now given, do for mathematics courses.

If what has been said about the need for such a course seems reasonable, then we should proceed to make such a unified presentation. There remains a problem as to just what to call such a course. I submit that an appreciable part of such teaching has been given for some years at various schools under the name of "dielectrics" courses. It therefore seems reasonable, although perhaps not entirely unobjectionable, to suggest that the name "electromagnetics of matter" be used for the broader unified discipline which includes the study of the interaction of matter with electric and magnetic fields. This would release the restrictions upon the study of dielectrics which previously held—restrictions which precluded study of the effects of other than "adiabatic" electrical polarization and dipolar influences. This release of the restrictions would be an advantage, for we now know that electronic processes such as conduction, incipient corona, avalanching, and ionization play major roles in real materials, roles that cannot be incorporated in the older, classical considerations.

The phenomena of polarization, excitation, conduction, and magnetization would be the essence of the newer, unified discipline.

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On Cause and Effect in Biology

In the thoughtful article on cause and effect in biology [*Science* **134**, 1501 (1961)], Ernst Mayr mentions some difficulties presented by the classical concept of final cause. He acknowledges that this concept was introduced by Aristotle in order to explain the goal-directed activities of organisms and to account for the over-all harmony of the world. However, the definition of final cause which Mayr quotes and seems to accept—namely, "the cause responsible for the orderly reaching of a preconceived ultimate goal"—is not easily harmonized with the letter of Aristotle or with his natural realism.

Aristotle frequently refers to the final cause as that for which or for the sake of which something is made or done. He defines the final cause as the goal of action and says that it is recognizable as such when we see that it is regularly

attained by either natural or purposive agents and is something worth acting for—that is, something better than what was before the action took place. According to Aristotle, the cause at work for the orderly reaching of a goal is the efficient cause, not the final cause. In order to attain a goal regularly, not by accident, the efficient cause must be oriented toward the goal and act for it, but it need not have any knowledge of the goal. Man is the only natural being capable of acting for a preconceived goal which he understands as such and freely chooses to pursue. The activities most characteristic of man are purposive in this sense, but man performs many other activities which are naturally determined, not freely chosen, some of which require sense knowledge for direction to the goal, as in maintaining balance, while others, such as digestion, do not require knowledge of the goal.

Goal-directed action is more manifest and more marvelous in living organisms than in the chemical elements and compounds, but even these tend to preserve and protect themselves and to promote the general harmony of the world. Organisms are composed of elements and compounds united in a very complex and orderly system, and many of their goal-directed activities, as Mayr points out, have an analyzable physicochemical basis. However, it is by no means clear that physicochemical activity is purely mechanistic, and much less clear that the behavior of an organism is mechanistic. We understand machines better than organisms, and we know that a machine is not a natural unit but an artifact made from suitable but not specifically determined materials joined together by the maker working from without and aimed or directed to his goal. On the other hand, an organism is a natural and primary whole, not a mere aggregate, with complex but highly specific composition of parts joined and unified from within, and possessing an active nature by which it preserves and perfects itself and reproduces its kind. Mayr uses the word *individual* in such a broad sense that he includes both an organism and a machine, as if there were no essential differences, and then concludes that the purposiveness of the organism is purely mechanistic.

How organisms came to be as they are is a very interesting question. Mayr notes that the Aristotelians and their successors asked themselves what goal-directed process could have produced such a well-ordered design in nature.

It is no secret that they interpreted the evidence as pointing ultimately to divine wisdom. Human art does not suffice to produce a natural thing, nor does any man know exactly how natural things are produced now, or how new species were produced in the past. We are gradually learning more about natural processes, as Mayr's article shows, but he does not enlighten us very much by appealing to natural selection and excluding "Nature" or "God," who created a superior design or plan. Evolutionary adaptation may not require a special divine intervention, but it does seem to require more than chance variation and fortuitous preservation of the fittest.

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Ernst Mayr emphasizes that the phenomena of development and evolution, which were always the domain of teleologic concepts, are actually "teleonomic." This term is supposed to cover purposiveness programmed by a preformed code of information contained in DNA. Since it seems that this code was formed simply by a long historic process of an interplay between chance events (in the form of mutations) and natural selection, the "purposiveness" programmed by this code and appearing on the level of an individual is purely mechanistic. In conformity with the opinion now prevailing among biologists that there is no evidence for "plan and design" in nature, Mayr rejects any form of Aristotelian teleology as it appeared in the nonscientific ideologies of vitalism and finalism of Driesch, Bergson, and Lecomte du Noüy.

Without entering into a discussion of the philosophical implications of this view, I would like to point out, from the sense of historical justice, that vitalism and finalism were not so unscientific as one is led to believe. When Driesch, Bergson, or Lecomte du Noüy spoke about entelechy, élan vital, or telefinalism, they wanted to designate by one word an unknown x that presides over biological processes, unable as they were to convince themselves that mechanistic physics is a sufficient basis for a biological theory. It was not realized at that time how well suited the language of information theory is to formulation of the problems of biology. Had, for instance, Driesch realized that, from the viewpoint of the

second law of thermodynamics, biological phenomena are distinguished by a high degree of improbability, and had he known that thermodynamics was to be made the backbone of information theory of virtue of Szilard's recognition of the correlation between information and entropy, he could then have spoken about "negative entropy" instead of "entelechy" and would thus have avoided being accused of autonomizing biology. Of course, even then Driesch would probably have spoken about entelechy, Bergson, about élan vital, and Lecomte du Noüy, about telefinalism simply to emphasize that the manner by which biological open systems increase their information content is largely unknown. These men would probably have pointed out that there must be some unknown factor involved in the processes of development and evolution, of the type postulated, for example, by Prigogine and Wiame [*Experientia* **2**, 451 (1946)], which would come close to what they meant by those specific terms they chose for the unknown x. In any case, it was mainly because Driesch, Bergson, and Lecomte du Noüy were not dogmatists, because they looked upon the phenomena of life as something which can be gradually revealed in a never-ending inquiry rather than by a simple reduction to the physics of the day, that they subscribed to vitalism and finalism. This, to me, seems to be the crucial point.

The word *teleonomy*, suggested by Pittendrigh and used by Mayr, was coined on the opposite grounds—those of a dogma, which, moreover, rests upon a mere hypothesis. It is an established fact that development is programmed by an inherited code. But the assumption that this code is only a result of the interaction between the chance events of mutation and natural selection is an unproved hypothesis. It is dangerous to accept this hypothesis as a dogma only because, at the present time, we have no idea about the real mechanism. It means closing the door where it should have been left open. It is true that, at the present time, the only safe abode for theories of evolution is in Darwinism, but one should not assign to this abode more territory than it can cover. With all due recognition of the greatness of Darwin's achievement, we cannot remain blind to the fact that not a single step in the evolutionary mechanism has been clarified. Evolution means primarily an increase in the content of information in the code of DNA, but natural selection

means only the elimination of an error in information, or mutation (in the most favorable case, only a modification of information), not an increase in the quantity of information. Correcting a misspelled word or substituting one word for another one is, after all, something quite different from writing down a sentence, an article, a whole book.

For these reasons it seems to me that a teleonomic concept of biological end-directed processes in development and evolution is not any more "scientific" than a teleologic concept. It is only more dogmatic. The use of mechanical models for the representation of biological problems had in the past a rather comical effect on the thinking of a certain segment of scientific circles. When the great successes of mechanistic physics became obvious, concepts of free will and purpose disappeared from the writing and the theoretical (but not the practical) thinking of biologists. It was only when engineers constructed devices with prefabricated purposeful behavior in the form of computing machines, electronic brains, and so on, that the same scientific public felt that it may be, after all, legitimate to speak again about purposeful behavior, provided that we are machines with the information code built in by shuffling and reshuffling and by natural selection.

Of course, a man-made machine can be of excellent pedagogical value for demonstrating a point in physiology, but to expect that one can derive the principles of biological causality from these man-made models rather than from the study of biological systems themselves may seem rather far-fetched to those who are not dogmatically set upon mechanical models. The following proposal may illustrate the situation: When the biologist borrowed from the engineer the intellectual capital for the construction of his (the biologist's) theory, he might as well have repaid the engineer by suggesting that the latter construct the machines by a simple process invented by the biologist—namely, by shuffling and reshuffling and by selection—and then await the engineer's answer.

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The very interesting article by Mayr (*1*) seems to call for some comment. Mayr distinguishes between an unacceptable and an acceptable form of finalistic explanation; the former is teleological or vitalistic, the latter "teleonomic," or, in the phrase I have used, "quasi-finalistic." But having made this distinction between types of hypothesis, Mayr proceeds to assert that there is a corresponding cleavage between types of phenomena. "The development or behavior of an individual," he writes, "is purposive" [that is, in the acceptable sense], natural selection is definitely not"; and by "is not" he seems to mean "cannot be," since in another place he writes, "Historical processes, however, can *not* act purposefully." I have for some years been urging that quasi-finalistic types of explanation are called for in the theory of evolution as well as in that of development (*2*).

If any process is set going (for example, if two chemical substances are allowed to start reacting with one another) it will eventually reach some end. The question of "finalism" arises when there is something interesting about the end—in particular, when it is both complex and definite in character. We then have three main types of explanation available: (i) that the end itself acts as a cause, directing the process so that it terminates at the pre-

determined end state; this is Aristotelian finalism, which we reject because it involves a concept of causation quite outside our accepted range of ideas; (ii) that some nonmaterial agency directs the process to the predetermined end; this is "vitalism," which we also reject; (iii) that the end state of the process is determined by its properties at the beginning; this is "mechanism," and our recent experience of such mechanical systems as computers has led us to realize that it is a more powerful type of hypothesis than it had previously appeared to be. We can set up a process in such a way that it will reach an assigned end state by building into the initial situation a set of conditions which act as a "programme," and by providing suitable negative-feedback relations to bring the process back onto the right course if it should diverge from it. Conversely, if we find any process to be characterized by a programme and feedbacks, we can deduce that it will proceed towards some end which should in principle be ascertainable from the nature of the programme and the feedbacks (the degree of precision with which the end is determined will depend, of course, on the particular characteristics of the programme and feedbacks).

Mayr accepts the theory (*3*) that ontogenetic development depends on a quasi-finalistic mechanism of this type, the programme and feedback relations both being incorporated in the genotype which has been moulded by natural selection. But there is nothing in the nature of such quasi-finalistic mechanisms which makes it impossible to suppose that the evolutionary process itself is also of this kind. It is obviously characterized by a programme, that involved in the theorem of natural selection. This in itself suffices to determine, to a certain degree, the nature of the end towards which evolution will proceed; it must result in an increase in the efficiency of the biosystem as a whole in finding ways of reproducing itself. And there are, surely, many feedback relations which will serve to determine ends in a more precise fashion. The two to which I have previously directed attention are (i) that involved in the fact that an organism's behavior influences the nature of selection pressures which will operate on it (loosely, an animal selects its environment before its environment selects it), and (ii) that arising from the fact that selection of previous generations for stability or flexibility of development will influence the type of

phenotypic effect likely to be produced by new mutation. There are certainly many others. For instance, increasing phenotypic diversification of a population to fit it to deal with different habitats will eventually be counterbalanced by the development of barriers to interbreeding of the different varieties.

It seems to me that it is becoming inadequate to point out, as Mayr does, that natural selection is not purposive. In itself it is of course no more purposive than is the process of formation of interatomic chemical bonds. But just as the latter process is the basic mechanism underlying the protein syntheses which are integrated into the quasi-finalistic mechanism of embryonic development, so natural selection is the basic mechanism of another type of quasi-finalistic mechanism, that of evolution. The need at the present time is to use our newly won insights into the nature of quasi-finalistic mechanisms to deepen our understanding of evolutionary processes.

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References

1. E. Mayr, *Science* **134**, 1501 (1961).
2. C. H. Waddington, *The Strategy of the Genes* (Allen and Unwin, London, 1957); *The Nature of Life* (Allen and Unwin, London; Atheneum, New York, 1961).
3. ———, *Organisers and Genes* (Cambridge Univ. Press, Cambridge, England, 1940).

Father Kane would have a valid point if he were right in his belief that the "preservation of the fittest" was purely "fortuitous." The evidence is, of course, all opposed to this assumption. Natural selection is merely another word for differential reproduction, and the probability of successful reproduction is not strictly a matter of luck but is correlated with properties of the genotype. Both the observational and the experimental evidence for this is quite overwhelming. Naturally, reproductive superiority is true only in a statistical sense, and this is what makes the understanding of natural selection so difficult for one who is used to typological thinking. This is why the term *survival of the fittest* is so inappropriate and not used by modern evolutionists.

Evolution is a two-stage phenomenon. The first stage is the production of an enormous variety of different genotypes through mutation, recombination, and so on, and at this stage accident is rampant. The second stage, the selection of those genotypes that give rise

to the next generation, is to a large extent controlled by the quality of these genotypes. Genetic recombination puts together in each generation unique novel gene combinations which in turn are exposed to natural selection in the next generation. This is why geneticists like Muller and Fisher have emphasized the remarkable capacity of natural selection to make the highly improbable in the course of time a certainty, and this is why evolutionists like Simpson and Dobzhansky have quite rightly used the epithet "creative" for natural selection. Nothing is known, in the living universe, the evolutionary interpretation of which is inconsistent with the theory of natural selection. As a matter of fact, natural selection is not even a theory but, as differential reproduction, an easily demonstrable universal phenomenon.

I plead guilty to a less than precise definition of Aristotle's "final cause." But then Aristotle's own ambivalence makes such a definition a tough task. Aristotle used *final cause* both for the completed result of a process (obviously not a cause in the modern sense nor even strictly an *aition* in Aristotle's sense) and for the goal which was incipient in the process from its beginning. The latter meaning dominated medieval philosophy, as I see it, and has dominated finalistic ideologies ever since. It was the object of my discussion to show that there is a fundamental difference between those seemingly goal-directed processes that are based on a built-in program (teleonomic) and those that are not (teleological).

Fiala, by stating that I had labeled the phenomena of development *and evolution* teleonomic, by saying that "not a single step in the evolutionary mechanism has been clarified," and by implying that the biologist has derived his ideas on causation from a study of machines rather than of biological systems, reveals a mode of thinking so far removed from my own that we cannot, in the Letters column of *Science*, achieve a common basis for discussion. It is true that Driesch and other vitalists of his day had been provoked by exceedingly naive attempts to describe biological processes in grossly mechanistic terms. Modern vitalists cannot make this accusation against modern biologists. Indeed, one of the conclusions of my recent article was that the simplifications of classical mechanics were not applicable to biology.

In the case of the points raised by Waddington, it is evident that there is no disagreement as to facts, nor even a disagreement as to emphasis, but merely one of classification. Ultimately, the difference of opinion between Waddington and myself boils down to the question of what is deterministic. Waddington stresses the unquestioned fact that the complete interdependence of the gene-physiological and developmental processes sets severe limits to the evolutionary potential. No evolutionist would expect a line of tetrapods to develop suddenly a six-legged side line. A line of terrestrial animals that has become aquatic or has acquired the power of flight will go its merry way until it has either reached near-perfection in the adaptation to this new adaptive zone, or invaded a new zone, or become extinct. No good biologist would deny the pseudofinalistic nature of such processes. It was precisely observations such as these which led earlier biologists to sponsor theories of orthogenesis. And yet any detailed analysis of a single evolutionary line demonstrates the complete absence of any truly finalistic aspect to these

changes, no matter how regular they appear to be. But evolution is usually very capricious. Evolutionary lines may become stagnant, the trends may change their direction, the rates may accelerate for one organ and slow down for another, and one is forever impressed by the evident opportunism of evolution. When lines split it is quite impossible to predict how similar or different the independent lines will become in due time. In other words, one can often specify for an evolutionary line what it will not do (I would not expect any evolutionary line of cats to develop horns), but one cannot specify for a line what it will do.

Waddington states that if any process is set going, it will eventually reach some end. As far as evolution is concerned, he claims, only three ends are possible. Here I disagree with him. In addition to the three possibilities admitted by Waddington, there is a fourth one—namely, that the end state of the process is determined neither by the end itself nor by a supernatural agency nor by the properties at the beginning but by a general principle (natural selection) interacting successively with materials ever new in every generation.

Perhaps the difference between Waddington and myself is that we define *process* differently. He says "that the end state of the process is determined by its properties at the beginning; this is 'mechanism'." Where is the beginning and the end in a concrete case? Is the development of man from a primitive protozoan such a process? Does the development of man from marine prechordates qualify, or the development of man from primitive anthropoids? As soon as we list such illustrations, it becomes apparent how dangerous it is to say that "the end state of the process is determined by its properties at the beginning." The development of an adult organism from a fertilized or unfertilized egg cell can be considered a single process. For this, all that Waddington says about the quasi-finalistic nature of this process is correct. On the other hand, it seems inescapable to me that the change in an evolutionary line is not such a process —its end is not determined by its beginning and it is not finalistic, nor even quasi-finalistic. To my mind, the similarities between the changes in an evolutionary line and those of a developing individual are pure analogies. Surely there are evolutionary feedbacks, surely the biosystem tends to increase in efficiency (but there are countless ways of doing so), surely behavior plays a great role in evolution, but it seems to me that nothing is gained and much is lost by trying to imply that the changes in an evolving evolutionary line are of the same nature as those in a developing individual. I am sorry that somehow I cannot see how one can designate as "quasi-finalistic" a phenomenon as unpredictable as evolution.

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Resistance to Discovery

The article "Resistance by scientists to scientific discovery" by Bernard Barber [*Science* **134**, 596 (1961)] is provocative. Could it be that such resistance is a proper and desirable function of the scientist? To distinguish between fact and fallacy can be difficult. A scientist's bias toward acceptance or rejection can be influenced by some or all of the factors noted by Barber. It is possible for a correct new development to be introduced with insufficient