

VISIONS OF POLITICS

Volume 1: Regarding Method

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Introduction: Seeing things their way

'Facts alone are wanted in life', Mr Gradgrind assures us at the start of *Hard Times*.¹ Many historians appear to share Mr Gradgrind's sentiment, but some of the most powerful voices in recent philosophy have questioned whether there are any indisputable facts to be acquired. I am concerned in the chapters that follow with three principal aspects of this sceptical challenge. I shall mainly be writing as a practising historian reflecting on the task in hand. But I shall nevertheless have the temerity to suggest that there are good reasons in each instance for joining the sceptical camp.

One assault on the world of facts was launched some time ago from the direction of the theory of knowledge. This campaign was primarily waged by those who aimed to discredit the empiricist belief that our world consists of sense data capable of being directly perceived and uncontentiously described. It would not be too much to say that by now this particular dogma of empiricism has fallen into very general disrepute. Scarcely anyone nowadays believes in the possibility of building up structures of factual knowledge on foundations purporting to be wholly independent of our judgements.

I seek in chapters 2 and 3 to explore some implications of this post-empiricist critique, implications that seem to me of special relevance for practising historians. My aim in chapter 2 is to reconsider the familiar view that our goal as historians should be to assemble all the facts about a given problem and recount them as objectively as possible. I try to show that this approach is untenable, and to sketch an alternative and more realistic vision of the relationship between historians and their evidence.

In chapter 3 I turn to examine a more specific question about the world of facts. The issue here is one that cannot be evaded by anyone interested in understanding the beliefs of alien cultures or earlier societies. When we

¹ Dickens 1985, p. 47.

examine such beliefs, we often find that they are not merely unfamiliar but appear in many cases to be obviously false. What role should our sense of their truth or falsity play in our attempts to explain them? One influential answer has been that, since false beliefs point to failures of reasoning, we need to begin by considering the truth of the beliefs we study as an indispensable guide to explaining why they were held. My aim in chapter 3 is to demonstrate that this approach, although frequently recommended, is fatal to good historical practice, and I defend the view that the concept of truth is irrelevant to the enterprise of explaining beliefs.

Besides being assailed by epistemologists, the world of facts has been undermined in recent times by developments within the theory of meaning. The cardinal assumption of positivistic philosophies of language was that all meaningful statements must refer to facts, and thus that the meanings of sentences must be given by the method of verifying the assertions contained in them. Quine cast doubt on this whole approach with his insistence that there is no such 'unvarnished news' to report. So did Wittgenstein when he first emphasised the multifarious ways in which languages are actually used, and went on to argue that we should stop asking about the 'meanings' of words and focus instead on the various functions they are capable of performing in different language games.

These powerful critiques were subsequently extended in two related directions. J. L. Austin, John Searle and others proceeded to examine in detail what might be meant by investigating the uses as opposed to the meanings of words. Isolating the concept of a speech act, they pursued the implications of the fact that, whenever we use language for purposes of communication, we are always doing something as well as saying something. Meanwhile H. P. Grice and a number of theoretical linguists went on to reconsider the concept of meaning at issue when we ask what someone may have meant by saying or doing something. This related contribution likewise had the effect of shifting attention away from 'meanings' and towards questions about agency, usage and especially intentionality.

I attempt in chapters 4, 5 and 6 to explore the relevance of these developments for historians of philosophy and intellectual historians more generally. When I originally wrote the article republished here as chapter 4, I was working against a backdrop of assumptions about the importance of the 'perennial issues' in the history of Western thought. It was widely agreed that the question of whether the so-called classic texts remain worthy of study depends on the extent to which they can be

shown to address these perennial issues in a 'relevant' way. I protested that this approach is insensitive to the possibility that earlier thinkers may have been interested in a range of questions very different from our own. More specifically, I objected that, by appropriating the past in this fashion, we leave ourselves no space to consider what earlier philosophers may have been *doing* in writing as they wrote. I began, in other words, to invoke some insights derived from the theory of speech acts to criticise prevailing practices and to plead for a more historically-minded approach to the history of ideas.

My resulting discussion was mainly polemical, although I should add that, in reprinting this early article, I have softened the polemics as well as excising some clumsy formulations and repetitious arguments. While this essay remains more a critique than a programme, it already adumbrates the view of textual interpretation I go on to develop in chapters 5 and 6. In chapter 5 I engage in a ground-clearing exercise, looking for a pathway through the tangled debates about intentionality and the interpretation of texts. In chapter 6 I lay out my own approach to interpretation, attempting at the same time to protect it from a number of misunderstandings and to respond to a number of objections that have subsequently been levelled against it. As I have already intimated, the nerve of my argument is that, if we want a history of philosophy written in a genuinely historical spirit, we need to make it one of our principal tasks to situate the texts we study within such intellectual contexts as enable us to make sense of what their authors were doing in writing them. My aspiration is not of course to enter into the thought-processes of long-dead thinkers; it is simply to use the ordinary techniques of historical enquiry to grasp their concepts, to follow their distinctions, to appreciate their beliefs and, so far as possible, to see things their way.

As will be clear from my stress on the need to recapture what past writers were doing, I mark a strong distinction between what I take to be two separable dimensions of language. One has conventionally been described as the dimension of meaning, the study of the sense and reference allegedly attaching to words and sentences. The other is perhaps best described in Austin's terms as the dimension of linguistic action, the study of the range of things that speakers are capable of doing in (and by) the use of words and sentences. Traditional hermeneutics has generally concentrated almost exclusively on the first of these dimensions. I concentrate at least as much on the second, as will become clear to any reader of volumes 2 and 3 of the present work. One way of summarising my approach would thus be to say that I try to take seriously

the implications of the contention that, as Wittgenstein expresses it in his *Philosophical Investigations*, 'words are also deeds'.²

Reflecting on the idea that speech is also action, I came to the conclusion that the theory of speech acts might have something to tell us about the philosophy of action more generally, and in particular about the role of causality in the explanation of behaviour. I originally explored this suggestion at the end of the article reprinted here as chapter 4, but soon came to see that my argument was seriously confused. Later I decided to try again, and the outcome was the article that appears here (in a much revised and truncated form) as chapter 7. The thesis I defend is that, even if we agree that motives function as causes, there can nevertheless be non-causal explanations of action. This conclusion still seems to me tenable, and certainly represents a big improvement on my original argument. This being so, I have deleted from chapter 4 the section in which I initially tried to mount this case.

Having stumbled into studying the philosophy of action, I found myself confronting yet further questions that seemed to me of great importance for practising historians. What exact role is played by our beliefs in explaining our behaviour? What does it mean to speak of our beliefs as rationally held? What role should be assigned to assessments of rationality in the explanation of beliefs and behaviour? I first tried to broach these questions at the end of the article reprinted here as chapter 7, but again my initial effort was a failure. Here too I decided to try again, and eventually wrote the more extended treatment of these issues to be found in chapters 2 and 3. These discussions supersede my original account, so I have truncated and rewritten the closing sections of chapter 7 in which I first tried to address these themes.

The approach I follow in these chapters reflects my acceptance of the kind of holism we encounter in the philosophies of Quine, Davidson and especially the later Wittgenstein. One of my principal aspirations is to point to the relevance and importance of this movement in post-analytical philosophy for the interpretation of texts and the study of conceptual change. I seek to elucidate concepts not by focusing on the supposed 'meanings' of the terms we use to express them, but rather by asking what can be done with them and by examining their relationship to each other and to broader networks of beliefs. I assume in turn that the question of what it is rational to believe depends in large measure on the nature of our other beliefs. I attempt to interpret specific beliefs by

² Wittgenstein 1958, para. 546, p. 146.

placing them in the context of other beliefs, to interpret systems of belief by placing them in wider intellectual frameworks, and to understand those broader frameworks by viewing them in the light of the *longue durée*.³

So far I have been speaking of post-empiricist theories of meaning and knowledge and their role in destabilising the positivistic world of facts. I next want to consider a third way in which our traditional view of language as a vehicle essentially for expressing and communicating our thoughts has of late been extended and rendered more complicated. One of the most salutary achievements of post-modern cultural criticism has been to improve our awareness of the purely rhetorical aspects of writing and speech, thereby heightening our sensitivity to the relations between language and power. As we have increasingly been made to see, we employ our language not merely to communicate information but at the same time to claim authority for our utterances, to arouse the emotions of our interlocutors, to create boundaries of inclusion and exclusion and to engage in many other exercises of social control.

I proceed in chapters 8, 9 and 10 to address some questions about these textual strategies. It goes without saying that there is much more to be said and done along these lines. My own contribution is confined to the study of one particular range of rhetorical techniques, those concerned with exploiting the power of words to underpin or undermine the construction of our social world. Chapter 8 attempts, by reference to a specific historical example, to illustrate the dependence of social action on the normative descriptions available to us for legitimating our behaviour. This chapter is largely new, although the germ of it can be found in an article I published as long ago as 1974.⁴ Chapter 9 presents a typology of the strategies available for redescribing our social world in such a way as to re-evaluate it at the same time. Chapter 10 investigates in greater detail the specific rhetorical techniques by means of which these ideological tasks are capable of being performed.

Critics have persistently complained that my approach to the history of philosophy robs the subject of its point. If we cannot learn from the perennial wisdom contained in the classic texts, what is the value of studying them? To many of my critics it seems that, by treating these texts as elements in a wider discourse, whose contents change with changing

³ This means that, when I read in Bevir 2001, p. 188 that the holism espoused by Quine and Wittgenstein ‘has had little impact on the philosophy of history’, I feel that I have lived in vain. I imagine that colleagues such as James Tully must feel the same.

⁴ See Skinner 1974, pp. 289–301.

circumstances, I leave them bereft of anything except ‘the dustiest anti-quarian interest’.⁵ I foresaw this depressingly philistine objection and originally tried to counter it at the end of the article reprinted here as chapter 4. My response was far from sufficient to satisfy my critics, however, and I therefore tried to spell it out in greater detail at the end of the article reprinted here as chapter 6. But even that was not enough, and the objection that my work is purely historical, and that nothing can be learned from it, continues to be made.⁶

Perhaps it may be worth trying to restate my argument in a more forthright style. It is true that my work is as historical as I can make it. But it is nevertheless intended at the same time as a contribution to the understanding of our present social world. As I have elsewhere argued,⁷ one of the uses of the past arises from the fact that we are prone to fall under the spell of our own intellectual heritage. As we analyse and reflect on our normative concepts, it is easy to become bewitched into believing that the ways of thinking about them bequeathed to us by the mainstream of our intellectual traditions must be *the* ways of thinking about them. Given this situation, one of the contributions that historians can make is to offer us a kind of exorcism. If we approach the past with a willingness to listen, with a commitment to trying to see things their way, we can hope to prevent ourselves from becoming too readily bewitched. An understanding of the past can help us to appreciate how far the values embodied in our present way of life, and our present ways of thinking about those values, reflect a series of choices made at different times between different possible worlds. This awareness can help to liberate us from the grip of any one hegemonal account of those values and how they should be interpreted and understood. Equipped with a broader sense of possibility, we can stand back from the intellectual commitments we have inherited and ask ourselves in a new spirit of enquiry what we should think of them.

There is also much to be learned from reflecting on what we uncover when we begin to investigate the texture of moral, social and political thinking as it was actually carried on in the past. We encounter endless disputes about the application of evaluative terms; we witness continual struggles to win recognition and legitimacy; and we gain a strong sense of the ideological motivations underlying even the most abstract systems

⁵ Tarlton 1973, p. 314; Gunnell 1982, p. 327.

⁶ See, for example, Wokler 2001, pp. 156–7. But for a more sympathetic appraisal see Hampsher-Monk 2001, pp. 168–74.

⁷ I draw in this paragraph on the discussion in Skinner 1998, pp. 116–17.

of thought. We find, in short, that philosophical argument is often deeply intertwined with claims to social power.

As I indicate in chapter 10, there are several implications one might feel inclined to draw from this spectacle. One is that the principles governing our moral and political life have generally been disputed in a manner more reminiscent of the battlefield than the seminar room. (Or perhaps the moral is that seminar rooms are really battlefields.) A further and connected implication is that it may be right to view with a certain irony those moral and political philosophers of our own day who present us with overarching visions of justice, freedom and other cherished values in the manner of dispassionate analysts standing above the battle. What the historical record strongly suggests is that no one is above the battle, because the battle is all there is. A final moral to be drawn is perhaps that agency deserves after all to be privileged over structure in social explanation. Language, like other forms of social power, is of course a constraint, and it shapes us all. As I try to show in chapters 8 and 9, however, language is also a resource, and we can use it to shape our world.

There is thus a sense in which the following chapters, far from reflecting a depoliticised stance,⁸ may be said to culminate in a political plea. The plea is to recognise that the pen is a mighty sword. We are of course embedded in practices and constrained by them. But those practices owe their dominance in part to the power of our normative language to hold them in place, and it is always open to us to employ the resources of our language to undermine as well as to underpin those practices. We may be freer than we sometimes suppose.

⁸ The progressive depoliticisation of the professional study of political theory over the past two generations is the theme of Wokler 2001.

The practice of history and the cult of the fact

I

British historians are notoriously suspicious of philosophical reflections about the nature of their craft. The charge is no doubt exaggerated, but it is hard to deny that they have sometimes gloried in presenting themselves as straightforward empiricists for whom the proper task of the historian is simply to uncover the facts about the past and recount them as objectively as possible. Despite the inroads of post-modernist culture, this characterisation continues to hold good for many practitioners,¹ and lately their outlook has been defended anew in recent theoretical work.² Among those who have not only adopted this stance but have offered a theoretical justification of it, by far the most eminent in recent times has been Sir Geoffrey Elton, who always combined his large and distinguished output as an historian of early-modern Europe with a forthright willingness to reflect on the nature of historical enquiry, a topic on which he published no fewer than three books.³ While this readiness to come forward as a philosopher of history was unusual, Elton's actual philosophy was a reassuringly familiar one: he presented himself at all times as an unashamed exponent of the cult of the fact.⁴ Elton's theoretical writings may thus be said to offer a particularly illuminating means of assessing the strengths and weaknesses of this approach, and it is accordingly on his vision of the historian's task that I shall concentrate in what follows.

This chapter is a revised and extended version of an article that originally appeared under the title 'Sir Geoffrey Elton and the Practice of History' in *Transactions of the Royal Historical Society*, 6th series, 7 (1997), pp. 301–16.

¹ A point well emphasised in Roberts 1996. For the analogous place of what Peter Novick has called 'hyperobjectivism' in the American historical profession, see the fascinating details in Novick 1988, esp. pp. 573–629.

² See, most notably, Evans 1997, esp. pp. 75–102.

³ For the three main statements of Elton's creed see Elton 1969a, Elton 1970 and Elton 1991.

⁴ I owe this phrase to Liam Hudson, who originally applied it more generally to the methods of British social science. See Hudson 1972.

II

If we begin with Elton's first and fullest consideration of the methods and purposes of historical study, his book entitled *The Practice of History*, we find a revealing metaphor running through the argument. The aspiring historian is pictured as an apprentice – at one point specifically as an apprentice carpenter – who is aiming to produce a first piece of work to be inspected and judged by a master craftsman.⁵ Elton repeatedly speaks of the need for the young scholar to undergo 'a proper apprenticeship'. He must acknowledge that 'his life is that of an apprentice learning a craft'; that he needs to 'train himself to his trade'; and thus that he needs to be 'instructed, guided, and trained'.⁶

One assumption worth noting is that both teacher and pupil are always assumed to be male. A further and pivotal assumption is that teachers and writers of history are best viewed as practitioners of a *techné*, as craftsmen who have mastered a distinctive set of skills and are thus in a position to pass on what Elton describes as 'the truths of practice and experience'.⁷ This commitment is strongly reinforced by the authorial voice we hear throughout Elton's writings on historical method. The tone is very much that of someone who has rules to impart, rules that an apprentice will do well to read, mark and learn if he is to be 'thoroughly and properly trained'.⁸

The first important lesson that the apprentice learns from the opening chapter of *The Practice of History* is that 'history deals in events, not states; it investigates things that happen and not things that are'. From this it is said to follow that historians must think of their analyses 'as steps in a chain of events, as matters explanatory of a sequence of happenings'. They must therefore 'concentrate on understanding change, which is the essential content of historical analysis and description'.⁹ Subsequently this activity is equated with providing explanations of events. The historian's basic duty is 'to consider and explain change', and this ability is identified with the process of 'deducing consequences from disparate facts'.¹⁰

I am not sure how much headway we are to imagine that the apprentice may already have made in his historical studies. But he will not need to have read very much to know that all these contentions are highly debatable. Suppose he has at least turned the pages of some works in the

⁵ For the aspiring historian as an apprentice, see Elton 1969a, pp. 34–5, 144, 159, 216; as an apprentice carpenter, p. 214.

⁶ Elton 1969a, pp. 103, 113, 213, 221. ⁷ Elton 1969a, pp. 15, 19, 160, 187.

⁸ See Elton 1969a, p. 219, and for the theme of teaching more generally cf. pp. 178–221.

⁹ For these quotations see Elton 1969a, p. 22. ¹⁰ Elton 1969a, pp. 37, 128–9, 166.

history of art or philosophy. In that case he will know that by no means all historians are preoccupied with explanation, especially if by that process we mean (in Elton's formula) the deducing of consequences. Some are instead concerned with the provision of interpretations, and thus with the process of placing texts and other such objects within the fields of meaning from which their own individual meanings can arguably be inferred. If, in addition, the apprentice has read any religious or economic history, he will know that even historians concerned with explanation are by no means always interested in explaining events. Some are interested in accounting for such matters as the prevalence of particular belief-systems or the ways in which past systems of production and exchange have worked.

I suppose we are not to imagine that the apprentice will have read any works in the philosophy of history. Certainly he will not have done so if he has been following the lessons of the master, for Elton explicitly assures us in the Preface to *The Practice of History* that 'a philosophic concern with such problems as the reality of historical knowledge or the nature of historical thought only hinders the practice of history'.¹¹ Nevertheless, our imagined apprentice might surely be a sufficiently reflective person to wonder how it can possibly be the case that, as Elton maintains, the way in which historians explain events is by 'deducing consequences from disparate facts'.¹² It is true that a knowledge of consequences may sometimes lead an historian to reconsider the significance of an event. But the result of doing so will not be to explain it; it will merely be to re-identify what stands to be explained. When it comes to explanation, the historian surely needs to focus not on the outcome of events but on the causal conditions of their occurrence.

These considerations might lead one to conclude that Elton must simply have made a slip at this point, and that what he must have meant to write was that historians explain events by way of assigning their causes. Since he insists, however, that 'to suppose that causal relationships are the main content of history is an error', he apparently has no wish to be rescued in this way.¹³ But in that case I cannot make sense of his view of historical explanation, simply because I cannot see how the act of tracing the consequences of an event has any bearing upon the explanatory task of giving an account of why it occurred.

If we turn, however, to Elton's second book on the study of history, we encounter a more sophisticated and extended analysis of historical

¹¹ Elton 1969a, p. vii; cf. also p. 129, where the theoretical literature on historical explanation is dismissed as 'quite remarkably barren and irrelevant'.

¹² Elton 1969a, p. 129. ¹³ Elton 1969a, p. 23.

explanation in which the emphasis is placed entirely on causes rather than consequences. I am referring to *Political History: Principles and Practice*, which Elton originally published in 1970. The first three chapters are largely given over to a more genial if less incisive development of a number of claims already advanced in *The Practice of History* about the alleged primacy of politics in historical studies. But in chapter 4, entitled 'Explanation and Cause', Elton breaks a considerable amount of new ground. He also breaks a considerable number of lances, tilting at the entire philosophical literature on historical explanation with breathtaking self-confidence.

While the outcome is polemically spectacular, the argument is weakened by Elton's insistence that good theory in this area amounts to nothing other than a reflection and restatement of practice.¹⁴ Since it is historians who provide historical explanations, he repeatedly proclaims, it is for them to tell us what makes a good explanation, rather than listening to what he describes as philosophers' nonsense. What is needed is an account of 'what the historian does', an analysis of 'the historian's concept of cause', an investigation into 'what the historian might mean by talking about causes'.¹⁵

Elton may well be right to stress the pragmatic element in the notion of explanation, an element perhaps best captured by saying that good explanations are those which succeed in removing puzzles about the occurrence of facts or events. But it hardly follows that good historical explanations will consist of anything that practising historians may care to offer us in the way of attempting to resolve such puzzles. Historical explanations cannot be immune from assessment as explanations, and the question of what properly counts as an explanation is inescapably a philosophical one. The question cannot be what historians say; the question must be whether what they say makes any sense.

This is not to deny that Elton may be justified in claiming that the philosophers he discusses imposed too stringent a model by making it a requirement of good historical explanations that they be nomological in form, such that the task of the historian is held to be that of explaining facts and events by reference to empirical laws of which they can be shown to be instances.¹⁶ Nevertheless, the philosophers in question were surely right to insist that the provision of causal explanations in history must

¹⁴ See Elton 1970, esp. p. 135, and cf. Elton 1991, esp. pp. 3, 34, 51, 54, 61.

¹⁵ Elton 1970, pp. 125, 136, 145; on philosophers' nonsense see p. 129.

¹⁶ See Elton 1970, esp. pp. 124–30 for his attack on attempts to apply hypothetico-deductive models of explanation to history. His target is the kind of argument put forward in Hempel 1942.

to some extent depend on our capacity to relate particular instances to wider generalities. Elton strongly disagrees, arguing that generalisations are ‘no help at all’ in the search for historical explanations, since historians are always concerned with ‘the particular event’.¹⁷ But the *non sequitur* is blatant: even if it were true that historians are only concerned with particular events, it certainly does not follow that they are under no obligation to investigate causal uniformities in order to explain them. Despite Elton’s assurances, moreover, I cannot myself see how historians can hope to solve any puzzles about the occurrence of facts or events without making some attempt to relate such particulars to a broader explanatory background.

If we now return, however, to the point at which we left Elton’s argument in *The Practice of History*, we find that none of these considerations greatly matters to him after all, since these are not the questions that he chiefly wants the apprentice to address. At the end of chapter 1 he suddenly introduces a new and different claim about the objectives of history. The apprentice is now told that history, ‘to be worthy of itself and beyond itself, must concentrate on one thing’, namely the extraction from all the available evidence of what Elton later calls ‘the true facts’.¹⁸ This is not perhaps a very felicitous way of introducing the argument, since it subsequently emerges that, for Elton, a true statement is a statement of fact, so that the concept of a true fact turns out to be a pleonasm.¹⁹ Nevertheless, the new and contrasting claim he wishes to advance is not in doubt: it is that historians are basically engaged in the assembling of facts with the aim of arriving at the truth.²⁰ Announcing this commitment, Elton declares his unswerving allegiance to the cult of the fact. There can be no doubt, he insists, that ‘the truth can be extracted from the evidence’ and thus that, by uncovering the facts of history, the historian can aspire to discover ‘the true reality of the past’.²¹

Elton’s later pronouncements about historical method admittedly involve some shifting back and forth between these two perspectives. His inaugural lecture at the University of Cambridge, delivered in 1968 and reprinted in his book *Return to Essentials* in 1991, begins by reverting to the claim that ‘the essence of all history is change’.²² His second inaugural

¹⁷ See Elton 1970, pp. 132, 151–2, and cf. the attack on the place of generalisations in explanation at pp. 126–31.

¹⁸ Elton 1969a, pp. 68, 86. ¹⁹ Elton 1969a, pp. 86, 133.

²⁰ Elton 1969a, p. 70. Thereafter the point is continually reiterated; see pp. 74, 97, 101, 117, 123.

²¹ Elton 1969a, pp. 79, 97. ²² Elton 1991, p. 80.

lecture, delivered as Regius Professor of Modern History in 1983 and reprinted in the same volume, speaks in even more emphatic tones about 'the inadequacy of any historical analysis which is not predominantly directed towards an understanding of change through time'.²³ But on the whole it is the alternative idea of extracting the truth from the assembling of facts that wins the day. The first inaugural lecture insists that historians must engage in 'the proper assessment and proper study of evidence', adding that this is because they are 'concerned with one thing only: to discover the truth'.²⁴ Chapter 3 of *Political History*, which is actually entitled 'Evidence', likewise speaks about the bodies of material studied by historians and promises that 'something like the truth can be extracted from them'.²⁵ The second inaugural lecture ends by repeating once more that the sole aim of the historian is that of 'telling the truth about the past'.²⁶ Finally, these are precisely the 'essentials' to which Elton recalls us in his *Return to Essentials* of 1991. The apprentice must acquire 'a professional training' in 'the treatment of the historical evidence' about every event he investigates, with the eventual aim of arriving at 'the truth of the event and all that surrounds it'.²⁷

The second chapter of *The Practice of History* adds some examples to clarify what Elton means by speaking about items of historical evidence.²⁸ The sort of thing he has in mind, he says, is something like a financial account, or the record of a court case, or one of the material relics of the past, such as a house. These are 'far and away the most important and common' types of evidence that the apprentice can expect to encounter, and these are the sorts of documents and factual materials out of which he must extract the truth.²⁹

I imagine the apprentice exhibiting a certain surprise at this point. Perhaps these forms of evidence are the most common, but is it obvious that they are 'far and away the most important'? What about the major works of theology, philosophy and science that adorn our libraries? What about the heritage of great paintings and other works of art that fill our museums and galleries? Elton gives his answer in the concluding chapter of *The Practice of History*. The apprentice must learn to distinguish between optional aspects of historical study and 'real' or 'hard' history.³⁰ The 'hard outline' of historical research and teaching 'must consist of the actions of governments and governed in the public life of the time', this being the

²³ Elton 1991, p. 120.

²⁴ Elton 1991, pp. 89, 91.

²⁵ Elton 1970, p. 84.

²⁶ Elton 1991, p. 125.

²⁷ Elton 1991, pp. 30, 54.

²⁸ The examples are repeated in Elton 1970, pp. 12–13.

²⁹ Elton 1969a, p. 101.

³⁰ Elton 1969a, pp. 190, 197, 199. On 'real' history see also Elton 1970, esp. pp. 22, 32.

only theme ‘sufficiently dominant to carry the others along with it’.³¹ But as long as this forms the ‘backbone’ of our historical studies,³² there is no harm in adding such optional extras as intellectual history or the history of art, although the latter admittedly encourages ‘woolliness and pretence’.³³ Elton even allows that some kinds of intellectual history – for example, the history of political theory – may have a positive value, since the study of people’s thinking about politics ‘bears directly on a main part of the student’s “hard” history’ through its connection with ‘the problem of political organisation and action’.³⁴ By the time Elton came to publish *Return to Essentials*, however, he had noticed with evident dismay that in the meantime the history of ideas had been ‘suddenly promoted from the scullery to the drawing room’.³⁵ To cope with this unforeseen impertinence, he takes greater care in his later work to warn the apprentice that intellectual history is not ‘real’ history at all. ‘By its very nature’ it is ‘liable to lose contact with reality’, and is indeed ‘removed from real life’.³⁶

The apprentice is thus left with some very definite instructions about what to study and how to study it. He must concentrate on ‘hard’ history, and thus on the type of evidence originally singled out in chapter 2 of *The Practice of History*: the evidence provided by such things as the record of a court case or a material relic such as a house. He should then make it his business to extract the facts, and thus the truth, from such forms of evidence. He must remember, as chapter 2 puts it, that ‘historical method is no more than a recognised and tested way of extracting from what the past has left the true facts and events of that past’.³⁷ Nor need the apprentice have any doubt ‘that the truth can be extracted from the evidence by the application of proper principles of criticism’.³⁸ Provided that he follows his instructions properly, the goal can unquestionably be achieved. As with all successful cults, the cult of the fact promises to guide us towards a final truth, ‘a truth which’, as Elton somewhat gnomically intones, ‘is more absolute than mere truthfulness’.³⁹

By this stage I imagine the apprentice beginning to feel slightly bewildered. Elton has offered him the example of a house as an instance

³¹ Elton 1969a, p. 172. The point is still more emphatically made in Elton 1970, esp. pp. 7, 65, 157, 177.

³² Elton 1969a, p. 197. Cf. also Elton 1970, where he insists (p. 73) on the ‘primacy’ of political history and singles it out (p. 68) as ‘the most important’ subject of historical research.

³³ Elton 1969a, p. 190.

³⁴ Elton 1969a, p. 190. For a repetition and enlargement of the argument, see Elton 1970, pp. 43–53.

³⁵ Elton 1991, p. 12. ³⁶ Elton 1991, pp. 27, 60. ³⁷ Elton 1969a, p. 86.

³⁸ Elton 1969a, p. 97. ³⁹ Elton 1969a, pp. 73–4.

of the type of evidence from which he is expected to extract the facts in such a way as to arrive at the truth. But how can one hope to set about seeking the truth, *simpliciter*, about such a thing as a house? Will it not be necessary to approach the study of the house with some sense of why I am studying it, why it might be of interest, before I can tell how best to go about examining it?

Elton has of course foreseen the anxiety and offers an interesting response. The opening chapter of *The Practice of History* introduces a distinction between 'real' historians and amateurs.⁴⁰ Amateurs such as Lord Acton or G. M. Trevelyan (who was 'a really fine amateur') intrude themselves and their enthusiasms upon the past.⁴¹ By contrast, real historians wait for the evidence to suggest questions by itself. As Elton later puts it, the questions a real historian asks are never 'forced by him upon the material'; rather they are forced by the material upon the historian. The real historian remains 'the servant of his evidence', of which he 'should ask no specific questions until he has absorbed what it says'.⁴² The distinction recurs in chapter 3, in which we are again informed that the questions we ask as historians must 'arise out of the work' and 'not be sovereignly imposed on it'.⁴³

This kind of injunction has been central to the German tradition of hermeneutics, and is prominent in the writings of Hans-Georg Gadamer, especially his *Wahrheit und Methode* of 1960.⁴⁴ It is true that Gadamer's name makes no appearance in *The Practice of History*, and that when Elton later invokes him in *Return to Essentials* it is only to dismiss him as ponderous and confused.⁴⁵ It seems to me, however, that Elton is not only echoing one of Gadamer's most characteristic themes, but that the argument they are both putting forward embodies a salutary reminder about the need to be aware of our inevitable tendency towards pre-judgement and the fitting of evidence into pre-existing patterns of interpretation and explanation.⁴⁶ Moreover, the warning seems all the more valuable in view of the fact that the premature consignment of unfamiliar evidence to familiar categories is so hard to avoid, as even apprentice historians know.

There remain some difficulties about applying this rule in practice. Gadamer would certainly not approve, in the first place, of the positivistic confidence with which Elton asserts it. Consider again Elton's example of a house as an instance of the kind of raw evidence that an apprentice

⁴⁰ Elton 1969a, pp. 29–36.

⁴¹ Elton 1969a, p. 31.

⁴² Elton 1969a, p. 83.

⁴³ Elton 1969a, p. 121.

⁴⁴ See Gadamer 1960 and cf. Gadamer 1975.

⁴⁵ Elton 1991, pp. 29, 38.

⁴⁶ Gadamer 1975, esp. pp. 235–74.

might confront. Gadamer would point out that Elton has already begged the question by characterising the object under investigation as a house. It will be unwise for Elton to retort that the object under investigation must be a house because it is described as such in all relevant documents. The House of Commons is described as a house in all relevant documents, but it is not a house. Nor will Elton fare any better if he replies that the object must be a house because it looks like a house. On the one hand, an object might look nothing like a house and nevertheless be a house. (Think of lighthouses now used as houses.) On the other hand, an object might look very like a house and nevertheless not be a house. (Think of the mausoleums designed by Sir John Vanbrugh.) As Gadamer always stresses, we are already caught up in the process of interpretation as soon as we begin to describe any aspect of our evidence in our own words.⁴⁷

A second and more intractable problem arises as soon as we ask how far we can hope to carry Elton's idea of confronting a piece of evidence such as a house and allowing it, as he repeatedly demands, to force its questions upon us. Elton is adamant that 'the only proper ambition' for an historian is 'to know all the evidence', with the result that the task of the apprentice historian must be to begin by acquiring 'total acquaintance with the relevant material' if he is to end up by telling the truth about it.⁴⁸ The underlying aspiration to arrive at a definitive reading of a body of evidence dies surprisingly hard. Elton's commitment has more recently been echoed, for example, by Peter Gay, who has written of his regret at his decision to entitle his major work on the eighteenth century *The Enlightenment: An Interpretation*. Gay remarks that while "'the Interpretation'" would have sounded immodest' this would nevertheless 'have been what I meant'.⁴⁹

But what would it mean to offer *the* interpretation of the Enlightenment? It would mean, at the very least, offering an analysis sufficiently comprehensive to enable us either to incorporate or to set aside every rival reading of every piece of evidence that might be thought relevant to the provision of a total picture of the high culture of the eighteenth century. Not merely is such a project of doubtful intelligibility, but the mere attempt to undertake it would consume endless lifetimes. Any analysis

⁴⁷ On language as the medium in which all interpretative activity is carried on, see Gadamer 1975, esp. pp. 345–66.

⁴⁸ See Elton 1969a, pp. 87, 96 and cf. pp. 88, 92, 109.

⁴⁹ Gay 1974, p. 211 n. But Gay generally pleads for a perspective more akin to the one I am defending here; see, for example, Gay 1974, pp. 210–13, 217. For a discussion of Elton's and Gay's arguments see Novick 1988, pp. 610–12.

of the phenomenon of the Enlightenment will inescapably be based on a series of prior judgements about the nature of its most characteristic preoccupations, together with a further series of judgements about how best to illustrate them. But to engage in such judgements is already to recognise that we are, of course, offering only *an* interpretation. Our resulting survey may be a model of fairminded inclusiveness, but it cannot possibly include everything, and will therefore be open to continuous re-interpretation both by scholars who discover new facts and by scholars who offer new interpretations of the significance of existing ones.

The same objections apply even in the case of Elton's seemingly more modest demands upon the apprentice historian. As we have seen, Elton's basic suggestion is that, when confronting an item of evidence such as a house, the apprentice should begin by acquiring 'total acquaintance' with it if he is to end up by telling the truth.⁵⁰ Again, however, the question is how we can hope to render intelligible the idea of seeking total acquaintance with an item of evidence such as a house. Consider, for example, the project of acquiring total acquaintance with Chatsworth House, and thereby arriving at the truth about that principal residence of the Dukes of Devonshire. A complete study of all the facts about Chatsworth would be literally endless. It would take a lifetime for the apprentice to accumulate anything like a full description (whatever that may mean) of the house itself. (How many windows does it have? How many panes of glass? How big is each pane? How much do they weigh? Where did they come from? How much did they cost?) So far the apprentice has not even entered the muniment room to stare glassily at the scores of manuscript volumes devoted to the lives of Chatsworth's owners and the process of building it. (How many volumes? How many pages in each volume? How many words on each page? What sort of ink was used?)

As Elton's discussion proceeds, however, he evidently begins to see the difficulty, or at least begins to shift his ground. In chapter 3 of *The Practice of History* he is still assuring us that historians 'can discover something fairly described as the truth' about the objects of their research.⁵¹ But in chapter 4 he frequently replaces this contention with the very different and vastly more modest claim that historians can hope to arrive at some particular truths. Whereas chapter 2 had spoken of recovering 'the truth' about 'past realities', chapter 4 prefers to speak of the historian's capacity

⁵⁰ See Elton 1969a, pp. 87, 96 and cf. pp. 88, 92, 109.

⁵¹ See Elton 1969a, p. 117; but cf. pp. 179, 221, where he continues to insist on his earlier claims about 'the truth'.

to find out ‘solid truths’ and thereby to ‘establish new footholds in the territory of truth’.⁵²

It subsequently turns out that this more modest account of the historian’s task is what really matters to Elton. The aim of the ‘real’ historian is that of arriving at new truths by way of adding to the number of incontrovertible facts. It is because of his sense that, as he puts it in chapter 3 of *The Practice of History*, there are many things that historians ‘know beyond doubt’ and ‘can say with certainty’⁵³ that Elton later savages the deconstructionists and their scepticism about facts with such assurance. Elton knows beyond question ‘who the eldest surviving child of Henry VIII was’; this is one of an ‘enormous number’ of historical facts ‘on which no dispute is possible’.⁵⁴ It follows that, when he finds himself obliged to confront such deconstructionist critics as Dominick LaCapra with their claim that ‘there cannot be any ascertainable certainties in history’, Elton is in no doubt about how to respond.⁵⁵ Although he does not know how to spell Professor LaCapra’s name, he knows for a fact that LaCapra is merely exhibiting ‘the mindless arrogance of the self-satisfied’ if he is attempting ‘to deny the existence of facts’.⁵⁶

It is true that Elton betrays himself into some blank contradictions in the course of mounting this argument. The earlier chapters of *The Practice of History* are emphatic that ‘a great deal of history’ is ‘knowable and known beyond the doubt of anyone qualified to judge’, and thus that ‘some historical writing is simply and obviously right’.⁵⁷ But in the final chapter, and again in *Return to Essentials*, Elton is no less emphatic that the historian ‘must be a professional sceptic’,⁵⁸ and that one of the main functions of ‘real’ historians must be ‘to cast doubt upon the possibility that in historical studies anyone will ever be finally “right”’.⁵⁹

Elton’s restatement of his ideal is far from coherent, but his ideal itself is surely clear and unexceptionable. If we now return to Chatsworth with no higher ambition than to say a number of true things about it, we can surely hope to succeed. We may be able to determine such factual matters as its overall height, the size of its grounds and perhaps even the number of its rooms with absolute finality, so long as we take care to avoid any problems of an interpretative kind (such as, for example, what is to count as a room). If this is all that is meant by the quest for

⁵² Elton 1969a, pp. 168, 177. ⁵³ Elton 1969a, p. 111. ⁵⁴ Elton 1969a, p. 80.

⁵⁵ For the discussion of LaCapra’s views see Elton 1991, pp. 58–61.

⁵⁶ Elton 1991, p. 59. ⁵⁷ Elton 1969a, pp. 107, 123. ⁵⁸ Elton 1991, pp. 23–4.

⁵⁹ Elton 1969a, p. 206. On the need for ‘sceptical thinking’ and ‘critical scepticism’ on the part of historians see also pp. 55, 103, 205.

the truth – that is, the capacity to uncover and state a number of facts – then it can certainly be granted to Elton that, as he puts it in chapter 3 of *The Practice of History*, historians are often able to end up by offering statements ‘of manifest and incontrovertible truth’.⁶⁰

Unlike his initial demand, Elton’s more modest proposal at least has the merit of suggesting a research programme that could in principle be carried out. It is not clear, however, that this will necessarily alleviate the anxiety originally expressed by our imagined apprentice. He now knows that his job is to find out a number of facts about Chatsworth with the aim of stating a corresponding number of truths about it. But he also knows that the facts about Chatsworth are so numerous that he will never be able to find out more than a very small fraction of them. (If he stupidly decides, for example, to start by finding out how many stones went into its construction, he will certainly never finish his dissertation on time.) Moreover, since every fact he discovers will have to be expressed in words, and since Michel Foucault has by now familiarised even apprentice historians with the thought that all classificatory schemes are subject to endless challenge and revision, he may even begin to wonder how many genuinely incontrovertible facts he can hope to state. Suppose, for example, he decides to catalogue the works of art contained in Chatsworth. He wants to know whether he should include the furniture. The correct answer, obviously, is that he should include only those items of furniture which are also works of art. But what is required for something to be a work of art? On the one hand, the question clearly has no simple answer, perhaps no answer at all. But on the other hand, the apprentice needs an immediate answer if he is going to be able to state as a matter of incontrovertible fact how many works of art Chatsworth contains. Perhaps there are fewer incontrovertible facts than he has been led to believe.

The apprentice need not despair, however, for Elton is on hand to reassure him that (as he remarks in speaking of my own writings on this subject) these are unduly high-falutin doubts.⁶¹ But even if the apprentice feels duly reassured, he is still in need of some advice about how to start work on his thesis about Chatsworth. What sort of incontrovertible facts should he be looking for? What sort of facts should he be trying to find out?

One obvious way of replying would be to revert to the somewhat Socratic approach I initially proposed. What first attracted you, one

⁶⁰ Elton 1969a, p. 176.

⁶¹ Elton 1991, p. 42.

might ask in return, to the idea of making a study of Chatsworth? What made you think that a dissertation on this particular mansion of the late seventeenth century might be of interest? I think this would certainly be my own response. I would expect the apprentice to have some views about why it might be of some value – here and now, to himself and others – to know more about Chatsworth and its history. Just as the value of factual information depends on what the historian wants to understand, I would argue, so the attempt to uncover new facts needs to be guided by a sense of what appears to be worth understanding.⁶² I would urge the apprentice, in other words, to solve the problem of how to study Chatsworth by first asking what might be the purpose of studying it at all.

If our imagined apprentice is expecting some such answer from Elton, however, he is in for a rude shock. It is Elton's view that asking such questions is the quickest way of revealing that you have failed to understand the nature of the historian's craft. He insists in *The Practice of History* that our historical studies must be kept entirely separate from any such concerns,⁶³ and in *Return to Essentials* he reiterates the point with even greater vehemence. 'The fundamental questions we put to the evidence' must remain 'independent of the concerns of the questioner'.⁶⁴ We must recognise that Chatsworth – or any other relic of the past – must be studied 'in its own right, for its own sake', and that this constitutes 'the first principle of historical understanding'. What distinguishes 'real' practitioners of history is their willingness to grant the past 'full respect in its own right'.⁶⁵

It might be supposed that what Elton means is that, once we have selected a topic for investigation, we must be sure to treat it in its own terms, even though the topic will of course have been selected on the grounds that it seems to us to possess some inherent value and interest. This would be to say – to cite an epigram of John Dunn's – that the historian should be Whig as to subject matter, Tory as to truth.⁶⁶ But to assume that this is Elton's position would be seriously to underestimate the sweep of his argument in *The Practice of History* about the need to approach the past 'in its own right, for its own sake, and on its own terms'.⁶⁷ It is Elton's view that we must take the greatest care *not* to select our topics on the grounds that they seem to us to have some current interest or (worse still) some contemporary social relevance or importance. The point is made with ferocious emphasis, and with Elton's

⁶² For a classic account of a similar view of factual evidence see Carr 1961, pp. 1–24.

⁶³ Elton 1969a, p. 65. ⁶⁴ Elton 1991, p. 55.

⁶⁵ Elton 1969a, pp. 18, 66, 86.

⁶⁶ Dunn 1980, p. 26. ⁶⁷ Elton 1969a, p. 86.

habitual repetitiousness, in every chapter of his book. The historian must avoid any attempt 'to justify his activity as a social utility'. To proceed in this way is to commit 'the cardinal error'. He must recognise that his entire pursuit 'involves, above all, the deliberate abandonment of the present'.⁶⁸ The same point is made yet again in *Return to Essentials*. We are now assured that the entire project of historical research ('all of it') must be completely divorced from the 'needs and concerns of the present'.⁶⁹

By this stage I imagine the apprentice becoming seriously worried, perhaps even a touch desperate. Does this mean that all the facts I might discover about Chatsworth are of equal interest? Am I just to go there and start making a list of anything it occurs to me to say about it? If this is all I am expected to do, might I just as well be studying something else, perhaps anything else?

If the apprentice is insolently attempting a *reductio ad absurdum* he is in for another rude shock, for it turns out that this is exactly what Elton believes. When he addresses the question of teaching in the closing chapter of *The Practice of History*, he goes so far as to declare that the actual content of what we teach, and *a fortiori* what we study as historians, 'matters in essence very little' and is indeed 'of no importance'.⁷⁰ Real historians, as he had earlier put it, are not distinguished by the problems they study but by 'the manner of their study'; their problems may appear 'narrow or petty', but they gain their importance from 'the techniques of study' they impart.⁷¹ This is a truth that needs to be grasped not merely by teachers of history but by 'anyone concerning himself with historical studies in any form'. The purpose of our studies must be sought 'in the intellectual training they provide', and it is because 'all history, properly deployed' can equally well supply this training that 'it matters in essence very little what particular sections of it are taught'.⁷²

I imagine the apprentice stunned at this point into incredulity. So it doesn't matter in the least what facts I find out about Chatsworth, so long as I employ the right techniques to find them out? This is precisely Elton's point. 'The University', as he patiently explains, 'must train the mind, not fill the untrained mind with multi-coloured information and undigested ideas, and only the proper study of an identifiable discipline according to the rules and practices of that discipline can accomplish that fundamental purpose'.⁷³ But what of our ability to learn from the past about unfamiliar social structures, about developments in art, religion and philosophy, about the conditions and mechanisms of political and

⁶⁸ Elton 1969a, pp. vii, 66, 86.

⁶⁹ Elton 1991, p. 72.

⁷⁰ Elton 1969a, pp. 187, 188.

⁷¹ Elton 1969a, pp. 34, 69.

⁷² Elton 1969a, pp. 186, 188.

⁷³ Elton 1969a, p. 199.

economic change? Some of these examples are Elton's, but they leave him unmoved. 'This is nothing to do with the framing of courses for study and examination, with the real work of intellectual training.'⁷⁴ But what about his earlier insistence that it matters very much what kind of history we learn and teach, since 'the actions of governments and governed' alone provide us with a backbone of 'real' or 'hard' history? Here I do not know what to say, for as far as I can see Elton makes no effort to reconcile this argument with his yet more strongly voiced belief in the overriding importance of technique.⁷⁵

III

It is surely worth pausing at this sensational moment to reflect on the completeness of the disjunction that Elton eventually draws between the content and the justification of our historical studies. What could have prompted so great a scholar to paint himself into such a dark and dismal corner? The clue lies, I believe, in considering the nature of the intellectual crisis so painfully reflected in the pages of *The Practice of History*. By the time Elton came to publish the original version of this manual in 1967, he had issued some of his best-known technical scholarship as well as two of his most widely used textbooks. As *The Practice of History* makes clear, he not only thought highly of this *oeuvre*⁷⁶ but had managed to persuade himself that the kind of research in which he himself specialised called for the exercise of exceptional human powers. He speaks of the need for a searching intelligence, for sympathy and judgement, for 'imagination controlled by learning and scholarship'.⁷⁷ He even speaks in an uncharacteristic moment of pomposity of the historian's 'obligations as an artist' as well.⁷⁸

Elton was acutely aware, however, that a number of prominent historians had meanwhile ceased to believe in the validity or importance of the sort of administrative and political history in which he had made his name. Among those particularly singled out in *The Practice of History* for

⁷⁴ Elton 1969a, p. 200.

⁷⁵ One possible reconciliation might take the form of saying that the required technical skills can best be gained from studying certain types of document, and that the most suitable types on which to practise are those concerned with English central government. So far as I am aware, Elton never explicitly suggested this reconciliation, although he arguably hinted at it in Elton 1969b, p. 33.

⁷⁶ See, for example, Elton 1969a, pp. 174–6.

⁷⁷ See Elton 1969a, pp. 112, 177 and cf. Elton 1970, p. 108 on the exceptional skills needed to write political history.

⁷⁸ See Elton 1969a, pp. 158–9 and cf. p. 124.

arguing that such preoccupations have ‘ceased to be valid’ are Richard Southern and Keith Thomas.⁷⁹ As Elton concedes, both acknowledge that political history retained its importance so long as the teaching of history in British universities remained closely tied to the training of a political elite and a civil service capable of running a great empire. With the loss of these social conditions, however, Southern and Thomas were led to conclude that the justification for singling out this kind of history had come to an end as well. Both accordingly enter what Elton describes as unacceptable pleas for a new sense of why history might matter to our society, together with a call for the cultivation of new forms of historical enquiry – a call for more intellectual history in the case of Southern, more social history in the case of Thomas.⁸⁰

A surprising feature of *The Practice of History* is that Elton makes no attempt to respond to these arguments by seeking to vindicate the social value or cultural significance of his own very different kind of research. He could surely have attempted – as several of his admiring obituarists did – to convey some sense of why the study of administrative and constitutional history might still be thought to matter even in a post-imperial culture dominated by the social sciences. It is true that, a couple of years later, he made some gestures in this direction in his first inaugural lecture. But it is striking that he almost instantly stopped short, apologising for starting to speak in such a ‘very vague and rather vapoury’ way.⁸¹ Faced with the question of how a knowledge of history might help the world, he preferred to advise historians to ‘abandon and resign’ such aspirations altogether.⁸²

Why was Elton so doubtful about assigning any social value or utility to his own brand of history? I am not altogether sure, although the answer must certainly be connected with his curious but persistent belief that any attempt to vindicate the usefulness of studying the past must include a demonstration of the historian’s capacity to issue predictions.⁸³ This is particularly a theme of Elton’s first inaugural lecture. ‘We are told’, he confides, that what historians must do if they are to be socially useful is to answer the question ‘What help can the past offer to the future?’⁸⁴

⁷⁹ Elton 1969a, pp. 17–18, 185.

⁸⁰ For a discussion of these claims see Elton 1969a, esp. pp. 17–18, 185–6.

⁸¹ Elton 1991, p. 93. ⁸² Elton 1991, p. 96.

⁸³ The same anxiety afflicted J. H. Hexter at much the same time, but he instead responded by attempting to vindicate the historian’s predictive powers. See Hexter 1971, esp. pp. 36–42. But Hexter appears to miss his own point, for the predictions he discusses – although presented as those of an historian – are not issued in virtue of his being an historian at all.

⁸⁴ Elton 1991, p. 84.

But who tells us this? It is hard to think of any contemporary historian or philosopher of history who has advanced this argument, and Elton himself mentions no names. He can scarcely have in mind his two *bêtes noires*, Southern and Thomas, both of whom are exclusively concerned with the question of how the past might be made relevant to the present. Nor can he be thinking of the Marxist historian he most frequently attacks, Christopher Hill, for while it was undoubtedly an aspiration of classical Marxism to make use of historical materials to formulate predictive social laws, Christopher Hill has never exhibited anything more than a passing interest in that aspect of Marxist philosophy.

There remains something of a mystery surrounding the sources of Elton's scepticism about the broader educational value of his own studies. About the fact of his scepticism, however, he leaves us in no doubt. His second inaugural lecture robustly declares that 'we should not trouble ourselves too much' about the alleged lessons of history, since this would be to study the past for an 'inappropriate and usually misleading purpose'.⁸⁵ Eight years later, in the version of his Cook Lectures published in *Return to Essentials*, his mood had become even more dismissive. He begins by stigmatising the nineteenth-century belief in the lessons of history as little more than an influential absurdity, and goes on to warn us against the 'temptation' of believing that the study of history is of any relevance to our future or present state.⁸⁶

Elton clearly recognised, however, that these commitments left him with only two possible ways of convincing us – as he always remained anxious to do – that the study of history should nevertheless be recognised as a vocation 'appropriate to the highest abilities of the human reason'.⁸⁷ One alternative would be to abandon any attempt to vindicate the social value of his own kind of history in favour of claiming that the value of the subject somehow lies in the study of the past as a whole. This is the line he begins to follow in *Return to Essentials*, and especially in the three Cook Lectures included in that book. The first lecture opens by informing us that 'history teaches a great deal about the existence of free will'. The second adds that a professional assessment of the past can be used to demolish a number of comfortable myths. The third concludes that history can tell us about the unexpected and, again, about the reality of human freedom.⁸⁸

These are not perhaps very promising lines of thought, and it is surely to Elton's credit that he never made any effort to explain or develop them.

⁸⁵ Elton 1991, p. 114.

⁸⁶ Elton 1991, pp. 4, 9.

⁸⁷ Elton 1969a, p. 16n.

⁸⁸ Elton 1991, pp. 7–8, 45–6, 73.

He was undoubtedly aware that the past has always been studied for a myriad of changing reasons, and that any attempt to summarise them will almost inevitably degenerate into just such a string of clichés. But this leaves him with only one means of vindicating the importance of his own studies. As we have already seen, he is forced into arguing that any attempt to offer a social justification of history is an irrelevance, the reason being that what matters in history is not the content of our studies but the range of techniques we deploy in practising them. This is the conclusion which, in effect, supplies him with the theme of both inaugural lectures reprinted in *Return to Essentials*. The second proclaims that the value of historical study lies entirely in the ‘mind-training capacity’ it provides. Even more bluntly, the first concludes that what historians ‘are here to teach the world’ is nothing other than ‘the proper assessment and proper study of evidence’.⁸⁹

We can now see what makes Elton’s image of the historian as a master carpenter such a revealing one. What matters, he believes, is not whether we are engaged in making tables, chairs or wooden spoons; what matters is the nature of the craft skills equally required for engaging in any of these activities. Like Mr Gradgrind, Elton believes that ‘facts alone are wanted’. It follows, in Elton’s philosophy, that the most important task must be to learn how best to find them out.

By now I should expect the apprentice to have given up trying to write his dissertation on Chatsworth, perhaps devoting himself instead to a career in retailing (as Elton appears to recommend at one point).⁹⁰ I fear that some such feeling of discouragement would certainly have been my own response, although Elton’s outstanding success as a teacher suggests that there must be some way in which I am failing to respond with adequate appreciation to his advice to neophytes. Be that as it may, I should like to end by summoning my imagined apprentice once more to ask Elton if he doesn’t fear that something of broader educational significance may have been forfeited by his unrelenting insistence on technique at the expense of content. It turns out, however, that Elton has no regrets, since he is not sure about the value of a broader liberal education in any case. This darkest vein of scepticism surfaces – without preamble or explanation – in his first inaugural lecture, in the course of which Sir Richard Morison, one of Henry VIII’s propagandists, is approvingly cited for the view that education is a great cause of sedition and other mischiefs in commonwealths. Elton follows up the quotation with a disconcerting flurry of questions. ‘Should we’, he suddenly asks,

⁸⁹ Elton 1991, pp. 89, 108.

⁹⁰ Elton 1991, p. 94.

‘really be practising education? Are we not overestimating it as a power for good, or possibly underestimating it as a power for evil? Ought we not sometimes to stand away from the whole question of education?’ Even more disconcerting is his response. Education ‘is a livelihood’, he concedes, ‘but it may be a folly’, and it is undoubtedly a cause of mischief in commonwealths.⁹¹

Elton’s fundamental reason for wishing to emphasise technique over content appears to have been a deeply ironic one: a fear that historical study might have the power to transform us, to help us think more effectively about our society and its possible need for reform and reformation. Although it strikes me as strange in the case of someone who spent his life as a professional educator, Elton clearly felt that this was a consummation devoutly to be stopped. Much safer to keep on insisting that facts alone are wanted.

⁹¹ Elton 1991, p. 85.

Interpretation, rationality and truth

I

Many historians make it a principal part of their business to investigate and explain the unfamiliar beliefs we encounter in past societies. But what is the relationship between our provision of such explanations and our assessment of the truth of such beliefs? The question is obviously a highly intractable one, but no practising historian can hope to evade it, as many philosophers have recently and rightly pointed out. Within the Anglophone tradition, the most eminent philosopher to highlight these issues of late has been Charles Taylor, and it is on his formulation of the question that I shall begin by focusing as I try to work my way towards my own answer to it.

II

The key issue for historians, as Taylor states it, is whether they should seek to avoid ‘taking a stand on the truth of the ideas’ they investigate.¹ Is it desirable, or even possible, to ‘bracket’ the question of truth, ‘to insulate questions of historical explanation from those of truth’?² My first response is that I am not altogether clear what Taylor means by the ‘bracketing’ of truth. Sometimes he seems to be asking whether historians should somehow seek to discount or set aside the fact that they themselves hold certain beliefs to be true and others false. If this is Taylor’s question, then my answer is that I am sure no historian can ever hope to perform such an act of forgetting, and that in any case it would be most unwise to try.

This chapter has been adapted and developed from the central section of my ‘Reply to my Critics’ in *Meaning and Context: Quentin Skinner and his Critics*, ed. James Tully (Cambridge, 1988), pp. 235–59.

¹ See Taylor 1988, p. 224 and cf. Shapiro 1982, esp. p. 537.

² Taylor 1988, pp. 220, 223.

Consider the fact that so great a political philosopher as Jean Bodin believed there to be witches in league with the devil.³ Or the fact that so great a student of nature as Aristotle believed that bodies change quality whenever they change place.⁴ Living in the twenty-first century, we are likely to feel – and unlikely to be able to repress the feeling – that these claims are simply false. But we are also likely to find our interest quickened by the discovery that such eminent authorities, capable of saying so many things that seem straightforwardly true, were also capable of entertaining such apparent absurdities. If we begin by focusing on such beliefs, we shall provide ourselves with a good starting-point for investigating the structure of Aristotle's or Bodin's thought. For here at least we come upon something that cries out to be explained. We shall also provide ourselves with a good means of ensuring that our eventual explanation takes a sympathetic and non-anachronistic form. For whatever account we provide will have to include an explanation of the fact that such admittedly bizarre beliefs nevertheless commended themselves to such unquestionably distinguished minds.⁵

At other points in his discussion Taylor seems to be asking a different question: whether the views that historians take about the truth-value of the beliefs they expound ought to affect the types of explanation they give of them.⁶ My answer here is that this depends on what is meant by speaking about the truth-value of beliefs, a topic on which Taylor writes in a somewhat ambiguous way.

Sometimes the issue he raises is whether our explanations ought to vary – or are sure to vary – with our sense of whether the beliefs we investigate are 'true or valid in relation to the needs of the people who live under them'.⁷ This question – seemingly inspired by the hermeneutics of Gadamer – appears to me to embody an unhelpfully wide, even a metaphorical, extension of the concept of a true belief. But if this is the issue on which historians are asked to pronounce, then my own answer would be that of course our explanations are bound to vary with whatever judgements we make about truth in this extended sense. If we encounter an ideology that we judge to be true to the needs of the society living under it, we are sure to treat that fact as part of our explanation for its success. If we come upon an ideology that seems demonstrably untrue

³ Bodin 1595, p. 49. For a denunciation of Bodin for holding these beliefs see Anglo 1973. For a defence see Monter 1969 and for a full reconstruction of Bodin's demonology and its associated vision of politics see Clark 1997, pp. 668–82.

⁴ For this formulation of Aristotle's belief see Kuhn 1977, p. xii.

⁵ Kuhn 1977, pp. xi–xii. ⁶ Taylor 1988, p. 213. ⁷ See Taylor 1988, p. 223 and cf. p. 226.

in this extended sense, we shall certainly be obliged to explain its success in a very different way. (But unless we find that the society in question is on the point of dissolution, we are surely more likely to conclude that we cannot hope to explain such a phenomenon at all.)

At most points in his discussion, however, Taylor speaks about true beliefs in a more familiar and restricted way. When he asks whether historians should take account of the fact that a particular belief is true when seeking to explain it, what he generally seems to be asking is whether we should take account of the fact that the belief in question accords with our own best current beliefs about the matter at issue. I am not of course (nor is Taylor) offering this as a definition of truth. I am only observing that this is how we generally use the term.⁸ (Though the moral of this, as Donald Davidson has suggested, is perhaps that we ought not to ask for a definition.)⁹ I take it, accordingly, that the question with which Taylor is principally occupied is this: whether we as historians can or ought to avoid asking ourselves whether we endorse the beliefs we are seeking to explain.

Taylor himself maintains that it is undesirable and probably impossible to bracket truth in this way,¹⁰ a conclusion that aligns him with a large number of Anglophone philosophers writing about the topic of social explanation.¹¹ Taylor himself remains deliberately tentative about this commitment,¹² but if we turn to some of these other philosophers we find two main reasons usually given for espousing it. One line of argument, stressed in particular by Graham Macdonald and Philip Pettit, derives from Donald Davidson's theory of radical interpretation.¹³ The suggestion is that, unless we begin by assuming that the holding of true beliefs constitutes the norm among the peoples we study, we shall find ourselves unable to identify what they believe. If too many of their beliefs prove to be false, our capacity to give an account of the subject matter of those beliefs will begin to be undermined. Once this starts to happen, we shall find ourselves unable even to describe what we hope to explain. The implication, as Davidson himself puts it, is that 'if we want to understand others, we must count them right in most matters'.¹⁴

⁸ For the suggestion that the moral of this is that we should adopt a pragmatic concern with solidarity at the expense of our traditional quest for objectivity see Rorty 1985.

⁹ Davidson 1986. ¹⁰ Taylor 1988, p. 220.

¹¹ See, for example, MacIntyre 1962, p. 62 (a passage cited with approval in Hollis 1972, p. 101); Jarvie 1970, esp. pp. 245–7; Lukes 1973, p. 247; Newton-Smith 1981, pp. 252–7; Macdonald and Pettit 1981, pp. 33–4; Graham 1981, pp. 173, 177; Shapiro 1982, pp. 556, 577; Hollis 1988.

¹² Taylor 1988, pp. 218, 220.

¹³ Macdonald and Pettit 1981, pp. 186–7. For their application of Davidson's theory see esp. pp. 18–29.

¹⁴ Davidson 1984, p. 197.

I cannot see that this view of radical interpretation possesses the relevance for historians that some of Davidson's more enthusiastic followers, such as Macdonald and Pettit, have supposed. Davidson is merely proposing a general strategy for using assertions to get at underlying beliefs, the strategy of beginning by assuming general agreement. It may well be that we need to start with some such assumption if we are to find another culture intelligible. If I am to identify the nature of Bodin's beliefs about witches, or even to establish that they are beliefs about that particular subject-matter, it certainly seems plausible to assume that Bodin and I must share a considerable number of ancillary beliefs. It is arguable, however, that Davidson has overemphasised the significance of this consideration and too comfortably ridiculed the notion of radically different conceptual schemes.¹⁵ Certainly it does not follow that I need to assume that Bodin's beliefs specifically about witches are mainly true before I can be sure of identifying them as beliefs about witches. It may be that practically everything Bodin says about that particular topic strikes me as obviously false. But by learning his language (an easily recognisable form of French) and by seeing what concepts he uses and how he reasons with them, I can nevertheless hope to identify without much difficulty where he is talking about witches and what he thinks about them. It is true that, if I am to keep up with his arguments, it may be necessary for him to reassure me at various points that he is still talking about witches. As long as he continues to make it clear that this is so, however, there seems no reason to fear that I may suddenly feel obliged to conclude that he must be talking about something else, even if practically everything he is saying strikes me as patently absurd.¹⁶

I turn to the second reason often given for supposing that the issue of truth must never be bracketed. False beliefs, it is said, point to failures of reasoning, and failures of reasoning require additional explanations of a kind not needed in the case of true beliefs. This appears, for example, to be the thought underlying Keith Graham's contention that we shall be acting 'in a spirit of ill-judged humility' as historians if we fail to consider the points at which the social beliefs we investigate are 'flawed or inadequate'.¹⁷ A similar thought underlies Steven Lukes's discussion of the special explanatory problems he takes to arise in connection with the need to 'identify the mechanisms that prevent men from seeing the falsity' of their beliefs.¹⁸ The same commitment likewise emerges from

¹⁵ A claim powerfully argued in Forster 1998.

¹⁶ See McGinn 1977 and cf. Hacking 1982, esp. p. 60.

¹⁷ Graham 1981, p. 177. ¹⁸ Lukes 1973, p. 242.

Macdonald and Pettit's more extended analysis of the way in which judgements about truth and falsity are bound to enter into 'the kind of explanation one gives' of alien beliefs.¹⁹ When a belief under investigation proves to be true, they assert, no further explanation is required. But when a belief is 'manifestly false' or 'obviously incorrect' there is something further to be explained. We need in particular to consider the kinds of 'social function or psychological pressure' that could have prevented the agent in question from recognising 'the mistaken nature of the belief'.²⁰

If this is the argument on which historians are asked to take a stand, then my own response is a simple and emphatic one. It is I think nothing less than fatal to good historical practice to introduce the question of truth into social explanation in this way. To do so is to assume that, whenever an historian encounters a belief which he or she judges to be false, the explanatory problem must always be that of accounting for a lapse of rationality.²¹ But this is to equate the holding of rational beliefs with the holding of beliefs that the historian judges to be true. And this is to exclude the obvious possibility that, even in the case of beliefs that nowadays strike us as manifestly false, there may have been good grounds in earlier historical periods for holding them to be true.

Having gestured at the concept of rationality, I ought to stress that I intend nothing very grand or precise by that much abused term.²² When I speak of agents as having rational beliefs, I mean only that their beliefs (what they hold to be true) should be suitable beliefs for them to hold true in the circumstances in which they find themselves. A rational belief will thus be one that an agent has attained by some accredited process of reasoning. Such a process will in turn be one that, according to prevailing norms of epistemic rationality, may be said to give the agent good grounds for supposing (as opposed to merely desiring or hoping) that the belief in question is true.²³ A rational agent will thus be someone who, as David Lewis excellently summarises, believes what he or she ought to believe.²⁴

¹⁹ Macdonald and Pettit 1981, p. 34. ²⁰ Macdonald and Pettit 1981, pp. 9, 34, 42.

²¹ For explicit statements to this effect see Lukes 1977, pp. 121, 132, 135.

²² My attempt to construe the concept in an informal way is indebted to Putnam 1981, pp. 150–200.

²³ To speak of rationality simply in terms of having good reasons for our beliefs is to run the danger of eliding the distinction between epistemic and practical rationality. For examples of this elision see Laudan 1977, p. 123 and Stout 1981, pp. 165–6. It is true that the distinction is one that pragmatists bid us elide. See for example Rorty 1979, pp. 328–9. As I emphasise below, however, I do not see how historians can hope to operate satisfactorily without it. For a helpful analysis of the distinction itself see Mortimore and Maund 1976.

²⁴ Lewis 1974, p. 336.

None of this implies that rational agents need to hold any specific beliefs, save for those which may be indispensable to bare survival.²⁵ So this means in effect that a rational agent will be someone whose beliefs are held in the light of a certain attitude towards the process of belief formation itself. This attitude must certainly include an interest in consistency. Rational agents want their reasons for holding their beliefs to bear upon their truth. But to espouse a given belief as well as its contradictory is to hold at least one belief that must be false. A rational agent will thus be concerned, at least in seriously troubling cases, to identify and eliminate any such obvious inconsistencies. Above all, rational agents must be interested in the justification of their beliefs.²⁶ They must be concerned with the kinds of coherence, and where appropriate the kinds of evidence, that give them grounds for concluding that their affirmations of belief can in fact be justified. They will thus be concerned to view their own beliefs critically, to consider whether they really can be justified by considering the degree to which they may be said to fit with each other and with perceptual experience.

It seems difficult to go further. In particular, it seems positively erroneous to try to arrive at a single criterion, and hence a method, for discriminating rational beliefs. The relations between the ideal of rationality and the practices embodying it seem too complex and open-ended to be captured in the form of an algorithm.

It is true that recent epistemology has been much concerned to discover such procedures or sets of rules. Among positivist philosophers, this at first gave rise to the proposed test of verifiability. But this seems much too strict. Apart from other difficulties, it provides the historian with a potentially anachronistic – and in any case a far from perspicuous – notion of direct observational evidence as the basis for justifying beliefs. This in turn appears to overlook the fact that it may be rational to hold a given belief, even in the absence of any such evidence, as long as it can be plausibly inferred from other rationally held beliefs.²⁷ The enemies of positivism later proposed an alternative criterion, that of falsifiability. But this seems even less satisfactory. As I have suggested, it appears a minimal characterisation of rational agents to say that the reasons they give for their beliefs should be reasons for holding them to be true. But

²⁵ Putnam 1981, pp. 38–40 calls these ‘directive’ beliefs. But in spite of what some commentators have implied (for example, Macdonald and Pettit 1981, pp. 26–8), this class seems to me of vanishingly small significance from the point of view of the historian.

²⁶ See Putnam 1981, esp. pp. 54–6, 155–68 and cf. McCullagh 1984.

²⁷ Putnam 1981, pp. 105–13; Mortimore and Maund 1976, pp. 14–20.

on the one hand, the fact that a given hypothesis may have resisted attempts to falsify it scarcely gives us any grounds for supposing it to be true.²⁸ And on the other hand, the application of such a test has the effect of excluding as irrational a number of otherwise well-confirmed and well-justified beliefs.²⁹

These remarks seem to me as much as it is appropriate to try to say about rationality in general terms. I now turn to explain why it seems to me fatal to satisfactory social explanations to exclude the possibility of holding a false belief in a wholly rational way. My reason is an obvious and familiar one. It is simply that the kinds of explanations we offer for beliefs we judge to be rationally held are of a different order from the kinds of explanations we feel obliged to offer if we come to doubt whether a given belief is held in a rational way. To equate the holding of false beliefs with lapses of rationality is thus to foreclose, in advance of knowing whether this is appropriate, on one type of explanation at the expense of another.

This is not to claim, as some philosophers have done, that rational belief is its own explanation.³⁰ This thesis has been vigorously espoused by Martin Hollis and others, but one obvious problem with this approach is that it overlooks the gap between demonstrating the rationality of a belief and explaining why it was held. Even if we can show that it was rational for some particular historical actor to espouse a certain belief, the explanation of why he or she espoused it may always be independent of that fact.³¹ Hollis's formulation also conveys the impression that, once a given belief is exhibited as rational, there will be nothing further to explain. It is certainly true that we find the phenomenon of rational belief less puzzling than blatant lapses from rationality. But therein lies the danger. For it remains true that the attainment of rationality will always be an achievement. So an enquiry into the conditions that enable us to attain that state will never be any the less legitimate, and may in some cases be no less necessary, than an enquiry into the conditions that may prevent us from attaining it.

To say all this is not to claim – as Martin Hollis, Alasdair MacIntyre and others have done – that the forms of explanation appropriate to rational and irrational belief must differ because ‘rational belief cannot

²⁸ For this point see Stove 1982.

²⁹ This claim has often been made in relation to both Freud's and Darwin's theories. See Putnam 1981, esp. pp. 196–200. For a restatement see Lakatos 1978, esp. pp. 8–101.

³⁰ See, for example, Hollis 1974; Hollis 1988, pp. 140, 144.

³¹ I urge this objection against Hollis in Skinner 1978a, pp. 61–3. See also Elster 1982.

be explained in causal terms'.³² I see no reason to doubt that, if there is a sufficient reason for an agent to accept a given belief, that reason may function as a cause of its acceptance. I agree, that is, with the proponents of the so-called 'strong programme' that it seems appropriate to adopt what David Bloor has called a requirement of impartiality in the explanation of beliefs, a requirement that they should all be approached and explained in the same causal terms.³³ But I see no reason to add, as the exponents of the strong programme have done, that this requirement is incompatible with making judgements about rationality.³⁴ To insist on the relevance of such judgements is not to deny that we ought to be looking for causal explanations of the capacity to achieve rationality no less than of failures to achieve it.

When I insist on the need to ask whether a given belief is or is not rational as a preliminary to explaining it, my reason is rather that the different cases raise explanatory puzzles of different kinds. Even if we assume that our explanations will in each case be causal in form, the causes of someone's following what are taken to be the relevant norms of reasoning will nevertheless be of a different order from the causes of their violating them. It follows that, unless we begin by enquiring into the rationality of the belief concerned, we cannot be sure of correctly identifying what it is that needs explaining, nor in consequence of directing our investigations along appropriate lines. If the belief proves to be one that it was rational for the agent to have held, we shall need to investigate the conditions of that achievement. If it was less than rational or palpably absurd to have held it, we shall need to enquire into the very different sorts of conditions that may have inhibited or prevented the agent from following accepted canons of evidence and argument, or perhaps supplied the agent with a motive for defying them.³⁵

To reject this line of argument, as the advocates of the strong programme have done, it is necessary to insist not merely on a requirement of impartiality in the explanation of beliefs, but also on what David Bloor has called a requirement of symmetry.³⁶ This further principle, as Barry Barnes expounds it, requires that we reject any contention to the effect that one belief can be stigmatized as more 'ideological' than another

³² See MacIntyre 1971, pp. 255, 246–7 and cf. Hollis 1988, esp. pp. 140, 145. See also Hollis 1982, esp. pp. 80, 85 for the distinction between 'rational' and 'structural' explanations of belief.

³³ Bloor 1976, p. 5. See also Barnes 1974, p. 43; Barnes and Bloor 1982, p. 23.

³⁴ See, for example, Barnes and Bloor 1982, p. 25.

³⁵ Laudan 1977, pp. 188–9; Stout 1981, pp. 170–1; Newton-Smith 1981, pp. 253–7. But for a critique of my attempt to turn the concept of rationality into a tool for historians see Bartelson 1995.

³⁶ Bloor 1976, p. 5.

in consequence of being in some way 'unsatisfactory' or insufficiently grounded.³⁷ We have to recognise that all our beliefs are socially caused in such a way that, to some degree, their objects remain masked from us. It follows that all of them must be approached and explained in one and the same way.

If this is nothing more than a stipulation about how we ought to use the term 'ideological', then perhaps it will do no harm. But if it is a proposal about how historians ought to set about the business of explaining beliefs, then it seems to me fatal for just the reasons I have sought to give. It refuses to recognise that one of the reasons why someone may hold a certain belief is that there is good evidence in favour of it, that it fits well with their other beliefs, and so on – in a word, that it is rational for them to hold it. If we refuse to speak in these terms, we deprive ourselves of an indispensable means of identifying the most appropriate lines of enquiry to follow in any given case.

It may be helpful to offer an illustration of what I mean by speaking of the fatal consequences of failing to ask in this way about the rationality of beliefs. Consider the influential explanation of witchcraft beliefs offered by Emmanuel Le Roy Ladurie in his classic study, *The Peasants of Languedoc*.³⁸ Ladurie starts by stressing that such beliefs were of course manifestly false, a mere product of 'mass delirium'.³⁹ He proceeds to infer that they could never have been rationally held. As he explicitly asserts, those who espoused them were simply 'slipping savagely into the irrational in belief and behaviour'.⁴⁰ The effect of this commitment is to direct Ladurie's attention as an historian in one particular way. He assumes that what he must be looking for is an explanation for a breakdown in normal reasoning, a situation in which 'the peasant consciousness suddenly broke loose from its moorings'.⁴¹ The question, as he puts it, is how to account for such an upsurge of obscurantism, such an epidemic of pathological beliefs.⁴²

One element in Ladurie's answer is that, with the progress of the Reformation, the peasantry began to fear a loss of their traditional spiritual help. 'Far from their priests, the peasants found themselves alone with

³⁷ Barnes 1974, pp. 43, 128–30.

³⁸ For the suggestion that the study of witchcraft beliefs offers a good illustration of the role played by rationality postulates in historical study I remain much indebted to MacIntyre 1971, pp. 244–59. For a full account of the specific example I discuss see James 1984, pp. 166–71, an analysis to which I am also much indebted.

³⁹ Ladurie 1974, pp. 203–5. For a full discussion see James 1984, pp. 166–71.

⁴⁰ Ladurie 1974, p. 210. ⁴¹ Ladurie 1974, p. 208.

⁴² Ladurie 1974, pp. 203–4, 206–7. Cohn 1976, p. 258 makes similar claims about witchcraft beliefs as a 'collective fantasy'.

their anxieties and their primordial fears – and abandoned themselves to Satan.⁴³ But Ladurie's principal hypothesis is that they felt a deep sense of frustration at the collapse of the social upheavals associated with the Reformation itself. With the failure of social reform, their continuing desire to improve their lot took on a 'mythical dress', and was forced to express itself in the 'chimerical and fantastic revolt of the witches' Sabbath, an attempt at demonic forms of escape'.⁴⁴

I am not concerned with Ladurie's actual explanations, although it hardly seems an incidental consequence of his approach that they turn out to be so dizzyingly speculative.⁴⁵ I am solely concerned with the fact that, by treating it as self-evident that a certain set of beliefs could never be rationally held, Ladurie leaves himself no space to consider a quite different sort of explanation.⁴⁶ He cannot allow that the peasants may have believed in the existence of witches as a result of holding a number of other beliefs from which that particular conclusion might reasonably have been held to follow.

To consider only the simplest possibility, suppose that the peasants also held the belief – widely accepted as rational and indeed indubitable in sixteenth-century Europe – that the Bible constituted the directly inspired word of God. If this was indeed one of their beliefs, and if it was rational for them to hold it, then it would have been the height of irrationality for them to have disbelieved in the existence of witches. For the Bible not only affirms that witches exist, but adds that witchcraft is an abomination and that witches must not be suffered to live.⁴⁷ To announce one's disbelief in the existence of witches would thus have been to announce a doubt about the credibility of God's word. What could have been more dangerously irrational than that?

Ladurie excludes in advance the possibility that those who believed in witches may have done so as a result of following out some such recognisable chain of reasoning. But this not only means that he puts forward an explanation of witchcraft beliefs which, for all he knows, may be completely irrelevant. It also means that he bypasses a range of questions about the mental world of the peasants which it may be indispensable to answer if their beliefs and behaviour are to be satisfactorily understood.⁴⁸

⁴³ Ladurie 1974, p. 207. ⁴⁴ Ladurie 1974, p. 203.

⁴⁵ As Clark 1997, pp. 25–6 observes, they also require Ladurie to make even less plausible claims about what was going on in the minds of individual witches.

⁴⁶ The type of explanation explored in Clark 1997.

⁴⁷ See, respectively, Deuteronomy 13.10–12; Galatians 5.20; Exodus 22.18.

⁴⁸ For two classic studies in which the mental world of witchcraft beliefs is sympathetically recovered see Thomas 1971, pp. 435–583 and Clark 1997.

A common objection to the above line of argument has been that it presupposes an excessively objectivist conception of rationality. Disciples of the later Wittgenstein such as Peter Winch, as well as exponents of the strong programme such as Barry Barnes and David Bloor, have all converged on this point. As Barry Barnes puts it, echoing and endorsing Peter Winch's case, to claim that we can assess and criticise the rationality of beliefs is to presuppose 'external standards' of rationality of an 'objective' kind.⁴⁹ But we have no access to any such 'super-cultural norm', and in consequence no prospect of being able to 'discriminate existing belief-systems or their components into rational and irrational groups'.⁵⁰ The very idea of assessing the rationality of beliefs is thus dismissed as nothing better than an intrusion, a forcible imposition of our own epistemic standards on an alien 'universe of discourse' or 'form of life'.

This objection is I think totally misconceived. But my reason for saying so is not that I imagine – as Martin Hollis does – that we can hope to vindicate a substantial and objective conception of reason and employ it in the assessment of beliefs.⁵¹ It is rather that the abandonment of any such project does not preclude the idea of assessing beliefs for their rationality. If an historian stigmatises the upholding of a particular belief within a particular society as irrational, this judgement need never flow from the application of an allegedly objective conception of what can or cannot properly count as rationality. The historian need only be claiming that he or she has uncovered the prevailing norms for the acquisition and justification of beliefs in that particular society, and that the belief in question appears to have been upheld in the face of, rather than in the light of, those norms themselves. The historian need only be claiming that the agent in question fell short of – or perhaps abandoned, manipulated or in some other way deliberately defied – some generally accepted standard of epistemic rationality.

If historians were to adopt this approach, they would be engaging in the assessment of beliefs in just the manner I have recommended. But at no point would they be applying an 'external' standard of rationality in an 'intrusive' way. They would not be asking themselves whether the belief in question was rational according to their own standards (still less *the* standards) of epistemic rationality. They would merely be reporting that

⁴⁹ Barnes 1974, pp. 69–70, 130. Cf. Winch 1970. But Lear 1982 shows that Wittgenstein's argument cannot be assimilated to that of the sceptical relativist.

⁵⁰ See Barnes and Bloor 1982, p. 27 and cf. Barnes 1974, p. 41.

⁵¹ See Hollis 1988, pp. 141–2 and his earlier discussion of 'objectively rational' beliefs in Hollis 1982, esp. p. 72. See also Laudan 1977 and the discussion of his position in Newton-Smith 1981, esp. pp. 245–7, 270–3.

it was not an appropriate belief for that particular agent to have espoused in that particular society at that particular time.

It might seem that this conclusion is bound to deprive the concept of rationality of any explanatory power. This is certainly the inference drawn by Richard Rorty, who assumes that once we give up the idea of rationality as a concept ‘floating free of the educational and institutional patterns of the day’, we shall have to admit that we cannot hope to employ the notion in the assessment or explanation of beliefs.⁵² We shall find that practically everyone is capable of putting their desires and opinions together in such a way as to satisfy a pragmatist test of rationality. So the idea of asking whether it was in fact rational for them to hold their resulting beliefs becomes devoid of content and hence of explanatory force.⁵³

A number of intellectual historians have recently endorsed the same viewpoint. Once we discover the inner coherence of a given system of beliefs, they maintain, we can hardly fail to count it rational for the system to have been upheld.⁵⁴ So the project of assessing the rationality of individual beliefs again drops out of sight. ‘If ways of thinking are recreated sympathetically, then one never refutes but always sustains’ whatever beliefs are identified.⁵⁵

I concede that accusations of irrationality must only be hurled in the last ditch, if at all. We need to begin by recreating as sympathetically as possible a sense of what was held to connect with what, and what was held to count as a reason for what, among the peoples we are studying as historians. Otherwise we are sure to commit the characteristic sin of ‘whig’ intellectual history: that of imputing incoherence or irrationality where we have merely failed to identify some local canon of rational acceptability. I cannot see, however, why it should be supposed to follow that our interpretative charity must always be boundless. On the contrary, there may be many cases in which, if we are to identify what needs to be explained, it may be crucial to insist, of a given belief, that it was less than rational for a given agent to have upheld it.

As an illustration of what I have in mind, consider one of the beliefs fundamental to early-modern political philosophy, the belief that the quality of *virtù* is indispensable to military and political success. It was owing to the loss of this quality, Machiavelli particularly insists, that the Florentines of his own age became so disastrously incapable of defending themselves. In his early writings Machiavelli merely asserts this belief, but

⁵² Rorty 1979, p. 331. ⁵³ See Rorty 1979, p. 174 and cf. Rorty 1983, esp. pp. 585–6.

⁵⁴ See, for example, Clark 1980, esp. p. 100.

⁵⁵ See Greenleaf 1972a, p. 28 and cf. Greenleaf 1972b, p. 140.

in the course of his *Istorie Fiorentine* he goes on to support it with some impressive examples. Describing the battle of Anghiari, for instance, he notes that in four hours of combat only a single soldier was killed.⁵⁶ Describing the even more farcical battle of Molinella, he adds that in the course of half a day's fighting there were no fatalities at all.⁵⁷ By focusing on these and similar cases, he builds up his evidence for saying that his fellow-countrymen were abjectly lacking in the kind of *virtù* they needed in order to preserve their liberty.

If we turn to Machiavelli's sources, however, we find that they hardly support these conclusions at all. What they suggest is that a total of seventy soldiers were killed and 600 wounded at Anghiari, while at Molinella there was fierce fighting and several hundred fatalities.⁵⁸ If we turn, moreover, to later sixteenth-century discussions of the *Istorie Fiorentine*, we find a number of Machiavelli's younger contemporaries complaining about his attitude towards his evidence. Scipio Ammirato, for example, insists that Machiavelli gives no adequate grounds for his conclusions; instead he changes names and alters evidence in such a way as to make his authorities say whatever he already wants us to believe.⁵⁹

It is true that a sufficiently charitable historian could easily rescue Machiavelli at this point. Machiavelli fervently believed that the quality of *virtù* had been lost in the modern world, and he was not without strong grounds for this belief. He also believed that a willingness to behave courageously was one of the most obvious characteristics of a *virtuoso* people. But this means that he could hardly fail to conclude that his fellow-countrymen were lacking in courage. Nor could he readily interpret their military conduct except in terms of their axiomatic lack of this *virtuoso* quality.

As his own contemporaries insisted, however, Machiavelli was only able to maintain this particular article of faith at an extravagantly high cost. He was obliged to falsify the relevant authorities, and in consequence fell rather grievously short of the standards recognised by his own peers for the assessment of evidence and the justification of beliefs. As a number of them rightly observed, the outcome was a commitment which it was not appropriate for Machiavelli to uphold, or at least not in the unequivocal form in which he always upheld it. To put the point in the jargon I have been using, it was not a rational belief.

⁵⁶ Machiavelli 1962, V. 33, p. 383. ⁵⁷ Machiavelli 1962, VII. 20, p. 484.

⁵⁸ For these details, and for a discussion of contemporary sources (especially Biondo, Capponi and Poggio), see Villari 1892, vol. 2, pp. 452, 458–9.

⁵⁹ Ammirato 1846–9, Bk. 23, ch. 5, p. 169. For a discussion see Anglo 1969, pp. 185, 258.

I have already emphasised why it matters to be able to make such judgements. As soon as we permit ourselves such an uncharitable conclusion, we confront a new set of questions about Machiavelli's beliefs, a set of questions we had no occasion to ask or even to notice as long as we felt able to assume their rationality. Why is he so excessively insistent on the military incompetence of his fellow-countrymen? Is he nursing some private grievance? Or is he merely nostalgic for the bygone days of citizen militias? Or is he unduly influenced by the classical assumption that such forces are alone capable of displaying courage? These questions in turn suggest to the historian some wider ones. Should we be looking for a strongly emotional component in others of Machiavelli's political beliefs? Should we think of him as habitually credulous in his response to the political writings of ancient Rome? Only by enquiring into the rationality of his beliefs can we hope to recognise the range of explanatory puzzles they actually pose.

III

The above argument in response to Charles Taylor and the other Anglophone philosophers I have cited can in turn be expressed in the form of a set of maxims for historians concerned with the description and explanation of beliefs. The golden rule is that, however bizarre the beliefs we are studying may seem to be, we must begin by trying to make the agents who accepted them appear as rational as possible.⁶⁰

This golden rule in effect embodies three precepts. The first merely states a condition *sine qua non* of the whole enterprise. We need to assume what David Lewis has called a convention of truthfulness among the peoples whose beliefs we are seeking to explain.⁶¹ Our first task is obviously to identify what they believe. But our only evidence of their beliefs will normally be contained in whatever texts and other utterances they may happen to have left behind. It is of course likely that some of these may be pervasively marked by hidden codes such as irony. But we have no option but to assume that, in general, they can be treated as relatively straightforward expressions of belief. Unless we can assume some such convention of truthfulness, we cannot hope to make any headway with the project of explaining what they believed.

The second and closely connected precept states that we must initially be prepared to take whatever is said, however bizarre it may seem, as far

⁶⁰ Hollis 1970a, p. 219. See also the discussion in Skorupski 1978, pp. 88–9.

⁶¹ Lewis 1969, pp. 148–52.

as possible at face value. If the people we are studying assert that there are witches in league with the devil, we must begin by assuming that this is exactly what they believe. This will not only serve to keep before us the precise character of our explanatory task; it will also enable us to steer clear of a familiar but condescending form of interpretative charity. It will prevent us from purportedly rescuing the rationality of the people we are studying by way of suggesting that, whenever they say something that strikes us as grossly absurd, it will be best to assume that the speech act they were performing must have been something other than that of stating or affirming a belief.

There have been two widespread applications of this principle. One of them, essentially Durkheimian in inspiration, suggests that we ought rather to assume that such statements express in symbolic form a proposition about the structure of the speaker's society and his or her commitment to upholding it. This version was until recently popular with a certain school of social anthropologists, as the writings of Beattie, Leach and others attest.⁶² The second application, more Freudian in inspiration, suggests that we ought instead to assume that such statements express in a displaced or distorted form some deep and unacknowledged feelings such as frustration or anxiety. It is this version of the principle we have already encountered in Laturie's explanation of witchcraft beliefs.

The obvious difficulty with the principle in either form is that the only criterion we are offered for distinguishing those propositions we are to take literally from those we are to take symbolically is our own cognitive discomfort. If we find it too embarrassing to take what is said literally, we are in effect instructed to take it instead as a symbolic or displaced way of saying something else. To reject this approach is not of course to deny that beliefs may perform a crucial role in expressing a society's view of itself, its unacknowledged fears, its aspirations, its sense of solidarity. Nor is it to deny that the Freudian approach in particular may be able to furnish us with indispensable insights, unavailable to the agents themselves, into why they may have held (and held on to) their particular beliefs. It is only to assert that we shall be assuming what has to be established if we take it that we can move directly to such forms of causal explanation in advance of asking whether the agents in question may not in addition have had good reasons, by their lights, for holding what they believed to be true.

⁶² But for excellent criticisms see Hollis 1970b, esp. p. 226; Skorupski 1978, pp. 85–6; Macdonald and Pettit 1981, p. 15 and note. See also the general discussion, to which I am much indebted, in Papineau 1978, pp. 132–58.

The third precept states the positive task to which we commit ourselves as historians by virtue of this approach. We must seek to surround the particular statement of belief in which we are interested with an intellectual context that serves to lend adequate support to it. As we have seen, this commits us to something more than trying to establish that the people we are studying may have had good practical reasons for saying what they said. It commits us to trying to establish that their utterances were not merely the outcome of a rational policy, but were also consistent with their sense of epistemic rationality. The primary task must therefore be that of trying to recover a very precise context of presuppositions and other beliefs, a context that serves to exhibit the utterance in which we are interested as one that it was rational for that particular agent, in those particular circumstances, to have held to be true. As one recent commentator has proposed, the task might thus be characterised as ‘archaeo-historicist’ in character.⁶³

We cannot know in advance what range of beliefs this may require us to excavate. So my proposal stands in contrast with one that has often been put forward in particular by historians of science. They have sometimes argued that, as Mary Hesse has put it, we ought to concentrate on studying ‘the received internal tradition’ of scientific discovery, and hence on deepening our knowledge of the established canon of major scientists, rather than trying to ‘spell out in tedious detail every minor writing or trivial biography of forgotten figures’.⁶⁴

In criticising this approach I am not questioning the appropriateness of concentrating on the received tradition of discovery if that is what historians of science happen to find most interesting. Rather I take it that, as I have already emphasised in chapter 2, all worthwhile forms of history are bound to be whiggish in this sense. The problems on which historians feel it worth expending their energies will be certain to reflect their own sense of intellectual priorities. It would be strange indeed if they were to conduct their researches according to a set of priorities they themselves felt to be mistaken. I am only insisting that, once we recognise that an understanding even of a received canon of major figures requires us to surround them with whatever intellectual context makes best sense of them, we cannot afford to be too quick about dismissing any feature of that context as tedious or irrelevant. To an historian of science, the details of the Anglican Church hierarchy in Sir Isaac Newton’s time may very probably appear in that light. But it may well be that for Newton

⁶³ See Hume 1999, pp. 61–71 for an analysis of ‘reconstructing contexts’ in this way.

⁶⁴ See Hesse 1970a, p. 149 and cf. Hesse 1973.

the isomorphism between such hierarchies and those he found in the heavens gave him good reason, by his lights, for believing in the truth of his celestial mechanics. To dismiss the first as a 'religious' belief, with no relevance for Newton's scientific studies, may well be to impose such a deeply anachronistic view of how to divide up the world, and of what can count as a reason for what, as to close off the possibility of understanding Newton's most obviously 'scientific' achievements. Any impatience with what we think of as irrelevance or triviality may cheat us of just the historical understanding we seek.⁶⁵

Philosophers sympathetic to this approach, such as Richard Rorty, are apt to insist that we can hope to state it a good deal more briskly than I have managed. What it amounts to, they assure us in Wittgensteinian style, is simply that we need to get into the swing of whatever exotic language-games are being played by the people whose beliefs we are trying to describe and explain.⁶⁶ This seems true but unhelpful. We surely need to ask about the most suitable strategy for breaking in upon such unfamiliar activities and forms of life. How in practice ought we to proceed?

As a first step, it will perhaps be best to recall that statements of belief rarely present themselves individually to the historian with evidence conveniently attached. As I have observed, the question of what it is rational to believe depends in part on what else we believe. Any particular belief in which an historian is interested will therefore be likely to present itself holistically as part of a network of beliefs, a network within which the various individual items supply each other with mutual support. As I have already implied, it follows that if an historian wishes, say, to discover whether it was rational for Jean Bodin to have believed in demonic possession, the soundest course of action will be to begin by asking whether Bodin held any other beliefs in the light of which this admittedly bizarre commitment might in some way have appeared to make good sense.⁶⁷

Some philosophers – I am again thinking particularly of Martin Hollis – have objected that it will only be rational to hold such a belief if it was in turn rational to hold the core beliefs from which this specific item is said to follow.⁶⁸ But this image of a rational bedrock strikes me as confused. What does it mean for a purportedly core belief to be rationally held? On the one hand, it can hardly mean that we are capable of

⁶⁵ See Jacob 1976 and cf. Jacob and Jacob 1980. ⁶⁶ Rorty 1979, p. 267.

⁶⁷ The classic statement of this kind of holism remains the concluding sections of Quine 1961, esp. pp. 37–46. But even Quine seems to me too inclined to employ the metaphor of core and periphery.

⁶⁸ Hollis 1982, pp. 75, 83–4.

giving good reasons for holding it. For in that case it would be a derivative rather than a core belief. But on the other hand, I cannot see – as I have already conceded – what else it can mean to describe a belief as being held in a rational way. I cannot see, in short, that Hollis's proposal can be deployed in such a way as to set limits to the kind of holism I am trying to expound. Even in the most primitive perceptual cases, even in the face of the clearest observational evidence, it will always be reckless to assert that there are any beliefs we are certain to form, any judgements we are bound to make, simply as a consequence of inspecting the allegedly brute facts. The beliefs we form, the judgements we make, will always be mediated by the concepts available to us for describing what we have observed.⁶⁹ But to employ a concept is always to appraise and classify our experience from a particular perspective and in a particular way. What we experience and report will accordingly be what is brought to our attention by the range of concepts we possess and the nature of the discriminations they enable us to make. We cannot hope to find any less winding a path from experience to belief, from observational evidence to any one determinate judgement.⁷⁰

To contentions such as these, Hollis has repeatedly retorted that, at least in the case of 'simple everyday beliefs', the historian or ethnographer 'needs to discover' that the people he or she is studying have 'common perceptions, common ways of referring to things perceived and a common notion of empirical truth'.⁷¹ If history and ethnography are to be possible, he maintains, there must be a firm bridgehead of shared experiences which are conceptualised in an invariant way. He infers that there must be some corresponding terms in any language for the expression of these bridgehead concepts, and he roundly advises the historian or ethnographer to set about finding and translating them.⁷²

Quite apart from the fact that Hollis's principle does not tell us where to look, it strikes me as a serious misconception to suppose that we can ever hope, even in 'simple perceptual situations', to isolate and describe 'what a rational man cannot fail to believe'.⁷³ Even the simplest action or event can be fitted into a variety of more or less complex classificatory schemes, and can in consequence be labelled in an indefinite variety

⁶⁹ For the claim that any disposition to think of a world of neutral materials awaiting conceptualisation amounts to a third dogma of empiricism see Rorty 1972.

⁷⁰ For an influential source of this line of argument see Hesse 1970b and Hesse 1974, esp. pp. 9–73. Hesse's arguments are invoked and developed in Barnes 1974, esp. p. 16; in Barnes and Bloor 1982, pp. 37–9; and in Papineau 1978, esp. pp. 134–8.

⁷¹ Hollis 1970b, pp. 228, 230–1. ⁷² See Hollis 1970a, p. 216 and Hollis 1970b, p. 229.

⁷³ Hollis 1982, p. 74.

of ways. Consider, for example, a report of the simplest possible kind of ‘perceptual situation’: a report, say, to the effect that it is raining.⁷⁴ When ancient Romans stated and shared this belief, they used the word *imber*, this being the only term available in classical Latin to denote a fall or shower of rain. This means that, if an ancient Roman and a modern Briton were to find themselves standing damply together, there might be many instances in which, faced with exactly the same evidence, they might arrive at conflicting statements of belief. If the Roman were to report that they were experiencing an *imber*, and if the Briton were to take this to mean a fall or shower of rain, the latter might actually dispute the judgement. The Briton might wish to insist, say, that they were enduring nothing worse than the faintest drizzle.

This is not of course to deny the obvious fact that in some sense the Roman and the Briton must be experiencing and talking about the same event. It is only to insist that, whenever we report our beliefs, we inevitably employ some particular classificatory scheme; and that, as Thomas Kuhn has especially emphasised, the fact that different schemes divide the world up in different ways means that none of them can ever be uncontentiously employed to report indisputable facts.⁷⁵ This is not to deny that there are facts to be reported. It is only to insist – *pace* Hollis’s insistence that there must be ‘a bridgehead of true assertions about a shared reality’⁷⁶ – that the concepts we employ to report the facts will always serve at the same time to help determine what are to count as facts. Is it or is it not raining? There will be instances in which the Roman says *yes* while the Briton says *not really*.

It follows that we cannot hope to make the distinction – which Ian Shapiro has urged me to make in his critique of my own work – between those concepts which mask and those which truly reveal ‘what is actually going on’ in the social world.⁷⁷ This is to presuppose that our social world contains unequivocal objects and states of affairs that any adequate system of signs can hope to pick out in such a way that no sensitive observer can fail to see what is actually going on. But it is precisely this presupposition which, it seems to me, needs to be questioned. Rather we need to recognise that any system of signs will serve to single out just those objects and states of affairs which it in turn enables us to denote,

⁷⁴ My example is adapted from the discussion in Papineau 1978, pp. 135–6.

⁷⁵ Kuhn 1962, esp. pp. 43–51, 110–34.

⁷⁶ See Hollis 1970a, p. 216, and cf. the even stronger stress on ‘the independence of facts’ in Hollis 1982, p. 83.

⁷⁷ Shapiro 1982, p. 556.

while other systems will always be capable of performing that task in different and potentially conflicting ways.

To advance these claims is to argue that our concepts are not forced upon us by the world, but represent what we bring to the world in order to understand it. To embrace this conclusion may appear to be embracing a thesis of Idealism. But this is not so. I do not mean to deny the existence of a mind-independent world that furnishes us with observational evidence as the basis of our empirical beliefs. I am only arguing that, as Hilary Putnam has put it, there can be no observational evidence which is not to some degree shaped by our concepts and thus by the vocabulary we use to express them.⁷⁸

As I have noted, however, Hollis's principal objection – and that of many other Anglophone philosophers⁷⁹ – to this line of argument has been to say that it renders the task of the historian or ethnographer impossible.⁸⁰ Hollis's main contention is that, if we cannot 'pair' the terms used by alien peoples with 'counterparts' in our own language, then we cannot embark on the task of translating their utterances.⁸¹ But if we cannot be sure how to translate what they say, we can never hope to identify what they believe.⁸² For Hollis, as for many other philosophers of social science, translatability is thus taken to be a condition of intelligibility, with the result that the main issue is held to be that of establishing how translation is possible.⁸³

Sometimes this thesis has been stated in a form that makes it seem straightforwardly false. John Gunnell, for example, contends that 'to learn a new language is only possible because one already knows a language'.⁸⁴ If this were true, no infant would ever be able to master its own mother tongue. But even in the form in which Hollis and others have defended it – as a thesis about the need to be able to pair basic terms of alien languages with equivalents in our own – the claim that intelligibility presupposes translatability is surely mistaken. Often there will be no prospect of translating terms in an alien language by means of anything approaching counterparts in our own. But this does not prevent us from learning the use of such alien terms, and in consequence finding out

⁷⁸ Putnam 1981, p. 54.

⁷⁹ See, for example, Turner 1983, esp. pp. 283–4; Keane 1988, p. 210. But for a valuable corrective see Jones 1977 and (with specific reference to my own work) Jones 1981.

⁸⁰ See Hollis 1970a, p. 216 and cf. Hollis 1970b, p. 222. ⁸¹ Hollis 1970a, p. 215.

⁸² See Hollis 1970a, p. 215 and cf. Hollis 1982, p. 74.

⁸³ On this assumption see also Hawthorn 1979, esp. p. 477; Dunn 1980, esp. p. 96; Macdonald and Pettit 1981, esp. p. 45.

⁸⁴ Gunnell 1979, p. 111.

what discriminations they are employed to make. If we can do this, we can eventually hope to understand the applications even of those terms which remain wholly resistant to translation. It is true that we can never hope to tell someone what those terms 'mean' by citing synonyms in our own language. The fact that translation is to this degree indeterminate seems inescapable. But the moral of this, as Quine long ago taught, is perhaps that we ought to give up the quest for 'meanings' in such an atomistic sense.⁸⁵

It is perhaps needless to add that I am not pleading for historians to re-enact or re-create the experience of being sixteenth-century demographers or peasants of Languedoc or any other such alien creatures.⁸⁶ I am only pleading for the historical task to be conceived as that of trying so far as possible to think as our ancestors thought and to see things their way. What this requires is that we should recover the concepts they possessed, the distinctions they drew and the chains of reasoning they followed in their attempts to make sense of their world. What I cannot see is why this should be thought to require us to map their distinctions and the terms they used for expressing them on to the very different distinctions and expressions we happen to use ourselves. Historical understanding is a product of learning to follow what Ian Hacking has called different styles of reasoning; it is not necessarily a matter of being able to translate those styles into more familiar ones.⁸⁷

Donald Davidson has notoriously retorted that the resources of existing natural languages seem perfectly adequate for dealing with even the most dramatic cases of purported incommensurability reported by writers like Benjamin Whorf and Thomas Kuhn.⁸⁸ But Davidson's argument seems questionable in itself, relying as it does on such a strict application of the verification principle in order to rule out the idea of alternative conceptual schemes.⁸⁹ Furthermore, Davidson's scepticism is insufficient to undermine the sense in which I am defending anything resembling a thesis of incommensurability. I am merely contending that it will always be a mistake for an historian to assume that the task of explicating an alien concept can be reduced to that of finding a counterpart in his or her own language for the term that expresses it.

⁸⁵ Quine 1960, pp. 206–9.

⁸⁶ For an excellent account of why this aspiration is beside the point see Geertz 1983, pp. 55–70. Cf. also Inglis 2000, pp. 107–32.

⁸⁷ See the valuable remarks in Hacking 1982, pp. 59–61 and in Geertz 1983, pp. 58, 68–70.

⁸⁸ For this attempted deflation see in particular Davidson 1984.

⁸⁹ For a development of this criticism see Blackburn 1984, esp. pp. 60–2 and for a powerful critique of the argument in Davidson 1984 see Forster 1998, esp. pp. 141–6.

This still strikes me, however, as a methodological precept of considerable importance. To illustrate why this is so, let me revert to the example I have already considered from early-modern political philosophy: that of the concept of *virtù* as employed by Machiavelli and his contemporaries. Seeking a translation for this term, Anglophone historians have generally begun by observing that, even in Machiavelli's writings, persons of courage and prudence are often described as *virtuosi*. This leads to the conclusion that Machiavelli 'sometimes uses *virtù* in a traditional Christian sense'.⁹⁰ But Machiavelli also describes a number of talented but wicked leaders as *virtuosi*. This leads to the suggestion that perhaps the term has in addition 'a different meaning', signifying skill or ability in political or military affairs.⁹¹ As further anomalous usages are uncovered, however, commentators generally come to the conclusion that the term appears to have no determinate meaning at all. Rather it bears 'a wide variety of meanings in the writings of Machiavelli', who uses it 'in a great variety of senses'.⁹²

As the example indicates, such Anglophone historians have taken the task of understanding the concept of *virtù* to be that of explicating its 'meanings' by discovering their counterparts in modern English. But the example also illustrates, I hope, what is wrong with this approach. One outcome is that a different and far more promising line of enquiry is automatically closed off. The historian cannot consider the possibility that Machiavelli may have been using the term with perfect consistency to express a concept so alien to our own moral thought that we cannot nowadays hope to capture it except in the form of an extended and rather approximate periphrasis. Perhaps, for example, he used the term if and only if he wished to refer to just those qualities, whether moral or otherwise, that he took to be most conducive to military and political success. (As far as I can see, this is generally the case.) A further and consequential outcome is that a genuinely whig fallacy is almost automatically perpetrated. Such Anglophone historians begin with the assumption that, if Machiavelli's use of the term *virtù* refers to a clear concept, there must be some equivalent term in modern English for expressing it. But they quickly find themselves disappointed in their quest. As a result, it is all too easy to arrive at the completely unwarranted conclusion that Machiavelli must have been confused, since he appears (as one expert has put it) to be 'innocent of any systematic use of the word'.⁹³

⁹⁰ Price 1973, pp. 316–17.

⁹¹ Price 1973, p. 319.

⁹² Price 1973, pp. 315, 344.

⁹³ Whitfield 1947, p. 105.

It would be easy to multiply examples. (Consider, for instance, the many ‘confusions’ that historians of philosophy have found in discussions about causation prior to Hume.) But I hope that the general point needs no further emphasis. A term such as *virtù* gains its ‘meaning’ from its place within an extensive network of beliefs, the filiations of which must be fully traced if the place of any one element within the structure is to be properly understood.⁹⁴ Doubtless we can only hope to embark on such a task if there is some considerable overlap between our beliefs and the beliefs of those whom we are trying to investigate. But this overlap may nevertheless be far too exiguous to allow for anything approaching term-by-term translations of the concepts involved. To suppose otherwise is not merely a philosophical error but one that leads to just the deleterious practical consequences I have tried to illustrate.

Having arrived at this position, it is possible to suggest an answer to a further and closely connected question that practising historians as well as philosophers of history have repeatedly raised.⁹⁵ As Charles Taylor puts it in the essay I began by discussing, the question is whether we are ever justified in revising the language of the peoples we are studying in such a way as to bring our descriptions into conflict with those they offered themselves.⁹⁶ Can we ascribe to past thinkers concepts they had no linguistic means to express?⁹⁷

There is one way in which it will obviously be legitimate to go beyond, even if not to contest, the stock of descriptions available to the peoples studied by ethnographers and historians. This will be if we wish not merely to identify what they believed but to comment on the place of those beliefs within some larger historical pattern or narrative. Arthur Danto in particular has emphasised the asymmetries that are bound to result.⁹⁸ When, for example, Edward Gibbon remarked that Boethius was the last Roman who would have been recognisable as such by Cicero, he offered a comment on Boethius’s beliefs to which Boethius himself could not possibly have assented. We may nevertheless wish to insist that what Gibbon says about Boethius’s beliefs is true. Certainly it would be absurd to reject the description as misleading simply because Boethius himself was in no position to recognise its truth.

⁹⁴ Goodman 1978, p. 93 summarises this as ‘meanings vanish in favour of certain relationships among terms’.

⁹⁵ See, for example, Pocock 1985, p. 13. ⁹⁶ Taylor 1988, p. 221.

⁹⁷ Here I invoke the title of Prudovsky 1997, a careful critique of my own response to this question.

⁹⁸ See Danto 1965, pp. 149–81 and cf. Dunn 1980, esp. pp. 19–20, 104–5.

There is a further point at which it will often be legitimate not merely to go beyond but to repudiate the descriptions offered by the peoples we are studying. This will be when we are confident that we have identified what they believe and wish to go on to explain why they believed it. It would be a quixotic form of self-denying ordinance to insist that our language of explanation must at this juncture match whatever language the people in question applied or could have applied to themselves. If we wish to furnish what we take to be the most powerful explanations available to us, we are bound to employ what we believe to be the best available explanatory theories and the concepts embodied in them. As a result, there will be many cases in which – to comment on a further issue raised by Taylor – we shall want to insist that, even if our resulting explanations conflict with those offered by the people we are studying, ours must be regarded as the ‘superior’ ones.⁹⁹ This is only to say that one of our own beliefs is that our stock of social explanations has become enriched over the course of recent centuries. If we believe, for example, that Freud’s concept of the unconscious represents one of the more important of these enrichments, we shall not only want to do our best to psychoanalyse the dead, but we shall find ourselves appraising and explaining their behaviour by means of concepts that they would have found, initially at least, completely incomprehensible.

Some intellectual historians have wished to defend a third type of revision. What matters, they claim, is not the terms in which people happen to express their beliefs, but the nature of the distinctions they draw by the use of those terms. This means that, as long as we preserve their distinctions, it may be positively helpful to revise their terms. For example, we may wish to say that, although John Locke never uses the word ‘image’ in outlining his theory of ideas, we gain a clearer sense of what he is talking about if we speak of ‘images’ where he speaks of ‘ideas’.¹⁰⁰

Although apparently unexceptionable, this further proposal strikes me as treading on more dangerous ground. The terms we substitute may well perform the illuminating task of capturing more of the implications of a theory than its own author may have recognised. But they will almost certainly serve at the same time to import a number of irrelevant and even anachronistic resonances. As soon as this begins to happen, the intellectual historian will be failing in what I take to be his or her primary task: that of identifying and describing the beliefs to be explained. So

⁹⁹ Taylor 1981, esp. pp. 208–9. Cf. also Taylor 1982, pp. 87–105 for a similar stress on the cognitive superiority of the theories generated by modern scientific conceptions of rational acceptability.

¹⁰⁰ For a sceptical view of this proposal see Yolton 1975, pp. 507–8.

it seems to me that where an historian is trying to identify beliefs – as opposed to the logically subsequent task of explaining or commenting on them – it will generally be fatal to revise the terms in which they are expressed. The beliefs in question will only be identifiable as possessing their precise subject-matter by virtue of the particular terms in which the agents themselves chose to express them. To revise those terms will be to talk about a different set of beliefs.

As an illustration, let me conclude by reverting once more to the example of Machiavelli, and specifically to the political argument outlined in his *Discorsi*. Historians in the Anglophone tradition have often discussed Machiavelli's theory in terms of its account of the relationship between the rights and interests of individual citizens and the powers of the state.¹⁰¹ But Machiavelli himself never employs the terminology of rights (*diritti*) or interests (*interessi*) at any point. The effect of revising his vocabulary in this way has been to supply him with a range of alleged beliefs about a number of topics on which he never pronounced. It is of course possible that he possessed the concept of a right even though he never talked about rights. But as I began by stressing, historians have no option but to begin by assuming that what people actually talk about provides us with the most reliable guide to their beliefs. To begin by insisting that they must really be talking about something else is to run the highest risk of supplying them with beliefs instead of identifying what they believed.

IV

The way we live now is such that anyone who defends the type of position I have outlined above is certain sooner or later to find themselves denounced (or commended) as a relativist. Sure enough, my critics have repeatedly hurled this piece of conceptual bric-à-brac at my head.¹⁰² It is of course true that I have relativised the idea of 'holding true' a given belief. I have asserted that it may well have been rational for Jean Bodin to hold it true that there are witches in league with the devil, even if such beliefs no longer strike us as rationally acceptable. But at no point have I endorsed the thesis of conceptual relativism. I have never asserted that it *was* true that at one time there were witches in league with the devil, even though such a belief would nowadays strike us as false. To put the point generally, I have merely observed that the question of what it may

¹⁰¹ See, for example, Cassirer 1946, pp. 133–41; Colish 1971, pp. 345–6.

¹⁰² Graham 1981, p. 173; Shapiro 1982, p. 537; King 1983, p. 297; Hollis 1988, p. 146.

be rational for us to hold true will vary with the totality of our beliefs. I have never put forward the reckless and completely different thesis that truth itself can vary in the same way.

I have certainly claimed that, when we say of a given belief that we hold it true, what we are saying is that we find it rationally acceptable. But this is not to claim, as the conceptual relativist does, that there is nothing more to truth than acceptability. Unlike the relativist, I am not trying to offer a definition of truth. I am not in general talking about truth; I am talking about what different peoples at different times may have had good reasons by their lights for holding true, regardless of whether we ourselves believe that what they held true was in fact the truth.

I have not even suggested that the reasons people give for their beliefs need be such that an historian who recovers them need find them so much as recognisable as reasons for holding true the beliefs concerned. Historians frequently study what Martin Hollis has called ritual beliefs, cases in which the contents of the beliefs under investigation may remain unintelligible.¹⁰³ The most we can hope to do in such circumstances is to place the beliefs in question within an appropriate explanatory context of other beliefs.¹⁰⁴ We can certainly hope as a result to indicate why someone operating from within that context might come to assent to the propositions we ourselves find unintelligible. But we cannot hope to do more. In such cases we discharge our task as interpreters if we can explain, say, how Aquinas was able to reach and defend the belief that God is at once three persons and an indivisible Being.¹⁰⁵ We need not suppose that we have to be able to perform in addition what may strike us as the impossible feat of explaining what exactly it was that Aquinas believed. To paraphrase Hollis, the aim of the historian is to produce as much understanding as possible, a task not to be confused with that of producing converts.¹⁰⁶

I am convinced, in short, that the importance of truth for the kind of historical enquiries I am considering has been much exaggerated. I take this to be a product of the fact that so much of the meta-historical discussion has hinged around the analysis of scientific beliefs. In such cases the question of truth may perhaps be of some interest. But in most of the cases investigated by historians of ideas, the suggestion that we need to consider the truth of the beliefs under examination is, I think, likely

¹⁰³ Hollis 1970b, pp. 221, 235–7. For a contrasting viewpoint see Papineau 1978, p. 150.

¹⁰⁴ See the valuable discussions in Skorupski 1976, pp. 225–43 and Skorupski 1978, pp. 98–102.

¹⁰⁵ For an account of how we can hope to do this much see Stout 1981, pp. 3, 8–9, 106–9, 173–4.

¹⁰⁶ Hollis 1970b, p. 237.

to strike the historian as strange. Take, for example, one of the cases I have already discussed: Machiavelli's fervently held belief that mercenary armies always jeopardise political liberty. There is of course nothing to prevent us from asking whether this is true, but the effect of doing so will be somewhat analogous to asking whether the king of France is bald. The best answer seems to be that nowadays the question does not arise.

To say this is not to adopt the position, sometimes ascribed to Wittgenstein, that we are *precluded* from asking about the truth of such beliefs on the ground that they can only be understood as part of a form of life that may be ultimately no less cognitively justifiable than our own.¹⁰⁷ On the contrary, that way of stating the thesis of conceptual relativism strikes me as self-refuting as it stands, embodying as it does the statement of a preferred point of view while denying that any such point of view can be attained.¹⁰⁸ I am merely insisting (to revert to my example) that our task as historians is to try to recover Machiavelli's point of view; and that, in order to discharge this task, what we need to employ is solely the concept of rational acceptability, not that of truth.

Some historians have admittedly sought to reintroduce the question of truth by arguing that their findings serve to underpin the thesis of conceptual relativism. Thomas Kuhn has been widely, if mistakenly, interpreted in this way, but the clearest statement of this claim has been put forward by proponents of the 'strong programme' such as Barry Barnes and David Bloor. As we have seen, they think that they have established from their historical case-studies that all our beliefs have social causes, and that all such causes operate in such a way as to distort our capacity to get in touch with the objects of our beliefs. From this they have inferred that the only possible judge of the truth of our beliefs must be whatever consensus over norms and standards may happen to prevail in what they call our local culture.¹⁰⁹

I cannot see that the generalisation extracted by Barnes and Bloor from their research bears on the thesis of conceptual relativism at all. Suppose it is true that the social causation of our beliefs is such as to mask their objects from us. The obvious inference is that we have no good grounds for holding those beliefs to be true, not that we have satisfactory grounds for holding them to be true according to some relativised notion of truth.¹¹⁰ By contrast with Barnes and Bloor, it seems to me that, if the practice of intellectual history serves to suggest any theoretical insights,

¹⁰⁷ On this point see Lear 1983, pp. 44–6.

¹⁰⁸ For this objection see Putnam 1981, pp. 119–20 and Lear 1983, p. 55.

¹⁰⁹ Barnes and Bloor 1982, pp. 22–9. ¹¹⁰ A point excellently made in Hollis 1982, pp. 82–3.

these must be of an anti-relativist kind. I infer this from the fact that the truth of conceptual relativism and the practice of intellectual history appear to be incompatible.

The point I have in mind here is an obvious and familiar one. If we are to use our ancestors' utterances as a guide to identifying their underlying beliefs, it is indispensable that we should hold in common with them a number of assumptions at least about the process of belief formation itself. The most basic of these assumptions – to which I have already alluded – is the one stressed above all by Quine.¹¹¹ We must be able to assume, in advance of our historical enquiries, that our ancestors shared at least some of our own beliefs about the importance of consistency and coherence. We must be able, for example, to assume their acceptance of the principle that, if we affirm the truth of a given proposition, we cannot at the same time affirm the truth of the denial of that proposition.¹¹² Beyond this, we need to share with our ancestors some assumptions about the process of using our existing beliefs to arrive at others. This is because, even if we can identify some of their individual beliefs, we may still find our efforts at understanding defeated unless we can make some fairly strong assumptions about the character of the reasoning they must have employed in fitting their beliefs together.¹¹³

It can easily be made to look like pure dogmatism to insist on such anti-relativist considerations in an *a priori* style. But the need to do so can, I think, be readily vindicated if we simply recall the nature of the intellectual historian's task. The aim is to use our ancestors' utterances as a guide to the identification of their beliefs. But if they display no concern for consistency, if they employ no recognisable modes of inference, we shall have no means of marking off which of their utterances are to be classed as instances of the speech acts of stating or affirming or defending their beliefs. If they are willing, for example, both to affirm and deny the truth of some particular proposition, then we can never hope to say what belief they hold about it. As a number of philosophers have insisted, following in Quine's wake, the idea of holding rational beliefs and the idea of holding beliefs that are mainly true by our lights certainly come together at this point.¹¹⁴

¹¹¹ Quine 1960, p. 59.

¹¹² Following Quine, many philosophers have stressed this point. See, for example, Hollis 1970b, pp. 231–2; Lukes 1977, pp. 133–5.

¹¹³ Lear 1982, pp. 389–90.

¹¹⁴ Hollis 1970b, pp. 231–2; MacIntyre 1971, pp. 250, 256; Lukes 1977, pp. 133–5; Papineau 1978, p. 138; Macdonald and Pettit 1981, pp. 30–1.

This is not to assert that the idea of a 'pre-logical mentality' – an idea ritually wheeled out by philosophers in this context – is necessarily an impossibility. It is only to assert that, if an historian were really to encounter a people for whom it caused no discomfort to affirm and deny the same propositions, there would be no prospect of reporting what they believed. Nor am I denying that historians may well encounter abnormal forms of discourse in which the law of non-contradiction is deliberately flouted. I am only saying that such forms of discourse must be abnormal, and parasitic on recognisable forms, if we are to understand the linguistic community in which they take place. Nor am I even denying that agents engaged in normal discourse may turn out to have a number of beliefs about their beliefs which, strictly speaking, reveal inconsistencies.¹¹⁵ I am only saying that an historian will be unable to grasp the content of any beliefs that turn out to be contradictory in and of themselves.

These conclusions can also be stated in the form of one further precept about historical method. If as historians we come upon contradictory beliefs, we should start by assuming that we must in some way have misunderstood or mistranslated some of the propositions by which they are expressed. As a simple instance of what I have in mind, let me end by considering a yet further example from Machiavelli's political works. In his *Discorsi* Machiavelli affirms that liberty is possible only under a *repubblica*.¹¹⁶ But he also affirms that Rome lived *in libertà* under her early kings.¹¹⁷ What then does he believe? Does he or does he not think that liberty and monarchy are incompatible?

Historians have tended to reply that he seems to be confused: he affirms but he also denies that liberty is possible only under a republic.¹¹⁸ I am suggesting, however, that before we endorse such a conclusion we ought first to consider whether we may not in some way have misunderstood what he said. Sure enough, if we investigate the full range of contexts in which the term *repubblica* occurs, we discover that for Machiavelli the term can be used to denote any form of government under which the laws may be said to foster the common good. It follows that for Machiavelli the question of whether a monarchy can be a *repubblica* is not an empty paradox, as it would be for us, but a deep question of statecraft. The question is whether kings can ever be relied upon to pass only such laws as will serve the common good. This gives us an alternative reading:

¹¹⁵ Elster 1978, p. 88. ¹¹⁶ Machiavelli 1960, II. 2, p. 280.

¹¹⁷ Machiavelli 1960, III. 5, pp. 388–90.

¹¹⁸ See, for example, Colish 1971, p. 330 on Machiavelli's alleged 'lack of univocity' on this point.

Machiavelli is telling us that, under Romulus and his successors, the laws of Rome served the common good, so that the government, although monarchical in form, was an instance of a *repubblica*. Since this has the effect of resolving the contradiction, I am suggesting that this is also the interpretation we ought to prefer.

But what if the initial contradiction had refused to yield to any such re-interpretative efforts? I have already given my answer: at that point we should have to admit that we cannot say what Machiavelli believed. Before throwing up our hands in this way, we need to make sure that we really are in the last ditch. But if we are, we are left with no alternative. Nor should we feel that we ought to have done better. To look for complete intelligibility is to adopt an unduly optimistic view of what we can hope to bring back from the foreign lands of the past.