nonalbino, whereas we can be perfectly confident that, when it occurs, it is a mutant, and we can sometimes be confident of the focus of the mutated gene on the chromosome and even of the cause and *modus operandi* of the mutation.

As a fifth and final point, one which does directly contradict one of Darwin's conclusions in the first edition of *The Origin of Species,* we must mention another side of the second point, about "accidental deaths." Just as some organisms and species are exterminated regardless of their characteristics, so some survive despite the handicap of maladaptive (that is, the handicap of antiadaptive and of nonadaptive) characteristics. The "pressure of the environment" is a statistical pressure, and Fisher's proofs of the efficiency of this pressure even on small differences in adaptiveness, being statistical proofs, implicitly allow the possibility that sometimes the unlikely will occur. It is evident from the fossil record that it must have occurred many times, and dynasties have stood when an all-or-none law of selection would have felled them—have stood and have founded a genealogy that would not otherwise have existed. The notion of "random preadaptation," an important explanatory device in neo-Darwinism, relies on just this point. What is true of organisms is true of characteristics, and we have to abandon Darwin's original belief that "every detail of structure in every living creature" has either current or ancestral utility. Not only the mainly nonadaptive form of some antelope horns but some antiadaptive characteristics—either linked genetically with more useful properties, or providing a component for a highly adaptive heterozygote, or by chance alone—will survive for a greater or lesser time, with small and large effects on the course of organic development. The best we can do in the face of such difficulties is to talk of "differential reproduction of the fittest *and the fortunate.*" Yet, here again, as in the case of mutations, we have explanations at hand which have no counterpart in the realm of predictions. We can explain the unlikely outcomes of partially random processes, though we cannot predict them. We are not hard put to explain that a man's death was due to his being struck by an automobile, even when we could not have predicted the event. Now this kind of case does admit of hypothetical probability prediction, but as we shall see, there are cases where not even this sickly relative of ordinary predictions is possible.

The Logic of Predictions and Explanations

It is natural enough that the logic of explanation should appear to parallel that of prediction. Sometimes, in fact, it does. There are specific occasions, particularly in classical physics, when we explain and predict by reference to the same laws. But this is an accident, not a necessity, as it turns out. Put the matter in general logical terms and the similarity still appears to hold: to predict, we need a correlation between present events and future ones—to explain, between present ones and past ones. And who would wish to insist that a difference of tense has any logical significance? As Hempel and Oppenheim say (*2*, pp. 322, 323), "The difference between the two is of a pragmatic character . . . whatever will be said . . . concerning the logical characteristics of explanation or prediction will be applicable to either. . . ." They suggest, plausibly enough, that if we cannot derive the event to be explained from known general laws which connect it with antecedent conditions, we are likely to be deceiving ourselves if, in retrospect, we regard it as explained by reference to those antecedent conditions. And if we can so derive it, then we are in a position to predict it.

Naturally, previous writers on this subject have not overlooked such examples as unpredictable catastrophes being used as the explanation of their consequences. But they have taken the existence of hypothetical probability predictions, which are of course possible in such cases (*10*), to show that the event explained could *in principle* have been predicted. That is, a prediction of the event being explained was possible if we had known, or after we did know, the *catastrophe* was going to occur, but *before* the event. This is a somewhat unhelpful sense of "in principle," since until that day when *everything* is predictable, there remains the fact that we can often explain what we could not predict, and surely this feature should be mirrored in any analysis of these notions. Furthermore, there are good grounds for saying we *cannot even in principle* predict everything (uncertainty principle, classical unpredictability of a computer's state); hence, good grounds for saying that even in principle explanation and prediction do *not* have the same form. Finally, it is not in general possible to list all the exceptions to a claim about, for example, the fatal effects of a lava flow, so we have to leave it in probability form; this has the result of eliminating the very degree of certainty from the prediction that the explanation has, when we find the fossils in the lava. But we can go further; we can show, quite independently, a gross logical difference between the two. (There is a large area of noncausal explanation in the sciences in which the two are completely unrelated, but I confine my remarks to causal explanation.)

For when we get down to some exact cases, we do discover something asymmetrical about the two situations, prediction and explanation. What we are trying to provide when making a prediction is simply a claim that, *at a certain time,* an *event* or state of affairs will occur. In explanation we are looking for a *cause,* an event that not only occurred earlier but stands *in a special relation* to the other event. Roughly speaking, the prediction requires only a correlation, the explanation more. This difference has as one consequence the possibility of making predictions from indicators other than causes—for example, predicting a storm from a sudden drop in the barometric pressure. Clearly we could not say that the drop in pressure in our house caused the storm: it merely presaged it. So we can sometimes predict what we cannot explain. But can we ever explain what we could not have predicted, *even if we had had* the information about the antecedent conditions? That is, can we explain when even hypothetical probability prediction is impossible? This seems less likely, roughly because finding causes is harder than finding correlations. Yet it is possible, and, in some areas of knowledge, common. For sometimes the kind of correlation we need for prediction is absent, but a causal relationship can be identified. Although the point is the same, it may be helpful to take an example from a different field.

Retrospective Causal Analyses

If we discover that certain industrial chemicals, frequent abrasion, and a high level of radiation exposure sometimes cause skin cancer, we are in no way committed to the view that cancer *frequently follows* exposure to these irritants. Among the vocations which involve such exposure, cancer may be very rare (although *substantially more frequent* than in other vocations). It is presumed that some unknown conditions such as hereditary predisposition, low perspiration production, or accidental environment factors are responsible for the difference between those who develop cancer and those who do not. Nevertheless, when a middle-aged fisherman comes in to a clinic, his face and hands black from years of ultraviolet exposure, and a growth on the back of one hand is diagnosed as a small carcinoma, the physician who can discover no evidence for the relevance of other known causal factors is in a very good position to assert that the cause *was* excessive exposure to the sun.

The form of this argument, which is so often used by the evolutionary biologist, the engineer, and the historian among others in the non-Newtonian fields, is quite complicated and is best approached by taking a very simple example first. (This corresponds to the example of the barometer which enables one to predict but not explain a storm.) Here, we can explain but not predict, whenever we have a proposition of the form "The only cause of X is A" (I)—for example, "The only cause of paresis is syphilis." Notice that this is perfectly compatible with the statement that A is often not followed by X—in fact, very few syphilitics develop paresis (*11*). Hence, when A is observed, we can predict that X is *more* likely to occur than without A, but still extremely unlikely. So we must, on the evidence, still predict that it will *not* occur. But if it does, we can appeal to (I) to provide and guarantee our explanation. Naturally there are further questions we would like answered if we are research scientists, such as what the particular conditions are that, in this case, combined with A to bring about X. But the giving of causes, and of scientific explanations and descriptions in general, is not the giving of "complete" accounts; it is the giving of useful and enlightening partial accounts. In fact, even the "complete" account merely includes some extra relevant factors—it simply generates even more puzzling questions as to why the *whole* set of factors is sufficient. The search for a *really* complete account is never-ending, but the search for causes is often *entirely* successful, and someone who saw a man killed by an automobile but refused to accept the coroner's statement that this was the cause of death on the grounds that some people survive being hit by a car, does not understand the term *cause.* The coroner is perfectly correct, even though other factors are involved.

Turning to the more general form of the argument, where several causes of X are known, we see that it has the following form:

1) Conditions or events $A, A', A'', \ldots$ sometimes cause X (for example, prolonged sunburn or skin abrasion or some other factor sometimes causes skin cancer).

2) There are some unknown causes of X, but the majority of those cases of X which are preceded by A or A' or . . . are caused by that A.

3) The incidence of X in the population of A's is very small (for example, only a few people in groups receiving the same amount of sun develop skin cancer).

4) A particular individual i is known to have met the condition A, but not the conditions $A', A'' \ldots$ (for example, i has had as much sun as is needed to produce cancer in some people).

From these premises, the only prediction we can make about i is that he will not develop X. Suppose now that:

5) i develops X.

We may now deduce that the cause of this was probably (and sometimes certainly) A. Hence an event which cannot be predicted from a certain set of well-confirmed propositions can, if it occurs, be explained by appeal to them, and there is no "in principle" possibility of predicting 5 from 1 to 4. It is of course true, and trivial, that other data might enable one to predict 5. But I have only wished to argue that the kind of knowledge we do have about evolution enables us to provide well-justified and informative explanations, without predictions.

To go one step further, it is probably not possible to list all the known causes for an evolutionary event such as the extinction of a species; but we do not need to, as long as we can *recognize* them with some reliability. When they are present, we can still identify the causes of events *after* they happen, without committing the fallacy of *post hoc ergo propter hoc,* which the requirement of predictability-in-principle was designed to avoid.

Careless use of such arguments does produce *ad hoc* explanations; but it is an error to conclude that in general such arguments are vacuous, as do those who think the theory of evolution wholly empty, and thus capable of "explaining" anything. Cannon says, "forty years ago, it appeared to me that orthodox Mendelism . . . was capable of explaining *any* genetical result" (*6,* p. 83); and he regards neo-Mendelism as even more "omnipotent." But he mistakes the explanatory fertility of a theory for explanatory omnipotence—that is, vacuity. If we find a markedly nonbinomial distribution of characteristics in each generation of descendants from a genotypically well-identified pair, we cannot explain this by merely mentioning some *possible* cause. We have to show, as in the cancer case, that (i) this cause was in fact present, (ii) *independent* evidence supports the claim that it can produce this effect, and (iii) no other such causes were present. That this can be done is the mark, and a well-earned mark, of success: in this case, of Mendelism, and in more general cases, of evolutionary theory.

Notice that we do not have to be able to give a law of the usual form of classical physics, a universal functional relationship, let alone a mathematical one. Indeed I prefer to avoid using the term "law" of propositions like 1; they are, logically speaking, particular and not universal hypotheses. However, they can usually be established only by study of a range of cases and hence in some sense might be said to "reflect" a regularity or set of regularities. The logical key to the whole affair is that one can identify a cause without knowing what the conditions are which are necessary for its causal efficacy. When someone says that the explanation of the Irish elk's extinction was the swamping of its habitat, he means that *in the circumstances* this event was sufficient to ensure its extinction, and had this event not occurred, it would have survived. But he would immediately agree that (i) he could not exhaustively specify the circumstances which are essential, although we have in mind the terrain and climate and the animal's weight, hoof size, predators, reproductive habits, and so on; and (ii) there are other possible causes (for example, an invasion of Arctic wolves) which, had *they* been present, would have led to the same effect in circumstances which were in every respect the same except for their presence and the absence of flooding. A more complex but basically similar analysis is required for other cases—for example, the explanation of man's uniqueness, among the bipedal mammals, in running rather than leaping like the kangaroo, in terms of his arboreal ancestors (*12*). These cases illustrate the weakness of talking about "applying a universal law" in order to explain; if you have one, it may be helpful, but if you do not, you may still know a good deal about the possible and actual causes of the events you are studying. Without the universal law, it is not possible to make predictions. The elks might have survived that degree of flooding for all we could produce in the way of laws to the contrary; but if they did not, and nothing else changed, we can reasonably conclude that the explanation is the flooding.

It is not surprising, therefore, that when we turn to the attempts of Darwin and the Mendelians to formulate some laws of the traditional kind, or to make predictions, we find the results to be very unsatisfactory. As Waddington says, even the modern attempt to develop a mathematical approach to evolution has not "led to any noteworthy quantitative statements about evolution. . . . The formulae involve parameters . . . most of which are still too inaccurately known to enable quantitative predictions to be made or verified" (*13*). And if this is the case for the mathematical theory, the case is much worse for exact statements which do not involve the flexibility of mathematical relationships. What can be said is well expressed by Darwin in his autobiography, where he says that *when* there is a struggle for existence, "favorable variations would *tend* to be preserved, and unfavorable ones to be destroyed." Tendency statements like this are explanation-indicators; they justify no more than very weak hypothetical predictions with unspecified conditions ("*if* everything else was the same, then . . ."), for they tell us nothing about the likelihood of conditions of struggle or the strength of the tendency. Perhaps the best way to express their empirical content is to say that they suggest that certain future states of affairs are very *unlikely*—namely, equilibrium of a mixed population *when* there is competition for survival. Indeed Darwin too readily concludes from Malthus' argument that "a struggle for existence inevitably follows," or again, "there must in every case be a struggle for existence, either one individual with another of the same species, or with the individuals of distinct species, or with the physical conditions of life" (*14*). The legitimate conclusion must contain the qualifying terms "eventually" and "*ceteris paribus,*" and a less definite basis for purposes of prediction would be hard to find.

But when we have only such statements, we have a great deal, though we lack much. Often it will be beyond the capacity of a particular subject, such as evolutionary biology or molar psychology, to provide more than this, especially when dealing with past events. Darwin's greatness lay in the use to which he put such statements in explanation, and as he says in the last chapter of *The Origin of Species,* "It can hardly be supposed that a false theory would explain, in so satisfactory a manner as does the theory of natural selection, the several large classes of facts above specified." His work indeed showed that the theory was not *false.* I hope that this study may make clearer why it is not *trivial,* although its principles cannot be precisely formulated, and although it is not committed to any predictions about the future course of evolution, despite Darwin's hopeful voice on the last page of his great work: "We may feel certain that the ordinary succession by generation has never once been broken, and that no cataclysm has desolated the whole world. Hence we may look with some confidence to a serene future of great length." I wish that the great strength of his theory did indeed justify such a prediction. But I fear it is only committed to the view that *if* the struggle for existence continues, the forms of life will *probably* change. Its great commitment and its profound illumination are to be found in its application to the lengthening past, not the distant future: in the tasks of explanation, not in those of prediction.

References and Notes

1. See, for example, R. B. Braithwaite, *Scientific Explanation* (Cambridge Univ. Press, New York, 1953); K. R. Popper, *The Logic of Scientific Discovery* (Hutchinson, London, 1959), p. 59.
2. C. G. Hempel and P. Oppenheim, "The logic of explanation," in H. Feigl and M. Brodbeck, Eds. *Readings in the Philosophy of Science* (Appleton-Century-Crofts, New York, 1953).
3. The grounds are roughly that we already know of crucial variables that are not within

the observational range that defines the science. (See "A possible distinction between traditional scientific disciplines and the study of human behavior," in *Minnesota Studies in the Philosophy of Science,* vol. 1, *The Foundations of Science and the Concepts of Psychology and Psychoanalysis,* H. Feigl and M. Scriven, Eds. (Univ. of Minnesota Press, Minneapolis, 1956).
4. The best advanced discussion of "fitness" with which I am acquainted is J. M. Thoday, "Components of fitness," in "Evolution," *Symposia Soc. Exptl. Biol. No. 7 (1953)* (1954).
5. In quantum physics we envisage the further possibility that there are no such factors, only the irregularity in the individual events, but we have the partial compensation of some statistical regularities. These are in some respects more informative than the nonquantitative probability and tendency statements of psychotherapy, personality theory, psephology, and so on.
6. H. G. Cannon, *The Evolution of Living Things* (Manchester Univ. Press, Manchester, England, 1958).
7. But senile adults may have properties of evolutionary interest—for example, in a gregarious society, especially a gerontocracy. It might seem that we can then include them as environmental conditions for the prospective parents, but this is inadequate (an example is the well-known case of the worker bees). A case where senile maladaptiveness is irrelevant is that of the coiled oyster.
8. The problem of accounting for, for example, the departure of the dinosaurs did not in fact arise until 34 years after Linnaeus' death, with Cuvier's work; but it is too commonly assumed that nonevolutionists would have *had* to assert, as they usually did with the few fossils of extinct forms recognized in the 18th century, that the animals still existed in some as-yet-unexplored part of the globe. They could also have said that a catastrophe that indiscriminately annihilated the life forms in some area was responsible—that is, one of the catastrophes discussed in the second point above. This involves no commitment to evolution.
9. Darwin believed in unpredictable variation, of course, but the several genetic origins of this were not understood by him, nor for that matter were they clear to Mendel.
10. For example, "If there is a volcanic eruption which produces a vast lava stream, then organisms in its path will probably be destroyed."
11. People have sometimes argued that if *A really* is the cause of *X*, it must *always* be followed by *X*. This is to confuse causes with sufficient conditions, and practically to abolish them from the applied sciences, since there are almost no absolutely reliable statements of sufficient conditions available there. Causes are not necessary conditions either; their logical nature is complex, though there is relatively little difficulty in using the term "cause" correctly—a situation which characterizes other fundamental terms in science, such as "probability," "truth," "explanation," "observation," "science," and "simplicity."
12. J. Maynard Smith, *The Theory of Evolution* (Penguin, Baltimore, Md., 1958), p. 245.
13. C. D. Waddington, "Epigenetics and evolution," in "Evolution," *Symposia Soc. Exptl. Biol. No. 7 (1953)* (1954).
14. C. R. Darwin, *The Origin of Species;* these and the preceding passages are quoted by Flew in his illuminating essay "The Structure of Darwinism," in *New Biology* (Penguin, Baltimore, Md., 1959).

Further Reading

The outstanding work on the logical problems of biology, and, in my view, an extremely important book, is Morton Beckner's *The Biological Way of Thought* (Columbia Univ. Press, New York, 1959).

Russian-English Transliteration

An exchange of views on this problem shows that a universally accepted solution is not yet at hand.

Comment by Hamp

The article by Gregory Razran [*Science* 129, 1111 (1959)] on the transliteration of Russian draws welcome attention to our inconsistent practice in a matter where we could readily do better. I can only applaud Razran's sensible attitude and second his call for improvement. I think, however, that we can clarify the problem further, and understand in some measure the present confusion, by raising a point of principle which Razran does not touch.

It has taken a fair part of the last half century for workers in linguistics to appreciate clearly the fundamental distinction that must be drawn between speech and writing. A glance at any of the modern textbooks on linguistics will amply illustrate this. Linguists are still all too conscious of the fact that the purport of this finding has in many respects not yet been brought home to the literate public at large, which includes their fellow scientists.

For our present purpose, this distinction means that the graphic system used in a particular culture area (a specific subtype of Cyrillic, in this case) is not identical with the phonemic system of a particular language (Russian in this instance). Indeed, the two can be analyzed quite independently. Only in rare instances are the two systems nearly congruent (Finnish is such a case), so that the distinction may be ignored altogether. In addition, we must remember that there are, too, the graphs of the target culture (a subtype of West European Roman, in our own case) and the phonemes of the target language (American English for us). There are, then, four separate systems in play, whose useful combinations we must now consider.

No one, presumably, is interested in matching Cyrillic graphs directly with English phonemes—that is, devising an arbitrary way for reading off a line of printed Russian with a thoroughly English accent. We get that result without strain from the less apt students in a Russian class. (The question is not idle, however, in principle; Egyptologists must decide how to cite forms intelligibly to one another aloud, even though they can scarcely guess at all what a large portion of the language sounded like.) Similarly, we have no immediate use for Roman letters with a Russian accent, unless perhaps we are training actors. There is very great use for comparison of Russian phonemes with English phonemes; that is what a linguist must consider in designing adequate and efficient teaching materials—both for Russians and for Americans. Finally, there is the problem of matching Cyrillic graphs with our Roman graphs; we will call this task "transliteration," *sensu stricto*.

At one point (page 1111), Razran says: "The rationale of the practice is presumably that of facilitating library cataloging and filing by indicating that the English combinations of letters correspond to single Russian letters. But, plainly, this limited and doubtful advantage must be pitted against the fact that ligatures and extra capitals are both expensive and unesthetic, add nothing from the standpoint of approximate pronunciation, and, indeed, have hardly ever been maintained consistently." Consistency is something which, like Razran, we all hope for, but which the linguistic engineer cannot enforce. Expense and esthetics are problems apart, and we must consider them judiciously in turn. But the "limited and doubtful advantage" of unambiguous transliteration is a matter of considerable concern to a