our present knowledge of the structure of DNA. To decide which of these two types of scientists was *primarily* responsible is equivalent to asking whether one kind could have succeeded without the other. To answer this question decisively requires a controlled experiment which obviously cannot be performed, but the only reasonable guess is that Wilkins would have determined the DNA structure in due course, without the participation of hypothesis-destroyers. It is certainly true that molecular structures in general have been determined mostly by the x-ray diffraction method, and mostly without outside help. On the other hand, I doubt whether the double-stranded helix could have been proposed, or possible alternative structures disproved, without the existence of the experimental facts I have cited.

Platt is surely correct that the method of strong inference is the fastest way to arrive at a conclusion once the basic experimental facts are assembled. The bulk of the effort, however, lies in the accumulation of the basic experimental facts, and the major credit should perhaps also go to those who do the accumulating.

CHARLES TANFORD
Department of Biochemistry, Duke University Medical Center, Durham, North Carolina

There is tremendous value in what Platt has to say, particularly regarding the "method-oriented" versus the "problem-oriented" researcher. I share his respect for the recent achievements of "strong inference" in molecular biology. I am disturbed, however, by his statements about and his attitude toward mathematics. The following three sentences are, I think, representative of his viewpoint:

> Equations and measurements are useful when and only when they are related to proof; but proof or disproof comes first and is in fact strongest when it is absolutely convincing without any quantitative measurement. . . . The logical box is coarse but strong. The mathematical box is fine-grained but flimsy.

I have always believed that mathematics is logic in its most condensed and powerful form. The function of mathematics is not just proof, but descriptive, explanatory unification of experimental fact, from which follows prediction of new fact. The mathematical description deepens understanding of the total situation. New thought paths arise from mathematical deduction which would not arise from experiment and inductive, qualitative logic alone.

Platt says that "a theory is not a theory unless it can be disproved." However, even molecular biology, an offspring of "strong inference," sometimes finds itself in this embarrassing position. For example, the concept of the "reading reference frame" from the work of Crick, Barnett, Brenner, and Watts-Tobin [*Nature* **192**, 1227 (1961)] clearly predicts that mutations involving a sequence of altered amino acids in protein will be found. All evidence to date shows that mutations usually involve single amino acid changes; and when multiple changes occur, they are not sequential [A. Tsugita, *J. Mol. Biol.* **5**, 293 (1962)]. If we do not find these sequential amino acid changes, we can always maintain that they will be found in the future. Clearly this theory cannot be disproved. Yet it is a valuable theory.

Platt cites the achievements of Maxwell and Newton as singular and "outside any rule or method." They are singular in magnitude, but not in method. A capable student can be taught these methods just as Platt proposes that we teach "strong inference." We need more biologists with strong mathematical foundations to balance the current destructive view that mathematics is unnecessary in biology since rapid progress can be made without it.

LILA L. GATLIN
Drexel Institute of Technology, Philadelphia 4, Pennsylvania

Platt's is one of the more useful articles I have read recently concerning scientific methodology and thinking. It should be read by all those who are endeavoring to make science their career. I will certainly make use of it in my graduate teaching.

There is one point on which I disagree, however, and that is regarding qualitative versus quantitative science. Certainly a qualitative hypothesis or finding is of initial importance. The application of this finding, however, requires quantitation, an aspect of science which may not then be pursued with enough vigor. Maybe some will not classify this activity as scientific. Nevertheless, science must find utility, and I think its greatest utility comes when natural phenomena can be quantified. In my field, nutrition, the discovery of required vitamins, minerals, and so on is very "exciting" work, but determining the quantitative requirement and factors affecting this requirement then becomes as important as the original finding itself.

R. L. PRESTON
Laboratory of Medical-Veterinary Chemistry, State University, Utrecht, Netherlands

Platt really hits hard in his article. We, the third-rates, what are we to do? Shoot ourselves? Leave our laboratories and join the salesmen and technicians? Or just carry on and hope no one will notice?

What strikes me as being the central issue is that scientific endeavor should stick to the point. Results are achieved when each experiment is based on the one before and leads to the one ahead. The connection does not necessarily have to be a hypothesis. It can also be simple extrapolation without any a priori explanations—as is the case, for instance, in the statistical method developed by Box and Wilson for determining optimum conditions for a given process.

We cannot all be a Niels Bohr or a Francis Crick. We cannot all head for the Nobel Prize. Besides, physics and molecular biology are relatively simple subjects. What about biology in general? Who knows enough of all the many variables involved in a biological process to make a well-founded hypothesis? And if a hypothesis is not well founded it is worse than nothing, since it also narrows the horizon.

I suggest we react to the kick-in-the-pants we have been awarded by telling ourselves, again and again, that any deduction from an established fact is better than a fancy idea. Whether we use hypotheses or extrapolations can depend on our abilities. Dear John R. Platt, let's compromise.

HEINZ HANSEN
Danish Atomic Energy Commission Research Establishment, Risö, Denmark

New Ideas: Law Suits and Other Inhibitors

Munster and Smith ("Savants, sandwiches, and space suits," 18 Sept., p. 1276) discuss the legal problems surrounding the preservation of trade secrets to the organizations employing the persons who originate the knowledge. A recent article in *For-*

tune ("Who owns what's in your head?") also discusses an aspect of these problems at some length (*1*). They appear to be important. It seems to me, however, that the core of the matter is being overlooked—that is, the question of the continuity of the supply of ideas. If the supply should cease, the problems would disappear.

Munster and Smith say, "The greater number of patented or protected items are the result of coordinated research in great laboratories." This is debatable. While Seymour Melman has written, "The [modern] conditions of interdependence in inquiry render the concept of the inventor obsolete to a considerable extent" (*2*), Admiral Rickover, on the other hand, says that "Nothing is created by a team or an organization. Every new idea comes out of a single human mind" (*3*), and Edwin Land that "There is no such thing as group originality or group creativity or group perspicacity" (*4*). In a report on "Group influence on creativity in mathematics," the authors conclude that "the contribution of the group has been overly emphasized. In none of the five research studies completed did the group factor make any contribution to problem solving. On the contrary, there seems to be a consistent, if slight, advantage to solving problems alone" (*5*).

Current U.S production of significant scientific publications and patented inventions fails to support any optimism about the effectiveness of our massive group efforts. The total annual issue of patents now is no more than it was in 1930, or even in 1915. In terms of patents per unit population, it was less in 1960 than in 1870. The number of U.S. patents per unit of money spent on technological effort was about 90 times less in 1960 than in 1930. A sampling of data on large defense contracts has shown that roughly $8 million was spent for each patent that arose from that employment. A sampling of papers listed in *Science Abstracts, Section A* (*Physics*) indicates that the number of papers of American origin has declined from about 9300 per billion dollars spent on R&D in 1920 to 213 per billion in 1960 (*6*).

It is popularly assumed that to get any given task performed it is only necessary to hire people to do it. But more sophisticated experience shows, I think, that rational individuals tend to balance the rewards of an endeavor against the risks, and to act so as to maximize the benefit. Thus if an act of a rather special nature, such as producing an invention, gets no recognition, it is not likely to be performed again. And if such an act is believed to threaten awkward and unfamiliar problems and penalties for the individual, such as law suits, the individual may logically decide to avoid it.

We need more study of the relation between scientific creativity and the sociological and economic factors affecting it. We risk being naive when we assume that a given expenditure of money will produce a corresponding value in new ideas. It may well produce none.

LAWRENCE FLEMING
285 South Holliston Avenue, Pasadena, California

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Submarine Basalt: A Correction

Enrico Bonatti has pointed out to us that the two photographs of submarine basalt shown in our recent article [*Science* **146**, 477 (1964)] have been erroneously located. The photograph shown in Fig. 3 (p. 481) actually was taken at latitude 18°30′S, longitude 126°30′W, the station designated D5 on our map (Fig. 2, p. 479). The depth of water in Fig. 3 is about 3200 meters. This corrected position is on the west flank of the East Pacific Rise.

The photograph of pillowed basalts shown on the cover of the issue containing our article was taken at ship's station 57, latitude 18°45′S, longitude 141°00′W. The depth of water is approximately 2100 meters. This locality is on the flank of a seamount on the south side of the Tuamotu Ridge, which projects northwest from the Rise, as shown in our Fig. 2. No volcanic rock was recovered from station 57, and Bonatti raises the question whether under these circumstances the pillowed basalt should be described as oceanic tholeiite. The exact composition of this pillowed basalt cannot be told from the photograph.

The mismatch of photographs and dredge sites is in part due to the fact that separate numbers are given to designate ship's station, camera station, and dredge station, and these separate numbers are employed in the classification of rocks and photographs.

We wish to thank Bonatti, a member of the scientific staff involved in these operations, for his help in correcting this error.

A. E. J. ENGEL
University of California at San Diego, P.O. Box 109, La Jolla

Undergraduate Training

While S. G. Bradley (Letters, 6 Nov., p. 718) complains that undergraduate majors in microbiology are not trained as technicians, I am surprised to learn that undergraduates can major in anything more specialized than biology. When he referred three times to undergraduate "training," he should have mentioned "education" at least once.

Two other sources in the same issue bear me out. One is A. J. Sharp's article (p. 745) on "The compleat botanist." The other is an advertisement (p. 844) in which the prerequisites for employment in a certain consulting firm are said to include "the ability to apply critical perception to unusual problems, as well as a strong interest in meanings and relationships and an eye for both the theoretical and the practical." These abilities are prerequisite to all significant work in science, and while they may depend on inborn traits, I think they are not themselves inborn, but are educed in college.

Traditionally in our culture the bachelor's degree marks both the end of supervised general education and an opportunity to change one's major field. After this comes professional training. The college curriculum is the student's last chance to be taught anything outside his professional field, and his last chance to learn how to choose his professional field. It is therefore a waste of time and a disservice to the student to teach in college the technical skills of any single profession.

MARTIN BRILLIANT
Booz-Allen Applied Research, Inc., Fort Leavenworth, Kansas