Scientists and Logicians: A Confrontation

The logic of science is too often sundered from science; both might benefit from an effective liaison.

Norwood Russell Hanson

"History of science is only for retired scientists."

"A good laboratory man is too much concerned with *what is the case now* to worry about what was the case in centuries past."

"Even a good historian could rarely have been more than a third-class scientist."

"The logician of science ponders only the 'disjecta membra' of living laboratory research."

These are sentiments which all historians and logicians have heard, which some of them have even occasionally expressed, against which others of them feel defenseless or apologetic, and which all of them wish they could dispose of with a satisfying reply.

Here I attempt just such a reply, opening with a frontal attack. Since history of science and logic of science are fundamentally species of history and of philosophy, and since history and philosophy are clearly undertakings different from any ordinary scientific enterprise, it follows that history of science and philosophy of science embody objectives, techniques, and criteria wholly different from those that obtain in the sciences. It is naively misleading to evaluate such disparate disciplines as if they were subject to the same criteria. My theme, therefore, is this: Let no man join what reason reveals as sundered. But to this I should add: Let no man completely sunder disciplines which are connected through a common concern with ideas, concepts, reasoning, and argument.

It is one thing to use the scientific concepts we now have for generating conclusions about the natural world. It is another thing to ask, "What historical factors led to our having the concepts we've got?," and yet another thing to inquire, "What makes the employment of concepts in *this* scientific argument logically less vulnerable than their employment in *that* argument?"

Phillip Frank once said that the concept "H_2SO_4" contained the whole history of mankind. Historians and logicians will certainly feel this comment to be too comprehensive, but an inorganic chemist is not likely even to understand it. From his standpoint there are so many more interesting chemical concepts, fitting into so many intricate arguments, that Frank's remark may seem both trivial and false, What Frank probably meant was this: that a detailed description of the practical decisions, the notational conventions, and the theoretical practices which obtained when "H_2SO_4" was first formulated in chemical laboratories almost two centuries ago would provide both a richer understanding of the state of chemistry in the 18th and 19th centuries and a better grasp of the idea of "H_2SO_4." It would afford, also, an insight into the state of chemical science before Lavoisier—into the place of chemistry in the science of that day and the part it played in the development of modern thought. Such a description would advance on all fronts, terminating ultimately in a comprehensive account of the intellectual development of man since Prometheus.

Similarly the logician will scrutinize the place and function of "H_2SO_4" in the argument–structures of past and present scientific thinking to see which kinds of testable conclusions can, and which cannot, be generated via this vitriolic concept. But neither the historian nor the logician will be concerned with "H_2SO_4" as a first-order scientific stepping-stone leading to some empirical conclusion concerning, for example, the absorptive properties of certain atmospheric aerosols, the resistance to decomposition of fluorine-tested crystals, or the electrolytic capacity of some untested component in an electric accumulator. Such projects would never concern the historian *qua* historian or the logician *qua* logician—not because the historian and logician are second-rate scientists but because they are not scientists at all. They are concerned not with making discoveries about nature but with making discoveries about discoveries: How did these discoveries come about? What obstacles had to be surmounted, what arguments constructed, and what novel concepts created in order to generate such findings?

Historical Inquiry

To appraise such undertakings requires an understanding of the criteria appropriate to the evaluation of, for example, *any* historical undertaking. Such understanding was far from evident in a recent review of Partington's *History of Chemistry*. The reviewer, a bench chemist, could see no contemporary value in Partington's book, so he undertook to attack it. And he did so in a way that indicated that he probably could not have coped with any historical work—with *Gibbon,* or *Ranke,* or *Bury,* for those also would be found wanting if judged as manuals of directions concerning what we should do to resolve our present perplexities. By the reviewer's standards, since Gibbon had not ridden with drawn sabre against a Roman phalanx, he was not qualified to write *The Decline and Fall of the Roman Empire*. But Gibbon's is a great piece of history because of the controlled ingenuity, the imagination, and the scholarly care with which he perceives, describes, and demonstrates factors which corroded the Golden Age of the Antonines. Gibbon was in fact a poor soldier—a poor swordsman, and hopeless around horses. But great soldiers have not uniformly revealed mastery of the techniques and objectives of historical inquiry. A few of the great generals of World War

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II did indeed feel that their actions against the enemy qualified them to write histories of the 1940's. The bulky tomes of Alexander, Montgomery, Clark, and Eisenhower are the result. These are manifestos of powerful personalities, to be sure. But a graduate of West Point can hardly hope to satisfy the demands of serious historical scholarship without as much training in the literature and techniques of history and historiography as he has in weaponry and battle tactics. The parallel with science is clear.

Great scientists have shown the same kind of scholarly innocence when, in their *obiter dicta*, they have undertaken to make comprehensive historical appraisals of the state of science in their own times. The result is, at best, bad history and, at worst, prejudice or opinion. Lifetime preoccupation with the intricacies of a particular science is hardly preparation for work in a wholly different field. The histories of science of Berry, of Schrödinger, and of de Broglie are cases in point. These histories are analagous to what one would have expected of the physicist Hooke in Newton's day, of the publisher Osiander in Copernicus' time, and of the astronomer Hipparchus in Archimedes' era.

Yet we hear it said on every hand—in college committees, in budget meetings, in public debates—that only first-class scientists can teach history of science. The corollary is that no one not actually working with a cyclotron should presume to wax historical. It's a little like expecting only a very prolific mother to write the definitive work on obstetrics.

The administrations of many of our American universities and colleges have come to value studies in history and philosophy of science, but rarely for the right reasons. Such programs have been turned over to retired Fellows of the Royal Society, or to third-rate graduate students in mathematics. Clearly, what is needed is nothing less than a first-class historian, whose special concern is the growth of scientific thinking in Western culture.

Logical Analysis of Science

I turn now to the logicians, and to the logic of science, with a review of Carnap's treatise on probability uppermost in my mind. Here again the reviewer, in this case an eminent microbiologist, stressed laboratory criteria: "How is all this concept-analysis and symbolic derivation and idea-splitting going to help us to become the first into outer space?" What has Carnap ever discovered about probability that will make our modern textbooks in science different from the older versions?

Blast these questions! It is as if a journal of dentistry put Bertrand Russell on its *index prohibitorum* because *Principia Mathematica* did not lead to the discovery of novocaine! But *Principia Mathematica* did go to the heart of a corpus of perplexities concerning the nature of number, the concept of zero, and the process of numerical succession. Few mathematicians felt these perplexities deeply at the turn of the century, but their work was profoundly affected by uncertainties that surrounded these ideas and concepts. Why should they have felt them deeply? They were already too busy *using* these notions in the generating of mathematical insights in functional analysis, area theory, and so on, to stop to analyze the structure and logic of notions like number, zero, and succession. Aircraft pilots are too busy using high-octane gasoline to climb out for a bit of on-the-spot petroleum analysis. Similarly, mathematicians were, and often are, too much taken up with mathematics to begin logically analyzing the tools of their trade *à la* Russell and Whitehead—and Carnap.

But such jobs need doing. Why? Because they have not yet been completely done, and one cannot predict the ultimate outcome of untried or unfinished intellectual undertakings. The logicians invited profound rethinking within mathematics and within probability theory. From the work of Russell and Whitehead stemmed that of Sheffer and Huntington, of Zermelo and Brouwer, of Heyting and Hilbert and Post and Skolem and Gödel and Church and Quine, of Kleene and Rosser; and the work continues. The most influential figure in the probability treatises of Sir Harold Jeffreys has been Carnap himself, despite deep disagreements between the two thinkers. Most of today's workaday mathematicians, algebraists, geometers, and topologists have little intimate knowledge of the work of all these logicians. Is this important? Indeed, too much attention to elementary number theory has been known to seduce promising mathematicians away from the frontiers of contemporary work in mathematics, forcing them to learn some logic. But please, let us call this "moving into another field" and not insinuate that it is a sort of flunking out of a hard, worth-while discipline and lapsing into a woolly subject somewhat akin to philosophy. Let me counter any comment of that kind by saying that the most promising young logician I ever knew was seduced by mathematicians and is now lost in a faceless, featureless department of a gargantuan university, teaching elementary algebra; he *could* have shaped new ideas for tomorrow.

Then, to be personal, let me cite my own happy progress from high school chemistry to college physics, to wartime aeronautics, to advanced research in microwave technique and theory, all disrupted by a nagging interest in the fundamental axioms of microphysics, the operational significance of Dirac's delta function, the tricky technique of renormalization, and, in general, the attractions of logic. "If only Hanson had stuck to particles instead of scrutinizing propositions, he might have become a reasonable scientist instead of. . . ." I can't go on—it's too painful. And here, as before, the pain stems from the fact that someone looks on the philosophy of physics as an undertaking for unsuccessful physicists, not as what it is—philosophy and logic for those who have some understanding of the problems and of the literature of contemporary physics. Grünbaum and Feyerabend, Woodruff and Watanabe, Putnam and I, do not perform crucial experiments in order to attack each other's positions; we argue. Our tools are not benches, burners, and beta-beams but logic, language, and the literature of physics, past and present. We disagree about almost everything. The result, however, is not a universal confusion about the make-up of the natural world but occasional illumination concerning the way physicists think, the way their theories are made and changed, the way physical thinking in general has changed thinking in general.

In short, research scientists don't own the literature of science; they certainly don't own the history of science; and they most assuredly do not possess the sole right to analyze the logic of scientific arguments, past, present, and future. Individuals who are deeply interested in, and highly trained in, the analysis and evaluation of literature, of history, and of logic—and who are also trained as scientists—might at least be granted the opportunity (as they are in increasing numbers) to undertake some serious study of those differently designed disciplines, the history and logic of science. They should be granted this opportunity without having to prove that such studies will be pedagogically useful in doubling our nation's production of scientists, in humanizing our technologists, or in designing handy manuals for discovery. These last are the wrong reasons for backing the study of the history and logic of science, although much of the support, sad to say, is based on them. We who work in these fields must continue to insist on being supported for what we are—historians, logicians, and philosophers whose attention is riveted on modern natural science, which provided the greatest intellectual jolt the Western mind has ever received. Our objectives, it must be announced at the start, have nothing to do with departmental power, science-faculty pedagogy, or university politics; we are concerned with clarity and understanding for their own sakes.

Common Ground

Having thus sundered science from the logic of science, I may have succeeded only in articulating what no reasonable man has ever doubted for a moment. Now I must undertake to mix the slippery oil of science with the icy water of logic, believing as I do that the resultant solution is really the life fluid of both physics and logic. This is not an attempt to conjoin the thesis presented, which I shall call *P*, with its negation, not-*P*. Rather, I hope to show that *P* and *Q*—science and the logic of science—are fundamentally different disciplines which nonetheless intertwine.

My thesis to this point has been simple: Different subject matters should never be measured by the same meter stick. My thesis from here on is equally simple: Both physicists and logicians often use their heads.

Scientists argue about the world. Logicians argue about arguments. I have already suggested that the scientist's arguments and the logician's arguments are subject to quite different criteria of evaluation. A scientist wins an argument if the facts of nature confirm his conclusions. A logician wins an argument if he reveals all alternatives to his argument to be inconsistent, or otherwise conceptually untenable. Nonetheless, both individuals are vitally concerned with argument per se. Evaluating alternatives, deducing conclusions, eliminating redundancies and inconsistencies, and designing tight and highly relevant tests—these are essential undertakings for any scientist who is grappling with a perplexity about the natural world.

The history of science is stocked with examples of conceptual analysis and argument appraisal, with the generation of "further conclusions" and the reexamination of initial conditions. Consider Claudius Ptolemy's formal demonstration that the curve resulting from epicyclical motion around a fixed point is identical to that resulting from circular motion around a point which moves with the original epicycle's motion. Think of Galileo's argument that the instantaneous velocity of a freely falling body is proportional not to the distance it has fallen but to the duration of its fall. Consider Newton's philosophical discussions of the nature of action at a distance, and of the axiomatically required properties of physical space. Think of Leibniz' criticisms of the "method of fluxions," an early version of the differential calculus (not to mention Berkeley's shrewd scrutiny of the *idea* of an *infinitesimal*). And what about Ernst Mach's attack on the classical idea of mass, an attack directly connected (historically and conceptually) with Einstein's reflections on the operational definition of simultaneity, energy propagation, and space? Think of the simultaneous demonstration by Eckart and Schrödinger, in 1926, that matrix mechanics and wave mechanics, then thought to be wholly different, are really formally identical—a demonstration which is itself vulnerable to the same kind of logical reappraisal that Eckart and Schrödinger thought they were initiating. Indeed, even Heisenberg's detection of the existence of a noncommutativity between the time and energy operators within the quantum theory of 1927—that is, his discovery of the uncertainty relations—is fundamentally a species of analysis of existing theory. Related also is the neat observation that Heisenberg contends, at the moment, that the distinction between elementary particles and composite particles is semantically meaningless within contemporary quantum theory. Think also of Blackett's recognition, in 1933, that the arguments of Dirac and the quite distinct arguments of Anderson terminated in a prediction of the same particle, the positron—yet another example within science of the operation of sharp conceptual analyses, of the logical examination of argument structures, and of the strict and relentless tracing of inferences from initial conditions, through laws, to observationally vulnerable conclusions.

This tracing of inferences is something scientists do very often in the course of their daily work. And it is what logicians of science do every day of their lives. The objectives in these undertakings are still distinguishable—scientists and logicians decompose arguments for quite different reasons. But the undertakings themselves are remarkably similar—so much so that it is often hard to decide, for example, whether Mach and Einstein were pursuing "logic" or "science" during certain phases of their work.

A Distinction

Thus there are areas common to science and the logic of science. But there is a salient distinction. The logician who knows no science whatever *may* succeed as pure logician, but he is unlikely to get far in the philosophy of science. Considerable exposure to the literature of some science is indispensable for the logician of science. But very few practicing scientists, although their roles as argument-analyzers seem so obvious, feel any need to work in the area of the logic of science. That is unfortunate. One cannot pick up skill in the use of logical tools through osmotic exposure to laboratory benches. Exposure to mathematics professors rarely helps either. Using one's head to maximum effect requires *learning* how most effectively to use one's head. For 2000 years logicians have been concerned with precisely this. These days they are becoming rather good at it. And when this undertaking is specifically geared to scientific argumentation, logic of science is the result—a result which is not simply the side effect of well-designed experimentation.

Conclusion

I am not pleading for the inclusion of more logic of science in the training of scientists. Such a plea would rest on the very targets I have been shooting at. But there is a common ground here, without doubt. Or, to put it another way, logicians have too long smarted under the comment that they are not engaged in laboratory science although they purport to be illuminating it. I would respond to that comment with another: Although scientists very often are forced to analyze arguments and concepts in the course of their daily work, very few of them ever receive any formal training for doing this. Whether or not the history of science would have been written differently had this not been the case I cannot say. Here I only want to stress the fact that mastery of the techniques of science and the techniques of logic requires experience and study and that logicians of science have perceived the need for both techniques, whereas practicing scientists, although they very often undertake both, are usually trained in only one.

In short, the logic of science has been sundered from science and from the training of scientists. This comes from viewing them as independent undertakings, requiring different criteria and different skills. I am suggesting here that the deep connections between these two undertakings make them natural conceptual allies in the context of our general intellectual development.

It is not my purpose to explore the ways in which practicing scientists and practicing logicians of science can actually achieve an effective liaison, for the ultimate benefit of both. Senior scientists and junior university presidents seem to be confident that they possess the answer. But that the *doing* of science and the *thinking about* science are different disciplines which cannot be fused together but which nonetheless are interdependent—this is the intellectual symbiosis I have sought to delineate. Fusion of the two would result in a formless pulp and serious science or serious logic of science would suffer. But complete cleavage of the two would ultimately result in the death of each.

NEWS AND COMMENT

Manpower Race: Panel Offers Proposal To Turn Out More Scientists, Engineers

In its efforts to steer young people into careers in science and engineering, the Soviet Union holds an important advantage over this country.

The Soviets can tell any talented student that if he wants a higher education, it will have to be in engineering or the sciences. For vast numbers of students this is not an unpalatable choice by any means, and through this combination of push and natural inclination, the Soviets outproduce this country three to one in engineers; if teachers are included, they are also ahead nearly two to one in what are loosely called the sciences. And they have achieved these results with a total higher-education enrollment that is smaller than this country's.

It can be argued that the Soviets need more engineers to accomplish tasks that we accomplished years ago. And there is also the likelihood that a careful examination of quality would reduce the significance of the disparity, but in the context of East-West competition, the manpower race cannot be brushed aside. Accordingly, last January, the President announced that he had asked his science advisers and the National Academy of Sciences to recommend steps to increase this country's output of engineers and scientists, a goal that is not at all easy to achieve under the Western tradition of students deciding what careers to follow.

The first report to result from that request was issued last week ("Meeting manpower needs in science and technology, Report No. 1." A report of the President's Science Advisory Committee. U.S. Government Printing Office, Washington 25, D.C. 20¢). Briefly, the report proposes that the most effective first step would be to make certain that high-quality facilities are available and that financial problems do not discourage qualified undergraduates from full-time graduate study in engineering mathematics, or the physical sciences (EMP). The proprosal, in a sense, is a modest one, since financial assistance in these graduate fields is already fairly extensive (last year, 40,000 of the 56,000 full-time graduate students in these fields were receiving "full-scale" support—stipends of several thousand dollars a year, plus their educational expenses). But, at the same time, the proposal would openly establish the principle that since the federal government, directly and through contracts, is the main "consumer" of EMP manpower, it should assume responsibility for education in these fields.

By bits and scraps, through a variety of fellowship and training grant programs, the government has already moved a good way toward this role, most markedly in the life sciences. But the report, which was endorsed by the President, recommends that the nation, with the federal government taking the lead, now go all the way in EMP support; that *all* full-time EMP graduate education costs, to the student as well as to the institutions, be fully financed through a "National Program" in which the federal government would be main source of funds.

To this extent, the proposal steps out onto new ground, but in terms of the numbers of students it would bring into graduate school and the numbers of degree holders it would produce, it is deliberately aimed low, apparently in deference to the often-overlooked fact that the whole issue of scientific and engineering manpower is an enormously complex one that is beset by many pat assertions and surprisingly little reliable information.

It is obvious that in many fields, especially those related to the space effort, the present pool of specialized manpower is running low in spots, and the situation is likely to become even more critical as the nation's technical commitments increase. But the measure of inadequacy involves a good deal of guesswork, since, despite the widespread alarm over manpower shortages, no comprehensive and reliable study has yet been made. At best, there can be no firm answer to the question of what do we have and what will we need, but in this statistics-rich age the