Letters

Detonation-Wave Phenomena

Study of detonation-wave phenomena in high explosives is a necessary adjunct to the understanding of explosive effects and their practical applications. The phenomena associated with a single detonation wave are complex; the collision of several may be spectacular. The sequence of ultra-high speed photographs on the front cover of this issue of *Science* shows the collision of eight waves. These photographs, made by the Naval Weapons Laboratory, Dahlgren, Virginia, are a by-product of a general study of initiation, propagation, and interaction of detonation waves undertaken as an aid to explosive system design. The action which occurs in just a few microseconds shows a symmetry in the detonating explosive which rivals that of the snowflake. When photographed in color, the growth and fading of this rather strange explosive "snowflake" give an appearance of unreal beauty.

These photographs were made with a Beckman and Whitley model 189 framing camera operating at about 600,000 frames per second, with individual exposure times of about 0.6 microsecond. A disk of DuPont EL 506C sheet explosive 25.4 centimeters in diameter by 0.379 centimeter thick was mounted on plywood. The explosive was initiated simultaneously at eight equidistant points on its rear surface with exploding bridge-wire detonators.

A frame-by-frame description (beginning with the upper left-hand corner and reading downward) follows:

1) A still shot of the disk mounted on plywood.

2–3) Detonation begins simultaneously and expands uniformly.

4–9) Extreme pressures at the collision of wave fronts produce lines of intense luminosity resulting from the ionization of the air.

10) Collision lines reach the center.

11–15) Reflected shock waves produce secondary collision lines bisecting the angles formed by the original waves.

The remarkable uniformity of initiation time and detonation rate is shown by the geometrical symmetry of the pattern formation and expansion, even though the detonation speed is approximately 6700 meters per second. Some idea of this may be obtained if one realizes that a time interval of only 1.65 microseconds separates the adjacent frames.

David D. Abernathy
U.S. Naval Weapons Laboratory, Dahlgren, Virginia

Data and Hypothesis

I wish to take issue with John Platt's article, "Strong inference" (16 Oct., p. 347). I agree with him that it is incomparably better science to set up alternative hypotheses, and then to devise procedures for excluding all but one, than it is to propose a single hypothesis and then to set about "proving" that the hypothesis is true. My own area of science has suffered much from the latter approach. However, I do not share the view that this is the only worth-while method of scientific research, and that government agencies should use adherence to the method as a criterion by which to judge the effectiveness of scientists. More particularly, I disagree strongly with Platt's disparaging remarks about survey studies and single-instrument scientists. I think a strong argument can be made for the proposition that the advance of scientific understanding depends primarily on the skillful and intelligent acquisition of new experimental or theoretical data, without previous formulation of hypotheses, and that a scientific problem is in principle already solved when enough information exists to permit alternative hypotheses to be devised.

The question I have posed might well be approached by Platt's own method, for we have many recent examples of spectacular advances in science, so that the question lies in an information-rich field where intelligent hypotheses can easily be formulated. Let us propose just two hypotheses:

1) That spectacular advances in science depend primarily on the development of new methods and on the intelligent use of both new and old methods to stockpile information relevant to a particular problem. The experience and skill required to gather such information often dictates that a scientist must devote most of his career to a single method or type of instrument.

2) That spectacular advances in science depend primarily on the purposeful setting up and destruction of hypotheses.

To test these alternatives (and to exclude one), I shall consider the first example cited by Platt, the Watson-Crick proposal for the structure of DNA. This proposal rests on two experimental facts: the x-ray diffraction patterns of Wilkins and the remarkable regularity in the base composition of DNA's from a variety of sources ($A/T = G/C = 1$). The acquisition of this experimental information occurred by procedures of which Platt would not approve. X-ray diffraction is a complex technique, and practitioners of it are by necessity single-instrument scientists. Moreover, x-ray crystallographers as a class do not normally begin with alternative structural hypotheses, but work from the knowledge that structural information is certain to emerge from their studies if they are sufficiently expert and persistent. In the determination of base compositions, too, the method of strong inference was surely not involved. Such analytical data are simply an integral part of the initial survey of the chemical properties of any substance. And let it not be forgotten that accurate and usable data of this kind depend on considerable skill in purification of the material to be analyzed and in the execution of the analysis.

To judge from Platt's description of the typical day in Crick's laboratory, it may be supposed that the method of strong inference was used to arrive at the final structure of DNA. In any event it was certainly used in the subsequent steps forward which have capitalized on this structure. Thus both data-gatherers and hypothesis-destroyers have been involved in achieving 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-*