



The Irrelevance of History of Science to Philosophy of Science to Philosophy of Science

Author(s): Norwood Russell Hanson

Source: *The Journal of Philosophy*, Vol. 59, No. 21, American Philosophical Association

Eastern Division: Symposium Papers to be presented at the Fifty-ninth Annual Meeting, New York City, December 27-29, 1962, (Oct. 11, 1962), pp. 574-586

Published by: Journal of Philosophy, Inc.

Stable URL: <http://www.jstor.org/stable/2023279>

Accessed: 02/06/2008 13:48

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=jphil>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is a not-for-profit organization founded in 1995 to build trusted digital archives for scholarship. We enable the scholarly community to preserve their work and the materials they rely upon, and to build a common research platform that promotes the discovery and use of these resources. For more information about JSTOR, please contact support@jstor.org.

(ii) Whittaker made no allowance at all for the crucial *logical* differences between the pre-Einsteinian and Einsteinian conceptions of the status of any extraterrestrially observed contraction of a rod moving with the earth and pointing in the direction of the latter's motion.

Thus, only philosophical awareness of the fact that Einstein's conception of the Lorentz transformations is *not* to be construed along the modified ether-theoretic lines of Lorentz and Poincaré makes possible the discernment of the mistake in Whittaker's historical treatment of the STR.

It is to be hoped that the open questions concerning the history of the STR will be tackled by historians having intellectual command of the philosophical foundations of that theory.

ADOLF GRÜNBAUM

UNIVERSITY OF PITTSBURGH

THE IRRELEVANCE OF HISTORY OF SCIENCE TO PHILOSOPHY OF SCIENCE *

I

THERE is but one question before us: **can a philosopher utilize historical facts without collapsing into the "genetic fallacy"?** **If he can, will his analyses be improved?**

Failure to answer this question has vitiated many discussions concerned with the role of historical facts within philosophy of science, as well as the **role of logical analysis within history of science.** Some philosophers have set their sights on *Weltphilosophie*, noting that every historian has one. Explicitly or implicitly it controls his selection of salient subjects, his alignment of data, his conception of the over-all objectives of the scientific enterprise, and his evaluations of the heroes and villains within the history of science. That the historian's interpretation is shaped by covert cosmic commitments is clear in the writings of Waddington, Bernal, and Needham. It is apparent also in the works of Whewell, Meyerson, and Poincaré. Moreover, unspoken and unspectacular *Weltphilosophien* provide the intellectual reticulum in terms of which we must view even our most honored "objective" historians of science—Tannery, Duhem, Sarton, and Koyré. As has been suggested recently by Professor R. Cohen at the Xth International Congress of History of Science, to be

* To be presented in a symposium on "The Mutual Relevance of the History and the Philosophy of Science" at the fifty-ninth annual meeting of the American Philosophical Association, Eastern Division, December 28, 1962.

human at all is to fit the elements of one's outlook into uncriticized philosophical patterns; this is no less true for eminent historians of science than for the rest of us.

Those who stress the silent operation of a *Weltphilosophie* in the studies of historians of science then suggest that without philosophical awareness and acuity the reader must remain at the mercy of the historian's unspoken assumptions. "[The physicist's] dominant faculties, the doctrines prevalent around him, the tradition of his predecessors, the habits he has acquired, the education he has received will serve him as guides, and all these influences will be rediscovered in the form taken by the theory he conceives."¹ Presumably, it requires a logician critically to understand what is significant in the great expositions of the evolution of modern science. The conclusion is that *history of science without philosophy of science is blind.*

A more modern plea for the use of philosophy of science within history of science has concerned not the capital-lettered "isms" that direct the historians work, but rather the conceptual details making up that work; not the philosophical architecture, but the conceptual bricks and beams constituting the structure of particular histories of science. We have all encountered naive accounts of scientific discovery with their crude uses of terms like *law, cause, explain, predict, observe, verify, refute, deduce*; even conceptions like *science* and *discovery* themselves are accorded the same unsatisfactory treatment. One often reads histories of science for illumination about the genesis of such "philosophical lubricating terms." The light cannot go on when these terms trip off tongues untried and untrained, indeed tied into knots by factual overconfidence. One almost imagines that some historians² cloak their conceptual confusions in clouds of data-clusters: dates, editions, acquaintances, and genealogies. But in the best histories this is not so. Mach's analyses of Newton's laws, Duhem's studies of *force* and theory, Koyré's treatise on Galileo's law of freely falling bodies, and Rosen's examination of the interrelations of *circulus* and *orbis* in the work of Copernicus—these are cases of philosophical acuity leading to the conceptual clarity that makes great history of science. But some will mark these as the brilliant exceptions that prove the dull rule. Thus the indisputable suggestion that closer attention to the logical structure and deductive consequences of key "philosophical" concepts within the history of science would help most historians immeasurably; *ergo* philosophical insight is what historians of science need more of!

¹ Pierre Duhem, *Revue des questions scientifiques*, 34 (1893): 377.

² For example, Berry, *History of Chemistry*; Whittaker, *History of Theories of Aether and Electricity*; Nordenskiöld, *History of Biology*.

Recently (at the Xth International Congress) I stressed a third kind of interpenetration between history of science and philosophy of science. This had little to do with the *Weltphilosophie* of historians, still less with the concept-spectra exploited in their expositions, and more to do with the understanding of their arguments; the prime target was the arguments of scientists that historians purport to illuminate. My suggestion was that we should take our scholarly spotlights away from the architecture, and away from the bricks and beams. We ought to play them more on the structures, interrelations, and, indeed, the engineering connections that have made science the intellectual concern of our time. Let me say a little more about the centrality of argumentation within the endeavors of both historians of science and philosophers of science.

Logicians are concerned with arguments, logicians of science with scientific arguments. Their enquiries presuppose answers to worries about the conceptual "stuff" of arguments: unless you know *what is being* argued you cannot determine the argument's soundness. Unless you understand the historical force of concepts in seventeenth-century science, what *force* and *gravitation* and *mass* meant to Kepler, Galileo, Huygens, and Newton, you cannot determine the soundness of those particular classical arguments in which such concepts so gloriously figured.

The "higher-level perplexities" involved in understanding the historians' *Weltphilosophie* themselves assume detailed knowledge of the actual arguments which, for the historian, constituted a scientific advance. But few have stressed the dependence of their broader interests in science on the analytic assessment of scientific arguments. To parody the diverse approaches: philosophers of science like Buchdahl, Hesse, and Toulmin often focus on understanding the evolution of scientific concepts as constituting the intersection of philosophy of science and history of science. Philosophers like Mandelbaum, Meyersohn, and Cohen have their sights on the ultimate direction and objectives of historians of science. Doubtless, both kinds of concern are important in any critical understanding of the literature of history of science. But to me it is in the detailed analysis of the detailed arguments of scientists and historians where philosophy can most help, and *be helped*.

Keynes shows us the way. He argues³ that no scientific statement is ever *probable* in itself, but *probable only* on the assumption of given evidence. To say of a proposition that it is probable or has a probability of .9 is, for Keynes, like saying that it is "equal to" or "greater than" or "divisible by." Such relational

³ *Treatise on Probability* (London: Macmillan, 1952).

characterizations make no sense whatever when only one of the relata has been designated. No; Keynes perceives that the probability relation is an inferential connection between scientific premises or initial conditions, and observable consequences—a connection the assessment of which must always be deductive in form. The analysis of arguments in these terms is an enterprise for which the logician of science should have received some rigorous training. Assuming an advanced familiarity with a scientific subject matter, then, the logician of science should be capable of assessing the formal cogency of arguments of, e.g., “steady-state” cosmologists as against “big-bang” theorists: he should be able (in principle) to determine which claims of reasoning are the “best made,” which conclusions are most likely *on the evidence given*, which assumptions *en route* are most and least vulnerable. He ought to be able coolly to reconsider the experimental evidence available to microphysicists in 1931 and determine therefrom who had the best arguments—those who quickly opted for the existence of anti-particles (like the positron), or those (like Bohr and Rutherford) who sought to reinterpret the shocking cloud-chamber tracks of Anderson, Blackett, and Occhialini, and the perplexing “negative-energy” equations of Dirac, in terms of more familiar ideas well known in the 1920s. The philosopher of science should place into logical counterpoise the explanations of Asaph Hall in 1896 and Einstein in 1916—explanations of the disturbing secular advance of the perihelion of Mercury—in order to see which investigator, on the evidence before him, reasoned most relentlessly toward his conclusion. This does not always mean that rigorous and precise determinations of the probability of past scientific arguments are within the easy grasp of the logically trained historian. There will always be difficult “twilight” cases: most of the really important cases may lie in this region. But midnight is still very different from noon. The *very* probable and *highly* unlikely are always separable on *logical* grounds, given a carefully formulated set of initial conditions on which to base one’s inferences. This, even though any assignment of a probability like .92 to Fizeau and one like .71 to latter-day Newtonians may be practically beyond achievement.

When we argue that Darwin gives valid grounds for our accepting his theory of natural selection, we do not simply mean that we are psychologically inclined to agree with him; it is certain that we also intend to convey our belief that we are acting rationally in regarding his theory as probable. We believe that there is *some real objective relation between Darwin’s evidence and his conclusions*, which is independent of the mere fact of our belief, and which is just as real and objective, though of a different degree, as that



which would exist if the argument were as demonstrative as a syllogism. We are claiming, in fact, to cognize correctly a logical connection between one set of propositions which we call our evidence and which we suppose ourselves to know, and another set which we call our conclusions, and to which we attach more or less weight according to the grounds supplied by the first (5).

Keynes goes on :

It would be as absurd to deny that an opinion *was* probable, when at a later stage, certain objections have come to light, as to deny, when we have reached our destination, that it was ever three miles distant; and the opinion still is probable in relation to the old hypotheses, just as the destination is still three miles distant from our starting point (7).

In other words, for Keynes the probability relation that obtains between a conclusion and its premises is so "objective" that one can characterize it in a time-independent way at any future date. One can determine the probability obtaining between some conclusions advanced by the young Darwin and the evidence or data from which they were drawn; no matter what mature findings may have been made by Darwin later in his research, that original estimate of the original conclusions' probability, on the basis of the original evidence, remains fixed for all time.

Two things are immediately clear from this. (1) Such logical evaluations of historically significant arguments are not "subjective": they do not depend on the logicians' or the historians' prejudices or choice of heroes—no more than would a mathematician's evaluation of the soundness of a purported proof of Fermat's last theorem have to rest on extra-formal considerations. Given a premise set, a putative conclusion either does or does not follow. If it does, it does so necessarily. If it does not, then the assertion that it does is inconsistent. Similarly, given a set of physical premises and initial conditions, a physical consequence will either have a probability P on these premises necessarily or the assertion that its probability is P will be demonstrably self-contradictory. Mapping this out as a path for philosophers through the jungles of history of science, I am expressing a thesis counter to that of Professor Cohen: ". . . while philosophers of science think about deductively-formulated theories, they had better do so inductively" (Xth International Congress). My claim is that *while philosophers of science think about inductively formulated theories, they had better do so deductively*. Or, at least, the justification for a philosophical analysis had better never consist in any gross appeal to the facts.

(2) Our assessments of which argument at time t was the *best* argument (given the data available) will not always award the

guerdon to the argument that is ultimately correct. This point is of the utmost importance to any historian or philosopher of science: that scientific advance and rigorous logic do not always walk arm in arm is an exciting disclosure, but it should always be spelled out in logical detail, not painted poetically in words (as historical scholars are sometimes wont to do). Consider Galileo's (correct) contention that the instantaneous velocity of a falling body is functionally related to the duration of its fall rather than to the distance it falls: Duhem's proof that this was based on a formally fallacious argument was a triumph of analytical scholarship. Similarly, today's universal recognition that Archimedes was *not* a Copernicus of antiquity, save in a *very* special and debatable way—since, on evidence then available, the arguments of Hipparchus and Apollonius were logically much preferable—this is the kind of conceptual vortex that quickens history of science into genuine intellectual excitement. As a probe for the testing of such vortices, I submit that the logical analysis of arguments within the history of science is no less rewarding than is concept-genealogy (as with Toulmin) or the recognition of the pervasive *Weltphilosophien* (referred to by Cohen). Indeed, for the understanding of the turbulent ripples in the flow of western science—as when we face the counter claims of Gold vs. Gamow, Anderson vs. Rutherford, Hall vs. Einstein, Adams vs. Airy, Young and Fresnel vs. the later Newtonians, Lavoisier vs. Priestley, Kepler vs. Brahe, Copernicus vs. Müller, etc., attention to the logical cogency of rival arguments is of maximal scholarly value. Even when final decisions elude the investigator, such a confrontation of historically important arguments can strip the history of science to its logical bones. At such moments, logical analysis of the historically significant arguments (on the evidence then available) might even be *identified* with history of science at its best. The giants were of this analytical cast: Tannery, Duhem, and Koyré.

Here then is the “hot” junction box which connects the conceptual circuitry in history of science with that of philosophy of science. Professionally, the logician and the historian will often be concerned exclusively with the rational wiring within that box—the scientific argument itself—and not just with the intricate intellectual geometry leading to it and away from it, nor with the lights that may go on in the world of science, and the illumination afforded by historians of science, as a consequence of that circuitry and that junction box being designed as they are. The historian of science and the logician are both concerned with the structure of scientific ideas. These concerns

fuse into one when the scientific *argumentation* of the past takes the spotlight.

But all this has fallen into my earlier aphoristic mold: that history of science without philosophy of science is blind. I must now undertake to show that philosophy of science without history of science is empty.

II

The foregoing looks parochially professional. It seems to survey the ways philosophers of science tell historians of science how to do their jobs better. Historians will quickly retort that, since I have only characterized what they do in their everyday work anyhow, it is only a constellation of platitudes. This reaction must now be qualified with a few "plongitudes"—a few plunges into the troubled waters which separate history of science and philosophy of science.

The maelstrom within these waters is the elusive, yet pervasive "genetic fallacy," around which I have rowed for these 00 pages. When experimental psychologists, social anthropologists, and "kultur vultures" (e.g., Levy-Bruhl, John Dewey, Talcott Parsons, and George Mead) sail glibly into strictly logical discussions concerned with the semantical content of technical concepts and the logical structure of formal arguments—how satisfying it is sometimes to hear the bold retort: "That is merely a matter of fact!" There can be no doubt about it, within the history of philosophy illumination has been lost and scattered through clouds of conceptually irrelevant historical detail. A simple question is asked concerning whether a given conclusion follows from a given argument and, too often, the air becomes charged with quotations from Plato and Aristotle "who thought it did" and Spinoza and Gassendi "who thought it did not." Indeed, *the* standard "goof off" amongst professional philosophers is to serve up a tray of facts when what is really needed is **the sharp scalpel of analysis**. We are all sometimes guilty of this—when weary or disinterested, or rushed. Some of us are always doing this. But we all know that this is a poor excuse for philosophy, just as covering fences with colorful thickets is a poor excuse for town and country planning.

Some of our greatest philosophers of science: Schlick, Carnap, Reichenbach and Popper, have been sensitive to the ways in which **scholars sometimes dull the scalpels of philosophy by burying them in the historical gravel**. Conceptual clarity is primarily the result of unfettered logical analysis; allusions to actual occurrences in the history of science were at most illustrations (for Schlick,

Carnap, Reichenbach, and Popper) of arguments which commended themselves on rational grounds alone. What does it matter that von Neumann, Jeffreys, or Clerk Maxwell invoked the probability calculus this way or that? They were concerned to explain and predict the workings of physical nature: they rarely faced the logical structure of probability arguments *per se*. What does it matter that Mach, Newton, and some of the Schoolmen thought of laws of nature as statistical summaries of observed data and, as such, generable (or "deducible") from the facts? Again, these men were Natural Philosophers, not philosophers of science.

That X is done universally does not in itself make X the universally correct thing to do. That all past and present scientists do X or say that X —or are said by historians to have done or to have said that X —does not in itself make X the correct thing to do or to say. Philosophy of science is, like all philosophies, not simply a rehearsal and recitation of what is done and said; it is also an analysis and an appraisal of the *rationale* and logical justification of scientists doing and saying what they do. Just as a child has not defended his misbehavior by the claim: "Johnny does it too" or "Johnny did it too," so also—it can be argued—the real business of philosophy of science is in no way furthered or illuminated by pronouncements like "Heisenberg says it too" or "Newton did it too."

This much seems completely to have sundered history of science from philosophy of science—and let no man join what reason reveals as sundered. That will constitute my leading conclusion *re* the logical relevance of history of science to philosophy of science. The former has *no* logical relevance whatever: should anyone ever attempt to buy off the validity of an argument by reciting facts out of the history of science, he deserves the scolding inevitably to ensue.

Nonetheless, when stressing that history of science and philosophy of science had a common concern in the structure and function of scientific arguments, some softening of this rigid logical proscription was heralded. Let no man completely sunder disciplines that are intimately connected through their common concern with ideas, concepts, reasoning, and the argumentation of scientists.

As we have been told, philosophy has no subject matter. But philosophy of science has, namely, science. It is all very well for a philosopher of science to argue "If there were a discipline in which conservation principles P_1 and P_2 held and within which laws L_1 and L_2 were adhered to, then from initial conditions I_1 and I_2 conclusions C_1 and C_2 would strictly (i.e., deductively)

follow." If what the philosopher of science says in such a context is true at all, it is necessarily true. If it is not true, then it is logically false. And no facts about the theoretical constitution of present or past scientific events can have any logical bearing on the appropriate appraisal of the philosopher's analysis. But it still remains that the philosopher of science may be discussing no genuine state of affairs at all! This is the reaction of most practicing microphysicists when they read Reichenbach's *Philosophical Foundations of Quantum Mechanics*, and it is a standard response of historians of science when they confront works in the philosophy of science, especially within the tradition built up in the wake of George Sarton. Historians see in the works of such "formalistic" philosophers of science as Carnap the "fallacy of misplaced abstraction." Without some concrete treatment of the *de facto* development and present state of modern science, philosophy of science strikes many as unilluminating. But those philosophers of science who shy away from "historicism" find the facts within history of science equally unilluminating. To the historian such philosophy of science is often unilluminating because it does not enlighten one about any *thing*: nothing in the scientific record book is treated in such symbolic studies. To the philosopher, histories of science are often unilluminating because, as a result of their chaotic diffuseness, they never reflect monochromatically: only spectra of concepts and arguments result. For the historian formal philosophical analyses are often empty. For the philosopher the historian's factual compendia seem blind. This suggests a loose analogy within the development of theoretical hydrodynamics and aerodynamics.

The rigorous mathematical explorations of Euler and Bernoulli were models of logical precision even though they dealt with a highly fluid and unstructured subject matter. The relationships between velocity and pressure, between boundary layers and turbulence, between flow direction and "lift," are beautifully mapped within the elegant algebra of these accomplished mathematicians and their inspired followers. Alas: the elegance was illusory. The algebra and the elegant analysis were all based on the assumption of an *ideal fluid*—one utterly lacking in resistance and viscosity. The result was that practical hydrodynamicists, ship designers, civil engineers, plumbers, and aeronautical enthusiasts could not use one line of what the Euler-Bernoulli theoretical tradition had produced. There are no ideal fluids! Oil, water, and air all offer considerable resistance and have pronounced viscosity (thank heavens). A more "pragmatic" discipline was quickly fabricated. It was called "hydraulics." This was no ele-

gant, axiomatically generated calculus in the Euclidean manner. It was, rather, a chaotic collection of recipes, hints, descriptions, and techniques—a plumber's tool box. But without knowledge of this kind we should never have understood the phenomenon of heavier-than-air flight, much less actually built aircraft. A considerable interplay between practical aerodynamics and classical hydrodynamics has at last been effected, despite the fact that some of the standard problems within aerodynamic theory have been completely beyond any general mathematical treatment. (Many perplexities arise through the required use of partial, nonlinear differential equations of the second order in time—for which no *general* mathematical solution can grind out past or future "state-descriptions" of phenomena, comparable to what is encountered with the linear differential equations of the first order as encountered within Newtonian mechanics.) Here again is an intellectual contest: classical hydrodynamic theory, following Euler and Bernoulli, generates the sharpest possible answers to a cluster of beautifully formulated hypothetical questions. The only difficulty is that these answers cannot help in practical hydrodynamics, wherein there has never been an ideal fluid with properties and behavior like those so magnificently described in the eighteenth and nineteenth centuries. Against this there are *practical* hydrodynamics and hydromechanics—the sophisticated recipe compendium originally called "hydraulics." Within this discipline one knows that every element corresponds to some observed phenomenon. But it is difficult to interrelate these observed phenomena, to see any rhyme or reason in the connections they do manifest, or even to formulate physically cogent questions, much less provide logically satisfactory answers. When practical aerodynamics was just getting off the ground, classical hydrodynamics was viewed as a mathematical toy. It was an elegant but empty discipline. Its perennial preamble was: "If there were an inviscid, nonresisting, and irrotational fluid, it would be observed to do the following things. . . ." Against this presentation the *facts* concerning what kind of fluids there really are must remain wholly irrelevant. The formal hydrodynamicist seems thus only to be working through the structure of an argument; plumbers and plummets are thus beside the logical point.

Still, a position with which we can all be sympathetic was adopted by practical hydrodynamical engineers and aerodynamicists. They had to learn about fluid media *de novo*, without any help from the lofty ivory towers of the theoreticians. How much more valuable the work within the Euler-Bernoulli tradition would have been had these thinkers immersed themselves somewhat in a

study of *what there is!* Their analyses would not have been made one whit more rigorous—or less rigorous—by so complicating their premises. But the results would have looked more like military strategy than like chess, more like physics than like pure algebra. By analogy, the analyses of the philosopher of science pick up nothing in rigor or elegance, nor do they lose, when the rubric: “If there were a science in which . . .” is dropped and the premises become instead: “Within experimental hydrodynamics it is observed that. . . .” But the illumination afforded by uncompromising philosophical analysis beginning with the sciences *as they really are* can be as rewarding as the efforts of Euler and Bernoulli would have been had their immense powers been turned on the *de facto* subject matter of hydrodynamics and not upon the properties of nonexistent ideal fluids. Surely, the arguments of philosophers can only gain in stature when directed at the conceptual perplexities and the perceptual complexities actually known to occur at the frontiers of science.

“Purified” logical and philosophical studies can be found throughout the literature, studies concerned with deciding between statistical hypotheses, the construction of models, the nature of theoretical terms, the verification and falsification of theories, the axiomatic rewriting of classical mechanics, etc.⁴ These intrinsically valuable exercises can only increase in their timely value when the author indicates that the *occasion* for his unflinching analysis is some flesh-and-blood perplexity possessed by physicists⁵ or some complexity encountered within chemical theory⁶ or some beastly ambiguity badgering biologists.⁷ That his “springboard” problems are *real* problems is, of course, no guarantee that the philosopher’s analysis will be cogent, sound, and valid. The latter must be assessed in terms invulnerable to any form of the “genetic fallacy.” Nonetheless, if a critic’s appraisal of the philosopher’s analysis is justified, it will remain justifiable whether or not the philosopher has chosen to begin with a *de facto* scientific problem rather than with some sundry suppositions about hypothetical sciences from which rigorous inferences are guaranteed.

⁴ Cf. Braithwaite, *Scientific Explanation*; Carnap, *Probability*; Reichenbach, *Philosophical Foundations of Quantum Mechanics*; Bunge *Metascientific Enquiries*; Hutten, *The Language of Physics*; Suppes and McKinsey, “Axiomatic Foundations of Classical Mechanics,” *British Journal for the Philosophy of Science*; and Popper, articles on Probability, *ibid.*

⁵ Cf. Scriven, “The Age of the Universe,” *British Journal for the Philosophy of Science*, 1954.

⁶ Cf. Denbigh, “The Direction of Time,” *ibid.*, 1952.

⁷ Pirie, “Concepts out of Context,” *Science News*, 25.

There are at least two ways of "cheating" in our examinations of western science. One way is to begin with the data and problems as they actually obtain or did obtain, but then, because of difficulties in generating analyses from such intricate and recalcitrant beginnings, to befog the result with clouds of facts. Ask about consistency, validity, or redundancy, conceptual connections, or the design of an hypothesis—and your answer comes back studded with quotes and dates.

The other "cheat" way is to secure a rigorous analysis and argument at any cost, even to the extent of adjusting the starting point so that it corresponds to no actual scientific problem.

The "hard" way—the *only* way—is to begin with an accurate description and delineation of some experimental or theoretical perplexity, one with which no historian of science could quarrel. This then would be subjected to a philosophical analysis characterized by a rigor that any logician might respect. As an ideal this may be unattainable. But it does possess maximum heuristic value. And in putting the matter thus we can at last demarcate the relationship between history of science and philosophy of science.

III

The logical relevance of history of science to philosophy of science is nil. Staring at novel facts has never made old arguments invalid, new arguments valid (or vice versa). Fresnel, Fizeau, and Foucault *did* prove that light was undulatory, despite the "granular" discoveries of Hertz and Einstein (photoelectric effect), Compton, and Raman. These later investigators did not show the "three Fs" to be wrong, but disclosed only that light is more complex than they had imagined. Similarly, Aristotle's analysis of Eudoxos' astronomy, the critiques Buridan and Oresme leveled at Aristotle's theory of motion, Gassendi's remarks about Osiander's preface to *De Revolutionibus Orbium Coelestium*, Berkeley's examination of infinitesimals and the idea of absolute space, Peirce's analysis of the discovery of Kepler's Laws, Duhem's account of some of Galileo's demonstrations, Mach's demolition of the classical concept of *mass*, Schlick, Feigl, and Grunbaum on relativity, Reichenbach and Feyerabend on microphysics . . . the internal validity of such philosophical studies of the sciences depends only on questions of logic and conceptual analysis. Historical data just cannot function legitimately in appraisals of the philosophical and logical acceptability of these great works.

But already a patent artificiality is clear from this anxious attempt to avoid the genetic fallacy. Schlick may not contradict

himself, and his arguments may be philosophically illuminating and conceptually enriching. But if he just doesn't have the facts about Special Relativity, its genesis, or its present state, Schlick's considerable insights must be adjudged somewhat sterile within the literature of philosophy of science. Had Duhem never read *Il Saggiatore*, had Peirce never opened *De Motibus Stellae Martis*, and had Berkeley never perused Newton's *Principia*, their works would strike us rather as do the crackpot's "proof" of a sixth dimension, the attempts of Soviet politicians to abrogate the Uncertainty relations, Lindsay's "disproof" of Einstein, and the Paduan philosopher's rejection of Galileo.

For a work in philosophy of science to be shot down by philosophers, it must at least get off the ground. This is done only via a runway of facts concerning the history and present state of the science with which the investigator is concerned. Such facts are not germane to the sophisticated professional appraisal of the intellectual flight and logical maneuvers demonstrated thereafter. But the philosopher of science who does not know intimately the history of the scientific problem with which he is exercised is not even airborne. His analytical skill may be admirable, but it does not take us anywhere.

So, history of science and philosophy of science are not logically related: to claim that they are would be either to underestimate or to misunderstand the genetic fallacy. But the risk of inferring that there is thus no connection at all between the two is the risk that philosophers of science may not know what they are talking about, a verdict none of us can accept silently.

NORWOOD RUSSELL HANSON

INDIANA UNIVERSITY

ON KNOWING, BELIEVING, THINKING *

I

ONE of the most important problems of contemporary analytic philosophy is that of giving a materially correct and theoretically sound analysis of sentences expressing belief, i.e., of sentences expressing that *person X believes that such and such*. By 'materially correct' is meant roughly that the analysis should reflect more or less accurately our common use and usage of the

* To be presented in a symposium on "Thoughts, Beliefs, and Intentions" at the fifty-ninth annual meeting of the American Philosophical Association, Eastern Division, December 28, 1962.