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**Marc Trachtenberg: The Craft of International History**

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## Chapter One

# THE THEORY OF HISTORICAL INQUIRY

THIS IS A BOOK about method. It's about the techniques historians use to understand international politics. But issues of method cannot be dealt with in a vacuum. To see *how* historical work needs to be done, you first have to have some sense for *what* it is exactly that historians should be trying to do. What's the aim of historical analysis? What's the point of this whole branch of intellectual activity? These questions are of fundamental importance, even in practical terms. To understand the goal of historical work—to know what historical understanding is and what historical explanation is—can be of great value to the working historian. That knowledge can serve as a kind of beacon. It can help the historian see how to proceed.

Does the philosophy of history literature provide historians with the guidance they need? This question is the focus of the first two sections in this chapter, but in a word the answer is no. Does that mean that the philosophers have nothing much to offer the historians? Again, the answer is no. There are important insights available, but they are to be found in the philosophy of *science* literature. That these writings are of real value to the practicing historian is the point of the argument in the final section of this chapter. In that section I want to draw out some of the insights to be found in that literature and show how they apply to historical work.

### THE CLASSIC TRADITION: HEMPEL VERSUS COLLINGWOOD

In 1942 the philosopher Carl Hempel published a paper called “The Function of General Laws in History” in which he laid out a theory of historical explanation.<sup>1</sup> In history as in science, Hempel said, explanation meant deduction. An explanation would show that certain initial conditions existed and would lay out general laws that governed what would happen if those conditions were met; the occurrence of the event in question would follow as

<sup>1</sup> Carl Hempel, “The Function of General Laws in History,” *Journal of Philosophy* 39 (1942): 35–48; reprinted in Patrick Gardiner, ed., *Theories of History* (New York: Free Press, 1959), pp. 344–56. Further references are to the text as it appeared in the Gardiner book.

a matter of course from those laws and those initial conditions. Unless a historical account had that form, Hempel wrote, that account could not be considered a real explanation. It would at best be a mere “explanation sketch.” This theory of explanation, the “covering law” theory as it is often called, was a focus of philosophical discussion until about 1970.<sup>2</sup> Indeed, as one leading scholar noted, the Hempel paper was so fundamental that most participants in the debate on historical explanation “quickly found themselves classified as either pro-Hempel or anti-Hempel.”<sup>3</sup>

This theory was attractive because it appealed to people’s sense for what an explanation should be. If an account does not explain why an event *had* to happen, if it simply explains why it *might* have happened, then, in a certain sense, it is not a real explanation at all. As one leading philosopher of history put it: “If what we give in explanation of an event does not rule out the possibility of that event’s failing to occur, then we can scarcely claim that we know why in that particular case it *did* occur: why in that case, in other words, the possibility of its not occurring was not realized instead. The only way we can rule out such a possibility is by arguing that the event *had* to occur: that it *necessarily* occurred. And that is what the deductive requirement of scientific explanation insures.”<sup>4</sup>

This point, however, carried little weight with most historians. Their feeling was that the Hempel approach was abstract and formalistic and did not take actual historical practice as its point of departure. It did not look at what explanation meant to the historian and then try to build out from there. Hempel, with his emphasis on social scientific “laws,” would force interpretation into much too rigid a mold. He did not seem to have any real feel for

<sup>2</sup> See William Dray, *Laws and Explanation in History* (Oxford: Oxford University Press, 1957). Alan Donagan characterized this view as the “Popper-Hempel” theory. Alan Donagan, “The Popper-Hempel Theory Reconsidered,” *History and Theory* 1 (1964): 3–26, reprinted with minor changes in *Philosophical Analysis and History*, ed. William Dray (New York: Harper and Row, 1966), pp. 127–59; further references are to this latter version of the article. It should be noted, however, that by the time this article came out, Popper was very critical of what he called “historicism,” which he defined as “an approach to the social sciences which,” among other things, was concerned with discovering the laws “that underlie the evolution of history.” Karl Popper, *The Poverty of Historicism* (London: Routledge, 1961), p. 3. He later went out of his way to praise Donagan “for fighting this danger [of historicism] so forcefully.” Paul A. Schilpp, ed., *The Philosophy of Karl Popper*, 2 vols. (La Salle, Ill.: Open Court, 1974), 2:1174. The term “historicism,” of course, has been used in many different ways. See Georg Iggers, *The German Conception of History* (Middletown, Conn.: Wesleyan University Press, 1968), pp. 287–90 (or pp. 295–98 in the revised edition published in 1983), and the references cited there.

<sup>3</sup> William Dray, *On History and Philosophers of History* (Leiden: Brill, 1989), p. 13. See also Donagan, “Popper-Hempel Theory,” p. 127.

<sup>4</sup> William Dray, *Philosophy of History* (Englewood Cliffs, N.J.: Prentice-Hall, 1964), pp. 6–7 (emphasis in original). Dray was here paraphrasing another philosopher’s argument.

history as a discipline with an intellectual personality of its own.<sup>5</sup> And a number of philosophers sympathized with the view that standards were not to be arbitrarily imposed on the discipline from the outside.<sup>6</sup> They rejected the idea that what could not be “cut down to analytic size” in terms of those standards was to be “stripped of the epaulets of cognitive honor” and agreed that a field like history was to be taken essentially on its own terms.<sup>7</sup> Their feeling was, as one of them put it, that the social sciences in general and history in particular were not to be remodeled into “deformed likenesses of physics.”<sup>8</sup> And they sympathized with the historians’ view that the covering-law approach was unacceptable because it failed to allow for human agency—for the role that individual human beings play in shaping the course of events.<sup>9</sup>

Those philosophers, moreover, were able to show that the Hempel theory was not particularly impressive, even on its own terms. Alan Donagan, for example, in one section of his well-known article on the “Popper-Hempel theory,” effectively demolished Hempel’s assumption that covering laws were readily available. Among other things, he showed that one example Hempel had given in his original article—an explanation drawing on three explicit covering laws—did not hold up because “all three were obviously false!”<sup>10</sup> A more basic problem was that Hempel, by his own admission, did not even purport to show what an explanation *was*. All he did was to point to one of the things that an explanation of an event in his view had to be. It needed, he said, to provide a *sufficient* basis for expecting that that event had occurred. The problem here, as he himself pointed out, was that “certain kinds of information”—the “results of a scientific test,” for example—might provide a sufficient basis for believing that some event had occurred “without in the least explaining why.”<sup>11</sup> A certain barometric reading might predict a worsening of the weather, but it could scarcely be said to cause the change in atmospheric conditions. Predictive power was just not enough for something to qualify as a real explanation. Something more was needed, but what? This was a fundamental problem, but Hempel essentially walked away from it.

<sup>5</sup> See especially J. H. Hexter, “The One That Got Away,” *New York Review of Books*, February 9, 1967.

<sup>6</sup> See, for example, Dray, *Laws and Explanation in History*, p. 12, and Alan Donagan, “Can Philosophers Learn from Historians?” in *Mind, Science, and History*, ed. Howard Kiefer and Milton Munitz (Albany: State University of New York Press, 1970), p. 244.

<sup>7</sup> Marvin Levich, “Interpretation in History,” *History and Theory* 24 (1985): 61.

<sup>8</sup> Donagan, “Popper-Hempel Theory,” p. 157.

<sup>9</sup> William Dray, “The Historical Explanation of Actions Reconsidered,” in *Philosophy and History: A Symposium*, ed. Sidney Hook (New York: New York University Press, 1963), p. 133.

<sup>10</sup> Donagan, “Popper-Hempel Theory,” p. 142.

<sup>11</sup> Carl Hempel, “Reasons and Covering Laws in Historical Explanation,” in Hook, *Philosophy and History*, p. 146. See also N. R. Hanson, *Observation and Explanation: A Guide to Philosophy of Science* (New York: Harper, 1971), pp. 39–49, esp. pp. 42, 48–49.

This does not mean that the sort of thinking represented by the Hempel article is devoid of practical value. The Hempel approach might have been overly rigid in the reliance it placed on social scientific laws, but (as will be seen) the argument that causal explanation is closely related to logical deduction is in fact quite important. And the Hempel approach does shed some light on some second-order issues. Hempel's point, for example, that explanation and prediction are cognate concepts—that to explain an event is to be able to predict, given some general principles and certain particular conditions, that that event would occur—translates into an important point of method.<sup>12</sup> At any given point in a historical argument, the historian can ask, given what was said up to that point, whether it would be possible to predict how things would develop. This provides a useful test of the *power* of the argument: a strong interpretation should have a certain predictive force. An interpretation, moreover, generates expectations: if it is valid, then what else would one expect to find? Consciously or unconsciously, the historian will be making predictions about what as yet unexamined sources would reveal, and those predictions can provide a useful yardstick for judging the *validity* of the argument.

So the Hempel tradition is not to be dismissed out of hand. The fact remains, however, that on the central issues the practicing historian will not find much of value here. But this was not the only approach the philosophers of history were able to come up with. There was, in fact, one basic alternative to the Hempel doctrine, the approach associated with the British philosopher R. G. Collingwood. Indeed, in the philosophy of history literature in the 1950s and 1960s, Collingwood's ideas were often treated as the only real alternative to Hempel's. But did this alternative approach give the historians what they needed?

The Collingwood theory was quite extraordinary. According to Collingwood, the historian was concerned not with events as such but with actions—that is, with “events brought about by the will and expressing the thought of a free and intelligent agent.” The historian, he said, “discovers this thought by rethinking it in his own mind.” The “reliving of past experiences” through the “rethinking” of past thought: this for Collingwood was what history was about, and this was what historical explanation amounted to. “An historical fact once genuinely ascertained,” he argued, “grasped by the historian's re-enactment of the agent's thought in his own mind, is already explained. For the historian there is no difference between discovering what happened and

<sup>12</sup> Hempel, “Function of General Laws,” pp. 347–48. Popper argued along similar lines. See Karl Popper, *The Open Society and Its Enemies*, 4th ed. (London: Routledge, 1962), 2:262. The relationship between the two concepts, however, is not as simple as Hempel thought. See the chapter on “Explaining and Predicting” in N. R. Hanson, *The Concept of the Positron: A Philosophical Analysis* (Cambridge: Cambridge University Press, 1963), pp. 25–41.

discovering why it happened.” When a historian asks, for example, “‘Why did Brutus stab Caesar?’ he means ‘What did Brutus think, which made him decide to stab Caesar?’ The cause of the event, for him, means the thought in the mind of the person by whose agency the event came about: and this is not something other than the event, it is the inside of the event itself.”<sup>13</sup>

This, according to Collingwood, was one of the things that distinguished history from science. “The processes of nature,” he wrote, could be “properly described as sequences of mere events, but those of history cannot. They are not processes of mere events but processes of actions, which have an inner side, consisting of processes of thought; and what the historian is looking for is these processes of thought.” The historian discovered them by rethinking those thoughts “in his own mind.” To understand why Julius Caesar, for example, did certain things, the historian tries “to discover what thoughts in Caesar’s mind determined him to do them. This implies envisaging for himself the situation in which Caesar stood, and thinking for himself what Caesar thought about the situation and the possible ways of dealing with it.” “The history of thought,” he concluded, “and therefore all history, is the re-enactment of past thought in the historian’s own mind.”<sup>14</sup>

The historian’s goal was thus to bring the past back to life by rethinking past thoughts in the present. Indeed, according to Collingwood, that was the historian’s *only* goal. History, he insisted, was “*nothing but* the re-enactment of past thought in the historian’s own mind.” The thoughts that a historian “can re-think for himself” are “*all* he can know historically.” “Of everything other than thought,” he said, “there can be no history.” Human reason was the only factor of interest to the historian. Montesquieu, he said, had “misunderstood the essential character” of the differences between various nations and cultures: “instead of explaining their history by reference to human reason, he thought of it as due to differences in climate and geography.” “History so conceived,” he argued, “would become a kind of natural history of man, or anthropology, where institutions appear not as *free inventions of human reason* in the course of its development, but as the necessary effects of natural causes.” To be sure, he admitted, there was “an intimate relation between any culture and its natural environment; but what determines its character is not the facts of that environment, in themselves, but what man is able to get out of them; and that depends on what kind of man he is.”<sup>15</sup>

This whole approach would today, I think, strike even the most conservative historians as narrow and dogmatic and in fact as a bit bizarre. Philosophers

<sup>13</sup> R. G. Collingwood, *The Idea of History* (New York: Oxford University Press, 1956), pp. 175–78, 214–15.

<sup>14</sup> *Ibid.*, p. 215.

<sup>15</sup> *Ibid.*, pp. 78–79, 218, 228, 304 (emphasis added).

have traditionally tended to view the Collingwood approach more sympathetically, but even some philosophers have found that approach a little hard to take.<sup>16</sup> How could Collingwood simply assume, for example, that social institutions were “free inventions of human reason”? How could he be so dismissive of factors having little to do with “action” and “rational thought” in his sense? Collingwood would simply lay it down as a basic principle that “so far as man’s conduct is determined by what may be called his animal nature, his impulses and appetites, it is non-historical.”<sup>17</sup> But this view was obviously rather arbitrary. To be sure, conscious thought plays a role, sometimes a very important role, in shaping the course of events, and one of the historian’s basic techniques is to try to look at things through the eyes of the people he or she is studying. But the historian’s goal is to make sense of the past—to see how things fit together, to understand the logic underlying the course of events—and often that logic has a great deal to do with nonintellective factors. Demographic change, economic growth, shifts in the distribution of power among states: developments of that sort are obviously of fundamental historical importance. To explain why Brutus stabbed Caesar (to take Collingwood’s own example), the historian would want to see what was going on in Rome at the time socially, economically, culturally and above all politically: the goal would be to see not just what was in Brutus’s mind at a particular moment, but to understand the whole process that had led up to the assassination of Caesar. Or to put the point in more general terms: historical evolution, like evolution as a whole, is not always driven by intent; the “structure selects,” the environment, both human and natural, plays a key role, and the “why” questions are thus not always answered by looking essentially to conscious thought.<sup>18</sup>

So for most historians the Collingwood theory was not taken too seriously. And what this meant was that neither the Collingwood school nor the Hempel school gave the historians much that they found useful in the way of philosophical guidance. The two schools represented opposite ends of a spectrum: one emphasized structure and law-like regularity, and the other free will and human agency. But every practicing historian knows that both sorts of factors come into play. Part of the art of doing history is being able to figure

<sup>16</sup> See especially R. F. Atkinson, *Knowledge and Explanation in History* (Ithaca: Cornell University Press, 1978), pp. 26–27.

<sup>17</sup> Collingwood, *Idea of History*, p. 216.

<sup>18</sup> This simple point lies at the heart of evolutionary theory. For the application of this type of thinking to political life, see Kenneth Waltz, *Theory of International Politics* (New York: McGraw-Hill, 1979), esp. pp. 76–77, 82–88, 118; the phrase quoted is on p. 92. Note also the general approach taken in Robert Axelrod’s *The Evolution of Cooperation* (New York: Basic Books, 1984). Axelrod approached his problem in evolutionary terms and thus did not assume intelligence shaped action. An organism, as he put it, “does not need a brain to play a game,” and his theory of “cooperation” applied to bacteria as well as to human beings. *Ibid.*, p. 18; note also chapter 5.

out how exactly in any particular case the balance between them is to be struck, and this of course is an empirical and not a philosophical problem. The two schools together had dominated Anglo-American philosophy of history in the 1950s and 1960s, but from the point of view of the practitioners, neither tradition had generated much in the way of insight into what history should be.

### THE CONSTRUCTIVIST CHALLENGE

Practicing historians by the late 1960s had thus come to have a fairly low opinion of the philosophy of history literature. J. H. Hexter, for example, referred in 1967 to the “long-standing failure of a considerable number of talented philosophers writing about history to say anything of much interest to historians.”<sup>19</sup> Many other historians felt much the same way. But the tradition Hexter was criticizing was already petering out, and within the space of a few years a very different body of theory had emerged. This time the theorists *were* saying things of considerable interest to historians. But did this new body of theory actually meet their needs any better than the body of theory it had replaced?

The new movement was based on the idea, not particularly new in itself, that history is not so much discovered as invented.<sup>20</sup> The argument was that the past itself no longer exists; what happened in the past cannot be perceived and is not directly knowable; it therefore takes an act of the imagination to create a picture of the past. That picture could take many different forms, all equally legitimate. As Hayden White, the leading figure in the movement, put it: “any historical object can sustain a number of equally plausible descriptions or narratives of its processes.”<sup>21</sup>

Indeed, White contended, one could never take it for granted that there is any coherent story that captures the historical reality of the subject being

<sup>19</sup> Reply to Morton White, *New York Review of Books*, March 23, 1967, p. 29.

<sup>20</sup> The British philosopher Michael Oakeshott, for example, argued in 1933 that “the historian’s business is not to discover, to recapture, or even to interpret: it is to create and construct.” Michael Oakeshott, *Experience and Its Modes* (Cambridge: Cambridge University Press, 1933), p. 93. Collingwood, incidentally, considered the chapter in which Oakeshott had made this argument to be “the most penetrating analysis of historical thought that has ever been written.” Quoted in David Boucher, “The Creation of the Past: British Idealism and Michael Oakeshott’s Philosophy of History,” *History and Theory* 23 (1984): 193.

<sup>21</sup> Hayden White, *The Content of the Form: Narrative Discourse and Historical Representation* (Baltimore: Johns Hopkins University Press, 1987), p. 76. For a discussion of the surprisingly common argument about the “non-existence” of the past, see Atkinson, *Knowledge and Explanation in History*, pp. 51–53.

studied: “the conviction that we can make sense of history stands on the same level of epistemic plausibility as the conviction that it makes no sense whatsoever.”<sup>22</sup> Because the material the historian has to work with grossly underdetermines the sort of interpretation that is produced, historical writing was much more inventive than the historical profession had traditionally been willing to admit. It followed, he argued, that “if one treated the historian’s text as what it manifestly was, namely a rhetorical composition, one would be able to see not only that historians effectively *constructed* the subject of their discourse in and by writing, but that, ultimately, what they actually wrote was less a report of what they had found in their research than of what they had *imagined* the object of their original interest to consist of.”<sup>23</sup>

Old-fashioned historians were thus wrong, in White’s view, to think that narrative discourse was “a neutral medium for the representation of historical events.” It was instead “the very stuff of a mythical view of reality”; in fact, the literary structure of the historical text carried the meaning.<sup>24</sup> The “factual” content, such as it was, was not to be taken too seriously; “every historical narrative” was to be regarded as “allegorical”—that is, “as saying one thing and meaning another.”<sup>25</sup> The historian, according to White, in adopting a rhetorical strategy, performs “an essentially *poetic* act, in which he *prefigures* the historical field and constitutes it as a domain upon which to bring to bear the specific theories he will use to explain ‘what was *really* happening’ in it.”<sup>26</sup> In White’s view, as one commentator put it, the heart of the interpretation was “packed into the historian’s original creative act.”<sup>27</sup> Since “the possible modes of historiography,” White argued, were “in reality *formalizations* of poetic insights that analytically precede them,” and since none of these poetic insights had a more legitimate claim to being “realistic” than any of the others, the historian’s choice of an interpretative strategy did not depend on what best captured “reality”: the choice was “ultimately aesthetic or moral rather than epistemological.”<sup>28</sup> The conclusion, shocking to old-fashioned historians, was that “we are free to conceive ‘history’ as we please, just as we are free to make of it what we will.”<sup>29</sup>

<sup>22</sup> White, *Content of the Form*, p. 73.

<sup>23</sup> Hayden White, response to Arthur Marwick, *Journal of Contemporary History* 30 (April 1995): 240 (emphasis in original).

<sup>24</sup> White, *Content of the Form*, pp. ix, 43–44.

<sup>25</sup> *Ibid.*, p. 45.

<sup>26</sup> Hayden White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (Baltimore: Johns Hopkins University Press, 1973), p. x (emphasis in original).

<sup>27</sup> Maurice Mandelbaum, “The Presuppositions of *Metahistory*,” *History and Theory* 19, Beiheft (1980): 46.

<sup>28</sup> White, *Metahistory*, p. xii (emphasis in original).

<sup>29</sup> *Ibid.*, p. 433.

Historical narratives were thus to be treated as “verbal fictions”; there was no fundamental difference between history and myth.<sup>30</sup> The basic concepts people normally used to distinguish between the two, concepts like “truth” and “reality,” were themselves problematic. Here people like White drew freely on the writings of literary theorists like Roland Barthes, who had challenged the idea that one could meaningfully distinguish between historical and fictional discourses and who had insisted that thought was a captive of language.<sup>31</sup>

People who argued along these lines, as White himself pointed out, were often charged with promoting “a debilitating relativism that permits any manipulation of the evidence as long as the account produced is structurally coherent.” Such an approach, the critics alleged, would for example permit a Nazi version of history, a version that would even deny the reality of the Holocaust, “to claim a certain minimal credibility.” Wasn’t it the case, those critics asked, that according to his theory whether or not the Holocaust had actually occurred was “only a matter of opinion,” and that one could “write its history in whatever way one pleases”?<sup>32</sup>

White did not quite say no. Indeed, he admitted that “the kind of perspective on history” he favored was “conventionally associated” with “the ideologies of fascist regimes.” But this, he said, was no reason for shying away from it: it was important, in his view, to “guard against a sentimentalism that would lead us to write off such a conception of history simply because it has been associated with fascist ideologies.” He still insisted “that when it comes to apprehending the historical record, there are no grounds to be found in the historical record itself for preferring one way of construing its meaning over another.”<sup>33</sup> But once again this raised the issue of whether a Nazi interpretation of history was as legitimate as any other.

White dealt with that issue not directly but rather by considering the question of whether the Nazis’ victims, the Jews, could legitimately concoct a historical interpretation for their own political purposes. His answer was that they could. Israeli ideologists, he said, had adopted the theory that the Holocaust was the inevitable outcome of life in the Diaspora. “The totalitarian, not to say fascist, aspects” of Israeli policy on the West Bank, according to White, might be rooted in that theory, but it should nonetheless be considered as a “morally responsible response to the meaninglessness” of Jewish history in the Diaspora. It was not to be dismissed as an “untruth.” Indeed,

<sup>30</sup> Hayden White, *Tropics of Discourse: Essays in Cultural Criticism* (Baltimore: Johns Hopkins University Press, 1978), p. 82.

<sup>31</sup> White, *Metahistory*, p. xi; White, *Content of the Form*, pp. 35, 37. A quotation from Barthes (“le fait n’a jamais qu’une existence linguistique”) was used as the motto for that latter book.

<sup>32</sup> White, *Content of the Form*, p. 76; White, *Metahistory*, p. 433.

<sup>33</sup> White, *Content of the Form*, pp. 74–75.

“its truth, as a historical interpretation, consists precisely of its effectiveness in justifying a wide range of current Israeli political policies that, from the standpoint of those who articulate them, are crucial to the security and indeed the very existence of the Jewish people.” And how, in White’s view, did that sort of history compare with history that purports to be objective—with history that claims “to have forgone service to any specific political cause and simply purports to tell the truth about the past as an end in itself,” and that claims to provide a relatively impartial view that might lead to “tolerance and forbearance rather than reverence or a spirit of vengefulness”? The more politicized approach was actually to be preferred. The balanced view, the view that suggests that the “desire for revenge” be put aside, is the sort of view, he says, that “always emanates from centers of established political power,” but the kind of tolerance it recommends “is a luxury only devotees of dominant groups can afford.” The attempt to write history objectively is thus ruled out on *political* grounds—ruled out precisely because it would lead to mutual tolerance. The truth was not to be sought “as an end in itself”; the test of validity was political effectiveness.<sup>34</sup>

White was not the only scholar to argue along these lines, and some writers were even more extreme. Hans Kellner, for example, argued that the belief in “historical objectivity” was not just a form of self-deception. The set of norms associated with that concept was actually an instrument of repression. “‘Truth’ and ‘reality,’” according to Kellner, “are, of course, the primary authoritarian weapons of our time.”<sup>35</sup>

The effect of this type of thinking was to sanction a highly politicized form of historiography. For if it were true, as Michel Foucault argued, that “we cannot exercise power except through the production of truth,” then one could try to gain power by creating one’s own “truth”—that is, by shaping a text so as to serve one’s own political purposes.<sup>36</sup> And this had to be the goal of writing history, because the competing conception of what the goal of history was—the old-fashioned idea that the aim was to “tell the truth about the past as an end in itself”—had been so thoroughly discredited. From this point of view, the historian did not even have to try to be honest. “White’s view of history” was thus praised by one of his supporters for allowing “for those ‘creative, interpretive distortions’ which, optimistically, go beyond orthodox ways of reading the past the present and the future in *utopian* ways.”<sup>37</sup> The point

<sup>34</sup> *Ibid.*, pp. 80–81; see also p. 73.

<sup>35</sup> Hans Kellner, “Narrativity in History: Post-Structuralism and Since,” *History and Theory* 26, Beiheft 26 (December 1987): 6.

<sup>36</sup> Michel Foucault, *Power/Knowledge* (New York: Pantheon, 1980), p. 93.

<sup>37</sup> Keith Jenkins, *On “What Is History?”: From Carr and Elton to Rorty and White* (London: Routledge, 1995), p. 42 (emphasis in original).

of doing history, the argument ran, was therefore not “to get the story straight” but rather to “get the story *crooked*.”<sup>38</sup>

Given how sharply these views diverged from traditional notions of what historical scholarship was supposed to be, it was scarcely to be expected that they would be accepted uncritically. As it turned out, in their pure form they had only a negligible impact on what historians actually did. White’s writings, for example, as his own supporters note with chagrin, have had “virtually no discernible influence” on historical work. “Inadequately read, rarely reviewed in journals read by historians, infrequently cited, little discussed, and then routinely and grossly misunderstood”—that was the upshot of one such study of White’s impact.<sup>39</sup> Those writers had their own ideas about why this was so, but perhaps the basic reason was that White’s theory, and especially the idea of the historian performing an “an essentially *poetic act*,” did not ring true in terms of the historians’ understanding of their own work. An interpretation can take years, and sometimes decades, of intense study to work out. The whole intellectual process of making sense of the evidence seemed to the people engaged in it to play a fundamental role in shaping the final product. Practicing historians could scarcely admit that interpretation ultimately boiled down to a simple “poetic act.”

So most historians found these arguments hard to accept, and yet the movement was not without consequence. Many of the notions with which it was associated were broadly accepted, albeit in watered-down form. The view that it was legitimate for historical work to be shaped, at least to some degree, by a political agenda became quite respectable.<sup>40</sup> The old ideal of historical objectivity, on the other hand, fell into disrepute. It was often taken for granted that the belief that historical work could be objective was a delusion; the inference was sometimes drawn that there was little point to even trying for objectivity and that the important thing was to make one’s own biases explicit.

<sup>38</sup> Stephen Bann, quoted in Hans Kellner, *Language and Historical Representation: Getting the Story Crooked* (Madison: University of Wisconsin Press, 1989), p. 3, and also quoted in J. L. Gorman’s review of the Kellner book in *History and Theory* 30 (1991): 359 (emphasis in original).

<sup>39</sup> Nancy Partner, “Hayden White (and the Content and the Form and Everyone Else) at the AHA,” *History and Theory* 36 (1997): 104, and Richard Vann, “The Reception of Hayden White,” *History and Theory* 37 (1998).

<sup>40</sup> See, for example, Joyce Appleby, Lynn Hunt, and Margaret Jacob, *Telling the Truth about History* (New York: Norton, 1994), p. 4: “We seek a vision of the past and an intellectual stance for the present that will promote an ever more democratic society.” See also Thomas L. Haskell, *Objectivity Is Not Neutrality: Explanatory Schemes in History* (Baltimore: Johns Hopkins University Press, 1998), p. 150, and the cases cited in Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (Cambridge: Cambridge University Press, 1988), p. 598.

Why were ideas of this sort taking hold even among mainstream historians? For one thing, the general intellectual climate was changing rapidly in the late twentieth century. There was a growing tendency throughout the humanities in the 1970s and 1980s especially to challenge the very idea of truth, and to mount that challenge in a rather radical way. “The secret of theory,” according to one leading theorist (Baudrillard), is “that truth does not exist.”<sup>41</sup> According to another even more famous theorist (Foucault), “reality does not exist,” “only language exists.”<sup>42</sup> And this kind of thinking was by no means limited to a number of well-known French writers.

In America as well, there was a certain tendency, as represented, for example, in the work of the philosopher Richard Rorty, to downgrade notions like “rationality” and “objectivity,” to blur the distinction between knowledge and opinion, and to insist that “there is no way to get outside our beliefs and our language.” For Rorty, terms like “truth” and “knowledge” were simply “matters of social practice”—mere “compliments” paid to “beliefs we think so well justified that, for the moment, further justification is not needed.”<sup>43</sup> Insofar as there was a difference between knowledge and opinion, for Rorty it was simply the difference between ideas that are generally shared and ideas “on which agreement is relatively hard to get.”<sup>44</sup> From his point of view, “the desire for objectivity” was thus simply “the desire for as much intersubjective agreement as possible.”<sup>45</sup> To reach for something more—“to explicate ‘rationality’ and ‘objectivity’ in terms of conditions of accurate representation”—is just a “self-deceptive effort to eternalize the normal discourse of the day.”<sup>46</sup> Indeed, according to Rorty, the archaic vocabulary of Enlightenment rationalism had become an “impediment to the progress of democratic societies”: it was “obsolete” and should be replaced by a new way of speaking more in line with our current political values.<sup>47</sup> These views were quite influential. They served in particular to give a veneer of philosophical respectability to the new antiobjectivist view of history.<sup>48</sup>

<sup>41</sup> Jean Baudrillard, interview with Sylvère Lotringer, “Forgetting Baudrillard,” *Social Text*, no. 15 (Fall 1986): 142.

<sup>42</sup> This widely quoted remark originally appeared in the “Débat sur le roman,” published in *Tel Quel* 117 (Spring 1964): 45, and reprinted in Michel Foucault, *Dits et Ecrits, 1954–1988*, vol. 1 (Paris: Gallimard, 1994), p. 380.

<sup>43</sup> Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton: Princeton University Press, 1979), pp. 10, 11, 178, 385; Richard Rorty, *Objectivism, Relativism, and Truth* (Cambridge: Cambridge University Press, 1991), p. 24.

<sup>44</sup> Rorty, *Objectivism, Relativism, and Truth*, p. 23.

<sup>45</sup> *Ibid.*

<sup>46</sup> Rorty, *Philosophy and the Mirror of Nature*, p. 11.

<sup>47</sup> Richard Rorty, “The Contingency of Community,” *London Review of Books*, July 24, 1986, p. 10.

<sup>48</sup> See especially Jenkins, *On “What Is History?”: From Carr and Elton to Rorty and White*. Note also John Searle’s discussion of Rorty’s influence on the humanities in general in his “The Storm over the University,” *New York Review of Books*, December 6, 1990, p. 40.

But there was something quite odd about Rorty's whole line of argument. Who, for example, really thinks that the belief that the earth was flat was no less "true" in its day than the opposite belief is today? But people tended to be somewhat bowled over by Rorty's prestige and by the claim that his views had come to represent something of a consensus. Who, it was asked, still actually believed in such notions as "reality" and "truth"? "Metaphysical prigs" of that sort, according to people like Rorty, had become increasingly hard to find.<sup>49</sup>

The Rorty view, however, did not represent anything like a consensus. There were many philosophers, including some quite well-known ones, who took a position on these issues much more in line with common sense.<sup>50</sup> John Searle, for example, one of America's most distinguished contemporary philosophers, even had the temerity to declare that the "idea that there is a real world independent of our thought and talk" and "the idea that our true statements are typically made true by how things are in the real world" were "essential presuppositions of any sane philosophy."<sup>51</sup>

These were not, of course, new issues for the philosophers. The problem of the nature of knowledge had been at the top of their agenda since ancient times. And during those centuries of philosophical debate, certain key points had become clear. One such point was that there was no way to disprove the claims of the skeptic. There was no way, for example, to rule out the possibility "that the world sprang into being five minutes ago, exactly as it then was, with a population that 'remembered' a wholly unreal past."<sup>52</sup> Knowledge was therefore never absolute. There was no way to prove that the external world even exists. But the point was trivial. It simply meant that everything we do of an intellectual nature is premised on the assumption that we are not being systematically misled: we take as our point of departure the assumption that

<sup>49</sup> Richard Rorty, "Deconstruction and Circumvention," in his *Philosophical Papers*, vol. 2, *Essays on Heidegger and Others* (New York: Cambridge University Press, 1991), p. 86. This paper was originally published in *Critical Inquiry* (September 1984).

<sup>50</sup> W. V. Quine, for example, explicitly repudiates the view Rorty attributes to him (the "claim that there is no 'matter of fact' involved in attributions of meaning to utterances"); he says that the meaning of sentences is "very much a matter of fact." Donald Davidson says: "We can accept objective truth conditions as the key to meaning, a realist view of truth, and we can insist that knowledge is of an objective world independent of our thought or language." W. V. Quine, "Let Me Accentuate the Positive," and Donald Davidson, "A Coherence Theory of Truth and Knowledge," in *Reading Rorty: Critical Response to Philosophy and the Mirror of Nature (and Beyond)*, ed. Alan Malachowski (Oxford: Blackwell, 1990), pp. 117, 120–21.

<sup>51</sup> John Searle, *The Construction of Social Reality* (New York: Free Press, 1995), p. xiii. An argument supporting these propositions is laid out in chapters 7–9 of that book.

<sup>52</sup> Bertrand Russell, *The Analysis of Mind* (London: George Allen and Unwin, 1921), p. 160. Arguments of this sort are common in epistemology. Descartes, for example, raised the specter of "a deceiver of supreme power and cunning who is deliberately and constantly deceiving me." René Descartes, *The Philosophical Writings of Descartes*, trans. John Cottingham, Robert Stoothoff, and Dugald Murdoch (Cambridge: Cambridge University Press, 1984), p. 17.

our basic beliefs about reality—about the existence of an external world knowable through the mind and through the senses, acting in tandem—are in fact correct. As Hume put it: “’tis in vain to ask, *Whether there be body or not?* That is a point, which we must take for granted in all our reasonings.”<sup>53</sup> The skeptic is not refuted; the basic epistemological problem is recognized, but the normal assumptions—about the existence of reality and the possibility of knowledge—are made, and we just move on from there.

Because these basic conclusions apply to knowledge in general, they apply to historical knowledge in particular. But does the problem end there, or is there a distinct problem of *historical* knowledge that needs to be considered—distinct because historical knowledge is essentially different from other forms of knowledge? In particular, is historical knowledge fundamentally different from scientific knowledge? If not, then perhaps the philosophy of science can give us the guidance we are looking for.

#### PHILOSOPHY OF SCIENCE AS PHILOSOPHY OF HISTORY

How does historical analysis differ from scientific analysis? Is science different because it deals with the natural world, whereas history deals with the human world?<sup>54</sup> This is obviously not the case: many sciences—experimental psychology, for example—take human beings as objects of study. Is science different because in science truths are discovered “through observation and experiment exemplified in what we actually perceive, whereas the past has vanished and our ideas about it can never be verified as we verify our scientific hypotheses?”<sup>55</sup> Again the answer is no. Science often deals with the vestiges of past phenomena. This is as true of astrophysics as it is of evolutionary biology, geology, and many other fields. The biologist has a fossil record to work with, and the historian has a documentary record. From an epistemological point of view, how is the one body of evidence essentially different from the other?

It is often argued that the real difference has to do with the level of generality at which a subject is studied. The historian, it is often said, is concerned with the particular, and the scientist’s concern is with general phenomena—that the scientist’s aim is to “formulate a system of general laws,” whereas the historian’s “central preoccupation” is with “the precise course of individual

<sup>53</sup> David Hume, *A Treatise of Human Nature*, ed. L. A. Selby-Bigge, 2d ed. (Oxford: Clarendon, 1978), p. 187.

<sup>54</sup> As claimed, for example, by Anthony O’Hear in his *Introduction to the Philosophy of Science* (Oxford: Clarendon, 1989), p. 6.

<sup>55</sup> Collingwood, *Idea of History*, p. 5.

events.”<sup>56</sup> But most scientists, to judge from the sorts of articles published in their journals, are concerned with very specific matters—to be sure, in the context of a more far-reaching set of interests. The same point, however, applies to the historians: the topic may be relatively narrow, but fundamental conceptual issues are rarely entirely absent.

Sometimes the argument is that historical analysis is necessarily selective and is therefore subjective and unscientific.<sup>57</sup> But as one philosopher points out, it is “wholly mistaken to hold that history is selective and science not. In truth the sciences are much more rigorous and explicit in selecting the facts or aspects of fact which concern them than history ever is.”<sup>58</sup>

Sometimes professional consensus is taken as the key indicator. Scientists, it is said, have “evolved a standard way of thinking” about their subject matter; as a result, scientific thinking is “impartial and impersonal”; assumptions and principles are shared, and the conclusions reached are accepted by all competent scientists. This, it is argued, is one of the main things that makes a field like physics “scientific” and why it is that natural science can be said to provide “objective knowledge.”<sup>59</sup> Historians, on the other hand, commonly disagree sharply among themselves on many important issues; history, the argument runs, therefore cannot be considered a science.

But consensus is not a measure of quality in the natural sciences: the most impressive advances in physics, for example, took place during times when no consensus existed and when fundamental claims were vigorously disputed, and the same kind of point can be made about evolutionary biology in our own day. Nor would it necessarily be a sign of scientific status and a higher level of objectivity if historians, for their part, did accept standard interpretations on a whole range of issues. Degree of consensus is very much a surface indicator. Scientific status really depends on the nature and the quality of argumentation: on whether insights can be developed in a logically compelling way and on whether those insights are rooted in the empirical evidence at hand.

The scientific status of a discipline turns therefore on the nature of the method used. In that regard history and science have more in common than one might think. Collingwood developed the point quite effectively:

Francis Bacon, lawyer and philosopher, laid it down in one of his memorable phrases that the natural scientist must “put Nature to the question.” What he was denying,

<sup>56</sup> W. H. Walsh, *An Introduction to the Philosophy of History* (London: Hutchinson’s, 1951), pp. 24, 38. See also Maurice Mandelbaum, *The Problem of Historical Knowledge: An Answer to Relativism* (New York: Liveright, 1938), p. 3.

<sup>57</sup> This familiar point goes back at least to Descartes; see the passage quoted in Collingwood, *Idea of History*, p. 59.

<sup>58</sup> Atkinson, *Knowledge and Explanation in History*, p. 79.

<sup>59</sup> Walsh, *Introduction to the Philosophy of History*, pp. 96–97, 114.

when he wrote this, was that the scientist's attitude towards nature should be one of respectful attentiveness, waiting upon her utterances and building his theories on the basis of what she chose to vouchsafe him. What he was asserting was two things at once: first, that the scientist must take the initiative, deciding for himself what he wants to know and formulating this in his own mind in the shape of a question; and secondly, that he must find means of compelling nature to answer, devising tortures under which she can no longer hold her tongue. Here, in a single brief epigram, Bacon laid down for all the true theory of experimental science.<sup>60</sup>

Collingwood's real insight here was that the point did not just apply to science. Bacon, he said, had also hit upon the "true theory of historical method." And Collingwood stressed the point, both in *The Idea of History* and in his earlier work, that historical analysis should, like science, be question-driven. "You can't collect your evidence before you begin thinking," he noted, and because thinking meant "asking questions," nothing was evidence "except in relation to some definite question."<sup>61</sup>

This point is of absolutely fundamental importance. In doing historical work, if you are doing it the right way, you are not just studying a topic. You are trying to answer a question, or perhaps a set of questions. And it really matters how those questions are framed. Questions need to be set up in such a way that the answers turn on what the evidence shows. There is little point in setting out to answer essentially trivial questions—that is, in doing what historians contemptuously refer to as "antiquarian" work. If the goal is to get some insight into the really big issues (such as what makes for war, or for a stable international system), then the particular questions the historian sets out to answer should have some larger importance, in the sense that the way they are answered should shed some light on one of those basic issues. This does not mean, of course, that the historian should try to tackle some very broad issue in a very direct way. To do that is like grabbing at a cloud. To get at such an issue, the scholar has to try to bring it down to earth and give it some concrete content: not just "what makes for war?" but "what caused the First World War?" or even "why did events take the course they did during the July crisis in 1914?" The narrower the question, the more studiable it is. But in defining the question more narrowly, it is important not to lose sight of the more basic conceptual issue. The historian's findings need to have some broader importance. But this point applies not just to historical work but to science as a whole.

So there is less to the distinction between science and history than meets the eye, and indeed the sense that the two fields are radically different is rooted in a rather old-fashioned and idealized sense for what science is. Science, according to that traditional view, was based on observed fact. Facts had a kind

<sup>60</sup> Collingwood, *Idea of History*, p. 269.

<sup>61</sup> *Ibid.*, p. 281.

of elemental quality: once discovered, they could not be disputed, and they were the basic building blocks used to construct theories. Scientists might disagree about theoretical matters, but fact, discovered through experiment, was a “court of final appeal”: observation would decide the issue and would decide it in a way that was intellectually compelling. There was, in other words, a method, practically an algorithm, linking empirical observations to theoretical conclusions. That meant that the theory produced by that method was in no sense arbitrary: the natural world was being revealed for what it really was.<sup>62</sup>

That whole view was basically abandoned in the 1960s. The logic of scientific development, the argument ran, was much looser than people had thought, and the whole notion of an algorithm did not come close to capturing the way science actually worked. For one thing, the basic idea that facts were what they were, independent of theory, came to be seen as somewhat problematic: an observation was meaningful only in the context of a set of theoretical assumptions, so the line between fact and theory was not nearly as sharp as the old positivist tradition had assumed.<sup>63</sup> And theory itself never took shape in a kind of mechanical way from facts gathered up “like pebbles on the beach.” The facts never just “speak for themselves.”<sup>64</sup> That certainly was Einstein’s view. There was, he said, “no inductive method which could lead to the fundamental concepts of physics”; “in error are those theorists who believe that theory comes inductively from experience.”<sup>65</sup>

But if theory is not a simple product of observation, what, if anything, makes a particular theory intellectually compelling? The test of evidence is not quite as decisive as it might seem. Since observations do not just “speak for themselves” and have to be interpreted, theoretical assumptions inevitably come into play. So there is a certain problem of circularity here. And theories,

<sup>62</sup> Thomas Kuhn, *The Trouble with the Historical Philosophy of Science* (Cambridge, Mass.: Harvard History of Science Department, 1991), pp. 4–5; Stephen Toulmin, “From Form to Function: Philosophy and History of Science in the 1950s and Now,” *Daedalus* 106, no. 3 (Summer 1977): esp. 147, 150–51.

<sup>63</sup> This is one of the basic themes of N. R. Hanson’s work. See Hanson, *Observation and Explanation*, pp. 1–15, esp. 4–5; N. R. Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (Cambridge: Cambridge University Press, 1958), chaps. 1 and 2; and N. R. Hanson, *Perception and Discovery: An Introduction to Scientific Inquiry* (San Francisco: Freeman, Cooper, 1969), parts 2 and 3. This way of looking at things was by no means entirely new; the key ideas had, in fact, been laid out a half century earlier by Pierre Duhem. See especially Pierre Duhem, *La Théorie physique, son objet, sa structure* (Paris: Chevalier et Rivière, 1906), and published in English as *The Aim and Structure of Physical Theory* (Princeton: Princeton University Press, 1954); note especially part II, chaps. 4 and 6.

<sup>64</sup> Hanson, *Perception and Discovery*, pp. 220, 237. See also the quotations in Hanson, *Patterns of Discovery*, pp. 183–84.

<sup>65</sup> Albert Einstein, “Physics and Reality,” in Albert Einstein, *Ideas and Opinions* (New York: Crown, 1954), pp. 301, 307. This article was first published in 1936.

it turns out, are not abandoned simply because evidence turns up that seems to contradict them. Evidence of that sort can often be written off with ad hoc arguments, and, even when it cannot be dealt with in that way, a theory can be adjusted to accommodate observations which, in its original form, it had been unable to account for.<sup>66</sup> So the method of justification—the method for treating the “facts” as a kind of final arbiter—is much weaker than one might think. But if all this is true, what then *does* determine theory choice? Does all this mean that theory choice is an essentially arbitrary process?

For the philosophers concerned with these issues in the 1950s and early 1960s, people like N. R. Hanson, Stephen Toulmin, and Thomas Kuhn, it seemed that the history of science might point the way to some answers. The older formalist tradition had been largely ahistorical. That tradition had emphasized the “context of justification.” The assumption had been that methodology had to do with the rational justification for theories in their final form; it was concerned with the formal methods for establishing the validity of such theories. Questions about discovery—about the actual historical process through which theory took shape—were dismissed as matters of “mere psychology.”<sup>67</sup> Karl Popper, for example, although he wrote a book called *The Logic of Scientific Discovery*, was not really interested in the historical process that led to the emergence of new theories.<sup>68</sup> Hanson made fun of that kind of approach. What Popper and other scholars in that school were interested in, he said, was not so much the “*logic of discovery*” as the “*logic of the Finished Research Report*.”<sup>69</sup> But one of the assumptions of the new, more historicist approach was that such distinctions were much too sharp and that the “*logic of discovery*” was of a piece with the “*logic of justification*.”

If theories, however, were the product of a historical process, a process that could have developed in more than one way, what did this imply about their epistemic status? It seemed that to admit that the issue of theory choice is not decided in a purely rational and objective way—to admit that theory choice “depends on a mixture of objective and subjective factors”<sup>70</sup>—was to open the floodgates. How could the truth claims of science survive the admission

<sup>66</sup> See especially Imre Lakatos, “Falsification and the Methodology of Scientific Research Programmes,” in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970), pp. 100–101.

<sup>67</sup> Imre Lakatos, quoted in Stephen Toulmin, *Human Understanding*, vol. 1 (Princeton: Princeton University Press, 1972), p. 482n.

<sup>68</sup> Karl Popper, “Normal Science and Its Dangers,” in Lakatos and Musgrave, *Criticism and the Growth of Knowledge*, pp. 57–58. Kuhn quotes (and criticizes) this passage in one of his own contributions to that volume, *ibid.*, p. 235.

<sup>69</sup> N. R. Hanson, *What I Do Not Believe, and Other Essays* (Dordrecht: Reidel, 1972), pp. 288–89.

<sup>70</sup> Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), p. 325. One scholar quotes this passage as

that theory had no purely rational basis, compelling in itself? Wouldn't the "door to subjectivism" be "wide open"?<sup>71</sup>

Thomas Kuhn was the best-known champion of the new approach, and in his amazingly successful book *The Structure of Scientific Revolutions* he presented the new thinking in a particularly sharp form. Kuhn's analysis focused on what he called "paradigms"—that is, basic frameworks for scientific thinking during a particular stage in the history of science.<sup>72</sup> Science, in his view, developed in two distinct ways. There was "normal science," the work that took place in the context of a particular paradigm, and "revolutionary science," the process leading to the replacement of old paradigms by newer ones. His fundamental claim was that new paradigms did not emerge, and older ones were not overthrown, for purely rational reasons; the "issue of paradigm choice," he wrote, "can never be unequivocally settled by logic and experiment alone."<sup>73</sup> Instead, he saw a process of anomalies piling up as "normal science" ran its course; normal science was unable to deal with these anomalies, precisely because it took the existing paradigm as given and could therefore not question it. The resulting crisis could therefore only be resolved through a kind of revolution: the emergence of a new paradigm and the "conversion" of large numbers of scientists to it.

That whole process, Kuhn argued, was not to be understood in entirely rational terms. Crises are terminated, he says, not by a lengthy process of "deliberation and interpretation" but "by a relatively sudden and unstructured event"—by a kind of "gestalt switch," by means of which everything is suddenly seen in an entirely new light. "Scientists then often speak," he says, "of the 'scales falling from the eyes' or of the 'lightning flash' that 'inundates' a previously obscure puzzle, enabling its components to be seen in a new way that for the first time permits its solution. On other occasions the relevant illumination comes in sleep. No ordinary sense of the term 'interpretation' fits these flashes of intuition through which a new paradigm is born."<sup>74</sup> The new view, he wrote, is essentially accepted "on faith";<sup>75</sup> the old view is not exactly shown to be false, and its adherents are not convinced to abandon their view

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evidence for his point that, for Kuhn, "all theory choices in science are subjective"! Larry Laudan, *Beyond Positivism and Relativism: Theory, Method, and Evidence* (Boulder, Colo.: Westview, 1996), p. 249.

<sup>71</sup> R. W. Newell, *Objectivity, Empiricism and Truth* (London: Routledge, 1986), pp. 2–3, 115 n. 1. See also Toulmin, *Human Understanding*, 1:19.

<sup>72</sup> Kuhn was not the first scholar to use the term in this sense. See, for example, Stephen Toulmin, *Foresight and Understanding: An Enquiry into the Aims of Science* (New York: Harper, 1961), p. 16.

<sup>73</sup> Thomas Kuhn, *The Structure of Scientific Revolutions*, 2d ed. (Chicago: University of Chicago Press, 1970), p. 94. The first edition was published in 1962.

<sup>74</sup> *Ibid.*, pp. 122–23.

<sup>75</sup> *Ibid.*, p. 158.

through an essentially rational process of argument. Indeed, in such discussions the two parties generally talk past each other; the scientists involved find it hard to “make complete contact with each other’s viewpoints.”<sup>76</sup> (This Kuhn called the problem of “incommensurability.”) The new view gradually wins out, in large part (although not exclusively) for “subjective and aesthetic” reasons.<sup>77</sup> Resistance is intellectually legitimate; there is no point at which it becomes “illogical or unscientific.”<sup>78</sup> It dies out because the supporters of the older paradigm die out; the assumption is that the older paradigm is never really vanquished intellectually.

Kuhn’s argument was certainly overstated. Consider, for example, his argument about how in science new insight emerges in a “relatively sudden” way through a kind of “gestalt switch.”<sup>79</sup> It was Hanson who had introduced the basic concepts of Gestalt psychology to the philosophy of science in his *Patterns of Discovery*, and Kuhn in his discussion of the issue took Hanson’s work as his point of departure. But Hanson had taken great pains to demonstrate that important new theories did *not* emerge in a “relatively sudden” way.<sup>80</sup> In his more sober view, the rational element was fundamental: the whole point of his close analysis of the long and intellectually strenuous process that led to the formulation of Kepler’s laws was that the emergence of a major new way of looking at things was a lengthy and difficult process, with a distinct logic of its own.<sup>81</sup>

But Kuhn’s argument was framed in more extreme language and thus provoked rather unrestrained counterarguments. Imre Lakatos, for example, charged that “in Kuhn’s view scientific revolution is irrational, a matter for mob psychology.”<sup>82</sup> Such accusations Kuhn categorically rejected and, in his later work especially, he took pains to distance himself from the idea that the development of science was not at all to be understood in rational terms. That point of view he considered “absurd,” an “example of deconstruction gone

<sup>76</sup> Ibid., p. 148.

<sup>77</sup> Ibid., p. 156.

<sup>78</sup> Ibid., pp. 152, 159.

<sup>79</sup> Ibid., p. 122.

<sup>80</sup> Hanson, *Patterns of Discovery*, pp. 8–19, 90; note also the references to the psychology of perception literature on pp. 180–81. Kuhn, *Structure of Scientific Revolutions*, p. 113, for the reference to Hanson.

<sup>81</sup> Hanson, *Patterns of Discovery*, pp. 72–85, for the discussion of Kepler. Note also his comment that “Galileo struggled for thirty-four years before he was able to advance his constant acceleration hypothesis with confidence” (p. 72) and his remark that the “initial suggestion of an hypothesis” is “not so often affected by intuition, insight, hunches, or other imponderables as biographers or scientists suggest” (p. 71).

<sup>82</sup> Lakatos, “Falsification and the Methodology of Scientific Research Programmes,” p. 178 (emphasis in original).

mad,” and he was disturbed by the fact that it was “developed by people who often called themselves Kuhnians.”<sup>83</sup>

Kuhn was not an irrationalist. In his view of scientific development, rational factors loomed large. But in themselves they were not decisive. Indeed, they could not be decisive. That point seemed to put him on the relativist side of the fence. But this was a fence that he was in reality trying to straddle, and that was the heart of the problem. On the one hand, the process of scientific development was what it was: somewhat loose and not wholly determined by purely rational factors. On the other hand, there was clearly something quite extraordinary about science, something that gave it (to use Toulmin’s phrase) a “genuine intellectual authority over us.”<sup>84</sup> How could this sort of process lead to such results? Given the way science actually worked, how could it produce the conclusions it did, “true or probable conclusions about the nature of reality”? This Kuhn took to be a “serious question,” and he admitted that our “inability to answer it is a grave loss in our understanding of the nature of scientific knowledge.”<sup>85</sup>

That problem was fundamental, not just for Kuhn but for other pioneers of the new approach, like Toulmin and Hanson. They too were looking for a “via media,” a “middle way,” between relativism and absolutism.<sup>86</sup> They too viewed the development of science in historicist terms. Like Kuhn, they were not comfortable with the notion of scientific “truth” and preferred to talk instead about the rationality of the process. Theory choice was a social phenomenon; individual scientists made up their minds on the basis of a whole series of criteria, which they were free to weigh differently.<sup>87</sup> Those criteria—“accuracy,

<sup>83</sup> Thomas Kuhn, “Reflections on My Critics,” in Lakatos and Musgrave, *Criticism and the Growth of Knowledge*, pp. 259–64; Kuhn, *Essential Tension*, pp. 320–21; and Kuhn, *Trouble with the Historical Philosophy of Science*, pp. 3, 8–9. The constructivist view is indeed sometimes pushed to extremes. Note for example Bruno Latour’s denial of the existence of the tuberculosis bacillus prior to its discovery by Robert Koch in 1882, discussed in Alan Sokal and Jean Bricmont, *Fashionable Nonsense: Postmodern Intellectuals’ Abuse of Science* (New York: St. Martin’s, 1998), pp. 96–97. For another example, see Richard Dawkins, “The Moon Is not a Calabash,” *Times Higher Education Supplement*, September 30, 1994, p. 17. Dawkins had once asked a social scientist about a hypothetical tribe that believed that the “moon is an old calabash tossed just above the treetops.” Did he really believe that that tribe’s view would be “just as true as our scientific belief that the moon is a large Earth satellite about a quarter of a million miles away?” The social scientist replied, according to Dawkins, that “the tribe’s view of the moon is just as true as ours.”

<sup>84</sup> Toulmin, *Human Understanding*, 1:50.

<sup>85</sup> Kuhn, *Trouble with the Historical Philosophy of Science*, p. 8.

<sup>86</sup> Toulmin, *Human Understanding*, 1:88; Hanson, *Observation and Explanation*, pp. 1, 13.

<sup>87</sup> See, for example, Toulmin, *Human Understanding*, 1:135 and 139 (for his evolutionary perspective), 168–70 and 225–26 (for his attitude toward “truth”), and 229 (for the standard criteria). For Kuhn’s attitude toward “truth,” see his *Structure of Scientific Revolutions*, pp. 170–71 and 206, and his “Reflections on My Critics,” pp. 264–65.

simplicity, fruitfulness and the like”<sup>88</sup>—might be, as Kuhn put it, “subjective and aesthetic” in character.<sup>89</sup> But that did not in itself mean that the decision was irrational. Quite the contrary: the fact that choice is made by a scientific community—the fact that the decision is rooted in the mature judgment exercised by the members of that community—is the closest we can come to guaranteeing the rationality of the process.<sup>90</sup> Even Lakatos, as Kuhn pointed out, stressed the importance of “decisions governed not by logical rules but by the mature sensibility of the trained scientist.”<sup>91</sup>

What does all this mean to the historian? First of all, it gives us a reasonable standard by which to assess the rationality and scientific status of historical work: science as it actually is, not science as it was supposed to be. And there is no reason to assume that historical work can never measure up to that more modest standard. Historians exercise judgment, but so do scientists, and if the process is rational in science, it can be just as rational in history.<sup>92</sup> The demoralizing assumption that objectivity is impossible, that the mere fact that interpretation is unavoidable means that historical work can never be free from the “taint of subjectivity,” and that there is perhaps therefore little point in even trying to be objective—all this is no more warranted in history than it is in science.

But beyond that, the basic point that science and history are not all that different in epistemic terms means that the insights developed by philosophers of science—about truth and knowledge and understanding and explanation—carry over directly into corresponding notions about history. This act of translation yields, if not quite a ready-made philosophy of history, then at least a very useful framework for thinking about the sorts of problems we are concerned with here.

<sup>88</sup> Kuhn, *Structure of Scientific Revolutions*, p. 199.

<sup>89</sup> *Ibid.*, pp. 155–56, 158. The emphasis scientists place on aesthetic considerations is in fact quite striking. One Nobel Prize-winning scientist told a story about a 1957 meeting at which the physicist Murray Gell-Mann was describing a new theory of the weak interactions that he had just worked out with Richard Feynman. There were three recorded experiments that contradicted that theory, but “Gell-Mann boldly asserted that these three experiments must be faulty, because his new theory was too beautiful to be wrong. And future experiments decisively proved that Gell-Mann was right.” That scientist then went on to quote another famous physicist (Dirac), who said that it was “more important to have beauty in one’s equations than to have them fit experiments.” Jerome Friedman, “Creativity in Science,” in Jerome Friedman et al., *The Humanities and the Sciences*, ACLS Occasional Paper No. 47 (New York: American Council of Learned Societies, 1999), pp. 12–13 (emphasis added). See also Toulmin, *Foresight and Understanding*, p. 81.

<sup>90</sup> Kuhn, “Reflections on My Critics,” pp. 237–38, 262–64, and Toulmin, *Human Understanding*, 1:227, 229, 242–43, and esp. 482.

<sup>91</sup> Kuhn, “Reflections on My Critics,” p. 233. Note also Toulmin’s discussion of Lakatos in *Human Understanding*, 1:482, and Toulmin’s own reference to the “judgement of authoritative and experienced individuals,” *ibid.*, p. 243.

<sup>92</sup> See Toulmin, *Human Understanding*, 1:371.

Above all, there is the question of the historian's fundamental goal. Is the aim to get at the truth? It is (or at least used to be) commonly assumed that this was the goal, but it was also taken for granted that this goal was essentially unreachable. The well-known Dutch historian Pieter Geyl made this common point in a lecture he gave at Yale in 1954. The goddess History, Geyl said, "may be in possession of the truth, the whole truth and nothing but the truth," but to the historian she will at best "vouchsafe a glimpse. Never will she surrender the whole of her treasure. The most that we can hope for is a partial rendering, an approximation, of the real truth about the past." The reason was, according to Geyl, that to make the past intelligible, to *understand* the bare facts, the historian had to "use his material by choosing from it, ordering it, and interpreting it. In doing so he is bound to introduce an element of subjectivity; that is, he will tamper with or detract from the absolute, unchanging truth."<sup>93</sup>

"The absolute, unchanging truth": philosophers like Kuhn and Toulmin were uncomfortable with such notions, and justifiably so.<sup>94</sup> The very concept of "the truth" is both highly problematic and unnecessary in practical terms. What does it mean to talk about the "truth" about some aspect of nature or some period in history? The term seems to imply that the object of study has a kind of well-defined essence, wrapped up in a neat package somewhere, but never quite discoverable in its entirety—a treasure, as Geyl put it, that can be glimpsed but never actually acquired. But the vision conjured up by such metaphors scarcely makes sense, and the concept is certainly not needed for any practical purpose. Reality is what it is; the past was what it was; and what is being studied can be studied on its own terms. The coming of the First World War, for example, can be studied as a problem in its own right; there is no need to say that the goal is learn the truth about the origins of the First World War.

So the idea that the aim is to learn the truth about the object of study can safely be abandoned. But if the concept drops away, then the whole notion that the effort to understand is tainted because it does violence to the "absolute, underlying truth" also has to be abandoned. Understanding then emerges as the central goal in its own right—as the end in itself, not as the means for getting at the mysterious and ultimately unreachable goal of the "truth." Making the past intelligible is, in that case, not to be viewed à la Geyl as a source of distortion. Instead it should be seen as the heart of the historical enterprise. This, and not the uncovering of the "truth," is what the historian should be striving to achieve.

<sup>93</sup> Pieter Geyl, *Use and Abuse of History* (New Haven: Yale University Press, 1955; reprint, Archon Books, 1970), pp. 62–64 (emphasis in original).

<sup>94</sup> See the references cited in note 87 above.

But what exactly does “understanding” mean? Hanson’s work is a major source of insight here, and for him understanding essentially meant seeing how things fit together. The scientist, as Hanson lays out the argument, begins with observations, and “they set the problem.”<sup>95</sup> The aim is to explain the data: the physicist’s “goal is a conceptual pattern in terms of which his data will fit intelligibly alongside better-known data.”<sup>96</sup> And that is how theories are generated:

Physical theories provide patterns within which data appear intelligible. . . . A theory is not pieced together from observed phenomena; it is rather what makes it possible to observe phenomena as being of a certain sort, and as related to other phenomena. Theories put phenomena into systems. They are built up “in reverse”—retroductively. A theory is a cluster of conclusions in search of a premiss. From the observed properties of phenomena the physicist reasons his way towards a keystone idea from which the properties are explicable as a matter of course.<sup>97</sup>

A theory was thus a deductive system, one that “guarantees inferences from cause to effect.”<sup>98</sup> Indeed, one needed this sort of guarantee to claim that a causal relationship existed: it was “this logical guarantee that theories place upon causal inferences that explains the difference between truly causal sequences and mere coincidences.”<sup>99</sup> For two phenomena to be related to each other as cause and effect, there had to be a *necessary* connection between the two, and that element of necessity could only be supplied by a theory. Only a theory—precisely because it was a deductive system—could show you why cause and effect *had to be* related to each other: “the necessity sometimes associated with event-pairs construed as cause and effect is really that obtaining between premisses and conclusions in theories which guarantee inferences from the one event to the other.”<sup>100</sup>

How does this differ from what Hempel said? For Hempel, explanation also meant deduction, but he essentially just left it at that. For Hanson, the Hempel approach was just too mechanical, too formalistic, and he insisted that theories that were mere “predicting devices”—and those theories met Hempel’s basic criterion for what theories should be—would not provide genuine explanations. A theory had to do something more. People also had to have the sense that something was really being explained—that thanks to

<sup>95</sup> Hanson, *Patterns of Discovery*, p. 72.

<sup>96</sup> *Ibid.*

<sup>97</sup> *Ibid.*, p. 90. “Retroduction” (or “abduction” as the concept is also called) means “studying facts and devising a theory to explain them.” The term is defined in a passage from the philosopher Charles Sanders Peirce, quoted *ibid.*, p. 85.

<sup>98</sup> Hanson, *Perception and Discovery*, p. 309.

<sup>99</sup> *Ibid.*, pp. 292, 309 (for the quotation).

<sup>100</sup> Hanson, *Patterns of Discovery*, p. 90.

the theory, they understood something they had not understood before.<sup>101</sup> Suppose, for example, that your goal is to understand why for a particular right triangle the sum of the squares of the two short sides is equal to the square of the long side. It would not do simply to measure the sides of a large number of right triangles, note that for all those cases the square of the hypotenuse was equal to the sum of the squares of the two remaining sides, proclaim that as an empirical “law,” and then “explain” the case you are interested in by citing that “law.” But if you studied the Pythagorean theorem—if you followed the proof, if you saw how the conclusion followed from relatively plausible assumptions—you would understand why things had to be as they were. The observed phenomenon would be explained: you would have a certain “sentiment of comprehension” that would otherwise be missing.<sup>102</sup>

There are other important points to be taken away from Hanson’s analysis—points about the importance of theory in causal explanation, about the nature of understanding, and about whether understanding can ever be “objective.” Hanson was trying to get at what such concepts as understanding, explanation, and causation actually meant, and in his analysis he wanted to get away from an overly mechanistic approach to these issues.

In particular, he wanted to get away from mechanistic (or “causal-chain”) models of causation. He was trying to view causation not as a phenomenon that could be observed directly in the real world but rather as something that had meaning only in the context of a theory. “Causes certainly are connected with effects,” he wrote, “but this is because our theories connect them, not because the world is held together by cosmic glue.”<sup>103</sup> Even billiard-ball interactions are not self-explanatory. As Maupertuis argued in 1732: “people are not astonished when they see a body in motion communicate this motion to others; the habit that they have of seeing this phenomenon prevents them from seeing how marvellous it is.”<sup>104</sup> One needed to have some theory of nature (“however primitive,” as Hanson says) to interpret such phenomena in causal terms.<sup>105</sup>

Theory thus existed in people’s heads. The “locus of causal-talk,” Hanson wrote, was “not in the physical world.” “There is nothing we can see, touch, or kick in nature that will answer to the name ‘causal connection.’” But one

<sup>101</sup> Hanson, *Observation and Explanation*, pp. 41–49, esp. pp. 42–44 and 48–49.

<sup>102</sup> *Ibid.*, p. 44. Note also Hanson’s comment in *Patterns of Discovery*, p. 71: “The reason for a bevelled mirror’s showing a spectrum in the sunlight is not explained by saying that all bevelled mirrors do this.” The Pythagorean theorem example, however, is mine.

<sup>103</sup> Hanson, *Patterns of Discovery*, chap. 3 (p. 64 for the quotation); N. R. Hanson, “Causal Chains,” *Mind* 64 (1955): 289–311; and Hanson, *Perception and Discovery*, pp. 312–13.

<sup>104</sup> Maupertuis (1732), quoted in Alexandre Koyré, *Newtonian Studies* (Cambridge, Mass.: Harvard University Press, 1965), p. 162.

<sup>105</sup> Hanson, *Perception and Discovery*, p. 292.

could not kick a fact or a true statement either. That did not mean, however, that there was anything “grossly subjective” or “chimerical” about such notions: “facts, true statements, and causal connections are all what they are because the world is what it is” and there was “nothing subjective about arguing validly to true conclusions.”<sup>106</sup>

But this was not quite an objectivist view. And indeed, for Hanson the goal of theory was not to provide an exact mirror of reality. The accumulation of factual material, he argued, was not an end in itself. The aim was to understand. One therefore had to prevent the picture from being cluttered by extraneous detail. “The more like a reflection a map becomes,” he pointed out, “the less useful it is as a map.”<sup>107</sup> A theory was like a model. The main purpose of a model was to provide a kind of “awareness of structure.” A theory also had to try to bring out what was of fundamental importance in the object of study. A degree of stylization was thus needed not just for a model to be a model, but also for “theories to *be* theories” and even for “sciences to *be* sciences.”<sup>108</sup>

A premium thus had to be placed on simplicity. The aim was to develop an explanatory system based on a handful of relatively simple assumptions: “The great unifications of Galileo, Kepler, Newton, Maxwell, Einstein, Bohr, Schrödinger and Heisenberg were pre-eminently discoveries of terse formulae from which explanations of diverse phenomena could be generated as a matter of course.”<sup>109</sup> The reaching for simplicity was reflected in a reaching for mathematical form, an attitude captured by Kant’s famous dictum that “in every specific natural science there can be found only so much science proper as there is mathematics present in it.”<sup>110</sup>

If the goal was understanding, the criteria for theory selection therefore *had* to be what they were. Those fundamental criteria—analytical elegance, explanatory fertility, and the like—were not just arbitrary guidelines, reflecting the subjective and aesthetic preferences of the scientific community. They performed a *rational* function.<sup>111</sup> A simple, elegant structure, where a handful of core assumptions had broad and far-reaching implications, allowed hypotheses to be developed deductively; those hypotheses could be tested experimentally, even when the core assumptions could not be tested directly.<sup>112</sup> If a particular hypothesis turned out to be valid, this tended to strengthen the theory; but even if it failed, that finding could be of considerable value.

<sup>106</sup> *Ibid.*, pp. 312–13.

<sup>107</sup> Hanson, *Patterns of Discovery*, p. 28.

<sup>108</sup> Hanson, *Observation and Explanation*, pp. 81–82 (emphasis in original).

<sup>109</sup> Hanson, *Patterns of Discovery*, p. 109.

<sup>110</sup> *Ibid.*, p. 193.

<sup>111</sup> Hanson, *What I Do Not Believe*, p. 300.

<sup>112</sup> Hanson, *Perception and Discovery*, pp. 230–36.

Because the hypothesis had been inferred from general assumptions, its failure would have general implications. It is structure that generates insight: a random observation is of little importance, but the failure of a hypothesis drawn from a larger body of theory could force people to deal with basic issues. "Truth," as Bacon said, "emerges more readily from error than from confusion."<sup>113</sup>

Analytical elegance and explanatory fertility are thus not arbitrary, subjective criteria at all. To develop a powerful deductive system is not just something that people do for essentially aesthetic reasons. The goal of such a system is to provide a certain "awareness of structure": the aim is to see how things fit together. And all this is important because this is *what understanding is*: this is *what it means* to understand a phenomenon.

How much of this applies to historical work? A historian also begins with observations, and those observations "set the problem." A war breaks out: how is it to be explained? The physicist's goal "is a conceptual pattern in terms of which his data will fit intelligibly alongside better-known data."<sup>114</sup> The historian also tries to develop such a pattern—to understand the logic underlying the course of events. In science, "an event is explained when it is traced to other events which require less explanation" and "when it is shown to be part of an intelligible pattern of events."<sup>115</sup> In history, too, the goal is to show how particular events are part of an "intelligible pattern of events." When dealing with events that are at first glance hard to explain (the Pearl Harbor attack, for example), a successful explanation will make those events intelligible by tracing them to causes that are not quite so hard to understand—that is, by constructing a story.

A historical interpretation is the analogue of a physical theory. The aim of an interpretation is also to provide a framework "within which data appear intelligible." A historical interpretation, like a physical theory, "is not pieced together from observed phenomena; it is rather what makes it possible to observe phenomena as being of a certain sort, and as related to other phenomena." The facts in history as in science never just "speak for themselves." And historical interpretations are built up the same way Hanson says physical

<sup>113</sup> Cited in Kuhn, *Structure of Scientific Revolutions*, p. 18. Kuhn does not give the reference, but for the original quotation see Francis Bacon, *The New Organon*, ed. Lisa Jardine and Michael Silverthorne (Cambridge: Cambridge University Press, 2000), p. 173 (book II: XX). The British logician Augustus De Morgan made a similar point: "Wrong hypotheses rightly worked from, have produced more useful results than unguided observation." Quoted in Hanson, *Perception and Discovery*, p. 236. Note also what Karl Popper says in the very first sentence of his *Conjectures and Refutations*, 2d ed. (New York: Basic Books, 1965): "The essays and lectures of which this book is composed are variations upon one very simple theme: the thesis that *we can learn from our mistakes*" (p. vii; emphasis in original).

<sup>114</sup> Hanson, *Patterns of Discovery*, p. 72.

<sup>115</sup> *Ibid.*, p. 94.

theories are. The physicist, he says, “rarely searches for a deductive system *per se*, one in which his data would appear as consequences if only interpreted physically. He is in search, rather, of an explanation of these data; his goal is a conceptual pattern in terms of which his data will fit intelligibly alongside better-known data.” Similarly, the historian rarely sets out to develop a particular interpretation; the interpretation emerges naturally as the historian tries to understand what was going on and to make sense of what the data show.<sup>116</sup>

For Hanson, theories were deductive systems; phenomena were explained by deducing their occurrence from a handful of relatively simple axioms. Such systems placed a heavy premium on simplicity: the “elegance” of a theory was a key measure of its value. In history too, there is a certain premium placed on simplicity and analytical elegance—that is, in giving a picture of the past that does not try to be a photographic reproduction but rather seeks to bring out what was really important. If an interpretation can account for a good deal of what was going on in terms of a few relatively simple and plausible premises, that is very much to its credit; if those premises explain much that is otherwise puzzling or unexpected, then that is even stronger evidence of its power.

So to a certain degree an attempt to understand some historical phenomenon may lead to the construction of a deductive system. And historical explanation—a formalization of the sort of understanding that is reached in the course of doing historical work—should to the extent possible have a kind of deductive structure. To explain the Eisenhower administration’s basic policy toward Europe, for example, the historian could begin by showing that President Eisenhower wanted to pull out of Europe in the not-too-distant future. He or she could then go on to show how this implied that western Europe should constitute a “third great power bloc” in world affairs, able to stand up to Soviet power without direct American support. The different aspects of America’s European policy in that period—the support for European integration, the interest in reducing American troop levels in Europe, the nuclear sharing policy, and so on—could then be shown to follow logically from that basic policy choice. This counts as explanation, even though no social scientific law à la Hempel comes into play. What makes it an explanation is the logic that links the general with the specific—that given Eisenhower’s basic thinking, the specific policies he pursued followed “as a matter of course.”

The historian, however, in reaching for such a structure, has to take care not to push the effort too far. In historical processes, contingent factors loom large; the logic of historical change is never as tight as the logic of a mathe-

<sup>116</sup> Ibid., pp. 72, 90. For the facts not “speaking for themselves”: Hanson, *Perception and Discovery*, p. 200, and also Hanson, *Observation and Explanation*, p. 25.

mathematical theorem. The goal might be to see the degree to which a wide variety of observed phenomena can be accounted for by a handful of relatively simple factors. But at the same time the historian needs to be careful not to read more structure into historical reality than is actually there.

The reality test is thus fundamental. Historical interpretations are constructs. They exist in people's minds. But, as Hanson said about causal statements in general, this does not in itself mean that there is anything "grossly subjective" and therefore arbitrary or chimerical about such notions: "they are what they are because the world is what it is."<sup>117</sup> It is not as though observations are like dots on a piece of paper that can be connected to each other however one pleases. Only certain connections can legitimately be drawn: the sort of picture that can legitimately be painted on the basis of those data is limited by the fact that "the world is what it is"—or, in the case of history, by the fact that the past was what it was.

How far can a scholar go in constructing an interpretation that is both powerful and intellectually compelling? The answer turns on a whole series of factors. It depends on the nature of the particular subject being studied and on the sort of evidence that is available. But it also depends on the skill and the training of the scholar doing the work. And those skills can be developed. There is a method for dealing with historical problems, a method people can actually learn how to use.

<sup>117</sup> Hanson, *Perception and Discovery*, pp. 312–13.