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.

ERNST MAYR
Museum of Comparative Zoology,
Harvard College,
Cambridge, Massachusetts

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

or even erroneous "proof." It has taken centuries to provide calculus with a rigorous basis, and some say the job is not yet completely accomplished. Right now there are eminent men on both sides of the Krebiozen contest. Only in the future can we find out whether the worthy doctors are standing against a dangerous fallacy or resisting medical progress.

In connection with the Abel incident cited in Barber's article, let it be said that the problem of the solution of the fifth-degree equation was in the air at the time. It was one of those problems that sounded easy enough to be tackled by incompetent enthusiasts. Gauss had probably received many manuscripts that were not worth the time it would have taken to decipher them. There was no way for him to recognize a genius from a cursory glance at the work of an unknown.

I wonder if investigation would not reveal that there has been more resistance to the ideas that contradicted accepted theory than to those that extended or complemented it.

RUTH M. BALLARD
University of Chicago, Chicago, Illinois

The article by Barber was most interesting. However, I would like to point out that he utilized a common misconception to illustrate his thesis that substantive theories held by scientists prevent their acceptance of new ideas. The illustration used was the rejection of the heliocentric theory of Copernicus by the astronomers of his era because they firmly believed that the earth was motionless and at the center of the universe.

At the time of Copernicus the heliocentric theory could no more be justified than could the geocentric theory. Neither could be chosen as superior on the basis of observations accumulated up to that date. The rejection of the Copernican universe, therefore, did not imply a stubborn and blind clinging to outdated concepts on the part of the comtemporaries of Copernicus. Indeed, it was only after the formulation of the Newtonian synthesis, which presented mechanical reasons for believing the heliocentric theory superior, that this theory began to gain wider acceptance. Both were taught at Harvard and Yale in the 18th century. The first observational evidence from which the motion of the earth could be inferred was not obtained until 1838, when several observers were able to determine annual stellar parallax.

Many new theories are rejected because there is no way in which a firm decision may be made at that particular time, due to lack of evidence in favor of either theory. Either may be satisfactory, and it is only after criticism, discussion, and further investigation have produced more definite evidence that the more adequate theory will survive.

ESTHER B. SPARBERG
Hofstra College, Hempstead, New York

Racial Differences and Witch Hunting

In a recent issue of *Science* (*1*), Santiago Genovés of the University of Mexico discourses at some length concerning a paper of mine published in the *Mankind Quarterly* last year (*2*). Genovés objects to my criticism of Klineberg's chapter "*Race and psychology*," included in the UNESCO publication *The Race Quesion in Modern Science* (ed. 2, 1956). He confuses the issues through bad logic and too much vehemence. What I actually did in my paper was to show, I think conclusively, that the evidence for no race differences presented by Klineberg is far too meager, too ambiguous, and too inconclusive to justify his sweeping assertion that "the scientist knows of no relation between race and psychology." My paper would have been "unscientific racism" (Genovés's term) only if its main purpose had been to support the doctrines of a "master race" or "chosen people." As its aim was simply to point out the flimsy nature of Klineberg's data, it is a legitimate enterprise, unless one considers *any* criticism of equalitarianism to be morally untenable.

Genovés is critical of my view that widespread Negro-white hybridization has in the past led to illiteracy, social and economic backwardness, and degeneracy. He assumes that I condemn all race mixing, which is untrue. Most racial hybrids are viable, and many are successful people, as witness the Hawaiian-Chinese and Japanese-American crosses in Hawaii. But one need go no farther afield than the West Indies, Central America, and parts of South America to be convinced of the bad effects of Negro-white crosses when these are numerous. My concern was