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**For K. M. K.,
still my favorite eschatologist**

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The Relations between the History and the Philosophy of Science

Previously unpublished Isenberg Lecture, delivered at Michigan State University, 1 March 1968; revised October 1976.

The subject on which I have been asked to speak today is the relations between the history and the philosophy of science. For me, more than for most, it has deep personal as well as intellectual significance. I stand before you as a practicing historian of science. Most of my students mean to be historians, not philosophers. I am a member of the American Historical, not the American Philosophical, Association. But for almost ten years after I first encountered philosophy as a college freshman, it was my primary avocational interest, and I often considered making it my vocation, displacing theoretical physics, the only field in which I can claim to have been properly trained. Throughout those years, which lasted until around 1948, it never occurred to me that history or history of science could hold the slightest interest. To me then, as to most scientists and philosophers still, the historian was a man who collects and verifies facts about the past and who later arranges them in chronological order. Clearly the production of chronicles could have little appeal to someone whose fundamental concerns were with deductive inference and fundamental theory.

I shall later ask why the image of the historian as chronicler has such special appeal to both philosophers and scientists. Its continued and selective attraction is not due either to coincidence or to the nature of history, and it may therefore prove especially revealing. But my present point is still autobiographical. What drew

me belatedly from physics and philosophy to history was the discovery that science, when encountered in historical source materials, seemed a very different enterprise from the one implicit in science pedagogy and explicit in standard philosophical accounts of scientific method. History might, I realized with astonishment, be relevant to the philosopher of science and perhaps also to the epistemologist in ways that transcended its classic role as a source of examples for previously occupied positions. It might, that is, prove to be a particularly consequential source of problems and of insights. Therefore, though I became a historian, my deepest interests remained philosophical, and in recent years those interests have become increasingly explicit in my published work. To an extent, then, I do both history and philosophy of science. Of course I therefore think about the relation between them, but I also live it, which is not the same thing. That duality of my involvement will inevitably be reflected in the way I approach today's topic. From this point my talk will divide into two quite different, though closely related parts. The first is a report, often quite personal, of the difficulties to be encountered in any attempt to draw the two fields closer together. The second, which deals with problems more explicitly intellectual, argues that the *rapprochement* is fully worth the quite special effort it requires.

Few members of this audience will need to be told that, at least in the United States, the history and the philosophy of science are separate and distinct disciplines. Let me, from the very start, develop reasons for insisting that they be kept that way. Though a new sort of dialogue between these fields is badly needed, it must be inter- not intra-disciplinary. Those of you aware of my involvement with Princeton University's Program in History and Philosophy of Science may find odd my insistence that there is no such field. At Princeton, however, the historians and the philosophers of science pursue different, though overlapping, courses of study, take different general examinations, and receive their degrees from different departments, either history or philosophy. What is particularly admirable in that design is that it provides an institutional basis for a dialogue between fields without subverting the disciplinary basis of either.

Subversion is not, I think, too strong a term for the likely result of an attempt to make the two fields into one. They differ in a number of their central constitutive characteristics, of which the most

general and apparent is their goals. The final product of most historical research is a narrative, a story, about particulars of the past. In part it is a description of what occurred (philosophers and scientists often say, a *mere* description). Its success, however, depends not only on accuracy but also on structure. The historical narrative must render plausible and comprehensible the events it describes. In a sense to which I shall later return, history is an explanatory enterprise; yet its explanatory functions are achieved with almost no recourse to explicit generalizations. (I may point out here, for later exploitation, that when philosophers discuss the role of covering laws in history, they characteristically draw their examples from the work of economists and sociologists, not of historians. In the writings of the latter, lawlike generalizations are extraordinarily hard to find.) The philosopher, on the other hand, aims principally at explicit generalizations and at those with universal scope. He is no teller of stories, true or false. His goal is to discover and state what is true at all times and places rather than to impart understanding of what occurred at a particular time and place.

Each of you will want to articulate and to qualify those crass generalizations, and some of you will recognize that they raise deep problems of discrimination. But few will feel that distinctions of this sort are entirely empty, and I therefore turn from them to their consequences. It is these that make the distinction of aims important. To say that history of science and philosophy of science have different goals is to suggest that no one can practice them both at the same time. But it does not suggest that there are also great difficulties about practicing them alternately, working from time to time on historical problems and attacking philosophical issues in between. Since I obviously aim at a pattern of that sort myself, I am committed to the belief that it can be achieved. But it is nonetheless important to recognize that each switch is a personal wrench, the abandonment of one discipline for another with which it is not quite compatible. To train a student simultaneously in both would risk depriving him of any discipline at all. Becoming a philosopher is, among other things, acquiring a particular mental set toward the evaluation both of problems and of the techniques relevant to their solution. Learning to be a historian is also to acquire a special mental set, but the outcome of the two learning experiences is not at all the same. Nor, I think, is a compromise possible,

for it presents problems of the same sort as a compromise between the duck and the rabbit of the well-known Gestalt diagram. Though most people can readily see the duck and the rabbit alternately, no amount of ocular exercise and strain will educe a duck-rabbit.

That view of the relation between enterprises is not at all the one I had at the time of my conversion to history twenty years ago. Rather it derives from much subsequent experience, sometimes painful, as a teacher and writer. In the former role I have, for example, repeatedly taught graduate seminars in which prospective historians and philosophers read and discussed the same classic works of science and philosophy. Both groups were conscientious and both completed the assignments with care, yet it was often difficult to believe that both had been engaged with the same texts. Undoubtedly the two had looked at the same signs, but they had been trained (programmed, if you will) to process them differently. Inevitably, it was the processed signs—for example their reading notes or their memory of the text—rather than the signs themselves that provided the basis for their reports, paraphrases, and contributions to discussion.

Subtle analytic distinctions that had entirely escaped the historians would often be central when the philosophers reported on their reading. The resulting confrontations were invariably educational for the historians, but the fault was not always theirs. Sometimes the distinctions dwelt upon by the philosophers were not to be found at all in the original text. They were products of the subsequent development of science or philosophy, and their introduction during the philosophers' processing of signs altered the argument. Or again, listening to the historians' paraphrase of a position, the philosophers would often point out gaps and inconsistencies that the historians had failed to see. But the philosophers could then sometimes be shocked by the discovery that the paraphrase was accurate, that the gaps were there in the original. Without quite knowing they were doing so, the philosophers had improved the argument while reading it, knowing what its subsequent form must be. Even with the text open before them it was regularly difficult and sometimes impossible to persuade them that the gap was really there, that the author had not seen the logic of the argument quite as they did. But if the philosophers could be brought to see that much, they could usually see something more important as well—that what they took to be gaps had in fact been introduced by

analytic distinctions they had themselves supplied, that the original argument, if no longer viable philosophy, was sound in its own terms. At this point the whole text might begin to look different to them. Both the extent of the transformation and the pedagogic difficulty in deliberately bringing it about are reminiscent of the Gestalt switch.

Equally impressive, as evidence of different processing, was the range of textual material noticed and reported by the two groups. The historians always ranged more widely. Important parts of their reconstructions might, for example, be built upon passages in which the author had introduced a metaphor designed, he said, "to aid the reader." Or again, having noticed an apparent error or inconsistency in the text, the historian might spend some time explaining how a brilliant man could have slipped in this way. What aspect of the author's thought, the historian would ask, can be discovered by noting that an inconsistency obvious to us was invisible to him and was perhaps no inconsistency at all? For the philosophers, trained to construct an argument, not to reconstruct historical thought, both metaphors and errors were irrelevant and were sometimes not noticed at all. Their concern, which they pursued with a subtlety, skill, and persistence seldom found among the historians, was the explicit philosophical generalization and the arguments that could be educed in its defense. As a result, the papers they submitted at the end of the term were regularly shorter and usually far more coherent than those produced by the historians. But the latter, though often analytically clumsy, usually came far closer to reproducing the major conceptual ingredients in the thought of the men the two groups had studied together. The Galileo or Descartes who appeared in the philosophers' papers was a better scientist or philosopher but a less plausible seventeenth-century figure than the figure presented by the historians.

I have no quarrel with either of these modes of reading and reporting. Both are essential components as well as central products of professional training. But the professions are different, and they quite properly put different first things first. For the philosophers in my seminars the priority tasks were, first, to isolate the central elements of a philosophical position and, then, to criticize and develop them. Those students were, if you will, honing their wits against the developed opinions of their greatest predecessors. Many of them would continue to do so in their later professional life. The

historians, on the other hand, were concerned with the viable and the general only in the forms that had, in fact, guided the men they studied. Their first concern was to discover what each one had thought, how he had come to think it, and what the consequences had been for him, his contemporaries, and his successors. Both groups thought of themselves as attempting to grasp the essentials of a past philosophical position, but their ways of doing the job were conditioned by the primary values of their separate disciplines, and their results were often correspondingly distinct. Only if the philosophers were converted to history or the historians to philosophy did additional work produce significant convergence.

A quite different sort of evidence of a deep interdisciplinary divide depends upon testimony so personal that it may convince only its author. Nevertheless, because the experience from which it derives is comparatively rare, the testimony seems worth recording. I have myself, at various times, written articles in physics, in history, and in something resembling philosophy. In all three cases the process of writing proves disagreeable, but the experience is not in other respects the same. By the time one begins to write a physics paper, the research is finished. Everything one needs is ordinarily contained in one's notes. The remaining tasks are selection, condensation, and translation to clear English. Usually only the last presents difficulties, and they are not ordinarily severe.

The preparation of a historical paper is different, but there is one important parallel. A vast amount of research has to be done before one begins to write. Books, documents, and other records must be located and examined; notes must be taken, organized, and organized again. Months or years may go into work of this sort. But the end of such work is not, as it is in science, the end of the creative process. Selected and condensed notes cannot simply be strung together to make a historical narrative. Furthermore, though chronology and narrative structure usually permit the historian to write steadily from notes and an outline for a considerable period, there are almost always key points at which his pen or typewriter refuses to function and his undertaking comes to a dead stop. Hours, days, or weeks later he discovers why he has been unable to proceed. Though his outline tells him what comes next, and though his notes provide all requisite information about it, there is no viable transition to that next part of the narrative from the point at which he has already arrived. Elements essential to the connection have been omitted from an earlier part of his story because at that point the narrative structure did not demand them. The historian must therefore go back, sometimes to documents and notetaking, and rewrite a substantial part of his paper in order that the connection to what comes next may be made. Not until the last page is written can he be altogether sure that he will not have to start again, perhaps from the very beginning.

Only the last part of this description applies to the preparation of an article in philosophy, and there the periods of circling back are far more frequent and the concomitant frustrations far more intense. Only the man whose memory span permits him to compose a whole paper in his head can hope for long periods of uninterrupted composition. But if the actual writing of philosophy shows some parallels to history, what comes before is altogether distinct. Excepting in the history of philosophy and perhaps in logic, there is nothing like the historian's period of preparatory research; in the literal sense there is in most of philosophy no equivalent for research at all. One starts with a problem and a clue to its solution, both often encountered in the criticism of the work of some other philosopher. One worries it—on paper, in one's head, in discussions with colleagues—waiting for the point at which it will feel ready to be written down. More often than not that feeling proves mistaken, and the worrying process begins again, until finally the article is born. To me, at least, that is what it feels like, as though the article had come all at once, not seriatim like the pieces of historical narrative.

If, however, there is nothing quite like research in philosophy, there is something else that takes its place and that is virtually unknown in physics and in history. Considering it will take us back directly to the differences between the perceptions and behaviors of the two groups of students in my seminars. Philosophers regularly criticize each other's work and the work of their predecessors with care and skill. Much of their discussion and publication is in this sense Socratic: it is a juxtaposition of views forged from each other through critical confrontation and analysis. The critic who proclaimed that philosophers live by taking in each other's washing was unsympathetic, but he caught something essential about the enterprise. What he caught was, in fact, what the philosophers in my seminars were doing: forging their own positions by an analytic confrontation with, in this case, the past. In no other field, I think,

does criticism play so central a role. Scientists sometimes correct bits of each other's work, but the man who makes a career of piecemeal criticism is ostracized by the profession. Historians, too, sometimes suggest corrections, and they also occasionally direct diatribes at competing schools whose approach to history they disdain. But careful analysis is, in those circumstances, rare, and an explicit attempt to capture and preserve the novel insights generated by the other school is almost unknown. Though influenced in extremely important ways by the work of his predecessors and his colleagues, the individual historian, like the physicist and unlike the philosopher, forges his work from primary source material, from data that he has engaged in his research. Criticism may take the place of research, but the two are not equivalent, and they produce disciplines of very different sorts.

These are only first steps in a quasi-sociological account of history and philosophy as knowledge-producing enterprises. They should, however, be sufficient to suggest why, admiring both, I suspect that an attempt to make them one would be subversive. Those whom I have convinced or those who, for one or another reason, have needed no convincing will, however, have a different question. Given the deep and consequential differences between the two enterprises, what can they have to say to each other? Why have I insisted that an increasingly active dialogue between them is an urgent desideratum? To that question, particularly to one part of it, the remainder of my remarks this evening are directed.

Any answer must divide into two far-from-symmetrical parts, of which the first here requires no more than cursory summary. Historians of science need philosophy for reasons that are, at once, apparent and well known. For them it is a basic tool, like knowledge of science. Until the end of the seventeenth century, much of science was philosophy. After the disciplines separated, they continued to interact in often consequential ways. A successful attack on many of the problems central to the history of science is impossible for the man who does not command the thought of the main philosophic schools of the periods and areas he studies. Furthermore, since it is utopian to expect that any student of the history of science will emerge from graduate school with a command of the entire history of philosophy, he must learn to work this sort of material up for himself as his research requires it. The same holds true for some of the science he will need, and to both areas

he must first be initiated by professionals, the men who know the subtleties and the traps of their disciplines and who can inculcate standards of professional acumen, skill, and rigor. There is no reason of principle why the historians in my seminars should have been clumsy when dealing with philosophical ideas. Given adequate prior training, most of them would not have been. Nor would the effects of such training have been limited to their performance when dealing with philosophical sources as such. Scientists are not often philosophers, but they do deal in ideas, and the analysis of ideas has long been the philosopher's province. The men who did most to establish the flourishing contemporary tradition in the history of science—I think particularly of A. O. Lovejoy and, above all, Alexandre Koyré—were philosophers before they turned to the history of scientific ideas. From them my colleagues and I learned to recognize the structure and coherence of idea systems other than our own. That search for the integrity of a discarded mode of thought is not what philosophers generally do; many of them, in fact, reject it as the glorification of past error. But the job can be done, and the philosopher's sensitivity to conceptual nuances is prerequisite to it. I cannot think that historians have learned their last lessons from this source.

These are sufficient reasons to urge the revivification of a more vigorous interaction between philosophers and historians of science, but they are also question begging. My assignment was the relation of the history of science to philosophy of science rather than to the history of philosophy. Can the historian of science also profit from a deep immersion in the literature of that special philosophical field? I have to answer that I very much doubt it. There have been philosophers of science, usually those with a vaguely neo-Kantian cast, from whom historians can still learn a great deal. I do urge my students to read Emile Meyerson and sometimes Léon Brunschvicg. But I recommend these authors for what they saw in historical materials not for their philosophies, which I join most of my contemporaries in rejecting. The living movements in philosophy of science, on the other hand, particularly as the field is currently practiced in the English-speaking world, include little that seems to me relevant to the historian. On the contrary, these movements aim at goals and perceive materials in ways more likely to mislead than to illuminate historical research. Though there is much about them that I admire and value, that is because my own

concerns are by no means exclusively historical. No one in recent years has done so much to clarify and deepen my consideration of philosophical problems as my Princeton colleague C. G. Hempel. But my discourse with him and my acquaintance with his work does nothing for me at all when I work on, say, the history of thermodynamics or of the quantum theory. I commend his courses to my history students, but I do not especially urge that they enroll.

Those remarks will suggest what I had in mind in saying that the problem of the relations between history and philosophy of science divides into two parts, which are far from symmetrical. Though I do not think current philosophy of science has much relevance for the historian of science, I deeply believe that much writing on philosophy of science would be improved if history played a larger background role in its preparation. Before attempting to justify that belief, I must, however, introduce a few badly needed limitations. When speaking here of the history of science, I refer to that central part of the field that is concerned with the evolution of scientific ideas, methods, and techniques, not the increasingly significant portion that emphasizes the social setting of science, particularly changing patterns of scientific education, institutionalization, and support, both moral and financial. The philosophical import of the latter sort of work seems to me far more problematic than that of the former, and its consideration would, in any case, require a separate lecture. By the same token, when speaking of the philosophy of science, I have in mind neither those portions that shade over into applied logic nor, at least not with much assurance, those parts that are addressed to the implications of particular current theories for such longstanding philosophical problems as causation or space and time. Rather I am thinking of that central area that concerns itself with the scientific in general, asking, for example, about the structure of scientific theories, the status of theoretical entities, or the conditions under which scientists may properly claim to have produced sound knowledge. It is to this part of the philosophy of science, and very possibly to it alone, that the history of scientific ideas and techniques may claim relevance.

To suggest how this could be so, let me first point out a respect in which philosophy of science is almost unique among recognized philosophical specialties: the distance separating it from its subject matter. In fields like logic and, increasingly, the philosophy of mathematics, the problems that concern the professional are generated by the field itself. The difficulties of reconciling material implication with the "if . . . then" relation of normal discourse may be a reason for seeking alternative systems of logic, but it does not reduce the importance or fascination of the problems generated by standard axiom systems. In other parts of philosophy, most notably ethics and aesthetics, practitioners address themselves to experiences which they share with vast portions of humanity and which, are not, in any case, the special preserves of clearly demarcated professional groups. Though only the philosopher may be an aesthetician, the aesthetic experience is every man's. The philosophies of science and law are alone in addressing themselves to areas about which the philosopher *qua* philosopher knows little. And philosophers of law are far more likely than philosophers of science to have received significant professional training in their subject-matter field and to concern themselves with the same documents as the men about whose field they speak. That, I take it, is one reason why judges and lawyers read philosophy of law with far more regularity than scientists read philosophy of science.

My first claim, then, is that history of science can help to bridge the quite special gap between philosophers of science and science itself, that it can be for them a source of problems and of data. I do not, however, suggest that it is the only discipline that can do so. Actual experience in the practice of a science would probably be a more effective bridge than the study of its history. Sociology of science, if it ever develops sufficiently to embrace the cognitive content of science together with its organizational structure, might do as well. The historian's concern with development over time and the additional perspective available when studying the past may give history special advantages, to the first of which I shall later return. But my present point is only that history provides the most practical and available among several possible methods by which the philosopher might more closely acquaint himself with science.

Against this suggestion there is available a considerable arsenal. Some will argue that the gap, if unfortunate, does no great harm. Many more will insist that history cannot possibly supply a corrective. The part of philosophy of science currently under discussion does not, after all, direct itself to any particular scientific theory, except occasionally as illustrative. Its objective is theory in general.

Unlike history, furthermore, it is comparatively little concerned with the temporal development of theory, emphasizing instead the theory as a static structure, an example of sound knowledge at some particular, though unspecified, time and place. Above all, in philosophy of science, there is no role for the multitude of particulars, the idiosyncratic details, which seem to be the stuff of history. Philosophy's business is with rational reconstruction, and it need preserve only those elements of its subject essential to science as sound knowledge. For that purpose, it is argued, the science contained in college textbooks is adequate if not ideal. Or at least it is adequate if supplemented by an examination of a few scientific classics, perhaps Galileo's *Two New Sciences* together with the "Introduction" and "General Scholium" from Newton's *Principia*.

Having previously insisted that history and philosophy of science have very different goals, I can have no quarrel with the thesis that they may appropriately work from different sources. The difficulty, however, with the sorts of sources just examined is that, working from them, the philosopher's reconstruction is generally unrecognizable as science to either historians of science or to scientists themselves (excepting perhaps social scientists, whose image of science is drawn from the same place as the philosopher's). The problem is not that the philosopher's account of theory is too abstract, too stripped of details, too general. Both historians and scientists can claim to discard as much detail as the philosopher, to be as concerned with essentials, to be engaged in rational reconstruction. Instead the difficulty is the identification of essentials. To the philosophically minded historian, the philosopher of science often seems to have mistaken a few selected elements for the whole and then forced them to serve functions for which they may be unsuited in principle and which they surely do not perform in practice, however abstractly that practice be described. Though both philosophers and historians seek the essentials, the results of their search are by no means the same.

This is not the place to enumerate missing ingredients. Many of them are, in any case, discussed in my earlier work. But I do want to suggest what it is about history that makes it a possible source for a rational reconstruction of science different from that now current. For that purpose, furthermore, I must first insist that history is not itself the enterprise much contemporary philosophy takes it to be. I must, that is, argue briefly the case for what Louis Mink has perceptively called "the autonomy of historical understanding."

No one, I think, still believes that history is mere chronicle, a collection of facts arranged in the order of their occurrence. It is, most would concede, an explanatory enterprise, one that induces understanding, and it must thus display not only facts but also connections between them. No historian has, however, yet produced a plausible account of the nature of these connections, and philosophers have recently filled the resulting void with what is known as the "covering law model." My concern with it is as an articulated version of a widely diffused image of history, one that makes the discipline seem uninteresting to those who seek lawlike generalizations, philosophers, scientists, and social scientists in particular.

According to proponents of the covering law model, a historical narrative is explanatory to the extent that the events it describes are governed by laws of nature and society to which the historian has conscious or unconscious access. Given the conditions that obtained at the point in time when the narrative opens, and given also a knowledge of the covering laws, one should be able to predict, perhaps with the aid of additional boundary conditions inserted along the way, the future course of some central parts of the narrative. It is these parts, and only these, that the historian may be said to have explained. If the laws permit (only) rough predictions, one speaks of having provided an "explanation sketch" rather than an explanation. If they permit no prediction at all, the narrative has provided no explanation.

Clearly the covering law model has been drawn from a theory of explanation in the natural sciences and applied to history. I suggest that, whatever its merits in the fields for which it was first developed, it is an almost total misfit in this application. Very likely there are or will be laws of social behavior capable of application to history. As they come into being, historians sooner or later use them. But laws of that sort are primarily the business of the social sciences, and except in economics very few are yet in hand. I have already pointed out that philosophers turn generally to writings by social scientists for the laws they attribute to historians. I now add that, when they do draw examples from historical writing, the laws they educe are at once obvious and dubious: for example, "Hungry men tend to riot." Probably, if the

words "tend to" are heavily underscored, the law is valid. But does it follow that an account of starvation in eighteenth-century France is less essential to a narrative dealing with the first decade of the century, when there were no riots, than to one dealing with the last, when riots did occur?

Surely the plausibility of a historical narrative does not depend upon the power of a few scattered and doubtful laws like this one. If it did, then history would explain virtually nothing at all. With few exceptions, the facts that fill the pages of its narratives would be mere window dressing, facts for the sake of facts, unconnected to each other or to any larger goal. Even the few facts actually connected by law would become uninteresting, for precisely to the extent that they were "covered," they would add nothing to what everyone already knew. I am not claiming, let me be clear, that the historian has access to no laws and generalizations, nor that he should make no use of them when they are at hand. But I do claim that, however much laws may add substance to an historical narrative, they are not essential to its explanatory force. That is carried, in the first instance, by the facts the historian presents and the manner in which he juxtaposes them.

During my days as a philosophically inclined physicist, my view of history resembled that of the covering law theorists, and the philosophers in my seminars usually begin by viewing it in a similar way. What changed my mind and often changes their's is the experience of putting together a historical narrative. That experience is vital, for the difference between learning history and doing it is far larger than that in most other creative fields, philosophy certainly included. From it I conclude, among other things, that an ability to predict the future is no part of the historian's arsenal. He is neither a social scientist nor a seer. It is no mere accident that he knows the end of his narrative as well as the start before he begins to write. History cannot be written without that information. Though I have no alternate philosophy of history or of historical explanation to offer here, I can at least outline a better image of the historian's task and suggest why its performance might produce a sort of understanding.

The historian at work is not, I think, unlike the child presented with one of those picture puzzles of which the pieces are square; but the historian is given many extra pieces in the box. He has or can get the data, not all of them (what would that be?) but a very

considerable collection. His job is to select from them a set that can be juxtaposed to provide the elements of what, in the child's case, would be a picture of recognizable objects plausibly juxtaposed and of what, for the historian and his reader, is a plausible narrative involving recognizable motives and behaviors. Like the child with the puzzle, the historian at work is governed by rules that may not be violated. There may be no empty spaces in the middle either of the puzzle or of the narrative. Nor may there be any discontinuities. If the puzzle displays a pastoral scene, the legs of a man may not be joined to the body of a sheep. In the narrative a tyrannical monarch may not be transformed by sleep alone to a benevolent despot. For the historian there are additional rules that do not apply to the child. Nothing in the narrative may, for example, do violence to the facts the historian has elected to omit from his story. That story must, in addition, conform to any laws of nature and society the historian knows. Violation of rules like these is ground for rejecting either the assembled puzzle or the historian's narrative.

Such rules, however, only limit but do not determine the outcome of either the child's or the historian's task. In both cases the basic criterion for having done the job right is the primitive recognition that the pieces fit to form a familiar, if previously unseen, product. The child has seen pictures, the historian behavior patterns, similar to these before. That recognition of similarity is, I believe, prior to any answers to the question, similar with respect to what? Though it can be rationally understood and perhaps even modelled on a computer (I once attempted something of the sort myself), the similarity relation does not lend itself to lawlike reformulation. It is global, not reducible to a unique set of prior criteria more primitive than the similarity relation itself. One may not replace it with a statement of the form. "*A* is similar to *B*, if and only if the two share the characteristics *c*, *d*, *e*, and *f*." I have elsewhere argued that the cognitive content of the physical sciences is in part dependent on the same primitive similarity relation between concrete examples, or paradigms, of successful scientific work, that scientists model one problem solution on another without at all knowing what characteristics of the original must be preserved to legitimate the process. Here I am suggesting that in history that obscure global relationship carries virtually the entire burden of connecting fact. If history is explanatory, that is not because its

narratives are covered by general laws. Rather it is because the reader who says, "Now I know what happened," is simultaneously saying, "Now it makes sense; now I understand; what was for me previously a mere list of facts has fallen into a recognizable pattern." I urge that the experience he reports be taken seriously.

What has just been said is, of course, the early stage of a program for philosophical contemplation and research, not yet the solution of a problem. If many of you differ with me about its likely outcome, that is not because you are more aware than I of its incompleteness and difficulty, but because you are less convinced that the occasion demands so radical a break with tradition. That point, however, I shall not argue here. The object of the digression from which I now return has been to identify my convictions, not to defend them. What has troubled me about the covering law model is that it makes of the historian a social scientist *manqué*, the gap being filled by assorted factual details. It makes it hard to recognize that he has another and a profound discipline of his own, that there is an autonomy (and integrity) of historical understanding. If that claim now seems even remotely plausible, it prepares the way for my principal conclusion. When the historian of science emerges from the contemplation of sources and the construction of narrative, he may have a right to claim acquaintance with essentials. If he then says, "I cannot construct a viable narrative without giving a central place to aspects of science that philosophers ignore, nor can I find a trace of elements they consider essential," then he deserves an audience. What he is claiming is that the enterprise reconstructed by the philosopher is not, as to certain of its essentials, science.

What sort of lessons might the philosopher learn by taking the historian's narrative constructions more seriously? I shall close this lecture with a single global example, referring you to my earlier work for other illustrations, many of them dependent on the examination of individual cases. The overwhelming majority of historical work is concerned with process, with development over time. In principle, development and change need not play a similar role in philosophy, but in practice, I now want to urge, the philosopher's view of even static science, and thus of such questions as theory structure and theory confirmation, would be fruitfully altered if they did.

Consider, for example, the relation between empirical laws and theories, both of which I shall, for purposes of this brief conclusion, construe quite broadly. Despite real difficulties, which I have elsewhere perhaps overemphasized, empirical laws fit the received tradition in philosophy of science relatively well. They can, of course, be confronted directly with observation or experiment. More to my present point, when they first emerge, they fill an apparent gap, supplying information that was previously lacking. As science develops, they may be refined, but the original versions remain approximations to their successors, and their force is therefore either obvious or readily recaptured. Laws, in short, to the extent that they are purely empirical, enter science as net additions to knowledge and are never thereafter entirely displaced. They may cease to be of interest and therefore remain uncited, but that is another matter. Important difficulties do, I repeat, confront the elaboration of this position, for it is no longer clear just what it would be for a law to be purely empirical. Nevertheless, as an admitted idealization, this standard account of empirical laws fits the historian's experience quite well.

With respect to theories the situation is different. The tradition introduces them as collections or sets of law. Though it concedes that individual members of a set can be confronted with experience only through the deductive consequences of the set as a whole, it thereafter assimilates theories to laws as closely as possible. That assimilation does not fit the historian's experience at all well. When he looks at a given period in the past he can find gaps in knowledge later to be filled by empirical laws. The ancients knew that air was compressible but were ignorant of the regularity that quantitatively relates its volume and pressure; if asked, they would presumably have conceded the lack. But the historian seldom or never finds similar gaps to be filled by later theory. In its day, Aristotelian physics covered the accessible and imaginable world as completely as Newtonian physics later would. To introduce the latter, the former had to be literally displaced. After that occurred, furthermore, efforts to recapture Aristotelian theory presented difficulties of a very different nature from those required to recapture an empirical law. Theories, as the historian knows them, cannot be decomposed into constituent elements for purposes of direct comparison either with nature or with each other. That is not to say

that they cannot be analytically decomposed at all, but rather that the lawlike parts produced by analysis cannot, unlike empirical laws, function individually in such comparisons.

A central tenet of Aristotle's physics was, for example, the impossibility of a void. Suppose that a modern physicist had told him that an arbitrarily close approximation to a void could now be produced in the laboratory. Probably Aristotle would have responded that a container emptied of air and other gases was not in his sense a void. That response would suggest that the impossibility of a void was not, in his physics, a merely empirical matter. Suppose now instead that Aristotle had conceded the physicist's point and announced that a void could, after all, exist in nature. Then he would have required a whole new physics, for his concept of the finite cosmos, of place within it, and of natural motion stand or fall together with his concept of the void. In that sense, too, the lawlike statement "there are no voids in nature" did not function within Aristotelian physics quite as a law. It could not, that is, be eliminated and replaced by an improved version, leaving the rest of the structure standing.

For the historian, therefore or at least for this one, theories are in certain essential respects holistic. So far as he can tell, they have always existed (though not always in forms one would comfortably describe as scientific), and they then always cover the entire range of conceivable natural phenomena (though often without much precision). In these respects they are clearly unlike laws, and there are inevitably corresponding differences in the ways they develop and are evaluated. About these latter processes we know very little, and we shall not learn more until we learn properly to reconstruct selected theories of the past. As of today, the people taught to do that job are historians, not philosophers. Doubtless the latter could learn, but in the process, as I have suggested, they would likely become historians too. I would of course welcome them, but would be saddened if they lost sight of their problems in the transition, a risk that I take to be real. To avoid it I urge that history and philosophy of science continue as separate disciplines. What is needed is less likely to be produced by marriage than by active discourse.

2

Concepts of Cause in the Development of Physics

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Why should a historian of science be invited to address an audience of child psychologists on the development of causal notions in physics? A first answer is well known to all who are acquainted with the researches of Jean Piaget. His perceptive investigations of such subjects as the child's conception of space, of time, of motion, or of the world itself have repeatedly disclosed striking parallels to the conceptions held by adult scientists of an earlier age. If there are similar parallels in the case of the notion of cause, their elucidations should be of interest both to the psychologist and to the historian.

There is, however, also a more personal answer, perhaps applicable only to this historian and this group of child psychologists. Almost twenty years ago I first discovered, very nearly at the same time, both the intellectual interest of the history of science and the psychological studies of Jean Piaget. Ever since that time the two have interacted closely in my mind and in my work. Part of what I know about how to ask questions of dead scientists has been learned by examining Piaget's interrogations of living children. I vividly remember how that influence figured in my first meeting with Alexandre Koyré, the man who, more than any other historian, has been my *maître*. I said to him that it was Piaget's children from whom I had learned to understand Aristotle's physics. His response—that it was Aristotle's physics that had taught him