Theory and Hypothesis

If you had chosen to reprint (7 May, p. 754) the 1897 *Journal of Geology* revision of T. C. Chamberlin's paper, "The method of multiple working hypotheses," you would have included the following desirable footnote:

I use the term theory here instead of hypothesis because the latter is associated with a better controlled and more circumspect habit of mind. This restrained habit leads to the use of the less assertive term hypothesis, while the mind in the habit here sketched more often believes itself to have reached the higher ground of a theory and more often employs the term theory. Historically also I believe the word theory was the term commonly used at the time this method ["the habit of precipitate explanation"] was predominant.

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Subnuclear Particles: A Question of Social Priorities

I wish to explore two interrelated, disturbing attitudes which were exhibited by most of the authors quoted in the collective appeal, "Purposes of high energy physics" (26 Mar., p. 1548). These authors, all leading theoretical physicists, reveal a narrow view of the relation of the intellectual and practical contributions of modern physics to the foundations of other parts of science, to our society, and to our culture in general, and it could be intellectually (and ultimately technologically and socially) debilitating were such attitudes inculcated in future generations of physicists or nonphysicists. And in their "remarkably unanimous plea for support for high energy physics and for the construction of much more powerful particle accelerators" they completely fail to give attention to the kinds of evaluation that policy makers should have available when they must weigh the physicists' values against the values of other segments of our society. They therefore innocently encourage the kinds of political decision-making that have led to disproportionate support of such "scientific" undertakings as the Apollo project.

"The world view of the physicist sets the style of the technology and the culture of the society and gives direction to future progress," says Schwinger. "But I believe that particle physics deserves the greatest support among all branches of our science because it gives the most fundamental insights. . . . [T]his is indeed the most basic field of knowledge in the physical world," says Bethe. "If we cut back on [high energy physics] for reasons of budgetary limitations or political squabbling, I think we will have seriously damaged the best single element we have contributed to human culture," says Feinberg. "A great society is ultimately known for the monuments it leaves for later generations. . . . . [S]uch a machine will without question be a source of inspiration for new science and a monument to our days," says Pais. It seems fantastic that these physicists should ask the scientific community and the American people to underwrite a billion-dollar project with such flimsy metaphysical arguments as these.

Weisskopf properly argues the importance of "intensive" research (research associated with those fundamentals of ordering and classification that can lead to the discovery of fundamental laws of nature) as the necessary base for "extensive" research ("the explanation of phenomena in terms of known fundamental laws"). In his judgment, "High-energy physics and a good part of nuclear physics are intensive"; biology is "perhaps" extensive. "It is granted that further progress, say in biology or in solid state physics, is possible without any further research into the subnuclear field. But let there be no doubt that the style of the scientific community would change its character if *the* frontier of intensive research were hampered . . ." (italics mine). Such exuberance may be understood in terms of the impact of our recent feast of "elementary" particles and quasars. This has brought an end to that relative famine of observational stimuli to further "intensive" research in physics which followed the successes of quantum mechanics and electrodynamics (1926–1950). That the famine was not science-wide has, however, been apparent to at least one renowned theoretical physicist (*1*). It needs also to be said that, although in principle quantum mechanics and electrodynamics permit the solution of most microscopic and macroscopic problems of chemistry and biology, there are formidable computational barriers to a solution in even so simple a system as the three-body problem (*2*). This points up the fact that most fundamental progress in biology has been and must continue to be of the same "intensive" kind as at present enchants the high-energy specialists in this latest renaissance in the physics of the elementary particles. Darwinian evolutionary theory and evolutionary taxonomy, Mendelian inheritance, the rules of chromosomal inheritance, the Watson-Crick model of DNA, the genetic code, and nonchromosomal genetics (*3*) are all of the same genre as SU-3 symmetry, which Bethe so feelingly describes. And I choose these particular examples of biological concepts—which have set, are setting, and will set "the style of the scientific community" and of "the culture of the society" at least as strikingly as any contributions of high-energy physics—because they developed virtually independently of any contributions from the fundamental "intensive" researches into the physics of matter or cosmology. In fact, it could be argued that the shoe is sometimes on the other foot. For example, it appears that Darwin's "most wonderful mechanical theory" explaining natural processes (those of evolution) in statistical terms provided an important stimulus for Boltzmann's development of the statistical formulation of the second law of thermodynamics, which led to the birth of statistical mechanics (*4*).

It seems to me highly undesirable, at this juncture in history, to foster attitudes in and of science which would give any significant primacy to the study of matter over the study of life (or conversely, perhaps, of life over matter). But in a dollar-conscious culture, the investment in a 10^{12}-electron-volt alternating-gradient synchrotron, like the investment in the Apollo project, will necessarily encourage attitudes among our youth which must have exactly this effect.

Congressional largess is not unlimited, and without any doubt expenditures on high-energy physics and space will necessarily limit expenditures elsewhere in science—as well as outside science. The decision to spend or not to spend requires an evaluation of the "purposes of high-energy physics" relative to other possible expenditures. "By ignoring this question, we have been trying to escape to science as an endless frontier, and to turn our backs on the more difficult problems that it has produced" (*5*). Some standard or standards of value (and taste) must and will be used, and just which those will be should be of considerable concern to us all. Since "it seems that all sciences are considered by their professors, as equally significant" (*5*), cultural relativists often argue that all deserve equal dollar support. However, those who are persuaded by the kind of naturalism which measures ethical value by biological adaptive-value, as well as pragmatists and even Puritan utilitarianists (*6*)—and their combined numbers in our tax-supported democracy are legion—are not likely to accept this kind of position. "Accordingly, we need to consider not only the practical relation of scientific institutions to the economy and government, but also the theoretical relation of science to political values, and to the principles that are the foundation of the constitutional system" (*5*). The financing of the as yet meagerly supported "extensive" and "intensive" scientific studies related to the population explosion and increasing longevity; the future evolution of man (*7*); the potential impact of computers on technology, on employment, and on the resulting complex of problems involving leisure, nurture, and human nature (*8*); and the problems of our plundered planet vis-à-vis energy reserves, fresh water supplies, and natural resources in general might well be placed far ahead of a 10^{12}-electron-volt AGS and the Apollo project, if all these problems were adequately examined in the public arena.

It is about time that the scientific community and our society as a whole face up to this kind of policy problem and discuss it out in the open. I and others have different billion-dollar (*8*) as well as thousand-dollar programs that must necessarily compete with those of the high-energy physicists, and all of these must be judged not only on their so-called "individual merits" but in competition with all other demands on our national resources, both economic and intellectual.

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References and Notes

1. For example, as recently as 1947 Herman Weyl said, "In our survey of the formation of concepts and theories by science . . . we saw how casual analysis proper is preceded by ordering and classification . . . [T]his preliminary stage that still plays a major role in biology . . . has become of subordinate importance in physics" [*Philosophy of Mathematics and Natural Science* (Atheneum, New York, 1963), appendix].
2. Even the most sophisticated computers now under design seem to hold no promise for an "exact" solution to the three-body problem. See also the discussions of "Does quantum mechanics exclude life?" by E. P. Wigner and P. T. Landsberg, *Nature* **205**, 1307 (1965), and P. T. Landsberg, *ibid.* **203**, 928 (1964).
3. R. Sager and Z. Ramanis, *Proc. Natl. Acad. Sci.* **53**, 1053 (1965).
4. L. Boltzmann, *Almanach Akad. Wiss. Wien* **36**, 225 (1886); *Populare Schriften* (Barth, Leipzig, 1925), p. 314.
5. D. K. Price, *Science* **148**, 743 (1965).
6. D. G. Barry, *ibid.* **147**, 1524 (1965).
7. See T. Dobzhansky, *Mankind Evolving* (Yale Univ. Press, New Haven, 1962), and G. Hardin, *Science* **144**, 1531 (1964).
8. L. Ornstein, *J. Mt. Sinai Hosp.* **32**, 437 (1965).

I think it would be worth while to reiterate the objective of the book from which the articles in *Science* were taken [*Nature of Matter—Purposes of High Energy Physics*, L. C. L. Yuan, Ed. (Brookhaven National Laboratory, Upton, N.Y., 1965) by quoting the following passage in the preface:

In the course of these studies [on the design and experimental program requirements for a super high energy accelerator in the energy region of multi-hundred BeV] we have found enthusiastic support and encouragement from theorists and experimentalists alike. However, we also sensed the apparent existence of some misunderstanding of the objectives of high energy physics, not only among the general public but also among the scientific community as a whole. It seems, therefore, that some communication by way of explanations is urgently needed among high energy physicists, the scientific community as a whole, and the general public. For this purpose, we have asked some thirty leading theoretical physicists, both in this country and abroad, each to present his own view on this subject, and to discuss some of the problems and implications involved. In this way, a collection of diversified views embracing many different aspects of the subject would provide a comprehensive basis for a better understanding of the fundamental importance and great depth of high energy physics.

During a recent congressional hearing on high-energy physics before the Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Chairman Melvin Price of the subcommittee made a statement, addressed to me, when I was called to testify. The substance of that statement was as follows:

Dr. Yuan, the Committee has in its files a publication of many papers which you edited and put under one cover, "Nature of Matter—Purposes of High Energy Physics." The staff has gone over it very thoroughly and considers it a well-done job, and I just noted hurriedly a couple of sentences in your Preface, that I think are pertinent to what we are trying to do in these hearings. You state that the scientists also sense the apparent existence of some misunderstanding of the objectives of higher energy physics, not only among the general public but also among the scientific community as a whole. You stated in your Preface, "It seems, therefore, that some communication by way of explanation is urgently needed among high energy physicists, the scientific community as a whole, and the general public."

That is what we are trying to do in these hearings, and we are glad to see that the scientific community itself recognizes the importance of these explanations going out to the public.

I wish to add that nowhere in this book was any mention made of the relative merits of high-energy physics versus any other branch of science, either the natural or the social sciences. Nor is there any attempt to try to minimize the importance of the other sciences.

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I tried to distinguish two kinds of research: the intensive research which goes for new fundamental laws, and the extensive research which goes for the explanation of phenomena in terms of known fundamental laws. No value judgment was implied, and I emphasized that the two types of research were of equal importance. I classified high-energy physics as belonging to the intensive type and expressed my uncertainty in classifying biology as extensive with the word "perhaps" for the same reasons that Ornstein brings up. We are not quite sure today whether life phenomena may not hide, after all, some unknown new fundamental law of nature.

The point I wanted to make is the following. Science can prosper only if intensive and extensive research are balanced. The style of science would change its character if the frontier of intensive research were hampered. The difficulty we are facing comes from the fact that most of today's intensive research is more expensive than extensive research, in terms which are hard to define—say dollar per important discovery. The reason for this is obvious: the search for new fundamental laws requires the study of nature under conditions which are progressively different from normal conditions and hence progressively more expensive to establish. We must find, therefore, a reasonable distribution of efforts over the total frontier of science, so that none of the really great intellectual enterprises suffer. Such a distribution may, in terms of dollars, seem to favor some of the "expensive" sciences.

So far the amounts asked for are

still so small compared to the total national product that society can afford to support all of the worth-while scientific projects. It is not yet necessary to slow down the search for answers to basic questions, such as questions of the ultimate structure of matter or of the nature of life. As long as we still live in a period of scientific expansion, the community of scientists should fight together for a larger support for science as a whole. This is better done if scientists restrict their public activities to the praise of their own fields and refrain from attacking the fields of their colleagues. There will be a time in the not-too-distant future, however, when much wisdom and insight will be required to establish a healthy and broad scientific frontier within the limitation of means which may be no longer small compared to the total national product.

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Ornstein dismisses the effort to justify high-energy physics in terms of certain general goals or attitudes of society as "flimsy metaphysics." But in the long run, pure science of any kind must be justified in these terms, rather than for the benefits it brings to society in the form of technology. To do otherwise is to distort the very aims of scientific research. It is simply false to pretend that physicists, and perhaps most biologists, are highly motivated in their research by the desire to improve social welfare; and if society will support only those working toward that end, it will have set up restrictions around science that will eventually destroy it. Or else the scientists will be driven to making overoptimistic claims for the possible applicability of their research, regardless of the distortion involved in doing so. Examples of this are already common in much of the dialogue between scientists and government. At least some of the contributions to *The Nature of Matter* were designed to give a different type of justification for support of high-energy physics.

Surely Ornstein must realize that societies do carry out expensive projects not for immediate benefits but for reasons of the type he calls metaphysical. One may cite such examples as the building of the Pyramids or of the medieval cathedrals—or, to use his own example, the Apollo project, which properly should be regarded not as a scientific experiment but rather as an expression of the human spirit. I for one am pleased that such motives play some role in social decisions.

The relation of atomic or particle physics to chemistry and biology is not a simple one, and Ornstein's comments seem to me to be somewhat incomplete. It can be granted that future research in particle physics is unlikely to turn up new laws relevant to biology. The fact remains that much of the best research in contemporary biology is strongly influenced by modern physics. The paper of Watson and Crick on DNA is a good case in point. This paper is written in the language of molecular physics and would have been incomprehensible to anyone unfamiliar with such physics. Indeed, the role of physics in biology can hardly be to describe biological phenomena as a special case of the 10^{23} body problem. Physics does not work this way even in such areas as solid-state phenomena. Instead, what physics does for other sciences is to state the general laws which all material systems must obey, such as conservation of energy, and to sometimes suggest specific mechanisms which may play an important role in systems of interest to another science. All of this is so elementary that one hesitates to dwell on it, but there is a danger that simple things may be obscured by deep feelings.

It is a good thing for scientists to discuss such issues among themselves. One might hope that the discussion will be carried out in a fraternal spirit rather than as a struggle for the lion's share of the public watering hole. If a scientist has a project that he considers worth while, no matter how expensive, he should propose it for the consideration of other scientists and society on its own merits. One cannot expect a man with a deep interest in a particular area to weigh its merits objectively in comparison with other fields in which he has no such interest. What one can expect is that he clearly indicate what he wishes to do, and say honestly why he thinks it is worth doing. This is what the high-energy physicists have attempted in *The Nature of Matter.*

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