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EPISTEMOLOGICAL CRISES, DRAMATIC NARRATIVE AND THE PHILOSOPHY OF SCIENCE

I

What is an epistemological crisis? Consider, first, the situation of ordinary agents who are thrown into such crises. Someone who has believed that he was highly valued by his employers and colleagues is suddenly fired; someone proposed for membership of a club whose members were all, so he believed, close friends is blackballed. Or someone falls in love and needs to know what the loved one *really* feels; someone falls out of love and needs to know how he or she can possibly have been so mistaken in the other. For all such persons the relationship of *seems* to *is* becomes crucial. It is in such situations that ordinary agents who have never learned anything about academic philosophy are apt to rediscover for themselves versions of the other-minds problem and the problem of the justification of induction. They discover, that is, that there is a problem about the rational justification of inferences from premises about the behaviour of other people to conclusions about their thoughts, feelings, and attitudes and of inferences from premises about how individuals have acted in the past to conclusions expressed as generalizations about their behaviour—generalizations which would enable us to make reasonably reliable predications about their future behaviour. What they took to be evidence pointing unambiguously in some one direction now turns out to have been equally susceptible of rival interpretations. Such a discovery is often paralysing, and were we all of us all of the time to have to reckon with the multiplicity of possible interpretations open to us, social life as we know it could scarcely continue. For social life is sustained by the assumption that we are, by and large, able to construe each others' behaviour—that error, deception, self-deception, irony and ambiguity, although omnipresent in social life, are not so pervasive as to render reliable reasoning and reasonable action impossible. But can this assumption in any way be vindicated?

Consider what it is to share a culture. It is to share schemata which are at one and the same time constitutive of and normative for intelligible action by myself and are also means for my interpretations of the actions of others. My ability to understand what you are doing and my ability to act intelligibly

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(both to myself and to others) are one and the same ability. It is true that I cannot master these schemata without also acquiring the means to deceive, to make more or less elaborate jokes, to exercise irony and utilize ambiguity, but it is also, and even more importantly, true that my ability to conduct any successful transactions depends on my presenting myself to most people most of the time in unambiguous, unironical, undeceiving, intelligible ways. It is these schemata which enable inferences to be made from premises about past behaviour to conclusions about future behaviour and present inner attitudes. They are not, of course, empirical generalisations; they are prescriptions for interpretation. But while it is they which normally preserve us from the pressure of the other-minds problem and the problem of induction, it is precisely they which can in certain circumstances thrust those very problems upon us.

For it is not only that an individual may rely on the schemata which have hitherto informed all his interpretations of social life and find that he or she has been led into radical error or deception, so that for the first time the schemata are put in question—perhaps for the first time they also in this moment become visible to the individual who employs them—but it is also the case that the individual may come to recognise the possibility of systematically different possibilities of interpretation, of the existence of alternative and rival schemata which yield mutually incompatible accounts of what is going on around him. Just this is the form of epistemological crisis encountered by ordinary agents and it is striking that there is not a single account of it anywhere in the literature of academic philosophy. Perhaps this is an important symptom of the condition of that discipline. But happily we do possess one classic study of such crises. It is Shakespeare's *Hamlet*.

Hamlet arrives back from Wittenberg with too many schemata available for interpreting the events at Elsinore of which already he is a part. There is the revenge schema of the Norse sagas; there is the renaissance courtier's schema; there is a Machiavellian schema about competition for power. But he not only has the problem of which schema to apply; he also has the other ordinary agents' problem: whom now to believe? His mother? Rosencrantz and Guildenstern? His father's ghost? Until he has adopted some schema he does not know what to treat as evidence; until he knows what to treat as evidence he cannot tell which schema to adopt. Trapped in this epistemological circularity the general form of his problem is: 'what is going on here?' Thus Hamlet's problem is close to that of the literary critics who have asked: "What is going on in *Hamlet*?" And it is close to that of directors who have asked: "What should be cut and what should be included in my production so that the audience may understand what is going on in *Hamlet*?"

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The resemblance between Hamlet's problem and that of the critics and directors is worth noticing; for it suggests that both are asking a question which could equally well be formulated as: 'what is going on in *Hamlet*?' or 'how ought the narrative of these events to be constructed?' Hamlet's problems arise because the dramatic narrative of his family and of the kingdon of Denmark through which he identified his own place in society and his relationships to others has been disrupted by radical interpretative doubts. His task is to reconstitute, to rewrite that narrative, reversing his understanding of past events in the light of present responses to his probing. This probing is informed by two ideals, truth and intelligibility, and the pursuit of both is not always easily coherent. The discovery of an hitherto unsuspected truth is just what may disrupt an hitherto intelligible account. And of course while Hamlet tries to discover a true and intelligible narrative of the events involving his parents and Claudius, Gertrude and Claudius are trying to discover a true and intelligible narrative of Hamlet's investigation. To be unable to render oneself intelligible is to risk being taken to be mad, is, if carried far enough, to be mad. And madness or death may always be the outcomes which prevent the resolution of an epistemological crisis, for an epistemological crisis is always a crisis in human relationships.

When an epistemological crisis is resolved, it is by the construction of a new narrative which enables the agent to understand *both* how he or she could intelligibly have held his or her original beliefs *and* how he or she could have been so drastically misled by them. The narrative in terms of which he or she at first understood and ordered experiences is itself made into the subject of an enlarged narrative. The agent has come to understand how the criteria of truth and understanding must be reformulated. He has had to become epistemologically self-conscious and at a certain point he may have come to acknowledge two conclusions: the first is that his new forms of understanding may themselves in turn come to be put in question at any time; the second is that, because in such crises the criteria of truth, intelligibility and rationality may always themselves be put in question—as they are in *Hamlet*—we are never in a position to claim that now we possess the truth or now we are fully rational. The most that we can claim is that this is the best account which anyone has been able to give so far, and that our beliefs about what the marks of 'a best account so far' are will themselves change in what are at present unpredictable ways.

Philosophers have often been prepared to acknowledge this historical character in respect of scientific theories; but they have usually wanted to exempt their own thinking from the same historicity. So, of course, have writers of dramatic narrative; *Hamlet* is unique among plays in its openness to reinterpretation. Consider, by contrast, Jane Austen's procedure in *Emma*.

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Emma insists on viewing her protegé, Harriet, as a character in an eighteenth-century romance. She endows her, deceiving both herself and Harriet, with the conventional qualities of the heroine of such a romance. Harriet's parentage is not known; Emma converts her into the foundling heroine of aristocratic birth so common in such romances. And she designs for Harriet precisely the happy ending of such a romance, marriage to a superior being. By the end of *Emma* Jane Austen has provided Emma with some understanding of what it was in herself that had led her not to perceive the untruthfulness of her interpretation of the world in terms of romance. *Emma* has become a narrative about narrative. But Emma, although she experiences moral reversal, has only a minor epistemological crisis, if only because the standpoint which she now, through the agency of Mr. Knightly, has come to adopt, is presented as though it were one from which *the* world as it is can be viewed. False interpretation has been replaced not by a more adequate interpretation, which itself in turn may one day be transcended, but simply by the truth. We of course can see that Jane Austen is merely replacing one interpretation by another, but Jane Austen herself fails to recognise this and so has to deprive Emma of this recognition too.

Philosophers have customarily been Emmas and not Hamlets, except that in one respect they have often been even less perceptive than Emma. For Emma it becomes clear that her movement towards the truth necessarily had a moral dimension. Neither Plato nor Kant would have demurred. But the history of epistemology, like the history of ethics itself, is usually written as though it were not a moral narrative, that is, in fact as though it were not a narrative. For narrative requires an evaluative framework in which good or bad character helps to produce unfortunate or happy outcomes.

One further aspect of narratives and their role in epistemological crises remains to be noticed. I have suggested that epistemological progress consists in the construction and reconstruction of more adequate narratives and forms of narrative and that epistemological crises are occasions for such reconstruction. But if this were really the case then two kinds of questions would need to be answered. The first would be of the form: how does this progress begin? What are the narratives from which we set out? The second would be of the form: how comes it, then, that narrative is not only given so little place by thinkers from Descartes onwards, but has so often before and after been treated as a merely aesthetic form? The answers to these questions are not entirely unconnected.

We begin from myth, not only from the myths of primitive peoples, but from those myths or fairy stories which are essential to a well-ordered childhood. Bruno Bettelheim has written: "Before and well into the oedipal period (roughly, the ages between three and six or seven), the child's ex-

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perience of the world is chaotic. . . . During and because of the oedipal struggles, the outside world comes to hold more meaning for the child and he begins to try to make some sense of it. . . As a child listens to a fairy tale, he gets ideas about how he may create order out of the chaos that is his inner life."[1] It is from fairy tales, so Bettelheim argues, that the child learns how to engage himself with and perceive an order in social reality; and the child who is deprived of the right kind of fairy tale at the right age later on is apt to have to adopt strategies to evade a reality he has not learned how to interpret or to handle.

"The child asks himself, 'Who am I? Where did I come from? How did the world come into being? Who created man and all the animals? What is the purpose of life?'. . . . He wonders who or what brings adversity upon him and what can protect him against it. Are there benevolent powers in addition to his parents? *Are* his parents benevolent powers? How should he form himself, and why? Is there hope for him, though he may have done wrong? Why did all this happen to him? What will it mean to his future?"[2] The child originally requires answers that are true to his own experience, but of course the child comes to learn the inadequacy of that experience. Bettelheim points out that the young child told by adults that the world is a globe suspended in space and spinning at incredible speeds may feel bound to repeat what they say, but would find it immensely more plausible to be told that the earth is held up by a giant. But in time the young child learns that what the adults told him in indeed true. And such a child may well become a Descartes, one who feels that all narratives are misleading fables when compared with what he now takes to be the solid truth of physics.

Yet to raise the question of truth need not entail rejecting myth or story as the appropriate and perhaps the only appropriate form in which certain truths can be told. The child may become not a Descartes, but a Vico or a Hamann who writes a story about how he had to escape from the hold which the stories of his childhood and the stories of the childhood of the human race originally had upon him in order to discover how stories can be true stories. Such a narrative will be itself a history of epistemological transitions and this narrative may well be brought to a point at which questions are thrust upon the narrator which make it impossible for him to continue to use it as an instrument of interpretation. Just this, of course, happens to Descartes, who having abjured history as a means to truth, recounts to us his own history as the medium through which the search for truth is to be carried on. For Descartes and for others this moment is that at which an epistemological crisis occurs. And all those questions which the child has asked of the teller of fairy tales arise in a new adult form. Philosophy is now set the same task that had once been set for myth.

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II

Descartes's description of his own epistemological crisis has, of course, been uniquely influential. Yet Descartes radically misdescribes his own crisis and thus has proved a highly misleading guide to the nature of epistemological crises in general. The agent who is plunged into an epistemological crisis knows something very important: that a schema of interpretation which he has trusted so far has broken down irremediably in certain highly specific ways. So it is with Hamlet. Descartes, however, starts from the assumption that he knows nothing whatsoever until he can discover a presuppositionless first principle on which all else can be founded. Hamlet's doubts are formulated against a background of what he takes to be—rightly—well-founded beliefs; Descartes's doubt is intended to lack any such background. It is to be contextless doubt. Hence also that tradition of philosophical teaching arises which presupposes that Cartesian doubts can be entertained by anyone at any place or time. But of course someone who really believed that he knew nothing would not even know how to begin on a course of radical doubt; for he would have no conception of what his task might be, of what it would be to settle his doubts and to acquire well-founded beliefs. Conversely, anyone who knows enough to know *that* does indeed possess a set of extensive epistemological beliefs which he is not putting in doubt at all.

Descartes's failure is complex. First of all he does not recognise that among the features of the universe which he is not putting in doubt is his own capacity not only to use the French and the Latin languages, but even to express the same thought in both languages; and as a consequence he does not put in doubt what he has inherited in and with these languages, namely, a way of ordering both thought and the world expressed in a set of meanings. These meanings have a history; seventeenth-century Latin bears the marks of having been the language of scholasticism, just as scholasticism was itself marked by the influence of twelfth and thirteenth-century Latin. It was perhaps because the presence of his languages was invisible to the Descartes of the *Discours* and the *Meditationes* that he did not notice either what Gilson pointed out in detail, how much of what he took to be the spontaneous reflections of his own mind was in fact a repetition of sentences and phrases from his school textbooks. Even the *Cogito* is to be found in Saint Augustine.

What thus goes unrecognised by Descartes is the presence not only of languages, but of a tradition—a tradition that he took himself to have successfully disowned. It was from this tradition that he inherited his epistemological ideals. For at the core of this tradition was a conception of knowledge as analogous to vision: the mind's eye beholds its objects by the light of reason. At the same time this tradition wishes to contrast sharply

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knowledge and sense-experience, including visual experience. Hence there is metaphorical incoherence at the heart of every theory of knowledge in this Platonic and Augustinian tradition, an incoherence which Descartes unconsciously reproduces. Thus Descartes also cannot recognise that he is responding not only to the timeless demands of scepticism, but to a highly specific crisis in one particular social and intellectual tradition.

One of the signs that a tradition is in crisis is that its accustomed ways for relating *seems* and *is* begin to break down. Thus the pressures of scepticism become more urgent and attempts to do the impossible, to refute scepticism once and for all, become projects of central importance to the culture and not mere private academic enterprises. Just this happens in the late middle ages and the sixteenth century. Inherited modes of ordering experience reveal too many rival possibilities of interpretation. It is no accident that there are a multiplicity of rival interpretations of both the thought and the lives of such figures as Luther and Machiavelli in a way that there are not for such equally rich and complex figures as Abelard and Aquinas. Ambiguity, the possibility of alternative interpretations, becomes a central feature of human character and activity. *Hamlet* is Shakespeare's brilliant mirror to the age, and the difference between Shakespeare's account of epistemological crises and Descartes's is now clear. For Shakespeare invites us to reflect on the crisis of the self as a crisis in the tradition which has formed the self; Descartes by his attitude to history and to fable has cut himself off from the possibility of recognising himself; he has invented an unhistorical self-endorsed self-consciousness and tries to describe his epistemological crisis in terms of it. Small wonder that he misdescribes it.

Consider by contrast Galileo. When Galileo entered the scientific scene, he was confronted by much more than the conflict between the Ptolemaic and Copernican astonomies. The Ptolemaic system was itself inconsistent both with the widely accepted Platonic requirements for a true astronomy and with the perhaps even more widely accepted principles of Aristotelian physics. These latter were in turn inconsistent with the findings over two centuries of scholars at Oxford, Paris and Padua about motion. Not surprisingly, instrumentalism flourished as a philosophy of science and Osiander's instrumentalist reading of Copernicus was no more than the counterpart to earlier instrumentalist interpretations of the Ptolemaic system. Instrumentalism, like attempts to refute scepticism, is characteristically a sign of a tradition in crisis.

Galileo resolves the crisis by a threefold strategy. He rejects instrumentalism; he reconciles astronomy and mechanics; and he redefines the place of experiment in natural science. The old mythological empiricist view of

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Galileo saw him as appealing to the facts against Ptolemy and Aristotle; what he actually did was to give a new account of what an appeal to the facts had to be. Wherein lies the superiority of Galileo to his predecessors? The answer is that he, for the first time, enables the work of all his predecessors to be evaluated by a common set of standards. The contributions of Plato, Aristotle, the scholars at Merton College, Oxford, and at Padua, the work of Copernicus himself at last all fall into place. Or, to put matters in another and equivalent way: the history of late medieval science can finally be cast into a coherent narrative. Galileo's work implies a rewriting of the narrative which constitutes the scientific tradition. For it now became retrospectively possible to identify those anomalies which had been genuine counterexamples to received theories from those anomalies which could justifiably be dealt with by ad hoc explanatory devices or even ignored. It also became retrospectively possible to see how the various elements of various theories had fared in their encounters with other theories and with observations and experiments, and to understand how the form in which they had survived bore the marks of those encounters. A theory always bears the marks of its passage through time and the theories with which Galileo had to deal were no exception.

Let me cast the point which I am trying to make about Galileo in a way which, at first sight, is perhaps paradoxical. We are apt to suppose that because Galileo was a peculiarly great scientist, therefore he has his own peculiar place in the history of science. I am suggesting instead that it is because of his peculiarly important place in the history of science that he is accounted a particularly great scientist. The criterion of a successful theory is that it enable us to understand its predecessors in a newly intelligible way. It, at one and the same time, enables us to understand precisely why its predecessors have to be rejected or modified and also why, without and before its illumination, past theory could have remained credible. It introduces new standards for evaluating the past. It recasts the narrative which constitutes the continuous reconstruction of the scientific tradition.

This connection between narrative and tradition has hitherto gone almost unnoticed, perhaps because tradition has usually been taken seriously only by conservative social theorists. Yet those features of tradition which emerge as important when the connection between tradition and narrative is understood are ones which conservative theorists are unlikely to attend to. For what constitutes a tradition is a conflict of interpretations of that tradition, a conflict which itself has a history susceptible of rival interpretations. If I am a Jew, I have to recognise that the tradition of Judaism is partly constituted by a continuous argument over what it means to be a Jew. Suppose I am an American: the tradition is one partly constituted by continuous argu-

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ment over what it means to be an American and partly by continuous argument over what it means to have rejected tradition. If I am an historian, I must acknowledge that the tradition of historiography is partly, but centrally, constituted by arguments about what history is and ought to be, from Hume and Gibbon to Namier and Edward Thompson. Notice that all three kinds of tradition—religious, political, intellectual, involve epistemological debate as a necessary feature of their conflicts. For it is not merely that different participants in a tradition disagree; they also disagree as to how to characterize their disagreements and as to how to resolve them. They disagree as to what constitutes appropriate reasoning, decisive evidence, conclusive proof.

A tradition then not only embodies the narrative of an argument, but is only to be recovered by an argumentative retelling of that narrative which will itself be in conflict with other argumentative retellings. Every tradition therefore is always in danger of lapsing into incoherence and when a tradition does so lapse it sometimes can only be recovered by a revolutionary reconstitution. Precisely such a reconstitution of a tradition which had lapsed into incoherence was the work of Galileo.

It will now be obvious why I introduced the notion of tradition by alluding negatively to the viewpoint of conservative theorists. For they, from Burke onwards, have wanted to counterpose tradition and reason and tradition and revolution. Not reason, but prejudice; not revolution, but inherited precedent; these are Burke's key oppositions. Yet if the present arguments are correct it is traditions which are the bearers of reason, and traditions at certain periods actually require and need revolutions for their continuance. Burke saw the French Revolution as merely the negative overthrow of all that France had been and many French conservatives have agreed with him, but later thinkers as different as Péguy and Hilaire Belloc were able retrospectively to see the great revolution as reconstituting a more ancient France, so that Jeanne D'Arc and Danton belong within the same single, if immensely complex, tradition.

Conflict arises, of course, not only within, but between traditions and such a conflict tests the resources of each contending tradition. It is yet another mark of a degenerate tradition that it has contrived a set of epistemological defences which enable it to avoid being put in question or at least to avoid recognising that it is being put in question by rival traditions. This is, for example, part of the degeneracy of modern astrology, of some types of psychoanalytic thought, and of liberal Protestantism. Although, therefore, any feature of any tradition, any theory, any practice, any belief can always under certain conditions be put in question, the practice of putting in question, whether within a tradition or between traditions, itself always

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requires the context of a tradition. Doubting is a more complex activity than some sceptics have realised. To say to oneself or to someone else "Doubt all your beliefs here and now" without reference to historical or autobiographical context is not meaningless; but it is an invitation not to philosophy, but to mental breakdown, or rather to philosophy as a means of mental breakdown. Descartes concealed from himself, as we have seen, an unacknowledged background of beliefs which rendered what he was doing intelligible and sane to himself and to others. But suppose that he had put that background in question too—what would have happened to him then?

We are not without clues, for we do have the record of the approach to breakdown in the life of one great philosopher. "For I have already shown," wrote Hume,

> "that the understanding, when it acts alone, and according to its most general principles, entirely subverts itself, and leaves not the lowest degree of evidence in any proposition, either in philosophy or common life. . . . The *intense* view of these manifold contradictions and imperfections in human reason has so wrought upon me, and heated my brain, that I am ready to reject all belief and reasoning, and can look upon no opinion even as more probable or likely than another. Where am I, or what? From what causes do I derive my existence, and to what condition shall I return? Whose favour shall I court, and whose anger must I dread? What beings surround me? and on whom have I any influence? I am confronted with all these questions, and begin to fancy myself in the most deplorable condition imaginable, inviron'd with the deepest darkness and utterly depriv'd of the use of every member and faculty.[3]

[*Treatise*, ed. Selby-Bigge, Bk. I, iv, vii, pp. 267–69]

We may note three remarkable features of Hume's cry of pain. First, like Descartes, he has set a standard for the foundations of his beliefs which could not be met; hence all beliefs founder equally. He has not asked if he can find good reasons for preferring in respect of the best criteria of reason and truth available some among others of the limited range of possibilities of belief which actually confront him in his particular cultural situation. Secondly, he is in consequence thrust back without any answers or possibility of answers upon just that range of questions that, according to Bettelheim, underlie the whole narrative enterprise in early childhood. There is indeed the most surprising and illuminating correspondence between the questions which Bettelheim ascribes to the child and the questions framed by the adult, but desperate, Hume. For Hume by his radical scepticism has lost any means of making himself—or others—intelligible to himself, let alone, to others. His very scepticism itself becomes unintelligible.

There is perhaps a possible world in which 'empiricism' would have become the name of a mental illness, while 'paranoia' would be the name of a

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well-accredited theory of knowledge. For in this world empiricists would be consistent and unrelenting—unlike Hume—and they would thus lack any means to order their experience of other people or of nature. Even a knowledge of formal logic would not help them; for until they knew how to order their experiences they would possess neither sentences to formalize nor reasons for choosing one way of formalizing them rather than another. Their world would indeed be reduced to that chaos which Bettelheim perceives in the child at the beginning of the oedipal phase. Empiricism would lead not to sophistication, but to regression. Paranoia by contrast would provide considerable resources for living in the world. The empiricist maxims 'Believe only what can be based upon sense-experience' or Occam's razor, would leave us bereft of all generalizations and therefore of all attitudes towards the future (or the past). They would isolate us in a contentless present. But the paranoid maxims 'Interpret everything which happens as an outcome of envious malice' and 'Everyone and everything will let you down' receive continuous confirmation for those who adopt them. Hume cannot answer the question: "What beings surround me?" But Kafka knew the answer to this very well: "In fact the clock has certain personal relationships to me, like many things in the room, save that now, particularly since I gave notice—or rather since I was given notice. . . —they seem to be beginning to turn their backs on me, above all the calendar. . . .Lately it is as if it had been metamorphosed. Either it is absolutely uncommunicative—for example, you want its advice, you go up to it, but the only thing it says is 'Feast of the Reformation'—which probably has a deeper significance, but who can discover it?—or, on the contrary, it is nastily ironic."[4]

So in this possible world they will speak of Hume's Disease and of Kafka's Theory of Knowledge. Yet is this possible world so different from that which we inhabit? What leads us to segregate at least some types of mental from ordinary, sane behaviour is that they presuppose and embody ways of interpreting the natural and social world which are radically discordant with our customary and, as we take it, justified modes of interpretation. That is, certain types of mental illness seem to presuppose rival theories of knowledge. Conversely every theory of knowledge offers us schemata for accepting some interpretations of the natural and social world rather than others. As Hamlet discovered earlier, the categories of psychiartry and of epistemology must be to some extent interdefinable.

III

What I have been trying to sketch are a number of conceptual connections which link such notions as those of an epistemological crisis, a

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narrative, a tradition, natural science, scepticism and madness. There is one group of recent controversies in which the connections between these concepts has itself become a central issue. I refer, of course, to the debates which originated from the confrontation between Thomas Kuhn's philosophy of science and the views of those philosophers of science who in one way or another are the heirs of Sir Karl Popper. It is not surprising therefore that the positions which I have taken should imply conclusions about those controversies, conclusions which are not quite the same as those of any of the major participants. Yet it is perhaps because the concepts which I have examined—such as those on epistemological crisis and of the relationship of conflict to tradition—have provided the largely unexamined background to the recent debates that their classification may in fact help to resolve some of the issues. In particular I shall want to argue that the positions of some of the most heated antagonists—notably Thomas Kuhn and Imre Lakatos—can be seen to converge once they are emended in ways towards which the protagonists themselves have moved in their successive reformulations of their positions.

One very striking new conclusion will however also emerge. For I shall want to reinforce my thesis that dramatic narrative is the crucial form for the understanding of human action and I shall want to argue that natural science can be a rational form of enquiry if and only if the writing of a true dramatic narrative—that is, of history understood in a particular way—can be a rational activity. Scientific reason turns out to be subordinate to, and intelligible only in terms of, historical reason. And if this is true of the natural sciences, *a fortiori* it will be true also of the social sciences.

It is therefore sad that social scientists have all too often treated the work of writers such as Kuhn and Lakatos as it stood. Kuhn's writing in particular has been invoked time and again—for a period of ten years or so, a ritual obeisance towards Kuhn seems almost to have been required in presidential addresses to the American Political Science Association—to license the theoretical failures of social science. But while Kuhn's work uncriticised—or for that matter Popper or Lakatos uncriticised—represent a threat to our understanding, Kuhn's work criticised provides an illuminating application for the ideas which I have been defending.

My criticisms of Kuhn will fall into three parts. In the first I shall suggest that his earlier formulations of his position are much more radically flawed than he himself has acknowledged. I shall then argue that it is his failure to recognise the true character of the flaws in his earlier formulations which leads to the weakness of his later revisions. Finally I shall suggest a more adequate form of revision.

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What Kuhn originally presented was an account of epistemological crises in natural science which is essentially the same as the Cartesian account of epistemological crises in philosophy. This account was superimposed on a view of natural science which seems largely indebted to the writings of Michael Polanyi (Kuhn nowhere acknowledges any such debt). What Polanyi had shown is that all justification takes place within a social tradition and that the pressures of such a tradition enforce often unrecognised rules by means of which discrepant pieces of evidence or difficult questions are often put on one side with the tacit assent of the scientific community. Polanyi is the Burke of the philosophy of science and I mean this analogy with political and moral philosophy to be taken with great seriousness. For all my earlier criticisms of Burke now become relevant to the criticism of Polanyi. Polanyi, like Burke, understands tradition as essentially conservative and essentially unitary. (Paul Feyerabend—at first sight so different from Polanyi—agrees with Polanyi in his understanding of tradition. It is just because he so understands the scientific tradition that he rejects it and has turned himself into the Emerson of the philosophy of science; not "Every man his own Jesus," but "Every man his own Galileo.") He does not see the omnipresence of conflict—sometimes latent—within living traditions. It is because of this that anyone who took Polanyi's view would find it very difficult to explain how a transition might be made from one tradition to another or how a tradition which had lapsed into incoherence might be reconstructed. Since reason operates only *within* traditions and communities according to Polanyi, such a transition or a reconstruction could not be a work of reason. It would have to be a leap in the dark of some kind.

Polanyi never carried his argument to this point. But what is a major difficulty in Polanyi's position was presented by Kuhn as though it were a discovery. Kuhn did of course recognise very fully how a scientific tradition may lapse into incoherence. And he must have (with Feyerabend) the fullest credit for recognising in an original way the significance and character of incommensurability. But the conclusions which he draws, namely that "proponents of competing paradigms must fail to make complete contact with each other's viewpoints" and that the transition from one paradigm to another requires a "conversion experience" do not follow from his premises concerning incommensurability. These last are threefold: adherents of rival paradigms during a scientific revolution disagree about what set of problems provide the test for a successful paradigm in that particular scientific situation; their theories embody very different concepts; and they "see different things when they look from the same point in the same direction." Kuhn concludes that "just because it is a transition between incommensurables" the

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transition cannot be made step by step; and he uses the expression "gestalt switch" as well as "conversion experience." What is important is that Kuhn's account of the transition requires an additional premise. It is not just that the adherents of rival paradigms disagree, but that *every* relevant area of rationality is invaded by that disagreement. It is not just that threefold incommensurability is present, but rationality apparently cannot be present in any other form. Now this additional premise would indeed follow from Polanyi's position and if Kuhn's position is understood as presupposing something like Polanyi's, then Kuhn's earlier formulations of his positions become all too intelligible; and so do the accusations of irrationalism by his critics, accusations which Kuhn professes not to understand.

What follows from the position thus formulated? It is that scientific revolutions are epistemological crises understood in a Cartesian way. Everything is put in question simultaneously. There is no rational continuity between the situation at the time immediately preceding the crisis and any situation following it. To such a crisis the language of evangelical conversion would indeed be appropriate. We might indeed begin to speak with the voice of Pascal, lamenting that the highest achievement of reason is to learn what reason cannot achieve. But of course, as we have already seen, the Cartesian view of epistemological crises is false; it can never be the case that everything is put in question simultaneously. That would indeed lead to large and unintelligible lacunas not only in the history of practices, such as those of the natural sciences, but also in the personal biographies of scientists.

Moreover Kuhn does not distinguish between two kinds of transition experience. The experience which he is describing seems to be that of the person who having been thoroughly educated into practices defined and informed by one paradigm has to make the transition to a form of scientific practice defined and informed by some radically different paradigm. Of this kind of person what Kuhn asserts may well on occasion be true. But such a scientist is always being invited to make a transition that has already been made by others; the very characterization of his situation presupposes that the new paradigm is already operative while the old still retains some power. But what of the very different type of transition made by those scientists who first invented or discovered the new paradigm? Here Kuhn's divergences from Polanyi ought to have saved him from his original Polanyi-derived conclusion. For Kuhn does recognise very fully and insightfully how traditions lapse into incoherence. What some, at least, of those who are educated into such a tradition may come to recognise is the gap between its *own* epistemological ideals and its actual practices. Of those who recognise this some may tend towards scepticism and some towards instrumentalism. Just this, as we have already seen,

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characterised late medieval and sixteenth-century science. What the scientific genius, such as Galileo, achieves in his transition, then, is not only a new way of understanding nature, but also and inseparably a new way of understanding the old science's way of understanding nature. It is because only from the standpoint of the new science can the inadequacy of the old science be characterized that the new science is taken to be more adequate than the old. It is from the standpoint of the new science that the continuities of narrative history are reestablished.

Kuhn has of course continuously modified his earlier formulations and to some degree his position. He has in particular pointed out forcefully to certain of his critics that it is they who have imputed to him the thesis that scientific revolutions are nonrational or irrational events, a conclusion which he has never drawn himself. His own position is "that, if history or any other empirical discipline leads us to believe that the development of science depends essentially on behavior that we have previously thought to be irrational, then we should conclude not that science is irrational, but that our notion of rationality needs adjustment here and there."

Feyerabend however, beginning from the same premises as Kuhn, has drawn on his own behalf the very conclusion which Kuhn so abhors. And surely if scientific revolutions were as Kuhn describes them, if there were nothing more to them than such features as the threefold incommensurability, Feyerabend would be in the right. Thus if Kuhn is to, as he says, "adjust" the notion of rationality, he will have to find the signs of rationality in some feature of scientific revolutions to which he has not yet attended. Are there such features? Certainly, but they belong precisely to the history of these episodes. It is more rational to accept one theory or paradigm and to reject its predecessor when the later theory or paradigm provides a stand-point from which the acceptance, the life-story, and the rejection of the previous theory or paradigm can be recounted in more intelligible historical narrative than previously. An understanding of the concept of the superiority of one physical theory to another requires a prior understanding of the concept of the superiority of one historical narrative to another. The theory of scientific rationality has to be embedded in a philosophy of history.

What is carried over from one paradigm to another are epistemological ideals and a correlative understanding of what constitutes the progress of a single intellectual life. Just as Descartes's account of his own epistemological crisis was only possible by reason of Descartes's ability to recount his own history, indeed to live his life as a narrative about to be cast into a history—an ability which Descartes himself could not recognise without falsifying his own account of epistemological crises—so Kuhn and Feyera-

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bend recount the history of epistemological crises as moments of almost total discontinutiy without noticing the historical continuity which makes their own intelligible narratives possible. Something very like this position, which I have approached through a criticism of Kuhn, was reached by Lakatos in the final stages of his journey away from Popper's initial positions.

If Polanyi is the Burke of the philosophy of science and Feyerabend the Emerson, then Popper himself or at least his disciples inherit the role of J.S. Mill—as Feyerabend has already noticed. The truth is to be approached through the free clash of opinion. The logic of the moral sciences is to be replaced by *Logik der Forschung*. Where Burke sees reasoning only within the context of tradition and Feyerabend sees the tradition as merely repressive of the individual, Popper has rightly tried to make something of the notion of rational tradition. What hindered this attempt was the Popperian insistence on replacing the false methodology of induction by a new methodology. The history of Popper's own thought and of that of his most gifted followers was for quite a number of years the history of successive attempts to replace Popper's original falsificationism by some more adequate version, each of which in turn fell prey to counterexamples from the history of science. From one point of view the true heir of these attempts is Feyerabend; for it is he who has formulated the completely general thesis that all such attempts were doomed to failure. There is *no* set of rules as to how science *must* proceed and all attempts to discover such a set founder in their encounter with actual history of science. But when Lakatos had finally accepted this he moved on to new ground.

In 1968, while he was still a relatively conservative Popperian, Lakatos had written: "the appraisal is rather of a *series of theories* than of an isolated *theory*." He went on to develop this notion into that of a research program. The notion of a research program is of course oriented to the future and there was therefore a tension between Lakatos's use of this notion and his recognition that it is only retrospectively that a series of theories can be appraised. In other words what is appraised is always a history; for it is not just a series of theories which is appraised, but a series which stand in various complex relationships to each other through time which is appraised. Indeed what we take to be a single theory is always "a growing developing entity, one which cannot be considered as a static structure."[5] Consider for example the kinetic theory of gases. If we read the scientific textbooks for any period we shall find presented an entirely ahistorical account of the theory. But if we read all the successive textbooks we shall learn not only that the kinetic theory of 1857 was not quite that of 1845 and that the kinetic theory of 1901 is neither that of 1857 nor that of 1965. Yet at each stage the theory bears the marks of its

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previous history, of a series of encounters with confirming or anomalous evidence, with other theories, with metaphysical points of view, and so on. The kinetic theory not merely has, but is an history, and to evaluate it is to evaluate how it has fared in this large variety of encounters. Which of these have been victories, which defeats, which compounds of victory and defeat, and which not classifiable under any of these headings? To evaluate a theory, just as to evaluate a series of theories, one of Lakatos's research programs, is precisely to write that history, that narrative of defeats and victories.

This is what Lakatos recognised in his paper on *History of Science and Its Rational Reconstructions*.[6] Methodologies are to be assessed by the extent to which they satisfy historiographical criteria; the best scientific methodology is that which can supply the best rational reconstruction of the history of science and for different episodes different methodologies may well be successful. But in talking not about history, but about rational reconstructions Lakatos has still not exorcised the ghosts of the older Popperian belief in methodology; for he was quite prepared to envisage the rational reconstruction as 'a caricature' of actual history. Yet it matters enormously that our histories should be true, just as it matters that our scientific theories makes truth one of its goals.

Kuhn interestingly and perhaps oddly insists against Lakatos on truth in history(he accuses Lakatos of replacing genuine history by "philosophy fabricating examples"), but yet denies any notion of truth to natural science other than that truth which attaches to solutions to puzzles and to concrete predictions. In particular he wants to deny that a scientific theory can embody a true ontology, that it can provide a true representative of what is 'really there'. "There is, I think no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle."[7]

This is very odd; because science has certainly shown us decisively that some existence-claims are false just because the entities in question are *not* really there—whatever *any* theory may say. Epicurean atomism is not true, there are no humours, nothing with negative weight exists; phlogiston is one with the witches and the dragons. But other existence-claims have survived exceptionally well through a succession of particular theoretical positions: molecules, cells, electrons. Of course our beliefs about molecules, cells and electrons are by no means what they once were. But Kuhn would be put into a very curious position if he adduced this as a ground for denying that some existence-claims still have excellent warrant and others do not.

What however, worries Kuhn is something else: "in some important respects, though by no means in all, Einstein's general theory of relativity is

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closer to Aristotle's mechanics than either of them is to Newton's."[8] He therefore concludes that the superiority of Einstein to Newton is in puzzle solving and not in an approach to a true ontology. But what an Einstein ontology enables us to understand is why *from the standpoint of an approach to truth* Newtonian mechanics is superior to Aristotelian. For Aristotelian mechanics as it lapsed into incoherence could never have led us to the special theory; construe them how you will, the Aristotelian problems about time will not yield the questions to which special relativity is the answer. A history which moved from Aristotelianism directly to relativistic physics is not an imaginable history.

What Kuhn's disregard for ontological truth neglects is the way in which the progess toward truth in different sciences is such that they have to converge. The easy reductionism of some positivist programs for science was misleading here, but the rejection of such a reductionism must not blind us to the necessary convergence of physics, chemistry and biology. Were it not for a concern for ontological truth the nature of our demand for a coherent and convergent relationship between all the sciences would be unintelligible.

Kuhn's view may, of course, seem attractive simply because it seems consistent with a fallibilism which we have every reason to accept. *Perhaps* Einsteinian physics will one day be overthrown just as Newtonian was; perhaps, as Lakatos in his more colourfully rhetorical moments used to suggest, all our scientific beliefs are, always have been, and always will be false. But it seems to be a presupposition of the way in which we do natural science that fallibilism has to be made consistent with the regulative ideal of an approach to a true account of the fundamental order of things and not vice versa. If this is so, Kant is essentially right; the notion of an underlying order—the kind of order that we would expect if the ingenious, unmalicious god of Newton and Einstein had created the universe—*is* a regulative ideal of physics. We do not need to understand this notion quite as Kant did, and our antitheological beliefs may make us uncomfortable in adopting it. But perhaps discomfort at this point is a sign of philosophical progress.

I am suggesting, then, that the best account that can be given of why some scientific theories are superior to others presupposes the possibility of constructing an intelligible dramatic narrative which can claim historical truth and in which such theories are the subject of successive episodes. It is because and only because we can construct better and worse histories of this kind, histories which can be rationally compared with each other, that we can compare theories rationally too. Physics presupposes history and history of a kind that invokes just those concepts of tradition, intelligibility, and epistemological crisis for which I argued earlier. It is this that enables us to

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understand why Kuhn's account of scientific revolutions can in fact be rescued from the charges of irrationalism levelled by Lakatos and why Lakatos's final writings can be rescued from the charges of evading history levelled by Kuhn. Without this background, scientific revolutions become unintelligible episodes; indeed Kuhn becomes—what in essence Lakatos accused him of being—the Kafka of the history of science. Small wonder that he in turn felt that Lakatos was not an historian, but an historical novelist.

A final thesis can now be articulated. When the connection between narrative and tradition on the one hand, and theory and method on the other, is lost sight of, the philosophy of science is set insoluble problems. Any set of finite observations is compatible with any one out of an infinite set of generalizations. Any attempt to show the rationality of science, once and for all, by providing a rationally justifiable set of rules for linking observations and generalizations break down. This holds, as the history of the Popperian school shows, for falsification as much as for any version of positivism. It holds, as the history of Carnap's work shows, no matter how much progress may be made on detailed, particular structures in scientific inference. It is only when theories are located in history, when we view the demands for justification in highly particular contexts of a historical kind, that we are freed from either dogmatism or capitulation to scepticism. It therefore turns out that the program which dominated the philosophy of science from the eighteenth century onwards, that of combining empiricism and natural science was bound either at worst to break down in irrationalism or at best in a set of successively weakened empiricist programs whose driving force was a deep desire not to be forced into irrationalist conclusions. Hume's Disease is, however, incurable and ultimately fatal and even backgammon (or that type of analytical philosophy which is often the backgammon of the professional philosopher) cannot stave off its progress indefinitely. It is, after all, Vico, and neither Descartes nor Hume, who has turned out to be in the right in approaching the relationship between history and physics.

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NOTES

1. Bruno Bettelheim, *The Uses of Enchantment* (New York: Alfred A. Knopf, 1976), pp. 74–75.
2. Ibid., p. 47.
3. David Hume, *Treatise of Human Nature*, ed. L.A. Selby-Bigge (London: Oxford University Press, 1941), Bk. I, iv, vii, pp. 267–69.

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4. Letter to his sister Valli, in *I Am a Memory Come Alive*, ed. Nahum N. Glatzer (New York: Schocken Books, 1974), p. 235.

5. Richard M. Burian, "More than a Marriage of Convenience: On the Inextricability of History and Philosophy of Science," unpublished paper, p. 38.

6. I. Lakatos, "History of Science and Rational Reconstructions," in Boston Studies in the Philosophy of Science, Vol. VIII, ed. Roger C. Buch and Robert S. Cohen (Dordrecht-Holland: D. Reidel Publishing Co., 1974).

7. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2d ed. (Chicago: University of Chicago Press, 1970), p. 206.

8. Ibid., pp. 206–7.

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