

HISTORICAL JUDGEMENT

The Limits of Historiographical Choice

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Preface

It was Steven Gerrard, Publisher, who, at the Joint Session of the Aristotelian Society and the Mind Association at Queen's University Belfast in 2003, encouraged me to write this book, and I am most grateful to him and to a number of anonymous readers from North America and the UK, both philosophers and historians, for their support and observations. Fortunately, I did not have to start from scratch, because I first began to worry philosophically about the issues involved in the book as an undergraduate at Edinburgh University under Leon Pompa's supervision. That supervision developed into lifelong mentorship and friendship, and I take pleasure here in affirming how much I have valued, and continue to value, our relationship. Inevitably, given that my quality of philosophical thought rarely achieves his, there have been both agreements and disagreements over the years, and this book is a contribution to such debates rather than a resolution of them. Here I acknowledge also other continuing influences on my thinking about history: another tutor at Edinburgh was George Davie, who introduced me to French philosophy (interesting but irrelevant, I then thought), and whose *The Democratic Intellect* had not long before been published, and it was he who (alone) claimed to recognise in my writing a feeling for history itself rather than, as historians might see it, mere philosophy. A lesser but still important influence and source of encouragement was W.H. Walsh, not so much for his work in philosophy of history but for his lectures on F.H. Bradley, by whose holistic empiricism he thought I was over-impressed. This outcome I ascribe to the persuasiveness of his lecturing and his supporting scholarship.

I was indeed impressed by Bradley's philosophical approach. I moved to Peterhouse, Cambridge for my Ph.D. research in order to work under W.B. Gallie, whose writing on narrative historical understanding I much appreciated, and it took me a while to notice the relevance to philosophy of history of another of his interests, American pragmatism, with its connections to Bradley's holistic empiricism. Pragmatism and Bradley come together in the work not so much of Gallie's Peirce but of Quine, and a further important influence was Quentin Skinner's lectures on that philosophically difficult material. (Later, Quine himself helped, a tiny bit.) If Skinner's position as a

historian teaching Quine was not quite sufficient to remove any sense I had of the divisions between historians and philosophers, then the last step in that direction was provided by Ian Hacking, my adviser of philosophical studies at Peterhouse, who was thought of by we philosophy students as mainly a probability theorist, but who surprised us by lecturing (almost as if he had also learnt from George Davie) on Bachelard, Foucault and Althusser. My second Ph.D. supervisor was Mary Hesse, and she – another appreciator of Quine’s philosophy – and Gerd Buchdahl (less so), of the History and Philosophy of Science Unit, showed me just how complex a task the development of a proper philosophy of history would have to be, while Bernard Williams did not think it could be done. But all I had to do was apply the complexity of Cambridge’s fast-developing new philosophies of science to history; and then I crossed the road back to Peterhouse and talked to the historians.

The practical, if not theoretical, divisions between history and philosophy were rapidly reinstated. Herbert Butterfield had recently retired as Master, but was still present; I was philosophically baffled when conversing with him. Maurice Cowling cross-examined my thinking, usually before breakfast (and I tried, with limited success, to learn how to cross-examine him back; it was rather different from philosophical discussion, despite his philosophical expertise). Joe Lee – another lifelong friend – delighted me with his brilliance (but what made this a different brilliance from that of Bernard Williams?). E.A. Wrigley displayed the mathematics and science in history. Denis Brogan, Martin Golding, Roger Lovatt, Edward Norman, M.M. Postan, David Watkin and Brian Wormald are foundational in my memory as Peterhouse historians who, in their different ways, helpfully illustrated for me the philosophical tasks which I faced. In that wider Cambridge beyond the College, G.R. Elton (whom I criticise later) was very helpful, and I believe I recall correctly that it was he who introduced me to Arnaldo Momigliano in London, with whom I had long discussions. Perhaps most influential, however, was Peter Laslett through his History of Ideas Seminar, earlier members of which had included Skinner and John Pocock, for it was to that Seminar in Trinity College that Richard Vann, Editor of the then fairly new journal *History and Theory*, came, seeking original thinking in just the area to which I wished to contribute. *History and Theory* has given me many opportunities and much

encouragement over the years, its influence on my work is immeasurable, and I should like here to express my thanks to Richard Vann and to the current Editor, Brian Fay.

All these early influences will be apparent in this book, but it has had much more recent help. Much of chapter 2, “The philosophy of a discipline”, builds on an extended version of a paper which, at Michael Bentley’s invitation, I delivered in summary form in July 2005 at the meeting of the panel “Beyond Science in Historical Theory”, organised by the International Commission on the History and Theory of Historiography, at the 20th International Congress of Historical Sciences in Sydney, Australia. I am glad to acknowledge the support of The British Academy in attending this panel. At the meeting, Eduardo Tortarolo trustingly accepted that I could provide a publishable version of the paper, and an early development of it appeared as “The truth of historical theory” in the journal he edits, *Storia della storiografia* 48, 2006. I am very grateful to the many respondents to that paper, especially Maria Grever, for discussion, and particularly to Michael Bentley also, who, in addition to his own many stimulating contributions to historical theory and historiography, had earlier organised a workshop on behalf of the Commission in St Andrews in July 2004, a formative time for me in the production of this book, when I was the discussant in a valuable session of papers given by Jörn Rüsen, Ewa Domańska, Rolf Torstendahl and Mark Day. I would also like to thank George Pavlakos for many discussions in legal theory which I have used as a central source in historical theory, and, in addition to some of those mentioned earlier, to thank David Evans, Christopher McKnight and Alan Weir for comments on part of the book’s early drafts.

I dedicated my first book, *The Expression of Historical Knowledge*, to my wife Kyra, who by 1982, when it was published, had been more close than anyone could reasonably wish to someone else trying to write philosophy. She has with her usual and supererogatory grace put up with a very great deal more by now; and she helps me to write better than I would naturally do.

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Chapter 1 The Argument

This book contains an argument which is summarised in this first chapter. In very brief outline, the argument begins by showing, in the second chapter, that the philosophy of a discipline requires drawing on its historiography in a particular way, so that the philosophy of historiography should draw on the historiography of historiography in this way. That second chapter also argues that the philosophical issues concerning a discipline should arise from that discipline itself, as shown in its historiography. The following chapter, chapter three, seeks to write the required historiography of historiography, and that search, in disclosing its own presuppositions of writing historiography, discloses also the philosophical issues which arise for historians, which have to do with their factual and moral judgements made in a context of a multiplicity of choices. The form which these philosophical issues take for many contemporary historians is a worry about the postmodern destabilisation of historical reality, and chapter four analyses postmodernism, overcoming its problems for historians by showing its practical limits. Finally, chapter five shows how, in the light of those limits, the historical world may be established in our factual and moral understanding.

Next we¹ explain how the various elements of the book connect to each other. While the book is organised in terms of five chapters including this one, the titles of which give a broad overview of their contents, the ongoing argument of the book is presented in a series of sections, listed on the “Contents” page, each naturally following its predecessor throughout. In so far as the book consists of a developing argument as just briefly outlined, it has a traditionally analytical philosophical structure. However, it is not necessary to read it by starting at the beginning. Those with philosophical and those with historiographical backgrounds may well have different interests. For example, some readers may wish to skip the metaphysical arguments in the next chapter “The Philosophy of a Discipline”, take for granted the conclusions reached,

¹ Why not “I”? In part, for the ordinary literary reason that the construction of meaning is intended to be shared with the reader, but more importantly because “I” has a raw use for which I wish (we will

and proceed directly to the issues involved in the third chapter “Writing the History of Historiography”. Again, some readers may take the conclusions in that chapter for granted, and go directly to “Pragmatic Postmodernism”; similarly for “The Room for Judgement”. The remainder of this introductory chapter will help with such decisions.

In a little more detail than the very brief outline just given, this book argues for a historiography-friendly philosophical response to historians’ theoretical concerns, first showing how to model historiography in a philosophical way by analysing the metaphilosophical and historiographical moves made in the philosophy of other disciplines, in particular science and law. The philosophy of a discipline requires the historiographical recovery of that which the practitioners of the discipline conceive as characterising their discipline and under which they conceive themselves to be operating. Philosophical and historiographical points of view are compared, and associated problems are addressed. The unavoidability of writing our own historiography of historiography is then argued for, and as this proceeds the fundamental conditions of writing that historiography are demonstrated. The nature of historical questions and their link to historiographical choices and other relevant presuppositions are explained. Historians are themselves used as our primary sources, and this matter too is explained. Historians’ self-understanding is then recovered through critical construction on the basis of historians’ own views, both as directly expressed and as expressed in their historiographies of historiography. Contrasting with recent epistemological discussions of “consensus”, historians’ characteristic modes of questioning are recovered, identifying not just what they agree about but what they agree they are disagreeing about. Relevant philosophical thought about historiography is presented in passing. The historiography of historiography, written by us using the material now explained, presents historians as thinking different things about the nature of historiography. Yet they can only disagree with each other if in some broad sense they share the issues about which they are disagreeing. What is characteristic is that historians disagree about interrelated issues: the role and nature of truth and truth-telling, the acceptability and grounds of moral judgement, the synthesis of facts, and their role or function in society. The book concludes with a

find we will wish) to reserve it. The contrast between “I” and “we” matters for our later argument

pragmatic analysis of these matters.

And now, the detailed summary of the argument which is the purpose of this first chapter. Chapter 2, “The Philosophy of a Discipline”, presents our first section, “Respect for historiography”. This section analyses the word “history” in some detail, and our reasons for using the word “historiography” to refer to the discipline “history” are explained, reserving the word “history” for the subject matter of that discipline. This terminology is to some extent artificial, but it is introduced because historians commonly use the word “history” in ways which may, given the distinctions we will present, seem ambiguous. However, while, for exactness and consistency, our terminology is used throughout the book, later we will see justification for historians’ normal usage. Following the analysis of “history”, we then outline a dismissive attitude on the part of a number of historians towards the philosophy or theory of their subject, even when that is written by historians themselves. Some historians see philosophers as imposing their theoretical positions on historians while in ignorance of their successful methodological practices. They think philosophers should focus on what historical practice actually is, and should provide some sort of portrait of historians. They think that philosophers should have more respect than they do for historians and historical work. They are right: some philosophers should indeed have more respect for historiography than they have shown, and this section explains why all philosophers should. A central feature to be understood is the historian’s historically situated yet privileged position of *hindsight*. A further reason for making historiography central for philosophical interest is its continuity with everyday commonsense understanding. Ultimately, the acceptability or otherwise of our judgements of truth can only be an acceptability *to us* in our everyday world, itself a world continuous with the past.

Nevertheless, to some extent the dislike on the part of some historians for historical theory displays a misunderstanding of philosophers’ interests in historiography. Many of those philosophers with an interest in the subject approach it with a desire to locate historiography as a knowledge-acquiring or knowledge-expressing discipline, and their

and is particularly dealt with in the section “Quine not postmodern enough”, in Chapter 4.

attitudes reflect their background theories of knowledge. Some, for example, may take historical knowledge for granted, and ask with Immanuel Kant (1724-1804) how that knowledge is possible. Others may, with René Descartes (1596-1650), doubt all knowledge claims, and insist as Descartes did on standards of proof which no usual discipline could successfully meet. Positions like Descartes' unquestionably suggest philosophical arrogance, but this perception is difficult to avoid in the case of philosophers with an interest in the theory of knowledge. This is because knowledge, even in our everyday understanding of that term, can only be such if some justification is available. Theorists of knowledge are in the business of questioning and analysing justifications, usually not taking them on trust, but here – reflecting the *pragmatic* approach which characterises this book – we recognise that there is important room for the idea that claims to knowledge might be self-justifying. In this section we describe the wide range of interpretations of “justification” in this context, and make explicit a feature of the pragmatic approach which is crucial to our argument, the presence of *choice*.

Yet our own concern in undertaking the philosophy of historiography is more general than this. It is to provide an understanding of historiography for those who are puzzled by it, by what it does, by what it achieves, by its role. Those puzzled may be within and without the discipline. Their curiosity may or may not have been induced by the views of philosophers, for many historians have claimed things of their discipline with which other historians would disagree. Thus part of our task is to sort this puzzlement into clear questions. Importantly, the philosophy of historiography should not be seen as essentially an exercise in the theory of knowledge. The philosophical problems of historiography should arise from the study of that discipline, not be imposed upon it.

It is right for philosophy to take historiography seriously. We are not alone among philosophers of historiography in thinking that, but that is a very general characterisation, and we argue for a specific new approach. In the section “Modelling a discipline: the truth of historical theory” we analyse what it is to take a discipline seriously from a philosophical point of view. We see that the removal of philosophical puzzlement about historiography requires a “model” of historiography. A model says

something “true” about the matter being modelled, and we explain what it is for a philosophical model to be successful. Both philosophers and historians have, in the last one hundred years or more, worried about whether historiography should properly be regarded as scientific, and it is the imposition of models of science on historiography which has typically given rise to those already-mentioned complaints about philosophical arrogance. We eschew the question whether historiography is a science, but the history of the debate about this matter gives us the opportunity to understand how the philosophy of science has modelled science, and we learn from this some main ways in which philosophy can model any discipline. In this section we examine in particular work by Karl Popper and Carl Hempel, and in doing so we recover issues about the nature of historiography from the fact that our presentation is, in part, a historiography of the philosophy of science. We observe, too, how in particular Hempel tries to model historiographical explanation, and how in particular Popper uses his approach to express a view about the ethics of historical change. Both make causation essential to their analysis of historiography. We do not.

Continuing this approach for illustrative purposes, in the section “Description and prescription” we emphasise two features of the Popper-Hempel model of science, first, that it purports to describe scientific thought or practice, and second, that it sets a prescriptive standard for that thought or practice. That the model is true, if it is, means that the model is true in both ways, and this requires that both the descriptive and prescriptive elements are appropriately justified. Using material from the philosophy of law (which attempts to model law), we analyse and query the “descriptive”/“prescriptive” – often seen as the “fact”/“value” – distinction, an issue we keep in mind as we introduce Thomas Kuhn’s work. The way in which Kuhn’s approach is to be interpreted as descriptively and prescriptively inconsistent with that of Popper and Hempel is newly analysed. Separating description from prescription, we first see that Popper and Hempel may be historiographically wrong by contrast with Kuhn, that is, that they disagree about the history of science. Nevertheless, given the descriptive/prescriptive distinction, their work as setting a standard for science might still stand. Yet stand it did not. Kuhn did not revolutionise philosophy of science by merely offering a descriptive update of the history of science, and the way in which his philosophy sets a standard for science is analysed. A part of our analysis here

introduces the idea of pluralism, that more than one standard for a science might be appropriate. Kuhn and Popper-Hempel conflict in their standards of justification, of what counts as good reason to believe something, and it is their models of justification which are shown as themselves needing justification.

In the section “Justification in the second-order context: Popper and Hempel” we thus distinguish first-order and second-order contexts of justification, and look at the justification Popper and Hempel offer for their own model. We see that their justification of their model as *descriptive* is historiographical. With regard to the justification of their model as *prescriptive*, we analyse the issue using our earlier claim that knowledge, and in particular a model in this context, might be self-justifying. Self-justification fails at the level of the Popper-Hempel attempt to model *science* because of the existence of rival models which all meet a minimal standard of acceptance in this context. However, the issue is whether there are rival models which justify models of science. If there are not, then the second-order model of justification may then be, in principle, self-justifying, rather than having itself to be justified on some further ground. Moreover, the model we need in the second-order context to decide between opposed models in the first-order context will be a model of justification even if the first-order models are not, so permitting the modelling of historiography even if the theory of knowledge is shown to be less central to that discipline.

As to the second-order justification that they offer, the Popper-Hempel view purports to be justified on the basis of David Hume’s view that everything we know must be derived from our experiences. That no relevant question is begged here, when properly understood, is shown, following an analysis of positivism and its association with empiricism. Nevertheless, the ultimate situation is one where different philosophical approaches may vie with each other. There are no philosophically independent standards, while Popper and Hempel in fact offer no relevant philosophical arguments in favour of their philosophical approach, largely taking its correctness for granted.

However, we also show that, even if, in some possible world, there were no

alternatives to Hume's philosophical approach, his philosophy would be still not be self-justified as providing a second-order justification for choosing one rather than another of rival models of science, because that approach does not even reach the minimum standard of acceptability. This is for the simple Humean reason that we are seeking to justify a choice between *prescriptions* and not between *descriptions*, given this Hume-based distinction. We cannot, given Hume, use criteria for "factual" determination to yield prescriptive outcomes. Hume in effect only offers us taste for selecting one prescription rather than another, and this does not give us the second-order justification we need. Pre-Kuhn positivists wrongly took the second-order justification of their model for granted.

In fact, the persuasiveness of the Popper-Hempel model failed when it was accepted that it did not meet the facts of scientific practice, as disclosed by Kuhn's history of science. By developing the descriptive understanding of science, Kuhn was understood to be advancing also our prescriptive model of science. In the section "Justification in the second-order context: Kuhn" we analyse Kuhn's justification of his own position. Kuhn confusedly thinks of his own work as having distinct descriptive and prescriptive modes, but, in so far as this Humean distinction is fundamental to his work, this leaves him open to the same "mere taste" objection as we have by now explained applies to Popper and Hempel. In fact, he is not committed to this Humean distinction. His actual justification is that scientists do in fact behave as his theory says they should, and this persuasion by reference to historical facts was, in practice, powerful and effective. While the debate about Kuhn generated much opposition, contributors agreed that historical facts were central. Thus Imre Lakatos thought that the historiography of science has to bear out our theory of scientific rationality. Given our approach so far, we show that an appeal to historical facts might be taken to be self-evidently justified, just in so far as all concerned share the same view of their centrality, and agree or presuppose that there is no effective rival to that position.

However, as to this centrality of historical facts in second-order justification, it is not enough to accept this approach as self-evident merely because and in so far as there is no rival to the position. This is because the justification might fail to reach the

minimum standard of being plausibly effective in the second-order context. Like Hume's, it might not do so: thus, why would a correct description of what scientists do yield a correct prescription of what they ought to do? Even without a categorical distinction between fact and value, it seems to be an invalid move. However, a proper understanding of descriptions in this context is that they express what we *count* as scientists, and this means that we recognise or affirm that they are people who meet the rules which govern scientific behaviour. Social institutions are to be understood as systems of rules, and these include academic disciplines like science and historiography. The rules in question exist in so far as they specify what it is to act as member of the discipline in question and in so far as they are accepted by practitioners as appropriate standards for criticism. A person, in successfully occupying a role, meets prescriptive standards. This, in principle, blocks the Humean objection to so-called "descriptions" justifying so-called "prescriptions". Further issues depend upon the detail. In particular, certain problems of circularity are removed in this section.

We thus argue that it is appropriate to think of the offices, roles and practices and the rules or principles which specify or express them as a model which specifies a "standard" which characterises the discipline. Even a philosophical model can be recognised and adopted by a discipline as constitutive of the self-understanding of the practitioners of that discipline. In the case of science, it is the practitioners of the discipline who determine who counts as being a scientist. A theory so used within the discipline may be self-evidently justifiable to the practitioners of the discipline just in so far as it in fact expresses the self-understanding of the practitioners of the discipline. We conclude the section by arguing that the philosophy of a discipline is in the first instance the historiographical recovery of the rules or principles or model in terms of which the practitioners of the discipline conceive themselves to be operating.

In the section "Rival historiographies of science" we apply this approach to the issues between Popper-Hempel and Kuhn. In so far as they appeal to historical facts about scientists, they are not appealing to prescription-independent "facts", but are developing a model for understanding the history of science itself. Having already argued that the justification of a scientific model lies with the historiography of science, we now see that that historiography itself involves rival models. For us, rival models

in this context are merely illustrative of our own problem, how to justify a model of historiography in general. In this section we stress the historiographical *choices*, and so the historiographical *judgements*, which need to be made, so opening room for differing degrees of discretion, arbitrariness or determinacy in the necessary decisions. Historiography, we argue, is shot through with choices.

At this stage of our argument, we conclude that the philosophical model of a discipline is typically offered as being both descriptive and prescriptive (these being necessarily linked), and that such a model needs to be justified. Justification typically requires an appeal to historical facts, in particular the model which the practitioners of the discipline conceive as characterising their discipline. Given this, we regard the historiographical recovery of such a model as the appropriate way to engage in the philosophical modelling of historiography. We begin this investigation in the section “Historiography of historiography: prior considerations”, which is the first section of Chapter 3, “Writing the History of Historiography”. In order to recover historians’ self-understanding of their discipline, we need to undertake the historiography of historiography, yet also show philosophical respect for historiography by not imposing on historiography some external philosophical model. It is historians’ views that must count, for they are paradigmatically authoritative as to what their self-understanding is. Yet while we wish to turn immediately to historians’ views, step after step of theoretical considerations have first to be dealt with, over several sections.

A number of preliminary points are made in the first section of this chapter as to the choices which historians have to make in approaching both historiography in general, and the historiography of historiography in particular. Central to this is our selection of primary sources, and the next section, “Our primary sources”, deals with this, analysing the difference between the use of primary sources and R.G. Collingwood’s “scissors-and-paste” approach, considering also the different “primary sources”/“secondary sources” distinction. Here the status of sources as “authoritative” is examined, and the relationship between philosophy and historiography, when the philosophy of historiography is being undertaken, is further analysed. Respect for historiography means that we must take historians as authoritative primary sources. Yet we show that we are thereby ourselves inevitably

acting as historians, and that philosophy and historiography are not *rival* points of view. Rather, they are continuous with each other. The philosophical value of historical hindsight is again stressed in this section.

In the following section “Our use of primary sources” we analyse how sources can be justifiably interpreted, initially drawing on Carl Hempel and Aviezer Tucker. We face a specific problem of how we can obtain, from a historian’s own text, that historian’s understanding of the character of the discipline of historiography. The various ways in which texts in the history of ideas can be interpreted are explained, and the literal distinguished from the contextual in ways which both philosophers and historians, with characteristically contrasting traditions, can accept. It is on the basis of these arguments that we take as authoritative the explicit literally understood historiographical texts which we will use as our primary sources.

There is a question as to how our, or any, historiography, or historiography of historiography, ought to be written. Our selection of *questions* characterises our engagement with the discipline of historiography, and in the section “Choices and questions” we deal with this. There are issues such as time frame, sources, modes of interpretation, or what other subordinate disciplines might be used, quite apart from the question whether historiography should be approached by using historiography at all. Just because we seek a historiography of historiography, we then seek a historiography of historians’ choices. Historians and philosophers alike can ask historiographical questions, and their questions are not categorically distinct. In addition, questions have presuppositions. Contingently, these vary with the approach, but our concern will be with those matters which are *characteristically* historiographical.

The section “Character and consensus” deals with historians as practising a rule-governed discipline, and we seek that which they share, that with respect to which there is some consensus. What it is for a consensus to exist is analysed, and it is argued that the notion is not to be philosophically imposed, as some philosophers do, but rather should be historiographically recovered. The recovery of both choices and non-choices is analysed, the latter in terms of Collingwood’s notion of “absolute

presuppositions”. These are shown to mark the contingent limits of choice in the historical judgements made by past historians, where they face alternatives. Historical hindsight is analysed in terms of this, as is the way in which the choices of past societies and individuals can either be empathised with or modelled. We conclude that the recovery of an absolute presupposition, of a “non-choice”, is characteristically historiographic just in so far as it is capable of being understood in terms of characteristically historiographic ways of ascribing choices and thoughts to past individuals.

In the section “Historiography of historiography” we examine some historians’ historiographical work, for here we see what they count as such. Recognising that we cannot escape our own presentational choices, we select historians who are paradigmatic of the discipline. Here we first examine Herbert Butterfield, both as subject of and author of historiographies of historiography, both as primary source and as secondary source. We observe that, like some other historians, he begins his understanding of the history of the subject with Ranke and historians of a similar generation, and we provide an outline of the history of historiography from Herodotus to the twentieth-century against which that judgement can be assessed. By contrast with Butterfield, we see from Arnaldo Momigliano’s and others’ work that Herodotus and other classical writers have a significant place as a touchstone in historians’ self-understanding. We compare Herodotus and Ranke as paradigmatic historians and find that characteristic historiographical questions have not changed since early times.

In the section “Historians’ self-understanding” we observe that student historians who are concerned to reflect on the character of their discipline are often advised to read works which are not historiographies of historiography. Here we examine at length E.H. Carr, G.R. Elton, Marc Bloch, Charles-Victor Langlois and Charles Seignobos, commenting on many other historians, earlier and later, in passing. Striking is the fact that the *history* of historiography is characteristically not a part of historians’ self-understanding. That historians, in seeking to explain their own discipline, do not characteristically use historiography to do it, is commented upon. The central questions of truth (was Herodotus a liar?) and rhetoric, of comprehensiveness of approach and of moral judgement, while variously answered over historical time, have

continually underpinned historians' self-understanding.

The characteristic present-day historians' worry is dealt with in the next section, "Postmodernism", which is the first in Chapter 4, "Pragmatic Postmodernism". This is a subject recently addressed by Richard J. Evans. He argues against the view, derived from a number of philosophers and accepted by a number of historians, that historians' language does not correspond to historical reality, a view which makes historiography essentially indeterminate and unjustifiable. Evans' own solution, briefly, evidence and agreement, is not, however, sufficient to address the powerful foundations of postmodernism. In this section we argue that postmodernism is best seen as antirealist, with a sophisticated view of reality as being what we *count* it to be. That being so, language "fails to correspond to reality" because there is no independent reality for it to correspond to. It is not then, of course, a *failure*, and we need to make sense of words such as "knowledge", "truth" and related terms in ways that make sense, not in ways that do not. Given antirealism, it is a mistake to suppose that only a language-independent reality would ensure determinacy. The postmodern position suggests unlimited *freedom of choice* in the context of what to believe about reality, and hence it is a contingency what people understand reality to be. This section examines the nature and limits of that claimed freedom. As the chapter proceeds, we find that there are sufficient resources within the antirealist approach to give us the certainty and objectivity we need, without any requirement to defend antirealism against realism.

For the anti-postmodernist historian, sound commonsense should block this claim to unlimited freedom of choice as to how reality is to be understood. In the section "Commonsense and experience: Hume" we hold that the antirealist postmodernist is better conceived as broadly empiricist rather than rationalist, since the position brings together a pragmatic tradition in such as Richard Rorty, a position which began with Humean empiricism, and a "Continental" tradition in such as Michel Foucault, a position which seeks the limitations of Kantian rationalism and which involves a philosophical attention to experience, such as in phenomenology, and to choices, such as in existentialism. It is the view of reality as essentially known through experience which is often taken to mark the sound commonsense of the historian. However,

Humean empiricism is *atomistic* in its form: it holds that experience comes in small bits which are associated and re-associated with each other, both in reality and in our understanding or knowledge of it. Yet both experience itself and our best philosophical efforts have failed to show that this is a correct characterisation of the situation. Present experience presents itself all at once, and not bit by bit, and our understanding involves the active focussing of attention. To the postmodern empiricist, a Kantian solution in terms of fixed mental categories will not suit, not merely because of some empiricist dogmatic denial of such rationalism but because in fact we can and do, on occasion, have *choices* how to count reality. This is demonstrated. What we count as facts depends in part upon the rest of what we believe. These decisions are commonly socially, rather than individually, made. The “truth” of these many beliefs is mutually supporting, and based on practical grounds. This being so, it is best to adopt here a *holistic* rather than an *atomistic* conception of knowledge and truth.

To make clear this pragmatic holistic empiricism, in the section “Quine as postmodernist” we isolate and examine one central feature of W.V.O Quine’s philosophy, namely his view that our beliefs form a “web” which meets reality as a whole. Quine recognises that there is room for decision as to which sentences we wish to hold true and which false. We can amend our knowledge claims as convenient, and there are in principle many ways of doing this. In a famous expression, Quine said “Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system”. Moreover, “no statement is immune to revision”, so *unlimited* adjustment is available. Postmodernism in this form gives us freedom to count reality as we wish, *except* that any consequential cost of our decisions must be met by sufficient adjustment elsewhere in our system of beliefs.

With the postmodern claim that we can decide what to believe now expressed with some accuracy, we next turn to two arguments from Bernard Williams who denies that we can decide such things. The first of Williams’ arguments relies on a certain kind of causal theory of belief, and presupposes conflict between that and a “freewill” conception of human decision. Quine might well – if perhaps inconsistently with his holist position – want a causal theory, but postmodernism more generally has no such

commitment, and causation is consistent with decision in many ways. Williams' second argument, unconnected to the first, amounts to an assertion that we just cannot successfully represent reality at will, because our *concept* of reality does not give us this power. That concept, however, denies us such a power as *individuals*; it does not deny it of us as a *community*.

The section continues with an analysis of Quine's important claim that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system", involving as it does an ambiguity of scope at a subtle level. Here we emphasise that his "web" of beliefs purports to *constrain* particular beliefs. The upshot of the analysis is that Quine's claim should be read as (1) the categorical claim that we can hold true any statement; (2) the conditional or hypothetical claim that, if we are to hold true any statement, then we must make sufficient adjustments for the purpose; (3) the categorical claim that we can make sufficient adjustments for the purpose.

Claim (2) may seem to constrain the operation of claim (1), since our factual decisions require meeting *costs*, which vary according to the decision made. Yet Quine also holds that *unlimited* adjustment is available, in other words that we can *always* meet the cost of adjustment, no matter how high that cost is. Thus (2) is no constraint at all. This seems to make Quine the most chaos-asserting of postmodernists.

Can we *always* meet the costs of belief? In the section "The costs of belief" we note that it is a contingency when we face costs. They arise in situations of choice, where we desire to revise or are obliged to revise, and decision is called for when we observe the *conflict* between beliefs. For Quine, they particularly arise when "recalcitrant experiences" place other beliefs at risk. Such experiences are not theory-independent, and we analyse "recalcitrant experience" and the way in which such "costs" arise, drawing initially on David Hume, who spells out an atomistic version of the costs of revising belief. Costs, on Quine's approach, have by contrast to be holistically understood, and the Humean error here is clarified. Bernard Williams makes just such an error, and we deal with what is intended by him to be a difficult example, which we show involves supposing that revising a belief is equivalent to trying to live with a false

belief. On the holistic approach, by contrast, revising a belief requires accepting a belief system in which we do not face recalcitrant experiences relative to it. We argue that, for many beliefs which we might think of as “established”, it is practically impossible to revise them, for alternative beliefs which actually meet the required costs are not in practice available. The outcome is that imagining the revision of belief is not imagining what it would be like to live with a false belief, but imagining what it would be like to live with a true one. Successful adjustment is, in the main, pragmatically impossible. Quine is then wrong to imply that we can always meet the costs of adjustment. Free postmodern revisability of beliefs is not available. Reality, while in principle a function of human choice, is nevertheless largely “fixed”.

Sadly, it is not that easy. In the section “Quine not postmodern enough” we point out that Quine’s argument, and ours, has presupposed the availability of standards of logic which force us to choose between conflicting beliefs. A postmodernist such as Foucault would deny us reliance on logic. Maybe the insistence on universal consistency, presupposed in our presentation of Quine’s position, is a totalitarian desire to impose a logical order on things. In this section we analyse the practical judgement of what we recognise as conflicting beliefs, basing that on what we judge our psychological capacities to be rather than on some conception of logic as an independent set of standards which might be imposed by some power structure. While it is a contingency what we hold to conflict and what we do not, this gives us enough “logic” to drive our recognition that some beliefs conflict, and that we have to choose between them. However, the argument works differently from the first-person singular point of view as compared with the first-person plural point of view. Moreover, “we” is ambiguous in a way in which “I” is not. If reality is no more or less than what “we” (conceived as a collection of first-person singulars) count it to be, then, briefly, in so far as I count the world as p and you count it as $\text{not-}p$, reality is such that p and $\text{not-}p$. Contingently, however, we desire to *share* our reality. We value others, and we value sharing reality with them. From the perspective of the individual, “truth”, “reality” and “consistency” and the like are not wholly personally determinable but rather function as external and objective. Such “objectivity” is antirealistically understood, and we recognise that truth, reality and consistency function as values for us which express our desire and need to share the same world.

It is nevertheless a contingency how much agreement we need, or how much we value sharing, and this is crucial to the nature and expression of historical knowledge, a matter with which we deal in the next chapter, “The Room for Judgement”, beginning with its first section, “Narrative truth”.

A crucial feature of historical writing, going back to Herodotus, is that historiographical writing is characteristically a lengthy affair. Herodotus did not think that he was offering merely a list of discontinuous facts, but in some sense *unifying* them. It then seems that historical accounts should be seen as a *synthesis* of *atomic* facts. Langlois and Seignobos and others analyse historiography in this way. Leon J. Goldstein thinks this completely wrong, and here we present his views and analyse them and his associated (and other) historiographical examples at some length, arguing for a conception of historiographical “synthesis” in narrative which does not face his difficulties. Using this, we then in the section “A fancy view of truth” recognise that the atomic factual sentences in a historiographical account and the synthesis of those sentences alike have to have sound cognitive support. Using constructed historiographical examples with highly tendentious selection, we demonstrate the difference between truth at the atomic sentence level and at the whole account level, and also display whole account level inconsistency as distinct from atomic sentence level inconsistency. We recognise that historians characteristically think of the expression of historiographical truth not as a mere list of truths but as a grouping of truths. The historical account typically has some synthesising feature on the basis of which historians characteristically recognise which facts are relevant and which are irrelevant.

Since historiographical accounts are the characteristic way for historians to count reality, since they each express a truth claim – *the* truth, rather than just a collection of truths – about their subject matter, then our previous arguments concerning the nature of truth and reality apply to them at the level of synthesis. The postmodern claim is then that historiographical accounts can configure historical reality in many different ways, analogous to the claim that we can believe what we like. We may then use our Quinean argument, including his assumption of consistency, that any reconfiguration of historical reality requires adjustments elsewhere in our system, which we now

understand to be a system not just of atomically understood beliefs but one including whole historiographical accounts, including our evidence for them. Adjustments are then required when we have to decide between some historiographical accounts and others when they conflict at the whole account level. Accounts may then be *established* historiographically when holistically understood conflicting alternatives are not pragmatically available.

What might be called “whole account” truth is not truth-functional, and in the section “Holistic choice” we apply the second part of our Quinean argument, where independent standards of logic are not to be taken for granted. Once again, shared standards of consistency arise when we, contingently, seek to share with others what we recognise as conflicting and, contingently, also share the recognition that the conflict needs to be resolved in so far as we seek to share our reality. This historiographical resolution would again have to be holistic. Again, with consistency thus pragmatically understood, many historical accounts would thus be established in their truth-telling status at the level of synthesis, being pragmatically indubitable at that level. The pragmatics of holistic empiricism would then again warrant for us the broad reliability of our ordinary understanding of historical reality, as that is expressed at the whole account level.

However, while our shared language operates successfully at the level of the atomic sentence, it does not completely do so at the level of the whole account. To make it work, we need to count reality in such a way that we can share some part of it as our subject matter, we need to share with others a desire to share the reality in question, we need to recognise where we have, if we do, conflicting ways of counting that shared reality, and we need to recognise that any such conflict needs to be resolved, just because we wish to share the reality in question. It is *contingent* whether we recognise these things. We do indeed do so at the level of the atomic sentence, while equally historians often share with each other the view that they are each seeking, in their different ways, to describe or count the same reality at the whole account level. They recognise that they may conflict in doing this, and they frequently seek for resolution of such a conflict. However, historians, like the rest of us, are sometimes unwilling to share. Not all historians think that every factual disagreement is a worry

to be overcome. Many historians have accepted a roughly postmodern view, and think that history can be narrated in conflicting ways, but without thinking that the conflict is either resolvable or needs to be resolved. The need to share historical reality is not always felt.

Anti-postmodern historians will need to find ways of overcoming factual inconsistency at the whole account level and of making historical writing more determinate at that level. Historical judgement requires an understanding of the synthesising factual choices available at the whole account level, and what their pragmatic limits are. In the section “Structuring factual synthesis” we deal with the limits of choice with respect to synthesising structures, beginning with an analysis of “narrative” in the context of historiographical examples. We show that modes of causal explanation and related philosophies of action do not work, because of their essentially atomistic character. We need a theory, shown to be appropriate to historiographical practice, which permits the judgement of relevance for the purpose of selection, and which will place causal and other atomistic modes of explanation in a synthesis of atomic facts. Hayden White’s *Metahistory* offers this.

Since, given antirealism, reality is what we count it to be, language is at the centre of our understanding of reality. The “linguistic turn” means that all the resources of our language are available for this purpose and this includes “tropes”, or literary devices. The particular details of White’s literary theory are not relevant to our overall argument, for what really matters at this point is how much *choice* we have over the use of such devices. Such tropes may be conceived as Kantian categories, or they may be conceived as culturally variable. In fact, that historiography is characterisable as having a poetic foundation seems to have continued without change since before Herodotus. Our argument, as before, is that such tropes are plausibly fixed unless alternatives are available. Pragmatically, there just may not be alternatives.

Categorisation in terms of storylike modes may, contingently, be foundational for us because this is how the human brain works. But the development of modern science has epistemically privileged atomistic modes of expression, for no good reason. The limits of our choice here are most likely to be shown by our art than by our science.

Finally, in the section “Moral judgement in historiography” we present the range of historians’ views about the merits or otherwise of moral judgements in historiography. We concentrate particularly on Ranke’s wish to avoid judging the past, and to say rather how it essentially was, explaining his position with respect to the philosophy of his own time. Recalling that much of factual assertion may involve decision, we argue against that needing to be the exercise of moral judgement. While we are not “passive observers”, it is a contingency how moral or political our historical judgements are, and this varies with our audience or readership. We show the contingencies which make Ranke “objective” for his own time but not for ours. Ranke requires of himself that he consciously avoid projecting his *subjectivity* into the subject of enquiry. That, personal to him as it is, he characteristically does.

Ranke, facing not some ideological commitment but a continuing historiographical choice predating Herodotus, *chooses* not to judge the past. It is a moral, not an epistemological, question, whether historians should engage in moral judgement. We discuss whether it is morally right to require of historiography that it be a dispassionate discipline, recognising the historical contingency of our answer. Historians are certainly in a privileged position for moral judgement, having both hindsight knowledge of consequences and discounted passions over time. Ranke recognises that historians have in fact been called upon to judge the past and instruct for the future. This, however, is a contingency, as is what we count as a moral issue. Given pragmatic holistic empiricism, we can count the world differently, and we can count the moral world differently. We can count morality as absolute, or we can count it as variable. Importantly, the alternatives are pragmatically available; at least for now. We conclude by pointing out that we must understand and ensure the moral and social responsibility of historiography.

Chapter 2 The Philosophy of a Discipline

Respect for historiography

Says Michael Bentley, “Rarely has a generation had the opportunity of the current cohort of students to rethink what history means. Very heaven is it now to be young, bright and eager to think about the past and what the study of it can yield”.² That opportunity has long been there, but many historians continue to be averse to the philosophy or theory of their discipline. They are often impatient with the sceptical things that some theorists of history have said about, for example, the quality of historical knowledge, and alert to what they perceive as the arrogance of those philosophers or other theorists who make judgements about historians’ outputs or recommendations for historians’ methods. “Some historians,” says Richard J. Evans, thinking particularly of Geoffrey Elton, “have even disputed the right of non-historians to write about the nature of historical knowledge at all”.³ Even when written by historians, works of this kind may not fare much better: Charles-Victor Langlois and Charles Seignobos said of works which deal with historical methodology, “specialists despise them” and that the great majority of such works are “superficial, insipid, unreadable, sometimes ridiculous”.⁴ They could not themselves escape objection: Bentley, reporting rather than commenting, refers to “their notorious manual of method” and to “the grotesquely maligned French historian Seignobos, who had co-authored one of the least-loved manuals of historical method in modern times”.⁵ Even G. Kitson Clark, himself an author of a manual (in addition to being a historian of Victorian Britain), remarks “a good many books have been written by historians of varying eminence on the methods of historical research. ...you need not read any of

² Michael Bentley, *Modern Historiography: An Introduction*, London: Routledge, 1999, p. ix.

³ Richard J. Evans, *In Defence of History*, London: Granta, 1997, p. 11. Evans characterises in this book a wide range of attitudes of historians to the theory of history, particularly postmodernism. See also his “Afterword” in the 2000 edition. As a philosophy Ph.D. student trying to make sense of history, I found Elton very kind; but he certainly thought I needed to do history in order to understand it philosophically.

⁴ C.V. Langlois and C. Seignobos, *Introduction to the Study of History* [1898], trans. G.G. Berry, with a Preface by F. York Powell, London: Duckworth, 1912, pp. 3, 5.

⁵ Michael Bentley, *Modern Historiography*, pp. 104, 85.

them”.⁶ Here, for another example, is historian Jack Hexter reviewing philosopher (and historian) Morton White’s *Foundations of Historical Knowledge*: “It is hard to avoid the suspicion that Historian White is something of a patsy when Philosopher White lays down the law”,⁷ concluding by ascribing to White (as a philosopher) “an intellectual imperialism generated by the sin of intellectual pride”.⁸ White is “peremptory, pre-emptive, and prescriptive about ‘meaning’, ‘knowing’, ‘understanding’, ‘explanation’, and ‘truth’”.⁹ Plainly, Hexter sees White as meaning to change things, perhaps even seeing him as an inheritor of Karl Marx’s thesis “the philosophers have only interpreted the world, in various ways; the point is to change it”.¹⁰ Historians, we are to understand, need do no more than direct our attention to these unfortunate philosophical character traits. After all, such historians commonly say, history is a major *discipline* which involves successful methodological practices. As to theorising about those practices, “the general rules to be observed are largely the rules of common sense”.¹¹ It is no surprise that, on the whole, “philosophers and historians have flagrantly and wantonly ignored each other”.¹²

While we should recognise that history is rightly characterised as a discipline (whether or not we, thinking as philosophers, agree that it is one which involves successful methodological practices), we should note that the word “history” is also used, by historians and non-historians alike, to refer to (or purport to refer to) that reality which the writing¹³ produced by the discipline “history” is supposed to be about. In an attempt to avoid confusion, we shall here use the word “historiography” to refer to the

⁶ G. Kitson Clark, *Guide for Research Students Working on Historical Subjects*, 2nd edn., London: Cambridge University Press, 1968, p. 9.

⁷ J.H. Hexter, “The one that got away” (review of Morton White, *Foundations of Historical Knowledge* and A.C. Danto, *Analytical Philosophy of History*), *The New York Review of Books*, 9 February 1967, pp. 24-28 at p. 26. Hexter was a master of invective.

⁸ *Op. cit.*, p. 28.

⁹ J.H. Hexter, reply to letter (pp. 28-29) from Morton White responding to Hexter’s review, *The New York Review of Books*, 23 March 1967, pp. 29-31 at p. 31.

¹⁰ Thesis XI, Karl Marx, *Theses on Feuerbach*, rep. in David McLellan (ed.), *Karl Marx: Selected Writings*, Oxford: Oxford University Press, 1977, p. 158.

¹¹ G. Kitson Clark, *Guide for Research Students*, p. 9.

¹² Frank Ankersmit and the other Editors, “The philosophy of history: an agenda”, *Journal of the Philosophy of History* 1, 2007, pp. 1-9 at p. 1.

¹³ We may recognise that there are other modes of meaningful expression in addition to writing: speech or film or maps, for example.

discipline “history”, and reserve the word “history” for the subject matter of that discipline.¹⁴

Our use of “historiography” here needs to be further explained. “History” as a word has a long history. “The word ‘history’ itself comes to us from these sixth-century [Greek, BCE] Ionians and is the name they gave to their achievement. It meant not the telling of a tale, but the search for knowledge and the truth. It was to them much what philosophy was to the later Athenians or science to us. ...It was not until Aristotle, and more especially Polybius, that we have it definitely applied to the literary product instead of to the inquiry which precedes it”.¹⁵ First, then, the enquiry, and second the product of the enquiry, but where, here, is a third use of “history” as the past reality? Confusion continues, and it is not just in English: “Biester once replied thus to Frederick the Great’s inquiring after what he was doing: he occupied himself ‘famously with history’ (*vorzüglich mit der Geschichte*). The king stopped short at that and asked whether that meant the same as *Historie* – because, Biester supposed, the king was unfamiliar with the expression *die Geschichte*. Of course Frederick knew the word *Geschichte*, but not the new concept: history as a collective singular without reference to an associated subject or, alternatively, an object determined by narration”.¹⁶ W.H. Walsh points out that it is a “simple and familiar fact that the word ‘history’ is itself ambiguous. It covers (1) the totality of past human actions, and (2) the narrative or account we construct of them now”.¹⁷

But it is not a “simple and familiar fact”. Between Biester and Walsh is much room for misinterpretation. In an attempt to ensure clarification, Aviezer Tucker proposes the use of “historiography” to refer to the writing of historical accounts, extending it slightly but importantly to refer also to the construction of those accounts on the basis

¹⁴ This distinction, like the claim that historians successfully *refer* to a past reality, raises a range of philosophical issues in metaphysics and the philosophy of language and in particular that of realism versus antirealism. While these questions are here passed by, the expression “purport to refer” is included to show that they are not begged. See also the end of the section “A fancy view of truth”.

¹⁵ James T. Shotwell, *The History of History*, vol. I, revised edition of *An Introduction to the History of History*, New York: Columbia University Press, 1939, pp. 8-9.

¹⁶ Reinhart Koselleck, *Futures Past: On the Semantics of Historical Time* (trans. Keith Tribe), Cambridge, Mass. & London: The MIT Press, 1985, p. 201.

¹⁷ W.H. Walsh, *An Introduction to Philosophy of History* [1951], London: Hutchinson, 3rd edn., 1967, p. 16.

of evidence, while again reserving the word “history” for the past subject matter itself.¹⁸ “Historiography” as then used by Tucker, referring to historical account construction, differs from the use proposed in our present work, where “historiography” primarily refers to history as a discipline. “Historiography” as used by Jack Hexter differs, too, since for him it refers to the craft of writing history but without the evidential side: “Historiography is different from the collection of historical evidence, the editing of historical sources, the exercise of historical thought and imagination, the criticism of historical writing, and the philosophy of history, but it is related to all of them and overlaps some of them”.¹⁹ Unhelpfully, by contrast with our own, with Walsh’s and with Tucker’s positions, Hexter reserves the word “history” for the *discipline* and not for the past itself, for which no third word is offered. However, we do not here require further terminology, for in here conceiving historiography as a discipline we also conceive it to be prior to and to include historiography conceived in these other ways. Historiography as a discipline is also understood to include historians themselves (whether as individuals or as a community), in addition to their writings, methods, criticism and the like. Historiography is not, however, taken *a priori* to include the philosophy of history as undertaken by professional philosophers, since most practitioners of historiography would not include such philosophy of history as part of their remit.²⁰

It is then *historiography*, we are to understand, which involves “successful methodological practices”. It is clear, to those historians who say this, that historical knowledge and methods are not to be undermined or overturned by mere theoretical speculation on the part of those who have no knowledge or experience of the subject. Philosophers of historiography, they think, should first make themselves familiar with the practice of historiography and the knowledge achieved in that practice, and only then proceed with their philosophical reflections. Some philosophers agree with this, such as Leon J. Goldstein: “It is my opinion that the ease with which philosophical

¹⁸ Aviezer Tucker, *Our Knowledge of the Past: A Philosophy of Historiography*, Cambridge University Press, 2004, p. 1.

¹⁹ J.H. Hexter, “The Rhetoric of History” [1968], in *Doing History*, London: George Allen & Unwin, 1971, pp. 15-76 at p. 15.

²⁰ A different distinction, not dealt with here, is between “the past and history”. See J.H. Plumb, *The Death of the Past*, Harmondsworth: Penguin, 1969.

writers do that – move outside the practice of the discipline in order to impose upon it – is another impediment to the development of a genuine epistemology of history”.²¹ As someone recently said in private correspondence, “surely one must focus on what the participants in a discipline *actually do*, and then subject their doings to a (philosophical/theoretical) evaluation”. But this recalls Arthur Danto’s response to what he described as Hexter’s “vagrant and irrelevant” review of his own and Morton White’s books:²² “What Hexter evidently wants...is some sort of exact composite portrait of the working historian”.²³

Philosophers may observe that there is a hint of Kant’s transcendental questioning in such historians’ attitude: the demand is that philosophers, in so far as they engage in the epistemology²⁴ of historiography, should not take the Cartesian view that historical knowledge, like all such claims, should be wholly doubted until the philosophers find proper proof of that knowledge, but should rather begin with the view that historical knowledge is successfully achieved, and then ask the transcendental²⁵ question – as Kant asked in a different context – how that knowledge is possible. More generally – since the philosophy of historiography is not necessarily the *epistemology* of historiography – such historians’ requirement is that philosophers should have more respect than they do for historians and historical work.

Philosophers may *observe* that many historians are averse to the philosophy of their discipline, note the characteristic reasons for this, and even wish to respond in an irenic way. However, formulating a more historiography-friendly reply is not straightforward. Recalling that the philosophy of historiography has typically centred on its epistemology, then, in broad brush terms, philosophers have typically had to use something like Descartes’ “method of doubt”. In its original form, the method

²¹ Leon J. Goldstein, “Impediments to epistemology in the philosophy of history”, *History and Theory*, Beiheft 25, “Knowing & telling history: the Anglo-Saxon Debate”, 1986, pp. 82-100 at p. 87.

²² A.C. Danto, letter responding to Hexter’s review, *The New York Review of Books*, 18 May 1967, pp. 41-42 at p. 42.

²³ *Op. cit.*, p. 41.

²⁴ Epistemology: the philosophy of knowledge.

²⁵ One which professes to pass beyond the limits of possible [historiographical] experience; see Immanuel Kant, *Critique of Pure Reason*, trans. Norman Kemp Smith, London: Macmillan, 1933, p. 299. “We...do not have to ask whether such knowledge is possible (for it is real), but only *how it is*

required that all belief be suspended (“there is nothing at all that I formerly believed to be true of which it is impossible to doubt”²⁶), that is, suspended until something is found “that is certain, or at least, if I can do nothing more, until I shall know with certainty that there is nothing certain”.²⁷

There are two central moves here: first, the need to begin by doubting those of our beliefs formerly received as knowledge; second, the specification of the criterion or criteria to be used for the re-admission of beliefs to knowledge. Descartes was a rationalist, and with regard to the second issue his criterion for knowledge was rational certainty: “bodies themselves are not properly perceived by the senses nor by the faculty of imagination, but by the intellect alone”²⁸; “all that is very clearly and distinctly apprehended is true”²⁹. No empiricist³⁰ would or could accept such a criterion, but even empiricists have to follow Descartes in the first issue, that is, follow the Cartesian method of doubt, and this is so just because and in so far as, first, they accept Descartes’ approach which makes epistemology central to philosophy, and second, they in particular accept with Descartes that a knowledge claim cannot be admitted as such until it is *justified*.

Why would even empiricists have to accept this? For modern rationalists and empiricists alike, knowledge or knowing is commonly *analysed* as some version of “justified true belief”. There are two problem areas attaching to this. First, all elements of the analysis are often supposed to be separately necessary for knowledge and jointly sufficient for it. Conceiving a successful analysis as specifying necessary and sufficient conditions may yet be an incorrect way of understanding the nature of analysis. Second, a successful analysis of knowledge will unquestionably take a much more nuanced form than “justified true belief”, and an analysis of the expression of

possible...”, Immanuel Kant, *Prolegomena to any Future Metaphysics that will be able to present itself as a Science*, trans. Peter G. Lucas, Manchester: Manchester University Press, 1953, p. 29.

²⁶ Descartes, *Meditations on The First Philosophy*, Meditation I, “Of the things of which we may doubt”, from Descartes, *A Discourse on Method and Meditations*, trans. John Veitch, London: Everyman [1637 and 1641], 1965, p. 83.

²⁷ *Op. cit.*, Meditation II, “Of the nature of the human mind; and that it is more easily known than the body”, p. 85.

²⁸ *Op. cit.*, p. 94.

²⁹ *Op. cit.*, p. 96.

³⁰ An epistemologist who holds that knowledge comes only from experience.

historical knowledge or knowing will make a crucial input to this. Nevertheless, avoiding these two problem areas, our current understanding of knowledge, while recognising that more needs to be said, analyses knowledge as being *necessarily* in terms of *justification*. This locks a requirement for some version of Descartes' sceptical presupposition into our epistemology. It is, then, only correct to say that historians provide historical "knowledge" if justification is there.

As it stands, however, this requirement for justification is very weak because no determination of "justification" has yet been given. Subtleties apart, there are two major interpretations of "justification" available in this analysis of knowledge: first, we might well read "justification" as *meaning* or at least requiring that which is sufficient to refute scepticism about the knowledge claim. To correctly describe historical "knowledge" as knowledge then entails that it is beyond refutation, that any relevant scepticism – whatever its source or kind – has been defeated. Refutation of scepticism is here built into the analysis of knowledge itself. Associated with this first major interpretation is recognition that scepticism can take different forms: it might be a full-blooded rationalist scepticism which will not admit as knowledge anything which it is logically possible might be false; or it might be some empiricist scepticism which suggests, for example, that knowledge must be "beyond all reasonable doubt" or "scientifically proved"; and other standards performing a similar function might be drawn from the epistemological literature. Despite such variation, there is, in all such approaches, some standard of justification which has to be achieved for a knowledge claim to be allowed. While philosophers usually think of the standard as being very or at least fairly demanding, it may well be less so, in principle: there is here logical room for a wide range of approaches to what counts as justification, from Cartesian *a priori* reasoning requiring absolute certainty to the view that if your parents believe it then it's good enough for you. The inbuilt refutation of "scepticism" of the knowledge claim may then also be more or less demanding, as appropriate to the standard used.

Yet a different major interpretation of "justification" in the analysis of knowledge might be adopted. We might well think that analysing "knowledge" as some version of "justified true belief" involves some kind of ordinary language analysis, and we might well think that it is improper to build into the word "justified" any external

standard at all, whether some solution to the hard philosophical problems of scepticism or something much weaker, as above. The “justification” requirement might, we could for example hold, be met if – in the typical conversational situation – both speaker and hearer (both perhaps supposed ignorant of philosophy) were sufficiently satisfied that it were met, “satisfied” not because some doubt had surfaced and been removed, even easily removed, but because there was *no doubt at all*. Taking assertions on trust may well be essential to communication: satisfaction with testimony³¹ might be the *default* position. Knowledge claims might be self-justifying in the sense that they are innocent until proved guilty.³² Neither speaker nor hearer, we might suppose, had in practice a reason (a reason which they recognised as such, rather than a reason philosophers might ascribe to them) to doubt it: maybe only *change* of belief needs justification, and it is not our existing beliefs (as self-justified) which require reasons, but doubt itself.³³ We might then see the linguistic analysis of knowledge as one epistemological matter, and the assertion or refutation of scepticism as very obviously a different one.

We observe³¹, then, that there is a very wide range of interpretations of “justification” which might be adopted. We face choice. One of the conflicts in debates about historiography is between those historians who believe that their historical method is self-evidently a good justification in itself and thus don’t like “theory”, and those philosophers who won’t accept this and seek to recover some latent or inexplicit justification opaque to such historians (if not to all), or to rely on some different and non- or extra-historical standard of justification, or to argue on some postmodern basis that no justification is in any event possible.

For some historians to require from philosophers “respect for historiography”, in the epistemological issue just introduced, sounds suspiciously like an arbitrary and question-begging assertion or implication that what counts as a sufficient justification

³¹ See C.A.J. Coady, *Testimony: A Philosophical Study*, Oxford: Clarendon Press, 1992 and my essay on that book in *History and Theory* 33, 1994, pp. 230-241.

³² This approach was earlier used in reviewing C. Behan McCullagh’s *The Truth of History*, London and New York: Routledge, 1998. See Jonathan Gorman, “Freedom and history”, *History and Theory* 39, 2000, 251-262.

for historical knowledge is historians' current practices, practices that are beyond philosophers' questioning. Insistence on focussing on what the participants in the discipline of historiography "actually do", and then subjecting their doings to philosophical "evaluation" (an evaluation which, it is implied, cannot be a wholesale demolition of historical knowledge claims since that would involve asserting that they ought to be doing something else entirely), runs unacceptably close to asserting that we must pretty much take historical knowledge claims and their associated justificatory practices for granted.

We have noted the hint, in the attitude of some historians characterised earlier, of Kant's transcendental questioning, and we have contrasted that with a Cartesian approach. Kant's own transcendental questioning took place at a time of great Enlightenment confidence in science, a confidence built on what were seen as massive advances in knowledge. Without our necessarily thinking him right, we can recognise that it was highly appropriate for Kant, given his Enlightenment convictions, to ask how scientific knowledge, already accepted as such, is possible when, following Galileo, Newton and others, humanity had achieved so much. (It would be missing the *universality* of what had been achieved on behalf of all possible human beings, as the *philosophes*, Kant and other Enlightenment thinkers saw it, for us to think of that achievement as merely a set of beliefs held by certain particular individuals or at most by an educated European class.) Enlightenment confidence was that objective truth had been and would continue to be found, and this justified Kant's transcendental questioning. However, the actual and theoretical room for historians' disagreements – disagreements between each other, not necessarily disagreements between historians and philosophers – contrasts with this Enlightenment certainty. It is taking respect for historiography much too far to treat it as yielding truth claims which are so obviously and transparently and indubitably true that our central epistemological question must be the transcendental one of how such epistemological achievement in historiography is possible.

³³ As argued by Christopher Hookway, "Peirce and scepticism", public lecture, Belfast Branch of the Royal Institute of Philosophy, 2nd February 2006.

But to raise this objection is not to beg the question against the view that historiographic practices are sufficient justification for historical knowledge claims. It is to say that the epistemological issue must not be foreclosed by dogmatic assertion on either side. We leave the choice undecided. That we judge a Kantian transcendental question, when asked *a priori*, to be inappropriate for claims to historical knowledge, does not mean that we have prejudged the matter in favour of an approach which sets some philosophical hurdle which historians have to jump, let alone that we have prejudged in favour of some Cartesian scepticism or some positivist³⁴ conception of science where the criterion for the achievement of knowledge is such that historiography's epistemological failure is ensured. At this stage of our argument we can and do leave *open* the questions of how the epistemology of historiography should be undertaken, how "justification" is best understood, and how far historiographical conclusions are justified, including their being self-justified. The "true" epistemology of historiography, if any, is not our immediate concern. (That does not imply that any answer will do here.) Moreover, recall that the philosophy of historiography is not necessarily the *epistemology* of historiography at all. Philosophy is here, as so often and so typically, trying to crystallise puzzlement into questions, into finding out what the questions ought to be, and that is a central part of our task in undertaking the philosophy of historiography.

Philosophers should indeed have more respect for historians and historical work than they often do. This is partly because we all live in time, in some temporal process, and historians offer an understanding – perhaps better, if only *prima facie*, a range of understandings – of ourselves and our world which may be plausibly argued to be foundational to our understanding of our place in that process. Just how far it is foundational is itself a major philosophical issue. We sometimes understand ourselves (particularly when we think of science as central) as trying to achieve "objective truth" about our place in the world, but perhaps this is not possible and we are unable to escape some necessary human input. Thus in Thomas Nagel's felicitous expression,

³⁴ "Positivism" is the view that science is the only way of achieving knowledge. It is explained further below. The word has other meanings, two of which – logical positivism and legal positivism – are relevant and are also explained below.

we might seek the independence and objectivity offered by the “view from nowhere”,³⁵ but such a view may not be philosophically available. In default, the historian’s historically situated yet privileged position of *hindsight* may be as good an objective view as it can get, and the need for philosophers to respect historical understanding may be partly justified in terms of this. We have to recognise, however, that – while hindsight might be the best view we can get – it might not be what we hope for: Simon Schama comments “Historians have been overconfident about the wisdom to be gained by distance, believing it somehow confers objectivity, one of those unattainable values in which they have placed so much faith”.³⁶

Yet the need for respect for historiography is also partly a claim about the *ordinariness* of much historical understanding. Not all such understanding, necessarily: while we might learn much about the everyday from, for example, a contribution to historical demography such as E.A. Wrigley’s and others’ *English Population History from Family Reconstitution 1580-1837*,³⁷ it might be thought that there is perhaps not much “everyday” about the *understanding* involved, requiring as it does a grasp of statistical argumentation. However, Leon Pompa rightly expressed the position as follows: “historical reasoning is continuous with our everyday reasoning about matters of fact”,³⁸ so sharing the widespread historians’ view that historical method is “common sense”.³⁹ While to be “continuous with” is not, necessarily, to be “identical to”, nevertheless everyday understanding involves making judgements about situations, making decisions about what is likely to be true or what is likely to be valuable, and presenting that understanding in forms which are appropriate for effective communication. Historians do these things too. The historical world is also *our* world, and to use, for example, mathematically and statistically refined methods of

³⁵ Thomas Nagel, *The View from Nowhere*, Oxford: Oxford University Press, 1986. Here I use the word “objectivity”, but it is not a task of this book to analyse that concept. However, by the end of the book readers may feel that they have read a contribution to the debate about objectivity. For elucidation of “objectivity”, particularly as historians have understood it, see Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession*, Cambridge: Cambridge University Press, 1988.

³⁶ Simon Schama, *Citizens: A Chronicle of the French Revolution*, London: Viking, 1989, p. xiii.

³⁷ E.A. Wrigley, R.S. Davies, J.E. Oeppen, R.S. Schofield, *English Population History from Family Reconstitution 1580-1837*, Cambridge: Cambridge University Press, 1997.

³⁸ Leon Pompa, “Truth and Fact in History”, in *Substance and Form in History*, eds. L. Pompa and W.H. Dray, Edinburgh: Edinburgh University Press, 1981, pp. 171-186 at p. 182.

³⁹ G. Kitson Clark, *Guide for Research Students*, p. 9.

understanding, or to present matters in the form of tables or maps, is simply to refine our usual rough judgements and modes of communication. Parallel points can be made for other technical approaches in historiography. Both history itself and historiography are continuous with the everyday. Just because the everyday world, and so the historical world, is there for us all, it follows that history is not the preserve only of historians, that is, those who practice professionally the discipline of historiography. It further follows that the philosophy of the professional discipline of historiography, while continuous with the philosophy of the “everyday”, what might be more generally called “historical understanding”, need not be identical with that. It is controversial whether, for example, moral judgements of historical matters may be appropriately made within the historiographical discipline, although they might be highly appropriate within our everyday historical understanding.

It is relevant here to introduce, and to distinguish from our own concerns, an issue from the current philosophy of mind, that which relates to the status of “folk psychology”. Says Paul Churchland, “The facts are these: in the course of our daily affairs we display a systematic ability to explain, predict and understand the behaviour of certain animated particulars in terms of the wants, beliefs, pains, cogitations, and other psychological states and sequences to which they are presumed subject, and our facility and success in such matters is astonishing. ...Bluntly, we share a moderately detailed general understanding or *theory* of what makes people tick”.⁴⁰ There is no doubt we share this commonsense understanding, but the theory that such “folk psychology” is a *theory* – known as “theory-theory” – may be taken to imply that the terms used (belief, desire and other “propositional attitudes”), just because they enable (for example) successful prediction, refer to mental states which are causally effective (or have some other “real” status). If that were so, we would need to understand how that is possible, and that would require us to develop (among other things) a clear view of how such mental states relate to states of the brain, since it seems we do know that brain functions are causally effective in the relevant respects in the physical world. Yet the difficulties in doing that may suggest that folk psychology, if we take it to be a theory, is a bad theory. After all, we do also know (if we believe that physics is true,

at least) that many “folk” beliefs about the natural world are false: for example, following Einstein, time and space interrelate in a way which shows that our ordinary understanding is false. Maybe our ordinary “theory” of people is just as bad. But then, maybe our commonsense understanding is not a *theory* at all, but a practice or process involving an ability to *simulate* other people.⁴¹ However, whatever our commonsense understanding amounts to in terms of such philosophical issues, historiography is paradigmatically a complex and advanced application of such everyday understanding to the everyday world, conceiving that as extending into the past. Ultimately, the acceptability or otherwise of our “scientific” views can only be an acceptability *to us*, whatever our failings; our “scientific” world still has to be continuous with the everyday world, if only for the purposes of communication. Our philosophy of historiography presupposes this, and we will not consider related issues in the philosophy of mind. However, we may note in passing that any developments in philosophy, the social sciences, psychology or cognitive science which amount to an effective attack on our everyday understanding, whether “theory” or otherwise, will also amount to an attack on historiography as a discipline in its current form. Bentley rightly remarks in one of these contexts “the use of economic models and the abstractions of economic theory called into question not only the method of Dilthey but the entire genre that they regarded as history”.⁴² Later, in the section “Structuring factual synthesis”, we will mention an important role for cognitive science.

Starting our present line of philosophical argument where we did, by characterising the attitude of some historians to philosophy, is to describe some people and their beliefs, and it is to presuppose recognition that these people, like we ourselves, are historically situated. It is thereby also to link our philosophical approach to that ordinary everyday understanding which is continuous with historiographical understanding. It is to situate our philosophy in that world which philosophers share with historians. In

⁴⁰ Paul M. Churchland, *Scientific Realism and the Plasticity of Mind*, Cambridge: Cambridge University Press, 1979, pp. 91-92.

⁴¹ See, *inter alia*, Martin Davies and Tony Stone (eds.), *Folk Psychology: The Theory of Mind Debate*, Oxford: Blackwell, 1995.

⁴² Michael Bentley, *Modern Historiography*, p. 88. See also H. Koegler and K. Stueber (eds.), *Empathy and Agency: the Problem of Understanding in the Social Sciences*, Westview Press, 2000, which is an anthology about the relevance of simulation theory to the philosophy of the social sciences.

taking this approach we are not necessarily committed to that full historicity sought by Wilhelm Dilthey and his successors in phenomenology and hermeneutics,⁴³ according to whom it is a foundational metaphysical belief that the human self is in some essential way constituted by its historical situatedness, and that epistemology and other philosophical issues must presuppose this and are subordinate to it. Nor do we deny that position. In addition, the recognition here of our historical situatedness should not be taken to imply a commitment to some historical relativism, an approach commonly seen as implying scepticism. The fact – if it is a fact – that there is fundamental historical *change* commonly drives the relativist view that there is no absoluteness to truth, but our present approach involves no more at this point than recognition that we are situated in the everyday world. That world has a past, and also (we hope) a future. That there is change is obvious, but whether this is superficial or fundamental from a philosophical point of view is not a question which is here being prejudged.

There nevertheless remains a contrast between our approach here and those who see the natural sciences as foundational to our self-understanding in such a way as to make science a *rival* to philosophy and to history rather than continuous with them.⁴⁴ Students of the humanities may well regard the science-based approach as rather an old-fashioned idea, characteristic as it is of advanced seventeenth-century thinking, and many will be repelled by the thought that philosophy itself might fall a victim to this positivist dogma: with R.G. Collingwood, we need to avoid being “under the domination of methodological ideas inherited from the nineteenth century when philosophy was in various ways assimilated to the pattern of empirical science”.⁴⁵ Philosophers tempted by this thought have nevertheless to recognise that so central is historical understanding to our lives that it is highly inappropriate to see philosophers

⁴³ Wilhelm Dilthey, *Introduction to the Human Sciences* (1883) and *Formation of the Historical World in the Human Sciences* (1910); Edmund Husserl, *Lectures on the Phenomenology of Inner Time-Consciousness* (1928): “human beings could not be without a past and a future”, Robert Sokolowski, entry on “Husserl”, *The Cambridge Dictionary of Philosophy*, general editor Robert Audi, Cambridge: Cambridge University Press, 1995; Dermot Moran, *Introduction to Phenomenology*, London: Routledge, 2000.

⁴⁴ Richard Dawkins, say, known *inter alia* for *The Selfish Gene*, 30th anniversary edition, New York: Oxford University Press, 2006; or Daniel C. Dennett, known *inter alia* for *Brainstorms: Philosophical Essays on Mind and Psychology*, Hassocks: Harvester Press, 1979.

and historians as themselves rivals in the provision of foundational self-understanding. Collingwood thought historical understanding as important to his and our own time as natural science had been in and to the seventeenth century, and he sought “a *rapprochement* between philosophy and history”:⁴⁶ “The chief business of twentieth-century philosophy is to reckon with twentieth-century history”.⁴⁷ (We will not comment on this wide-ranging selection of which centuries are relevant to this issue; and this will become clearer as, later, we present historiography’s own history.) But even if we do not see historical understanding as in any way importantly foundational, it remains the case that many of the philosophical problems which historiography generates are – like the philosophical problems generated by ordinary life – not easily located within our current philosophical understanding, and philosophy itself will benefit from widening its range of perceived difficulties.

⁴⁵ R.G. Collingwood, *An Essay on Philosophical Method*, Oxford: Clarendon Press, 1933, p. 7. Seventeenth or nineteenth century? Both.

⁴⁶ R.G. Collingwood, *An Autobiography*, Oxford: Oxford University Press, 1939, p. 77.

⁴⁷ *Op. cit.*, p. 79.

Modelling a discipline: the truth of historical theory

It is, then, for a range of reasons, right to require philosophers to “respect” historians and historical work: philosophy should take historiography seriously. But what is it for philosophy to take historiography “seriously” without begging major questions, and in particular major issues in epistemology? Despite the view of some historians described earlier, who think that philosophers typically impose their theories on historiography, a number of philosophers have been very explicit in their respectful use of historiographical material.⁴⁸ It is not, however, sufficient simply to adopt such “respect” as a general approach. What matters is how that is best to be done, and metaphilosophical argument concerning just how the philosophy of the subject should be undertaken needs to be provided beyond the cursory. Here, in contrast with other theories of historiography, we will provide argument concerning this. To begin with, taking historiography “seriously” involves satisfactorily answering two questions: first, what makes a philosophy a philosophy of *historiography*; second, what makes a philosophy of historiography “true” of historiography.

The answer to the first question – what makes a philosophy a philosophy of historiography? – is that philosophy is a philosophy of historiography in so far as it tries to “model” historiography in a philosophical way: it is philosophical puzzlement *about historiography*, puzzlement which we try to remove by producing a model of historiography. To speak of a “model” here is not, in this first characterisation, to speak of something with technical features; we need not think of it at this stage as being any more than a linguistic entity which says something “true” about the matter being modelled. It is apparent, however, that answering the question in this simple way does not take us very far. Before we can set about modelling historiography in a philosophical way, we have to understand what it is to create such a model, and to do this we have to understand what it would be for our model to be “successful”, and this requires answering our second question, what makes a philosophy of historiography “true” of historiography. What makes a philosophy of historiography “true” of historiography lies in the nature of the relationship between historiography and its

⁴⁸ W.H. Dray, in a range of books from 1957, stands out here.

purported philosophical “model”, and that is the main problem to be elucidated next. Understanding that will inform our “modelling” procedure in the philosophy of historiography.

Much of what philosophers – and indeed historians – have said about historiography has addressed the question whether historiography has the same epistemological status as science. We will not address this old question here:⁴⁹ since we have first to answer the question *what it is* for a model to be “true” of historiography, dealing with the different question which model, if any, is the “true” model of historiography has been postponed, and with that have been postponed two further matters: first, the second-order⁵⁰ or metaphilosophical question whether our model should be an *epistemological* model at all, the answer to which strongly influences whether the associated question “is historiography a science?” is worth asking; second, and on the assumption that this question is worth asking, the first-order philosophical question whether historiography is a science. This last will prove to be of little relevance, and in effect will be bypassed by our main line of argument, so ending the agenda set by that old question.

Nevertheless, while these issues concerning whether historiography is a science are not our immediate concern, the problem of the nature of the relationship of “truth” between historiography and its purported philosophical “model” can indeed be understood, albeit indirectly, in terms of the relationship between science and historiography, in the following way: we can use the different ways in which different models of science relate to science “itself”, and the different ways in which different models of science have been related to historiography, to *illustrate* the ways in which different models of historiography can in principle relate to historiography “itself”, enabling us to tease out a greater understanding of this latter relationship. We don’t

⁴⁹ See J.B. Bury, “The science of history” [1902], reprinted in Fritz Stern (ed.), *The Varieties of History: From Voltaire to the Present*, Cleveland: Meridian, 1956, pp. 210-223 and G.M. Trevelyan, “Clio, a muse” [1903], reprinted in shortened form in Fritz Stern (ed.), *The Varieties of History*, pp. 227-245.

⁵⁰ “First-order” and “second-order” are terms which suggest R.G. Collingwood’s “scale of forms” and “scale of philosophies”, but there is no commitment to that here. As here used they are relative to the context of argument. However, see R.G. Collingwood, *An Essay on Philosophical Method*, pp. 189, 194 and *passim*. See also W.V.O. Quine on “semantic ascent”, *Word and Object*, New York: John Wiley and Sons, 1960, p. 271 and Dallas Willard, “Why semantic ascent fails”, *Metaphilosophy* 14, 1983, pp. 276-290.

need to know here what it “really is” to be a science, and we don’t need to know here whether historiography is “scientific”. Whether one or other such models of historiography – “scientific” or not, and indeed epistemological or not – is actually “true” of historiography is, as we have explained, not the central issue at this point. What matters is what that “truth” can or should be understood to consist in.

How the “truth” of a model is to be understood is best illustrated, not by selecting the latest and best philosophies of science (for determining the nature of science is not a relevant concern), but by selecting the appropriate work of Karl Popper, Carl Hempel and Thomas Kuhn,⁵¹ for the issue between these philosophers of science of an earlier generation more clearly demonstrates than later work some main epistemological ways in which a model can relate to that discipline of which it is a model. After a new metaphilosophical analysis of their positions, we will be able to deal in an exact way with the problem of the place of historiography in modelling science, which will play a central role at a later stage of the argument. Self-reflectively, in presenting Popper’s, Hempel’s and Kuhn’s positions in the new analysis we are acting as historians in so far as we are to some extent engaging in the *historiography of the philosophy of science*, and we will be able to generalise to wider historiography from some of the issues raised in that engagement. A further reason for referring to these philosophers’ works is that, in fact, much of twentieth-century argument in the philosophy of historiography has been within the philosophical agendas which they set, and those agendas continue into the twenty-first century.

Kuhn was revolutionary. Says Gary Gutting like many others (while reviewing John Zammito’s *A Nice Derangement of Epistemes*), “it is by now a banality that Kuhn’s work on scientific revolutions itself precipitated an intellectual revolution. ...But”, he continues, “the question remains of just where, if anywhere, Kuhn’s effect was deep enough to be judged truly revolutionary. Certainly not in the history of science, where Kuhn himself purported to be offering nothing that historians had not known and been

⁵¹ And, later, Imre Lakatos.

practicing since Koyré”.⁵² Gutting’s answer is that the revolution was in philosophy of science itself, and so successful that it has since marginalised Kuhn’s own work. Here too – despite Zammito’s important interest – historians will long since have got used to the Kuhnian intellectual “impact”, which they may well see as a mid-twentieth-century recommendation that other disciplines move in their direction.

Here our preparedness to defer to historians must be put in abeyance for a while, for we shall be arguing that the philosophy of historiography needs to be undertaken on the basis of a historical approach, which some historians may think too obvious a conclusion to need argument. In practical terms, they would be wrong to think so. Apart from the philosophical issues, historians themselves, as we will see in Chapter 3, and perhaps surprisingly, do not characteristically present their understanding of their own discipline in historical terms. In any event, argument is necessary in order to avoid a merely arbitrary selection of one rather than another approach to modelling a discipline, while the details of the argument will give us exact conclusions which can themselves be used as a sound foundation for the rest of the argument. The conclusions of the present chapter are summarised at the beginning of Chapter 3.

We begin with Popper’s and Hempel’s model of science. Their philosophical attitudes were influenced by the empiricism of the Vienna Circle, a group organised by Moritz Schlick which met from the 1920s until the late 1930s, and which expressed philosophical beliefs derived from the Austrian physicist and philosopher Ernst Mach. On Mach’s view, our best scientific theories are justifiable only by reference to our sensations, and are acceptable only in so far as they continue to apply successfully to the world. Mach’s position was an extension of Hume’s empiricism: everything we know must be derived from our experiences. This approach involves attempting to build our entire understanding using only the building blocks of the immediately perceived data of the senses. Mach’s – and so the Vienna Circle’s – position was not only atomistically empiricist in this way but also strongly positivist. They believed that

⁵² Gary Gutting, “Zammito and the Kuhnian revolution” [a review of John Zammito, *A Nice Derangement of Epistemes: Post-positivism in the Study of Science from Quine to Latour*, Chicago: University of Chicago Press, 2004], *History and Theory* 46, 2007, pp. 252-263 at p. 253.

science alone provides proper knowledge, and that it does so just because it is proved by the empirical evidence.⁵³

Hempel and Popper share the view that a scientific theory is essentially a collection of scientific laws which are typically causal in nature.⁵⁴ These laws are warranted on the basis of experience. As such, they face the so-called “problem of induction”: since the laws cover all possible times, and since we cannot experience all possible times, they go beyond what experience can warrant. Popper pointed out that these universal generalisations can, however, in principle be falsified by experience. It is this capacity for falsifiability which lies at the heart of the Popperian criterion for science. This is not merely a technical point but one which expresses the essence of scientific method, for scientific method (as Popper understands it) consists in removing what may be false laws from the corpus of existing scientific beliefs by seeking out falsifications of them, that is, by testing them. Those beliefs are justified which cannot show to be false. It is the critical attitude expressed in the practice of testing received beliefs which best characterises the scientific approach. Scientific laws, while not proved to the point of necessity, are understood to be well confirmed beyond practical doubt by the empirical evidence.

A corollary of Popper’s position is that “testing” claims is something which we rationally *ought* to do.⁵⁵ Popper’s philosophy of science is *ethical* as well as epistemological: we *ought* not to accept beliefs – and for that matter institutions – dogmatically. Dogma is a kind of moral error. There is for Popper (and indeed for many others) a perceived congruence in terms of self-criticism between a properly

⁵³ This and the next half-dozen paragraphs express the “standard” account in terms which I have also used in my “From history to justice: understanding philosophy of history”, in Aleksandar Jokić (ed.), *Essays in Honor of Burleigh Wilkins: From History to Justice*, New York: Peter Lang, 2001, pp. 19-69.

⁵⁴ It was David Hume who clarified the relationship between causation and generalisations. See his *Treatise of Human Nature* [1739], ed. L.A. Selby-Bigge, Oxford: Clarendon Press, 1888, p. 87 and *passim*.

⁵⁵ It is plain that Popper understands this as what Kant would have called a “categorical” imperative, rather than a “hypothetical” one. Popper is not merely offering us a way of being scientifically rational, on the off-chance that some of us might have an arbitrary taste for that pursuit. However, whether this position is defensible, given the rest of Popper’s epistemology, is doubtful. See Burleigh Taylor Wilkins, *Has History Any Meaning? A Critique of Popper’s Philosophy of History*, Hassocks, Sussex: The Harvester Press, 1978, pp. 219-239.

structured scientific community and a democracy or “open society”.⁵⁶ To get things scientifically right is also to get things politically right. Popper thus objected to “historicism”, taking that to be a belief in a pattern of historical development such that prediction of the historical future is possible, together with a belief that the historical future is a moral improvement over the present.⁵⁷ He dedicated his 1957 book *The Poverty of Historicism* to the “memory of the countless men and women of all creeds or nations or races who fell victims to the fascist and communist belief in Inexorable Laws of Historical Destiny”,⁵⁸ so-called “laws” wholly unwarranted by a proper understanding of science.⁵⁹

Hempel, by contrast, was not at all overtly political in his philosophy and his conception of science is presented by him as essentially epistemological and without any explicit ethical implications. While sharing with Popper an understanding of the nature of science,⁶⁰ Hempel’s approach makes more explicit than Popper the Humean view that causation is to be understood in terms of generalisations. Hempel developed the philosophy of science by analysing scientific *explanation*, and it is this which, given positivism, was widely applied to other subjects. In particular it was applied to

⁵⁶ The allusion is to Karl R. Popper, *The Open Society and its Enemies*, vol. 1, *Plato*, and vol. 2, *Hegel and Marx*, London: Routledge and Kegan Paul, 1945. See also Burleigh Taylor Wilkins, *Has History Any Meaning?*, pp. 218-219.

⁵⁷ Robert D’Amico distinguishes two senses of “historicism”: first, that “history obeys a lawful order or logic and knowing its ‘laws of emergence’ allows for historical predictions”; second, that “historicism emphasizes that ways of reasoning are entrenched or embedded in contexts that can be judged either internally or retrospectively from the present. Rationality is inseparable from and judged internally to changing problem situations”. While Popper rejects historicism in the first sense, he is a historicist in the second sense. Robert D’Amico, *Historicism and Knowledge*, New York and London: Routledge, 1989, pp. 20-21 and *passim*. See also Arnaldo Momigliano, “Historicism in Contemporary Thought” [1961], reprinted in his *Studies in Historiography*, London: Weidenfeld and Nicolson, 1966, pp. 221-238.

⁵⁸ Karl R. Popper, *The Poverty of Historicism*, London: Routledge and Kegan Paul, 1957. The historian Maurice Cowling suggests that this quoted claim is simply a wholly inadequate explanation of a historical problem. Maurice Cowling, *The Nature and Limits of Political Science*, Cambridge: Cambridge University Press, 1963, p. 117.

⁵⁹ An associated rejection of historicism is provided by Isaiah Berlin, primarily on the ground that it embodies a single comprehensive moral point of view, which offends against Berlin’s moral pluralism. Burleigh Taylor Wilkins observes that Popper’s attacks on historicism “clearly anticipate” Berlin’s position: Burleigh Taylor Wilkins, *Has History Any Meaning?*, p. 14. Much of Berlin’s philosophy is usefully presented in a collection of his best-known essays: Henry Hardy and Roger Hausheer (eds.), *The Proper Study of Mankind*, London: Chatto & Windus, 1997.

⁶⁰ See also Carl G. Hempel, *Philosophy of Natural Science*, Englewood Cliffs, N.J.: Prentice-Hall, 1966.

historiography in Hempel's 1942 article "The Function of General Laws in History".⁶¹ Hempel shares with Popper the view that scientific explanations, which are essentially causal explanations, can be analysed in terms of the place within them of scientific laws. This analysis, in the light of its background Humean assumptions, and together with its later elaborations, the various responses to them and its consequential modifications, define what came to be seen as the standard tradition in philosophy of science.⁶² In a similar way discussion of the application of the associated analysis of explanation to historical understanding came to define a particular tradition in the philosophy of historiography.

Hempel's article generated for decades a debate, initially and most importantly with W.H. Dray,⁶³ about the application of philosophy of science to historical explanation. By 1959 Hempel's work had "attained the status of a kind of classic in the field", Patrick Gardiner noted.⁶⁴ Dray introduced widely used terminology in describing this analysis of explanation as the "covering law model of explanation".⁶⁵ The importance and influence of the covering law model was sufficient for some to worry about who thought of it first, Hempel or Popper. Hempel was the first of the two to publish, in the 1942 article, an explicit application to historical explanation of their shared empiricist philosophy of science. However, Burleigh Taylor Wilkins observes that "the history of the covering law model remains unwritten, but while it is present in Hume and J.S. Mill and even, as I have argued elsewhere, in a passage in Aristotle's *Posterior Analytics*, its importance to contemporary philosophy of history derives from the treatment and refinement it has received first from Popper in *The Logic of*

⁶¹ Hempel, Carl G., "The function of general laws in history", *Journal of Philosophy* 39, 1942, pp. 35-48. (Reprinted in *Theories of History*, ed. Patrick Gardiner, New York: The Free Press, 1959, pp. 344-356 and also in *Readings in Philosophical Analysis* eds. H. Feigl and W. Sellars, New York: Appleton-Century-Crofts, 1949, pp. 459-471.)

⁶² While the tradition continues, some will deny that it is any longer the "standard" position, after the publication of Thomas S. Kuhn's *The Structure of Scientific Revolutions*, Chicago: The University of Chicago Press, 1962. A source of reading in the standard approach to philosophy of science is Carl G. Hempel, *Philosophy of Natural Science*, Englewood Cliffs, N.J.: Prentice-Hall, 1966. Another is Ernest Nagel, *The Structure of Science*, London: Routledge and Kegan Paul, 1961.

⁶³ W.H. Dray, *Laws and Explanation in History*, Oxford: Clarendon Press, 1957.

⁶⁴ "Recent views concerning historical knowledge and explanation: Introduction", in Patrick Gardiner (ed.) *Theories of History*, pp. 265-274 at p. 269.

⁶⁵ W.H. Dray, *Laws and Explanation in History*, p. 1.

Scientific Discovery and later from Hempel in several articles”.⁶⁶ Characterising both Popper and Hempel, Dray rightly says that Hempel’s formulation “is more rigorous than Popper’s”,⁶⁷ and it is this degree of philosophical rigour which warrants the historical judgement that (against Wilkins) Hempel thought of the model – as we in the theory of historiography usually understand that model, with all the exactness which Hempel gave it – first. In passing, we may observe the way in which what purports to be a historical “fact” – because it purports to be truthfully answering the question “who thought of it first?” – is a *judgement* involving an *evaluation*; moreover, the criterion for the evaluation here is a philosophical one.

In his 1942 article Hempel asserts that the explanation of an event of some specific kind *E* has to have the following valid deductive-nomological form:

Whenever $C_1...C_n$ then *E*;

$C_1...C_n$;

Therefore, *E*.

Further, “the assertion that a set of events – say, of the kinds C_1, C_2, \dots, C_n – have caused the event to be explained, amounts to the statement that, according to certain general laws, a set of events of the kinds mentioned is regularly accompanied by an event of kind *E*”.⁶⁸ Each of the statements in the deductive-nomological argument must be empirically warranted, and this is understood in Popperian terms.

Hence Hempel’s view is that indicating causation by this means is both necessary and sufficient for explanation. This rules out alternatives: an example is the attempt to explain human action or indeed the world in general by reference to its “purpose”, or “final cause”, to use Aristotle’s term.⁶⁹ It also follows that there is no way of stating the cause of something other than by displaying a general law in the required way. In summary, the Popper-Hempel deductive-nomological model provides for us an analysis of scientific testing, prediction, explanations, causes and the place of general laws in

⁶⁶ Burleigh Taylor Wilkins, *Has History Any Meaning?*, p. 13. See also Burleigh Taylor Wilkins, “Aristotle on Scientific Explanation”, *Dialogue* IX, 1970, pp. 337-355.

⁶⁷ W.H. Dray, *Laws and Explanation in History*, p. 3.

⁶⁸ C.G. Hempel, “The function of general laws in history”, in Gardiner (ed.), *Theories of History*, p. 345.

⁶⁹ What we understand from Hempel to be the cause of something would be Aristotle’s “efficient” cause.

our understanding which structures all these in terms of the same logical model. Moreover the model is not merely an analysis of those terms in science but an analysis of those terms wherever they are used, so it is – as positivism requires – completely general in its application.

Historians, it is plausibly supposed by Popper and Hempel, seek to provide knowledge and understanding about the past,⁷⁰ and we are then required by the positivist approach to grasp how this historiographical project rationally ought to be attempted in the light of the scientific model which, on the positivist assumption, sets the standard for all knowledge and understanding. It follows from the positivist assumption that anything that does not match the scientific model of knowledge and understanding fails to provide knowledge or understanding at all. Historiography is seen by Hempel to fail the test just because its modes of explanation do not meet the standard: the best historians can do, according to Hempel, is to provide “sketches” of explanations.⁷¹

Dray criticises this, and the essence of his objection is that, when in historiography “we ask for the explanation of an action, what we very often want is a reconstruction of the agent’s calculation of means to be adopted toward his chosen end in the light of the circumstances in which he found himself”.⁷² Dray calls such explanations in historiography “rational explanations”: “an explanation which displays the *rationale* of what was done”.⁷³ Such an explanation does not, in Dray’s view, subsume what is to be explained under a general law. This debate between Hempel and Dray has a simple logic which sets a clear agenda in our present context.

Hempel says that explanation in historiography, like explanation in science, is only achieved by subsuming what is to be explained under a general law. Dray can defeat this position if he can show that at least one kind of explanation (in fact, he claims,

⁷⁰ Although the argument does not here depend on a correct characterisation of what it is that historians seek to provide knowledge and understanding about. In addition, it remains open whether Hempel, Popper and others are right to think that the central philosophical issues of historiography are *epistemological* issues, as here implied.

⁷¹ C.G. Hempel, “The function of general laws in history”, in Gardiner (ed.), *Theories of History*, p. 351.

⁷² W.H. Dray, *Laws and Explanation in History*, p. 122.

⁷³ *Op. cit.*, p. 124.

“rational explanation”) is successful – in other words, is a proper explanation – which does not subsume the matter explained under a general law. Dray in attempting to show this expresses an additional claim (which he does not need, so far as defeating Hempel is concerned), namely that “rational explanation” is characteristic of much historical understanding.⁷⁴ If Dray is right in his two claims then Hempel is wrong, but is shown to be wrong only in the *universality* of his claim: Hempel’s model may yet be correct for science, but (1) not correct for everything, and (2) in particular not correct for historiography. Dray is mainly concerned to make philosophical sense of historical understanding, and, if it were not for the universality of Hempel’s claim, could well regard the content of that claim as not worth considering. It is its *universality* which makes Hempel’s claim problematic for Dray: separable from the subordinate details of Hempel’s model of science is the universality of his core positivist assumption that science is *the only* way of achieving knowledge (and with knowledge, sound explanation), and it is this which drives the application of Hempel’s model to all disciplines including historiography.

After Hempel, a number of philosophers of historiography sought to analyse historical explanation or understanding as *sui generis*, and Dray is plausibly to be seen as among these. However, the more successfully these philosophers distanced historiography from the scientific model by, for example, distinguishing “historical understanding” from “causal explanation” by analysing “rational explanation” or “empathetic understanding”, or by analysing another important feature claimed as distinctive of or essential to historical writing, narrative, the more open historians were to the positivists’ accusation that their discipline was politically charged literature rather than a contribution to knowledge, because the general effect of any success on the part of their arguments was to deny that historiography met the universal standard for knowledge and explanation offered by the Popper-Hempel model. When Alan Donagan, following an analysis of historical examples, said, “the facts I consider concern what historians actually recognize as explanations”,⁷⁵ and concluded “the Hempelian theory, therefore, contradicts at least some good historians’ opinions about

⁷⁴ It should be apparent that the issue whether Dray is right in this characterisation of historical understanding is not relevant to the present argument.

their explanations”,⁷⁶ he said something which was intended as anti-Hempel but in fact cut both ways.

⁷⁵ Alan Donagan, “Explanation in history”, reprinted in Gardiner (ed.), *Theories of History*, pp. 428-443 at p. 430.

⁷⁶ *Op. cit.*, p. 433.

Description and prescription

As earlier explained, our present concern is not with whether any particular philosophical model is true of science or of historiography, but rather with what it would be for such a model to be “successful”. We need to understand what the “truth” of a model can or should be understood to consist in, so, in due course, what makes a philosophy of historiography “true” of historiography. For the sake of the argument, assume that the Popper-Hempel model of science is right as they present it, so that it is correct in both its content and its universality. Given this, we may stress two things: first, that this model of science truly represents or correctly *describes* in some summary form actual scientific thought and/or practice (the model is thus judged right in its descriptive *content*); second, that the model of science sets a *standard* – moreover, it is claimed, the *only* standard – for correct thought and/or practice (the model is thus judged right in its prescriptive *universality*). As to what “makes” the model true, to claim that the model is “true” in these two ways is to claim that both description and prescription are appropriately *justified*.

Just because, but also in so far as, science successfully achieves knowledge and understanding, the Popper-Hempel model of science (for the present assumed to be correct) truly describes science while at the same time setting a standard for knowledge and understanding which science itself achieves and which any discipline aspiring to provide knowledge and understanding has also to achieve. We can see here that the relationship of “truth” between science and its model is not a matter merely of the model’s being justified as descriptively true of science but also of the model’s being justified as “true” of an *ideal*, an ideal for *all possible* knowledge and understanding, an ideal which also happens (so the claim goes) to be instantiated in actual scientific thought and/or practice.

Using Popper’s and Hempel’s approaches for illustration, we have now found that one answer to the question of the nature of the relationship of “truth” between science and its model involves the model’s being “true” of – in effect, *expressing* – a universally claimed ideal standard for knowledge and understanding, a standard which is also claimed to be instantiated by scientific practice so that the model is also descriptively

true of science. There is here what might be seen as a double-sided conception of the truth involved: one side involving a correspondence to the facts of scientific practice and the other side involving a correspondence to an ideal of scientific practice. We may also note at this point the kind of ground on which some main objections to the Popper-Hempel position were made: with Donagan, that the position contradicts “at least some good historians’ opinions about their explanations”;⁷⁷ with Dray, that “what we very often want”⁷⁸ is something different from what Popper and Hempel prescribe.

To make this matter clearer, we need to introduce some material from the philosophy of law, for there we find some three hundred years of arguments which are exactly to the present point. The problem philosophers of law face is to make appropriate philosophical sense of law, just as we seek here to make appropriate philosophical sense of historiography and have been illustrating our argument with works which attempt to make appropriate philosophical sense of science. At this stage we will introduce just one side of that jurisprudential debate; by the end of the forthcoming section on “Justification in the second-order context: Kuhn” we will have argued our way to the other side. In 1832 John Austin, writing in an empiricist tradition derived from David Hume, expressed what came to be called a “legal positivist” position,⁷⁹ distinguishing in a famous passage between two questions: first, what is law? Second, what ought law to be? “The existence of law is one thing; its merit or demerit is another. Whether it be or be not is one enquiry; whether it be or be not conformable to an assumed standard, is a different enquiry. A law, which actually exists, is a law, though we happen to dislike it, or though it vary from the text, by which we regulate our approbation and disapprobation. This truth, when formally announced as an abstract proposition, is so simple and glaring that it seems idle to insist upon it”.⁸⁰

⁷⁷ *Ibid.*

⁷⁸ W.H. Dray, *Laws and Explanation in History*, p. 122.

⁷⁹ See – for continuing clarity – H.L.A. Hart, “Positivism and the separation of law and morals”, reprinted in F.A. Olafson (ed.), *Society, Law, and Morality: Readings in Social Philosophy from Classical and Contemporary Sources*, Englewood Cliffs, N.J.: Prentice-Hall, 1961. To reinforce that clarity, note that the “positivism” in Hart’s title is *legal* positivism and not *logical* positivism.

⁸⁰ John Austin, *The Province of Jurisprudence Determined* [1832], ed. Wilfrid E. Rumble, Cambridge: Cambridge University Press, 1995, Lecture V, p. 157. Again following Hume, Austin saw taste, that is, desire, as foundational to our understanding of evaluative rather than factual

This exactly maps the position in philosophy of science which it seems we have now reached, and we shall work for the present with the assumption of this distinction: what science is, is one enquiry; what science ought to be, is a different enquiry. On this assumption, the Popper-Hempel model of science, if true, and with the double-sided conception of the truth involved now apparent, provides the solution to both enquiries, for it truly describes what science is and – in so far as it presents “ideal” science – truly says also what science ought to be.

We need to keep in mind this assumed “simple and glaring” distinction as we look at a major development in the philosophy of science. While a rump of the logical positivism which informed Hempel’s approach still remains, the understanding of philosophy of science was revolutionised by the publication in 1962 of Kuhn’s *The Structure of Scientific Revolutions*. Kuhn, primarily a historian rather than a philosopher of science of the “standard” Popper-Hempel kind, noted that historical research revealed that science in the past had not operated in ways which accorded with the requirements of the standard model. Kuhn suggested that scientists normally operated against a vague and presupposed general understanding of the world or “paradigm” which they sought to articulate with explicit clarity.⁸¹ The historiography of science, according to Kuhn, disclosed many scientific paradigms. Paradigms did not persist but went through a period of rise and fall and replacement by other paradigms. When a paradigm collapsed it did so not for the logical reasons suggested by the logical positivists but for a variety of pragmatic – such as social or psychological, even aesthetic⁸² – reasons. The historiography of science disclosed a pattern of revolutionary changes between periods of normal science.

We have noted the claim that the Hempelian model of science truly describes science and, because and in so far as science successfully achieves knowledge and

matters, and assumed Utilitarianism as the appropriate criterion in answering the question what law ought to be.

⁸¹ Kuhn did not himself articulate the notion of a “paradigm” with much clarity. Margaret Masterman claimed that twenty-one different senses of the word “paradigm” appeared in Kuhn’s book: Margaret Masterman, “The Nature of a Paradigm”, in Imre Lakatos and Alan Musgrave (eds.) *Criticism and the Growth of Knowledge*, London: Cambridge University Press, 1970, pp. 59-89. Kuhn himself was not always the best judge of what he had said really meant.

understanding, at the same time this model sets a universal standard for knowledge and understanding which science itself achieves. It is then this model which is – truthfully? arrogantly? – imposed on historiography. But, given Kuhn’s position, this approach cannot be correct. Science – conceived now as the real judgements of real scientists in the real world as disclosed by the historiography of science – does not fit the Hempelian model of science.⁸³ The model of science does not truly describe science. Positivism,⁸⁴ however, holds that “science” is the only way of achieving knowledge. Kuhn’s position was not a straightforward refutation of positivism, for positivists could still hold that science was the only way of achieving knowledge, but they had – if Kuhn was right – to clarify what they meant by this: in saying that “science” is the only way of achieving knowledge, should they mean science as expressed by their ideal model, or science as it is actually practised? Kuhn’s historiography of science suggested that the positivist “ideal” was one thing and scientific practice another. Positivist philosophers of science therefore faced a dilemma: given a conflict between science as it actually was and their own deductive-nomological model of science, which should be taken as pre-eminent? While the deductive-nomological model of science was, given Kuhn, a false *description* of what scientists actually did, was it nevertheless still a good *prescription* for what scientists (and historians) ought to do? Kuhn’s historiography of science thus had an ambiguous impact on the Popper-Hempel position. Should we change the model to fit science as historically described, or change scientific practice to fit the model?

As a matter of elementary logic, this choice could go either way. If we are merely concerned to describe the facts it may well seem obvious that we have to change our model of science to fit the historical facts about science. Conversely if our model sets a standard which science ought to achieve then plainly science must change its practices as necessary. Imagining that our model both *describes* and *prescribes*, as in effect Hempel and Popper were doing, seems to require that conflicting aims be simultaneously met, and this yields only confusion. This is because we are, following

⁸² “Aesthetic” here includes the idea of simplicity. See, importantly, Elliott Sober, *Simplicity*, Oxford: Oxford University Press, 1975.

⁸³ “Failures of verisimilitude”, as Kuhn put it. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd edn., enlarged, Chicago: The University of Chicago Press, 1970, p. v.

⁸⁴ Other than legal positivism.

the distinction in John Austin's philosophy of law, trying to answer two different questions at the same time. There is nothing wrong in principle with trying to do this, for in principle we might succeed, but when we try and *fail* to satisfy both aims together,⁸⁵ as it seems Hempel and Popper have failed (if Kuhn is right), then we face a conflict where we are forced to choose between aims, between describing and prescribing; worse, we are given no obvious resources to help us decide. The best way of dealing with decisions made in such ignorance is usually to find some default position: in a criminal trial, for example, if we cannot tell if a person is innocent or guilty then the default position is that they are innocent. But where does the burden of proof lie in stark philosophical choices like whether a model should be *describing* or *prescribing* in the present context?

Stark philosophical choices like this are often signs of a still deeper confusion, and this problematic matter needs to be carefully unpacked in its simplest form. The choice we face needs to be clarified. As it is, at this stage, an unnecessary complexity which is, as will shortly become apparent, best isolated for later argument, let us first remove from the Popper-Hempel model the positivist claim itself, the idea that science – however we think of “science” – is the *only* way of achieving knowledge. We are then left with two opposed positions: (1) that the standard model of science represents the way of doing science or being scientific (representing the Popper-Hempel position), and (2) that science as historically described represents the way of doing science or being scientific (representing Kuhn's position). It might now seem that the Popper-Hempel approach (1) is an attempt to answer the *prescriptive* question and Kuhn's (2) is an attempt to answer the *descriptive* question. But that cannot be correct because Kuhn's approach, in “revolutionising” philosophy of science as it did, is *opposed* to the Popper-Hempel position; *that* we take for granted in our present argument. Popper-Hempel and Kuhn can only be opposed if they offer rival answers to the same question. Given that they are opposed, only one of these two models can be right. (They might both be wrong, but, since we are using these two philosophies of science only for the *illustration* of discipline-modelling, we will ignore alternative positions. Contemporary philosophies of science do not in fact display different approaches to

⁸⁵ Note that one can fail “both aims together” while succeeding in one of the aims, just as a

philosophical modelling of a kind that will affect our argument.) For the sake of the ongoing argument we shall assume that it is Kuhn who is right and Popper-Hempel who are wrong:⁸⁶ scientific practice in fact did not and does not fit the Popper-Hempel model.

It might be thought that in taking for granted Kuhn's claim to *know* this fact we are begging the question whether Kuhn's claim has an appropriate "scientific" basis, as if the situation were that, when the bases of knowledge are themselves called into question, we cannot build an argument by simply assuming that Kuhn's historiographical knowledge is properly founded. However, we can legitimately assume the truth of Kuhn's position here because, in making explicit the assumptions of our argument, we have *removed* the positivist claim. When we "call into question the bases of knowledge" we are in fact, at present, merely observing the calling into question only of the bases of *scientific* knowledge and, without positivism, what may be non-scientific historiographical knowledge is not necessarily at risk. Kuhn's historiography is not therefore itself subject to epistemological checking against "science" in any relevant sense of that word, and in particular it is not itself subject to epistemological checking against any model of science which is at issue. We can assume the truth of Kuhn's claim without generating inconsistency in the present argument, given that the positivist claim has been removed.

Given that the two positions offered by Popper-Hempel and by Kuhn are opposed, then – given John Austin's distinction – they must be opposed either in their *descriptions* or in their *prescriptions* or in both. Conceived as being opposed as two *descriptions*, the opposition is clear: the Popper-Hempel model is (so the claim goes) instantiated in *actual* scientific thought and/or practice; while we have also for the sake of the argument accepted Kuhn's opposing claim that scientific practice in fact does not fit the Popper-Hempel model of science. "Not fitting the model" is,

conjunction is false if only one of the conjuncts is false.

⁸⁶ Gary Gutting says correctly, "The most direct response to Kuhn would have been to argue against his critique on historical grounds: to show that the history of the best science did fit the accumulationist model. But to this extent at least, Kuhn – following the work of Koyré and other founders of the new historiography of science – had the history right." "Zammito and the Kuhnian revolution", p. 256.

however, ambiguous: it might mean that Kuhn's historiography shows that scientists did not do what, according to the Popper-Hempel model *conceived as an ideal or standard*, they ought to do. But it should be understood by now that this is not a relevant reading. Given Austin's distinction, with opposition between Popper-Hempel and Kuhn being here first understood to occur only at the level of description, "not fitting the model" has to be interpreted in its descriptive and not prescriptive form. In effect, Popper-Hempel and Kuhn, at the descriptive level of their opposition, need to be seen as disagreeing about the *history of science*, since that is the source of those historiographically discovered "facts" about science which are themselves the basis of what is here supposed to be their descriptive disagreement.

Notice that the "history of science" here is not intended to mean "past" as opposed to "present" science. We have already argued that "our" world and the "historical" world are continuous with each other: they are the same world. A further point is this: when Kuhn says that, in order to understand science, we need to turn to the history of science, it is not appropriate to ask why we need to engage in the *historiography* of science as if the historiography of something were a better way of understanding it by contrast with, say, sociology or psychology or anthropology or economics.⁸⁷ We can at the present stage of the argument assume that these many approaches to understanding human actions and affairs are all available for use by historians as appropriate to the historiographical tasks they set themselves. We will leave open those questions which seek to elucidate contrasts and comparisons between historiography and (other) such modes of understanding.

Given that Popper-Hempel and Kuhn disagree about science at this descriptive level, we might, at least speculatively, interpret Kuhn as writing a historiography of science which is entirely to be understood as answering the descriptive question, and as having no implications whatever for the prescriptive question. That is, we might, at least speculatively, think that there is no opposition whatever between Popper-Hempel and Kuhn with regard to the prescriptive question just because and in so far as, while Popper-Hempel offer such an answer, Kuhn offers none. The speculation is allowable

because, following Austin, these are entirely different questions. But this suggestion is not plausible. Kuhn did not revolutionise philosophy of science by ignoring the prescriptive question entirely while merely offering a descriptive update for those who had what might be no more than an antiquarian interest in the history of science. The opposition between Popper-Hempel and Kuhn is more than an opposition between the merely descriptive: they offer different *prescriptions* for science. Each expresses a view as to the *proper* way of doing science or being scientific.

Their opposition as prescriptions needs clarification.⁸⁷ First we need to address the idea of “opposition” in this context. Why could not the Popper-Hempel model of science be one proper representation of how science ought to be done, and science as historically described by Kuhn be another, such that both are available to us? Why insist on a single standard? If we think of the model of science as being merely a model of *explanation* rather than of unified scientific knowledge, then this pluralism of standards is indeed a possibility. There are many ways of explaining things: we might give reasons or we might give causes or we might tell stories, and we need not think of these as being alternatives between which we have to choose such that only one has to cover all possible explanations, despite Hempel’s 1942 article seeking to persuade us of precisely this. Much more plausibly, for explanation, some modes of explanation suit some circumstances and other modes of explanation suit others: the most we would normally think we have to do is to decide which mode is appropriate for which circumstance. But even that may well be too limiting, for there is no obvious reason why we cannot use different modes for the same circumstance: it is arguable, for example, that we might rightly say, with Dray, and with regard to explaining human action, that “we give reasons if we can, and turn to empirical laws if we must”.⁸⁹ We can sense Dray’s distaste for the latter, but either mode of explanation will nevertheless suit for human action. There might be *many* “true” models setting standards of explanation.

⁸⁷ This is consistent with the approach of the *Annales* school of (mainly French) historians; it is not, however, an approach to which our ongoing argument is committed.

⁸⁸ The difficulties about to be clarified could, with a little ingenuity, be raised also for the *descriptive* opposition, but it would be an unnecessary duplication.

⁸⁹ W.H. Dray, *Laws and Explanation in History*, p. 138.

However, this pluralist approach fails to make sense of the prescriptive opposition between Popper-Hempel and Kuhn. Theirs is an opposition which has to do with knowledge itself rather than explanation, linked though those are. Yet while there are obvious pluralist risks with respect to explanation, maybe knowledge itself is to be pluralistically understood. There might for example be one model for knowledge of the natural world, and one model for knowledge of the human world, as Vico, Collingwood and others have thought.⁹⁰ One of the issues in assessing the merits of the assumption of positivism is whether we need a single model of knowledge at all, for positivism excludes such epistemological pluralism. Yet even in science pluralism is in principle possible: while it is not one of the philosophies of science which we are using to illustrate the nature of the relationship between a model and that discipline which it models, one important philosophy of science is indeed pluralist at the epistemological level, namely that of Paul Feyerabend,⁹¹ who calls on scientists to generate alternative theories which may all be, in some appropriate sense, equally “true” or equally “justified”.

By contrast, Popper-Hempel and Kuhn conflict in their grounds of *justification*. Scientific beliefs justified according to one model will in many cases not be justified according to the other. The historically contingent “reasons” which in Kuhn’s terms justified the acceptance of Copernican theory were in various ways plausibly inconsistent with the kind of “reasons” which Mach had in mind in saying that our best scientific theories are justifiable only by reference to our sensations. Pluralism is not appropriate here, just because and in so far as the conflict between Popper-Hempel and Kuhn is one which requires resolution. Inconsistency is, in classical logic, a paradigm case of that which forces decision between choices. It is consistent with claiming inconsistency between Popper-Hempel and Kuhn at this point that there might be a *contingent* overlap in some cases; there is no reason why the two models might not

⁹⁰ See Leon Pompa, *Vico: A Study of the ‘New Science’*, Cambridge: Cambridge University Press, 1975, pp. 72ff.; Leon Pompa (ed. and trans.), *Vico: The First New Science*, Cambridge: Cambridge University Press, 2002. See R.G. Collingwood, *The Idea of History*, London: Oxford University Press, 1961. See Michael Bentley, *Modern Historiography*, chap. 9.

⁹¹ See, for example, Paul K. Feyerabend, “How to be a good empiricist – a plea for tolerance in matters epistemological”, reprinted in P.H. Nidditch (ed.), *The Philosophy of Science*, London: Oxford University Press, 1968, pp. 12-39, and Paul K. Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge*, London: NLB, 1975; 3rd edn. London: Verso, 1993.

purport to justify the same thing on occasion (although – given the conflict – only one would fully *succeed* in doing so), while in principle one model might be found to include the other,⁹² in which case we might have “half-way” and “rock-bottom” justifications.⁹³ In addition to the conflict between them at the level of description, we may therefore recognise that the opposition between Popper-Hempel and Kuhn with regard to the prescriptive version of their models of science is an opposition between standards of justification, and that is how they themselves and their readers have typically understood the choice here.

The conflict is best seen as a conflict over what counts as “good reason” for scientists to believe something. Says Kuhn, “the more carefully [historians] study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If these out of date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge”.⁹⁴ This neither says nor means what many readers of Kuhn have taken it to mean, an extreme scepticism or relativism, that *anything that anybody whatever* thought about the world was to be taken as being as scientific as today’s natural sciences. It does not mean that all myths (such as witchcraft) are as good as science. It means rather that *certain specific and exemplified theories or practices* had the appropriate valued status, *even if* they are to be called myths (which, of course, they don’t have to be). Moreover, that valued status involves believing on the basis of “good reason”, that is, they were “justified”.

But were they justified? Kuhn’s opponents do not see it that way, and, for illustrative purposes, the position which others have perceived in his work is perhaps better expressed by Virginia Woolf: “when a subject is highly controversial ... one cannot

⁹² This would be a contingency, not a dogma requiring such inclusion; see Paul K. Feyerabend, “How to be a good empiricist”, p. 17.

⁹³ To extend to justification J.W.N. Watkins’ distinction between “half-way” and “rock-bottom” explanations. See J.W.N. Watkins, “Historical explanation in the social sciences”, reprinted in Gardiner (ed.), *Theories of History*, pp. 503-514 at p. 504.

⁹⁴ Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 1962, p. 2.

hope to tell the truth. One can only show how one came to hold whatever opinion one does hold. One can only give one's audience the chance of drawing their own conclusions as they observe the limitations, the prejudices, the idiosyncrasies of the speaker. Fiction here is likely to contain more truth than fact".⁹⁵ If you succeed, by these means, in enabling others to see what led you to believe as you do, you have – so the point goes – thereby displayed what you took to be the *justification* for believing as you do. Yet such "justification", as some of Kuhn's opponents would see the matter, has nothing to do with *truth*, with what people *ought* to, or are justifiably *entitled* to, believe, on the basis of some objective person-independent rationality.

At this level, the best spokesperson for Popper and Hempel was Imre Lakatos. Lakatos saw Popper and Kuhn as clashing at the level of "our central intellectual values",⁹⁶ with differing views about the nature and value of "reason" in scientific justification, and he accused Kuhn of irrationality: "For Popper scientific change is rational or at least rationally reconstructible and falls in the realm of the *logic of discovery*. For Kuhn scientific change – from one 'paradigm' to another – is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the *(social) psychology of discovery*".⁹⁷

⁹⁵ Virginia Woolf, *A Room of One's Own* [1928], London: Penguin, 2004, p. 4, beginning to address the issue "women and fiction".

⁹⁶ Imre Lakatos, "Falsification and the methodology of scientific research programmes", in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge*, pp. 91-195 at p. 93.

⁹⁷ *Ibid.*

Justification in the second-order context: Popper and Hempel

In summary, generalising from these paradigm illustrations of modelling science, and following John Austin's distinction, we may hold that the "true" model of a discipline both describes and prescribes. In particular, we see that the two models from Popper-Hempel and from Kuhn are opposed as descriptions of scientific practice and opposed as prescriptions specifying how scientific knowledge is properly justified. The opposition between Popper-Hempel and Kuhn about who gives the true *description* of science would, as we have seen, have to be resolved in terms of who best achieves the relevant standards of historiographical justification (recalling that this is continuous with everyday justification of matters of fact); but what justification is available for the choice of *prescription*? What illustrations of such justification are available? Accepting the opposition now disclosed at the prescriptive level requires us to choose – and to justify our choice – between these two models of justification, only one of which can be "true".

The problem becomes this: what prescriptive standard of justification is to be used to justify our selecting one rather than the other of the two opposing prescriptive standards of justification which Popper-Hempel and Kuhn offer? Thus put, we can see the possibility of two different levels or contexts of justification emerging, and philosophical readers will observe the risk here of problems arising with regard to the possible self-application of a criterion.⁹⁸ We need most importantly to distinguish between these two different contexts. As earlier explained, to claim that a model is "true" of the discipline it represents, given the double-sided conception of truth involved (following Austin), is to claim that both description and prescription are each appropriately justified. With regard to prescription, Popper-Hempel and Kuhn have primarily engaged in modelling "justification" in what is here best seen as a "first-order" context, that is, they have offered prescriptive standards – models – which purport to justify scientific knowledge. This needs to be contrasted with a "second-order" context, which involves justifying the models themselves. It is in terms of a

⁹⁸ A problem which arose particularly in the logical positivists' attempt to specify a verificationist theory of meaning.

standard appropriate for this second-order context that we seek to judge who is right, Popper-Hempel or Kuhn.

Notice that this issue of justification in the second-order context arises, in principle, *independently* of the *content* of the Popper-Hempel and Kuhn positions (1) and (2) above. This point perhaps becomes most obvious when we reflect that the issue arises even when their models of science *are not offering rival criteria of justification at all*. The issue of second-order justification arises, in other words, when we have to justify a way of prescriptively modelling a discipline, regardless of the kind of prescriptive models in question, and so arises even when those models are not themselves models of justification (that is, not themselves epistemological models). If we are to understand the ways in which a philosophical model may be “true” of historiography, then we need to understand the ways in which it may be justified as such, even if we also think that the philosophical model of historiography should not be epistemological at all. We are currently seeking illustration from the philosophy of science to help us with this, but what we need is illustration of *second-order* justification, not *first-order*.

In passing, it should be noted that second-order justification is required *whether or not* we have – as we have here with Popper-Hempel and Kuhn if we are to understand the conflict between them – to *choose between* prescriptive models. If pluralism is appropriate, we may not need to choose, for it is the absence of pluralism and the consequent insistence on unification which forces choice. Notice also that *we decide* when pluralism is “appropriate”. Even when inconsistency is present, the forcing of choice may be required by classical logic but it remains open to us not to choose such a logic; the limits of this will be dealt with in the section “Quine not postmodern enough”, below. The philosophical issue here is highly controversial, but the essential point is that whether we should *have* to choose, as the absence of pluralism requires, is itself a choice: the unity of knowledge is not an unavoidable position. Ultimately it is, at least in part, a *moral* question, rather than a wholly logical one, how far pluralism is appropriate, that is, how far we should understand ourselves to share the same world as other people. The consistency of reality may well be for us a regulative ideal for knowledge, but that could be a contingent fact – if it is a fact – about our current

historically situated understanding. Consistent truth would be for us a *value*.⁹⁹ It is a core value of many disciplines, but we have to allow for the possible appropriateness of pluralism in some contexts. If pluralism is present, then different models can co-exist, but both or all will still need to be justified since not just *any* model will do; if, on the other hand, pluralism is not present, then justification is in addition needed for which model is more justified.

Earlier we argued that there are two major interpretations of “justification” in the analysis of knowledge: first, (a) we can read “justification” as involving some more or less demanding standard which has to be achieved for a knowledge claim to be allowed; second, (b) we can think of the “justification” requirement as being met in so far as knowledge claims are self-justifying when there is no positive reason to doubt them: they are innocent until proved guilty. This contrast was introduced partly to express the view of some historians that current historiographical practices are self-evidently epistemologically successful and are not to be undermined by mere philosophical speculation, and partly to avoid begging questions about the truth of the view held by those historians. In principle this contrast can arise in both first-order and second-order contexts, as just described: both our first-order and second-order contexts are epistemological contexts in the sense that – in our first-order examples – we wish to *know* what makes a scientific theory or practice “true”, that is, what justifies its acceptance (and Popper-Hempel and Kuhn offer conflicting models of this), while – in our own central second-order concern – we wish to *know* what makes the model of a discipline “true”, that is, what justifies its acceptance.

Either of the two major interpretations of “justification” (a) and (b) above is available for Popper-Hempel and for Kuhn to use in their first-order contexts, and the distinction is likely to be of importance in clarifying the detail of their positions: some may think that the (a) and (b) interpretations respectively best suit Popper-Hempel and Kuhn,¹⁰⁰ although this would not be a simple matter. In fact the broad characterisations of their positions given above provide no such detail with respect to justification, and our

⁹⁹ This point is further dealt with later, particularly in the section “Quine not postmodern enough”.

¹⁰⁰ Kuhn, like Rorty, may be seen as continuing the American pragmatist tradition and as inheriting some of the elements of Peirce’s philosophy as referred to above.

overall argument proceeds independently of such content. Moreover, as already explained, it is not here our concern to determine the “true” philosophy of science and it is thus equally not relevant to give at this point more detail of those first-order “justifications” for science which Popper-Hempel and Kuhn offer. In addition, we must recognise that a model for use in a first-order context might not be an epistemological model at all, in which case justification, under any interpretation, might in principle be an irrelevant issue in that first-order context.

Our ongoing argument does, however, require clarification of how the two major interpretations of “justification”, marked as (a) and (b) above, relate to our concern with justification in the second-order context. At this second-order level of argument now reached in the Popper-Hempel/Kuhn illustration, the distinction between the two major interpretations of “justification” in the analysis of knowledge is a distinction which may be seen as impacting in two different ways. One way, it makes no difference: the distinction is in one way irrelevant to the argument at second-order level for the following reason: *within* the second-order level, we cannot think of prescriptive models in the first-order context like those offered by Popper-Hempel and Kuhn being themselves “innocent” or self-justifying because, as earlier analysed, the opportunity for self-justification only arises *when there is no positive reason to doubt* the claim. However, in the present case involving rival prescriptive models, where we, as it were, sit in second-order judgement over rival first-order models,¹⁰¹ we do have a positive reason to doubt the claim of any first-order model: whichever first-order model is presented to us as innocent until proved guilty, the mere existence of the other first-order model provides a positive reason to refute that presupposition of innocence. There would have to be some minimal, minimum and available standard of plausible effectiveness, achievement or acceptance which a model would have to meet to be said to “exist” rather than to be laughed off, but, whatever that standard is, there is no doubt that Popper-Hempel and Kuhn meet it. Given their opposition, which is to say, given the inappropriateness of pluralism in this context, then the two prescriptive models of justification offered by Popper-Hempel and by Kuhn in the first-order

¹⁰¹ Should our judgement, analogously with the issue in courts of law, be part of an *adversarial* or an *inquisitorial* process? This philosophical choice is not decided here. Reflection may be aided by the works of Peter Abelard (1079-1142), in particular his *Sic et Non*.

context obviously cannot *both* be innocent until proved guilty (for that would mean that they could both be held true even though contrary to each other), and *neither* can be given the existence of the other. Understanding justification of those prescriptive models in terms of (b), self-justification, is then an understanding which is not available in this situation, and “justification” in the analysis of knowledge of the “true” model then defaults to the first interpretation (a), where we understand the appropriate “justification” as involving some more or less demanding standard which has to be achieved for a knowledge claim about which is the true prescriptive model of science (or, generalising, any other discipline) to be allowed. The distinction between (a) and (b) is in this way irrelevant.

However, notice that the distinction between the two major interpretations of “justification” in the analysis of knowledge, while in the way just described irrelevant to the argument at second-order level, may be relevant in a different way: thinking *about* rather than *within* the second-order level, while we understand second-order “justification” necessarily to involve (given the opposition between Popper-Hempel and Kuhn) some more or less demanding standard which has to be achieved for a knowledge claim about which is the true prescriptive model to be admitted in the first-order context, *there may not be more than one such standard*. As before, it is plausibly effective and available standards which are to count here. At the second-order level there may not be opposed rivals for this standard, and the second-order model of justification may then be, at least in principle, *self-justifying*, rather than having itself to meet some yet further standard.

As earlier argued, these things are so regardless of whether the prescriptive models at issue in the first-order context are themselves models of justification. Notice, however, that the model we need in the second-order context to decide between opposed models in the first-order context will in fact be a model of justification, even if the first-order models are not. Again as argued earlier, if pluralism is present, then both (or all, should we allow more) different models in the first-order context will still need to be justified according to the relevant “more or less demanding standard” appropriate to that judgement in the second-order context, even though we do not have to choose between them.

Seeking as we do an illustration of a justification which might be used in the second-order context of the justification of prescriptive models, it might be thought that we have two illustrations readily available, namely those offered by Popper-Hempel and by Kuhn for use at the first-order level. In principle this is right: distinguishing first-order from second-order *contexts* of justification is not necessarily the same as distinguishing first-order from second-order *justifications*. What we have occasionally referred to as “first-order justifications” or “first-order models” need to be understood as “justifications or models in the first-order context”, and similarly for second-order. Despite the fact that we have carefully distinguished first-order from second-order contexts of justification, it is thus in principle possible, while recognising this distinction, to allow that the same justificatory structure or approach might be effective at both levels. The Popper-Hempel and Kuhn models are, however, opposed, and we have seen that we have to choose between them; pluralism in the present matter at the first-order level is not appropriate. If both models are offered as effective at the second-order level, in addition to being offered as effective at the first-order level, and assuming that they are still *opposed* at the second-order level (for in principle they might not be), then we will need some third-order level standard for choosing between them; and we can imagine such reasoning proceeding *ad infinitum*. This is, however, merely a *possible* regress rather than a *vicious* regress, for the reasoning can be stopped at the point where any of the models fail. However, it does not follow from the Popper-Hempel and Kuhn models being plausibly effective at the first-order level that either or both are plausibly effective at the level we need, the second-order level, and this is the next question to be dealt with.

Again, seeking as we do illustrations of second-order justifications of prescriptive models, what justifications are in fact offered by our protagonists? The Popper-Hempel view was introduced as being – in broad terms, at least – an extension of Mach’s view that our best scientific theories are justifiable only by reference to our sensations, and this was itself an extension of Hume’s view that everything we know must be derived from our experiences, together with Hume’s view that causation is to be understood in terms of lawlike generalisations. Is this – to continue with the terms of our own present argument – a first-order context or a second-order one? It is

clearly and explicitly, at least in part, a first-order context, for it gives us a standard for justifying scientific theories, namely, that they are ultimately justified by our sensations. But our current question is not about the *content* of this standard for knowledge, but about the content of the standard which justifies our acceptance of that standard.

The justification given or presupposed by Popper-Hempel in the second-order context is roughly this: Hume and Mach are right, therefore this is the standard to use. But what justifies the view that Hume and Mach are right? The original detail of the approach appears in David Hume's *A Treatise of Human Nature*, which presents itself as applying the "experimental method of reasoning"¹⁰² to moral subjects, that is, subjects which we commonly think of as involving philosophical considerations where humankind is central. It is here, and on the basis of the "experimental method of reasoning", that we find the philosophical assumptions of atomistic empiricism spelt out, together with the analysis of causation which makes universal conditionals central to our understanding.¹⁰³ If we seek, as we do in seeking a second-order justification, a justification for relying on sensations or experiment in first-order contexts, then it may seem to be begging the question to reply that our approach is justified by using sensations or experiment. To generalise the suggestion, it may seem to be begging the question for our second-order justification to have the same content as our first-order justification: it may be thought that, if this happens, there is, in effect, no justification whatever. But that suggestion is not correct, for – *in principle* – Hume's "experimental method of reasoning" *may have no rival* in the second-order context; equally, a more careful detailed use of Hume's philosophical approach might – again *in principle* – justify Kuhn's model rather than that of Popper-Hempel. There is no need in principle, as earlier shown, to require that justification in the second-order context be different from that in the first-order context. Any circularity will depend upon the detail.

But, despite this, a fatal circularity does indeed arise in so far as we associate *positivism* with the Popper-Hempel approach. Reinstating (for a moment) this

¹⁰² David Hume, *A Treatise of Human Nature*, part of subtitle on original title page: "being an attempt to introduce the experimental method of reasoning into moral subjects".

¹⁰³ *Op. cit.*, Book I, Part III.

assumption amounts to dogmatically insisting that there must be a *single model* of justification to be used in *all* contexts. While this model is, for the positivist, a particular model of *science*, the problem does not lie with that but with the unjustified universality of the claim that there is only one way of knowing and so only one way of justifying, *thus a way necessarily useable in all contexts or levels*, moreover useable and in effect self-justifying *even when rivals are available*; this is what makes positivism a dogma. If there is a rival to that model in *any* context – as there is, Kuhn’s, in our first-order context – then it is begging the question to decide the issue of who is right, Popper-Hempel or Kuhn, by using one of the models at issue. Positivism, in effect, forces us to run together first-order and second-order contexts by making all questions questions which are answerable in the same way, so that a rival at any level is a rival at all levels. Again, that positivism forces us to run together first-order and second-order contexts by making all questions *scientific* questions which are answerable in the same way is not the central consideration.

This objection of circularity to the line of thought involved in Hume-Mach-Popper-Hempel is important because of the usual association of empiricism with positivism. However, empiricism is, as we have seen, not *necessarily* associated with positivism: one can, after all, be an empiricist without being a positivist. Without the positivist assumption, the main issue is not one of circularity but rather whether there is a rival for the Humean approach at the second-order level. There is: briefly, Hume offers one atomistic empiricist justification, whereas there are many other philosophical approaches, including different kinds of empiricism and various forms of rationalism. Justifying Hume’s approach then requires engaging with and defeating these alternative philosophies.

Any standards governing such engagement, for “victory” and “defeat” in choices of philosophical approach, would be third-level justifications. While “metaphilosophy” exists as an academic subject (we should perhaps not say “discipline”, because like much of what counts as philosophy it is – necessarily – unorganised), it exists *as a branch of philosophy itself*, and the upshot is that there is no clear distinction between second- and third-order contexts here. Thus, while deciding between rival second-order justifications has often taken the form of philosophical arguments which typically

differ from the content of the criteria they recommend, there is no clear way of escaping that second-order context and moving to the third-order level, for the same arguments typically surface in the third-order context, with the constant risk of questions being begged. Moreover, what counts as an “argument” in philosophy is itself controversial; there are no philosophically *independent* standards.

There is a tangle of complex and unresolved philosophical issues which are involved in the attempt to select, from a range of philosophical positions, just Humean atomistic empiricism as *the* criterion in the second-order context for choosing between first-order prescriptive models. Popper, Hempel and the earlier logical positivists made little contribution to these substantive and traditional philosophical questions, at least partly because their own empiricist tradition tried to minimise philosophical input (and on occasion tried to abolish it). That the answer – if there is an answer – lies within the subject “philosophy” itself seems to be as much as can be said at this point, and it is indeed traditional to conceive philosophy as paradigmatically the discipline which deals with second-order contexts. While there are many philosophies, one philosophical hope has often been held, that, ideally, only one philosophy would turn out to be true. Facing no rivals, it would then be self-justifying, on the basis of the analysis given earlier. However, all this is speculation: relevant content involving philosophical argument is absent from Popper and Hempel. In practice “philosophy” is an ineffective justification here just because there is, in fact, no determinate philosophical outcome. Says Collingwood, “I know of no philosophy that is not a voyage of exploration whose end, the adequate knowledge of its proper object, remains as yet unreached”.¹⁰⁴ Second-order justification of the Popper-Hempel model is missing.

That, as just claimed, no second-order justification is effectively and explicitly offered for the Popper-Hempel position might seem to be a local feature of the debate which we can afford to discount: it seems plain that a justification *ought* to be provided. It might be thought inappropriate to give up the argument and conclude only that philosophy is indeterminate. It might be thought that it would be fairer to present a more thorough overview of the philosophical detail involved in the empiricist tradition

¹⁰⁴ R.G. Collingwood, *An Essay on Philosophical Method*, p. 3.

that lies behind the position, offering in particular – as is the usual practice in philosophical discussion – the best available arguments in its favour, developing new ones where necessary and as appropriate, in order to present the strongest case without taking advantage of some *ad hominem* failing on the part of one or more of the authors. By this means we would perhaps approach more closely to an understanding of justification in this context.

Certainly that might be an appropriate practice if we were concerned to find the “true” model of science, that is, if we were not concerned, as we are here, only to *illustrate* the relationship(s) between a model and that discipline which it models. Yet the point is overtaken by a stronger consideration, which arises when we do no more than scratch the surface of the philosophical detail involved in the empiricist tradition. Even if, in some possible world, there were *no alternatives* to Hume’s approach in the second-order context, it is very doubtful if Hume’s philosophy would be self-justified in successfully providing second-order justification for choosing one rather than another of rival models of science. This is because Hume’s philosophy may well not even reach the minimum standard of being plausibly effective in the second-order context. This is for the following reason: a particular problem that arises for Hume is this, that in seeking to use his approach as a second-order standard which can be used to choose between first-order models (whatever they are; they need not be Humean first-order models and so circularity need not be an issue in any event), we are seeking to justify a choice between *prescriptions* and not between *descriptions*, given the Hume-based distinction which we have derived from John Austin. But it is essential to this distinction of Hume’s between fact and value that we cannot use criteria for “factual” determination to yield prescriptive outcomes. Empirical justification, for Hume, is essentially of descriptive choices and not of prescriptive choices. For prescriptive choices, his approach is that they are to be made on the basis of brute taste or preference: “the distinction of vice and virtue is not founded merely on the relations of objects, nor is perceiv’d by reason. ...it must be by means of some impression or sentiment they occasion”.¹⁰⁵ We seek illustrations of second-order justifications of *prescriptive* models, and Hume in effect offers us taste; *de gustibus non est*

¹⁰⁵ David Hume, *A Treatise of Human Nature*, p. 470.

disputandum, so we don't *have* to choose between Popper-Hempel and Kuhn at all, while, if we do choose, we can quite literally choose what we *like*. Thus Hume's approach does not successfully provide second-order justification for choosing one rather than another of rival models of science, for the reference to taste permits justification to go either way, or for that matter no way at all.

Now there is no denying that the brief points made here against Hume – while claimed to be sound – do not do full justice to the subtlety of his position nor to the almost three hundred years of empiricist philosophy of evaluation which have followed; moreover our own argument is proceeding (although only for a limited time) on the basis of John Austin's Hume-derived distinction. But what should be clear is that, so far as Popper and Hempel are concerned, and if we exclude as we should mere "taste" (which they would certainly not accept as justification, despite their Humean background), it is very difficult to see in their work at this point any clear standard – or even the rough outline of a standard – which could be used to justify in the second-order context the choice of a prescriptive model of science or – generalising to our overall concern – of historiography. We have not found even a mere illustration of such justification in the second-order context. In practice, and prior to Kuhn, the positivists took the second-order justification of their model for granted, as if their first-order position were self-evident. Self-evident it could not be, given the development of Kuhn's rival view.

Justification in the second-order context: Kuhn

Almost as if the second-order justification of prescriptive models did indeed, following Hume, depend upon taste, deciding between Popper-Hempel and Kuhn in practice partly involved relying on what may well seem to be the more rhetorical elements of persuasion, as follows: it was the *real* successes of science (as if we knew what they were independently of the justification provided) which were impressive and which had originally – although mistakenly – given the positivist model of science its persuasive power as an appropriate standard for knowledge. But, now that the model was shown not to fit the historical facts, it became no more than an ideal. How could a mere ideal which was now seen not to match or justify or explain actual scientific success properly claim to be a universal standard for knowledge? And, worse for the logical positivist with an essentially empiricist background, where was the model now to find its empirical rather than *a priori* support? When scientists in practice did not follow Hempel's stipulations, so that neither empirical evidence nor *a priori* reasoning were available to support those stipulations, certainly historians had no reason to follow them. Once the standard model of science had lost its descriptive validity it lost its prescriptive merit also. In so far as he was improving our descriptive understanding of science, Kuhn was thereby taken to be also improving our understanding of that which has prescriptive merit for us. He was seen as advancing our model of science in both its descriptive and prescriptive senses.

Rhetoric and taste apart, what second-order justification does Kuhn offer for his view? Given John Austin's approach, we have presented Hempel's and Kuhn's theories as operating in both descriptive and prescriptive modes, and Kuhn himself thinks of his own theory in just this way. In a "Postscript" he addresses directly the charge that he simply confuses these two modes:¹⁰⁶ "The preceding pages present a viewpoint or theory about the nature of science, and, like other philosophies of science, the theory has consequences for the way in which scientists should behave if their enterprise is to succeed",¹⁰⁷ and "...one set of reasons for taking the theory seriously is that scientists, whose methods have been developed and selected for their success, do in fact behave

¹⁰⁶ Thomas S. Kuhn, "Postscript", *The Structure of Scientific Revolutions*, 2nd edn., 1970, p. 175.

as the theory says they should”.¹⁰⁸ Kuhn continues, “the circularity of that argument is not, I think, vicious. The consequences of the viewpoint being discussed are not exhausted by the observations upon which it rested at the start”...the theory is “a useful tool”.¹⁰⁹

So Kuhn’s justification in the second-order context is, at least in part, that scientists “do in fact behave as the theory says they should”.¹¹⁰ This is, on the face of it, not a justification at all. But first, let us examine the difficulty he raises. At one elementary level Kuhn is right to deny vicious circularity. There is certainly no vicious circularity in using the same material in both descriptive and prescriptive modes, for the intelligibility of doing so is readily illustrated by the traditional educational approach of teaching by example: thus “do as I do” implicitly involves a true description of what I do and also sets that as a standard for what others ought to do. Any difference of meaning between a description and a prescription will ensure that, in Kuhn’s words quoted above, “the consequences of the viewpoint being discussed” – the prescriptive claim, that scientists should behave in a certain way – “are not exhausted by the observations upon which it rested” – the descriptive claim, that scientists do in fact behave in that way. Unquestionably there is some difference of content between descriptions and prescriptions, a difference which R.M. Hare familiarly sought to clarify with his distinction between different kinds of “neustic” for a common “phrastic” content.¹¹¹ For example, following Hare, the phrastic “door closed” may be associated with the neustic “yes”, making the sentence descriptive, or with the neustic “please”, making the sentence prescriptive. Following Austin, description is one thing, and prescription is a different thing: as a matter of elementary logic, one cannot derive “scientists ought to do this” from “scientists actually do this”. Whether we can generalise such an elementary logical claim to a *categorical* difference between “facts” and “values”, as David Hume (and Hare) sought to do, is not a matter which need

¹⁰⁷ *Op. cit.*, p. 207.

¹⁰⁸ *Op. cit.*, p. 208.

¹⁰⁹ *Ibid.*

¹¹⁰ *Ibid.*

¹¹¹ See R.M. Hare, *The Language of Morals*, London: Oxford University Press, 1952, p. 18. This is a simple example of a wider philosophical understanding which involves the functioning of modal operators in pragmatic contexts.

concern us at this point.¹¹² As we have seen, and in so far as we follow Austin, that scientists behave as they do is one matter; that they ought to is a different matter. Whether, despite Hume, we can somehow reason from one to the other does not affect the point that there is a difference, and therefore no vicious circularity of the kind Kuhn claims to be worrying about. So, any meaningful difference between them will then ensure that the prescriptive claim, that scientists should behave in a certain way, is not “exhausted” by the descriptive claim, that scientists do in fact behave in that way. This precludes Kuhn’s “viciousness”, at least at one level.

With this supposed vicious circularity removed by Kuhn, we can work with his view that “...one set of reasons for taking the theory seriously is that scientists ... do in fact behave as the theory says they should”.¹¹³ Such persuasion by reference to historical facts was powerful and effective. The actual development of the debate involved discussion of Kuhn’s conception of the historical facts about scientific development, and this importantly presupposed a position – shared with major figures¹¹⁴ among Kuhn’s opponents – about the centrality of historical facts in the first place. Thus, and in fact, the plausibility of Kuhn’s position depended on the correctness of his description of the historical facts. Unsurprisingly, that is also how Kuhn perceived the matter at a Colloquium in London in 1965, chaired by Karl Popper: “On almost all the occasions when we turn explicitly to the same problems, Sir Karl’s view of science and my own are very nearly identical. We are both concerned with the dynamic process by which scientific knowledge is acquired rather than with the logical structure of the products of scientific research. Given that concern, both of us emphasize, as legitimate data, the facts and also the spirit of actual scientific life, and both of us turn

¹¹² This involves taking “ought” and “is” as *copula* in traditional Aristotelian logic, and goes far beyond the elementary logical point made here. David Hume, *A Treatise of Human Nature*, pp. 469-470. See also John R. Searle, “How to derive ‘ought’ from ‘is’”, in Philippa Foot (ed.), *Theories of Ethics*, London: Oxford University Press, 1967, pp. 101-114, also in W.D. Hudson (ed.), *The Is-Ought Question: A Collection of Papers on the Central Problem in Moral Philosophy*, London: Macmillan, 1969, pp. 120-134, and other papers in those two collections.

¹¹³ Thomas S. Kuhn, “Postscript”, *The Structure of Scientific Revolutions*, 2nd edn., 1970, p. 208.

¹¹⁴ Even Hempel, who privately acknowledged the merits of Kuhn’s history-based philosophy of science when he gave his paper “Problems in the empiricist construal of theories” at a History and Philosophy of Science Seminar, Cambridge, 4th November 1971.

often to history to find them”.¹¹⁵ Popper gave a paper which began by stressing even further agreement.¹¹⁶

Lakatos, while, as earlier noted, seeing Popper and Kuhn as clashing at the level of “our central intellectual values”, and while conceptually sophisticated and passionate in favour of (objectively understood) reason in science, nevertheless agreed in making historical facts central. The historiography of science has to bear out our theory of scientific rationality, in his view,¹¹⁷ and in a multiplicity of references to examples from the history of science Lakatos picked out in his historiographical illustrations the kind of pragmatic considerations which Kuhn would also think relevant to the historiography of scientific development, for example: “But Bohr wanted to put his *authority* behind Fermi’s daring application of Heisenberg’s new big programme to the nucleus; and since Shankland’s experiment and Dirac’s and Peierls’s attack brought the beta-decay into the focus of the criticism of the new big programme, he overpraised Fermi’s neutrino programme which promised to fill in a sensitive gap. No doubt, the later development spared Bohr from a dramatic humiliation: the programmes based on conservation principles progressed, while no progress was made in the rival camp”.¹¹⁸

The second-order justification of Kuhn’s position thus involved an appeal to historical facts. In practice such an appeal might be taken to be self-evidently justified, just in so far as Kuhn’s opponents shared the same view of the centrality of historical facts and agreed or presupposed that there was no effective rival to that position. There was no effective rival in the second-order context: “philosophy” was at worst question-begging and at best indeterminate in its recommendations, and in any event ineffective; only “taste” justified the Popper-Hempel approach, on Hume’s own position, but then “taste” could equally justify Kuhn’s approach. In the end it was quite clear what was wrong with the Popper-Hempel approach: it did not match the facts. However,

¹¹⁵ Thomas S. Kuhn, “Logic of discovery or psychology of research?”, in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge*, pp. 1-23 at p. 1.

¹¹⁶ Karl Popper, “Normal science and its dangers”, in Lakatos and Musgrave (eds.), *Criticism and the Growth of Knowledge*, pp. 51-58 at p. 51.

¹¹⁷ Imre Lakatos, “Falsification and the methodology of scientific research programmes”, in Lakatos and Musgrave (eds.), *Criticism and the Growth of Knowledge*, pp. 91-195 at p. 115.

¹¹⁸ Lakatos’ emphasis. *Op. cit.*, p. 172.

distinguish between two different claims in this context: first, the claim shared by Kuhn and his opponents, so possibly taken to be self-evident, that the standard to be used in the second-order context was that first-order claims should match the historical facts; second, the claim, a specific claim on Kuhn's part, not shared with his opponents, about what the historical facts themselves actually were. While there may have been no rival to the first view, so possibly permitting its self-evident justification, there was certainly a rival to the second. We shall examine these two different claims in order.

We do not need a third-order justification why historically-derived facts about what science is should operate as a second-order justification for choosing a theory about science, if that second-order justification is self-evident. However, as to this centrality of historical facts in second-order justification, it is not enough to accept this approach as self-evident merely because and in so far as there is no rival to the position. This is because, as we saw in the analysis of Humean support for Popper-Hempel, the justification has still to reach the minimum standard of being plausibly effective in the second-order context, and like Hume's it might not do so. It may indeed not meet the minimum standard because there seems to be a Humean reason against it. Casting our argument in terms of the behaviour of scientists for brevity and for the convenience of matching Kuhn's expression (although the point can also be made for other philosophical approaches to science), we ask this: just because of the Humean distinction between prescription and description which Kuhn is so anxious to persuade us he understands, exactly why would facts about the actual behaviour of scientists, past or present, have any relevance at all to justifying a theory about how scientists ought to behave, given Kuhn's insistence on the difference between these?

The answer is simple: on the assumptions made, such facts have no relevance whatever, which is why we remarked earlier that Kuhn's point was, "on the face of it, not a justification at all". If Kuhn needs to rely on Hume's distinction between fact and value, then he, like Popper and Hempel, would be committed to being unable to use criteria for "factual" determination as criteria which yield prescriptive outcomes. Historiographical justification, for Kuhn, would be essentially of descriptive matters and not of prescriptive matters. No more than arbitrary taste is then available to

justify the theory about how scientists ought to behave. Just as for Popper-Hempel, the justification would not reach the minimum standard of being plausibly effective in the second-order context, because anything at all could be justified by it. Whatever the “facts” may be, maybe something entirely different could be what “ought” to happen.

Yet there is a *caveat* to this argument: Humean empiricism is very plausibly *committed* to the prescriptive/descriptive distinction, and those like Hempel and Popper who work within that tradition have, as we have seen, few resources to avoid the difficulties. Kuhn, by contrast, despite the “Postscript”¹¹⁹ to his work in which he tries to show that he can address the “criticism” that he confuses the distinction, is not *committed* to it. It is a commitment made within a tradition which he does not embrace. Yet Kuhn, and those of his opponents who join with him in making an appeal to historical facts central, do seem committed to – yet plainly would not want to be seen as holding to – at least the following invalid argument with its simplistic conclusion: “this is what scientists do; so do whatever scientists do”. While we may not need some commitment to a *categorical* distinction between fact and value to determine it to be invalid, it seems to be invalid all the same. But what is the importance of the invalidity?

We shall next examine the conclusion of this argument, and use our understanding of it to analyse the claim that the argument is invalid, in the process clarifying what is involved in the reference to historical facts in second-order justification. To say “do whatever scientists do” is not as clear as it might be, but on one obvious reading it permits us to say, to new present day scientists, “always do what past scientists have done”. This would not be plausible either for Kuhn or his opponents because they would share the view that it is contrary to the spirit of scientific method which – however we characterise it – permits *revision* of past theories and practices. Moreover, it implies that past scientists – and present ones – should be copied even *when they did what they did in error*. In addition, scientists engage in lots of things – everyday life, for example – which have no direct connection with their science.

¹¹⁹ Thomas S. Kuhn, “Postscript”, *The Structure of Scientific Revolutions*, 2nd edn., 1970.

In the above-mentioned argument's conclusion, "do whatever scientists do", we can then see that the word "scientists" is ambiguous between a "so-called" sense and a "real" sense. We don't mean scientists *qua* parents, for example, but scientists *qua* scientists. We don't mean what Aristotle would have called the things they accidentally do, but the things they, being scientists, essentially do. Bringing out the ambiguity involves an argument with ancient lineage. In Plato's *Republic*, Thrasymachus suggests that just laws are those which set down what is advantageous for the rulers, and justice requires that those ruled must obey their rulers. Rulers, he admits, can make mistakes, and Socrates traps him into recognising that it follows that it is just both to do what is to the ruler's advantage and also what is not.¹²⁰

Thrasymachus, goaded into some precision, says that his earlier claim had been a mere manner of speaking: no ruler *really* makes mistakes, because when he makes a mistake he is not at that point really a ruler. "Each of these men, insofar as he is what we address him as, never makes mistakes".¹²¹

Can scientists make mistakes? In a sense, obviously, but, when we are trying to find out what it is to be a scientist, we had better not copy one who is making a mistake. If we follow Thrasymachus, a person who "really is" a scientist does not make scientific mistakes. Nor are they "really" being scientists while they are engaging in something else. There is nothing wrong in saying "do whatever scientists do", if "scientists" is taken in this exacting "real" rather than "so-called" sense. Equally there is nothing wrong in telling present day scientists, "always do what past scientists have done", if "scientists" is taken in this same exact sense as meaning just those people who do not make scientific mistakes. There is no problem of error, and no need for revision, here. Of course, there might in principle not be any past scientists in this sense, depending on what counts as being a scientist, but the point is sound. It is a *contingency* whether there are, or were, any such.

We can advance, here, our understanding of the concept of a "mistake" that scientists and indeed historians might make. Think, as is familiar to social understanding in

¹²⁰ Plato, *The Republic*, 339. Various translations available.

post-war decades, of our *social institutions* as being understood as systems of *rules*. These “social institutions” are not necessarily formal: they may range from highly informal practices where “rules” are not consciously expressed, through societies with voluntary yet comparatively clear codes of normal or expected behaviour, to the formal and enforced structures of civil and criminal law which are usual in the modern state. While we might normally think that paradigm cases of “social institutions” would be such things as the judicial system, or the institution of civil partnership, or the system of charity care for disadvantaged children, or the army, here the idea goes far beyond that. “Social institution” covers, for example, individual family units and even children’s temporary friendships in spontaneous playground games, and it does so simply because they are *social*. The word “institution” does no more work here than to supply a noun-vehicle for that adjective, given that the grammatically-derived word “society” is far too entrenched in current use for more limited applications. The old-fashioned word “institution” is available for the purpose simply because it is no longer required, as it once was, to refer to entities such as orphanages or lunatic asylums. Social understanding, so explained, may seem to suggest that one first identify social institutions and then seek to recover the “rules” governing them, but that has the matter the wrong way round: rather, the idea is that the “social” is *identified or constituted* in terms of the “rule-governed”.

If language is rule-governed, then language is a social institution, and so is mathematics for the same reason. To see language or mathematics as social institutions – they are not merely *likened* to social institutions – is a powerful idea, suggesting among other things that they might involve some essential connection to, or be subject to, political or economic structuring or influence, in the same way as are many other social institutions. There might also be the same range of limitations in the application of, for example, principles of rationality. Understanding mathematics and language as social institutions is also, for that very same reason and for others, a dangerous idea, risking as it does the obliteration of intellectual advances which have characterised mathematics in such distinctive ways, or the undermining of the sense that language can be used in objective description. It is necessary, as we use this

¹²¹ Plato, *The Republic*, trans. Allan Bloom, HarperCollins, 1968, 340e, p. 18.

approach to social understanding, to recognise that far more clarification would be required for its full application to particular cases.

Academic disciplines like science and historiography, among all these other social entities, are “rule-governed institutions”. But we need to understand what it is to be “rule-governed” in a way which is relevant to our own concerns. Here we are using it, in part, to make sense of the concept of a “mistake”. The jurist H.L.A. Hart argued that law, like other social institutions, is to be understood as a system of rules with a particular source. What is it for a legal rule to “exist”? A problem seems to arise when we think of rules concerning, for example, speed limits, which are widely flouted. Following Hart, “if a social rule is to exist, some at least must look upon the behaviour in question as a general standard to be followed by the group as a whole”.¹²² A rule exists, Hart explains, not necessarily in so far as it is obeyed (and we may allow that the majority may disobey it), but in so far as it is accepted as an appropriate standard for criticism.

Making a mistake (or even conscious disobedience) when occupying a rule-governed role need not, therefore, involve ceasing to occupy the role, as Thrasymachus was claiming; although failing to recognise, as appropriate grounds for criticism or assessment, the standards governing the role, might indeed have this effect. Similarly, the historiography or any other description of science does not require the mention of “real” scientists who never make mistakes but it does require the mention of people who occupy the role of scientists, and this means people who “meet” the rules which govern scientific behaviour, rules which characterise what it is to be a scientist or to do science and which also – given the nature of science – contingently include some specification of what counts as a mistake. They meet these rules, not necessarily by never making mistakes, but by recognising those rules as characterising the nature of the discipline in which they are engaged and as setting the appropriate standards according to which their work should be assessed, by themselves and by others. We may if we wish think of these people in Kuhn’s terms as “normal” scientists, people working within a scientific *paradigm*.

¹²² H.L.A. Hart, *The Concept of Law*, Oxford: Oxford University Press, 1961, p. 55.

It is not claimed here that this is, necessarily, how we should understand Kuhn's notion of a "paradigm". Margaret Masterman claimed that twenty-one different senses of the word "paradigm" appeared in Kuhn's book,¹²³ but this is not one of them. Rather, this involves a general point of understanding social institutions, whichever they are, and by characterising science as a social institution in this way we also characterise science as a *discipline*, like other disciplines. It is, however, true – if Kuhn is right to distinguish it as he does – that not all science is normal science, for there is also *revolutionary* science. Plausibly, this too would be social and in its own way rule-governed although no doubt with different rules; but we may wish to hold that there were much earlier scientists who are best seen as creative individuals who operated outside any community at all and so occupied no rule-governed role;¹²⁴ except that, in being seen as *precursors* by later scientists, they may be seen by later scientists as paradigmatic of their later role and so "adopted" by the later community. This, incidentally, *does* fall within Margaret Masterman's senses of "paradigm", for example her (4),¹²⁵ where it refers to a past scientist's achievement which is unprecedented and which attracts later practitioners.

Consistent with this, there is no such thing as observed *behaviour*.¹²⁶ Maurice Mandelbaum famously reduced to absurdity a simplistic observation-based social understanding: "Suppose that I enter a bank, I then take a withdrawal slip and fill it out, I walk to a teller's window, I hand in my slip, he gives me money..."¹²⁷ Could a person ignorant of the banking system understand these actions? You cannot go up to just anybody in the street and expect money in return for a withdrawal slip, not even the bank teller. The matter "is unintelligible unless one views their behaviour in terms

¹²³ Margaret Masterman, "The Nature of a Paradigm", listed at pp. 61-65.

¹²⁴ Much as Wittgenstein said "the philosopher is not a citizen of any community of ideas. That is what makes him into a philosopher", *Zettel*, eds. G.E.M. Anscombe and G.H. von Wright, trans. G.E.M. Anscombe, Oxford: Basil Blackwell, 1967, § 455, p. 81e. Whether a person can fail to be a member of any community of ideas, or fail to be a member of any community whatever, are philosophical-historiographical questions of considerable interest.

¹²⁵ Margaret Masterman, "The Nature of a Paradigm", p. 62.

¹²⁶ Contrasted, that is, with mere movement.

¹²⁷ Maurice Mandelbaum, "Societal facts", reprinted in *Theories of History*, ed. Gardiner, pp. 476-488 at p. 479.

of their status and roles”,¹²⁸ and an intelligible description of it is one which includes reference to such status and roles. This requires not only, as Mandelbaum rightly says, that one grasps the relevant social institutions in terms of appropriate “societal concepts”, but also that one recognises the *prescriptive* standards that a person, in successfully occupying such a role, thereby meets.

Just how “prescriptive”, in the sense of more or less “demanding”, these standards are, will vary with the role. For being a bank teller, the standards will be very exacting. For being a scientist, there will be much more flexibility. Quite what they are for being an “artist” would be highly controversial, but one could nevertheless *fail* to be an artist. On the other hand, *claiming* to be an artist might, for some artists or approaches to art, be sufficient. To describe a subject as a “discipline” will involve some degree of constraint which is higher than this. As above, the people involved in such roles need not meet the standards governing those roles by always acting in accordance with them, so making no mistakes; the relevance of the possibility of “mistake” is in any event a *contingent* feature of roles – necessary for some, but no doubt inessential for others. Rather, they meet those standards by recognising them as the appropriate standards – or even in principle as mere boundaries – which characterise and govern their work. The *descriptive* facts about how scientists behave are not simply “observed” but are complex constructions involving the meeting of *prescriptive* standards, standards for what we *count* as being a scientist (or a bank teller).¹²⁹

Given the appropriately sound historiography of science, the appropriate historical facts are available. But when Kuhn insists on making the Humean prescriptive/descriptive distinction, a different problem arises, namely, the apparent invalidity of the move from “this is what scientists do” to “do whatever scientists do”. Referring to Austin’s distinction, the conclusion “do whatever scientists do” is *both* prescriptive and descriptive because, like “do as I do” mentioned earlier, it implicitly involves a true description of what scientists do and also sets that as a standard for

¹²⁸ *Ibid.*

what others ought to do. The invalidity problem seemed to arise because the justification for this seemed to consist entirely of a premise, “this is what scientists do”, which is purely descriptive. But we can now see that this premise is wrongly characterised. To give a true description of scientific behaviour in this premise is to imply that the behaviour so described meets the prescriptive standard for what counts as scientific behaviour. The same argument for the meaning of “scientists” in the conclusion of the argument “this is what scientists do; so do whatever scientists do” applies also to the premise. In other words, the relevant prescriptive standard occurs in both premise and conclusion. A parallel argument to that now given for scientific behaviour will show an analogous conclusion for the other ways in which we might approach a philosophical understanding of science.

Now that does not warrant the validity of the argument: technically, nothing could. This is because logic does not allow any difference whatever between premise and conclusion without some further assumption or principle of deduction permitting it. But what the point does is to block the reason we had to think that an appeal to historical facts could not meet the minimum standard of plausible effectiveness which a justification has to reach. The point does so by blocking the Humean objection to so-called “descriptions” justifying so-called “prescriptions”, and it succeeds because descriptive and prescriptive elements unavoidably appear in both premise and conclusion. There is then no obvious reason of principle why the historical facts should not justify the prescriptive conclusion. The original problem arose because we presupposed John Austin’s distinction between law as it is and law as it ought to be, a distinction “so simple and glaring that it seems idle to insist upon it”.¹³⁰ Austin thought, in consequence, that law could be clearly analysed descriptively without any reference to prescriptive – that is, moral or any other evaluative – considerations. But what is it to “analyse” law “as it is”? Is it merely to *describe* in some unreflective empirical observational way, perhaps to sit in court with the camera running? As we have seen, that won’t do. We thereby miss, for example, that self-understanding of legal practitioners according to which law may be in part *constituted* by moral

¹²⁹ This is not to say that they are *conscious* constructions: just as we may act rationally although automatically, so we may be educated to just “see” something in such a way that it is *as if* we had worked through an elaborate construction.

considerations, by roles and practices which meet appropriate evaluative standards, and current philosophy of law builds on such approaches as it continues its move away from traditional legal positivism.¹³¹ Philosophy is itself affected by such points: Collingwood rightly said that “the question what philosophy is, cannot be separated from the question what philosophy ought to be”.¹³²

Historical facts can then in principle justify a prescriptive conclusion because they already embody some prescriptive element, namely the standards characterising some role that a person, in successfully occupying such a role, thereby meets. We have asked, why would facts about the actual behaviour of scientists, past or present, have any relevance to justifying a theory about how scientists ought to behave, given Kuhn’s insistence on the difference between these? The brief answer is that relevance is not precluded because there is no such difference. In so far as we now have no reason to regard the appeal to historical facts as failing to meet the minimum standard of plausible effectiveness, and in the contingent absence of any rival, this justification of appeal to historical facts – such as it is – may be taken to be self-evident. Further issues of validity depend upon the detail.

It might seem that there is a problem here. If Kuhn uses some theory of his own to specify what counts as being a scientist, that is, to specify what one ought to do in order to count as being a scientist, and Kuhn then identifies past scientists on this basis and writes his historiography of science accordingly, then it is no surprise that he is able to draw from his “factual” historiography support for his conclusion that scientists ought to behave in accordance with his theory. More generally, whether we agree with Kuhn or not on the detail, if as historians we impose some theory which specifies what counts as being a scientist and we then identify scientists on this basis and write our historiography of science accordingly, then it is no surprise that we are able to draw from our “facts” the conclusion that this is how scientists ought to behave. We

¹³⁰ John Austin, *The Province of Jurisprudence Determined*, Lecture V, p. 157.

¹³¹ See, for example, the works of R.M. Dworkin, who – in what seems to be a deliberate running together of the prescriptive and the descriptive – thinks that judges characteristically do and always should enforce individual rights. R.M. Dworkin, *Taking Rights Seriously*, London: Duckworth, 1977; *Law’s Empire*, London: Fontana, 1986.

¹³² R.G. Collingwood, *An Essay on Philosophical Method*, p. 4.

would – like Kuhn – get out no more than we put in. “Were this procedure correct”, said Leopold von Ranke (1795-1886), “history would lose all independence. It would be ruled simply by a proposition derived from pure philosophy and would stand and fall with the latter’s truth”.¹³³ If we imagined that appealing to such historical “facts” would *support* our conclusion that this is how scientists ought to behave, we were wrong, because we were assuming what we set out to prove. Lakatos is at similar risk: as we noted earlier, in his view the historiography of science has to support our theory of scientific rationality,¹³⁴ and yet we need that theory to write the historiography. Plainly an appeal to historical facts in this way, if this is what is going on, is circular and we have no second-order justification. The appeal has then merely speculative status.

Notice that, so far as this argument is concerned, the imagined imposed theory which specifies “what counts as being a scientist” is *neutral* as to the kind of theory in question. It may or may not, for example, be a theory of what counts as achieving scientific knowledge or a theory of what counts as justifying a claim to knowledge, which was the primary matter of dispute between Kuhn and Lakatos. Thus the circularity arises even if the “theory of his own” here speculatively ascribed to Kuhn is not a model of justification at all. In any event, it would be circular to impose a philosophical theory of justification in writing historiography, when expecting that theory to be itself supported by the historical “facts”.

But to suppose that this circularity must inevitably arise is an error. Notice that it is appropriate to think of the offices, roles and practices (including paradigm case practitioners) together with the rules or principles (or even metaphors and analogies) which specify or express them as a *model* which specifies a “standard” or range of standards which – broadly or narrowly – characterise the discipline. In a very informal social institution such rules, as noted earlier, may not be consciously recognised. Nevertheless, such a model (whatever its merits) can be, although it does not have to be, recognised and indeed adopted by the discipline in question as constitutive of the

¹³³ Leopold von Ranke, “On the character of historical science” (a manuscript of the 1830s, trans. Wilma A. Iggers), in Leopold von Ranke, *The Theory and Practice of History*, eds. Georg G. Iggers and Konrad von Moltke [1973], New York, N.Y.: Irvington, 1983, pp. 33-46 at p. 36.

self-understanding of the practitioners of that discipline, *qua* practitioners. Earlier it was noted that the descriptive facts about how scientists behave are not “observed” as such but are complex constructions involving the meeting of prescriptive standards for what we count as being a scientist. But who is the “we” here? In practice it is *scientists future to them*, rather than historians of science, who make the assessments of and decisions with respect to previous scientists in terms of such community-inclusion. Science, we might reasonably think, then advances, in part, by such hindsight decision. There is then no historiographical circularity of approach just because (and as appropriate to Kuhn’s position) it is later scientists, and not the historians of science, who decide whether a past figure meets the prescriptive standards in question. It is not the historian’s prescriptive standard which is being used to determine who counts as being a scientist, but *that of the practitioners of the discipline*. The historian may choose to adopt that standard, and commits no circularity of argument in doing so. On the other hand, if the historian does not adopt that standard but imposes some different one then circularity of argument is indeed a risk. To “impose” a theory in this imagined way is to apply a theory or characterisation contrary to that (or those) used within the discipline; in such a situation, there must be rival theories, so the imposed theory is not self-evident, not “innocent”, and justification is then required; hence the circularity, if the “facts” the discipline produces are themselves relied upon as justification. Theory used within the discipline, by contrast, is self-evidently justifiable to the practitioners of the discipline in so far as, and to the extent that, it in fact expresses the self-understanding of the practitioners of the discipline. Such self-understanding is in practice a complex matter: there may, for example, be agreement within the discipline on heritage or goals but not (say) on methods. There may be conflicting “schools” within a discipline. No one theory, in such a case, can be entirely self-evidently justifiable, nor can it in any event be supported by an appeal to historical facts which are themselves arrived at on the basis of that theory. The discipline’s self-understood model may in principle be pluralist in form.

¹³⁴ Imre Lakatos, “Falsification and the methodology of scientific research programmes”, p. 115.

From the historian's point of view, it is a contingency what those standards are which determine what counts as being a scientist, in addition to its being a contingency whether any particular past individuals or communities met those standards.

Distinguish here two different matters: scientists themselves may determine the scientific-community-inclusion of previous scientists on grounds which may or may not be good grounds; they may even be grounds which involve some circularity. On the other hand, historians of science may justifiably use those judgements to characterise the scientific-community-inclusion of previous scientists on the ground that they are or were, as a matter of historical fact, the judgements made. It will be a historiographical question how far some model of science is or was recognised and adopted at some particular period as constitutive of the self-understanding of the practitioners of that discipline, *qua* practitioners. If the practitioners tangled themselves in circular arguments, then that is a historical fact to be recovered, not something which necessarily affects historiography itself.

More generally, the philosophy of a discipline is in the first instance the historiographical recovery (continuous with our everyday reasoning) of the model (or models) characterising the discipline, of the rules or principles or paradigm prescriptive examples in terms of which the practitioners of the discipline conceive themselves to be operating or to have operated. Need there in addition be an external assessment by philosophers of the merits of this characterisation, for example by an appeal to objective "reason" or on other grounds? Does the justification of the "rules", self-evident to practitioners or otherwise, have to extend outside the discipline? Not necessarily, in general. In particular, it is a philosophical issue in the case of the philosophy of historiography just how far such an approach is either necessary or possible. It is not necessarily the case that a historiography-independent ground is available from which to do so – there may be no "view from nowhere".¹³⁵ On the other hand, always available will be *historical hindsight*: historically-situated philosophical assessment after the event, which is neither external nor independent.

¹³⁵ Thomas Nagel, *The View from Nowhere*.

This offers one interpretation of Hegel's observation, "the owl of Minerva spreads its wings only with the falling of the dusk".¹³⁶

Some similarity may be detected between this conclusion and the thesis advanced by Peter Winch in his widely known work *The Idea of a Social Science and its Relation to Philosophy*.¹³⁷ Based on Wittgenstein's later theorising about language, Winch saw social science and philosophy as both engaged in the recovery of rules. However, there are important distinctions which are worth making briefly here. First, nothing in the present argument implies or is intended to express a view about the relationship between historiography and any social sciences. Second, while the philosophy of a discipline may be the historiographical recovery of "rules", that does not imply that historiography in general is to be seen as the recovery of "rules". Third, it is not to be supposed that a characterisation of the philosophy of a discipline is also an appropriate characterisation of philosophy itself: the philosophy of a discipline may require the recovery of "rules" without philosophy itself needing to be understood as engaged in that practice in its core subjects. Fourth, the heart of the Wittgenstein-Winch argument is the view that language is a social institution and that language-meaning is to be understood in terms of rules; however, we adopt in our argument here no particular theory of meaning and it is not to be supposed that the recovery of rules is necessarily the recovery of meaning. Fifth, "rules" as referred to here are in any event not necessarily rules as originally understood by Wittgenstein, for "rules" is not a simple concept: in general they are understood here to be prescriptive in the sense of "standard-setting", but in different circumstances this can cover anything from a heuristic and metaphorical analogy to a system of logical axioms. While appropriately used in the case of a *team game* or even some *disciplines* where we might think of demanding and explicit standards which have to be adhered to, "rules" can also cover the mere expression of the boundaries of the characterisation of a subject or institution. The essence of the idea for our purposes is that there is something which would count as failing to be (a member of) the entity in question. There have been major

¹³⁶ G.W.F. Hegel, (*Elements of the*) *Philosophy of Right* [1820, original title page 1821], Preface. See edn. trans. H.B. Nisbet, ed. Allen Wood, Cambridge: Cambridge University Press, 1991.

¹³⁷ Peter Winch, *The Idea of a Social Science and its Relation to Philosophy*, London: Routledge and Kegan Paul, 1958.

philosophical advances in our understanding of the complexities here.¹³⁸ We need not be, and are not here, committed to some one view as to what are to be understood as “rules”, and this extremely broad interpretation needs to be borne in mind in what follows.

It clearly remains the case that, while one could not write a historiography of science without using some standard of what counts as “science”, “scientists” or the like, it is possible that later practitioners of science, or historians themselves drawing on those later practitioners’ self-understanding, may use a standard which, while recognised by the later practitioners of science themselves, would not have been recognised by the relevant past individuals counted as scientists. Past individuals may have used different standards or a different characterisation in conscious self-understanding, or indeed no standards at all, for we have already seen that there may be informal practices when rules are not consciously expressed, while some actions we might suppose to be entirely novel.¹³⁹ These differences, however, are again contingencies: when Kuhn and those of his opponents who share the appeal to historical facts bring such a standard to their historiography of science, they are not *necessarily* bringing something external or even alien to the self-understanding of past “scientists”, but something that might well be appropriate to and recognisable by those past figures in some form.

Earlier we noted that positivism involves insisting that there must be a single model of justification to be used in all contexts, and that the problem with it was not that it made all questions *scientific* questions but was rather its dogmatic insistence on the universality of a single position. Does the appeal to historical facts, here argued to be primary, amount to another version of positivism which makes all questions *historical* questions? We might speculate that epistemological issues would be central to our philosophical model of historiography, and that our model would then set a standard

¹³⁸ For example, R.M. Dworkin, “Is law a system of rules?”, reprinted in R.M. Dworkin (ed.), *The Philosophy of Law*, London: Oxford University Press, 1977, 38-65. For more advanced material on rules as they relate to meaning, see Alexander Miller and Crispin Wright (eds.), *Rule-Following and Meaning*, Chesham: Acumen, 2002.

¹³⁹ The possibility of this is itself a philosophical issue.

for historical knowledge.¹⁴⁰ Such a model, in so far as it were to differ from any model of science which justifiably did the same for science (although any such difference has not been implied or discussed), would require the denial of that science-based positivism claimed by such philosophers as the logical positivists.

However, denying that science is the only way of achieving knowledge is ambiguous in its force. It might seem natural to assume that what would here be denied by the present speculation is merely the claim that science is the only way of achieving knowledge, on the ground that historiography achieves it too. That, certainly, is the minimum implication. But, with the new-found epistemological confidence in historiography (better, in that everyday understanding with which historiography is continuous), which we speculate our philosophical model of historiographical knowledge might give us, what we might instead assert would be that science does not achieve knowledge at all. The essence of the traditional positivist claim was that there must be a single model of justification to be used in all contexts. For the traditional positivist this was a particular model of science, but we saw that the force of the position lay not with that but with the universality of the claim that there is only one way of knowing and so only one way of justifying. We might keep to this universal claim, and speculate that no philosophical model of science was as sound as our philosophical model of historiography. The more heroic philosopher might then assert the following epistemological claim: that historiography is the only way of achieving knowledge.

Would this in its turn be a dogma, as was the original claim about the centrality of science? Dogma or not, perhaps it is true; moreover it would not be a dogma at all, if it could be justifiably shown to be true. If so, and drawing on earlier argument, it could be justified in so far as it were self-evidently so: it would then have no rivals. Its main rival in the present argument – the appeal to science (here assumed to differ from historiography in its essential model) – would have to fail to meet that minimal, minimum and available standard of plausible effectiveness, achievement or acceptance which a discipline (justifiably constituted and understandable as such in terms of its

¹⁴⁰ “A” standard; or “the” standard, depending on the appropriateness of pluralism.

model) has to meet. Given that historical reasoning is continuous with our everyday reasoning about matters of fact, and situating our philosophy in that ordinary world which philosophers share with historians, a world in which science (albeit contingently) has such a powerful place, this is not a plausible outcome.

Yet perhaps we should not appeal to present-day plausibility too quickly here, for, just because the power of science is a contingency, such power can wane; the contemporary raising of questions (not raised decades ago) about the ethics of science might even be a sign of that (although we will later notice the ethical questions which arise in historiography). Religion had once vast power in the West, but throughout the present argument has not been conceived as a rival mode of understanding at all; it is no longer, in the largely secular academic world, widely seen even as a sound source for ethics.¹⁴¹ Perhaps this will change, or change back; but we are here piling speculation on speculation, and we need to constrain speculation by some respect for that which our audience and readership will find plausible. What is more plausible than a speculative historiography-based “positivism” denying that science achieves knowledge is that science and historiography are not rivals after all. There may be some shared and self-evident model. Still, self-evidence is relative to an audience or readership, and a contingent feature of any model. Historiography and science may both yet share the speculative fate of being passing fashions of understanding.¹⁴²

¹⁴¹ See *History and Theory*, Theme Issue 45, “Religion and History”, 2006, particularly David Gary Shaw, “Modernity between us and them: the place of religion within history”, pp. 1-9.

¹⁴² For the end of historiography, see Paul Veyne, *Writing History: Essay on Epistemology*, trans. Mina Moore-Rivoluceri, Middletown, Connecticut: Wesleyan University Press, 1984; for the end of history “itself”, see Francis Fukuyama, *The End of History and the Last Man*, New York: Harper, 1993.

Rival historiographies of science

Our concern has not been to discover which, if any, philosophical model is true of science, but rather with what it is for such a model to be “true” or “successful”, so that we can approach in an informed way our own question, what makes a philosophy of historiography “true” of historiography. We have seen that typical models of science have claimed to describe correctly in some summary form actual scientific thought or practice and have also claimed to set the correct standard for scientific thought or practice. There are rival models, and to claim that a model is “true” in these two ways is to claim that both description and prescription are appropriately justified. We have seen that, in so far as we can isolate the descriptive content of a model, the justification for that content is historiographical in form. In addition, in so far as we can isolate the prescriptive content of a model, we have seen that the justification for that content is also historiographical in form.

We have seen that it would be wrong to regard Kuhn as “falsifying”, in some elementary Popperian way, the standard empiricist model of science by checking it against what scientists “actually do”, against simple “facts” drawn from the history of science. There is no such thing as “observed” behaviour, and that is so for any historiography partly because behaviour, being typically in the past, has in any event to be recovered by historiographic means rather than by observation. However, it is also so, partly but more importantly, because, as we have now argued, historians have to use some approach to specify what counts as being behaviour of the relevant kind, and write their historiography accordingly. Justification for the historiographical understanding of science is not “factual” in some purely descriptive observational sense, but involves rule-governed classifying or “prescription”, so that such “matters of fact” are not to be seen as categorically different from matters of evaluative judgement. Kuhn, and those of his opponents who join with him in making the appeal to historical facts central, are not then making a general appeal to past facts, but are appealing to historical facts *about scientists*. Kuhn’s position did not and could not involve an appeal to prescription-independent “facts” from the history of science, but involved the development of a *model for understanding the history of science itself*.

Given this, the context of justification of a scientific model shifts to the historiography of science, so that the choice between rival models of science needs to be historiographically justified, recalling that “historiographical” justification is “continuous with our everyday reasoning about matters of fact”.¹⁴³ Earlier we distinguished between two different claims: first, the claim shared by Kuhn and his opponents (now allowably taken to be – as a matter of contingent fact – self-evident to the audience in question, but this can change), about the centrality of historical facts in second-order justification; second, the specific claim on Kuhn’s part, not shared with those of his opponents who wished to continue their opposition, about what the historical facts themselves were. The facts at issue between Kuhn and his opponents involved – but only as a matter of contingency about this particular debate – determining how far certain past individuals or groups (those who are, as it were, candidates for being counted as scientists) believed what they did for *good reason*. What were the “historical facts” about this?

Lakatos, building on Popper, offered a rival model to Kuhn’s: “The history of science has been and should be a history of competing research programmes (or, if you wish, ‘paradigms’), but it has not been and must not become a succession of periods of normal science”.¹⁴⁴ “Has been”; “should be”; we can here stress the very obvious double-sided descriptive and prescriptive elements in Lakatos’ approach, typical of the models we have already seen. As we earlier noted, Lakatos’ position is ultimately justified by him in terms of the claimed centrality of “our central intellectual values”, and “truth” for him is indeed a *value*, just as scientific knowledge itself came to be a value for certain historians at a particular stage in the development of historiography. If it is a value, then, if Hume is right, choosing as a value truth over what Lakatos saw as Kuhn’s truthless alternative is a matter of *taste*. Lakatos does not want that: defending “truth” against Kuhn, he wants “truth” to be the arbiter in the progress of science, rather than Kuhn’s presentation of some “mystical conversion”¹⁴⁵ to what might well be fiction. Lakatos wishes to rely on *reason* rather than taste in such first-order and indeed second-order contexts, but it is not that simple, even for him. Can

¹⁴³ Leon Pompa, “Truth and Fact in History”, p. 182.

¹⁴⁴ Imre Lakatos, “Falsification and the methodology of scientific research programmes”, p. 155.

¹⁴⁵ *Op. cit.*, p. 93.

reason supply a sufficient defence? Not easily: like all philosophers, he was unable to provide some ultimate justification for his position. “I would take my machine-gun to such a man”, said Lakatos on a later occasion to a questioner who described with elaborate care relativistic opposition to his approach, pushed to its extreme.¹⁴⁶

With this remark, Lakatos leaves us with an apparently unjustified choice between “reason” for belief and “mystical conversion”, a choice which he does not want us to have unless it can itself be made on (non-question-begging) rational grounds, and yet a choice which has to be made in the recovery of the historical facts at issue. How are we to decide, to justify a choice, between rival historiographies of science? It may be that we can express some self-evident standard according to which a judgement between these approaches might be made. Or it may be that, on examination, one of these approaches cannot meet some minimal, minimum and available standard of plausible effectiveness, achievement or acceptance, and the other approach is then self-justifying at least in the sense that it is innocent until proved guilty. But it is not appropriate here to engage directly in those questions in the philosophy of scepticism or relativism which are raised by the historiography of science.

To make better sense of the truth of a model of historiography in general, in the light of such rival models in the historiography of science, we need here to recognise that the issue of *choice* in historiography is not limited to Lakatos’ problem and has a much wider relevance. Throughout our discussion we have seen various choices which need to be made and we have sought to illustrate criteria which might be used in the decisions relating to those choices. Lakatos and Kuhn each had to *choose* how to write his historiography of science: he had to *choose* what questions to ask and *choose* what counted as answers to his questions and *deliberate* about what the facts were. Each made philosophical assumptions in choosing as he did, and chose those assumptions too, and the “facts” each produced (which Kuhn in particular used to “disprove” the standard philosophy of science) were partially a product of those assumptions. Each brought a model of science (albeit not the standard model) to writing his historiography of science, and what each got out was at least partly a

¹⁴⁶ at a History and Philosophy of Science Seminar, Cambridge, 27th May, 1971.

function of what each put in. There was an interplay between philosophical assumptions and putative facts here (and Kuhn in particular was not aware of all the detail involved in his own position).

It may be that the “facts” in the historiography of science which are at issue between Kuhn and Lakatos involve some relativistic indeterminacy at the level of ultimate philosophy, given the first-order epistemological concerns about science which they share, but there is no *a priori* reason to suppose that such problems affect the appeal to historical facts as second-order justification, for that we may already take to be (contingently) self-evident, nor that these problems affect such first-order models of historiography as do not deal with matters of epistemological concern. The historiography of science has, in our illustration, taken the form of following an epistemological interest, but in principle a quite different historiographical interest might have been chosen.

Where there is choice, there is judgement. Where there is judgement, there is discretion. But just because one makes choices does not mean that the choices made are arbitrary or that the discretion is unlimited. Historiography – and not just the historiography of science – is shot through with choices, and decisions between alternatives have to be made in many different contexts. It is *relativism* in support of *scepticism* which suggests that such decisions are essentially arbitrary decisions, but to understand science or historiography in terms of the making of choices is not of itself to commit one to doubt, for determinate criteria for the purpose may be available. While we ought to think of the practice of historiography in general as inevitably involving choices, if only in important part, we ought not to hold as a mere dogma that decisions between those choices cannot justifiably be made.

In any event, *notice that it is a contingency which choices do need to be made.* While the choice between Lakatos’ “reason” and “mystical conversion” in the historiography of science may be highly problematic and go to the heart of philosophy, not all historiographical choices are like that. We need to investigate historiography to show in more detail where choices arise and to isolate the kinds of choices that historians characteristically face. We need to investigate those choices and clarify the criteria

involved in the decisions made. We may recognise that *science* is a complex factual activity where, for contingent and particular reasons, epistemological issues have commonly driven the historiographical models used to understand it, but it is not appropriate to assume without further argument or evidence that such issues are also central to any understanding of *historiography*, and indeed, while we have assumed that historiography is the appropriate approach for the recovery of historical facts, we are not in a position to claim that the essential characteristic of historiography is, in general, that it is primarily a *factual* activity. We need to find out.

Chapter 3 Writing the History of Historiography

This chapter has the title it does, rather than merely “the history of historiography”, because setting about that writing requires, first, five sections of discussion of the necessary preliminaries before we finally achieve success in the last two.

Historiography of historiography: prior considerations

We may here summarise our position, which has involved generalising from the philosophical modelling of science to the philosophical modelling of historiography, in terms of some main elements as follows:

1. The philosophical model of a discipline is typically offered as being both descriptive and prescriptive.
2. Second-order justification is necessary for us to accept a model of a discipline as a true model, regardless of that model’s subject-matter (that is, it is irrelevant whether the model in its first-order context is itself a model of justification).
3. Generalising from the post-Hempel discussion in the philosophy of science, where philosophical opponents shared the appeal to historical facts about science, we regard the appeal to historical facts (being continuous with everyday understanding) as also the appropriate second-order justification for the philosophical modelling of historiography. It was self-evident to the parties to the post-Kuhn discussion that this historiographical appeal should be made, but that it is self-evident is a contingency relative to the state of our understanding.
4. In recovering historical facts about a discipline, the prescriptive and the descriptive are necessarily linked.
5. The philosophy of a discipline requires the historiographical recovery of the model characterising the discipline (recalling again that historiographical recovery is continuous with everyday recovery, and recalling again that the model may be pluralistically understood).
6. A model externally imposed on the discipline risks circularity, ungrounded speculation, and does not show the necessary respect for the discipline.

7. The philosophy of a discipline requires the historiographical recovery of the model or models which the practitioners of the discipline conceive as characterising their discipline and under which they conceive themselves to be operating.
8. Decisions between choices are inevitably made in that process of recovery.

We wish to engage in the philosophy of historiography. For this, we need in the way explained to recover historians' self-understanding of their discipline. Since that recovery is to be historiographical, we need, for this purpose, to engage in the historiography of historiography. Taking for granted, as we now may, that this appeal to historical facts is (albeit contingently) self-evidently an appropriate move here, and just as we have argued that the philosophy of a discipline is in the first instance the historiographical recovery of the prescriptive and descriptive character of the discipline, of the "rules" under which the practitioners of the discipline conceive themselves to be operating or to have operated, it is now appropriate for us to begin that engagement with the historiography of historiography.¹⁴⁷ It is a further essential part of our own approach to the historiography of historiography that we must show philosophical respect for the discipline of historiography by attempting to avoid the external imposition on the discipline of some externally created model. It is *historians'* understanding of what is characteristic of their discipline which counts, not some philosophical standard for that.

Because historiography is (at least since the nineteenth century) a discipline, and the historiography of historiography is a special case of that discipline; and because historians are exactly those people who are the paradigmatic practitioners of that discipline, it seems appropriate that we should move as rapidly as possible to ask historians themselves about the historiography of historiography. However, there is a problem with this, which is that, as we shall see, historians characteristically do not appeal to the historiography of their own subject in order to express its characteristics. Historians' historiography of historiography is thus only of indirect value to us. Before dealing with this issue, there are some further points which it is appropriate to explain first in clarification of the position now reached, and we will display as we

proceed some of the main choices we have to make in our engagement with the historiography of historiography.

In accordance with our approach, we take historians themselves to determine the historian-community-inclusion of previous historians. Earlier we noted that, with regard to the history of science, scientists themselves determined the scientific-community-inclusion of previous scientists, and that the reasons for their doing so might (in principle) be circular or poor in some other way. Despite this, historians of science might justifiably use those determinations to characterise the scientific-community-inclusion of previous scientists on the ground that they are or were, as a matter of historical fact, the determinations made. In doing this, historians of science were not themselves reasoning poorly even if the scientists in question were. By contrast, it might be thought that when historians write the historiography of their own discipline the risk of circularity is much greater, but that is not so. Historians themselves are paradigmatically authoritative as to what their self-understanding is with regard to the nature of their discipline and, parallel to the earlier argument about the historiography of science, it will be a historiographical question how far some particular approach to history is or was recognised and adopted at some particular past period as constitutive of the then self-understanding of the practitioners of their discipline. It remains logically open for us – either as philosophers or historians – to evaluate the merits of the self-understanding involved, without denying that such-and-such is indeed the self-understanding involved. Whether to evaluate such merits, however, would be a contingent philosophical choice for us to make.

We have said that it is the *historiography* of historiography that is required for the recovery of historians' own understanding of that which is characteristic of their discipline, and we have drawn that conclusion from an analogical argument which generalises from the philosophical modelling of science. Deciding to approach the matter in that way is one of our early first choices. In so far as historians are conceived to be practising a rule-governed discipline, we might then think that some social science, rather than historiography, is the appropriate discipline to use for the

¹⁴⁷ I called for this in my "From History to Justice: Understanding Philosophy of History", in *Essays*

purpose of recovering their self-understanding. However, to deal with a subject *historiographically* is not to deal with it to the exclusion of other disciplines or approaches, for we have also argued that historiography should not be understood to be exclusive in this way. All disciplines – typically, the social or psychological sciences, but in principle anything from geology to physics – are available for the historian to use. The historiographical “appeal to historical facts” is there to *expand* the base for our understanding, not *reduce* it.

To choose to deal with the historiography of historiography could be, since historiography is continuous with everyday understanding, merely a matter of looking at present-day historiography. For us, a further decision is involved: it is that we should deal with historiography both past and present. On the basis of argument already given, it would not be appropriate to adopt some approach which deals with the present to the *entire* exclusion of the past, for recall that past and present are understood in the present work to be *continuous* with each other. To deal with historiography is then to deal with it over time. As so often where historiography is concerned, in writing the historiography of historiography we face a choice about the time frame we choose. Historians and historiographical work do in fact have a past – we are not looking at some randomly associated set of people who happen to be alive today, or only at those practices which can be observed in the present. We may suspect *a priori* – indeed, if we forget philosophical caution, it is obvious – that the present association of such people or practices is due, if only in part, to their joining some ongoing academic tradition, so that a full understanding of that discipline would involve delving into its past. But, in addition to the point that we must not assume that our main business is to understand the present, we also must not assume that the past would be needed in order to understand the present. A better philosophical point is this, that if, in order to understand historiography, we deliberately select historians or historiographical work from one part of the past-present continuum to the exclusion of selecting from another part, then we are selecting in an arbitrary way. If proper distinctions are to be drawn between various parts of the past-present continuum, so far as the understanding of historiography is concerned, then that should be a

conclusion of our enquiry, not a premise. For us, the argument now given is an argument requiring that we cover a long time frame rather than only the present or recent past. Indeed, we should start, in our search for historians' self-understanding, as far back as we can go. By refusing to restrict our range of enquiry to (for example) only the present or some period of the more recent past, we ensure as best we can that we will not miss matters of importance, foreclose their discussion, or close down possibilities with which we ought to deal. We will not be disadvantaged by this move if it should prove, in due course, that we had not needed to select from so wide a sample.

Generalising from the philosophical modelling of science to the philosophical modelling of historiography, we hold it to be appropriate, in order to understand and model a discipline, to appeal to historical facts about that discipline. Hence, we have said, it is appropriate to engage in historiographical recovery. It is implied by this that historiography is an appropriate approach to use for the purpose of recovering the historical facts about a discipline. Recall that we have held that such historiographical recovery is continuous with everyday understanding, it is often "common sense",¹⁴⁸ and permits the use of many other disciplines. For us also, however, "historiography" is itself the name of the discipline we are concerned with, and we earlier pointed out that we still need to find out whether historiography is, in general, best characterised as a *factual* activity at all. It may seem that we are begging one of our central questions, by assuming that historiography involves factual recovery while at the same time claiming that we have still to find out whether historiography involves factual recovery. That, however, is not the situation. Our interest, by recovering the historical facts about historiography, will enable us to discover how far historiography is *characteristically* a factual activity. To hold that historiography, conceived as continuous with everyday understanding and permitting the use of many other disciplines, enables the recovery of historical facts, is not necessarily to hold that the best way of characterising historiography as a discipline is to see it as primarily and centrally a factual activity. In principle, it might, in essence, be something else.

¹⁴⁸ G. Kitson Clark, *Guide for Research Students*, p. 9.

Our primary sources

Our engagement with historiography involves a choice of method. Because for us historians are “authoritative” with respect to their understanding of the character of their discipline, it is initially plausible (although in need of analysis) that we should unquestioningly and indeed unashamedly adopt what may have the appearance of a “scissors-and-paste” approach, as Collingwood disapprovingly expressed it: “It was necessary to construct a patchwork history whose materials were drawn from ‘authorities’, that is, from the works of previous historians who had already written the histories of particular societies at particular times”.¹⁴⁹ The word “authorities” here is placed in scare quotation marks by Collingwood because these previous historians are not, for him, fully *authoritative* at all. “Scissors-and paste” historiography is – as most historians today have learnt from Collingwood, directly or indirectly – “bad” historiography, for the “good” historian should go direct to “the sources themselves” (documentary archives, artefacts and the like) rather than to “other historians”.

If we were writing about, say, the fourteenth century, it would be “scissors-and paste” merely to draw our material from what other historians had said about the fourteenth century, that is, it would be treating what those other historians had said as authoritative on the matter, when, for Collingwood, they are not. We should instead be looking at material which, typically, has been preserved from the fourteenth century. A presupposition of Collingwood’s here is that these preserved “sources”, by contrast with the “other historians”, *are* more authoritative. We need not question this at this point. However, it might be thought that this suggests that the preserved sources are in some way self-evidently true, almost as if they came ready-labelled with the “facts” to which they attest, but that is not the case. The status of sources as “authoritative” needs to be shown, case by case.¹⁵⁰ In general, however, historians themselves are

¹⁴⁹ R.G. Collingwood, *The Idea of History*, p. 33.

¹⁵⁰ What sources are “authoritative” of, and how this is worked out, is a complex matter. Collingwood requires empathetic understanding. Aviezer Tucker has a different view: historical propositions are justified as such by being the best explanation of present evidence, and it is an essential part of some historical proposition being the best explanation of present evidence that there is transmission of information, a causal chain, connecting the two. The “authority” of a source would be intimately bound up with this best explanation/justification reasoning. “Historians are interested only in particular types of causal chains, the ones that preserve information”, Aviezer Tucker, *Our*

understood to be authoritative only in so far as what they say is attested by the sources they have used, as appropriately interpreted, and it is not plausible to suppose that historians might validate each other without reference to any other sources.

We affirm that historians widely accept Collingwood's distinction here as one of the features of historiography which is characteristic of the discipline. The distinction is sometimes expressed differently and commonly thought to be the same as that between "primary" sources – the original documentation (perhaps contemporary accounts of an event, but often contemporary records of a quite different kind, such as letters, reports of judicial proceedings or lists of a monastery's purchases) – and "secondary" sources, such as works, typically by historians, which interpret or evaluate and build on the primary sources. As G. Kitson Clark describes it, "the secondary is the fruit of other men's thought based on the results of other men's work, the primary is the direct evidence turned up by your research which will be the basis of your own work".¹⁵¹ Both primary and secondary sources have to be interpreted or evaluated in so far as they are used by historians, but the lesser "authority" in the use of a secondary source derives from its being an interpretation of an interpretation. Whatever the relationship between the sources and historical "reality" may be, the characteristic historiographical view is that a secondary source is one step further removed from that reality.

It may be noted, in passing, that this position suggests that a *realist* metaphysics is involved in this characteristic historiographical view: that is, the idea that "reality", and in particular (for the historian) *past* reality, exists independently of any external human input or interpretation.¹⁵² (The "reality" itself, of course, will typically contain humans.) In our earlier section "respect for historiography" we referred to such independence and objectivity of reality in terms of Thomas Nagel's expression, the "view from nowhere",¹⁵³ but recall that such a view may not be philosophically available. If an independent "reality" is not intelligibly available to us, then a different

Knowledge of the Past, p. 94 and *passim*. While the preservation of evidence is crucial for historiography, and while it is philosophically plausible that causal chains are involved in that, it is philosophically controversial what "causal chains" are, and also controversial to hold that such chains are the *only* ones (if any, indeed) that historians are interested in.

¹⁵¹ G. Kitson Clark, *Guide for Research Students*, p. 42.

¹⁵² Is realism *really* characteristic of historians? See the end of the section "A fancy view of truth".

¹⁵³ Thomas Nagel, *The View from Nowhere*.

way must be found for distinguishing between primary and secondary sources. In so far as both kinds of source are matters requiring interpretation, then, as we will see later, justifying that distinction, even if it can be pragmatically made, will not be philosophically easy. Nevertheless, the distinction is *characteristic* of historiography, and easy for historians. “The distinction between primary and secondary sources on the whole has survived the withering theoretical hail rained down upon it by the postmodernists”, says Richard J. Evans,¹⁵⁴ implying that historians have not yet appreciated the force of antirealist arguments. Nevertheless, we shall assume such realism as is characteristically historiographically appropriate in the argument at this point.

For convenience and clarity, we will here use the expression “past historians” to name the subject matter of a historiography of historiography, and use the expression “other historians” to name the authors of such historiographies of historiography. “Other historians” are named “other” in order to contrast with “we ourselves”, where the “we” refers to the author of this book together with such readers as are sharing the construction of our meaning. One noticeable but weak implication of “other” here is that we ourselves are to be conceived as historians, at least in so far as we are engaging in our current analysis and in due course dealing with “past historians”.

Is the present book a work of historiography? We have already argued that to deal with a subject historiographically is to deal with it in a way which is permissively inclusive of other disciplines: *all* disciplines, we have said, are available for the historian to use.¹⁵⁵ That then, in principle, includes philosophy itself. The claim that historiography can use philosophy does not make historiography indistinguishable from philosophy, for the “inclusion” of philosophy here means only that historians can *consume* the relevant parts of philosophy; it does not mean that they do or can *produce* philosophy. The claim then involves a *contingent* connection between historiography and philosophy. We leave undetermined the possibility, so far as the

¹⁵⁴ Richard J. Evans, *In Defence of History*, p. 126.

¹⁵⁵ It is left open here whether historiography uses those disciplines in some special way; we are not assuming that historiography cannot be distinguished from other disciplines. The matter is clarified a little further below.

present argument is concerned, that historiography and philosophy might be essentially rather than merely contingently linked.

However, the weak implication that we ourselves, in virtue of such connections, are to be conceived as historians should not at present be drawn and this is another issue which should be left open: it remains to be seen how far this “we” (in so far as we are analysing as we are and in due course dealing with “past historians”) is itself to be properly characterised in such a way as to show that we are here engaging in the practice of historiography, and moreover may thereby be properly characterised as historians. A more natural way of understanding this “we”, at present, is to think of it instead as referring to ourselves as *philosophical observers* of historiography. The contrast between these two understandings – we as historians, we as philosophers – may, however, only be apparent, because we have already argued that the exercise of “philosophical observation” in this context *requires* the appropriate exercise of historiography, namely, the historiographical recovery of the model or models which the practitioners of the discipline conceive as characterising their discipline and under which they conceive themselves to be operating. Requirement, however, does not make identity. Further reflection on “we ourselves” is postponed.

We include in principle, in the range of reference of the expression “past historians”, *present* historians, so recognising that present-day historians can themselves be the subject matters of historiographies of historiography. We further allow, but similarly do not otherwise make explicit, that the subject matter of historiographies of historiography might be historiographical practices or other characteristic historiographical features as well as or instead of past or present historians themselves. With these various points now made explicit, we will take them for granted as implicit when we say that we will use “past historians” in naming the subject matter of a historiography of historiography, and use “other historians” in naming the authors of such historiographies of historiography. Using this simple terminology, Collingwood’s “scissors-and paste” versus “direct from sources” distinction can now be applied to the historiography of historiography. The contrast – in elementary principle – initially shows in the historiography of historiography in the following way: we would be writing “scissors-and paste” historiography of historiography if we wrote

the historiography of historiography by patching together what “other historians” have said about “past historians” in their historiographies of historiography. By contrast, it would be a “direct from sources” approach if we were ourselves to write that historiography of historiography by examining directly in an appropriately historiographical way the work of past historians. It is the choice between these two approaches which it seems we have to make in our engagement with historiography.

We have already suggested that we should adopt Collingwood’s “scissors-and-paste” approach, just because and in so far as we are taking historians, as paradigmatic practitioners of historiography, to be “authoritative” of their understanding of the character of their discipline. We do this in order to avoid any inappropriate imposition of philosophy on historiography. Moreover, it is appropriate to think of ourselves as historiographically untrained: we are eager to defer to historians. For the historiography of historiography, “other historians” *are* for us “the sources themselves”. However, this needs analysing. Think of a simple single instance of the historiography of historiography, namely one historian A writing about another historian B, whom we may imagine to be a historian who writes about the fourteenth century. We, “philosophically observing” this and imagining ourselves to be adopting the “scissors-and-paste” approach, assume that A is authoritative about B, so that we read A in the original and uncritically take A’s work on B for granted. What does it mean to take A’s work here for granted? It would not be a matter of taking for granted the merits of A’s methods or acknowledging the respect or professional status that A might enjoy. The essence of the authoritativeness is that we take A’s work as *authoritative about B*. That is, we uncritically take A’s account of B’s work to be *true*, and true, in particular, *about B* rather than about the fourteenth century.

We ignore many features about A as a historian in doing this. Notice that, as philosophical observers, it is not appropriate for us to examine, in particular, A’s historical methods or use of primary sources in order to satisfy ourselves that what A says about B is true. To do that would be ourselves to engage direct with the primary source and would not be “scissors-and-paste”. We simply read A, for it is authoritative for us. We plainly are to take no view about whether B’s account of the fourteenth century is true, whether A says that it is or not, for we are not interested in

the fourteenth century. We are not to look at B in the original and see for ourselves, in any event. We only look at A in the original. After all, it is assumed, *we* are philosophers and not historians, and so have to take for granted A's account of B.

Is it not arbitrary, for us so to read A in the original but not B? Not at this point of the argument, for we are not searching for knowledge about the fourteenth century but for knowledge about B in B's own appropriate historical context, that is, about B as the subject of a historiography of historiography. We read A because A is authoritative about B. But a typical "A" writing about a typical "B" would be more likely to be disputing with B some feature of the fourteenth century. There is then no point in reading A about B, if B says or implies nothing about the character of the discipline of historiography and writes only about the fourteenth century, and if in any event A draws no relevant conclusion about B's view about the character of the discipline. We can't be sure that an "A" would write the historiography of historiography in a way that is helpful to us. Not just any historiography of historiography will do. The "A" we need has to be one who offers us B's understanding about the character of the discipline. Only such an "A" will do.

Suppose, then, that B, in passing, does go beyond writing about the fourteenth century and expresses or implies something about B's own understanding of the character of the discipline of historiography. At last there is some point in reading an A who reports that part of B to us. But in that case, it does appear arbitrary for us not to read B in the original. Surely it is plausible to suppose – just because we are taking historians to be authoritative about such understanding – that we should not exclude such input from any historian. Surely it is not correct that we should hold that some passing expression by B of B's understanding of the character of the discipline of historiography is only authoritatively true for us if A says that it is, but is not authoritatively true for us if B says that it is.

Moreover, a second reason now arises not to read A, since A is not expressing A's understanding of the character of the discipline but rather B's. Is A more authoritative about B's understanding of the discipline than B is? Surely not. Why then read A? Furthermore, is anybody else more authoritative about A's understanding of the

discipline than A is? And if A is not expressing A's own understanding, then again, why read A? Both A and B seem to be authoritative about their own understanding, in so far as they express it; and surely we should then go to A's and B's writings in the original, treating them both as primary sources which express authoritatively their own understanding of the character of the discipline, again, in so far as they express that.

Because we must treat both these historians as authoritative sources, we are in one simple sense engaging in "scissors-and-paste"; but because they are primary and not secondary sources for our purpose, we are in another and more important sense not engaging in "scissors-and-paste". Indeed, surely we do not want secondary sources here, for if A expresses understanding of the character of the discipline then we should read A, and if B expresses such understanding then we should read B; and we would not wish to be one step removed from the "reality" here by perversely insisting on reading only B about A and A about B; or reading C about both of them. There has been an implied mapping so far, between the "primary sources"/"secondary sources" distinction and Collingwood's "sources themselves"/"historians as authoritative" distinction, but this mapping breaks down where the historiography of historiography is concerned, quite apart from the separate question whether these distinctions are at all appropriate for our understanding of historiographical method. Respect for historiography means that we must take historians as authoritative primary sources for our own historiographical recovery of the discipline.

Given that some historians express understanding of the character of their discipline, it may now appear that we should read them (in so far as they express that understanding) in the original as primary sources, and not allow ourselves to consider historians' historiography of historiography at all, for that would seem to permit a possible conflict, in illegitimately giving us two sources of material for our purpose: for example, primary source B, and (apparently a secondary source) A's account of B. A's account of B – the historiography of historiography – may now seem inappropriate for us to use, when we think that B's own expression of understanding of the character of the discipline (assuming B, as a historian, is engaged in that) is more authoritative than is A's about B. Yet, while B would indeed be authoritative *about B's understanding* of the character of the discipline, that is not the same thing as B being

authoritative about *the character of the discipline*. We cannot take B as authoritative of the character of the discipline if A's own understanding of the character of the discipline – perhaps expressed in A's account of B, perhaps in some other account by A – is different from B's. It is, then, appropriate for us to consider, in addition to those historians who directly express understanding of the character of their discipline, historians' historiography of historiography in so far as it expresses comment on that character. The difficulty we will find is that historians' historiography of historiography rarely does this, and, in so far as that is so, historians' historiography of historiography turns out *not* to be relevant to our enquiry.

When we seek to recover the model or models which “the practitioners” of the discipline conceive as characterising their discipline and under which they conceive themselves to be operating, we find an ambiguity between “the practitioners” conceived as individuals and “the practitioners” conceived as a group. We will not succeed in our task of recovering the character of historiography *as a discipline* if the outcome of our work were merely a list of different views on the part of different historians. We seek views sufficiently shared to amount to a *consensus* on the character of the discipline, on what historians count as being a member of or a contributor to the discipline, on what the “rules” are which specify this, such that a historian can in principle be characterised by other historians are *mistaken*. Historians may be relevantly “mistaken” here in the following way: we may think, for example, of the discipline's rules as specifying historical methods, such that a historian is mistaken when drawing historiographical conclusions through ignoring or misapplying those rules.

But we are not here thinking about historians being mistaken *about the character of the discipline*; on the contrary, it is only in so far as they share a view about the character of the discipline that historians recognise historiographical mistakes in others, and in themselves by comparison with those others. Historians may, of course, have different views about the character of the discipline. Inevitably, there will have to be some evaluation of the merits of some of those views if they differ from others. *But we should not do that as philosophers*. It is historians' own evaluations which will have to count for us, and this *does* require that we use as primary sources their

historiographies of historiography, again in so far as they are suitable for that purpose. Yet, once again, that is limited. While historians' historiographies of historiography will typically display who is taken to count as a historian, the discipline is rarely characterised in such a way as to justify those judgements, beyond the most elementary of considerations.

Hence, our current choice as we engage with historiography is not so much whether to adopt a "scissors-and-paste" rather than a "direct from sources" approach, but rather how to select our primary sources. We need to select that range of historians who express their understanding of the character of the discipline, and look at those in the original. But we should not look in the original at the work of those past historians who do not express their understanding of the character of the discipline and try to recover it for ourselves, for that is essentially a historiographical and not a philosophical task and we must defer to historians on this. We do this by taking as authoritative historians' historiographies of historiography, in so far as they provide this. However, that provision is rare. The outcome is that historians – but only some of them – are, for us, both primary and authoritative sources.

In approaching this limited range of primary sources as we do in search of the solution to the questions we have, we are engaging in a characteristically historiographical move. As such, it is after all *we ourselves* who are thereby writing the historiography of historiography we need. We ourselves *have* to write this historiography of historiography,¹⁵⁶ in order to produce the philosophy of the subject. Yet, like all (we will not here say, other) historians, we are limited in our range of sources and in the questions that interest us. Like all historians, we have *chosen* the limits of our range of sources. Our choice here, it may be noted, has been reasoned for: characteristically for historians, it is not arbitrary.

It is no surprise that we have reached the conclusion that we are, after all, ourselves having to recover historians' understanding of their subject and hence ourselves having

¹⁵⁶ I asserted this claim in "The truth of historical theory", *Storia della Storiografia* 48, 2006, 38-48. The "have to" is contingent: in principle, a historian might already have duplicated the approach adopted here, in which case that work should be taken as an authoritative source.

to write the required historiography of historiography. Think of the historiography of (say) the fourteenth century, in an analogous way to the approach used in our earlier sections on “justification in the second-order context”, as *first-order historiography*. Working with the characteristically historiographical “realism” which we have already noted and assumed for the immediate argument, first-order historiography is an approach to a “real” subject matter,¹⁵⁷ just as science is an approach to the “real” world; and it is, indeed, the same world. There are, of course, myriad other first-order historiographies, since the world is a big place: for example, of the nineteenth century; of Russian history; of art; or the economic historiography of a range of subject matters.¹⁵⁸ The historiography of first-order historiography involves distancing oneself from these first-order historiographies in the sense of distancing oneself from their particular subject matters, and presenting the historians themselves, their methods and the like in some appropriate overall historical context,¹⁵⁹ thereby engaging in what may readily be understood to be *second-order historiography*. Although rarely suitable for our purpose, there are a number of works of historiography of historiography. These too will have a history. One could write the relevant historiography of this, and one would then be writing the historiography of historiography of historiography. In principle, second-order, third-order and so forth could go on to infinity, but there is no vicious regress involved, for one can stop where one chooses.

Note that presenting the historians themselves, their methods and suchlike in some appropriate overall historical context is not the same thing as presenting the entire range of the *subject matters* of their first-order historiographies in some appropriate overall historical context: we can imagine a universal historiography in which a heroic historian sought to weave the nineteenth century, Russian history, the history of art, economic history and all the other historical subjects there are into a single coherent

¹⁵⁷ How “real” should be understood is not a relevant consideration at this point, but it is a philosophical issue which lies at the centre of our understanding here.

¹⁵⁸ Here “reality” comes labelled in nations or periods, and so forth; but much the same happens in science. We do not intend here to suggest that “reality” is raw of human input. That is a major philosophical issue which lies at the centre of our understanding.

¹⁵⁹ It need not be with the intention of recovering those historians’ understanding of the nature of their subject.

account. That would still be first-order historiography. By contrast, the historiography of historiography is second-order historiography.

In our earlier sections on “justification in the second-order context” we distinguished (for the sake of example) between first-order models and second-order models. First-order models of science were – contingently – those which sought to justify science, while second-order models were those which sought to justify, or justify our choice between, first-order models. These second-order models were, typically, “philosophical” models. We noted that it is traditional to conceive philosophy as paradigmatically the discipline which deals with second-order contexts: “bracketing off” an area of thought, and standing outside it so that it can be examined from some dispassionate distance, is for many philosophers (though here expressed in metaphor) the essence of philosophical activity.¹⁶⁰ It will not be missed by the reader that the historiography of historiography itself is, on this view, a characteristically philosophical move.

A question arose as to which of these “philosophical” second-order models to choose, should there be more than one, and yet further criteria were sought for this. Such standards, we explained, would be standards of *third-order* justification, standards which would enable us to justify selecting one philosophy rather than another. “Metaphilosophy”, we saw, seemed to exist as an appropriate third-order discipline for this purpose, but unfortunately it exists as a branch of philosophy itself, so that no clear distinction is to be drawn between second-order and third-order contexts. It is not possible to escape the second-order context by moving to the third-order, for the very same considerations re-arise in the third-order context. The distinction between second-order and third-order breaks down, we concluded.

For clarity of understanding in our present situation, which has to do with the historiography of historiography, we may, as earlier explained, take the subject matter of “other historians” to be “past historians”. The subject matter of “past historians” will be things like “the fourteenth century”. We have now presented historiographical

work such as that of “past historians” on the fourteenth century as first-order historiography, and we have presented historiographical work such as that of “other historians” on “past historians” as second-order historiography. *With respect to the fourteenth century*, which we may for a moment take as the “reality” in question, that distinction is perfectly clear. But it is also clear that the distinction between first-order and second-order contexts of historiography is a relative and not an absolute distinction, for a historian who writes the historiography of historiography is thereby engaging in *first-order work* on past historians, even if that work is at the same time second-order with respect to the subject matters of those past historians. Briefly, and with a different example, second-order historiography of first-order Russian historiography is at the same time first-order historiography of historians of Russia.

Thus, in so far as we suppose that “past historians” are engaged in first-order historiography about some “real” subject matter, then “other historians” may be supposed, relative to that subject matter, to be engaged in second-order historiography when they write the historiography of historiography of those “past historians”. But the “past historians” are themselves a “*real*” subject matter for those “other historians”, so that the “other historians” are *at the same time* engaged in first-order historiography. The historiography of historiography is itself a branch of historiography; as such, it is a first-order discipline as well as a second-order discipline. *Our* subject matter has a similar status, as follows: on the one hand, we are proposing (for reasons already given) to present from the original the positions of those historians who express their understanding of the character of the discipline. We thereby engage in first-order historiography. On the other hand, we are also proposing to present from the original the positions of those few historians who write what is for us the appropriate kind of historiography of historiography, that which recovers appropriate understanding of the character of the discipline. Here we engage in first-order historiography with respect to the writers of that historiography of historiography while, in taking those writers as authoritative, engaging in second-order historiography with respect to those historians about whom they write. Yet the writers of that historiography of historiography, while engaging in first-order

¹⁶⁰ The expression “bracketing” comes from Edmund Husserl (1859-1938), but the move is often

historiography in taking historians as their real subject matter, are at the same time, as already seen, engaging in second-order historiography relative to the subject matters of those historians about whom they write. In so far as they are, in that way, engaged in second-order historiography, then we, presenting them, are inevitably engaged in *third-order* historiography, relative to that situation which makes their work second-order. There are thus no absolutes with respect to what counts as one level and what counts as another: just as with various orders of models of science, so these historiographical orders collapse into each other. The upshot is that, whatever else we are doing and whatever other levels of order may also be involved, we are inevitably engaged in first-order historiography. As we have now seen, we have ourselves to write the historiography we need, taking historians as authoritative as appropriate, that is, taking them as primary sources for our purposes.

In so far as, for both philosophy and historiography, the various orders collapse into each other, each discipline offers a “point of view” which brackets “reality”, a point of view which is not itself to be encapsulated in some further point of view, for any further point of view cannot be clearly distinguished from it. This amounts to holding that philosophy and historiography are not *rival* points of view; if they were, then it would be intelligible to imagine one as defeating and then encapsulating the other. Philosophy and historiography may then be taken to be continuous with each other. The question still remains whether we can keep that which is “real” independent of input from our point of view. That is a central issue commonly understood as the philosophical problem of realism, variously contrasted with idealism or antirealism. Given the continuity between philosophy and historiography, it is not a peculiarly philosophical problem. Indeed philosophical reflection about historiography, involving as it commonly does the problem of how to achieve truth about a past which is no longer real, is one of the paradigm sources of the *prima facie* plausibility of idealist and antirealist approaches. As we saw earlier, the problem may be, in part, expressed in terms of the choice between Nagel’s “view from nowhere” and historical hindsight: the realist would think that there is an objective view from nowhere which is independent of historical hindsight. The antirealist (one with an eye on history, at

expressed in other ways.

least) would think that historical hindsight is the only, and so best, objective viewpoint we have.

We may deal briefly here with our postponed question about the nature of “we ourselves”. We have been forced to recognise that, despite our desire to defer to historians (a desire only partially satisfied by our deferential selection of some historians as appropriate authoritative sources), we have ourselves to write the historiography of historiography which we need. We have now identified our sources, in general terms. We will be working with them as primary sources. In so far as we ourselves are inevitably writing from primary sources that historiography of historiography which we need for the philosophy of the subject, do we thereby turn out to be historians after all? We do not *need* to be historians in a full sense, for (we have already seen) our historiographical recovery of historical facts in order to recover the character of historiography does not commit us to holding that historiography is primarily a factual activity, and hence we may be missing such further features of historiography as may be characteristic of it beyond factual recovery. Nevertheless it is appropriate to observe as the argument progresses whether we are engaged in some commitment which would prevent our approach from being characterised as fully historiographical.

We are not prevented from being historians merely because we have approached our historiography from a philosophical background, for philosophy is a discipline like any other which is available for historians to use. That, however, is not enough. For us to be historians would require that we be *accepted* as such by other historians. “Acceptance” here is not determined by some survey, but is rather in terms of whether our enquiry meets the historiographical standards of the discipline, and that depends on how far our enquiry is *characteristically* a historiographical one. We have earlier affirmed that the use of primary sources meets this requirement. However, there are different ways of “using” primary sources, and this we will examine next. It also remains to be seen whether our approach meets whatever other characteristic historiographical standards may be required to be met. Working this out requires two elements: first, a further characterisation of our own approach, as it exists so far; second, the historiographical recovery of those standards which the practitioners of the

discipline conceive as characterising their discipline. We will engage with these in detail later, in order.

Our use of primary sources

How is historians' use of primary sources to be understood? One philosophical approach is Hempel's, according to whom the historian "would have to establish his knowledge by indirect methods: by the use of universal hypotheses which connect his present data with those past events. This fact has been obscured partly because some of the regularities involved are so familiar that they are not considered worth mentioning at all".¹⁶¹ Hempel is himself not clear enough for us to tell quite what he means by this analysis, but Aviezer Tucker provides more exactness. According to him, what historians do is ask, "what is the best explanation of this set of documents...? The center of research is the explanation of the evidence, not whether or not a literal interpretation of the evidence corresponds with what took place".¹⁶² The "set of documents" are those documents which are before the historian now, in the present, and the documents' use as "evidence" for relevant parts of the past consists in that historical past being affirmed which best explains the documents' present existence. It seems that we face a choice when we read a primary source: on the one hand, we might (Tucker thinks mistakenly) take a literal interpretation of the text to state or imply what took place; on the other hand, we might ask what the best explanation of the documents' existence and content is.

Attempting to apply Tucker's view to the historiography of ideas (he does not do so himself), we recall that our concern is to recover certain historical facts: for example, historian A's own understanding of the character of the discipline of historiography. We are, out of respect for historiography, taking A as *authoritative* on this. Our concern is to recover that meaning from what we take to be explicit text. Yet a negative point of Tucker's position is that what historians do not do (we assume, do not characteristically do, for they certainly *can* do it) is derive what took place from "a literal interpretation of the evidence".¹⁶³ The problem then is how we can obtain, from A's own text, A's own understanding of the character of the discipline of historiography. Philosophers commonly study texts, sometimes taking them as

¹⁶¹ Carl G. Hempel, "The function of general laws in history", in *Theories of History*, ed. Gardiner, pp. 355-356.

¹⁶² Aviezer Tucker, *Our Knowledge of the Past*, p. 99.

authoritative and sometimes not. Aristotle was famously once “authoritative”, and religious texts can have a similar status. But there is an ambiguity here, as follows: is it Aristotle whom we are to take as authoritative, or Aristotle’s texts? Is it the Bible or the Koran which are authoritative of what God wants, or does God (accessed in some other way) authorise the relevant religious writings? Aristotle has lost authoritative status as philosophy has developed (quite apart from philosophers becoming more critical), but many current philosophical works are thought to be capable of resisting challenges and are still taken as authoritative. But is it the works or the authors? Is to say that a text is authoritative to say that it is self-interpreting as such, without reference to its author? Or is the author authoritative after all?

There is a sense in which philosophers study texts in the history of ideas, and a sense in which they don’t. The philosopher’s “history” of, for example, Leibniz’s thought, often involves reading the text in a very different way from that in which a historian might read it. Thus P.F. Strawson in *Individuals* writes on Leibniz’s system of monads, but (unlike most philosophers who take the point for granted) goes out of his way explicitly to distance himself from historians’ concerns and possible criticisms by adding this qualification: “that when I refer to the system of Leibniz, I shall not be much concerned if the views I discuss are not identical at all points with the views held by the historical philosopher of that name. I shall use the name ‘Leibniz’ to refer to a possible philosopher at least very similar to Leibniz in certain doctrinal respects; whether or not they are indiscernible in these respects matters little”.¹⁶⁴

Philosophers are often interested in whether what Leibniz said was *true*; they will search for truth in his writings. Imagine they find it. The historian who says that the work in which they find it is a later forgery, or that Leibniz used such-and-such a concept in an unexpected way and never meant to say that which the philosophers have picked out, would often be making points which are completely irrelevant from a philosophical point of view. If truth is found, then its source is irrelevant, for it is not the source which determines whether it is true or not (although that point is itself

¹⁶³ *Ibid.*

¹⁶⁴ P.F. Strawson, *Individuals: An Essay in Descriptive Metaphysics*, London: Methuen, 1959, p. 117.

philosophically controversial). Thus there is an obvious sense in which two and two make four whether or not Leibniz says so, even if students are also told by their philosophical tutors that Leibniz is authoritative about such mathematical things. Moreover, philosophers are often interested in possibilities rather than historical facts. They may well prefer Strawson's possible Leibniz to the real one, and with such an approach many philosophers *qua* philosophers commonly have little respect for historical facts. We need not convict Strawson of this historical indifference, for in taking the care he did to isolate in thought his possible Leibniz, he was at the same time ensuring that the historical Leibniz remained distinct rather than having yet another anachronistic philosophical distorting glass placed between us and him. The point was nevertheless quite obvious to Ranke: philosophers do not have "a feeling for and a joy in the particular in and by itself".¹⁶⁵

By contrast with this, Quentin Skinner, in his "Meaning and Understanding in the History of Ideas"¹⁶⁶ and other writings, elaborates, in the light of widespread philosophical advances in the theory of meaning, how historians should read texts: summarily, they should not mainly ask what the words in the text mean (as an analytical philosopher might be supposed to do) but rather ask what, say, Thomas Hobbes *meant by* them. Such understanding goes well beyond the literal interpretation of text, and covers not just a larger grasp of text-based meaning beyond the literal but requires a full understanding of pragmatic meaning in its historical context, that is, it requires a full understanding of Hobbes' political circumstances, the audience for which he wrote, and so forth. We may note that the input of historical context and literal text to "the meaning" may vary from author to author, text to text, language to language, and across historical time. Skinner's point may thus, contingently, be more true for Hobbes than for Leibniz; or vice-versa.

Much earlier, Charles-Victor Langlois and Charles Seignobos put well the need to go beyond the literal: "When we have analysed the document and determined the literal

¹⁶⁵ Leopold von Ranke, "On the relations of history and philosophy" (a manuscript of the 1830s, trans. Wilma A. Iggers), in Leopold von Ranke, *The Theory and Practice of History*, eds. Georg G. Iggers and Konrad von Moltke, pp. 29-32 at p. 30.

¹⁶⁶ Quentin Skinner, "Meaning and Understanding in the History of Ideas", *History and Theory* 8, 1969, pp. 3-53.

meaning of its phrases, we cannot even yet be sure that we have reached the real thoughts of the author. It is possible that he may have used some expressions in an oblique sense...: allegory and symbolism, jests and hoaxes, allusion and implication, even the ordinary figures of speech, metaphor, hyperbole, litotes".¹⁶⁷ It is an ancient worry: Dionysius of Halicarnassus (60 BCE-7CE) writes to a friend at length about making sense of Thucydides, saying among other things "In the choice of words he often adopts a figurative, obscure, archaic and strange diction, in place of that which was in common use and familiar to the men of his day. He takes the greatest trouble to vary his constructions, since it was in this respect chiefly that he wished to excel his predecessors. At one time he makes a phrase out of a word, at another time he condenses a phrase into a word. Now he gives a nominal in place of a verbal form, and again he converts a noun into a verb".¹⁶⁸ Langlois and Seignobos, however, while recognising the difficulties, think that "in practice we may be morally certain that an author is not using an oblique sense whenever his prime object is to be understood".¹⁶⁹ This has the effect of permitting historians in virtually all cases to ignore the difficulties and to interpret on the basis of literal meaning, but the large context of pragmatic meaning of which Skinner reminds us is far more pervasive and much less easily ignored. In addition, the context of the interpretation of texts is much larger than the historical circumstances of the original author. There remain, for example, further questions about how far the meaning of the text is a consequence of the author's contribution and how far a consequence of the later reader's interpretation.¹⁷⁰ In addition, there are received traditions of interpretation of texts. With such considerations to be dealt with, the historiographical recovery of what Hobbes, Leibniz, Thucydides and others "in fact" meant is complex, if not even impossible.

Yet while historians might discover, for example, that Hobbes did not "in fact" mean what most philosophers thought he meant, that fact would perhaps – even typically –

¹⁶⁷ Charles-Victor Langlois and Charles Seignobos, *Introduction to the Study of History*, p. 151; see also footnote on that page.

¹⁶⁸ Letter of Dionysius of Halicarnassus concerning the characteristics of the language of Thucydides, to his friend Ammaeus, trans. W. Rhys Roberts.

<http://www.classicpersuasion.org/pw/thucydides/dionysiusthucydides.htm>, accessed 2nd July 2007.

¹⁶⁹ Charles-Victor Langlois and Charles Seignobos, *Introduction to the Study of History*, p. 151.

¹⁷⁰ A point to be followed up with reflection on Roland Barthes' "The Death of the Author", in *Image, Music, Text*, trans. Stephen Heath, New York: Hill and Wang, 1977, pp. 142-148.

not be of philosophical interest. After all, it is, if we follow Skinner here, a *philosophical* position about meaning which suggests that (as a simple example) a significant contrast might arise between what words mean and what their utterer means by them, so that at one level it would not be a new “discovery” for philosophers – historians would merely be discovering a fact which would fit into a pigeonhole which philosophers had already opened for the purpose. Meanwhile, the philosophers’ “Hobbes” could continue to mean what philosophers thought he meant, even if the historical *Hobbes* didn’t. There is a philosophical *tradition* in the interpretation of Hobbes and other famous philosophers; that is in part what ensures their continuing significance. The self-understanding of the practitioners of philosophy might require a grasp of “Hobbes” but not of *Hobbes*. In principle, the “Hobbes” of a literal interpretation as passed down the philosophical tradition might be philosophically significant, while the historical figure with, *ex hypothesi*, different intentions, might not be. Notice again that these are matters of principle, and that the practical impact of such meaning-based philosophical issues is a *contingency*; it is not that there is some fixed model for working the matter out in any given case, nor indeed need we suppose that there is in fact a conflict between the various approaches to meaning in any given case. Contingently, “Hobbes” and *Hobbes* may be identical.

A similar contrast in textual interpretation arises in law: in our common law system, even the most ancient of statutes has to be interpreted according to rules of statutory interpretation which prioritise what the text means, not what the author of the text meant. Only when the literal meaning is completely indeterminate might the author’s (Parliament’s) intentions or beliefs be sought, and even then the wider justificatory context might well be eschewed: “It was a public mischief, [Gladstone] said, to look beyond the walls of Parliament for the influences that were to determine legislation”.¹⁷¹ For generations judges refused to look behind statutes to find evidence of how they were to be interpreted. And the same happens in the historiography of science: when

¹⁷¹ John Vincent, *The Formation of the British Liberal Party 1857-68*, Harmondsworth: Penguin, 1966, p. 260 (letter from Gladstone to Russell). There is a contrast between statutory interpretation and contract law. For the latter the intentions of the parties are crucial, although the text is nevertheless regarded as the best evidence for what those intentions were. Richard A. Posner, thinking about the United States Constitution, says “people who are sophisticated about interpretation know that text does not come first in any illuminating sense”. Richard A. Posner, *Overcoming Law*, Cambridge, Mass.: Harvard University Press, 1995, p. 175.

physicists or engineers read Newton's laws a completely different kind of understanding is involved from that with which a historian – aware, no doubt, of Newton's occult sympathies and the centrality of theological matters to his time – might read them. Notice again that the distinction is here plausibly explained by a Skinner-like philosophical account of the nature of meaning. The historian who, no doubt rightly, sees the physicist's understanding of Newton's laws as "unhistorical" and thereby in some relevant sense "untrue" is plausibly presupposing some such philosophical account of meaning which yields the required contrast. (Although "no one reads Newton or Lavoisier", Langlois and Seignobos remark; "it is enough for their glory that their labours should have contributed to the production of works by which their own have been superseded".¹⁷²) On the other hand, the physicist who supposes Newton's laws (as literally interpreted text) to be "untrue" typically supposes this on the basis of Einstein's work rather than on any knowledge of Newton's occult sympathies or religious tendencies.

In an over-simple way, a philosopher or a scientist might now be understood to see Aristotle's or Newton's *texts* as authoritative, while we may imagine a historian choosing to believe the authors themselves to be authoritative. We might suppose that philosophers, in forming their view of what is literally said, do something like abstracting the logical features of the text, and we may then further suppose that there is a "residue" of meaning which historians deal with, in addition to the literal. The choice we face is then perhaps best seen as offering historiographically available *alternatives* in the task of textual interpretation: the literal, or something else. Once again, historians have to choose. Notice, however, that the simple choice which they apparently face is here generated by a philosophical theory of meaning, as if historians were committed to this piece of philosophy. Yet it need not be a theory of meaning, and *a fortiori* need not be Skinner's theory of meaning, which best explicates the situation here.

The issues of textual interpretation raised here arise as contingencies in different historiographical contexts. Questions concerning authorial intention and abstract or

¹⁷² Charles-Victor Langlois and Charles Seignobos, *Introduction to the Study of History*, pp. 302-303.

literary or literal meaning are not obviously or necessarily relevant to every case and, if they are relevant, can only be part of a very complex web of historiographical reasoning. This web of reasoning, in any given case, will involve taking for granted many received historical “facts” assumed to be historically well-established (although these can, of course, themselves be at issue), and will involve attempting to answer specific historical questions, questions which will vary not just with the subject matter but with the historian’s situation and interests. Such questions need not always be asked. At the end of the day, Hobbes, Leibniz and others may, as a matter of contingent fact, have meant what they said and said what they meant, and done so in a way which is transparent to us.¹⁷³ It would be a historiographical question whether they did so, not a philosophical one. Yet such historiographical questions are themselves answered on the basis of (typically textual) evidence for which the same issues of interpretation arise. While it may not be appropriate to agree with Langlois and Seignobos, who thought that “in practice we may be morally certain”¹⁷⁴ that an author is not using an oblique sense, it may well be appropriate to hold that texts should be assumed to mean what they say in a transparent fashion unless there is reason to think otherwise. (Historiographically, there may nevertheless very often be reason to think otherwise.) To place the burden of proof the other way would likely make historiography impossible, by forcing on us a continual scepticism about meaning. It is on the basis of this argument that we ourselves, at least in general, decide to take as authoritative the explicit and literally understood historiographical *texts* which we will use as our primary sources. We may recognise, in deciding this, that the theoretical difference between the literal and deeper levels of meaning may be only a matter of degree and not of kind. We need not commit ourselves to a particular theory of meaning in our approach, while allowing that theories of meaning can generate historiographical choices in the interpretation of evidence.

¹⁷³ Although the late eighteenth century was “preceded by a period in which concepts are no longer intelligible to us without interpretation and exegesis, and followed by a ‘modernity’ in which the conceptual structure does not generally require such elaboration”. Translator’s Introduction, in Reinhart Koselleck, *Futures Past: On the Semantics of Historical Time*, p. x.

¹⁷⁴ Charles-Victor Langlois and Charles Seignobos, *Introduction to the Study of History*, p. 151.

Choices and questions

There is a question as to how our, or any, historiography, or historiography of historiography, ought to be written. We should not, however, assume *a priori* that this historiographical “ought” has any special or particular status which is peculiar to our present context, for, characteristically of the choice-ridden exercise of writing historiography, there are a number of options: the “ought” may, for example, relate to that which ought to be done in order to achieve some purpose, meritorious or otherwise (it need not necessarily be the achievement or expression of knowledge). It may be what “rationally” ought to be done. It may even be that which morally ought to be done, for historians face these choices too. The “ought” here is the kind of “ought” which will face every historian, for “ought” presupposes “can”, and “can” presupposes alternatives, and, where there are alternatives, the question which one ought to be selected will arise. There are many alternatives in historiography. Alternative choices prefigure the “ought” in question. Every historian will face choices, often more considerable than those involved here. In the present case the “ought” has to do with how best to achieve our specific purpose, the recovery of the character of the discipline as the practitioners of the discipline themselves conceive it. Thus, in addition to choosing sources and modes of interpretation, our selection of *questions* characterises our engagement with the discipline of historiography.

We have now seen that, as philosophers, we have ourselves to write the historiography we need. Paralleling Kuhn’s historiography of science, and like any historian, we have to choose how to write that historiography, choose which questions to ask, choose what count as answers to our questions and deliberate about what the facts are. We will inevitably make assumptions of a quasi-philosophical or at least a prescriptive kind in the choices we make, and the “facts” we produce will be at least partially a product of those assumptions. What we get out will be, at least in part, a function of what we put in. Other historians are in exactly the same position. So long as we do not use our “factual” conclusions to justify that very philosophy on the basis of which we write the historiography, there is no *a priori* difficulty here. Our selection of approach will depend, among other things, on the questions we seek to answer.

We now know our primary sources. But the historiography of historiography is not just a set of agreed facts ready for inspection, any more than is any other area of historiography. It might seem that all we need do is merely “ask” those historians whose works form our primary sources, by reading their (properly interpreted) relevant works with, in mind, our question seeking historians’ views about the character of the discipline of historiography. But it is not that simple. Our question is associated with other features of our approach and is not the kind of question to which a one-line answer might be suitable. As we attempt to write the historiography we need, a process which has already begun, we find ourselves facing a number of choices. Each choice we face generates questions: which to choose, how to choose. It is always a question what question to ask. We have already seen a range of these issues, such as time frame, sources or modes of interpretation, quite apart from the initial choice of whether to approach the matter using historiography at all. Historians characteristically face many more, such as what other disciplines might be used, what may be said in the absence of sources, or how to approach the conflict of sources, quite apart from issues of detail. We now remind ourselves that deciding to seek historians’ views about the character of their discipline has itself been a further choice, one now generating further issues. Understanding the questions we are asking is a crucial element in understanding what we are saying and why we are arguing as we are.¹⁷⁵

We thus face choices as historians do, and just because we seek a historiography of historiography, we then seek, at least in part, a *historiography of historians’ choices*. Understanding the history of historiography then involves – again, at least in part – understanding the course of historians’ decision-making. Our question “what is the character of the discipline?” is now associated with others: what choices do historians now make, and what choices did past historians make? Moreover, because choices generate questions, we thereby seek, again at least in part, a historiography of historians’ questions. The character of the discipline is to be revealed, at least in part, in terms of the questions which historians ask, the choices they make. There are, of

¹⁷⁵ This point is developed by R.G. Collingwood, *An Autobiography*, chap. V.

course, countless such choices, and our interest is in the characteristic kinds of questions involved.

There are thus questions in our own approach which it is appropriate to highlight as essential to that approach. These questions surface for consideration partly because of our philosophical argument and partly due to our continuing attention to what we find we need to do in order to engage in our historiographical recovery. While the exact questions in our own case may not be characteristic, meaning usual, of the discipline of historiography, it should be noted that explicit questions of some kind commonly govern historiographical approaches. Without explicit questions we may well find ourselves staring blankly at our primary sources, not knowing what to do with them, not knowing how to give them meaning. Explicit questions crystallise our curiosity and our puzzlement, which arguably are always there when we approach the past as historians. Nevertheless, with reasoning parallel to the case of factual recovery explained earlier, we can legitimately “question” our sources in order to recover the character of historiography, and regard ourselves as thereby engaging in historiography, without committing ourselves to holding that historiography is *characteristically*, meaning essentially, a questioning activity. That, as in the case of factual recovery, remains to be seen.

It may well be observed that the current outlining of our approach is framing the historiography we seek to produce, hence including a philosophical input despite our intention to use only historians as primary sources. We have again to avoid the point which we earlier quoted from Ranke: “were this procedure correct, history would lose all independence. It would be ruled simply by a proposition derived from pure philosophy and would stand and fall with the latter’s truth”.¹⁷⁶ The central point of Ranke’s objection here is not that we approach our material with a purpose, but that we approach it with a *philosophical* purpose. Ranke is not against purpose: “I am convinced that a historical work may also derive its internal logic from the intentions of

¹⁷⁶ Leopold von Ranke, “On the character of historical science”, p. 36.

the author and the nature of the task”.¹⁷⁷ Do we miss historical truth by approaching our historiographical material for a philosophical purpose? Not necessarily, for history is not the preserve only of professional historians. History is the subject matter for historiography, but it is continuous with the everyday and is the subject matter for us all. Moreover, Ranke’s contrast between philosophy and historiography was derived from his view of the Hegelian philosophical context of his time. Hegel sought to outline the substantive progress of history in terms of *a priori* philosophical reasoning, and Ranke plausibly saw this as a claim to discover historical facts without historiographical means, so seeking to render historiography redundant. However, it is question-begging merely to assume that *any* philosophical approach essentially involves a purpose which must be, like Hegel’s, contrasted with a *historiographical* purpose. On the other hand, we equally cannot just *assume* that our own philosophical purpose is one where such a contrast does not apply.

Part of our purpose is to recover that which is *characteristic* of historiographical work, and it is difficult to see on what basis one might object that approaching our sources with this question will hinder the elucidation of historiography. Moreover, unlike Hegel, we are planning to use historiographical means to achieve that purpose. A complaint might nevertheless be made that, while the question which asks for that which is characteristic of historiographical work may well help elucidate historiography, it is not itself a question which is characteristic of those asked by historians. “Characteristic” here is unclear. We may anticipate that *certain kinds* of question may be characteristic of historiography, but – if we restrict ourselves to the factual – there are at least as many historiographical questions as there are factual possibilities. Our question is, like many other historiographical questions, a factual one and is in that way characteristic. It is no doubt true that few historians will ask, or have asked, our particular question, and in that limited sense it is perhaps “uncharacteristic”, but this is no objection, for such a point can be made of many historiographical works. A problem would only arise if our questioning were an *unhistoriographical* thing to do, but that is not the case.

¹⁷⁷ Leopold von Ranke, “Preface to History of France” (1852, trans. M.A. Garvey, revised by Georg G. Iggers and Konrad von Moltke), in Leopold von Ranke, *The Theory and Practice of History*, eds. Georg G. Iggers and Konrad von Moltke, pp. 147-150 at p., 150.

Our philosophical approach in terms of “choices” is thus not unhistoriographical. Indeed, this analysis suggests that, while some philosophical approaches, such as Hegel’s, might be unhistoriographical, there is no particular reason to think that there is some fundamental opposition between “the kinds of questions historians ask” and “the kinds of questions philosophers ask” in our present context. On the contrary, we have already argued that historiography is an appropriate and characteristic way of engaging in the philosophy of a discipline. Reverting for a moment to our explanation earlier of Skinner’s presentation of the recovery of meaning in the history of ideas, and reversing his order of things, *of course* the literal meaning of a text could be central to a historian’s concern, just as authorial intention or social context could be central to a philosopher’s.¹⁷⁸ Many historians like Skinner make use of philosophical views. There is no doubt that some historians may read philosophical works and allow the ideas involved to influence their work. While there are often differences of question – thus a philosopher might be interested in a metaphysical question on the nature of numbers, and a historian might be interested in the influence of the gentry on royal policy – there need be no categorical differences here. Truth – and we may recognise that some philosophical sense has to be made of that – is an allowable (if not *necessarily* characteristic) goal for philosophers and historians alike. Truth for both is best seen as the answer to a question, and that is another of Collingwood’s relevant views.

We – historians and philosophers alike – can each put history to the question, and there are no obvious categories of question which would keep one discipline exclusive of the other. While there may be extremes on each side, philosophical questions are thus not necessarily unhistoriographical questions or even bad historiographical questions. As we saw earlier, even the move from first-order to second-order approaches has its analogue in both disciplines. Yet while there might not be any absolute distinction between philosophical questions and historical questions, there remains a practical difference between the way Skinner writes about Hobbes and the way Strawson writes about Leibniz. It is proper to conclude that philosophers could equally write a

¹⁷⁸ I do not mean to imply that Skinner would disagree with this.

historiography of historiography in a different way from historians, but it is also proper to conclude that philosophers could write it differently from each other, as could historians. What matters is that, just as Strawson does, the approach be made perfectly clear.

While what appears to be a “philosophical” purpose or question must not be *assumed* to contrast with a “historiographical” purpose or question, it is appropriate to observe that there is nevertheless a wide range of questions which might in principle be asked, and some may be more historiographically or philosophically problematic than others. Much of Kuhn’s approach to the history of science is problematic in both ways. For example, his approach forced on him a long term historical perspective. He needed comparatively *longue durée*¹⁷⁹ historiography rather than, say, contemporary sociology of science, because of the temporal length required for his question about paradigm change to work. Certainly either long-term or short-term difference and change are paradigmatic topics for historiographical study. Do we want the historiography of historiography to pose and answer such questions? Given Kuhn’s approach, it seems that we might well properly ask questions about theoretical change in historiography.¹⁸⁰

But there is more to a “paradigm” than mere “theory”. Says Nicholas Phillipson, “If it is true that revolutions in historical understanding predicate or are predicated by shifts of attention from the facts of history to the facts of human nature, Hume’s History may well turn out to be one of the few genuinely revolutionary histories of England”.¹⁸¹ But such a change of attention, albeit “revolutionary”, and despite being supported at the time by philosophical theory, does not necessarily mark a paradigm change. While historians change their “attention” and raise many new questions as time goes on, that does not mean that there is serious paradigm change such that, not only do new questions arise, but *old questions disappear*. Do they disappear? That

¹⁷⁹ An expression used by the *Annales* school of history, to characterise their approach to history which based historical understanding on long-term historical structures. This approach facilitated the use of social science methods in historiography.

¹⁸⁰ See Jonathan Gorman, “Revision in History”, *History and Theory, Theme Issue 46*, “Revision in History”, 2007, forthcoming.

¹⁸¹ Nicholas Phillipson, *Hume*, London: Weidenfeld & Nicolson, 1989, p 138.

philosophically generated question is itself a new historiographical question (although not one that marks a change of paradigm). It seems well worth asking.

Yet while old historiographical questions may disappear, they may do so because they, along with their answers, have been *forgotten*.¹⁸² For Kuhn, by contrast, when a question “disappears” it may do so because the problem *dissolves*. In astronomy, it was once thought natural that heavenly bodies should move in perfect circles, and what needed explanation – what raised a question – was those bodies (such as comets) which moved in other ways. Switch to Newton, however, and natural movement is held to be in straight lines. Gravity then explains why some heavenly bodies move in circular ways, but the question why bodies move in straight lines has disappeared. One question has been replaced by another, as background theories change. It seems that such questions *only* arise when the theory permits them to. Is there, in the history of historiography, an analogue for that situation in the history of astronomy? It is plausible that historiographical method, without some serious philosophising first, will make very little input to answering this question. However, it is not a question to which an answer will be developed here for, as we will see, the characteristic kinds of questions which historians ask remain broadly unchanged over time, even if some small scale Kuhnian “disappearance” has occurred.

The relativism of questions and their answers to background theories or paradigms is a main feature of Kuhn’s approach, as many readers have perceived it.¹⁸³ So is historical patterning: we could, in principle, ask how far historiography itself shows any kind of change or progression and use some independent philosophical standard for that. We can imagine that a historiography of historiography might be developed analogously with Kuhn’s view of the history of science, or indeed with Hegel’s or Marx’s views of history itself, and we can imagine discovering, doing our best to use historiographical methods, that the path of historiography displays some pattern,

¹⁸² Pat Tucker of the French-Canadian Society of Michigan has recently set in train a query, through History and Theory’s H-List, concerning how to identify historic memory loss. Replies include references to studies in oral history and also to Paul Ricoeur, *Memory, History, Forgetting* [2000], trans. Kathleen Blamey and David Pellauer, Chicago: University of Chicago Press, 2004.

¹⁸³ This hedged way of putting it is due to the uncertainty which often arises when asking what Kuhn means.

perhaps consisting of a rise and collapse in paradigms, maybe even with periods of “normal” historiography and of “revolutionary” historiography.¹⁸⁴ As philosophers we might well try to write the historiography of historiography with such metaphysical and epistemological questions in mind. Again, however, such historiographical questioning is problematic in its complexity, even if we need not say that it is impossible.

In any event, with regard to the aim of characterising historiography, we have not shown that these metaphysical and epistemological issues are centrally relevant. It is just imagination to suppose them to be so. Given our purpose, the arbitrariness of our approaching historiography with such questions should by this stage of our argument have become apparent. Was Kuhn’s approach part of the self-understanding of scientists? Not in general. For example, Nobel-prize winner and medical researcher Sir Peter Medawar O.M. F.R.S. (1915-1987), a scientist not merely of great distinction but also a role model for many others, saw his discipline as essentially Popperian in nature and was very influential in spreading that conception. We miss at least some of the actual understanding on the part of scientists of what is characteristic of their discipline if we slavishly follow Kuhn’s agenda in writing the historiography of science, and we risk a similar outcome if we follow a parallel agenda in writing the historiography of historiography. Many of Kuhn’s questions are the wrong ones to ask if we are concerned, as we are, to understand the character of historiography.

¹⁸⁴ Following Paul Veyne in his *Writing History*, history may be merely a fashion that ends. See my essay on this in *History and Theory* 26, 1987, pp. 99-114.

Character and consensus

But not all of Kuhn's questions are inappropriate or difficult. We well may think of a "paradigm" as an overarching theory which provides the background to our understanding, and which frames the way we see the world and so sets for us an agenda of questions. But to think of a paradigm in this way is to miss a central feature. So expressed, a paradigm is a kind of theory, and in principle just one person can hold that theory. But while a paradigm, as we saw earlier, can be characterised in many different ways, one of its central features is that it is taken by Kuhn to characterise a *community* of scientists. We, too, are thinking of historians as forming a community, for that, as earlier argued, is an essential part of seeing historiography as a rule-governed discipline. When we ask for that which the members of that community think of as characteristic of their community, we are looking for something which they take themselves to *share*. That is, we are seeking whatever *consensus* there may be.

Using this element of Kuhn's approach, should we then write the historiography of community consensus among historians? We need to distinguish our understanding of "consensus" from that used by Aviezer Tucker. In *Our Knowledge of the Past: A Philosophy of Historiography*, he expresses a (non-circular) thesis about knowledge which holds that knowledge is marked by the existence of a consensus among knowledge-holders. So if historians are to achieve knowledge they must have a consensus.¹⁸⁵ For Tucker, not just any agreement will count as a "consensus", for communities may agree on "silly" things.¹⁸⁶ The kind of consensus which counts is that for which *shared knowledge* is the best explanation. "The philosophy of historiography is interested then in comparing the hypothesis that consensus among historians reflects shared knowledge against myriad alternative hypotheses that explain this consensus"¹⁸⁷

¹⁸⁵ In later correspondence Tucker suggests that consensus is a *defeasible* marker.

¹⁸⁶ Aviezer Tucker, *Our Knowledge of the Past*, p. 24.

¹⁸⁷ *Op. cit.*, p. 25.

Tucker finds the right kind of consensus among historians who followed Ranke's historiography, which involved "the collation of manuscripts; the recovery of a purified text; the diagnosis of interpolations and corruptions; the discovery of earlier sources which the writer has used," but it was Ranke's creation of "a community where a continuous development could take place"¹⁸⁸ which matters, one which involved a consensus on *critical* rather than *traditional* cognitive values and theories. Traditional cognitive values involved relying on "authoritative" works such as scripture, while critical cognitive values demand the examination of evidence. The historiography of historiography, in the light of Tucker's philosophical approach, is then the historiography of this consensus.

There is an objection. While Ranke's critical approach to sources became increasingly influential through his many pupils, it was not wholly new, for it applied the biblical scholarship and methods of an earlier generation. His demand for "objectivity" was not even particularly influential, at the time: the "partisanship about England" of the early nineteenth century continued, and "if we seek evidence that Ranke's politically detached manner of writing history had still not swept the field, we may simply note that the three decades after 1830 witnessed the most feverish and politically motivated preoccupation of German historians with English constitutional norms".¹⁸⁹ There was more consensus on steadily rising German nationalism in historiography, a response to the Napoleonic wars, than on focussing on "the dust of archives".¹⁹⁰ It is *false*, so the objection goes, to say that there was a historiographical consensus around Ranke, or that this was the most significant consensus.

Notice that this objection amounts to a *historiographical* disagreement with Tucker. It uses a sense of "consensus" which is a characteristically historiographical sense in that it is an everyday use of the term rather than a technical term. "Consensus", as ordinarily understood, means acceptance or agreement by the group in question, and a typical feature of it is that the agreement in question is at least agreement by the

¹⁸⁸ *Op. cit.*, p. 70.

¹⁸⁹ Charles E. McClelland, *The German Historians and England: A Study in Nineteenth-Century Views*, London: Cambridge University Press, 1971, pp. 63-64.

¹⁹⁰ G.P. Gooch, *History and Historians in the Nineteenth Century* [1913], London: Longmans, 2nd edn., 1952, p. 101, referring to F.C. Schlosser.

majority of the group in question. It thus makes little sense to identify a consensus without identifying the group in question. The majority of historians is not the same thing as the majority of German historians, but in fact the group identified in the objection could be either, for the objection is that there was not a consensus around Ranke whether in Germany or further afield. This brief historiographical objection to Tucker is saying that a proper historiographical discovery of consensus shows consensus around the rise of German nationalism in German historiography, not consensus around Ranke's objectively neutral historiography.

But this objection misses Tucker's point. For Tucker, not just any agreement will count as a "consensus". The only kind that counts is that for which *shared knowledge* is the best explanation, and it is that which enables him to identify the groups he needs. It is open to Tucker to say that shared knowledge is not the best explanation of agreement around the rise of German nationalism, but it is the best explanation of consensus around Ranke, hence his conclusion. Notice, however, that the consensus is now, in effect, *defined* by this thesis about knowledge. In consequence, any of Ranke's students who followed him out of a sense of awe at his *authority* are not to be included in the Ranke-group, for the best explanation of their agreement is not shared knowledge but their sense of awe. Both the identification of the group and of their agreement are wholly determined *a priori* by using the criterion expressed in the knowledge thesis.

Thus the historiographical objection to Tucker will fail just in so far as "consensus" means different things for Tucker and for the objector. However, the historiographical objection now succeeds again by a different route, for Tucker's retrieval of "consensus" is unsuccessful as a *historiographical* retrieval. This is not because the existence or otherwise of a "consensus" is not a historiographical problem, even a characteristically historiographical problem. Rather, it is because the historian who works with the concept of "consensus" in this area would typically do so because, in attempting to construct a historiography of historiography, the evidence showed (or not, as the case may be) that such and such an agreement about such and such a matter did in fact come about. In other words, the ascription of "consensus" to the situation should be the outcome of historiographical work. Paradoxically, Tucker's account of

this work could well be right, for his theory permits us to rationalise the historiographical recovery of consensus in this context by understanding the existence of a consensus as the best explanation of the evidence we have. But he does not use his own theory in this context, drawing rather on his philosophical thesis about knowledge. A consensus – and in particular what “consensus” amounts to in the context – is there to be *discovered* historiographically and not *imposed* philosophically. The objection is then that Tucker’s is a historiographical *mistake* involving inappropriate reasoning, the outcome of what might be excellent epistemological reasoning but not of good historiographical reasoning. Kuhn’s historiography of science may, by contrast, be taken to have *disclosed* paradigms historiographically, not to have imposed them philosophically; although a parallel objection might of course, in principle, be developed here.

Our approach seeking the character of historiography involves recovering historiographical choices. Our interest is in the characteristic kinds of questions involved. We might suppose that this involves seeking characteristic – that is, *agreed* – historiographical choices. We thus search, it seems, for “consensus”. We frame our questions here with that purpose, and no doubt it is in part a philosophical purpose, but is there any reason to think that it is an unhistoriographical purpose? We need to understand what we mean by “choices”, and further elucidation is necessary. Here we have the help of R.G. Collingwood. Collingwood asserted, consistently with our own position, that there are historically contingent certainties. He called an “absolute presupposition” a belief or assumption underlying the beliefs and attitudes involved in our ordinary ways of life, an assumption which is a historical absolute for a time, in that it is contingently uncriticisable at that time.¹⁹¹ “Uncriticisability” here is a practical, not theoretical, impossibility.¹⁹² A belief or attitude is uncriticisable at a time because it is not even entertained at that time as a conscious thought, let alone doubted and actively contrasted with a serious alternative. It is nevertheless unthinkingly presupposed by past agents.

¹⁹¹ See W.H. Walsh, *Metaphysics*, London: Hutchinson, 1963, pp. 160ff.; R.G. Collingwood, *Essay on Metaphysics*; and R.G. Collingwood, *Autobiography*, chap. 8.

The task of philosophy, thought Collingwood, could only be the recovery of absolute presuppositions, which was an undertaking essentially involving historiographical method (by which he meant re-enactments of past thought, otherwise empathetic understanding, but in principle it would not have to be this¹⁹³). Isaiah Berlin similarly thought that the task of philosophy was to bring to the light of day the hidden moral, political and metaphysical models which lie behind our lives.¹⁹⁴ Despite being a philosopher of historiography (and a historian of Roman Britain), Collingwood did not explicitly apply this to historians themselves,¹⁹⁵ but we may draw on him for that purpose now. Add to the point, then, the further thought that this historiographical/philosophical recovery should be of the absolute presuppositions of past historiography.

Notice that this element of Collingwood's approach is consistent with the view espoused in the present book that historiographical work is shot through with choices, for "absolute presuppositions" may be taken to mark the (contingent) *limits* of choice in the historical judgements made by historians, present and past. It is then, plausibly, the existence and later development of the absolute presuppositions of past historians, as disclosed in historiographical texts, which should be at least part of the subject matter of the historiography of historiography, given that that is, at least in part, a historiography of historians' choices.¹⁹⁶ Absolute presuppositions occur at points at which historical hindsight shows that alternatives are in principle available, but which were not noticed at the time. They are beliefs or attitudes which were taken for granted by past historians. An absolute presupposition is (in practice) uncriticisable at a given time because it is not consciously contrasted with alternatives. While present historians have absolute presuppositions, only later historians will be able to recover these. Only with historical hindsight can later historians recognise that there were

¹⁹² This contrast needs to be understood in terms of what it was counterfactually possible for a past agent to do. The distinction is not a clear one.

¹⁹³ There is no necessary connection between these things, which is just as well, for we see below that the re-enactment of past thought is arguably *impossible* as a means of recovering absolute presuppositions.

¹⁹⁴ Isaiah Berlin, "The Purpose of Philosophy", *Concepts and Categories: Philosophical Essays*, Oxford: Oxford University Press, 1980, p. 11.

¹⁹⁵ Rather, he asked "By what steps and stages did the modern European idea of history come into existence?" *The Idea of History*, p. 14, referring to Parts I-IV of the book.

alternatives, because they can see, as the past historians could not, what the alternatives were and are. For historians later than them, the alternatives are consciously available, and a choice needs to be made. In principle, later historians might conclude with the same belief as the past historians, and so make the same “choice”, but the later historians consciously *chose* that choice and the past historians did not. Yet we might wish to say that the past historians “unconsciously” made the same choice. The category of choices thus includes what might be called “non-choices”. “I am searching for the gaps people have not spotted, for the clues they have missed”.¹⁹⁷ We recognise that choices deliberately made now might have been unthinkingly and unknowingly made then; such non-choices would not in the past have been *chosen*, although they *could* (theoretically, if not practically) have been chosen, we say with the benefit of hindsight, since later historians made decisions about them. This, however, needs explanation.

We have already characterised the historian’s activity in a fundamental philosophical way as involving choices, so that the historiography of historiography is at least in part the historiography of historians’ choices. When we ask about the choices historians make now and the choices they made in the past, we are in part asking what was unthinkingly assumed by past historians. It is present, or at least later, historians who are the proper judges of this. We need to be able to make sense of, for example, remarks such as Michael Bentley’s, who, thinking of the notions of a historical “period” and a historical “source”, says, “two terms escaped currency, however, until around 1780” and “what was absent in historical thinking for the first three-quarters of the eighteenth century seems, from a modern perspective, quite as suggestive as what was present”.¹⁹⁸ It is as if the terms were available to be captured, or in theory chosen, pre-1780, but that the historians of the time (somehow) in practice missed them, and unconsciously chose to stay with their existing modes of understanding. It would then seem appropriate to ask *why* past historians missed these things, but that would surely be an anachronistic historiographical question. It is certain that Bentley

¹⁹⁶ I am most grateful to Leon Pompa for discussion of this point. I doubt if I have satisfied him upon it.

¹⁹⁷ Theodore Zeldin, *An Intimate History of Humanity* [1994], London: Vintage, 1998, p. 13.

¹⁹⁸ Michael Bentley, *Modern Historiography*, pp. 3-4. He ascribes the first use of the word “source” in this context to William Robertson’s 1777 history of America.

(one of the comparatively small number of modern historians who reliably contributes at the appropriate philosophical level to the placing of historiography in historical context) is not intending to imply that pre-1780 historians were not paying conceptual attention as they should. But Bentley's is not a characteristic attitude. Even historians can adopt the anachronistic, for example G.R. Elton, who remarks that there is "a very real sense in which good modern historians are superior to greater minds and abler men who in earlier ages concerned themselves with history".¹⁹⁹ For him, such earlier historians *failed*.

We may well think at this point that the idea of being "theoretically" but not "practically" available for conscious choice is itself overly philosophical for proper historiographical purposes. We may well think that the idea of seeing past historians' beliefs and attitudes partly in terms of their "unconscious choices", perhaps just because it puts historians in the position of having to ask anachronistic questions, is essentially not a suitably historiographical approach to take. And if "unconscious choices" are an essential part of our philosophical approach, which involves choices, then that approach too is unhistoriographical. A further problem we would face – a further choice – is what historiographical methods or approaches we should in any event use in recovering these non-choices. How is historians' *self-understanding* of what they are doing involved in a non-choice? As an unreflective commitment, plainly the "understanding" in "self-understanding" need not involve any element of explanation or justification, for that would suggest – what is *ex hypothesi* not present – a conscious reasoned choice between alternatives. If these objections are effective, then we ought not even to be saying of past historians that they took certain things for granted.

We claim to be able to write the historiography of historiography in terms of the choices made by individual historians. However, if we follow Collingwood in wishing to recover absolute presuppositions, in particular the absolute presuppositions of past historians, we plausibly think of ourselves, as now seen, as writing this historiography partly in terms of what past historians took for granted, that is, in terms of the choices

¹⁹⁹ G.R. Elton, *The Practice of History*, London: Collins, 1969, p. 14.

which past historians did *not* make but which are apparent to later historians.²⁰⁰ This, it may now appear, is unfortunately “philosophical” and not properly historiographical for the reasons given. Nevertheless we do wish to recover the views of past historians, and they will include some choices. We may then well think – but it will be a contingent matter – that the past historians themselves were representing their own colleagues or their own society in the views that they held. That, indeed, would likely be one reason which made them of historical interest to us. Their choices would be their colleagues’ or their society’s choices, and this would open to us yet a further choice, namely, whether to adopt an *individualist or a holistic* approach to the historiographical recovery at issue.

Is such a distinction just more philosophy and so all the less historiographical? The distinction between holism and individualism is a complex one, well clarified by Steven Lukes.²⁰¹ The distinction has to do with the fundamental choices which arise in social understanding: for example, perhaps we should assume that only individuals exist, and that societies “exist” only as a manner of speaking, for they “exist” in virtue of – they are “nothing but” – the individuals who constitute them.²⁰² On the other hand, perhaps it is society which has the primary existence: traditional Marxism would exemplify seeing social matters in this metaphysical way. Associated with these contrary approaches, but not at all identical, are questions whether individuals alone have causal force in social change, or whether social institutions can themselves be causally effective. The same kind of holist/individualist choice which arises for the explanation of social affairs, arises also for the blame which we might wish to ascribe for the occurrence of certain events, and arises for the language which it is most perspicuous for us to use in describing the human world. Earlier we noted Mandelbaum’s approach in insisting upon the use of “societal” concepts, that is, those which relate to social institutions in (some) social understanding, and we used this approach to begin the understanding of historiography as a discipline rather than as an activity merely of individuals.

²⁰⁰ Or, Maria Grever comments, apparent to some dominant group of historians.

²⁰¹ See Steven Lukes, “Methodological Individualism Reconsidered,” in *The Philosophy of Social Explanation*, ed. Alan Ryan, London: Oxford University Press, 1973, pp. 119-129, and the section “History and the Social Sciences,” in *Theories of History*, ed. Gardiner, pp. 476-515.

²⁰² Philosophers call this a “reductionist” position.

It is in the recovery of social decisions, using perhaps (in economic historiography) the economic theory of the firm, that we typically learn that we can *ascribe* choices to people, using a model,²⁰³ although in principle we would not have to use economic theory as the model in question. When it comes to discovering or recovering past choices or decisions we can then use two general modes, first, the discovery of the actual conscious decisions that were made, or second, we can ascribe decisions on the grounds that the people involved acted *as if* such decisions were consciously made.²⁰⁴ It is plausible to hold that Collingwood's famous analysis of historiographical method as the "re-enactment of past thought" would make impossible the recovery of "absolute presuppositions", just because they are assumptions which are so firmly taken for granted that they are *not* a part of the past individual's conscious deliberation. We presumably could not put ourselves in the position of the past historian and recover a thought which was never consciously there. Moreover, even if the thought *were* there, *alternative* thoughts were not, and we could not uninvent our own understanding of the thought, which comes complete with the alternatives. The *meaning* of the thought would inevitably be ours, and not the past historian's, on this approach. This is a failing of Collingwood's "re-enactment of past thought" approach. Rather, the recovery of an absolute presupposition, of a "non-choice", may be *characteristically historiographic* just in so far as it is capable of being understood in terms of characteristically historiographic ways of *ascribing* choices and thoughts to past individuals. Engaging in cliometric historiography, which ascribes in this way, is indeed one of many approaches to history which are characteristic of the discipline.

Whether historians *empathise with* past choices, or *ascribe* to past individuals such choices, the two modes of understanding here presented may perhaps *both* be characteristic ways of historiographical understanding, or indeed it might be thought that *neither* is. However, what *is* characteristic is that the choices historians face include choices between such "philosophical" alternatives. An important part of the

²⁰³ See Jonathan Gorman, *Understanding History*, Ottawa: University of Ottawa Press, 1991, chap. 5, and Milton Friedman, "The Methodology of Positive Economics", in Frank Hahn and Martin Hollis (eds.), *Philosophy and Economic Theory*, London: Oxford University Press, 1979, pp. 18-35.

²⁰⁴ This is one suggestion for economic theory. See Milton Friedman, "The Methodology of Positive Economics", pp. 18-35. Other ways of understanding economic models are available.

“consensus” we seek is historians’ agreement, not so much on the *answers* to questions, but on *what the questions are*. Historians may decide things in different ways, but agree on what it is they are deciding about. To recover what is characteristic of historiography, it is not necessary to find simple agreement or consensus about its character. Instead, that historians disagree may be characteristic, and just as relevant would be where they agree to disagree, and where they agree about what they are disagreeing about.

Historiography of historiography

Summarising the approach of the arguments given in this chapter, we want our philosophical model of historiography to reflect truthfully what historians “actually do”. We have thus proposed a historiography of historiography to recover the self-understanding which we judge to be involved here. How are we to provide this? We should not look in the original at the work of those past historians who do not express their understanding of the character of the discipline and try to recover it for ourselves, for that is essentially a historiographical and not philosophical task and we must defer to historians on this. On the other hand, as we have seen, we do need to select that range of historians who write the kind of historiography of historiography which derives from the work of other historians’ their views about the character of historiographical activity, where those views are not explicit, and look at those historiographies of historiography in the original. We also need to select that range of historians who express their understanding of the character of the discipline, and look at those in the original. We need to decide, in other words, which historians to defer to. In approaching this limited range of primary sources as we do in search of the solution to the questions we have, we are engaging in a characteristically historiographical move. As such, it is after all *we ourselves* who are thereby writing the historiography of historiography. We philosophers turn out to be historians after all. Like all historians, we are limited in our range and in the questions that interest us. Like all historians, we have *chosen* the limits of our range and the nature of our questions.

Hence we will need to select historians, and historians’ historiography of historiography, for our purpose. That inevitable choice might be thought to allow unacceptable room for philosophical interference. But, if that is so, then historians themselves – since they also have to select – will also leave room for “interference” of some historiographically external kind including philosophy, so that our situation is no different. By contrast, if a properly undertaken historiography blocks such inappropriate input, then we can hope for the same advantage. The grounding of selection will be dealt with later, in the section “Structuring factual synthesis”. Those

problems apart, our choice is in fact highly constrained. For, while historians do indeed express their understanding of the character of their discipline and write the historiography of historiography, they have done so rather rarely. “Time was”, says Michael Bentley, “when ‘historiography’ featured as an optional extra in university and college curricula” – many teachers saw it as “superficial decoration”,²⁰⁵ and this situation has carried through to the fairly small number of works on the subject, although that number is growing. While our selecting from these works is unavoidable, since there are fewer of them our selection is more likely to be “typical”. Observes Lawrence Stone with a touch of sadness, “Critics now demand supporting statistical evidence to show that the examples are typical, and not exceptions to the rule”.²⁰⁶ Statistics will not help us here, however, for while our historians may or may not be “typical”, it is more important that they be *paradigmatic*, for that is the best way of recovering the relevant features of a rule-governed discipline.

We have argued that the philosophy of a discipline requires the historiographical recovery of the model or models which the practitioners of the discipline conceive as characterising their discipline and under which they conceive themselves to be operating. Following these arguments, we turn next to historians’ own judgements of who other historians are and who are to count as their historian predecessors. By this means we avoid the charge of philosophical arrogance, we avoid the risks of circularity, and we show a proper respect for historians without committing ourselves to the view that current historical practices are self-evidently justifiable and beyond philosophical consideration or criticism.

We seek contributors to a full historiography of historiography, both as agents within it and as writers of it. We may imagine that such a historiography would, on the face of it, and if it were in simple chronological terms, begin with ancient historians or ancient historical writing rather than ancient history, and might present, for example, and *inter alia*, and if any, the development or change of historiography or historical practice over time. In so far as this imagined historiographical work were chronological in

²⁰⁵ Michael Bentley, *Modern Historiography*, p. viii. Quite *what* “historiography” meant to those teachers is uncertain.

presentation (although it would be begging a relevant question to assume that historiographical work is characteristically chronological in presentation), then, say, eighteenth-century David Hume writing on James I and VI (1566-1625)²⁰⁷ would have to appear before, say, nineteenth-century Jacob Burckhardt writing on the fifteenth-century Italian Renaissance,²⁰⁸ rather than the other way around.²⁰⁹

Herbert Butterfield – whose *The Whig Interpretation of History* is surely familiar to every, at least British, student – seems for our purposes paradigmatic both as a historian and as a historian of historiography, and we begin with him. If this beginning seems arbitrary, we can only avail of F.H. Bradley’s defence of the structure of one of his books: “If we incur the reproach of starting in the middle, we may at least hope to touch the centre of the subject”.²¹⁰ Butterfield would have agreed in principle with this “new kind of enquiry”: “In a wild dream one could imagine a Cambridge Modern History which, instead of presenting a hard piece of narrative, took subjects like the French Wars of Religion or the Origins of the War of 1914, and gave a critical analysis of the whole course of their study and interpretation”.²¹¹

But notice that this imagined historiography of historiography would not necessarily be a historiography of *historians*, for, as we have already seen, it might be written by approaching the historiographic texts with philosophical interests which deliberately ignored the authors of the texts. Butterfield, in his own curious way, and in marked contrast to his *Man on his Past: The Study of the History of Historical Scholarship*, did just that in his *The Whig Interpretation of History*: “It is astonishing to what an extent the historian has been Protestant, progressive, and whig, and the very model of

²⁰⁶ Lawrence Stone, “The revival of narrative: reflections on a new old history”, *Past and Present* 85, 1979, pp. 3-24 at pp. 10-11.

²⁰⁷ David Hume, *The History of Great Britain* [1754], Harmondsworth: Penguin, 1970.

²⁰⁸ Jacob Burckhardt, *The Civilization of the Renaissance in Italy* [1878], trans. S.G.C. Middlemore, Harmondsworth: Penguin, 2004.

²⁰⁹ As we find in, say, Michael Bentley (ed.), *Companion to Historiography*, London: Routledge, 1997.

²¹⁰ F.H. Bradley, *The Principles of Logic*, vol. I, 2nd edn., London: Oxford University Press, 1928, p. 1.

²¹¹ Herbert Butterfield, *Man on his Past: The Study of the History of Historical Scholarship* [The Wiles Lectures, Queen’s University Belfast 1954], Cambridge: Cambridge University Press, 1969, p. 24, a reference I owe to 2003 Wiles Lecturer Michael Bentley, “Herbert Butterfield and the Ethics of Historiography”, *History and Theory* 44, 2005, pp. 55-71 at p. 68.

the 19th century gentleman”.²¹² But who were these historians? While Martin Luther’s name appears over and over again, one searches the text almost in vain for the names of historians. There is a half-hearted reference to Henry Hallam (1777-1859) on page 4 and an irrelevant nod of appreciation to Lucien Romier (1885-1944) on page 5. Yet “the whig historian” is referred to over and over again. Not until page 92 (of 132 pages) do we get two substantive references, one of whom is controversially described as a historian,²¹³ and neither of whom are presented as (whether or not they are) whigs. Indeed, each of these is mentioned only for his contribution to the “creative act of the historical imagination”: “The historian is not merely the observer; for if he were this only he would be a poor observer. In a special sense he goes out to meet the past and his work is not merely the function of mind, it is a venture of the personality. This is why Sir Walter Scott [1771-1832] has helped us to understand the Covenanters, and Thomas Carlyle [1795-1881] has made an important contribution to our estimate of Cromwell”.²¹⁴

Historically unlocated towards the end of the book are Edward Gibbon (1737-1794) and Ferdinand Gregorovius (1821-1891): “The fervour of the whig historian...was awakened in Gibbon and Gregorovius by the sight of the ruins of ancient Rome”.²¹⁵ “The historian may be cynical with Gibbon or sentimental with Carlyle”.²¹⁶ The first serious references to a historian appear in Butterfield’s last chapter, where – although he is still historically unlocated – we find Lord Acton (1834-1902): “It might be true to say that in Lord Acton, the whig historian reached his highest consciousness”.²¹⁷ E.H. Carr comments that Butterfield did not “name a single Whig except Fox, who was no historian, or a single historian save Acton, who was no Whig”.²¹⁸ Acton’s name appears regularly in this section of *The Whig Interpretation of History*, “Moral

²¹² Herbert Butterfield, *The Whig Interpretation of History* [1931], London: G. Bell and Sons, 1968, pp. 3-4.

²¹³ Sir Walter Scott; see Michael Bentley, *Modern Historiography*, p. 26.

²¹⁴ Herbert Butterfield, *The Whig Interpretation of History*, pp. 91-92.

²¹⁵ *Op. cit.*, p. 96.

²¹⁶ Carlyle so achieving his second mention. *Op. cit.*, p. 105.

²¹⁷ *Op. cit.*, p. 109.

²¹⁸ E.H. Carr, *What is History?*, London: Macmillan, 1961, p. 40.

Judgments in History”,²¹⁹ but this section is, we are to understand, certainly not a work of historiography, for “ethical questions concern the historian in so far as they are part of the world which he has to describe”, but otherwise the historian is not “on his own ground”,²²⁰ for “it is not his function to tease himself with questions concerning the place where moral responsibility lies”.²²¹

That assertion is, of course, an assertion about what historians ought or ought not to do. On Butterfield’s own account, it is not a contribution within historiography, or to the historiography of historiography, but rather to its moral philosophy. Despite this, it is clearly an expression of Butterfield’s understanding of the nature of historiography that it should eschew moral judgement. Perhaps paradoxically, we thus find Butterfield telling us something about the nature of historiography without engaging in historiography to do so. It is not, apparently, Butterfield’s function *qua* historian to say where moral responsibility does or does not lie, but he does exactly that anyway.

While in its way a very famous contribution to the historiography of historiography, *The Whig Interpretation of History* – just because it presents neither historically located historians nor their texts nor their methods – might on one plausible view be held not to be a contribution to that historiography at all. Perhaps a better view would be to recognise that historiography is vast enough to include a multiplicity of approaches, including this one. It does, after all, deal with a world of ideas in much the same critical way that the historiography of philosophy, or the historiography of philosophy of history, might do, and such historiography is commonly undertaken by philosophers. Yet Butterfield wishes to distance himself even from that, in his preface: “The subject is treated not as a problem in the philosophy of history, but rather as an aspect of the psychology of historians...[the present study’s] theses would be unaffected by anything the philosopher could state to explain them or to explain them away”. But that can’t be true: *every* thesis would be affected by universal scepticism, for example. Not only cannot Butterfield block philosophy in this way, but

²¹⁹ Reprinted in Hans Meyerhoff (ed.), *The Philosophy of History in Our Time: An Anthology*, New York: Doubleday Anchor, 1959, pp. 228-249. Contrast Herbert Butterfield, “Moral Judgments in History”, chap. 4 of his *History and Human Relations*, London: Collins, 1951, pp. 101-130.

²²⁰ Herbert Butterfield, *The Whig Interpretation of History*, p. 126.

²²¹ *Op. cit.*, p. 125.

he is himself, in *The Whig Interpretation*, contributing in a reflective and critical way to our understanding of part of the historical world of ideas, and thereby acting himself as a philosopher. Historical *evidence* – which we may otherwise take to be a characteristic feature of historical work – is missing.²²² He presents to us not so much historians' psychology as what he takes to be, at least for some of them, their unreflective *presuppositions*. The recovery of presuppositions is a characteristically philosophical thing to do, and – our own and Collingwood's view apart – maybe not very obviously a historiographical thing to do, but perhaps we should draw the conclusion that – just because Butterfield has done it – it is, rather, a *highly* historiographical thing to do.

Moreover, Butterfield, despite his notable contribution to the historiography of historiography in *Man on his Past*,²²³ observed “my attention was drawn to the subject when I was invited to lecture in certain German universities in 1938, and was told that readers of my *Whig Interpretation of History* were asking about the history of the Whig interpretation – a point into which *it had never occurred to me* to enquire”.²²⁴ It is a very particular and curious blindness which prevents such historians from seeing themselves, or other historians, in historical context. Yet, with or without that context, Butterfield tells us something about the character of historiography, and is clearly writing as a historian in doing so.

Butterfield here tells us something about the history of historiography, but not much. The first historian mentioned in *Man on his Past* is Lord Acton, and while there is much material on the newly developing historiography in Germany in the early nineteenth century, as far as this book is concerned virtually nothing seems to have taken place in the subject before the late eighteenth century. Only the briefest glimpse of earlier times shows: “Ranke...refuses to allow that anybody is a greater historian than Thucydides”,²²⁵ so even Thucydides is only mentioned indirectly. Ranke is

²²² He later recognised that it was “derived only very partially from the facts”. Adam Watson, “Introduction” to Herbert Butterfield, *The Origins of History* (edited and introduction by Adam Watson), London: Eyre Methuen, 1981, p. 7.

²²³ See Herbert Butterfield, *Man on his Past*. Even here, Butterfield gets to 1694 in a single page.

²²⁴ *Op. cit.*, p. xii, our emphasis.

²²⁵ *Op. cit.*, p. 106.

indeed a major founder of modern historiography, for Butterfield. “Since the Second World War, three men in particular have been repeatedly examined, both in Europe and in America. They are the German, Ranke, the Swiss, Jacob Burckhardt, and the cosmopolitan Englishman, Lord Acton”.²²⁶ The German leadership is here acknowledged, but, with respect to the nineteenth century, the historian “played an important part in the German national story in that period; for in effect it was he who said to the country: ‘See, *this* is your tradition, this is the line which the past has set for you to follow’. And now it is dawning on the Germans that their historians may have made the wrong diagnosis”.²²⁷ “For Germans this has meant rolling back their modern tradition of historical writing and enquiring more deeply into the basis of it. In particular it has meant a re-examination of the fundamental principles established by the greatest of the nineteenth-century historians, Ranke”.²²⁸ Butterfield, too, says that he has to seek the origins of the approach of Ranke who, with the classical historian Niebuhr, “established the modern method and inaugurated a new epoch in historical study”.²²⁹ However, his presentation of those eighteenth-century origins does not remove his sense of a new epoch with Ranke, and he writes with a kind of whig hindsight: thus a minor figure “foreshadows Ranke”,²³⁰ language which confirms the position he advances. Earlier times are closed off by his approach.

We have held that historiographical recovery is continuous with everyday recovery, and one implication of this is that there is no specification or limitation of just how far back in time the historiography of historiography ought to go. We have earlier argued that we should start, in our search for historians’ self-understanding, as far back as we can go. A long rather than short time frame is better in order to avoid arbitrarily ignoring what might be relevant material. It is important to draw on historians’ views about the nature of their subject from their writings about the earliest times, for here we see what they *count* as such. If Butterfield is characteristic, then it seems already to be characteristic for historians not to go very far back when they seek to understand their own discipline. But then maybe earlier times were outside that discipline:

²²⁶ *Op. cit.*, p. 21.

²²⁷ *Op. cit.*, p. 26.

²²⁸ *Op. cit.*, p. 28.

²²⁹ *Op. cit.*, p. 32.

²³⁰ *Op. cit.*, p. 43.

despite his sections on the historiography of historiography, Collingwood describes history which does not meet “the modern European idea” as “quasi-history”.²³¹

Butterfield’s truncated historiography of historiography is not characteristic, however, and, in due course, his limitation came to worry Butterfield, and late in his life his interest in early historiography grew.²³² Many historiographies of historiography, available while Butterfield was active, go back to ancient times,²³³ while full-length treatments are readily available.²³⁴ This is not surprising, for European thought in all areas has built on the thought of ancient Greece, and historiography is not an exception. Just how paradigmatic a writer of the historiography of historiography is Butterfield? Arnaldo Momigliano has a widely recognised status here, and is described by Bentley (twice) as “the greatest historiographer of the twentieth century”.²³⁵ We turn next to him. His famous collection *Studies in Historiography* contains in the Preface the following: “I am a student of the ancient world, and my primary aim is to understand and evaluate the Greek and Roman historians and the modern historians of the ancient world”.²³⁶ But it would be wrong to think of Momigliano only as a classicist, studying a time with which our own academic traditions have no connection. In fact, Momigliano counts the ancient historians *as historians*, and Bentley, presenting Momigliano as writing about historians, has the same view. Historiography – our own tradition of historiography, hence to some extent part of present-day historians’ self-understanding – here starts with the ancients, and particularly with Herodotus (ca. 484 BCE-ca. 425 BCE), the “father of history”, as Momigliano explains in “The place of Herodotus in the history of historiography”.²³⁷

Says Momigliano, “Herodotus was the father of history, because Thucydides implicitly recognized him as such; but he was believed to be untrustworthy, because such was

²³¹ R.G. Collingwood, *The Idea of History*, p. 14.

²³² Adam Watson, “Introduction” to Herbert Butterfield, *The Origins of History*, p. 9.

²³³ For example, James Westfall Thompson, *A History of Historical Writing*, vol. 1, *From the Earliest Times to the End of the Seventeenth Century*, New York: Macmillan, 1942 and James T. Shotwell, *The History of History*, vol. I.

²³⁴ For example, Ernst Breisach, *Historiography: Ancient, Medieval, & Modern*, Chicago: Chicago University Press, 1983, selected here for its straightforwardly comprehensive title.

²³⁵ Michael Bentley, *Modern Historiography*, pp. x, 121.

²³⁶ A.D. Momigliano, *Studies in Historiography*, London: Weidenfeld and Nicolson, 1966, Preface.

²³⁷ “The place of Herodotus in the history of historiography”, *op. cit.*, pp. 127-142.

the verdict of Thucydides. In other words the reputation of Herodotus in antiquity depends fundamentally on the direction that Thucydides imposed on historiography".²³⁸ So will later historians always have the advantage over their predecessors. Collingwood comments, "This contrast in Herodotus and Thucydides between the unreliability of everything farther back than living memory and the critical precision of what comes within living memory is a mark not of the failure of fifth-century historiography but of its success. The point about Herodotus and Thucydides is not that the remote past is for them still outside the scope of scientific history but that the recent past is within that scope. Scientific history has been invented".²³⁹ By contrast, Elton says "Herodotus may have been the father of history, but for a good many centuries the child he begot was to enjoy but a restricted and intermittent life. Thucydides, Polybius, Livy, Sallust and Tacitus – great names but, for some 600 years, not a terribly impressive tally".²⁴⁰

Before a closer examination, it is appropriate here to outline the later history of historiography. These ancients and their successors in antiquity informed western medieval and post-medieval historiography in a limited way, because it was so largely governed in the terms permitted by the then dominant Christian ideas. History, as E.H. Carr puts it, had then a *meaning*: "history itself became a theodicy".²⁴¹ Reinhart Koselleck is an important interpreter of the history of historiography, and he says "As long as the Christian doctrine of the Final Days set an immovable limit to the horizon of expectation (roughly speaking, until the mid-seventeenth century), the future remained bound to the past".²⁴² "Roughly speaking, until the mid-seventeenth century, expectation of the future was bounded by the approach of the Last Judgment,... .The art of political prognosis in particular was developed from the sixteenth century on and became a part of the business of all men of state. Such

²³⁸ "Historiography on written tradition and historiography on oral tradition" (1961-62), *op. cit.*, pp. 211-220 at p. 214.

²³⁹ R.G. Collingwood, *The Idea of History*, p. 26.

²⁴⁰ G.R. Elton, *The Practice of History*, p. 12. "The Latins were a poor lot", says Denys Hay, *Annalists & Historians: Western Historiography from the VIIIth to the XVIIIth Century*, London: Methuen, 1977, p. 11. "Herodotus had no successors", says Collingwood somewhat inconsistently, because to the Greek mind "only what is unchanging can be known. Therefore history is a forlorn hope". *The Idea of History*, p. 28.

²⁴¹ E.H. Carr, *What is History?*, p. 104.

²⁴² Reinhart Koselleck, *Futures Past: On the Semantics of Historical Time*, p. 277.

practice did not, however, fundamentally transcend the horizon of a Christian eschatology. Precisely because nothing fundamentally new would arise, it was quite possible to draw conclusions from the past for the future”.²⁴³ Historical study was subordinated to this approach.

In due course classical ideas – from philosophy rather than from historiography – came to undermine these religious certainties, and characteristically historiographical problems arose in the transmission of those ideas. According to Anthony Grafton, there were “two different notions of classical scholarship in conflict. On the one hand,... pedagogical: to produce well-behaved young men who could write classical Latin. ...On the other hand, ...scientific: to offer exact knowledge about minute details of ancient culture and to transmit sophisticated techniques for resolving difficulties in the ancient sources”.²⁴⁴ “One set of humanists seeks to make the ancient world live again, assuming its undimmed relevance and unproblematic accessibility; another set seeks to put the ancient texts back into their own time, admitting that reconstruction of the past is difficult and that success may reveal the irrelevance of ancient experience and precept to modern problems”.²⁴⁵

Donald R. Kelley, drawing on his deep scholarship in the historiography of historiography, comments on the sixteenth century that “Unfortunately, historians were, as always, slow in discovering the potentialities of philology. Students of more specialized and better-defined subjects such as law and the Bible soon recognized these possibilities and began to develop them in the sixteenth century. While historians like Paolo Emilio and Polydore Vergil and even their vernacular successors worried over the formal problems of historical narrative and value judgments, philologists like Budé and Erasmus were breaking new ground in the study of classical and Christian antiquity and even of the European middle ages. And it was their work – and that of their vernacular successors – that was to transform historical writing”.²⁴⁶

²⁴³ *Op. cit.*, p. 203.

²⁴⁴ Anthony Grafton, *Defenders of the Text: The Traditions of Scholarship in an Age of Science, 1450-1800*, Cambridge, Mass.: Harvard University Press, 1991, pp. 25-26.

²⁴⁵ *Op. cit.*, pp. 26-27.

²⁴⁶ Donald R. Kelley, *Foundations of Modern Historical Scholarship: Language, Law, and History in the French Renaissance*, New York: Columbia University Press, 1970, p. 53.

The seventeenth century, particularly Descartes (1596-1650), initiated the great intellectual transformations of the eighteenth century Enlightenment, but in a way which to an extent undermined historiography. Says John Gray, “It is in France that the Enlightenment assumed its central and paradigmatic form, ...and, though it would be incongruous to term him a thinker of the Enlightenment, it is in René Descartes that the most fundamental project of the Enlightenment, that of purging the human mind of all that is irrational, doubtful or groundless, and of reconstituting human thought and practice on rational foundations, is initiated. It is accordingly in Descartes that the first and most formidable critics of Enlightenment rationalism, Pascal and Vico, see their principal opponent”.²⁴⁷ With respect to this Enlightenment “project”,²⁴⁸ we might well add to “all that is irrational, doubtful or groundless”, all that is historically located and changeable, for even Godless truth was seen as eternal. Yet the “counter-enlightenment” (Isaiah Berlin’s term) was already there: “It is only with Joseph de Maistre, born in Savoy in 1753, that there arises in France a radical reaction to the Enlightenment, one which Berlin tells us is more penetrating than that of J.J. Rousseau, inasmuch as Rousseau retained the Enlightenment beliefs in natural man, in human goodness, and in a social contract, which de Maistre set out to destroy”.²⁴⁹ “The Counter-Enlightenment is coeval with the Enlightenment itself”.²⁵⁰ Coeval, perhaps, but not influential until later: it took Michelet (1798-1877) to draw the world’s intellectual attention to Vico’s anti-Enlightenment poetic conception of history.²⁵¹

Says Bentley of the undermining effect of the Enlightenment: “Perhaps the absorption with philosophy and science militated against the production of a great French historian in this generation. The French had to wait until the Revolution became the focus of modern experience and the stuff of a new history that Michelet would make his own fifty years later. The country which ought to have produced an enlightened

²⁴⁷ John Gray, *Berlin*, London: Fontana, 1995, pp. 123-124.

²⁴⁸ There is rather more to the Enlightenment than this. See *What’s Left of Enlightenment? A Postmodern Question*, ed. by Keith Michael Baker and Peter Hanns Reill, Stanford, California: Stanford University Press, 2001.

²⁴⁹ John Gray, *Berlin*, p. 124.

²⁵⁰ *Op. cit.*, p. 123.

historiography – America, the child of Parisian ideas – again did not do so in a significant form before 1800. Instead the extension of ‘enlightened’ thought into historical practice occurred elsewhere, most notably in Scotland and England”,²⁵² referring to David Hume’s 1754-1762 *History of England*, William Robertson’s histories of Scotland (1759) and America (1777), and Edward Gibbon’s 1776-1788 *The Decline and Fall of the Roman Empire*. Continues Bentley, “The importance for any critical form of enquiry of intellectual self-confidence and a rejection of metaphysical authority needs little argument, and to that extent the climate generated in Europe after 1750 contributed unquestionably to the development of historical ideas”.²⁵³ We see in the work of Kant this change: from the attack on metaphysical authority by the use of critical method in his *Critique of Pure Reason* of 1781 and 1787, which nevertheless held to the view that human nature was absolute and unchanging, to his speculations that human nature might change over time and so be historically contextualised in his “Idea for a universal history from a cosmopolitan point of view” of 1784 and “Perpetual peace” of 1795. And the development of historiographical ideas was quick: says Hayden White, “Chairs of history were founded at the University of Berlin in 1810 and at the Sorbonne in 1812. Societies for the editing and publishing of historical documents were established soon after... Government subsidies...national journals of historical studies...The profession became progressively academized”.²⁵⁴

In due course, and “Perhaps most importantly”, says Evans, “Ranke introduced into the study of modern history the methods that had recently been developed by philologists in the study of ancient and medieval literature to determine whether a text...was true or corrupted by later interpolations, whether it was written by the author it was supposed to be written by, and which of the available versions was the most reliable”.²⁵⁵ While for many historians the words will stand alone, in this quotation Evans uses “most importantly” to contrast the introduction of philological

²⁵¹ Hayden White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe*, Baltimore and London: The Johns Hopkins University Press, 1973, chap. 3. See also Leon Pompa, *Vico: A Study of the “New Science”*, and *Vico: The First New Science*.

²⁵² Michael Bentley, *Modern Historiography*, p. 11.

²⁵³ *Op. cit.*, p. 15.

²⁵⁴ Hayden White, *Metahistory*, p. 136.

²⁵⁵ Richard J. Evans, *In Defence of History*, p. 18.

methods with two other contributions from Ranke: first, that “he helped establish historiography as a separate discipline”; and second, that he tried “to understand the past as the people who lived in it understood it”.²⁵⁶ Bentley describes Ranke as “the nineteenth-century’s historical titan”,²⁵⁷ and the best reason for this judgement seems to be Evans’, that he was someone “whose exceptionally long life and extraordinary productivity made him something of a legend”.²⁵⁸ Georg G. Iggers and Konrad von Moltke say “taken together, Ranke’s histories present a monumental analysis of the growth of modern Europe”,²⁵⁹ and point out that “The newly formed American Historical Association in 1885 elected Ranke, whom George Bancroft at this occasion called ‘the greatest living historian’, as its first honorary member”.²⁶⁰ Many historians see Ranke as the source of an “objective” or “scientific” historiography which intended to say of the past “how it actually was”.

Evans rightly says of Ranke that he “was a profoundly conservative figure, who equated the actual and the ideal and regarded the European states of his day as ‘spiritual substances...thoughts of God’”.²⁶¹ Since all times were alike “thoughts of God”, all times were of equal value in God’s eyes, and so should be for the historian. Says Evans, Ranke tried “to understand the past as the people who lived in it understood it... One conclusion that followed from this doctrine was that at any given time, including the present, whatever existed had to be accepted as divinely ordained”.²⁶² In fact, the line of argument is best seen as being in the opposite direction: it is because of Ranke’s clear-headed but conservative religious views that he felt that the past needed to be understood as those who lived in it understood it. This indeed required a degree of impartiality, but it was limited; Ranke did not avoid political controversy, and alienated himself from his liberal friends by editing a political

²⁵⁶ *Op. cit.*, p. 17.

²⁵⁷ Michael Bentley, *Modern Historiography*, p. ix.

²⁵⁸ Richard J. Evans, *In Defence of History*, p. 16.

²⁵⁹ Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History* [1973], New York: Irvington, 1983, p. xxxviii.

²⁶⁰ *Op. cit.* “Introduction”, pp. xv ff.,

²⁶¹ Richard J. Evans, *In Defence of History*, p. 16, quoting from the 1973 Indianapolis edition of Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*.

²⁶² Richard J. Evans, *In Defence of History*, p. 17.

journal.²⁶³ “He argued against the transferability of foreign political ideas and institutions, specifically those of the French Revolution, to Germany. ...Every state, Ranke held, is unique and must develop according to its own inner principles”.²⁶⁴ His conservatism was thus similar to Hegel’s, much of whose religious and idealist metaphysics and epistemology he accepted, although he differed from Hegel’s philosophy of progress in his certainty of the divinely understood historical locatedness of moral judgement. Comment Iggers and von Moltke, “Ranke’s philosophy of value, teaching that every individual and state must be understood in terms of its own standards and that ‘every epoch is immediate to God’, is no longer credible to many historians after the political catastrophes of the twentieth century”.²⁶⁵

Thus Ranke may have been “scientific”, but his conservatism kept him to the traditional view of God’s place in the world. As Koselleck observes, that stable Christian structure “changed for the first time during the eighteenth century, as the impact of science and technology appeared to open up an unlimited space of new possibilities. ‘Reason’, said Kant in 1784, ‘knows no bounds for its designs’”.²⁶⁶ Kant, however, sought to frame these new possibilities in offering the key to history as progress towards the perfection of reason, conceiving reason also to be the foundation of morality, so that in due course humanity would achieve a perfectly moral state.²⁶⁷ Kant’s nineteenth-century successors Hegel and Marx sought, the one in an idealist, and the other in an economically materialist, way, to fill in the rational and “scientific” detail of this approach.

What some historians see as a break with a pre-scientific past came into being. To repeat more fully an earlier quotation, there was “a concentration on a process of transition to modernity in the late eighteenth century (casually nicknamed the *Sattelzeit* by Koselleck, since become a concept in its own right), preceded by a period in which

²⁶³ Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*, p. xxviii.

²⁶⁴ *Op. cit.*, p. xxix.

²⁶⁵ *Op. cit.*, p. lxviii.

²⁶⁶ Reinhart Koselleck, *Futures Past*, p. 203, inserting reference at this point “Kant, ‘Idea for a Universal History with a Cosmopolitan Purpose’, in *Political Writings* 42. (Slightly altered – Trans.)”

²⁶⁷ Immanuel Kant, “Idea of a universal history from a cosmopolitan point of view” [1784], in P. Gardiner (ed.), *Theories of History*, 22-34.

concepts are no longer intelligible to us without interpretation and exegesis, and followed by a ‘modernity’ in which the conceptual structure does not generally require such elaboration”.²⁶⁸ “An outcome of so-called modernity (*Neuzeit*) was that at the end of the eighteenth century the idea of a ‘new time’ was constituted. The concept of progress, which at that time was largely coincident with ‘history’, encapsulated a form of historical time which [was] subject to constant renewal. The common achievement of both concepts was that they renewed and extended the horizon of future expectation”.²⁶⁹

But, while many later historians saw themselves as having rejected Hegelian metaphysics and theories of progress, instead attempting to say “scientifically” with Ranke “how it actually was”, that is not in fact how historiography developed. Rankean methods, without their forgotten idealist presuppositions, became a set of *techniques*, while the big philosophical questions rolled on during his long life. E.H. Carr says that “Enlightenment rationalists, who were the founders of modern historiography, retained the Jewish-Christian teleological view, but secularized the goal; ...History became progress towards the goal of the perfection of man’s estate on earth”.²⁷⁰ Later historians quarrelled about, and eventually removed, the goal, but left in place the structures of “scientific” historical change which earlier historians had put in place to achieve it. While Hegel’s World Spirit disappeared, the Marxist mode of historical writing continued well into the second half of the twentieth-century, eventually competing with other social scientific approaches to history. We call them “social scientific” here to distinguish them from “scientific”, since that is a word many historians reserve for Ranke’s methods. Philosophy since the beginning of the twentieth century only occasionally serviced these historiographical trends. It did so with, for example, its Popperian denials of theories of progress, with its analyses of scientific and non-scientific explanation in historiography, with its many developments in the philosophy of the social sciences, and with, most recently, the provision of a Bayesian rationale for Rankean methods in Tucker’s *Our Knowledge of the Past*.

²⁶⁸ Translator’s Introduction, in Reinhart Koselleck, *Futures Past*, p. x.

²⁶⁹ *Op. cit.*, p. 203.

²⁷⁰ E.H. Carr, *What is History?*, pp. 104-5.

Not too paradoxically, the “social sciences” are not always *social*, for they may be *individualist* rather than *collectivist* in their assumptions. These approaches are many (and include inheritances from many of the older philosophers), but Lawrence Stone, writing in 1979, plausibly sorts them into three: “they are the Marxist economic model [flourishing 1930s-1950s], the French ecological/demographic model [*Annales*, flourishing 1950s-mid-70s], and the American ‘cliometric’ methodology [flourishing 1960s-early-70s]”.²⁷¹ “The trends here identified should not be taken to apply to the great mass of historians. All that is being attempted is to point to a noticeable shift of content, method and style among a very tiny, but disproportionately prominent, section of the historical profession as a whole”.²⁷² On this basis, he detects a return to *narrative*.

Says Koselleck, “One of the properties of the eighteenth-century experiential shift, in which history was formulated in terms of a new reflexive concept, was that the line dividing the camps of historians and creative writers became osmotically porous. It was demanded of the writer, especially the writer of novels, that he articulate historical reality if he wished to be convincing and have influence. On the contrary [other hand], the historian was asked to render plausible the possibility of his history through the use of theories, hypotheses, and reasoning. Like the writer, he was to distill from his history its meaningful unity”.²⁷³ “The historians of the Enlightenment were trained to regard history not only as a science but also – and precisely as a science conveying knowledge – rhetorically, as a form of representation”.²⁷⁴ Historians have had this function since Herodotus.

There is, it is very clear, much history to historiography. Like all history, historians can justifiably select for study some parts rather than others. If we seek to recover that which is characteristic or paradigmatic of the discipline, however, we ought not, as already argued, to truncate our scope arbitrarily. It is then plausible that writing the historiography of historiography should start with Herodotus, as in our outline, rather

²⁷¹ Lawrence Stone, “The revival of narrative: reflections on a new old history”, p. 5. All continued beyond the 1970s, to some extent.

²⁷² *Op. cit.*, p. 4.

²⁷³ Reinhart Koselleck, *Futures Past*, p. 214.

²⁷⁴ *Op. cit.*, p. 121.

than start with Ranke. Yet beginning with Ranke is clearly more appropriate for many historians' understanding of the history of their subject, and perhaps insisting on beginning with Herodotus involves failing to notice a characteristic feature of historiographical self-understanding. Butterfield is not alone in his early truncated view. For example, Richard J. Evans, in a 29-page chapter "The history of history",²⁷⁵ gives the first sentence to "historians down the ages", the second and third sentences to medieval and early modern times, and the remainder of the first paragraph to the Enlightenment. Then follows what is plainly for him the "real" beginning of historiography, with Ranke, and the chapter builds on that. While not primarily engaged in the historiography of historiography (although they both say more about pre-Ranke historiography than Evans does), E.H. Carr and G.R. Elton take a not dissimilar view. Carr sees the foundations of modern historiography as laid down in the eighteenth century and thinks that "the classical civilization of Greece and Rome was basically unhistorical",²⁷⁶ while Elton thinks that Clarendon, Gibbon and Macaulay "wrote in the prehistoric age"²⁷⁷ and "the scientific, ordered, systematic study of history really began only in the nineteenth century".²⁷⁸

It is not at all inconsistent to think both that historiography began with Herodotus, but that "real" historiography began with Ranke; it is clear that Ranke's conception of historical method was "scientific" in ways in which that of Herodotus was not. When historians think of their subject historiographically, they characteristically think of it in rough outline as beginning with Herodotus, with a jump into disciplinisation around Ranke, following the development of thinking in the Enlightenment. Yet, as we have observed, it is by no means second nature to historians to think of their own subject historiographically, that is, as itself in historical context, and they may even find it difficult: "In some ways it is difficult for us today to grasp this revolution in historiography and the change of paradigm than then occurred, for we have all accepted the new historical methods and the views that were then established".²⁷⁹ The

²⁷⁵ Richard J. Evans, *In Defence of History*, pp. 15-44.

²⁷⁶ E.H. Carr, *What is History?*, p. 103.

²⁷⁷ G.R. Elton, *The Practice of History*, p. 14.

²⁷⁸ *Op. cit.*, pp. 13-4.

²⁷⁹ Thomas H. Brobjer, "Nietzsche's relation to historical methods and nineteenth-century German historiography", *History and Theory* 46, 2007, pp. 155-179 at p. 157.

difficulty may in part be due to the earlier-noted claim that pre-modern concepts require “interpretation and exegesis” if they are to be made intelligible to us.²⁸⁰ Of historians of historiography, some begin with Herodotus, and some with Ranke; and historians who start with Ranke know that they are leaving out the earlier history of their subject.

However, a historiography of historiography which holds it appropriate to begin with Germany in the first half of the nineteenth century, on the ground that this is when “real” historiography starts, is also holding that the Rankean conception of the discipline is in some way essential to it, not merely necessary but perhaps even sufficient, so central indeed that earlier characteristics of historiography, in so far as they are different, can rightly be ignored and so *excluded* from our understanding of the discipline’s history and hence excluded from our understanding of the discipline. Now that approach might – just – be appropriate to our understanding of the subject if historiography’s earlier characteristics entirely continued into the Rankean period while accompanying the new developments, so that beginning with the Rankean period and concentrating on its developments lost nothing from the past while adding more in the way of increasingly scientific historiographical methods. In that case the reader would be missing nothing about the subject in being told only of the Ranke and post-Ranke situation. (Of course, given that the pre-Ranke situation had not been presented to them, they would have to take this imagined fact on trust.)

But, just because some historians of historiography begin with Herodotus and some with Ranke, we cannot say *a priori* that the *characteristic* understanding of the subject’s history is sufficiently grasped through the one approach rather than the other, and until far more study of historiography’s own history takes place, it is at least an open question what may or may not be missed by using the shorter time-frame, and, worse and importantly, this leaves unchallenged the view that Ranke’s conception of the discipline is essential to it. In our own era, when postmodernist attacks on “objectivity” have put so much of a “scientific” approach to historiography in doubt, when historiography is less allowed to be engaged in “for its own sake” than it once

²⁸⁰ Translator’s Introduction, in Reinhart Koselleck, *Futures Past*, p. x.

was, and when historians can often seize a place as “public intellectuals”, we may well think that Rankean skills in achieving “objectivity” are a mere means to what may be an impossible end, and are by no means the most significant feature of the discipline. Moreover, Herodotus and Thucydides are far more plausibly to be seen as “public intellectuals” than Ranke ever was.²⁸¹ Major questions may then be begged by ignoring historiography’s earlier history, not least those questions of change, progress and comparison which only arise over long periods of time.

Our own concern is to outline the history of historiography in order to highlight historians’ characteristic understanding of their subject. While it is clear that, for example, a study of Rankean methods may justifiably begin with Ranke, it is plain that the history of historiography, even if not always the historiography of historiography, is characteristically taken by historians to begin with Herodotus. Present-day historians are far more likely to accept him and other classical writers as predecessors in their self-understanding than they are, say, medieval chroniclers or others who saw history as theodicy. Historians may nevertheless write the historiography of medieval historiography, but they do so without thinking of it as *their* history. One practical reason for the comparative “nearness” of classical writers is the very foundation of modern historiography in the skills of classical philology. Those skills were refined in recovering and understanding the works of classical times, and we generally conceive European and more broadly western history as leaving behind medieval religiosity with the rebirth of that understanding. While history itself is surely very rarely – despite Marx’s claims – discontinuous or revolutionary at the macroscopic level, our historiography may well present it as being so. We have already noted the “jump” from the ancients to the Enlightenment in historians’ understanding of the history of their subject, and scholarly investigation of medieval historiography is comparatively recent, with the continuities still to be clarified and made more widely known. In this, the discipline of historiography characteristically perceives its own history in much the same way as other disciplines, and indeed political and cultural life more generally.

²⁸¹ They nevertheless wrote as private individuals, contrasting with “official” historiographers in ancient Chinese historiography. See F.-H. Mutschler, “Sima Qian and his western colleagues: on possible categories of description”, *History and Theory* 46, 2007, pp. 194-200.

Greek and Roman thought and practices are still a touchstone for us in a way in which most pre-Renaissance thought and practices are not.

How do Herodotus and Ranke compare as paradigms for historians? We begin with Ranke, albeit briefly, since we have already given some detail, and will provide more in the later section *Moral judgement in historiography*. The often-expressed view that Ranke's conception of historiography was merely as a truth-recovering exercise is itself unhistorical, in that it is itself not plausibly the outcome of a historiographical truth-recovering exercise. Iggers and von Moltke do excellent historiographical service in presenting Ranke's work. They point out that the famous expression *wie es eigentlich gewesen* "has generally been misunderstood in this country [the U.S.A.] as asking the historian to be satisfied with a purely factual recreation of the past".²⁸² Bentley summarises the point as "the need to say what 'really' happened encouraged an entire branch of historiography – the American – to persist with its cult of objectivity",²⁸³ although, say Iggers and von Moltke, "by the turn of the [twentieth] century, the Rankean tradition...was seriously challenged by the 'New Historians', J.H. Robinson, Frederick Turner, and Charles Beard in the United States, by Henri Berr in France, and by Karl Lamprecht in Germany".²⁸⁴

Say Iggers and von Moltke, "eigentlich" is best understood as "characteristic" or "essential" rather than "actually".²⁸⁵ "It is not factuality, but the emphasis on the essential that makes an account historical".²⁸⁶ As translated by Wilma A. Iggers, Ranke says "to history has been given the function of judging the past, of instructing men for the profit of future years. The present attempt does not aspire to such a lofty undertaking. It merely wants to show how, essentially, things happened".²⁸⁷ But, say Iggers and von Moltke in presenting Ranke's position, "the factual establishment of

²⁸² Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*, p. xix.

²⁸³ Michael Bentley, *Modern Historiography*, p. 39.

²⁸⁴ Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*, p. lxiii.

²⁸⁵ *Op. cit.*, p. xix.

²⁸⁶ *Op. cit.*, p. xx.

²⁸⁷ Leopold von Ranke, "Preface to the first edition of histories of the Latin and Germanic nations" [1824], trans. Wilma A. Iggers, in Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*, pp. 135-138 at p. 137.

events does not yet constitute history. The historian is not a passive observer who merely records the events of the past but, rather like the poet, he actively recreates a situation.”... He “assumes that every individual, institution or culture constitutes a meaningful unity, a *geistige Einheit*, which is capable of comprehension”.²⁸⁸ “Ranke reminds us that history is concerned not merely with the collection of facts but with understanding those facts. But this understanding proceeds only from the intuitive contemplation (*Anschauung*) of the historical subject matter. Such contemplation for Ranke requires that the historian consciously avoid projecting his subjectivity into the subject of inquiry”.²⁸⁹ “The ability to portray the forces of history without interjecting one’s own set of values is the core of objectivity. ...History centers around values”.²⁹⁰ While the historian unavoidably has a moral opinion, impartiality is preserved by that historiographical distance which is a consequence of objectively characterising the unique relationships between individuals in their own historical culture. With the morality of a time tied to that time, “every stage in history must be judged as an end in itself, not as a step in a progression to a higher state”,²⁹¹ and for Ranke this then excludes moral progress in history.

Just how different is this from Herodotus? While characteristically being identified as a historian by later historians, Herodotus is also criticised by some, sometimes to the point of suggesting that he is not really a historian at all. While a philosopher’s view of the matter is secondary to our concerns here, we can in the first instance support Herodotus with half-historian Collingwood’s view.²⁹² Collingwood says that there are four characteristics of historiography, and Herodotus sufficiently meets these for our purposes: “(a) that it is scientific, or begins by asking questions...; (b) that it is humanistic, or asks questions about things done by men at determinate times in the past; (c) that it is rational [answering questions on the basis of evidence]; (d) that it is self-revelatory, or exists in order to tell man what man is by telling him what man has

²⁸⁸ Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*, p. xlii.

²⁸⁹ *Op. cit.*, p. xlvii.

²⁹⁰ *Op. cit.*, pp. xlviiii-xlix.

²⁹¹ *Op. cit.*, p. xlix.

²⁹² Collingwood has many works, including three books, on Roman Britain. Richard J. Evans comments on Collingwood’s *Oxford History of Roman Britain* and *The Idea of History* that “they might have been written by two different people”. Richard J. Evans, *In Defence of History*, p. 11.

done”.²⁹³ (a), (b) and (d) characterise Herodotus, for Herodotus’ history involves a “science of human action”.²⁹⁴ As to (c), “Herodotus makes no mention of evidence”, although it is there because “one is left to gather from the body of his work what his idea of evidence was”; Thucydides, by contrast, made the reference to evidence explicit.²⁹⁵

Denys Hay remarks of Herodotus, Thucydides and Polybius, “only Herodotus can be described as a scholar”, although this may be only to mark a contrast between Herodotus’ largely scholarly life and the very active public lives of the others, rather than a term of academic commendation; nevertheless, Hay surely did not miss the latter implication.²⁹⁶ Bury says of Herodotus “his maxims of historical criticism may be set down as three: (1) Suspect superhuman and miraculous occurrences, which contradict ordinary experience. But this, in his application of it, leaves a wide room for portents, and it does not cover oracles and dreams. (2) When you are confronted by conflicting evidence or differing versions of the same event, keep an open mind; *audi alteram partem*. But this does not save him from a biased acceptance of Athenian tradition. (3) Autopsy and first-hand oral information are superior to stories at second hand, whether written or oral. This tends to take the naïve form, ‘I know, for I was there myself’, and it placed the historian at the mercy of the vergers and guides in Egyptian temples”.²⁹⁷ His application of these principles is “unsatisfactory and sporadic”, but “they are maxims of permanent validity; properly qualified they lie at the basis of the modern developments of what is called historical methodology”.²⁹⁸

Says Collingwood, quoting other historians in the following extract, “History is a Greek word, meaning simply an investigation or inquiry. Herodotus, who uses it in the title of his work, thereby ‘marks a literary revolution’... It is the use of this word, and its implications, that make Herodotus the father of history”,²⁹⁹ and so he is widely acknowledged to have been. Says Momigliano, “He succeeded in putting together a

²⁹³ R.G. Collingwood, *The Idea of History*, p. 18.

²⁹⁴ *Op. cit.*, p. 20.

²⁹⁵ *Op. cit.*, p. 19.

²⁹⁶ Denys Hay, *Annalists and Historians*, p. 7.

²⁹⁷ J.B. Bury, *The Ancient Greek Historians*, New York: Dover, 1958, pp. 69-70.

²⁹⁸ *Op. cit.*, p. 71.

²⁹⁹ R.G. Collingwood, *The Idea of History*, pp. 18-19.

trustworthy account of events he was too young to have witnessed and of countries whose languages he did not understand. ...Herodotus' success in touring the world and handling oral traditions is something exceptional by any standard – something that we are not yet in a position to explain".³⁰⁰ Yet, comments Bury, Herodotus made considerable use of earlier writers "of whom he only names Hecataeus, and usually for the purpose of hinting something uncomplimentary",³⁰¹ although "it was not the fashion or etiquette to name your authorities except for some special reason, – for instance, to criticize them, or to display your own learning".³⁰² Bury continues, "as the works of these writers have perished, it is very difficult to form a fair estimate of the achievements of Herodotus himself as a historical investigator – apart from his transcendent gifts as an artist and man of letters".³⁰³

We may learn here something of how some historians perceive Herodotus, but it is very unclear how Herodotus perceived himself. Herodotus' reputation, however, long predated these other historians' perceptions. Momigliano says, "I have often felt rather sorry for Dionysius of Halicarnassus. How embarrassing it must have been for a budding historian to have the father of history as his own fellow-citizen".³⁰⁴ This is tongue-in-cheek: it was Cicero (106BCE-43BCE), some four hundred years later than Herodotus, whom we know described him as such.³⁰⁵ It is a characteristic feature of historiography that one can use hindsight to describe a person or event in terms which were not available at the time. Thus it is true that the Thirty Years' War began in 1618, but nobody in 1618 could have known that.³⁰⁶ Whether or not Dionysius of

³⁰⁰ "The place of Herodotus in the history of historiography", p. 129.

³⁰¹ J.B. Bury, *The Ancient Greek Historians*, p. 69.

³⁰² *Op. cit.*, p. 50.

³⁰³ *Op. cit.*, p. 69.

³⁰⁴ They were not fellow-citizens at the same time: Herodotus ca. 484 BCE-ca. 425 BCE; Dionysius 60 BCE-7CE. "The place of Herodotus in the history of historiography", A.D. Momigliano, p. 127.

³⁰⁵ *Ibid.*

³⁰⁶ These are known as "narrative sentences", and the example is owed to Danto. They describe an earlier event in terms which involve referring to a later event not knowable at the earlier time. Thus only hindsight enables these to be constructed. Earlier we noted in Butterfield that a minor figure "foreshadows Ranke", which is also an example of a narrative sentence. See A.C. Danto, *Analytical Philosophy of History*, London: Cambridge University Press, 1965, p. 152 and chap. VIII, *passim*. (*Analytical Philosophy of History* was later published as *Narration as Knowledge*, New York: Columbia University Press, latest edn. 2007.) Mark Day uses the expression "open narrative sentences" for those constructions which, as it were, await yet fuller redescription: "An open narrative sentence is about a past event, but which refers to it in terms of later events, *including events that have not taken place by the time of the speaker*" (Day's emphasis). These can bind the future, in

Halicarnassus knew of Cicero's description of Herodotus as the father of history, being the "father of history" could not possibly be part of Herodotus' self-understanding. Dionysius could, however, still be embarrassed by what he perceived as some difference of quality between himself and Herodotus: he "was wholeheartedly devoted to the memory of his formidable predecessor. Dionysius is in fact the only ancient writer who never said anything unpleasant about Herodotus".³⁰⁷ "For the Greeks and the Romans he was the father of history. Sophocles [495-406; see his *Oedipus*] was his friend, Aristophanes [40 years younger, 446-388] parodied him, Theopompus [ca. 380-ca. 320, 100 years later] epitomized him, and Aristarchus [of Samothrace; ca. 217- ca. 145, over 250 years later] wrote commentaries on him".³⁰⁸

Herodotus owes prose history to his near contemporary Hecataeus [ca. 550-ca. 480], who developed the work of those we now sometimes describe as logographers rather than historians. Logographers attempted to organise into prose various poetic mythical traditions, including genealogies of families back to the gods and heroes. A developing feature of this approach was recognition that there were different such traditions in different geographical places, and Hecataeus is famous for the range and practicality of what has been described as in effect a multi-faceted travel guide. Herodotus seeks to unify this multiplicity of traditions in a single historical approach, meanwhile adopting a firmer critical attitude than did Hecataeus to the matters told in the various received traditions which function for him as sources.³⁰⁹

What, then, did Herodotus (or Dionysius) see themselves as doing? Even Dionysius, says Momigliano, "never dared to defend Herodotus from the most serious accusation of his enemies, the accusation of being a liar. To us it may perhaps seem odd that the ancients saw nothing incongruous in being at one and the same time the father of history and a liar. But, as far as I know, Francesco Petrarca was the first to notice the

so far as they may express the speaker's will about what should take place. Mark Day, *The Philosophy of History: An Introduction*, London: Continuum, forthcoming 2008.

³⁰⁷ "The place of Herodotus in the history of historiography", A.D. Momigliano, p. 127.

³⁰⁸ "Historiography on written tradition and historiography on oral tradition" (1961-62), A.D. Momigliano, *Studies in Historiography*, pp. 211-220 at p. 213.

³⁰⁹ Herodotus contrasts his experience in Egypt with that of Hecataeus in *Herodotus: Literally Translated from the Text of Baehr*, trans. Henry Cary, London: Routledge, approx. 1847, Book 2, *Euterpe*, chaps. 143-144.

implicit contradiction between these two terms and to object to it”³¹⁰ The curiosity is that, as Momigliano points out, it was Cicero who gave the two descriptions “father of history” and “notorious liar”, and moreover did so in the very same sentence; and it took Petrarch (1304-1374) to notice it. “Petrarch was shocked by the suggestion that the father of history could be the author of a forgery”.³¹¹ But all this turns out to be Petrarch’s fault, according to Momigliano, for Cicero was merely expressing the by his time traditional opinion: “Herodotus was not denied the place of ‘*primus inventor*’ of history, but at the same time was distrusted to the point of being considered a liar”.³¹² This seems a rather technical defence. It is plausible to hold that historiography that *lies* is historiography that *fails*, in which case the problem remains why Cicero, or anyone, would ascribe historiographical success (at any level) to Herodotus while also characterising him as a liar. Plainly truth is not the only important element for them.

Herodotus’ reputation continued positively and negatively with Thomas Babington Macaulay (1800-1859), who says, “of the romantic historians Herodotus is the earliest and best. His animation, his simple-hearted tenderness, his wonderful talent for description and dialogue, and the pure sweet flow of his language, place him at the head of narrators. He reminds us of a delightful child. There is a grace beyond the reach of affectation in his awkwardness, a malice in his innocence, an intelligence in his nonsense, an insinuating eloquence in his lisp. ...he is, from the first to the last chapter, an inventor”.³¹³ Bury, in a lengthy treatment of Herodotus and others, remarks “Gibbon happily observed that Herodotus ‘sometimes writes for children and sometimes for philosophers’; the anecdotes he relates often appeal to both”.³¹⁴ As Herodotus is the father of history, so also is he the father of prose composition”, says James Westfall Thompson.³¹⁵ The reputation continued negatively with Herbert Butterfield, who, commenting on La Popelinière’s (1540-1608) *Histoire des Histoires*,

³¹⁰ “The place of Herodotus in the history of historiography”, A.D. Momigliano, p. 127.

³¹¹ *Op. cit.*, p. 128.

³¹² *Ibid.*

³¹³ Thomas Babington Macaulay, “History”, *The Edinburgh Review*, 1828, from an extract in Fritz Stern (ed.), *The Varieties of History*, p. 73.

³¹⁴ J.B. Bury, *The Ancient Greek Historians*, p. 57.

³¹⁵ James Westfall Thompson, *A History of Historical Writing*, vol. I, p. 25.

reports without contradicting it that “repeatedly he speaks of the lies of Herodotus”;³¹⁶ otherwise Herodotus is mentioned only twice in *Man on his Past*, and then trivially. Certainly Herodotus’ work presents itself to our own eyes almost as mythical: “The entire work of Herodotus is in nine books, each of which bears the name of one of the Muses, a fact which in itself shows how intimate was the early association between history and epic poetry”.³¹⁷ But it is not what it seems: “The division into nine Books is not due to the author himself, for in his day such divisions had not yet come into fashion”.³¹⁸ On the other hand, says Bury, “he had never clearly defined the border between the domains of the credible and the incredible”.³¹⁹

So *what* did Herodotus successfully father? And what is the relationship between that and a reputation for lying? “In one sense, history writing for the Greeks began with Homer. In another more formal sense, history was not only a new literary genre but a radically new kind of genre when Herodotus and Thucydides began to write in the fifth century B.C. Because Herodotus and Thucydides wrote in prose, what they wrote was open ended in a way that poetry had never been. The first historians had to make explicit in their texts both the nature of their subject matter and the authorial stances that they intended to take”.³²⁰ “Herodotus was as much an investigator and an explorer as a reciter of narrative, and his life-long investigation was ‘history’ in his Ionian speech. Yet Herodotus himself hints that the word may also be applied to the story which the research has made possible, not to the guileless tale of the uncritical, to be sure, but to a narrative such as he and his soberly inquisitive fellows could tell”.³²¹

“Soberly inquisitive” Herodotus may have been, but “while he wrote their language, he could not rid himself of a strong native prejudice against the Ionians. They are practically the only people in his whole narrative to whom he is consistently unfair”,³²²

³¹⁶ Herbert Butterfield, *Man on his Past*, Appendix I, “La Popelinière’s ‘Histoire des Histoires’”, p. 205.

³¹⁷ James Westfall Thompson, *A History of Historical Writing*, vol. I, p. 26.

³¹⁸ J.B. Bury, *The Ancient Greek Historians*, pp. 37-38.

³¹⁹ *Op. cit.*, p. 60.

³²⁰ Carolyn Dewald, “Practical knowledge and the historian’s role in Herodotus and Thucydides”, in *The Greek Historians: Literature and History: Papers Presented to A.E. Raubitschek*, Department of Classics, Stanford University; Saratoga: ANMA Libri, 1985, pp. 47-63 at p. 47.

³²¹ James T. Shotwell, *The History of History*, vol. I, pp. 8-9.

³²² *Op. cit.*, p. 177.

says Shotwell, a point put differently by Bury: “if he had heard himself described as an Ionian writer, he would have been vastly indignant. He is at great pains to dissociate himself from Ionia and Ionian interests. ...he shows a hardly veiled contempt for a people which, as he says, had been thrice enslaved”.³²³ Herodotus’ work was “pragmatical; it reflected the author’s political beliefs, and exhibited a strong bias in the preference given to Athenian sources. But it was the work of a historian who cannot help being partial; it is not the work of a partisan who becomes a historian for the sake of his cause”.³²⁴ But maybe Herodotus’ failing lay in the impossible vastness of his enterprise: “Two views prevail concerning the scope of history. One regards it, to use the expression of an eminent living writer, as being concerned only with states... The other, which has found illustrious exponents from Herodotus downwards, conceives it to be a picture of the whole past, including everything that man has either thought or wrought”.³²⁵

And was the project impossibly vast? Says Thompson, “Herodotus conceived the idea that history was more than an array of striking and discontinuous facts, and that under their apparent disorder there was a unity and an association, and that the function of the historian was to distinguish greater from lesser facts and to associate them together in proper order”.³²⁶ Herodotus knowingly *excluded*, and at one point says, “over all these, as well as over those that formed the land-army, native officers were appointed to each; but I do not mention their names, for I am not necessarily constrained to do so for the purpose of the history”.³²⁷ Bury observes, “Herodotus is irreproachably comprehensive”, his work “has the higher quality of what we mean by universal history or *Weltgeschichte*, in focussing under one point of view, and fitting into a connected narrative, the histories of the various peoples who came into relations with one another, within a given range”.³²⁸ Thompson continues, “That ‘dignity of history’ which he deeply felt also makes Herodotus a moralist. Throughout his narrative he

³²³ J.B. Bury, *The Ancient Greek Historians*, pp. 61-62.

³²⁴ *Op. cit.*, p. 65.

³²⁵ “Prefatory Note” to *The English Historical Review* (Mandell Creighton, Editor) [1886], in Fritz Stern (ed.), *The Varieties of History*, pp. 174-177 at p. 175.

³²⁶ James Westfall Thompson, *A History of Historical Writing*, vol. I, p. 26.

³²⁷ *Herodotus*, trans. Henry Cary, Book 7, *Polymnia*, chap. 96.

³²⁸ J.B. Bury, *The Ancient Greek Historians*, p. 45.

sets forth the wisdom of those who govern; he makes history teach by example”.³²⁹ Yet – while in its way still teaching by example – what brings “rulers to defeat is a very practical kind of ignorance. Croesus dismisses his army too soon after an indecisive battle... Darius undertakes an invasion of Scythia without understanding the nature of the men and country against whom he marches”.³³⁰ Herodotus sets forth the lack of wisdom too.

Despite his later reputation, Herodotus worried about accuracy: he regularly made clear that such-and-such was *reported* to have happened, not that it *did* happen, as if to distance himself from affirming on his own part that the content reported was true: “The learned among the Persians assert that...”, “they say” and “they add” all appear in the first chapter of book 1, with a repeat of a similar set in chapter 86. Chapter 182 has “These same priests assert, though I cannot credit what they say”, and Book 4, chapter 105, includes “Though they affirm this, however, they do not persuade me; they affirm it nevertheless, and support their assertion with an oath”.³³¹ However, while some such distancing may disappear in translation, hundreds of pages pass without remarks of this kind. Collingwood, writing in a different context on Livy, remarks “Like Herodotus, he is often charged with the grossest credulity; but, like Herodotus, wrongly. He does his best to be critical; but the methodical criticism practised by every modern historian was still not invented”.³³² “Historians are not unaware”, remarks David M. Lewis, “that Herodotus’ truthfulness has been challenged from time to time, but on the whole they take no notice”.³³³ Momigliano judges that “we have now collected enough evidence to be able to say that he can be trusted. Curiously enough we are in a better position to judge him as a historian of the East than as historian of the Persian wars. ...Orientalists...have ascertained that he described truthfully what he saw and reported honestly what he heard. ...We are not so well placed for the history of the Persian Wars because Herodotus himself remains

³²⁹ James Westfall Thompson, *A History of Historical Writing*, vol. I, p. 26.

³³⁰ Carolyn Dewald, “Practical knowledge and the historian’s role in Herodotus and Thucydides”, p. 49.

³³¹ I owe the last two references to Carolyn Dewald, *op. cit.*, p. 48, note.

³³² R.G. Collingwood, *The Idea of History*, p. 37.

³³³ David M. Lewis, “Persians in Herodotus”, in *The Greek Historians: Literature and History*, pp. 101-117 at p. 101.

our main source”.³³⁴ The assumption here “has to be that Herodotus is right, except when he can be shown to be wrong. ...If you start with the postulate that you should use no Herodotus unless he can be shown to be right, you just won’t get very far”.³³⁵

Donald Lateiner puts the conclusion well: “The personality of a writer can be found only in his text. There we may detect patterns of thought, habitual standards of comparison, concepts of relevance and significance, historiographical notions, and ideas about morality and causality. Herodotus’ report on distant places and by-gone times attempted to overcome his contemporaries’ conceptual limitations. However, ...although Herodotus has characteristic ideas, expressions, and linguistic habits, his work lacks a dominant theory, a regulating ideology, or a controlling metaphor that can aid, or indeed, hinder historical understanding and research, and win a following”.³³⁶

Clearly, Herodotus and Ranke share a worry about historical *truth*, and later historians comment on past historians accordingly. Without mentioning Herodotus by name, Thucydides – “not an original thinker”³³⁷ – comments: “in investigating past history, and in forming the conclusions which I have formed, it must be admitted that one cannot rely on every detail which has come down to us by way of tradition. People are inclined to accept all stories of ancient times in an uncritical way... However, I do not think that one will be far wrong in accepting the conclusions I have reached from the evidence which I have put forward. It is better evidence than that of the poets, who exaggerate the importance of their themes, or of the prose chroniclers, who are less interested in telling the truth than in catching the attention of their public, whose authorities cannot be checked, and whose subject-matter, owing to the passage of

³³⁴ “The place of Herodotus in the history of historiography”, A.D. Momigliano, pp. 128-129.

³³⁵ David M. Lewis, “Persians in Herodotus”, p. 102. The most recent confirmation of Herodotus’ accuracy comes from DNA testing, confirming his assertion that the Etruscan civilisation was founded by seafarers from Turkey (Alessandro Achilli et al., “Mitochondrial DNA variation of modern Tuscans supports the Near Eastern origin of Etruscans”, *The American Journal of Human Genetics*, February 6, 2007; <http://www.journals.uchicago.edu/AJHG/> accessed February 17 2007.).

³³⁶ Donald Lateiner, “Limit, propriety, and transgression in the *Histories* of Herodotus”, in *The Greek Historians: Literature and History*, p. 87. However, Lawrence Stone observes in a different context, “the new British school of young antiquarian empiricists” are “trying to remove any sense of ideology”. Lawrence Stone, “The revival of narrative: reflections on a new old history”, p. 20.

³³⁷ M.I. Finley, “Introduction” to Thucydides, *History of the Peloponnesian War*, trans. Rex Warner [1954], London: Penguin, 1972, p. 31.

time, is mostly lost in the unreliable streams of mythology. We may claim instead to have used only the plainest evidence and to have reached conclusions which are reasonably accurate".³³⁸

But it is not just truth: "what chiefly interests Herodotus is the events themselves;³³⁹ what chiefly interests Thucydides is the laws according to which they happen",³⁴⁰ a point made by Collingwood with which many historians would agree. As noted, Herodotus excelled in distinguishing "greater from lesser facts" and associating them "in proper order",³⁴¹ but his mode of "association" did not "win a following". Of less concern to historians would be Collingwood's requirement that the historiographical attitude should not involve the "wrong" metaphysics: "In Herodotus we have an attempt at a really historical point of view. For him events are important in themselves and knowable by themselves. But already in Thucydides the historical point of view is being dimmed by substantialism. For Thucydides the events are important chiefly for the light they throw on eternal and substantial entities of which they are mere accidents".³⁴² That historians must structure their facts goes back to Herodotus. Later historians share his problems in recovering and structuring facts about the past. Is Ranke an advance on this general understanding of historiography? Not obviously: "Ranke...refuses to allow that anybody is a greater historian than Thucydides",³⁴³ and – the detail is unimportant in this overview – it is not at all clear that Ranke's "spiritual substances...thoughts of God"³⁴⁴ are an advance on what Collingwood calls Thucydides' "substantialism", and neither are ahead of Herodotus as paradigmatic of historians' self-understanding in this respect.

³³⁸ Thucydides, *History of the Peloponnesian War*, trans. Rex Warner, pp. 46-47.

³³⁹ Reminding us of Ranke's "a feeling for and a joy in the particular in and by itself". Leopold von Ranke, "On the relations of history and philosophy", p. 30.

³⁴⁰ R.G. Collingwood, *The Idea of History*, p. 30.

³⁴¹ James Westfall Thompson, *A History of Historical Writing*, vol. I, p. 26.

³⁴² R.G. Collingwood, *The Idea of History*, pp. 42-43.

³⁴³ Herbert Butterfield, *Man on his Past*, p. 106.

³⁴⁴ Richard J. Evans, *In Defence of History*, p. 16, quoting from the 1973 Indianapolis edition of Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*.

One often hears the quotation “The past is a foreign country: they do things differently there”.³⁴⁵ But it isn’t; for historians of historiography, it is largely the *same* country. That pretty implication of relativism is not true. Historians from Herodotus to the present have characteristically worried and disagreed about interrelated issues: the nature and justification of historical truth and the role of historiographical truth-telling, the acceptability and grounds of moral judgement in historiography, the historiographical synthesis of facts (including analytical and substantive theories of historical explanation), and historians’ role or function in society. We will now examine these issues in more detail.

³⁴⁵ L.P. Hartley, *The Go-Between*, Prologue.

Historians' self-understanding

Historians do not characteristically think that the historiography of historiography is a good way of presenting or examining the nature of their discipline. For many historians, the character of historiography is best outlined in a range of works of a certain kind which are offered by those historians to those student historians who are asked to reflect on what they are doing. Our historiography of historiography will introduce a number of these works. Yet while we are, in the appropriate sense, ourselves writing part of the historiography of historiography in presenting these historians, we may note in advance that when these historians introduce their subject to fledgling historians they too – like Butterfield in *The Whig Interpretation* – typically do not themselves introduce the *history* of their subject at all.

An example of this is *What is history?*, asked by E.H. Carr in 1961.³⁴⁶ Carr himself began by quoting Lord Acton's 1896 expectation that, all information being within the historian's reach, ultimate history would be available to a later generation. By contrast, the later generation, in the person of George Clark in 1957, again as quoted by Carr, denied that ultimate history would be available, and expected historical work to be constantly superseded. Are there, then, historical facts? Referring to Benedetto Croce, Michael Oakeshott, R.G. Collingwood and Friedrich Nietzsche, Carr presented in his first chapter some broad outlines of the philosophy of historical "facts", concluding with the view that history is "a continuous process of interaction between the historian and his facts".³⁴⁷ Subsequent chapters concerned the main philosophical issues of the day, organised much as philosophers might organise them, relating to the relationship between society and individual, the nature of science and morality, causation, and progress. Generations of British historians have cut (or more likely blunted) their philosophical teeth on Carr, although it is certain that they cut few historical teeth there, for the historiography of historiography is largely missing.

There are, however, some references to the past. "Herodotus", Carr remarks, "the father of history, defined his purpose in the opening of his work: to preserve a memory of the deeds of the Greeks and the barbarians, 'and in particular, beyond

³⁴⁶ E.H. Carr, *What is History?*, pp. 1-2.

everything else, to give the cause of their fighting one another’”. Herodotus, Carr continues, “found few disciples in the ancient world: even Thucydides has been accused of having no clear conception of causation”.³⁴⁸ Herodotus “had few children”,³⁴⁹ although Carr elsewhere quotes Polybius: “wherever it is possible to find out the cause of what is happening one should not have recourse to the gods”.³⁵⁰ Here neither Herodotus nor Thucydides are placed in any kind of historical context, but are included – indeed judged for inclusion – on the basis of their position with regard to causation. This is because, for Carr, “the study of history is a study of causes”.³⁵¹ The idea of causation, Carr says, was central to the foundations of modern historiography as they were laid down in the eighteenth century, and Carr here refers to Montesquieu’s claim that humanity is governed by laws, by causes and effects, and quotes Voltaire’s famous comment: “If you have nothing to tell us except that one barbarian succeeded another on the banks of the Oxus and Jaxartes, what is that to us?”³⁵² Carr’s tiny piece of historiography of historiography is a hindsight judgement of past historians – recognised as historians – made on the basis of a philosophy of historiography, one which puts causation at the centre of the modern discipline, and one which ascribes to eighteenth-century philosophers the foundations of modern historiography. Yet Carr does not suggest that Thucydides should be excluded from the ranks of historians if his concept of causation was not sound enough.

Despite the claimed centrality of causation, Carr has a different second view: “the classical civilization of Greece and Rome was basically unhistorical”,³⁵³ because on the whole there was little concern with either future or past: “Thucydides believed that nothing significant had happened in time before the events which he described, and that nothing significant was likely to happen thereafter”.³⁵⁴ So, we must conclude, a concern with meaning or significance is seen by Carr as at least appropriate, if not necessary, for historiography, for it was enough (at the time) to turn the unhistorical

³⁴⁷ *Op. cit.*, p. 24.

³⁴⁸ *Op. cit.*, p. 81. The accusation against Thucydides is attributed to F.M. Cornford.

³⁴⁹ *Op. cit.*, p. 103.

³⁵⁰ *Op. cit.*, p. 68, footnote.

³⁵¹ *Op. cit.*, p. 81.

³⁵² *Op. cit.*, p. 82.

³⁵³ *Op. cit.*, p. 103.

³⁵⁴ *Op. cit.*, p. 104.

into the historical. (Carr is the one saying this, of course, not Thucydides.) He notes that the required new element came in when history was seen, first by the Jews and then by the Christians, to have meaning or purpose: as we noted earlier, “history itself became a theodicy”,³⁵⁵ that is, historiography became exactly “a writing, doctrine, or theory intended to ‘justify the ways of God to men’”.³⁵⁶ “This was the mediaeval view of history”.³⁵⁷ History long kept its sense of meaningfulness as purpose, in due course seeing that purpose as the perfection of humankind. Carr refers here to Edward Gibbon’s observation of progress and Acton’s description of history as “a progressive science”.³⁵⁸ The modern historiographical beliefs in causation and in meaningful progress, either and both of which are used by Carr to identify past historiography, are seemingly alike valid for that purpose; they are, however, plausibly incompatible, and Carr presents G.W.F. Hegel and Karl Marx as philosophers who attempt to resolve the apparent incompatibility.³⁵⁹ Historiography, we may conclude from Carr’s own brief historiography of the subject, may be characterised by the desires to preserve the memory of, to give the causes of, or to give meaning to historical events, in addition to being characterised as “a continuous process of interaction between the historian and his facts”.³⁶⁰ It is to be noted, in brief summary, that Carr does not *derive* his view of the character of historiography from the work of past historians, but rather, where he refers to past historians, does so in the light of his own view of what historiography is.

Many teachers of history recognised Carr’s limitations or disliked his relativism, and often set against him Sir Geoffrey Elton’s 1967 *The Practice of History*.³⁶¹ Evans remarks that there is “something rather strange about two books written more than thirty years ago still serving as basic introductions to a scholarly discipline”.³⁶² There is something *unhistorical* about this, certainly; as if Carr and Elton had escaped their own historical context. In Elton we find pages of personal attack on Carr, for example the following: “the more obvious and common danger is that exemplified by

³⁵⁵ *Ibid.*

³⁵⁶ Entry on “theodicy”, *The Shorter Oxford English Dictionary*, 3rd edn., Oxford: Clarendon Press, 1959.

³⁵⁷ E.H. Carr, *What is History?*, p. 104.

³⁵⁸ *Op. cit.*, p. 105.

³⁵⁹ *Op. cit.*, pp. 107ff.

³⁶⁰ *Op. cit.*, p. 24.

³⁶¹ G.R. Elton, *The Practice of History*, 1969.

Mr Carr: to write off certain forms of historical study and to reserve approval for those to which one happens to incline oneself".³⁶³ We also find explicit philosophical opposition to Carr's relativism: "the independent and real existence of historical events implies that, in theory at least, they can be observed absolutely, and for a very large number of somewhat basic facts this holds good".³⁶⁴ "A man was kicked to death in 1850: that is a fact, an event, which took place and which nothing now can either make or unmake".³⁶⁵ Elton distances himself from philosophers as far as he can: although there are "sensible" books written by, for example, W.H. Walsh, P.L. Gardiner, W.B. Gallie and A.C. Danto, "every new number of *History and Theory* is liable to contain yet another article struggling to give history a philosophic basis... they do not, I fear, advance the writing of history".³⁶⁶ It is the views of his contemporary historians, in the main, which illustrate the book's debates.

In many ways Elton's is a book which is philosophically uninformed, not so much from ignorance (although that is undeniably present) but from an insistence on keeping philosophy out, particularly Carr's kind of philosophy. The ignorance is deliberately preserved. Nevertheless philosophy lies at the heart of it, because, like Carr's *What is History?*, it is the relevant philosophical issues of his day which the book mainly addresses: we are to understand from it the nature of facts (although with rather more on their evidential support than Carr provides), the nature of historical truth and writing, the kinds and categories and purposes of history. "Like all sciences, history, to be worthy of itself and beyond itself, must concentrate on one thing: the search for truth".³⁶⁷ "The quality of an historian's work must... be judged purely by intellectual standards".³⁶⁸ Causation, in opposition to Carr, is not central: "to suppose that causal relationships are the main content of history is an error",³⁶⁹ for causation is merely one of several ways in which historical events may be "linked and rendered

³⁶² Richard J. Evans, *In Defence of History*, p. 2. [See Evans' footnotes.]

³⁶³ G.R. Elton, *The Practice of History*, p. 27.

³⁶⁴ *Op. cit.*, p. 80.

³⁶⁵ *Op. cit.*, p. 76.

³⁶⁶ *Op. cit.*, pp. viii, 70.

³⁶⁷ *Op. cit.*, p. 68.

³⁶⁸ *Op. cit.*, p. 69.

³⁶⁹ *Op. cit.*, p. 23.

comprehensible”.³⁷⁰ This is consistent with Koselleck’s view: “Without any doubt, it is a property of modern historical methodology to avoid chance wherever possible. By contrast, up until the eighteenth century, it was quite usual to make use of chance, or luck in the form of fortune, in the interpretation of histories”.³⁷¹ As to the purpose of historiography, “the study of history is legitimate in itself, and any use of it for another purpose is secondary”.³⁷²

It would be natural, given Elton’s rejection of the philosophical, to suppose that he would be more ready than was Carr to place historians in historical context. But that is not so: there are even fewer references to past historians than Carr gives. Moreover, only once does Elton point out the need to set historians in historical context, and that is in the course of chiding a younger contemporary for failing to recognise the historical context of those past historians who were the butt of that younger contemporary’s criticisms. Those past historians were “possessed of principles and creeds as definite, and as potentially limiting, as those proclaimed by their critics”,³⁷³ a remark which is reminiscent of Kuhn’s, re-quoted here: “the more carefully [historians] study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If these out of date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge”.³⁷⁴ Elton does not realise the opening he here gives for the possibility of a Carr-like attack on “facts”. He also leaves us with the sense that, while he, Elton, stands above the process of history, other historians, and in particular his critics, do not.

³⁷⁰ *Op. cit.*, p. 23.

³⁷¹ Reinhart Koselleck, *Futures Past*, p. 117.

³⁷² G.R. Elton, *The Practice of History*, p. 66.

³⁷³ *Op. cit.*, p. 18.

³⁷⁴ Thomas S. Kuhn, *The Structure of Scientific Revolutions*, p. 2.

Although he observes that the classical minds of India and China are, unlike our own classical inheritance, “a-historical”,³⁷⁵ although he notes that “no other primitive sacred writings are so grimly chronological and historical as is the Old Testament”,³⁷⁶ and although he states that “the much advertised social questions – structure, habits and ambitions – have in fact been asked by historians since Herodotus”,³⁷⁷ these asides are not characteristic of Elton’s general position. More characteristic is his view that not all civilisations have been as concerned as we have been to “write human history as it really was”,³⁷⁸ so flagging up in passing an approving reference to Ranke’s *wie es eigentlich gewesen*, understood as “how it actually was”.³⁷⁹ The matter of presenting past historians, whether in or out of their historical context, does not even arise, since Elton here sees them, at least in general, as a rather poor lot.

“Medieval chroniclers occasionally rose above their annals to reflect and explicate... It was, in fact, the seventeenth century that took the first purposeful steps towards serious historical study”.³⁸⁰ “The scientific, ordered, systematic study of history really began only in the nineteenth century, because only then did historians absorb the lessons of the antiquarians and develop to the full the techniques which enabled them to answer the common charge that their reconstruction of the past was just a tale, amusing and instructive enough, but without any rigour, certainty or standard of truth”.³⁸¹ “It is not just knowledge... which makes the true historians; ...the difference lies in the different attitude and purpose brought to the study by a mind trained in history as a scientific and intellectual approach”.³⁸² Just like Carr, Elton does not derive his view of the character of historiography from the work of past

³⁷⁵ Such Asian minds are plausibly not “a-historical”, but our understanding of the comparisons is still developing. See “Forum: Chinese and Western historical thinking”, ed. Jörn Rüsen, *History and Theory* 46, 2007, pp. 180-232.

³⁷⁶ G.R. Elton, *The Practice of History*, p. 12.

³⁷⁷ *Op. cit.*, p. 39.

³⁷⁸ *Op. cit.*, p. 11.

³⁷⁹ As earlier mentioned, a better translation seems to be, “how it characteristically or essentially was”. See, for comment, “Introduction”, p. xix, Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*, and p. 137, “it merely wants to show how, essentially, things happened” (“Preface to the first edition of histories of the Latin and Germanic nations”, 1824). Elton, however, clearly believes in the “how it actually was” goal for historiography.

³⁸⁰ G.R. Elton, *The Practice of History*, pp. 12-3.

³⁸¹ *Op. cit.*, pp. 13-4.

³⁸² *Op. cit.*, p. 15.

historians, but rather, where he characterises past historians, does so in the light of his own view of what historiography is, and, given that, he *judges* them, albeit cursorily.

It might well be thought that Carr and Elton, in sharing this unhistorical recognition of the central issues in the characterisation of historiography, share something of importance; their difference over the “subjective” and “objective” understanding of facts may well seem more minor. However, even this difference is less clear than it might be: objective historical knowledge for Elton seems less central than one might expect from his opposition to Carr: “history always has posed and always will pose the sort of problems which give rise to dispute, acrimony, and the writing of hostile reviews. Why, at the very beginning of our science stands the prototype of all these arguments: history had barely begun when Thucydides attacked the methods and purposes of Herodotus. Debates among historians are coeval with the writing of history...”³⁸³

The outsider to historiography may well think that acrimony and hostility do not seem particularly appropriate to a serious, scientific, ordered, systematic and intellectual discipline such as Elton here claims historiography to be. Elton’s overall view is a somewhat confused one: historiography, conceived from its beginnings in Herodotus and Thucydides, is characterised by debate, by “reflecting and explicating”, and is also characterised (since the nineteenth century) by a scientific attitude which puts truth first. This last seems not to be supposed by Elton to have replaced the earlier conceptions, but to have been in some way continuous with them. No historiographical context is offered, however, on the basis of which such issues might be addressed. By now it perhaps goes without saying that no philosophical context is offered, either.

Almost contemporaneously with Elton, Arthur Marwick in *The Nature of History* describes the nature of historiography and its place in modern society, the public role of historians, the relationships between history and the social sciences, and some

³⁸³ *Op. cit.*, pp. 16-7.

practical problems of historical research and writing.³⁸⁴ M.M. Postan does much the same in *Fact and Relevance*.³⁸⁵ Jack Hexter's "The Rhetoric of History" analyses the craft of historical writing, the nature of historians' language, and their modes of explanation and communication.³⁸⁶ Decades later, Ludmilla Jordanova's *History in Practice* maps the current theory and practice of historiography, locates historiography with respect to other disciplines, and recognises that "in recent decades the practice of history has become ever more bound up with politics".³⁸⁷ Evans defends the discipline of historiography against postmodernism, again organising his material in terms of the familiar philosophical agenda of facts, causation, knowledge and objectivity.³⁸⁸ For these many authors, explaining historiography to us, just as with Carr and Elton, does not involve placing historians themselves in historical context.

Carr, Elton and these others are not just representatives of some peculiarly British failure to recognise historiography's own history as importantly constitutive of the subject and as an appropriate frame for presenting it. Earlier, French medieval and economic historian Marc Bloch had written in brave and ultimately fatal circumstances *The Historian's Craft*,³⁸⁹ analysing the boundaries between past and present, the nature of evidence, the critical method, historical analysis and historical causation. The book is deliberately unhistorical in its presentation of the historian's craft, and refers in passing (although not, as it plausibly should, in explanation of its own approach) to French group psychology: "Cournot long ago observed that the French people in the mass, everlastingly inclined to reconstruct the world on lines of reason, live their collective memories much less intensely than the Germans, for example",³⁹⁰ adding a further quotation from that "unhistorical Frenchman": "...I believe we must also take into account the scant popularity of our history and the underdeveloped consciousness

³⁸⁴ Arthur Marwick, *The Nature of History*, London: Macmillan, 1970. There are three pages here on past historians.

³⁸⁵ M.M. Postan, *Fact and Relevance: Essays on Historical Method*, London: Cambridge University Press, 1971.

³⁸⁶ J.H. Hexter, "The rhetoric of history", 15-76.

³⁸⁷ Ludmilla Jordanova, *History in Practice*, London: Arnold, 2000, p. 1.

³⁸⁸ Evans has a useful chapter on "The history of history" but it is largely a descriptive overview without setting historians in historical context, without, that is, what one might think of as historiographical *explanation*: "Ranke's principles still form the basis for much historical research and teaching today". Richard J. Evans, *In Defence of History*, p. 19.

³⁸⁹ Marc Bloch, *The Historian's Craft* [1941], trans. Peter Putnam, Manchester: Manchester University Press, 1954.

of historical tradition among our lower classes, for reasons too lengthy for analysis”.³⁹¹ Ranke, prefacing in 1852 his approach to the sixteenth- and seventeenth-century history of France, and intending to express only the “great facts”, “those of world-historical importance”,³⁹² remarks that he has found of the greatest value the original documentary matter “published in France during the last ten years, ...*none of which has ever before been used*”.³⁹³ The British were, at least, more historical than this. As Ranke saw it, “in no nation has so much documentary matter been collected for its later history as in England”, and “this epoch [the seventeenth century]... has been already often treated of, ...and that by nearly the best English historical writers”.³⁹⁴

Lucien Febvre remarked, in a note on the book’s manuscripts, “Marc Bloch long dreamed, as I have done, of putting down his ideas on history in an organized way. I often think with bitter regret, that while there was yet time we should have collaborated to give our younger generation a kind of new Langlois and Seignobos”.³⁹⁵ One can well imagine regret at the failure to collaborate with such a colleague, but it is less easy, with hindsight, to think of a new Langlois and Seignobos as some glorious missed opportunity. In 1898, with decades of influence to come, Charles-Victor Langlois and Charles Seignobos had published their *Introduction aux Études Historiques*,³⁹⁶ a work which presented historiography “in an organized way”, and one which presented itself as one of the first manuals of historiographical method and writing intended to be of use to historians. It acknowledged no predecessors, distinguishing itself at length from the “vast abstract constructions” of philosophy of history and the “most obvious and commonplace truisms” of other works on method.³⁹⁷ The Warring States Project of the University of Massachusetts at Amherst has put effort into analysing Langlois’ and Seignobos’ work: “In sum, of the 315

³⁹⁰ *Op. cit.*, p. 5.

³⁹¹ *Op. cit.*, p. 5, footnote.

³⁹² Leopold von Ranke, “Preface to History of France”, p. 150.

³⁹³ Our emphasis. *Op. cit.*, p. 149.

³⁹⁴ Leopold von Ranke, “Preface to History of England” (1875, trans. C.W. Boase, G.W. Kitchin, et al., revised by Konrad von Moltke and Georg G. Iggers), in Leopold von Ranke, *The Theory and Practice of History*, pp. 151-159 at p. 156.

³⁹⁵ Lucien Febvre, “A note on the manuscripts of the present book”, in Marc Bloch, *The Historian’s Craft*, pp. xiii-xviii at p. xiii.

³⁹⁶ Charles-Victor Langlois and Charles Seignobos, *Introduction to the Study of History* [1898], trans. G.G. Berry, London: Duckworth, 1912.

³⁹⁷ *Op. cit.*, pp. 1-11.

pages of the work proper, a total of 145 pages (46% of the whole) are devoted to the “analytic” operations of source criticism, and 124 pages (39%) to the “synthetic” operations of combining the source information into a historical account. In the authors’ view, without the first half of that methodology, to which they themselves devote more than half of their space, there is no second half”.³⁹⁸

“We propose”, say Langlois and Seignobos, “to examine the conditions and the methods, to indicate the character and the limits, of historical knowledge”.³⁹⁹ They summarise the central questions which their work addresses as follows: “what are historical facts? How are they to be grouped to make history?”⁴⁰⁰ The only reference to the historiography of historiography comes in chapter V, prefaced by the question “what are the forms in which historical works present themselves? These forms are, in fact, very numerous. Some of them are antiquated; not all are legitimate; the best have their drawbacks”.⁴⁰¹ Historians have differed about the purpose of historical work, “hence arise differences in the nature of the facts chosen, the manner of dividing the subject, that is, of co-ordinating the facts, the manner of presenting them, the manner of proving them”.⁴⁰² Historical works are understood by Langlois and Seignobos to be those “which are intended to communicate results obtained by the labour of historical construction”.⁴⁰³ “History is only the utilisation of documents”.⁴⁰⁴ We should then ask, “which of these represent truly rational types of exposition”.⁴⁰⁵

However, “the history of the modes of writing history has not yet been written well”.⁴⁰⁶ Langlois and Seignobos, writing chapter V in common,⁴⁰⁷ embark on an outline historiography of historiography, confining themselves to what is “strictly necessary for the understanding of the present situation” in historiography.⁴⁰⁸ Their “grouping” of facts for such a purpose is difficult to make consistent with their lengthy

³⁹⁸ <http://www.umass.edu/wsp/methodology/outline/langlois.html>; accessed 14th December 2006.

³⁹⁹ Langlois and Seignobos, *Introduction to the Study of History*, p. 2.

⁴⁰⁰ *Op. cit.*, p. 2.

⁴⁰¹ *Op. cit.*, p. 296.

⁴⁰² *Ibid.*

⁴⁰³ *Ibid.*

⁴⁰⁴ *Op. cit.*, p. 316.

⁴⁰⁵ *Op. cit.*, p. 296.

⁴⁰⁶ *Op. cit.*, p. 297.

⁴⁰⁷ *Op. cit.*, p. 13, footnote.

description of how historians should “group” facts,⁴⁰⁹ while the outline nature of it, since “the present situation” is itself a historical fact and causation (although poorly understood) is for them an appropriate mode of understanding, seems contrary to their injunction “Never seek the causes of an historical fact without having first expressed it concretely in terms of acting and thinking individuals”, which they don’t do here.⁴¹⁰

The approach also sits oddly with their claim that “it is an obsolete illusion to suppose that history supplies information of practical utility in the conduct of life”.⁴¹¹ In short, their outline historiography of historiography is *unhistorical* by their own lights.

That does not, of course, make their historiography of historiography incorrect, since it may be their conception of historiography which is limited. In detail, “in the time of Thucydides and Livy” (regrettably for the historical status of this claim, they were some 400 years apart⁴¹²), the aim of history was “to preserve the memory and propagate the knowledge of glorious deeds, or of events which were of importance to a man, a family, or a people. ...In addition, history was early considered as a collection of precedents, and the knowledge of history as a practical preparation for life, especially political life (military and civil). Polybius and Plutarch wrote to instruct, they claimed to give recipes for action”.⁴¹³ Aiming to please and instruct made historiography a branch of literature.⁴¹⁴ This approach was imitated by Renaissance writers: “for them, too, history was a literary art with apologetic aims or didactic pretensions”.⁴¹⁵ Renaissance historical literature, Langlois and Seignobos observe, added two new features: the writing of *universal* history, and the introduction of glosses or notes which “made it possible to distinguish between the historical narrative and the documents which support it”.⁴¹⁶

⁴⁰⁸ *Op. cit.*, p. 297.

⁴⁰⁹ *Op. cit.*, p. 232-251.

⁴¹⁰ *Op. cit.*, p. 289.

⁴¹¹ *Op. cit.*, p. 319.

⁴¹² Thucydides lived about 460-400 BCE and Livy 59 BCE-17 CE.

⁴¹³ Langlois and Seignobos, *Introduction to the Study of History*, p. 297.

⁴¹⁴ *Op. cit.*, p. 298.

⁴¹⁵ *Ibid.*, p. 298.

⁴¹⁶ *Op. cit.*, p. 299. This distinction is best seen as one of presentation. Epistemologically, a gloss or note is plausibly best conceived as a sentence expressing some historical source interpretation taken as evidence for that which is asserted in the narrative; in effect, both the gloss or note and the element of the narrative which it supports are alike factual, and – on the Quinean approach summarised in Chapter 1 and explained later in Chapter 4 – alike form elements of the web of belief. Historical

In the eighteenth century Montesquieu and Voltaire personified the new tendency for historians to take an interest beyond the political into “the evolution of the arts, the sciences, of industry, and in manners”,⁴¹⁷ and by the end of the eighteenth century “German professors” had created “a methodical collection of carefully justified facts, with no literary or other pretensions”,⁴¹⁸ which organised historiography as many different branches of study. Unnoted by them, Italian professors had made a contribution, too: Denys Hay refers to Ludovico Muratori (1672-1750): “in many ways he was, if not the father of modern historiography, then at least the grandfather”.⁴¹⁹ By contrast, Giambattista Vico (1668-1744), deep thinker though he was, would certainly be excluded by Langlois and Seignobos, for he became known primarily through the work of Jules Michelet, of whom they say “The great men who taught history in this illustrious institution (J. Michelet, for example), were not technical experts, nor even men of learning in the proper sense of the word”.⁴²⁰

Hence “scientific, that is, simple and objective, exposition began to compete with the rhetorical or sententious, patriotic or philosophical ideals of antiquity”.⁴²¹ There was an unfortunate flowering of historical literature during the Romantic movement at the beginning of the nineteenth century, when historians sought to “rouse the emotions of the public, by filling the mind with poetical images of vanished realities”, with historiography “dominated by an engrossing desire to produce an effect”, such that “some romantic historians have slid down this inclined plane to the level of the ‘historical novel’”.⁴²² However, since about 1850 all is well: “it is within the last fifty years that the scientific forms of historical exposition have been evolved and settled, in accordance with the general principle that the aim of history is not to please, nor to give practical maxims of conduct, nor to arouse the emotions, but knowledge pure and

referencing typically does not, however, involve glosses or notes; rather, source material is merely referred to rather than, say, quoted.

⁴¹⁷ Langlois and Seignobos, *Introduction to the Study of History*, p. 299.

⁴¹⁸ *Op. cit.*, p. 300.

⁴¹⁹ Denys Hay, *Annalists & Historians: Western Historiography from the VIIIth to the XVIIIth Century*, p. 166. I owe this reference to Michael Bentley, *Modern Historiography*, p. 4.

⁴²⁰ The institution referred to was the Collège de France. Langlois and Seignobos, *Introduction to the Study of History*, p. 336.

⁴²¹ *Op. cit.*, p. 300.

⁴²² *Op. cit.*, p. 301.

simple”.⁴²³ Michael Bentley shares this view of historiography: “It was in this period that history discovered its identity as a discipline: a distinctive way of organizing and representing knowledge”.⁴²⁴

Bloch was taught by both Langlois and Seignobos, and, having learnt from them the necessity of academic sincerity, and adopting an academic version of filial piety if only in that matter, he takes the opportunity to criticise them freely,⁴²⁵ and does so in particular by beginning his book with the question “what is the use of history?”, which was one of a number of “idle questions” earlier listed by Langlois and Seignobos.⁴²⁶ “What is the use of history, when the values of the past are being ruthlessly discarded?”, Joseph R. Strayer paraphrased Bloch in his Introduction to the book, “What is the use of history, when we repeat our old errors over and over again? ... [These questions] must have pressed on him with almost unbearable weight in the dark days of 1941, when he began this book”.⁴²⁷ Yet, so far as the main content of *The Historian’s Craft* is concerned, such weighty issues might never have arisen, let alone the idea of setting such historiographical issues in any kind of historical context. Perhaps he thought such questions are “idle” after all. There is a rather startling mismatch between Strayer’s apparent expectation and Bloch’s delivery in the main part of the book, with only the briefest of nods to the “dark days”: “we still speak (although, alas, with less assurance than our elders) of civilization in itself...”.⁴²⁸ Certainly Bloch’s Dedication of the book (to Lucien Febvre) has an appropriate dramatic quality: “Today our common task is threatened. ... We are vanquished, for a moment, by an unjust destiny”.⁴²⁹ But Bloch’s argument has little more than a bathetic conclusion.

⁴²³ *Op. cit.*, p. 303.

⁴²⁴ Michael Bentley, *Modern Historiography*, p. 1.

⁴²⁵ Marc Bloch, *The Historian’s Craft*, p. 4, footnote.

⁴²⁶ *Op. cit.*, p. 3, and footnote. See also Langlois and Seignobos, *Introduction to the Study of History*, p. 7: “...idle questions, such as: whether history is a science or an art; what are the duties of history; what is the use of history; and so on”.

⁴²⁷ Joseph R. Strayer, “Introduction”, Marc Bloch, *The Historian’s Craft*, pp. vii-xii at p. viii.

⁴²⁸ Marc Bloch, *The Historian’s Craft*, p. 187.

⁴²⁹ *Op. cit.*, p. v.

Bloch recognises ourselves and our forebears as “history-writing peoples”,⁴³⁰ and accepts the centrality of historical understanding to our civilisation and religion. Our identity – not his word – depends on our history: “In a Norman garden, stripped of our troops, we of the general staff consumed our idle hours in ruminating over the causes of the disaster. ‘Are we to believe that history has betrayed us?’ one of us cried”.⁴³¹ What is the use of history? A big question, but all the unnourishing inappropriateness of linguistic analysis is offered by Bloch: “what is here meant by ‘use’?”⁴³² Historiography can be the spur to action, with an entertainment value involving nearly universal fascination, but these are not enough to justify the trouble taken to write historiography with integrity and truth.⁴³³ Bloch then makes a move of doubtful efficacy: he changes the question to “what is it, exactly, that constitutes the legitimacy of an intellectual endeavour?”,⁴³⁴ his idea being that if historiography can “prove its legitimacy as a form of knowledge”⁴³⁵ then it can share the justification given to other intellectual endeavours, “in a world which stands upon the threshold of the chemistry of the atom, which is only beginning to fathom the mystery of interstellar space...”⁴³⁶ It is characteristic of Bloch’s time to accept that the sciences are paradigmatically justified as “intellectual endeavours”, and – given this historical context – he should not be blamed for failing to notice that, if “intellectual endeavours” in general fail to achieve the goals they set themselves, then historiography, in so far as it is such, will probably fail also.⁴³⁷

On such assumptions is Bloch’s answer constructed. “A science will always seem to us somehow incomplete if it cannot... aid us to live better”.⁴³⁸ We demand of history “the means to direct our actions”,⁴³⁹ and it must be trustworthy, so “our primary objective is to explain how and why a historian practices his trade. It will then be the

⁴³⁰ *Op. cit.*, p. 4.

⁴³¹ *Op. cit.*, p. 6.

⁴³² *Ibid.*

⁴³³ *Op. cit.*, pp. 7-9.

⁴³⁴ *Op. cit.*, p. 9.

⁴³⁵ *Ibid.*

⁴³⁶ *Ibid.*

⁴³⁷ See Keith Jenkins, “Ethical responsibility and the historian: on the possible end of a history ‘of a certain kind’”, *History and Theory, Theme Issue 43*, “Historians and Ethics”, pp. 43-60.

⁴³⁸ Marc Bloch, *The Historian’s Craft*, p. 10.

⁴³⁹ *Op. cit.*, p. 11.

business of the reader to decide whether this trade is worth practicing”.⁴⁴⁰ And what is this “trade”? History is a “science in movement”, a “science in its infancy”, “very young as a rational attempt at analysis”.⁴⁴¹ “We are much better prepared to admit that a scholarly discipline may pretend to the dignity of a science without insisting upon Euclidean demonstrations or immutable laws of repetition”.⁴⁴² Bloch then elaborates historical science in terms of evidence, analysis and causation. Sir Lewis Namier said that the book was “deep, penetrating, and highly practical, sane and serene”.⁴⁴³ What it is not is historical. Although Bloch includes a “history of the critical method”,⁴⁴⁴ past historians are treated in the most unhistorical fashion.

Hence it is striking how little of all this work is historiography of historiography. Plainly the historians concerned thought and think that a work of normal historiography would not be the appropriate way to deal with their concerns. The history of historiography is not a central part of their *self*-understanding. Broadly, their concerns were philosophical and methodological, and they sought understanding and justification of their approaches, but through quasi-philosophical and methodological analysis rather than through historiography. The historiography of historiography was not seen as the appropriate way to understand or to justify their own discipline. In approaching things in this way, they clearly did not share Kuhn’s strong sense of the centrality of historiography in such explanatory and justificatory matters. In this, they all – even Carr – shared rather the “scientific” view of Kuhn’s philosophical opponents, that one can stand outside a discipline, adopt some non-historical viewpoint, on the basis of which one can give comparatively “objective” answers. (Typically, Carr did not intend his relativism to apply to his own position.)

It is almost as if such historians adopted the view that historiography itself was not in a position to give those answers, as if locating historians in some historical context would introduce the very relativism many of them wished to keep at bay. That, however, would have been a philosophical error. If historiography has changed over

⁴⁴⁰ *Op. cit.*, p. 12.

⁴⁴¹ *Op. cit.*, p. 13.

⁴⁴² *Op. cit.*, p. 17.

⁴⁴³ *Op. cit.*, dustjacket.

⁴⁴⁴ *Op. cit.*, chap. III.

time – and it is a historiographical question whether it has done so – that does not of itself introduce relativism. On the contrary, historians of the (at Elton’s time) more typical Elton-like anti-relativist bent should – if their position is right – in principle be able to argue that any change in the discipline of historiography was change for the better, and they should be able to draw on the reasons, which past historians had themselves accepted as reasons for change, as reasons which justified the later version of the discipline. But they didn’t. It is a historiographical question why they didn’t. Was it a deliberate choice not to do so? Certainly for Ludmilla Jordanova, who firmly claims that she is not offering a “history of history”, although she doesn’t say why.⁴⁴⁵ But “Western man has always been historically minded”, remarked Joseph R. Strayer in his “Introduction” to Bloch,⁴⁴⁶ and did not notice how unhistorically minded Bloch’s book was.

⁴⁴⁵ Ludmilla Jordanova, *History in Practice*, p. xv.

⁴⁴⁶ Joseph R. Strayer, “Introduction”, Marc Bloch, *The Historian’s Craft*, p. vii.

Chapter 4 Pragmatic Postmodernism

Postmodernism

Says Richard J. Evans, “nothing has outdated the views not only of Elton, but even of Carr, more obviously than the arrival in the 1980s of postmodernist theory, which has called into question most of the arguments put forward by both of them”, rightly adding “postmodernism is a convenient label; it is not an organized movement, nor does it amount to a coherent ideology”.⁴⁴⁷ Simon Schama’s *Citizens*, says Evans, exemplifies “the best aspects of postmodernism’s influence on mainstream history”,⁴⁴⁸ but for Evans postmodernism is there to be resisted, not praised. For Schama himself, it is Hayden White’s approach which is seen as posing the danger: “Narratives have been described, by Hayden White among others, as a kind of fictional device used by the historian to impose a reassuring order on randomly arriving bits of information about the dead”.⁴⁴⁹ However, Schama distances himself from this “alarming insight” of the postmodern approach,⁴⁵⁰ ascribing his own point of departure in the writing of *Citizens* to reading David Carr’s “Narrative and the real world: an argument for continuity”: “As artificial as written narratives might be, they often correspond to ways in which historical actors construct events”.⁴⁵¹

It is, for Schama, a kind of *fact* that the French Revolution was “a thing of contingencies and unforeseen consequences”, “a much more haphazard and chaotic event and much more the product of human agency than structural conditioning”.⁴⁵² To handle this chaos, Schama seeks to justify his narrative approach (in “the form of nineteenth-century chronicles”) and carefully outlines his arguments.⁴⁵³ But Schama’s narrative structuring in *Citizens* is not the “fictional device” derived from Hayden White, for it is intended to correspond to the historical actors’ own perceptions:

⁴⁴⁷ Richard J. Evans, *In Defence of History*, p. 3 and endnote. See also his excellent reading list, pp. 355ff. in the 2001 edition of his book.

⁴⁴⁸ Richard J. Evans, *In Defence of History*, p. 364.

⁴⁴⁹ Simon Schama, *Citizens*, p. xvi.

⁴⁵⁰ *Op. cit.*, p. xvi.

⁴⁵¹ David Carr, “Narrative and the real world: an argument for continuity”, *History and Theory* 25, 1986, pp. 117-131.

⁴⁵² Simon Schama, *Citizens*, p. xiv-xv.

“many, if not most, public men see their conduct as in part situated between role models from an heroic past and expectations of the judgment of posterity. If ever this was true, it was surely so for the revolutionary generation in France”.⁴⁵⁴ True to this structure, itself claimed as broadly true, Schama begins *Citizens* with the tale of a plaster elephant on the site of the Bastille, intended by Napoleon as a stimulus for “imperial self-congratulation”,⁴⁵⁵ a reminder of Napoleon’s glory, and the obliteration of the memory of the Revolution, and the book ends with Lafayette’s self-righteous republican patriotism. Schama’s “exercise in animated description” has “a beginning, middle and end that tries to resonate with its protagonists’ own overdeveloped sense of past, present and posterity”.⁴⁵⁶

To be properly “postmodern” in Evans’ (and Schama’s) historiographically worrying sense, Schama would have to have recognised his own work as haphazard and chaotic, with any apparent order to it wholly unjustifiable. It is against the view that historiography is essentially like this, with all the opportunities that this view allows for expressing a wide range of evaluative prejudices, that Evans wishes to argue. He offers his defence against “the stress laid upon the shifting nature of concepts by postmodernists, and the emphasis given to the indirect, contingent or even arbitrary or non-existent correspondence of words to reality”.⁴⁵⁷ If historians’ language does not correspond to historical reality, then it seems historical “facts” are not available. If this is so then, for example, one might deny the occurrence of the French Revolution, or, more to the point for Evans, the Holocaust,⁴⁵⁸ for denial would be as justifiable as affirmation.

⁴⁵³ *Op. cit.*, p. xv and *passim*.

⁴⁵⁴ *Op. cit.*, p. xvi.

⁴⁵⁵ *Op. cit.*, p. 3.

⁴⁵⁶ *Op. cit.*, p. 6.

⁴⁵⁷ Richard J. Evans, *In Defence of History*, p. 13.

⁴⁵⁸ Evans, a distinguished historian of Germany, appeared in an English libel case as an expert witness on behalf of Deborah Lipstadt and against David Irving, whom Lipstadt had characterised as a Holocaust denier. Irving lost. Laws exist in a number of countries against holocaust denial, and in 2005-2006 Irving served a prison sentence in Austria for this, a sentence partially reduced on appeal. For material partially relevant to our historiographical concerns, see Richard J. Evans, “History, Memory and the Law: the Historian as Expert Witness,” *History and Theory* 41, 2002, pp. 326-345.

It is the simple, if simplistic, alleged failure of language to correspond to reality which generates the characteristic historiographical concern at this point.⁴⁵⁹ Keith Jenkins is one paradigm source of this position, for Evans: “Keith Jenkins has argued that historical method does not lead to historical truth”.⁴⁶⁰ Jenkins draws on Michel Foucault for illustrations of the “arbitrariness of the definition”, on George Steiner for the claim that “nothing whatever...in its phonemic components, etymological history or grammatical functions, has any correspondence whatever to what we believe or imagine to be the object of its purely conventional reference”, and on Richard Rorty for the view that “truth was always created and never found”.⁴⁶¹ Evans confidently asserts that historians can successfully respond to such claims: they “have as much right” as anybody else to deal with such arguments themselves, and “historians’ replies to at least some postmodernist critiques of history have caused some postmodernist theorists to shift their ground in crucial respects, just as historians themselves have been forced to shift their ground by these critiques”.⁴⁶² No doubt they have. “Even Jenkins uses footnotes”, says Evans; “interpretations really can be tested and confirmed or falsified by an appeal to the evidence; and some of the time at least, it really is possible to prove that one side is right and the other is wrong”.⁴⁶³ It is *evidence* that warrants factual judgements, and *agreement* among historians that does the trick, for Evans: “what counts as evidence is not determined solely by one historian’s perspective, but is subject to a wide measure of agreement which transcends the individual”.⁴⁶⁴

But postmodernism is more powerful than to be undermined by this kind of argument. It is common, if loose talk philosophically, to think of the conflict between those who think that historiography achieves truth and their postmodern opponents as a conflict between beliefs in “objectivity” and “subjectivity”. Both these words are complex in their associations and implications, but a particularly relevant confusion applies to “subjective”, and this most clearly appears in that word’s association with relativism.⁴⁶⁵

⁴⁵⁹ Historiographical concerns here are nevertheless complex. See Peter Novick, *That Noble Dream*.

⁴⁶⁰ Richard J. Evans, *In Defence of History*, p. 127.

⁴⁶¹ Keith Jenkins, *Re-Thinking History*, London: Routledge, 1991, pp. 30-31.

⁴⁶² Richard J. Evans, *In Defence of History*, p. 14.

⁴⁶³ *Op. cit.*, pp. 127-128.

⁴⁶⁴ *Op. cit.*, p. 128.

⁴⁶⁵ Relativism is not here dealt with. See Paul O’Grady, *Relativism*, Chesham: Acumen, 2002.

“Objectivity”, by contrast, is often thought to be “absolutist”. Simon Blackburn offers a widespread view of relativism in the following: “Suppose I believe that fox-hunting is cruel and should be banned. And then I come across someone (Genghis...) who holds that it is not cruel, and should be allowed. We dispute, and perhaps neither of us can convince the other. Suppose now a relativist (Rosie) comes in... ‘You absolutists,’ she says, ‘always banging on as if there is just one truth. What you don’t realize is that there is a plurality of truths. It’s true for you that fox-hunting should be banned – but don’t forget that it’s true for Genghis that it should not.’”⁴⁶⁶

Postmodernism, however, is *not* trying to replace “agreed” truth with “true for me” and “true for you.” We may all even *agree* in what we believe, but what we believe is nevertheless expressed in language which, so the postmodern claim in a simple version goes, does not correspond to reality. As a word, “subjectivity” hints strongly at *personal choice* as the ground for belief, as if Evans’ “agreement” could overcome that. But that is not where the problem lies: *community choice* would be just as problematic, according to postmodernism. Objectivity is not warranted by mere agreement.

Given postmodernism as Evans presents it, historical facts are not available; not available, at least, to historiography. For we may still imagine, or believe, that God knows such facts, and it would be for theologians to say whether divine knowledge was *a priori* or *a posteriori*, and to say in what, if any, eternally reliable language God knows them. In consequence, notice that most of the ways in which postmodernism is here expressed by Evans suggest realism: that *there is* an independent reality to which our chaotic human language fails to correspond; hence the room for God, ineffable though God would no doubt be on this approach. Moreover, Evans unhelpfully – in a range of words characterising postmodernism – says also that the correspondence of concepts to reality is *contingent*. He no doubt intends this to mean that the relationship is *changeable*, and so *unreliable*, but this terminology implies that the alleged failure of language to correspond to reality is a contingent failure, so that, contingently, language could also *succeed* in corresponding to reality. Perhaps Evans

⁴⁶⁶ This appears in Simon Blackburn, “Relatively Speaking,” *ButterfliesandWheels.com*, 2003, and is copied from the Royal Institute of Philosophy’s journal *Think*. See also Simon Blackburn, *Being Good*, Oxford: Oxford University Press, 2001.

does intend this, maybe in order to allow for contingent historiographical success in achieving historical truth, but postmodernism is more often seen as suggesting an *inevitable* failure of language to correspond to reality. If this is so, then the word “reality” refers to an inevitably unknowable something which, again inevitably, can have no place in understanding the “truth” of what we say. “Reality”, so understood, disappears as a useable concept.

Postmodernism is better seen as *antirealist* rather than realist. This position would typically involve claiming that language fails to correspond to reality just because there is no independent reality for it to correspond to, and one of Evans’ many words characterising postmodernism does indeed allow for this position. If there is no independent reality then language cannot properly be conceived as *failing* to correspond to that reality at all. “Reality” – and cognate terms like “truth”, “knowledge” and so forth – would have to be understood in ways which make conceptual sense, not in ways that do not. “Reality” must then disappear as a concept suitable for referring to that which is independent of our language, and be understood in some other way. “Reality”, without independence, is commonly conceived as no more than a term we use for that which *we take* our language to correspond to: in effect, we *construct* reality using our language. A proper postmodern understanding would have us *recognise* that such antirealism is the situation which we are in, and then have us recognise that *we should not* be taking our language to be corresponding to *anything else* which lies outside our human construction. As Richard Rorty put it, “We hope to do to Nature, Reason and Truth what the eighteenth century did to God”.⁴⁶⁷ But the point goes beyond those big concepts, all of which can in any event be brought back in in an antirealist way. In so far as there is no *real independent content* to what we say or purport to refer to, some may even wish to join Marshall McLuhan in saying that “the medium is the message”.⁴⁶⁸ There is only the language itself.

⁴⁶⁷ Richard Rorty, “The continuity between the Enlightenment and ‘Postmodernism’”, in Keith Michael Baker and Peter Hans Reill (eds.), *What’s Left of Enlightenment?*, pp. 19-36 at p. 19.

⁴⁶⁸ Although the expression of this still involves a distinction between medium and content. See Marshall McLuhan, *Understanding Media: The Extensions of Man*, New York: McGraw-Hill, 1964.

However, postmodernism, while a characteristic current concern of historians, is not for them an explicit worry about realism against antirealism and the implications of that and related issues in the philosophy of language. Whether there is an “independent” reality is a central philosophical question, linking the philosophy of meaning to metaphysics and the philosophy of mind. When we add to that philosophical mix any requirement for that independent reality to be, as it is for historians, the “dead and gone” past, then we are taken into the philosophy of time and so onward to the foundations of our most advanced scientific understanding. Evans, while seeking to respond to postmodernism, does not address issues like these. Historians are precisely those people who are very aware that the past is no longer with us, yet who typically also think that sufficient “reality” is there for true things to be said about that past. We have earlier noted the characteristic realism of historiography.⁴⁶⁹ How such realism is possible is, from a historiographical point of view, for some other discipline to worry about.

If we now see language as floating free of ties to an independent reality, we might well think that it is no wonder that it is haphazard and chaotic. However, that would be a mistake, for it presupposes that only ties to an independent reality would ensure linguistic order. We do not need to adopt philosophical realism in order to avoid postmodern historiographical chaos, and hence need not engage in the philosophical issues of realism versus antirealism. It should not be taken for granted that our language is haphazard and chaotic, merely because and in so far as we lose the notion of a metaphysically independent reality. It is a quite separate question how far our language is chaotic or arbitrary, if it is; and antirealism – which has various forms – need not imply that it is. Our argument will address the fundamental point for us here: the characteristic historiographical worry with respect to postmodernism has to do with this indeterminacy of language, and that is problematic whatever metaphysics philosophers think generates that problem. Our approach will address the claimed indeterminacy directly, without the need for engagement with the metaphysical issues or with related issues in the philosophy of reference.⁴⁷⁰ We will, in effect, assume for

⁴⁶⁹ In the section “Our primary sources”.

⁴⁷⁰ Martin Bunzl usefully analyses some of these philosophical issues against a background of historical examples in *Real History: Reflections on Historical Practice*, London: Routledge, 1997.

the sake of the argument a position which is broadly antirealist, and the argument will show that, even if that approach is adopted, we can still achieve what the characteristically realist historian wants, and gain much understanding on our way.

Barbara Hernstein Smith usefully describes the postmodern attitude as “a general conceptual style or taste, specifically played out here as (a) a conceptualization of the world as continuously changing, irreducibly various, and multiply configurable, (b) a corresponding tendency to find cognitively distasteful, unsatisfying, or counterintuitive any conception of the world as fixed and integral and/or as having objectively determinable properties, and (c) a corresponding disinclination or inability to use terms such as ‘reality’, ‘truth’, ‘meaning’, ‘reason’, or ‘value’, as glossed by...objectivist conceptions”.⁴⁷¹ However good or bad the metaphysical reasons may be, the postmodern position suggests *freedom of choice* in the context of what to believe about reality, and hence a contingency about what people understand reality to be.⁴⁷² The nature and limits of that claimed freedom need to be understood here.

Postmodernism is widely taken to suggest that we can believe what we like, that there is *unlimited choice in factual description*, and this is the characteristic historiographical worry. We earlier argued⁴⁷³ that historiography is shot through with choices, and it is essential to our position that these include factual choices. However, our question is whether determinate criteria for the purpose are available. Yet while many historiographical choices have so far been demonstrated, this talk of “choice” where facts are concerned will no doubt appear to the anti-postmodernist historian as irrelevant philosophical speculation, akin to that of the stage philosopher who denies the existence of tables, and as little to be taken seriously. For the anti-postmodernist

⁴⁷¹ Barbara Hernstein Smith, *Contingencies of Value*, Harvard U.P., 1988, at p. 151, as quoted by Richard Eldridge, “Narratives and moral evaluation”, *Journal of Value Inquiry* vol. 27, 1993, pp. 385-390.

⁴⁷² Stefan Collini put it briefly: “for a professional historian...writing at the end of the twentieth century, a distancing self-consciousness must supervene, and with it comes a sense of the contingency of so much that a well-connected Victorian gentleman took to be natural” (“Like Family, Like Nation” – review of David Cannadine, *A Life in History*, *The Times Literary Supplement* 4672, 16 October 1992, p. 4). Felipe Fernández-Armesto put it easily and at a little length in *Millennium*, London: Bantam Press, 1995, Prologue.

⁴⁷³ In the section “Rival historiographies of science”.

historian, the sound “common sense”⁴⁷⁴ which is characteristic of historiography should only need to be explicitly defended in order to keep up the historiographical morale of those unfortunates who suffer from postmodern doubt. Those whose feet are firmly on the ground should not need this.

⁴⁷⁴ G. Kitson Clark, *Guide for Research Students*, p. 9.

Commonsense and experience: Hume

Often, it is the view of reality as essentially known through experience which is taken to mark the sound commonsense of the historian. We shall present our explanation of the issues of factual choice with respect to this vague belief by following an empiricist line of argument.⁴⁷⁵ Postmodernism is better expressed on the basis of (very broadly conceived) empiricist rather than rationalist assumptions, bringing together as it does an “Anglo-American” pragmatic tradition which began with Humean empiricism and a “Continental” recognition of the limitations of Kantian rationalism as shown in cultural variation, deriving for example from the attention to experience involved in certain approaches to phenomenology and the choices implied in existentialism. David Hume put well the original empiricist attitude: we may assume a certain experienced “given”: the concrete material world which floods back in when we leave our metaphysical speculations behind.⁴⁷⁶ What this experienced “given” is – what does “flood back in” in the absence of philosophical speculation – is contingent, and it seems just a matter of contingent fact that experience seems straightforwardly to disclose that the world is a regular and consistent place. For Hume the world is full of experienced particular facts, agglomerated together into a vast complexity, a reliable complexity which displays both regularity and consistency.

Hume distinguished, on what he thought were empirical grounds, between what he called “simple” and “complex” experiences.⁴⁷⁷ The experienced world is divided by Hume into “simple” entities which combine with others into “complexes”. These complexes do not change the nature of the simple entities which compose them, and these persist in a determinate way throughout combination, separation and re-combination. This experienced reality induces in us ideas. While the imagination can play around with the ideas which ultimately derive from experienced reality and combine them to yield all manner of errors or inventions (for ideas too are persisting determinate entities which can be combined, separated and re-combined), our ideas are only to be understood as knowledge in so far as they are a faithful representation of

⁴⁷⁵ The following paragraphs are taken from my paper “Adjusting Reality – outline of a cost-effective metaphysics” delivered at the World Congress of Philosophy, Moscow, 1993 (Section 1, Metaphysics).

⁴⁷⁶ David Hume, *A Treatise of Human Nature*, p. 269.

experienced reality. And just as, for Hume, it is matter of contingent fact that we experience reality to be regular and consistent, so our claims to knowledge must express that regularity and consistency.

However, can we take Humean empiricism for granted as a sound interpretation of our commonsense reliance on experience? What lies behind Hume's particular version of empiricism is his belief that the building blocks of experienced reality and, equivalently, the building blocks of our knowledge about that reality are determinate simple entities. Simple experiences of reality guarantee the truth of our simple beliefs about reality because ultimately the beliefs are copied or derived directly from those experiences. Unfortunately for Humean empiricism, experience itself, in addition to the best philosophical efforts, has failed to show that experienced reality provides any guarantee of the truth of what we believe about it. This is because, against Hume, we cannot separate, within our total set of claims to knowledge, certain particular beliefs which meet experience, "copy" experience and are checkable by experience independently of the rest of what we believe. Present experience presents itself all at once, and not bit by bit. To pick out some experiences, classify them as "simple", and distinguish them from other simple experiences is not a matter of the passive reception of what we sense but rather a matter of the active focussing of attention. As the empiricist F.H. Bradley put it, "It is a very common and most ruinous superstition to suppose that analysis is no alteration, and that, whenever we distinguish, we have at once to do with divisible existence. It is an immense assumption to conclude, when a fact comes to us as a whole, that some parts of it may exist without any sort of regard for the rest".⁴⁷⁸

One way of illustrating the point is to consider kind-words, such as "blue". To describe something as "blue" is, among other things, to liken it to other things not now present to us, and thus to make a claim about reality which goes beyond "immediate" experience. As far as characterising experience in truthful language is concerned, then, because of the inevitable presence of kind-words, no sentence has a simple correspondence to fact, where "fact" is understood, as Hume would have wished, as an

⁴⁷⁷ *Op. cit.*, p. 2.

atomistic simple experience. It is a powerful and important empirical result, made particularly clear for empiricism in the twentieth century, that our beliefs about the world, and the conceptual scheme or schemes in terms of which our beliefs are expressed, are underdetermined by our experience.⁴⁷⁹ Experience does not directly deliver what we believe or think. The conscious focussing of conceptual attention is involved. In consequence, the particular experiences which we suppose ourselves to have are not authoritative of our beliefs. What we say about experienced reality is more than our experience alone warrants, even in the simplest cases.⁴⁸⁰

A follower of Kant might insist that our beliefs and conceptual schemes, demonstrably more than experience warrants as they are, should be understood in terms of the absolute and unchangeable rational categories involved in the human intellect. Such categories are supposed to be necessarily what they are. A different response might be that the demonstrable imposed human organisation of experience is something we are “hard-wired” to do, in computerspeak. The “postmodern” implication here, contrasting with Kant’s position, would be that the “hard-wiring” is no more than arbitrary and that we might have been “hard-wired” differently. Either way, it might be suggested that it is in these unavoidable – and perhaps arbitrary – human inputs to experienced reality that we should seek the grounds for our beliefs about the regularity and consistency of reality. Such *a priori* knowledge is denied by empiricists, however, and thus a broadly empiricist approach now recognises that it is not merely that the experienced world is a matter of contingency, but also that what *we count* as the experienced world varies with the contingency of circumstance. There is a double

⁴⁷⁸ F.H. Bradley, *The Principles of Logic*, vol. I, p. 95.

⁴⁷⁹ This derives from the problem of induction considered at length in the nineteenth century, and in particular from the failure of the verifiability theory of scientific laws to guarantee Hume’s regularities and the failure of the theory of sense-data. Karl Popper’s *The Logic of Scientific Discovery* (revised edn., London: Hutchinson, 1968) is the most appropriate source for the former. With regard to the latter, Isaiah Berlin in some of the papers collected in his *Concepts and Categories* made early contributions to the advance of relevant discussion. While the empiricist discussion referred to here is broadly twentieth century, *anti*-empiricist discussion of these issues dates back to Kant, if not Plato.

⁴⁸⁰ This result does not in itself presuppose empiricism. It is important not to confuse the concepts of “empiricism” and “empirical”. “Empiricism” is a thesis in the theory of knowledge, holding broadly that knowledge can be validated or justified *only* by experience. By contrast, the “empirical” marks merely that which experience in fact gives, whatever that is. The empirical underdetermination of the set of beliefs means that, if we add, to this empirical fact of underdetermination, the Humean view that particular experiences alone can authorise our beliefs, then our beliefs are not guaranteed. Humean scepticism results. Empirical underdetermination need not lead to scepticism if we are not Humean empiricists.

contingency. As Isaiah Berlin noted,⁴⁸¹ “the facts...are not at all identical for all men at all times”, for they are classified and arranged against the background of their general conception of the world. Counting – organising reality through the classifications of our language – is not, for the empiricist, in terms of fixed Kantian or other hard-wired categories but rather something human beings just do, and even *consciously* do by choice.⁴⁸² There are, as historiography and anthropology demonstrate about humanity in the world, many different ways of doing it.

The human organisation of what counts as reality affects the most immediate of experiences as well as the most abstract considerations. It is not just that *beliefs* fail to be “identical for all men at all times”, but that the *facts* themselves vary. This is because, as we have seen in presenting the postmodern approach, the facts are essentially what *we take* them to be. What we take to be facts depend in part upon our other beliefs. The truth of the claims we make about reality depends partly on our experience, and partly on the background or *a priori* assumptions which we bring to experience.⁴⁸³ We have many experiences and make many assumptions, and the “truth” of these is mutually supporting. This is a so-called “holistic” or “coherence” approach to truth.

The following simple example, involving a belief long used in the academic philosophy of science, is one way of demonstrating the claim that what counts as reality can be consciously chosen, in the light of background beliefs. Imagine an ignorant eighteenth century ornithologist, firmly convinced *a priori* that all swans are white, who travels to Australia and discovers a black one.⁴⁸⁴ There is for him a clear conflict between his background knowledge claim and what he takes his experience to be. But must he discard as false his belief that all swans are white? Not necessarily. Two inconsistent beliefs are in the forefront of his mind at the relevant point: “all swans are white” and “this is a black swan”. These beliefs are inconsistent with each other. Logic says that

⁴⁸¹ Isaiah Berlin, “The Purpose of Philosophy”, *Concepts and Categories*, p. 8.

⁴⁸² One has to count before one can do arithmetic. This is the same sense of the word “count”.

⁴⁸³ “*A priori*” beliefs on this empiricist approach are merely beliefs assumed to be true, beliefs adopted or presupposed prior to the experiences at issue. No Kantian implications of necessary truth or certain knowledge should be read into the notion.

⁴⁸⁴ Willem de Vlamingh did this in 1697.

one of them has to go.⁴⁸⁵ But neither logic nor experience tells us which it should be. The ornithologist can keep the belief that he has before him a black swan, and discard the belief that all swans are white. Equally, he can keep the belief that all swans are white, and discard the belief that this is a black swan, by supposing it to be some other kind of bird. There is therefore freedom of choice with respect to what counts as reality here. There is no fixed “real” answer.

Whichever belief the ornithologist decides to discard, it is a matter for human decision rather than what “reality” requires, and it is a decision which has consequences for the rest of his and indeed our beliefs to a lesser or greater extent. It is those consequences which measure the merit of the decision made, and which broadly mark this approach as a “pragmatic” one, so that what is taken to be true is that which “works”. As we have seen, whether and to what extent “human decision” is *individual* decision or *social* decision is a very different – but nevertheless contingent – matter. It will not necessarily be the ornithologist’s *personal* decision in this case, as the matter will typically be one for discussion with other zoölogists on his return, and indeed for much wider groups. Some decisions are often made “unconsciously” and without deliberation, being the unplanned consequences of other activities or decisions or even simply by habit. Once the decision is made, people are trained accordingly, particularly in the learning of language, and come to experience reality in terms of the linguistic conceptual pigeonholes which such decisions give to them. The ornithologist in due course comes to see the bird *as* a swan, or otherwise as appropriate.

Different cultures can reach different decisions about such things.⁴⁸⁶ There is cultural variability of ordinary everyday life. We should not suppose, for example, that the

⁴⁸⁵ Or perhaps, “Logic takes you by the throat and....” See C.L. Dodgson’s (Lewis Carroll’s) “What the Tortoise said to Achilles”, *Mind* n.s. 4, 1895, pp. 278-280; see also Douglas R. Hofstadter, *Gödel, Escher, Bach: An Eternal Golden Braid*, Harmondsworth: Penguin, 1980, p. 43 and *passim*. Relevant considerations concerning the place of logic in decision-making will be discussed below.

⁴⁸⁶ John Gray observes, “the idea of a language such that to use it at any point is to invoke, to touch or be touched by, all the rest, is only an instance or application of an idea that is perhaps not much older than Hamann and Herder, the idea of a *culture* – the idea, that is to say, of a people as having a pervasive form of life, in terms of which their activities, however otherwise disparate or miscellaneous, are given coherence and are renewed over time.” John Gray, *Isaiah Berlin*, pp. 129-130.

“material world” is necessarily and everywhere, for all time, to be taken to be that which we can touch, or the world with which physics or medicine deal, or that harsh constraining reality of the working man’s industrial experience pointed out by Marx, or the western urban experience of choice which so many parts of the world now wish to make true for themselves. On the contrary, philosophy, whether founded on empiricism or rationalism, has failed to disclose or validate any absolute and universal answer to the nature of experienced reality. What *does*, in Hume’s terms, “flood back in when we leave our metaphysical speculations behind”⁴⁸⁷ is contingent, with the implication of variation. Stones can be worshipped as well as tripped over. The position, nevertheless, remains that reality is as we experience it to be.

⁴⁸⁷ David Hume, *A Treatise of Human Nature*, p. 269.

Quine as postmodernist

Are we really committed to so uncertain a world? Is what counts as reality as easily adjustable, as “multiply configurable”, as it may now seem? The empiricist needs to adopt a clear philosophical account of the experienced world which makes sense of the way in which what count as facts can depend upon background beliefs. A suitable empiricist account which explicates the holism and pragmatism involved here is expressed in the analytical epistemology of W.V.O. Quine. We use here particularly the early Quine of “Two dogmas of empiricism”, first published in 1951, which appears in his *From a Logical Point of View*.⁴⁸⁸ Says Albert Hofstadter, discussing this collection in 1954, it is “not a systematically organized treatment of the major problems of epistemology. ...I shall call this view *Holistic Pragmatism*, an expression the use of which is perhaps not altogether a courtesy to the author, but which, in lieu of a better, is useful in pointing up the nature of his outlook upon knowledge”.⁴⁸⁹

Said Quine, “the fundamental-seeming philosophical question, How much of our science is merely contributed by language and how much is a genuine reflection of reality? is perhaps a spurious question... to answer the question we must talk about the world as well as about language, and to talk about the world we must already impose upon the world some conceptual scheme... [yet we are not] stuck with the conceptual scheme that we grew up in. We can change it bit by bit, plank by plank, though meanwhile there is nothing to carry us along but the evolving conceptual scheme itself. The philosopher’s task was well compared by Neurath to that of a mariner who must rebuild his ship on the open sea”.⁴⁹⁰

⁴⁸⁸ W.V.O. Quine, “Two dogmas of empiricism”, in his *From a Logical Point of View* (2nd edn.; New York: Harper and Row, 1961, pp. 20-46), a collection first published in 1953 and containing papers published between 1937 and 1951. See also his *Word and Object*, which was the source of revision of other papers for the second edition of *From a Logical Point of View*. Alan Weir comments on Quine’s theory of the indeterminacy of meaning in *Word and Object* that it “has more than a whiff of smoke-filled cafés on the banks of the Seine about it”. Alan Weir, “Quine on indeterminacy”, forthcoming in Ernie Lepore and Barry Smith (eds.), *The Oxford Handbook of Philosophy of Language*, Oxford: Oxford University Press.

⁴⁸⁹ Albert Hofstadter, “The myth of the whole: a consideration of Quine’s view of knowledge”, *The Journal of Philosophy* vol. LI, no. 14, 8th July 1954, pp. 397-417 at p. 397.

⁴⁹⁰ W.V.O. Quine, “Identity, ostension, and hypostasis”, in his *From a Logical Point of View*, pp. 65-79 at pp. 78-79.

We cannot then make sense of the world independently of our linguistic conceptualisation of it. However, Hofstadter was early among those who pointed out a problem with Quine's position, namely that he had – and, it is fair to say, continued to have – a second and plausibly inconsistent philosophical belief. Hofstadter's own explanation of this is not of present relevance,⁴⁹¹ and it is perhaps most natural to understand this second belief in the following way: there is an independent reality causing the speaker, by way of the senses, to agree with the sentence which describes the reality in question. When the world stimulates a response from us (such as “yes, there is a rabbit there”), there is – in Neurath's metaphor – a “plank” of reality here which we can rely upon. The world is then “fixed” at this point, rather than changeable as our conceptual schemes change. Such are discovered, not invented. Stimulated beliefs of this kind reflect reality, rather than being creations of our language. Our metaphorical epistemological boat cannot be, and does not need to be, rebuilt at such points. A cognitive science can then tell us how reality brings about our knowledge of the world. The causal relationship between the world and what we believe somehow bypasses what is meant to be a purely linguistic conceptualisation.

Is there an inconsistency here? It is controversial, and we will say more below, but any Quine who inconsistently retains a vestigial belief that we can access reality independently of our conceptualisation of it is not the Quine we will be using. As we have said, at this point of our argument the empiricist needs to make clear sense of the way in which what count as facts can depend upon our background beliefs. Our concern is not to elucidate Quine, but to elucidate an argument. Here we will ignore such inconsistencies as Hofstadter pointed to, and think of Quine's position as if it were entirely the holistic pragmatism first summarised above.⁴⁹² So circumscribed, Quine's position, while empiricist (for it is for him still experience which provides the ultimate ground for what we can properly claim to know), understands experience as holistically rather than (as with Hume) atomistically related to our beliefs: that is, in

⁴⁹¹ He wrongly thought it depended on Quine's notion of “ontological commitment”: the “ontological commitments” of a theory are those things which the theory is committed to affirming as existing, if that theory is true.

⁴⁹² This is sometimes referred to as the “Duhem-Quine thesis”, which is helpfully summarised in Mary Hesse, “Duhem, Quine and a new empiricism”, in *Knowledge and Necessity*, Royal Institute of Philosophy Lectures 3, London: Macmillan, 1970, pp. 191-209.

metaphorical terms, all our beliefs meet all of experience as a body, and not on a one-to-one piecemeal basis.

Thus, in contrast with Hume's position, on Quine's approach we hold that our beliefs look for their warrant, not to particular experiences, but to experience "as a whole".⁴⁹³ Like the causal stimulation of belief, this is problematic, for the metaphor hints that reality is independent of our conceptualisation, rather than what we count it to be. However, we again take from Quine his explicit holistic meaning, which here is that particular beliefs are constrained by their relationship to other beliefs within our total set of beliefs. We can then describe the set of beliefs, as he does, as a "web" of beliefs, with the implications of constraint and stickiness which the word "web" suggests. The web of other beliefs are the sole constraints upon a particular belief, and are equally the only source of validation or justification for that belief. Not all "our" beliefs will be, in fact, held by any one person. In historiography, our beliefs include such as express *evidence* for other beliefs.

Experience alone warrants no particular beliefs as certain. We recognise that no one of our beliefs is so fully and directly related solely to experience that we are forced to keep or amend just that one if experience requires it. Equally, what we choose to treat as *a priori* is not a matter independently given to us by the demands of pure reason or anything of the kind, for reason does not itself generate substantive beliefs. Quine recognises that, in trying to make sense of the experienced world, there is room for conscious and deliberate decision regarding which sentences we propose to hold true and which we propose to discard as false. It is open to us to amend our knowledge claims as we find pragmatically convenient, and there are in principle many ways of effecting any required change. As Berlin put it, "Any one proposition or set

⁴⁹³ Following the "impossible term-by-term empiricism of Locke and Hume", Quine says "what I am now urging is that even in taking the statement as unit we have drawn our grid too finely. The unit of empirical significance is the whole of science" (W.V.O. Quine, "Two dogmas of empiricism", p. 42). The expression "unit of empirical significance" is very unclear. In the sections "Narrative truth" and "A fancy view of truth", below, we examine the relationship between (a) statements considered atomistically as epistemologically independent of each other, and (b) sets of statements conceived as forming whole historical accounts. The relationship between what Quine is here urging and our later discussion is not a close one, and philosophical concerns about it are best removed by reflecting that it is a pragmatic question how large a "unit of empirical significance" is to be taken to be, with narratives a central feature for the epistemology of historiography.

of propositions can be shaken in terms of those that remain fixed; and then these latter in their turn; but not all simultaneously”.⁴⁹⁴ As Quine put it, “Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system”.⁴⁹⁵ Again, “no statement is immune to revision”,⁴⁹⁶ so it is not merely that “there is much latitude of choice”,⁴⁹⁷ but that *unlimited* adjustment to the web of beliefs is available.⁴⁹⁸

It is essential to postmodernism, in this pragmatic holistic empiricist form, that we can hold true what we wish, except that any consequential cost of our decisions must be met by sufficient adjustment elsewhere in our system of beliefs.⁴⁹⁹ However, before we can deal with the details of any adjustments which may be required by any of our choices about our beliefs about reality, we should note that, partly on the basis of a careful conceptual analysis, Bernard Williams in his 1970 article “Deciding to Believe” thinks that *one cannot decide to believe at all*.⁵⁰⁰ Two of Williams’ arguments need at this point to be addressed. First, Williams interprets belief, as we ordinarily understand it, to be an internal mental state,⁵⁰¹ although it is more than that, according to him, since, among other ideas, a further “very central idea” in the case of “empirical” belief⁵⁰² is that there be some kind of regular connection between the

⁴⁹⁴ Isaiah Berlin, “The concept of scientific history”, in his *Concepts and Categories*, pp. 103-142 at p. 115. Berlin continued here, “It is this network of our most general assumptions, called commonsense knowledge, that historians to a greater degree than scientists are bound, at least initially, to take for granted”.

⁴⁹⁵ W.V.O. Quine, “Two dogmas of empiricism”, p. 43.

⁴⁹⁶ *Ibid.*

⁴⁹⁷ *Op. cit.*, p. 42.

⁴⁹⁸ Moral beliefs, in so far as they can be held true also, have the same status as factual beliefs on this approach, and equally permit unlimited adjustment. It follows that there is no *empirical* warrant for the fact-value distinction, and thus, given empiricism, no epistemological justification for that distinction. The argument is analogous to Quine’s argument against the analytic-synthetic distinction given in his “Two dogmas of empiricism”. Quine himself does not consider questions of value, however.

⁴⁹⁹ Quine’s expression of the position rather than Berlin’s is used because its clarity is analytical rather than metaphorical.

⁵⁰⁰ This is an attack on deciding *to believe*, not an attack on deciding as such. Bernard Williams, “Deciding to Believe”, in his collection of essays *Problems of the Self*, Cambridge: Cambridge University Press, 1973, pp. 136-151.

⁵⁰¹ *Op. cit.*, p. 140. We are not committed here to accepting that view.

⁵⁰² Williams’ introduction of the word “empirical” here suggests a contrast between at least two irreducible kinds of beliefs, empirical and non-empirical. The holistic empiricist would not be willing to accept such a contrast.

environment, a person's working perceptual organs, and the beliefs he has.⁵⁰³

Williams' presupposition here is that the idea of "empirical" belief is to be understood in such a way that a certain kind of causal theory of belief is correct, with the chain of causation working through the sense-organs.⁵⁰⁴ Given that empirical beliefs are *caused* in this way, then it cannot also be the case that we can *decide* whether to adopt our empirical beliefs (hence the possible inconsistency in Quine which we have just noted). A mental state that could be produced at will could not be a state of empirical belief, because it could not meet the conceptual requirement that a state of empirical belief be caused by the required causal chain. Whether this *is* a conceptual requirement is the philosophical point at issue.

It is the specific chain of causation claimed by Williams for "empirical" beliefs which is problematic here, that is, the claim that the causal chain proceeds from a particular fact to a belief that that fact is so. However, nothing in the holistic empiricist position denies the general possibility of causation being involved in the case of belief.⁵⁰⁵ Thus it is open to the holistic empiricist to take the determinist view that universal causation is true. On this view all psychological states, including belief states, are caused. While traditional opposition to determinism relies on the belief in "free will", one determinist response to this consists in holding that "free will" itself is caused. While we do not here have to claim merit for the position, a holistic empiricist determinist might then adopt the following so-called "causal theory of belief": first, there is a causal foundation for conscious decision or "free will"; second, conscious decision or "free will" then "chooses" belief. So belief is caused, and yet it is also consistently held true that we can decide to believe, for that decision too is caused. The imaginary holistic empiricist's point would be that the belief that some fact is so, while caused by what appears to us to be a conscious free decision (for it is caused by our *decision*, although deciding is not "free"), is nevertheless caused (further down the causal chain) by the fact in question. From one's own internal point of view one would – wrongly

⁵⁰³ Bernard Williams, "Deciding to Believe", p. 149. Williams lists a total of five characteristics of belief, pp. 136-144, of which the present "very central idea" seems to be part of the fourth, p. 143. The other elements are not of immediate relevance.

⁵⁰⁴ Williams follows Hume in understanding causation in terms of a "regular connexion" between these things, p. 149.

⁵⁰⁵ However, consistently with the remainder of the position, "causation" must not be interpreted realistically as requiring "independence".

– understand the decision to be freely made. The route to belief proceeds from decision in such a way that the question whether the decision is itself “free” or “caused” is an independent question, a question to which the holistic empiricist can consistently give a determinist answer. In other words, we can decide to believe just as much as we can decide to do anything.

It is, however, true that “deciding to believe”, understood in a fully “free will” sense, does conflict with Williams’ causal theory of belief. The fact remains that, while our everyday concept of belief may involve a naïve view of causal dependence on experienced reality in simple cases or even in many cases, it is plain that we can and do, in fact, make decisions about all kinds of things, including on occasion decisions what to believe, as the case of the eighteenth-century explorer and the black swan shows. Only by defining “belief” in some technical way to suit Williams could this example be excluded. One might, of course, think that it *ought* to be excluded in order to save “normal” beliefs, but this would beg the question against the postmodernist, and in particular against Quine, for whom *any* belief is decidable or revisable. We can, in any event, to some extent decide how far to interpret the world in causal terms, for belief in the importance of causation in our understanding of the world is as revisable a belief as any other, given holistic empiricism. A developed theory of causal input is not ultimately to be understood as grounding our conceptualisations of reality, for it is itself subordinate to our *decisions* what to believe. Deciding to believe and causation can plainly co-exist in many ways.

Williams’ second argument, with no necessary connection to his first, relies on the claimed impossibility of knowing of some belief of mine both that I took it to be true and also that I had acquired it “at will”.⁵⁰⁶ “If I could acquire a belief at will, I could acquire it whether it was true or not; moreover I would know that I could acquire it

⁵⁰⁶ “At will” is here taken to be synonymous with “through conscious decision”, although outside this specific context it should not be assumed that “deciding to believe” something is the same as adopting that belief “at will”. For the latter suggests some ungrounded arbitrary taste or whim; see also John Gray: “Radical choices may well not be reason-based and yet not be acts of will”, *Isaiah Berlin*, p. 158.

whether it was true or not. If in full consciousness I could will to acquire ‘belief’ irrespective of its truth, it is unclear that before the event I could seriously think of it as a belief, i.e. as something purporting to represent reality”.⁵⁰⁷ His point may be summarised in the following way, although these are not entirely his terms: since a belief is essentially something believed to be true, one cannot sincerely adopt a belief at will, for this would amount to adopting what we think of as a *successful* representation of reality at will, which is not a power we understand ourselves to have.⁵⁰⁸ This point is plausible, given what is plainly Williams’ realist presupposition (seen by him as a conceptual truth, in this context) that reality cannot be determined “at will”. But it is clear that his conclusion would not follow if reality were to be understood in terms of what we freely chose to *count* as reality. If it is open to us to decide what counts as reality, and we recognise that we can do this, then evidently it is equivalently open to us to decide what to believe.⁵⁰⁹ It is begging the question merely to assert otherwise. To say this is to hold Williams wrong in his *conceptual* claim here. However, that does not mean that Williams is *substantively* wrong, for the position now reached suggests mere assertion on both sides. “Begging the question” is commonly dependent on where the *burden of proof* lies in a particular argument, and that is commonly a contingency. The central point Williams is trying to make will be dealt with below.⁵¹⁰

With Williams’ two conceptual arguments supporting the immediate objection, that we cannot decide to believe at all, now rejected, we can proceed further with the

⁵⁰⁷ Bernard Williams, “Deciding to Believe”, p. 148. It is not true that, when I “seriously think of it as a belief”, I think of it as “something *purporting* to represent reality” [our emphasis]. This is because, from the first person singular point of view, when I say or think “I believe that *p*”, I mean that I believe that *p* is true, that is, that *p* represents reality successfully. From the first person singular point of view, one cannot distinguish a serious (as opposed to a merely entertained) belief which *successfully* represents reality from a belief which merely *purports* to do so. Only an outsider can say of one’s belief that *p* that it “purports” to represent reality. No doubt, however, Williams intends this to be understood, and that “something purporting to represent reality” is to be read as “something successfully representing reality”. He is so read here.

⁵⁰⁸ It is not that reality, unlike us, has the power to induce a belief in us, for it may be allowed that we can cause our own beliefs (by hypnosis, for example). The point is that we do not have the power to make them *true*. The *independence* of reality is here presupposed.

⁵⁰⁹ It seems clear that our ornithologist could both sincerely believe it to be true of the world that all swans are white and also know that this belief was the result of human decision, in principle one that he had taken himself in the choice situation described above. It would not, incidentally, be surprising that an analysis of a commonsense concept should reflect the commonsense presupposition in favour of a naïve realism.

understanding of Quine's claim that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system".⁵¹¹ What exactly does this mean? We have already stressed the universality of the word "any" in this claim. Next, we will clarify the remainder of this sentence. To achieve the required clarity, it is usual in philosophy to translate a sentence with the grammatical form "B, if A" into the sentence "if A then B", ensuring that no change of meaning takes place by keeping the "if" and the tacit "then" firmly fixed to their associated clauses. Taking this step, Quine's claim then reads: "if we make drastic enough adjustments elsewhere in the system, then any statement can be held true come what may".

This now seems to have the form "if A then B", which standardly means that A is *sufficient* for B. Again standardly, it does not mean that A is *necessary* for B. Yet this amounts to reading Quine as saying that we can believe whatever we wish; and that one possible, although successful, way of doing this is by making sufficient adjustments in our belief system, should we wish to adopt that route. The reading leaves open the suggestion that we can believe what we wish for no reason at all. By contrast, it is quite clear from other expressions of Quine's position (such as the view that there is a "web" of beliefs which *constrains* particular beliefs), that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system" importantly implies "any statement can be held true come what may, *so long as* or *only* if we make drastic enough adjustments elsewhere in the system". In other words, Quine's claim ought to be read as follows: "any statement can be held true come what may, *if and only if* we make drastic enough adjustments elsewhere in the system".

Another difficulty of interpretation needs to be clarified. "Any statement can be held true come what may" is, on the face of it, ambiguous. It is common to distinguish "synthetic" from "analytic" statements,⁵¹² but Quine has a non-traditional view about

⁵¹⁰ In the section "The costs of belief".

⁵¹¹ W.V.O. Quine, "Two dogmas of empiricism", p. 43.

⁵¹² For readers unfamiliar with this, this terminology originally derived from Immanuel Kant, who produced the famous synthesis of empiricism and rationalism in his *Critique of Pure Reason* of 1781. The technical words "analytic" and "synthetic" are traditionally used to mark the two ways in which statements can be true, rather than the two ways in which statements can be *known* to be true. (*A priori* and *a posteriori* are used for the latter.) "Analytic" and "synthetic" exhaust the possibilities:

the nature of analytic statements, referring to “analytical statements, which hold come what may”.⁵¹³ The ambiguity appears as follows. On one reading, the scope of “come what may” is “true”, so that the quotation should be understood as “any statement can be held: true come what may”. This entails “any statement can be held analytic”, since “analytic” means “true come what may”. On this first reading, it follows that “any synthetic statement can be held analytic”. On the second reading, Quine’s claim is best understood if we think that a comma has been incorrectly elided, so that the quotation should be read as “any statement can be held true, come what may”. There is here no implied reference to a statement being held “analytic”, and “come what may” is just an airy way of saying that we can believe what we like.

The difference between the implied punctuation shows the ambiguity of interpretation, and this is important if we think of “synthetic” and “analytic” statements as distinct. Given this, then Quine’s view interpreted as being “any synthetic statement can be held analytic” may seem immediately unintelligible. It is appropriate briefly to clarify Quine’s position here. It would, using the traditional distinction, be taken as obvious that, to use one of Quine’s own examples, the statement “there have been black dogs”⁵¹⁴ is synthetic and not analytic. But Quine introduces an explanation of the “analytic”/“synthetic” distinction which expresses that distinction in terms of varying degrees of preparedness to accept or reject beliefs, a preparedness which is based on “pragmatic” grounds.⁵¹⁵ Broadly, a statement is at its most “synthetic” when we treat

for any true statement, the claim is that it is either analytic or synthetic. An analytic statement is true simply in virtue of the meanings of the words in it. A synthetic statement is true in virtue of some fact about the world, in virtue of reality actually being that way. A statement which is false by virtue of meaning, such as “all bachelors are married”, we call self-contradictory, while a statement which is false by virtue of fact is still, perhaps unhelpfully, called “synthetic”. Quine’s position is that it is a pragmatic choice whether “all swans are white” is treated as a statement describing experience (and is thus synthetic) or is treated as part of the definition of a swan (and is thus analytic). The upshot is that the analytic/synthetic distinction is not an absolute distinction at all. Hence we have Quine’s so-called denial of the analytic/synthetic distinction, although the words can still be used to contrast different degrees of preparedness to accept or reject beliefs and also to contrast definitions with what are taken as factual descriptions. “Analytic” and “synthetic” are terms which it is sometimes pragmatically convenient to use, although it is no longer easy to capture in a clear way our intuitions about the limits of their use.

⁵¹³ W.V.O. Quine, “Two dogmas of empiricism”, p. 43. See also W.V.O. Quine, *Word and Object*, p. 66.

⁵¹⁴ W.V.O. Quine, *Word and Object*, p. 66.

⁵¹⁵ The reasons are given in W.V.O. Quine, “Two dogmas of empiricism”, pp. 20-42. Karl Popper, as explained by Burleigh Taylor Wilkins, made a similar point, that the analytic/synthetic distinction rests for real languages primarily on the attitudes of [say] physicists to their theories, since they may

it as constantly at the mercy of passing particular experiences, and a statement is at its most “analytic” when we insist on its truth regardless of passing particular experiences. Since presumably nobody will deny that there have been black dogs, and as a past-tense statement it is highly invulnerable to present experience, then this statement counts as analytic when measured in Quine’s way, despite being synthetic when measured in the traditional way. A good deal of historical writing, in so far as it includes uncontroversial and well-established “facts”, is equally at the “analytic” rather than the “synthetic” end of Quine’s scale on this basis. Since there can be different degrees of preparedness to accept or reject beliefs and this can vary over time, there is for Quine no correctness to be had about whether a statement is “really” synthetic or analytic, and equally there is no clear demarcation line between the two. The traditional analytic/synthetic distinction is thus denied by Quine. That “any synthetic statement can be held analytic” is unintelligible is therefore not a correct conclusion to draw. “Any synthetic statement can be held analytic” simply means that any factual description can be held true regardless of any passing particular experience,⁵¹⁶ just as in the black swan example. Quine’s position here *presupposes* our capacity to choose such things.

Finally, note that “any statement can be held true come what may, if and only if we make drastic enough adjustments elsewhere in the system” is, like all “if...then” statements, *hypothetical*. However, Quine’s context makes it plain that his claim is not intended as a *hypothetical* claim at all. Rather, the expression of the claim masks the categorical assertion that we *can*, while meeting experience as a whole, make sufficient adjustments to hold true any one particular statement. The “can” here is the “can” of conscious choice. Quine’s claim at this point then amounts to the categorical assertions that we *can* hold true “any” one particular statement, and *can* make sufficient adjustments in the rest of our beliefs for this purpose. On the current approach, reality, far from being “independent”, is what we count it to be. Counting permits conscious decision what to believe. Reality is a function of our actual belief

choose to interpret them as either factual or as consisting of implicit definitions. Burleigh Taylor Wilkins, *Has History Any Meaning?*, p. 43 note 18.

⁵¹⁶ We could take “come what may” as “come what stimulation may”. See W.V.O. Quine, *Word and Object*, p. 66.

system, and thus also a function of our actual choices of beliefs, where those choices are made. The analysis of Quine's claim has revealed, in conclusion, three elements:

- (1) the categorical claim that we can hold true any statement;
- (2) the conditional or hypothetical claim that, if we are to hold true any statement, then we must make sufficient adjustments for the purpose;
- (3) the categorical claim that we can make sufficient adjustments for the purpose.

Quine, in apparent contrast to the "multiply configurable" element of the postmodernist position, offers in claim (2) what seems to be a constraint on the operation of claim (1). While we can decide to believe what we like about reality, our decision will require meeting costs, costs which will vary according to the decision made. While these costs, comprising adjustments to the web of beliefs, must be met, Quine holds that unlimited adjustment is nevertheless available. To say that there is unlimited adjustment available which enables us to hold a belief to be true is to say that we will always be able to meet the cost of adjustment, no matter how high that cost is. This ease of adjustment suggests that an assignment of truth-values within our web of beliefs is always available so that some particular belief can be held true, and this suggests a weak limitation on the number of possible assignments of truth-values.⁵¹⁷ (2) is, therefore, as it stands, not much of a constraint, for a cost which can always be paid is not much of a cost, and this result places Quine, perhaps surprisingly, close to the centre of the postmodernist camp.⁵¹⁸

Admitting the claim that we "can" make sufficient adjustments elsewhere in our belief system and so hold true any statement is nevertheless a hostage to fortune, when we are also told that we *must* make these adjustments, but do not yet know either what adjustments are required or the nature of the necessity involved. First, the adjustments – the costs of belief – need to be understood. The claim is that the costs

⁵¹⁷ Given the reasoning in the previous paragraph, the claim of "unlimited adjustment" or even "infinite adjustment" does not imply that there is an unlimited or infinite number of assignments of truth-values available, even though any one belief system may, logically, contain any infinite number of beliefs.

⁵¹⁸ Where he would, less surprisingly, join Richard Rorty. The "postmodern" implications of Quine's position were not particularly apparent at the time he was writing. It was Rorty who saw the implications.

of belief are such that we can *always* meet them. We can hold a statement true, “come what may”. But what may come, given holistic empiricism? What kinds of costs do we face? How do they arise? Are there no limitations whatever on our choice, so that the regularity and consistency of reality is denied by the present approach?

The costs of belief

When we seek to revise a belief, we face the costs involved, which consist in adjusting other beliefs. The costs of belief also arise when we note that some beliefs conflict with other beliefs, and we have to decide between them. We do not in fact face costs for *every* belief. It is a contingency when we face them. They do not arise for us in the case of established and accepted beliefs where no desire for revision exists and no grounds for revision are recognised. Costs essentially arise in situations of *choice*, where we desire to revise or are obliged to revise. *Decision* is called for. Decisions arise, most clearly, when we note that some beliefs conflict with other beliefs. They arise, importantly from an empiricist point of view, when particular “recalcitrant experiences”⁵¹⁹ are perceived to place other beliefs at risk. But it is clear that we cannot specify any example of a *particular* or “fixed” atomic recalcitrant experience without offending against the holistic position, for what counts as a “recalcitrant experience” cannot be theory-independent, cannot be independent of the web of beliefs. What is meant by “recalcitrant experience” can best be understood as follows: it is an actual particular experience which will be *counted* by people in a certain way: counted, first, as a “particular experience”,⁵²⁰ but counted more importantly as *prima facie* permitting a belief inconsistent with many beliefs already accepted. A recalcitrant experience is an experience counted as initially permitting a belief – a “recalcitrant belief” – which will require much adjustment elsewhere in the system of beliefs if it is to be fully accepted.

For example, from the point of view of the presupposed belief “all swans are white”, the experience which is *prima facie* describable as being of a black swan is a recalcitrant experience. To recognise the experience as a recalcitrant experience is to recognise the availability of this *prima facie* description,⁵²¹ at the same time recognising the inconsistency of that description with a pre-existing belief (“all swans

⁵¹⁹ This is Quine’s expression. W.V.O. Quine, “Two dogmas of empiricism”, p. 43.

⁵²⁰ That is, selective attention is consciously given to one part of the whole of experience rather than the rest. Choice is involved in selective attention, although not necessarily choice in how to describe what is attended to, since it is a contingency whether a choice of description arises for us. We can be, and for most things have been, *trained* to describe or count reality in a certain way.

⁵²¹ To recognise a *prima facie* description is to “entertain” the belief in question, to use older terminology.

are white”), and at the same time recognising that a *decision* needs to be made about which description to accept. To recognise that a choice comes into existence here is to recognise that both the recalcitrant description “this is a black swan” and the belief “all swans are white” have – at the point of choice – the status of being entertained for serious consideration, rather than the status of fully accepted beliefs. If we decide that the experience is to be counted as *really* being that of a black swan,⁵²² then, in accordance with Quine’s requirements, that is, “if we make drastic enough adjustments elsewhere in the system”,⁵²³ we will recognise that the originally fully accepted belief “all swans are white” will need to have its truth-value adjusted from true to false. So, in this example, the “cost” of believing “this is a black swan” is the requirement to adjust from true to false the truth-value of the belief “all swans are white”, and the “cost” of continuing to believe “all swans are white” is the requirement to classify in some other way what is provisionally labelled “black swan”. By contrast, believing “this is a white swan” on a different and appropriate occasion (that is, when looking at a white swan) typically involves facing no costs at all, unless one wished to revise the belief just for the sake of it (as postmodernism would permit one to do).

While Quine speaks in terms of recalcitrant “experiences” and so, in effect, speaks of the associated recalcitrant beliefs, the points now made can be generalised to cover recalcitrant beliefs, whether those beliefs are counted as having a plausibly direct reference to experienced reality or not. A “recalcitrant belief” in general is a belief entertained for serious consideration which, it is recognised, will require much adjustment elsewhere in our system of beliefs if it is to be fully accepted. Its recalcitrance consists in the availability of the belief together with the recognition that it is inconsistent with some element in the remainder of our belief system together with the recognition that a decision needs to be made about which belief to accept. To recognise the choice, as in the case of a recalcitrant “experience”, is in part to recognise that the conflicting beliefs have – at the point of choice – the status of being entertained for serious consideration rather than the status of fully accepted beliefs.

⁵²² Recall that “really” now has an antirealist interpretation.

⁵²³ W.V.O. Quine, “Two dogmas of empiricism”, p. 43. In effect, this is the assertion of an imposed requirement that our beliefs about reality be consistent. If we were to decide not to accept this requirement, then the belief “this is a black swan” need not be counted as a recalcitrant experience at all, since recalcitrance presupposes the requirement of consistency.

Making a decision where choice of belief is apparent need not be only a matter of how to count experienced reality but can also be a matter of determining what to believe in dealing with questions which go beyond whatever we count as “experienced reality”, questions which might include issues of morality, religion or mathematics, for example, as well as, most obviously, the unexperienced past of historiography.

As so often, David Hume is helpful, particularly here in beginning to understand the idea of the “cost” of belief. Hume saw the relevance of this concept, despite the generally atomistic nature of his empiricism. He remarked: “If we believe, that fire warms, or water refreshes, ‘tis only because it costs us too much pains to think otherwise”.⁵²⁴ There is a cost to revising the belief that fire warms, in the sense that not believing that fire warms would have a cost. The kind of cost envisaged by Hume here is exemplified, no doubt, in the pain of feeling cold, through keeping an unnecessary distance from a warming fire; or in the pain of being burnt, through putting one’s hand in a flame. It is apparent, then, that in Hume’s view we cannot revise our beliefs that fire warms or that water refreshes, because we cannot meet the costs of doing so: too much “pain” is involved. Such costs seem plain commonsense; however, they are examples of the simple experiences upon which Hume – here through a cost-benefit mediation – builds his atomistic empiricism. On a holistic empiricism like Quine’s, even beliefs about Hume’s simple experiences can be revised. Thus “any statement can be held true”, according to Quine, and this must include being able to hold true the statement “fire does not warm”. It is the costs of beliefs of this kind which need to be understood.

It is crucially important here not to muddle Hume’s and Quine’s kinds of empiricism. On the Humean version, the cost of believing that fire does not warm is a pain of some kind, and this is most naturally made sense of, in terms of Hume’s philosophy, by saying that the belief in question is false: we believe that fire does not warm, but this is contrary to some simple experienced fact. The ideas involved in the belief that fire does not warm do not reflect experience. Thus, presupposing Hume’s position, what

⁵²⁴ David Hume, *A Treatise of Human Nature*, p. 270. For Hume, the costs of belief can be set by “reason”, but only in so far as reason is something *felt*. He said: “Where reason is lively, and mixes

we are imagining when we reflect on the possibility of revising the belief that fire warms is what it would be like to try to live with a false belief. However, on the holistic approach, it is a gross error to suppose that this is what is involved in revising the present belief that fire warms.

Bernard Williams has produced an appropriately tough example in a related context.⁵²⁵ “Suppose a man’s son has apparently been killed in an accident. It is not absolutely certain he has, but there is very strong evidence that his son was drowned at sea. This man very much wants to believe that his son is alive.” Yet while he wants to be in this belief state, it would be foolishly inappropriate for the man to visit a hypnotist in order to have this belief state induced in him, for “*wanting to believe* means *wanting it to be the case*”.⁵²⁶ Williams refers in this example to the “apparent” death of the son, which is not “completely certain”. This lack of certainty may perhaps be included by Williams in order to give a psychologically more plausible opening for the supposition that the father might be hypnotised into believing a falsehood. However, it should be understood that it is a corollary of the holistic empiricist position that a sincere belief that the son has drowned could be revised *even if we all stood and watched* (so long as, following Quine, the costs of revision are met). The example should therefore not be seen as weaker than it might be because of the supposed absence of certainty. The apparent weakness in the example, implying as it does that reality may be such that the son is indeed still alive, is not relied on by holistic empiricism. Certainty about the death should be read into Williams’ example. Developing the example: while we might all deeply sympathise with the father’s desire to induce self-deception on the matter, surely it is clear that deciding to draw on postmodernism and revise what counts as reality here in such a way that the man’s son counts as still alive would be as much an exercise in self-deception as visiting a hypnotist in order to have instilled in one a false belief? Again, however, it is a gross error to suppose that living with a false belief is what is involved in revising the belief that the man’s son is dead.

itself with some propensity, it ought to be assented to. Where it does not, it never can have any title to operate upon us” (p. 270).

⁵²⁵ Bernard Williams, “Deciding to Believe”, pp. 149-150. It should be noted that Williams does not address in his article the holistic empiricist pragmatism expounded here.

⁵²⁶ *Op. cit.*, p. 150. Emphasis in the original.

The holistic empiricist postmodernist is *not* suggesting, and it does not follow from the position, that we might revise our beliefs so that some of them, such as “fire warms”, we blindly hold false, so that we wander the world getting alternately cold or burnt, and stupidly failing to grasp (or deliberately trying to avoid) the reason. Williams, indeed, imagines something like this suggestion for the bereaved father: “The man gets rid of this belief about his son, and then there is some belief which strongly implies that his son is dead, and that has to be got rid of. Then there is another belief which could lead his thoughts in the undesired direction, and that has to be got rid of. It might be that a project of this kind tended in the end to involve total destruction of the world of reality, to lead to paranoia. ...we have to have a project for steering ourselves through the world so as to avoid the embarrassing evidence. ...[yet] if he did not really know what was true, he would not be able to steer around the contrary and conflicting evidence”.⁵²⁷

This is not relevant to what is going on in the holistic empiricist argument. The revision of existing belief is a feature of a *holistic*, not of an *atomistic*, empiricism, and the point of allowing for revision is that we might come to count – or indeed we might once have counted – experience differently from the way we currently do. Put slightly differently, our present web of beliefs might once have been, or might one day be, organised differently from the way it is now, with “true” and “false” applied to different statements across the web. To understand the holistic empiricist position involves understanding how we might imagine counting experience in such a way that we *could* believe “fire does not warm” or (in the tragic context) “my son is alive”. Imagining ourselves to have adopted a point of view which includes the imagined beliefs “fire does not warm” or “my son is alive”, we may seem constantly to be faced with recalcitrant experiences which make those beliefs “false”. But we have seen that, given Quine’s holistic position, what counts as “recalcitrance” cannot be independent of the web of beliefs. A “recalcitrant experience” is to be understood as a particular experience counted as permitting a belief to be entertained for serious consideration which would require much adjustment elsewhere in the system if it were to be fully accepted.

⁵²⁷ *Op. cit.*, p. 151.

From the point of view of our imaginary belief system containing the imagined belief “fire does not warm”, the experience which is *prima facie* countable or describable as being of a hot fire is a recalcitrant experience. Its recalcitrance consists in the actual availability of the *prima facie* description, or entertained belief, “this is a hot fire”, together with the recognition that this description is inconsistent with an element (“fire does not warm”) in the imaginary belief system, together with the recognition that a decision needs to be made about which description to accept. If we, who are imagining ourselves to have this belief system, decide that the experience in question is to be counted as *really* being that of a hot fire, then our imagined belief “fire does not warm” will, we will recognise, need to have its truth-value adjusted from true to false. Yet, paralleling the similar argument concerning the “black swan” earlier, we can, given the holistic empiricist position, imagine ourselves so to decide things that we may keep the belief “fire does not warm” and deny the description “this is a hot fire”. Neither logic nor atomistic experience would determine the outcome of our decision here. A decision either way would resolve the recognised inconsistency and so remove the recalcitrant nature of our experience.

To recognise that a choice comes into existence here is to recognise that both the belief “this is a hot fire” and the belief “fire does not warm” have – at the point of choice – the status of being entertained for serious consideration, rather than the status of fully accepted beliefs. However, within this imaginary situation, deciding between the two options here is not affirming one belief and denying the other merely as a way of hypothesising. Nor is it merely a claimed freedom to entertain alternative beliefs, a trivial freedom to bracket off certain beliefs from others, so that one might accept temporarily those beliefs for the sake of some argument or set of supposed possibilities or some imaginary story. It is rather a claimed freedom to believe seriously, to believe successfully as true, something different about the world. The claim is that we can *hold true* of experienced reality anything we like, regardless of particular experiences. The outcome of our decision thus moves one of the two beliefs from the status of being “entertained” to the status of being “fully accepted”, that is, to the status of expressing *what we take reality to be*.

So, when the holistic empiricist suggests that we can revise our current beliefs so as to accept the belief “fire does not warm”, he is suggesting that we might revise our beliefs so that we *fully and sincerely accepted as true*, rather than merely entertained for consideration, the belief “fire does not warm”. But to imagine this would be to imagine, not merely our believing “fire does not warm”, but to imagine our believing “fire does not warm” *in addition to* our believing that we do *not* face recalcitrant experiences relative to it. This is because to recognise a recalcitrant experience is to recognise that a pre-existing belief, relative to which the recalcitrant experience is recalcitrant, has its status as “fully believed” under threat, and this is already to undermine its “full acceptance”. Thus to imagine believing with full acceptance and sincerity that fire does not warm is not to imagine merely entertaining the belief regardless of its consequences. It would be to imagine an *absence* of any recognised need to choose between entertained beliefs judged inconsistent, to imagine an *absence* of recalcitrant experiences relative to the belief “fire does not warm”, and therefore an *absence* of any recognised need to make major adjustments in our total web of beliefs. We would be imagining, in other words, that the choice of belief and thus all the necessary consequential adjustments *had already been made*. Given the way we presently count the world, even *imagining* that we might fully and sincerely believe “fire does not warm” is extremely difficult if not practically impossible to engage in, for we would be trying to imagine a vastly different set of successfully adjusted beliefs forming our total belief system, such that *nothing* was counted as a recalcitrant experience relative to the belief “fire does not warm”. This is utterly distinct from imagining what it would be like to live with a false belief.

It is no doubt extremely difficult and perhaps practically impossible to engage in the holistic imagining required here, but what *might* be involved in such a case is not beyond imagination. We can hint at possible changes in our organisation of experience in this case, suggesting, for example, that “fire” might come to symbolise for us not, perhaps, a warming flame but rather an illuminating flame, a flame which is, within our newly hypothesised imaginary belief system, accidentally rather than essentially connected with heat. Equally for the past as for the future, “fire” might once have been used to count reality differently from the way it does now. Quine gives us a curious instance of etymological change which displays a different

organisation of such experience over time. He says, a “startling example is black and the French blanc, ‘white’: it is conjectured that they are identical in prehistoric origin. The semantic link would have to do with fire – its soot on the one hand and its blaze on the other”.⁵²⁸ Similarly, we might imagine coming to think, or having once thought, of fire as essentially illuminating rather than essentially warming, and so imagine directing, or having once directed, our attention to the illumination-experience accordingly.

Explaining the holistic empiricist position again, this time in terms of Williams’ example, we are to imagine our having, or seeking to have, a belief system containing the belief “my son is alive”. From the point of view of this belief system, the experience which is *prima facie* countable or describable as being that of (at worst) my son’s drowned body is a recalcitrant experience. An inconsistency between the relevant element of the belief system and this *prima facie* description – and the need to determine which belief to accept – may be regarded as apparent to us. If we decide – one might well think, unavoidably – that this “recalcitrant” experience is to be counted as *really* being that of my son’s dead body, then we may well accept – one might think, instantaneously – that the belief “my son is alive” will need to be adjusted from true to false. Yet, on the holistic empiricist approach, it remains the case that we can, *given appropriate revision to our belief system*, decide *not* to count the experience of the dead body as requiring us to alter the pre-existing belief “my son is alive”.

It is clearly possible merely to *entertain* the thought that my son is still alive, despite the sight of his body. Indeed, I may well find belief in his death more than I can take. But the holistic empiricist suggestion is – in a sense – more brutal than this. It is suggested, not merely that we might entertain *for serious consideration* the thought that my son is still alive, but rather that we might revise our beliefs so that we *fully and sincerely* accepted the belief “my son is alive”, and “full and sincere acceptance” involves more than such Humean vivacity of belief as self-delusion might achieve. Such full and sincere belief, as explained earlier, involves our *not* facing recalcitrant experiences. To imagine believing with full acceptance and sincerity that my son is

⁵²⁸ W.V.O. Quine, *Quiddities: An Intermittently Philosophical Dictionary*, Harmondsworth:

alive is not to imagine merely being in the state of *choosing* whether to believe that he is alive or not, nor is it to imagine being in a psychological state of self-induced certainty. It is to imagine that no choice between entertained beliefs needs to be made, because with respect to the belief “my son is alive” we have *no* recalcitrant experiences and therefore we recognise *no* need to make major adjustments in our total web of beliefs. We would be imagining, as in the case of the example “fire does not warm”, that the choice of belief and thus *all the necessary consequential adjustments* had already been made.

Again, this is not a matter of imagining what it would be like to live with a *false* belief, but imagining what it would be like to live with a *true* one. This requires imagining our successful adjustment of a huge range of our present beliefs – far larger, indeed, than Williams implies. Again, this may well seem a practical impossibility, but, as in the case of the “fire does not warm” example, it is not in fact beyond imagination to hint at our adopting, for example, some semi-religious conception of reality which distinguished “real” life, involving perhaps a conception of life after death in some heavenly eternity, from the “appearance” of life in the passing earthly world. Such a conception need not be a wholly abstract matter, for there is no doubt that religion can turn despair to hope.

“He lives, he wakes, - ‘tis Death is dead, not he.” XLI.

“He hath awakened from the dream of life -

‘Tis we, who lost in stormy visions, keep

With phantoms an unprofitable strife,

And in mad trance, strike with our spirit’s knife

Invulnerable nothings.” XXXIX.⁵²⁹

On such an approach, “my son is alive” is given by us a real meaning and is also true, consistently with the sight of his dead body. The perceived inconsistency disappears. Still, my son is not *here*. The imagined alternative range of beliefs hinted at may not give us what we want and does not give us what we had. This is not least because such alternative ranges of beliefs *are* only imagined or hinted at. We can only try to

Penguin, 1990, p. 107, under the entry “Kinship of Words”.

⁵²⁹ Percy Bysshe Shelley, *Adonais* (1821).

find them from the point of view of what we already believe. *They are not, in practice, independently available at all.*⁵³⁰

All this is a crucial feature of the holistic empiricist position, properly understood. It is essential to being able to choose what to believe that entertained alternatives be actually recognised as serious alternatives. But an entertained alternative does not become a serious alternative merely by being generated by grammatical and logical devices – such as the introduction of the word “not” to an existing sentence – to produce denials of what we normally affirm or affirmations of what we normally deny.⁵³¹ The possibility of belief is not measured by logical possibility. It is a historical or sociological or psychological, and not logical, fact that something may be impossible for us to believe, and thus a historical or sociological or psychological, and not logical, fact that its contradictory may be held true come what may. Unlimited adjustment to the web of beliefs is in practice not available. Quine is wrong to imply that we can always meet the costs of adjustment.

The process of deciding to believe requires us to imagine seriously believing each alternative. It is not enough, in order to imagine seriously believing something which is normally unacceptable to us, merely to entertain a sentence which is “possible” according to the construction rules of some invented – or even some discovered – logic. Accepting, or imagining our acceptance, of an alternative serious belief – as opposed merely to entertaining an alternative belief – requires our acceptance, or imagined acceptance, of an alternative belief *system*. Such a “system” is not an imaginary logical construction but that which would contain the entirety of one’s actual beliefs, as assessed by the entirety of one’s actual experiences.⁵³²

⁵³⁰ Wrote Oliver Wendell Holmes, Jr., paradigmatic pragmatist, and asserting an antirealist position, “When I say that a thing is true I only mean that I can’t help believing it – but I have no grounds for assuming that my can’t helps are cosmic can’t helps”. Letter to Harold Laski, 11th June 1929, a reference owed to Richard A. Posner, *Overcoming Law*, p. 192.

⁵³¹ What is available to be *entertained* as an alternative belief may well be based on the logically possible, by mechanical generation from existing beliefs. We can do so most obviously by using the operations of elementary logic (although no doubt “lateral thinking”, if that is different, could also be an available route). Thus, for any existing belief we have of the general form “S is P”, we can generate an alternative “S is not P”. But all this is at the level of “entertainment”, not “serious belief”.

The “entirety” of one’s beliefs, as mentioned here, is limited by the word “actual” and so is finite, but it is not limited to immediately occurrent mental perceptions. For example, you would not count as a *serious* belief of mine one which was judged by me as consistent with the rest of my beliefs, when I had examined only a *subset* of my total belief system, owing to tiredness or laziness or poor memory. Examining one’s total belief system is examining as full a range of beliefs as is reasonably practicable. There are contingent limits to what is reasonably practicable, while the web does not have a determinate entirety in any event, other than as an abstracted ideal at some hypothesised point of time, and beliefs are best seen against the background of an ongoing and changing holistic process. Moreover, “our” beliefs are typically beliefs which are *shared*, at a given time or period of time. In discussion with others, cross-examination can help the practice of discovery here.

It is in this way that believing something in a serious way requires believing it together with the absence of any perceived need to make adjustments in our web of beliefs, because any necessary adjustments are in effect already made. We cannot succeed in imagining fully accepting or seriously believing a proposed alternative belief without imagining those adjustments which we also perceive as being required. Although metaphorical, the notion of “perception” of necessary adjustments is itself to be taken seriously: it does not count as “absence of perceived need to make adjustments” if one did not “look” in the first place, or adopted the deliberative analogy of keeping one’s eyes shut.⁵³³ Within these constraints, a great many of our ordinary beliefs are thus not revisable because alternatives are not recognised as seriously available.⁵³⁴ For an alternative to be “seriously” available is for a coherent alternative belief system to be recognised as available. It is a pragmatic question how comprehensively consistent that alternative system needs to be in order to provide us with what we recognise as an effective alternative, for our existing system may contain inconsistencies. We may well face and have to choose between what we judge to be sub-optimal alternatives.

⁵³² The reference to “one” here does not need to distinguish between singular and plural experiencers.

⁵³³ This point may be likened to Popper’s method of falsification. See his *The Logic of Scientific Discovery*.

⁵³⁴ We thus have a grounding for what might be thought of as “commonsense”, although this approach should not be thought of as a way of interpreting the eighteenth- and nineteenth-century Scottish philosophy of that name.

The speculative suggestions concerning the practical possibility of our fully believing “fire does not warm” are so limited as to be of no help whatsoever. Contingent though it may be, the belief “fire warms” is in fact an unchallengeable belief for us. There are in fact no alternatives because there are no alternative belief systems recognised as available, and, as explained, we do not find alternatives to existing beliefs merely by inserting the expression “it is not the case that...” in the appropriate place before those existing beliefs. We do not in fact have available any imaginable alternative belief system which would enable us to revise the belief that fire warms, and it would involve unacceptable intellectual expenditure to try to formulate such a belief system to a sufficient degree to give us a serious choice of description in the matter. We have no reason to revise our belief that fire warms, and any unfounded desire to do so will, we may predict, be unsatisfied. However, future historians might think this prediction wrong.

Equally, there is no available alternative belief system which would enable us to revise the belief that (imagining ourselves within Williams’ example) the son had died. It is unimaginable that one could, say, *revise* what would (within that example) currently be counted as an experience of paternal grief, a revision which would be unavoidably involved in the revision programme engendered by the father’s holding true “my son is alive”. Quine thinks that we can revise such claims “by pleading hallucination”.⁵³⁵ However, although the father can hallucinate, he cannot *plead* it, for under what circumstances could he do this? To claim “I am hallucinating that *p*” is in effect to admit that *p* is false. Hallucination is little different from the hypnotism in Williams’ example. To plead that the father believes that his son is still alive because he is hallucinating is to presuppose that the son is dead. It may explain how the father comes to have the belief state in question but it does not offer what the theory requires here, which is a revised representation of reality without recalcitrant experiences. To suppose that the father’s hallucination nevertheless involves the required alternative coherent representation of reality is mere assertion, and begs the question. The objection is that no alternative belief system is in fact available. It will not do to say

⁵³⁵ W.V.O. Quine, “Two Dogmas of Empiricism”, p. 43.

that the father has such a belief system, but we do not know what it is since it is locked inside his hallucinatory state. Such a suggestion leaves out of consideration the required absence of recalcitrant experiences. Importantly, it is not the father's own experiences which are alone relevant to the belief system, but ours too. Moreover, we cannot all plead hallucination. Whatever the medical condition of our mental states, the required alternative belief system is not in fact available to us.

If it is to be possible to doubt some existing belief, we need first to envisage an alternative belief and then, for the doubt to be serious, envisage an alternative belief system with adjustments successfully completed at least to the point of minimising recalcitrant experiences compared to the existing belief; and our actual view of the world, expressed in our particular community's totality of beliefs, may not in fact give us any such alternative. While it may be correct to say with Quine that we can hold true what we wish so long as we *can* meet the cost of adjustment, it is clear that we *cannot* meet the cost of adjustment in many cases. Many factual beliefs are thus inescapable in practice: we do not entertain alternatives to them and even if we do generate such alternatives we cannot imagine seriously believing them. In that sense such beliefs are indubitable. Such practically inescapable beliefs may be compared with, and to a small extent can help to analyse, Collingwood's "absolute presuppositions". As we saw earlier, these are beliefs or assumptions underlying the beliefs and attitudes involved in our ordinary ways of life, an assumption which is a historical absolute for a time, in that it is contingently uncriticisable at that time.⁵³⁶ They are uncriticisable because they are not entertained as conscious thoughts, therefore neither doubted nor actively contrasted with serious alternatives.

At any given historical time the absolute presuppositions for that time are practically irrecoverable and so uncriticisable by those existing at that time. Yet such presuppositions, as historical presuppositions, are contingent – in spite of their practical undeniability in their own time – and can be regarded as having been "properly recovered" at a later time in two different ways. First, they may be seen as having been successfully recovered when we are in a position to recognise them as

contingent, which is to *entertain* them in serious conscious deliberation as possibly either true or false. Recovery would then entail recognition of criticisability, therefore, although not necessarily replacement by an alternative with all costs met. The presupposition might resist criticism and continue as an “absolute”, that is, as a practically unchallengeable although newly explicit belief. Second, since, at the time of recovery, the historical “absolutes” in question may be seen as *ex hypothesi* anachronistic and so no longer absolute, their successful recovery can be interpreted to involve contrasting them with the seriously accepted beliefs held at the time of recovery. The philosophical and historical recovery and understanding of an absolute presupposition would then involve recognition, not merely of “possible” falsehood, but of its being currently held false, so that absolute presuppositions would lose absoluteness as they reached the light of day.

How much room for factual choice and judgement is there? The upshot of the reasoning here is that postmodern revisability, properly understood through a critical consideration of Quine’s conception of the revisability of our beliefs, can be accepted as in principle allowing a multiply configurable world, but that in many matters we do not have a practical choice of belief, and the world remains very much as we currently think it is in the light of our best efforts. Reality, while a function of human choice, is nevertheless “fixed”, in so far as there is a lack of pragmatically available alternatives to existing beliefs. Far from being a startling claim about the massive revisability of all commonsense beliefs, a proper understanding of the pragmatics of holistic empiricism reinforces the broad reliability of our understanding of reality. When new evidence arises, there may well be need for revision of our existing beliefs, and historiography can certainly advance in this way. However, in many everyday examples such as those we have seen, the room for choice, and so for revising factual judgement, is very limited.

Summarising, the postmodern position suggests freedom of choice in the context of what to believe about reality, and hence it is contingent what people understand reality to be. The claim is that reality is “multiply configurable”. Adopting an empiricist

⁵³⁶ See W.H. Walsh, *Metaphysics*, pp. 160ff.; R.G. Collingwood, *Essay on Metaphysics*; and R.G.

approach, we began the argument by analysing postmodernism in terms of Quine's view that our reality-expressing beliefs are holistically related to each other. Quine, we have seen, claimed three things: first, that we can hold true any statement; second, that, if we are to hold true any statement, then we must make sufficient adjustments for the purpose; third, that we can make sufficient adjustments for the purpose. We have observed that, on this view, adjustments are required when some beliefs conflict with other beliefs, and when we have to decide between them. We have seen that such conflict has to be resolved in a holistic and not atomistic manner, which means that any required adjustments to our existing belief system have to be completed so as to minimise recalcitrant experiences by comparison with it. Quine's third point was then wrong because, contingently, in many cases we *cannot* meet the cost of adjustment. Many factual beliefs are thus pragmatically inescapable and thus in practice indubitable.

Quine not postmodern enough

The costs of belief arise when we seek to revise a belief and so seek alternatives, and arise in particular when we have to decide between conflicting beliefs. They arise, importantly from Quine's empiricist point of view, when particular "recalcitrant experiences"⁵³⁷ are perceived to place other beliefs at risk. The argument of the preceding section concludes that we *cannot* meet the cost of adjustment in many situations, and this was exemplified in particular by the cases of "fire does not warm" and "his son is alive", as explained. The alternative belief systems which avoid relevant recalcitrant experiences are typically not available to us. Unlimited adjustment to the web of beliefs is therefore not available either. The cost of revising many beliefs is, in fact, more than we can bear.

A central element of the postmodernist position is that reality is "continuously changing, irreducibly various, and multiply configurable".⁵³⁸ This expresses a claim to total freedom to count reality as we wish.⁵³⁹ The claim seems, in practice, false. However, two ways of reading this claim should be distinguished. Imagine a consistent set of grammatically constructed sentences, *a, b, c, d,...* which express our beliefs about the world. Imagine that the existing belief system is such that we assign to these⁵⁴⁰ the following truth-values: T, F, T, F,... Following postmodernism in the quotation just given, we wish to revise this system, just for the sake of it, that is, just because postmodernism says we can. Quine's assertion that "any" statement can be held true is, however, ambiguous in its implications. It certainly implies that *a* can be held true, and implies that *b* can be held true, and so on for the entire set. But it does not necessarily imply that *a* and *b* can both be held true at the same time. The cost of making *a* false instead of true will typically require revision of the rest of our beliefs. Assume such an alternative system is available. Simplifying, this alternative system would have alternative assignments of truth-values from those involved in the existing

⁵³⁷ This is Quine's expression. W.V.O. Quine, "Two dogmas of empiricism", p. 43.

⁵³⁸ Barbara Hernstein Smith, *Contingencies of Value*, p. 151, as quoted by Richard Eldridge.

⁵³⁹ Even this does not amount to a claim to total freedom to count reality as *I* wish. The distinction between "we" and "I" is important, and will be dealt with later.

⁵⁴⁰ Or to the statements these sentences are used to make. The sentence/statement distinction is not relevant here.

belief system, so that T, F, T, F,... are replaced by, say, F, T, F, T,... Hence we could, if we wished, choose to believe that system instead. On one or other assignment T, F, T, F,... and F, T, F, T,..., every sentence has, as it were, the opportunity to be held true and the opportunity to be held false, yet the number of possible sets of assignments could be extremely limited, and perhaps as few as the two mentioned.

However, the claim that “any” statement can be held true could be taken to mean that “every” statement can be held true, so permitting the following truth-values to be assigned to the set: T, T, T, T,... The quoted expression of the postmodernist position suggests that this is permissible, *however logically perverse that might be*, and it would certainly be logically perverse if sentence *a* were “fire warms” and sentence *b* were “fire does not warm”. By contrast, the entire argument based on Quine’s approach took it for granted that consistency across the system constrained what we can believe, so that, where one belief conflicts with another, we *have* to choose. Equally, it was on the basis of this claim that “recalcitrance” had the importance that Quine, and we, gave it, and on the basis of this claim that alternative beliefs were held to be, in practice, commonly not available.

Foucault says,

“‘truth’ and similar expressions are devices to open, regulate and shut down interpretations. Truth acts as a censor – it draws the line. We know that such truths are really ‘useful fictions’ that are in discourse by virtue of power (somebody has to put and keep them there) and power uses the term ‘truth’ to exercise control: regimes of truth. Truth prevents disorder, and it is this fear of disorder (of the disorderly) or, to put this positively, it is this fear of freedom (for the unfree) that connects it functionally to material interests.”⁵⁴¹

We are aware that a naïve or commonsense realism might involve belief in a determinate reality, such that reality is supposed to be some fixed or unchanging and logically consistent conflict-free entity which is independent of us and of the beliefs we have about it and can be used as the test for the truth of our beliefs. However, we

have left that position behind in our adoption of antirealism. We allow that reality is what *we count* it to be, and in so far as this is a socially organised operation it is not open to us to refuse *a priori* such approaches from political theory or historiography as offer to explain that operation. We lose “truth”, objectively interpreted. The suggestion now is that we should add to the quotation from Foucault the thought that logic should be lost too, because it seems arbitrary to insist on realistically interpreted logic when we have given up on realistically interpreted truth.⁵⁴² We should not assume as Quine often does that some objective and independent standard of logic remains available.⁵⁴³ It is perhaps just a conservative error to govern our beliefs by requiring that they be “true” and not “false”, so imposing an unwarranted consistency. Maybe the search for universal consistency, presupposed in our presentation of Quine’s position, is a totalitarian desire to impose an order based on logic, as if logic had independent validity, when logic is no better than any other part of our web of beliefs.⁵⁴⁴ Maybe pluralism is, after all, appropriate to our understanding. Maybe our choices what to believe are not constrained at all. Quine is just not postmodern enough.

But truth and falsity, while they are no longer straightforwardly interpretable as comments upon the correspondence of our beliefs to reality, are still available as comments upon the relationships between our beliefs. Normally, if we think that two beliefs are inconsistent, then we think that we cannot believe both of them. The postmodernist now seems to be allowing: “go ahead: believe both of them”. The *power* which supposedly blocked this is now imagined to be absent. Lewis Carroll’s White Queen famously sometimes “believed as many as six impossible things before breakfast”, but it is often forgotten that she said this in response to Alice’s assertion that it was “impossible” for her to believe that the Queen was aged, as she claimed, one

⁵⁴¹ This quotation is from Keith Jenkins, *Re-thinking History*, p. 32, who quoted it from Foucault’s *Power/Knowledge*, New York: Pantheon, 1981, pp. 131-3.

⁵⁴² We lose, for example, the “law of excluded middle”, which asserts that “*p* or not-*p*” is true. A very wide range of material exists with relation to the philosophical issues here, most stimulated by the works of Michael Dummett. See, for a collection of essays, his *The Seas of Language*, Oxford: Oxford University Press, 1996.

⁵⁴³ Quine’s position is not as clear as it might be. Alan Weir concludes “Quine must be placed firmly in the camp of the anti-realists”. Alan Weir, “Quine on indeterminacy”, forthcoming.

hundred and one, five months and a day. Few of us would think that *impossible* (unless we took seriously the Queen's earlier claim that she was remembering both forwards and backwards). More helpful is the Queen's claim that "nobody can do two things at once, you know".⁵⁴⁵

When "incompatible", said of statements, means "cannot both be true", what does the "cannot" mean? Normally, we think it involves logical impossibility. In understanding this we might well refer to some standard outside – that is, independent of – our own first-person singular points of view. There are, we sometimes say, "laws of logic", and it is these which "prevent" our believing inconsistent things. But, if the laws of logic are some externally imposed standard, it seems that logic is merely a normative system which we can disobey. It seems to be a social practice or game whose rules can be broken. Like other laws, logic could be *enforced*, so allowing Foucault's suggestion that it be enforced on the basis of "material interests". But some regimes are less effective at enforcement than others. Moreover, there are different conceptions of the laws of logic. The incompatibility of statements may then mean "incompatible according to system X", so that it has the form of a *hypothetical*: "If I adopt system X, then I cannot believe these two things". We might then say that sense can be made of believing two incompatible things by judging them incompatible according to one system but believing them both according to another.

However, if I cannot believe two incompatible things, is it really political force – choice of laws, material interests or otherwise – which prevents me? Rather, it seems *impossible* for me to believe two incompatible things, for example, both that fire warms and that it does not. The incompatibility is for me *categorical* rather than *hypothetical*. I cannot believe two things which *I* judge incompatible, and I take "incompatible" to involve "cannot both be believed". "Believed", *from the first-person singular point of view*, means "believed to be true". So, when I judge that two things "cannot both be believed", that means that I judge that those two things "cannot both be believed to be true by me". The only sense to be made *by me* of my believing

⁵⁴⁴ With a curious meta-inconsistency, Quine himself says that the principles of logic are themselves "simply certain further statements of the system". W.V.O. Quine, "Two dogmas of empiricism", p. 42.

two inconsistent things is that I separate the two by difference of time, so that I take myself to have changed my mind or take the world to have changed in the meantime. My inability to believe two things which I judge to be incompatible is a personally perceived “psychological” constraint, not an externally judged political one.⁵⁴⁶ The point we make here is a practical one; this is not an *analysis* of consistency or inconsistency. When I try to believe two things which I myself judge incompatible, then the only “standard” I can use is one I adopt as reflective of my own psychological capacities for belief. I, in the relevant example given earlier, would be *unable* to believe that my son was still alive, given that I was *unable* to make the revising adjustments in my system of beliefs which I judged were necessary according to my own judgements of inconsistency.

Can we, then, believe incompatible things? Despite the above, *of course we can*. I cannot believe *p* and not-*p*, and neither can you; but certainly I can believe *p*, and you can believe not-*p*. We have here deliberately moved from the first-person singular to the first-person plural, assuming among other things that my *p* and not-*p* is the same as yours. So much reasoning is normally shared that moves of this kind are commonly not noticed, while our everyday reasoning is little the worse for it; here, for example, is David Hume writing in a way which completely ignores such distinctions: “All the perceptions of the human mind resolve themselves into two distinct kinds [third-person], which I shall call IMPRESSIONS and IDEAS [first-person singular]. The difference betwixt these consists in the degrees of force and liveliness with which they strike upon the mind [third-person], and make their way into our thought or consciousness [first-person plural]”.⁵⁴⁷ However, translating from one such point of view to another is not always neutral with respect to the arguments involved, and much of philosophy from the twentieth century onwards requires great care in identifying the points of view involved.⁵⁴⁸ In particular, “we” is ambiguous in a way in which “I” is not.

⁵⁴⁵ Lewis Carroll, *Through the Looking Glass*, chap. V. Various online and printed sources.

⁵⁴⁶ How that is to be explained is different matter, now that we no longer refer to our “faculty of reason”. Maybe the White Queen was right in advising Alice to “draw a long breath, and shut your eyes” in order to overcome her pitiable failure to believe.

⁵⁴⁷ David Hume, *A Treatise of Human Nature*, p. 1.

⁵⁴⁸ Descartes, with extreme clarity, tried to build a philosophy entirely from the first-person singular point of view. It led to such extreme scepticism as to be, for many, self-refuting. Even Descartes,

Here we use a distinction which is the same as that noticed earlier,⁵⁴⁹ when we observed an ambiguity between “the practitioners” of a discipline conceived as individuals and “the practitioners” of that discipline conceived as a group. It was not enough to list different views of the nature of historiography on the part of different historians. Rather, we needed to seek views sufficiently shared to amount to a *consensus* about the character of the discipline. If historians had contradictory views then no consensus would exist, and historiography would not exist as a discipline at all. If there were no shared “rules” which specified what counted as being a member of or a contributor to the discipline, then no historian could properly be characterised by other historians as mistaken.

When we ask if “we” can believe incompatible things, we have the same ambiguity in the interpretation of “we”. The first-person plural can here be conceived as a mere list of first-person singulars, or it can be conceived as a group, understood as sharing some relevant consensus. If we think in terms of a mere list, then, in the imagined public space between us, there are no constraints on what we believe, for there is no objective independent “reality” which will select for us which of us is believing truly. Here the postmodernist is effectively right: if reality is no more or less than what “we” (conceived as a collection of first-person singulars) count it to be, then, since I count the world as *p* and you count it as not-*p*, reality is such that *p* and not-*p* (continuing with our assumption that my *p* and not-*p* is the same as yours).

But, so imagined, you and I do not *share* our reality. Do we care? In fact, “we” contingently do, at least up to a point, and here we are asserting something about ourselves at the level of what might be called group consensus or community. On the whole, we *value* others, and we *value* sharing reality with them. Our *shared* language amounts to a *shared* way of organising reality. It is indeed this which permits the assumption, to which we wrongly helped ourselves three paragraphs ago, that my *p* and not-*p* is the same as yours. Truth as personally judged is not our goal, but rather

however did not appreciate the point made here: “Let us suppose, then, that we are dreaming...” René Descartes, *Meditation I*, “Of the things of which we may doubt”, p. 81.

⁵⁴⁹ In the section, “Our primary sources”.

truth as agreed. From the perspective of the individual, “truth”, “reality” and “consistency” are then not wholly personally determinable but rather function as external and objective. Such “objectivity”, however, is to be antirealistically understood, and this enables us to recognise that truth, reality and consistency function as values for us, values which express our desire and need to share the same world. You and I need to *decide*, in constructing our shared language, whether to count the world as *p* or as *not-p*, for neither one of us is capable of believing both, and we will not share the world without some agreement here. *Nevertheless, it is a contingency how much agreement we need, or how much we value sharing.*

Our existing language displays our current agreement, and also displays its contingent limits. We contingently share much of the world, and have little difficulty agreeing on our expressions of what we count reality to be when we use simple and short sentences such as “fire warms”. Many beliefs are, again contingently (for the web is in fact a changing process), accepted as part of a system which is largely unchallengeable. Yet even our disagreements typically demonstrate the same point. From my point of view, *p* is true. From your point of view, *not-p* is true. Reality, from my point of view, is expressed by what I say, but not by what you say. I recognise the conflict, and hold false what you say. You do the same. Importantly, you and I recognise the situation *as a conflict*. Each of us judges the other’s assertion to be inconsistent with his or her own, and we do so partly in virtue of a *commitment* to a *consistent* reality, which does not permit a pluralist approach to truth. Characteristically, we do not accept “true for you” and “true for me” in factual contexts. Disagreement is a worry, there to be overcome rather than left to continue. How it is to be overcome depends on the disagreement. In historiography, it is by historiographical methods.

However, such methods for overcoming conflict are again contingencies. Recalling an earlier point, the consistent web of beliefs is not an ideal determinate entity, but is best seen as an ongoing and changing holistic process, involving only contingently the resolution of conflict by an evolving search for consistency in the face of recalcitrance. Such contingencies are historically located: remarks Martin Bunzl, “The drive for historians to speak of the past with one voice was a powerful tool in the nineteenth-

century movement of professionalization”.⁵⁵⁰ It is only a contingency that, now, we share our language and its modes of translation with others to the extent that we do, and with that can if we choose seek a peaceful global understanding and accord that the more idealistic of us hope for. Historiography is only one contributor to our society’s self-understanding. Law is another, and (although with a different argument in mind) Richard A. Posner finds for us, from Louis L. Jaffe, a helpful expression of what is plausibly the position there: “In a society so complex, so pragmatic as ours, unity is never realized, nor is it necessary that it should be. Indeed, there is no possibility of agreement on criteria for absolute unity; what is contradiction to one man is higher synthesis to another. But within a determined context there may be a sense of contradiction sufficient to create social distress; and it is one of the grand roles of our constitutional courts to detect such contradictions and to affirm the capacity of our society to integrate its purposes”.⁵⁵¹ But there is no assurance of peaceful integration. We might instead seek to overcome conflict, including conflict about how to conceive our own history, by war, as we strive to remove those who hold what are to us alien conceptions.⁵⁵²

When we disagree, we each see ourselves as *contesting* with each other, and the contest arises characteristically because and in so far as we seek to *share* our reality.⁵⁵³ We are thus both contingently committed to a shared consistent reality. Our contingent need and desire to share reality with others, typically peaceful up to a point, and which may well be understood to be natural to us,⁵⁵⁴ demands shared communication and so a shared language and so shared conceptions of what we are able to believe. In disagreeing with each other, my inability to believe *p* and not-*p* is shared with you. By this means, shared standards of consistency to which we are contingently committed may be properly admitted to our understanding. The present

⁵⁵⁰ Martin Bunzl, *Real History*, p. 4, drawing on Peter Novick’s *That Noble Dream*.

⁵⁵¹ Richard A. Posner, *Overcoming Law*, p. 76, reference given as Louis L. Jaffe, *Judicial Control of Administrative Action*, 1965, pp. 589-590.

⁵⁵² Even on our own side in war, the contingencies of power can attempt to determine reality: thus, as in the 1840 “black bottle” incident with Captain Reynolds and Lord Cardigan, the bottle should be counted as porter, and not moselle, if the commanding officer says that it should. Not dissimilarly, juries can decide questions of fact.

⁵⁵³ This argument is expressed with different detail in my *Rights and Reason*, Chesham: Acumen, 2003, chap. 10.

⁵⁵⁴ A flourishing family would require it, if no bigger a unit.

argument then defaults to give our previous conclusion, so that, with broadly shared standards of consistency now available, unlimited adjustment to the web of beliefs is therefore not available. As earlier shown, the cost of revising many beliefs is, in fact, more – not just than *I* can bear – but than *we* can bear.

Summarising, we have now seen that both Quine's argument and our earlier response to it presupposed the availability of standards of consistency and so did not allow for postmodernism in a form which denied that. We have examined this denial, and have argued that, from the first-person singular point of view, the judgement of consistency or inconsistency is pragmatically to be understood in terms of our ability or inability to believe things which we contingently judge to be complementary or conflicting as ways of counting reality. Shared standards of consistency arise in the same way when we, contingently, share with others what we recognise as conflicting and, contingently, also share the recognition that the conflict needs to be resolved in so far as we seek to share our reality. This pragmatic approach in which we share an understanding of the need to avoid or overcome conflict gives us sufficient shared standards of consistency to drive our earlier conclusion: that the requirement for a holistic resolution of conflict makes many factual beliefs pragmatically indubitable.

Chapter 5 The Room for Judgement

Narrative truth

Historians create large structures of meaning with respect to which matters are more complex than so far discussed. Recalling our earlier quotations, Michael Bentley says of historiography's identity that it is "a distinctive way of organizing and representing knowledge",⁵⁵⁵ while, of the father of history, Shotwell says "Herodotus was as much an investigator and an explorer as a reciter of narrative, and his life-long investigation was 'history' in his Ionian speech. Yet Herodotus himself hints that the word may also be applied to the story which the research has made possible, ...to a narrative such as he and his soberly inquisitive fellows could tell".⁵⁵⁶ The argument of our previous section used short sentences for its examples, but historiographical writing is characteristically a lengthy affair. Characteristically historiographical is the view, going back to Herodotus, that it is the historical *account* or *narrative* which is factual. The historiographical account characteristically presents facts in a unified way, and, in so far as that unification is factually appropriate, it is, as a whole, in consequence factual itself. The nature of that "in consequence", however, is a complex matter.

Herodotus "succeeded in putting together a trustworthy account of events he was too young to have witnessed and of countries whose languages he did not understand".⁵⁵⁷ Associated with the historical account's claimed status as factual description is the view that it ought, in some way still to be understood, to be factually *complete*. Herodotus did not think that he was offering merely a list of discontinuous facts. As we saw earlier, the Editor of the *English Historical Review* remarked that "Two views prevail concerning the scope of history. One regards it...as being concerned only with states... The other, which has found illustrious exponents from Herodotus downwards, conceives it to be a picture of the whole past".⁵⁵⁸ Bury observes that

⁵⁵⁵ Michael Bentley, *Modern Historiography*, p. 1.

⁵⁵⁶ James T. Shotwell, *The History of History*, vol. I, pp. 8-9.

⁵⁵⁷ "The place of Herodotus in the history of historiography", A.D. Momigliano, p. 129.

⁵⁵⁸ "Prefatory Note" to *The English Historical Review* (Mandell Creighton, Editor) [1886], in Fritz Stern (ed.), *The Varieties of History*, pp. 174-177 at p. 175. Herodotus, we observed, did explicitly limit the detail he provided.

“focussing under one point of view, and fitting into a connected narrative... Herodotus is irreproachably comprehensive; ...his book...is a lesson in the unity of history”.⁵⁵⁹

A hindsight view of Herodotus would typically see him as very good at synthesis – “irreproachably comprehensive”⁵⁶⁰ – while somewhat unreliable in his presentation of some specific facts; hence his reputation as a liar.⁵⁶¹ It seems plain that the facts could be synthesised, that is, *chosen* and associated with each other, in different ways by different historians. Thus Thucydides chose differently: “Thucydides believed that nothing significant had happened in time before the events which he described, and that nothing significant was likely to happen thereafter”,⁵⁶² so he left out those facts. Compared with Herodotus, the merits of Thucydides commonly appear reversed: while not characteristically perceived by historians as offering a “lesson in the unity of history”, he has often been regarded as trustworthy on particular facts in a way in which Herodotus was not, and “history had barely begun when Thucydides attacked the methods and purposes of Herodotus”.⁵⁶³ While it is anachronistic to ascribe to Thucydides a proto-understanding of our current “scientific” historiography, by which individual facts can be derived from evidence in far more rational ways than were available to him, it remains the case that, even when later historians are warned to doubt the work of earlier historians, we “instinctively... affirm as established truth everything that has been said by Thucydides”.⁵⁶⁴ Herodotus and Thucydides have each been used as model historians, the one on the basis of his synthesis and the other on the basis of the reliability of his particular facts.

We now have an image of a historical account in which it consists of a selection of unified facts; strictly, a selection of factual sentences. (It may, of course, contain more than this.) Each “fact” is then conceived as a part of a whole account, and it then seems appropriate to understand each fact as an “atomic”⁵⁶⁵ element of that

⁵⁵⁹ J.B. Bury, *The Ancient Greek Historians*, p. 45.

⁵⁶⁰ *Ibid.*

⁵⁶¹ See “The place of Herodotus in the history of historiography”, A.D. Momigliano, *Studies in Historiography*, *passim*.

⁵⁶² E.H. Carr, *What is History?*, p. 104.

⁵⁶³ G.R. Elton, *The Practice of History*, pp. 16-7.

⁵⁶⁴ Langlois and Seignobos, *Introduction to the Study of History*, pp. 157-158.

⁵⁶⁵ Understood in its familiar sense as a foundational building block or smallest unit.

account, if we imagine choosing to “analyse” or break up the account into its constituent parts. Equivalently, the many facts, so understood as “atomic”, appear as “synthesised” into the coherent whole account.

Recall Langlois’ and Seignobos’ 1898 work *Introduction aux Études Historiques*,⁵⁶⁶ which presented historiography in an organised way, 46% of which was, according to the Warring States Project, on the “analytic” operations of source criticism, and 39% on the “synthetic” operations of combining the source information into a historical account. Staying with the image of a historical account in which it consists of a collection of unified atomic facts, such atomic facts are understood to be arrived at by warranting them on the basis of appropriate sources, which are interpreted or “analysed” for that purpose. The facts so achieved are then synthesised into the whole account. It then seems plain that, ideally, the historian should be able to defend both each factual element of the account and also the factual synthesis itself.

Says Leon J. Goldstein of Langlois’ and Seignobos’ book, “the moment it came from the press it was not less than three centuries out of date. The book does reflect a pervasive, tough-minded positivism, and perhaps that suited the mood of historians who were concerned about the status of their discipline as a science”⁵⁶⁷ However, he says, the analytic and synthetic stages are a “myth”: “The two stages do not exist”⁵⁶⁸. The Warring States Project somewhat ambiguously say of Langlois’ and Seignobos’ book, “In the authors’ view, without the first half of that methodology, to which they themselves devote more than half of their space, there is no second half”.⁵⁶⁹ This might be a trivial remark, noting that one cannot synthesise facts which are not there, so that the second half of the book requires the first half; or it might be a Goldstein-following hint that the second half amounts to little more than the first half, so that whatever is sufficient to warrant the particular facts is thereby sufficient to warrant the account as a whole, so that there ought to be no separate stage of synthesis.

⁵⁶⁶ Langlois and Seignobos, *Introduction to the Study of History*.

⁵⁶⁷ Leon J. Goldstein, “Impediments to epistemology in the philosophy of history”, *History and Theory*, Beiheft 25, “Knowing & telling history: the Anglo-Saxon Debate”, 1986, pp. 82-100 at p. 98.

⁵⁶⁸ *Ibid.*

To make his point that the two stages of analysis and synthesis do not exist, Goldstein draws on Murray G. Murphey's presentation of different historical accounts of Bacon's Rebellion, "a well-known event in American history".⁵⁷⁰ Virginia in 1676 was governed by royally appointed Sir William Berkeley with a council and an elected assembly. Nathaniel Bacon had arrived in Virginia in 1674 and established a plantation. Bacon became the leader of a revolt which drove the governor to the outer shore of the Bay, where he retained military and naval control. Bacon's sudden death in 1676 demoralised the revolutionaries, and Berkeley was able to crush the revolt.⁵⁷¹ Historians have offered different interpretations of the event, and Murphey discusses Wertenbaker's, Washburn's and Bailyn's.

"Briefly", says Murphey, "Wertenbaker interprets the rebellion as a prefiguration of the American Revolution in which the 'common people' of Virginia rose in defense of their rights against the corrupt and despotic rule of the royal governor".⁵⁷² By contrast, "Washburn is fully aware of the economic difficulties which plagued Virginia and he regards these as contributing causes of the revolt. But he denies that Berkeley was either corrupt or oppressive or that he in any way violated the rights of Virginians. ... Washburn's book presents Berkeley as the hero and Bacon as the rogue".⁵⁷³ Bailyn, however, "remarks that Bacon's Rebellion is not a unique event, but one of a series of revolts which occurred at approximately the same time in Carolina, Virginia, Maryland, New York, and New England. Although Bailyn deals exclusively with Virginia, this comparative perspective is never absent from his mind and it determines the kind of explanation which he offers. Thus, for example, questions of Bacon's or Berkeley's personality are irrelevant to an explanation which views the revolt in this perspective".⁵⁷⁴ Says Murphey, all three interpretations "seek to provide a causal explanation of events. ... Yet these three interpretations of the same event are

⁵⁶⁹ The Warring States Project of the University of Massachusetts at Amherst, analysing Langlois' and Seignobos' work. <http://www.umass.edu/wsp/methodology/outline/langlois.html>; accessed 14th December 2006.

⁵⁷⁰ Murray G. Murphey, *Our Knowledge of the Historical Past*, Indianapolis: Bobbs-Merrill, 1973, p. 102.

⁵⁷¹ Summarised from Murray G. Murphey, *Op. cit.*, p. 103.

⁵⁷² *Ibid.*

⁵⁷³ *Op. cit.*, p. 105.

strikingly different: they differ in their use of narrative, the significance which they attribute to events, and even the facts which they discuss”.⁵⁷⁵

Goldstein comments, “There is no possibility of seeing this dispute as the result of different selections that two historians have made from a pre-given set of facts or fact-containing sentences”.⁵⁷⁶ Again, “The statements in both accounts cannot all be true. They are not mutually compatible or consistent. Bacon’s Rebellion on Wertenbaker’s account of it is not compossible in the historical world that contains Washburn’s account. There was clearly no common stock of statements from which the two historians made their different selections”.⁵⁷⁷ Goldstein is wholly against the idea that historiography is to be usefully, even partially, understood in terms of synthesising a selection of atomic statements: “The statements which make up the account are not atomic in any way that is the least bit interesting from the standpoint of epistemology. They are brought into being altogether in the same activity of historical constitution, and when the work is finished there are no leftover statements which turn out not to have been selected. What historians write they write because the force of the evidence and the ways in which historians think about evidence lead them to the conclusion at which they arrive – not atomic sentence after atomic sentence, but one total sense of what some part of the human past was like”.⁵⁷⁸ “The account hangs together because its statements are not atomic, logically independent in the manner favored by logicians, but intimately intertwined in their genesis and function”.⁵⁷⁹

There are a number of elements in Goldstein’s position. Let us first clarify the idea of an account being a “selection” of “atomic sentences”. The idea of “selection” is perhaps more easily understood if we reflect on issues of *distributive justice*. Think of the way in which marks “are distributed” among students after an examination, some with more, some with less. Think of the way in which property “is distributed”

⁵⁷⁴ *Op. cit.*, pp. 107-108.

⁵⁷⁵ *Op. cit.*, p. 109.

⁵⁷⁶ Leon J. Goldstein, “Impediments to epistemology in the philosophy of history”, p. 99.

⁵⁷⁷ *Ibid.*

⁵⁷⁸ *Op. cit.*, p. 94. Goldstein’s word “constitution” here derives from Husserl. See Luke O’Sullivan, “Leon Goldstein and the epistemology of historical knowing”, *History and Theory* 45, 2006, pp. 204-228 at p. 217.

⁵⁷⁹ Leon J. Goldstein, “Impediments to epistemology in the philosophy of history”, p. 99.

throughout society, some with more, some with less. Think of the way in which natural talents such as strength “are distributed” throughout the population, some with more, some with less. In all these cases, we may ask whether the distribution is *fair* or *just*. We may produce a theory to determine that, and may then set about attempting to change the existing distribution. But in all these cases we think of the “distribution” as an *arrangement* of marks, property or natural talents. The word “distribution” is ambiguous between its sense as a mere arrangement, layout, structure or ordering, and its sense as an *act of distributing*.

When we look at an existing distribution, where “distribution” is a noun which names an arrangement, we are *not* committed to holding that it is the outcome of an act of distributing. It may or may not be. It plainly is the outcome of an act of distributing in the case of the examiner, and it plainly is not in the case of the distribution of natural talents, and it is plainly controversial how the distribution of property comes about: perhaps as the unintended outcome of natural and inevitable economic processes, or perhaps as the intended outcome of blameworthy grasping Dickensian landlords, or on the basis of a host of other approaches. The distribution is in any event what it is, however it came about, and we may analyse its features and judge its merits, however it came about. Moreover, a distribution *may not have come about at all*: in principle, it may always have been the way that it is, or it may be an ideal or universal form. For example, a molecule will involve a particular distribution of atoms.

The same distinction applies here: a historical account is characteristically long, not short; it contains many factual sentences. Different historical accounts will contain a different selection (that is, distribution, arrangement, layout, structure or ordering) of factual sentences. (Again, they may contain more than this.) Moreover, different historical accounts about the same subject matter – say, Bacon’s Rebellion – will contain different selections of factual sentences. One has only to read the accounts to see this elementary point. However, once again, we are *not* committed to holding that the existing selection is the outcome of *an act of selecting*. As with a distribution, in principle it may be, or it may not be. Understanding a historical account as a *selection* or *set* of atomic sentences implies nothing about how it got to be like that. In fact, of course, historical accounts contingently *are* entirely the outcome of historiographical

activity and, in so far as that is so, *therefore*, since they are selections, they are contingently the outcome of historiographical acts of selection. However, nothing whatever is so far implied about what this historiographical process is.

Note a further point: when we compare one distribution with another, there is no commitment to holding that the *same things* are distributed or selected in different ways. Thus, however property is distributed in society, changing that distribution will typically alter what the distribution consists in, such as the kind or total amount of property which is held. That is clear in the case of governments changing the distribution of property by the tax system (something we assume can be done, but is not necessarily the case). We may, in a simple political example, seek to judge the comparative fairness of a communist versus a capitalist distribution. It would be an error to suppose that it is the same amount which is to be imagined as distributed in the different systems. An essential part of the political argument has to do with the relationships between the total amount being distributed, the particular things which get to be distributed, and the way in which they are distributed. Hence a traditional argument against imposing communist equality is that we would all have less.⁵⁸⁰

Assume that only a capitalist system gives us all the opportunity to have a private vehicle. Assume that, under a capitalist system, we have so many million private vehicles. On the traditional argument, if we had been communistically equal, far fewer of us would have a private vehicle. Does that mean that vast numbers of private vehicles are left undistributed in the communist system? Obviously not: the argument is that they would not have been produced in the first place. It is an analogous error which Goldstein makes when he objects, as we quoted earlier, “There was clearly no common stock of statements from which the two historians made their different selections”,⁵⁸¹ and “when the work is finished there are no leftover statements which turn out not to have been selected”.⁵⁸² The historians offered different selections, all the same. An essential part of our argument here has to do, analogously with the political example, with the relationships between the total selection of unified facts in a

⁵⁸⁰ Or, at least, less of what we want. We might have more straw to lie on, the claim might be.

⁵⁸¹ Leon J. Goldstein, “Impediments to epistemology in the philosophy of history”, p. 99.

⁵⁸² *Op. cit.*, p. 94.

historical account, the particular facts which are included, and the way in which they are included.

Says Goldstein, “The account hangs together because its statements are not atomic, logically independent in the manner favored by logicians, but intimately intertwined in their genesis and function”.⁵⁸³ We may well accept that the factual sentences in a historical account are “intimately intertwined in their genesis and function”, but we should note that that does not mean that they are not “atomic”. If we do favour the so-called logicians’ “manner”, we should note that the sentences in historical accounts are most clearly and typically logically independent of each other. This normally means that there are no *implications* between them, and this may be demonstrated by such typical historical sentences as the following two chosen from Emmanuel Le Roy Ladurie’s *Montaillou*: “Guillaume Benet...was hereticated by Guillaume Authié in the presence of his wife Guillemette Benet and his son Bernard, and of Guillaume and Raymond Belot and Bernard Clergue. The ceremony took place in the part of the house where the cattle slept and where the sick man’s bed had been brought, probably for the warmth”.⁵⁸⁴

The sentences here are broadly conceived to be “atomic” because each sentence could be true or false independently of the truth or falsity of the other(s). The facts expressed by these sentences are *contingently* connected. While it is a full stop which separates the sentences in this example, a full stop marks only the roughest of approximations to what the atomic sentences are. Thus the second sentence effectively says two things rather than one: first, that the ceremony took place in the part of the house where the cattle slept; second, that the ceremony took place in the part of the house where the sick man’s bed had been brought. It is simplistic operations of this kind which mark the analysis of the account into atomic sentences. Although simplistic, these sentences are philosophically important because they are crucial to the expression of historical truth.

⁵⁸³ *Op. cit.*, p. 99.

⁵⁸⁴ Emmanuel Le Roy Ladurie, *Montaillou: Cathars and Catholics in a French Village, 1294-1324*, trans. Barbara Bray, London: Scholar Press, 1978, p. 219.

We may observe that, in this process of identifying the atomic sentences involved, we have *abstracted* from the second sentence the “two things” it says rather than one. Barbara Bray, the translator of *Montaillou*, had a *choice* whether to express these “two things” in two sentences or in one, as indeed did Le Roy Ladurie in the original. There are philosophical issues here that we will not deal with. In attempting to make sense of mind, language, logic and reality, philosophers sometimes distinguish *statements* or *propositions* from the *sentences* which are used to make those statements or assert predicates of subjects, giving rise to issues such as how it is possible to identify sameness of meaning across different sentences, or how to preserve truth in language. However, neither we nor Goldstein regard these matters as being relevant to our present argument. While historians and translators clearly have a choice of *presentation* in historiographical contexts, failures of communication or translation in historiographical work are characteristically rare. The particular literary device used here, of using one sentence to say two factual things, is not a matter which affects what is being said. However, other literary devices may.

Nevertheless, it remains at this point undecided how far the truth of such sentences, considered atomistically, amounts to the entirety of the truth-telling function of a historical account. Again, regardless of this issue, and detailing a point already made, that these sentences are atomic implies nothing about how they were historiographically arrived at. In fact, they were, from Le Roy Ladurie’s appended references, sourced from two different entries in the Inquisition Register of Jacques Fournier.⁵⁸⁵ While these entries are themselves only contingently associated with each other, we need not deny that the resulting sentences in Le Roy Ladurie’s account, while also only contingently connected to each other and so logically distinct, may nevertheless be consistently understood with Goldstein as “intimately intertwined in their genesis and function”.

Nevertheless, and again, nothing whatever is so far implied about the nature of the historiographical process of producing a historical account. It may, with Langlois and Seignobos, be a two-stage process: “what are historical facts? How are they to be

grouped to make history?”⁵⁸⁶ and “The criticism of documents only yields isolated facts. In order to organise them into a body of science it is necessary to perform a series of synthetic operations”.⁵⁸⁷ Or Goldstein might be right in suggesting that it is a one-stage process, that the many facts involved “are brought into being altogether in the same activity of historical constitution...not atomic sentence after atomic sentence, but one total sense of what some part of the human past was like”.⁵⁸⁸ In principle, historiography may involve quite different processes from these, for nothing about the historiographical process has so far been implied. However, whatever the process, it is a process which has an outcome with two obvious and characteristic features: that the account is, centrally, a *selection of atomic sentences*. Philosophically, we can attend to particular atomic sentences independently of those other sentences with which they are contingently associated; or we can attend to the selection as a whole.

Let us now examine how far we can conceive the historiographical process of producing a selection of atomic sentences as characteristically involving two *stages* of historical method: first, the achievement of a range of atomic factual sentences; second, the act of selecting, from within that range, a particular set of those sentences, organised in some unified way. With Langlois and Seignobos, we may well think of these two stages as stages *in time*: the first stage is supposed to involve the interpretation or analysis of appropriate sources, yielding what we might think of as a range of finished factual products. A number of these factual products are then, later, supposed to be associated together, synthesised, in a second stage. But the time difference is not necessary. When Goldstein denies that there are two different stages or operations here, he will not be mollified by a response that both operations take place at the same time. Even if they do take place at the same time, the two-stage claim is that they are nevertheless separate operations and so only *contingently* connected. Hence, in principle, they *can* be separated in time. It is the view that there are two distinct operations here which Goldstein denies. This implies that, for

⁵⁸⁵ *Le Registre d'Inquisition de Jacques Fournier, évêque de Pamiers (1318-1325)*, ed. Jean Duvernoy, Toulouse, 1965, i. 474, 401.

⁵⁸⁶ Langlois and Seignobos, *Introduction to the Study of History*, p. 2.

⁵⁸⁷ *Op. cit.*, p. 211.

⁵⁸⁸ Leon J. Goldstein, “Impediments to epistemology in the philosophy of history”, p. 94. Goldstein’s word “constitution” here derives from Husserl. See Luke O’Sullivan, “Leon Goldstein and the epistemology of historical knowing”, p. 217.

Goldstein, whatever is involved in the synthesis of factual sentences is not some separate stage from the initial interpretation or analysis of the sources, but part of it: analysing the sources involves unifying some sources with others. It is not, for him, that the two operations occur at the same time, but that they are part of a single operation. Thus the synthesis of factual sentences is for Goldstein *necessarily*, and not *contingently*, connected to the analysis of the sources.

But, on the face of it, Goldstein cannot be correct here. This is because there is no difficulty at all in successfully imagining the temporal separation of the operations of source analysis and account synthesis. We do so when we think of Collingwood's "scissors-and-paste" historiography: "a patchwork history whose materials were drawn from 'authorities', that is, from the works of previous historians who had already written the histories of particular societies at particular times".⁵⁸⁹ It is obvious that "scissors-and-paste" historiography, being just that, can be seen as involving a two-stage process: first, the work of other historians who produce a range of historical facts; second, the "scissors-and-paste" historian selecting from within that range to produce a new account. There is no possibility here of the many facts selected in that new account being in Goldstein's sense "intimately intertwined in their genesis and function".

However, "scissors-and-paste" historiography is, we learnt from Collingwood, "bad" historiography, for the "good" historian should go direct to the primary sources. It is clear that, if with Goldstein we are to understand historiographical method as essentially a single-stage process, then we must understand "scissors-and-paste" historiography *not to be a proper kind of historiography at all*. "Scissors-and-paste" historiography is not then a counter-example to Goldstein's single-stage claim, for we may understand him to be claiming that *proper* historiography must involve in this context a single-stage operation. (*That* stage can take time, of course.) Goldstein, in theorising as he does, is then *setting a standard* for historiography, and it is certainly appropriate for the historical profession to stipulate precisely that standard in the historiographical rules which the profession seeks to uphold. A historical account, we

⁵⁸⁹ See our previous section "Our primary sources". R.G. Collingwood, *The Idea of History*, p. 33.

are then to understand, is one which is produced in a historiographical way, and that way is a one-stage way. The reader of what purports to be a historical account may, of course, not be able to tell how it is produced. Not just anything which *looks* like a historiographical account will then necessarily count as such.

Goldstein sets his historiographical standards here on the basis of a philosophy of historical knowledge expressed in his book *Historical Knowing*.⁵⁹⁰ Here he distinguishes between the “infrastructure” and the “superstructure” of history.⁵⁹¹ The *superstructure* is “those products of the historical enterprise which are typically consumed by those readers who are not themselves historians”.⁵⁹² Against this, it is not inappropriate to think that many works of particularly academic history are only ever read by other historians, and certainly the entire genre of historiographical reviewing largely consists in “consuming” the “products of the historical enterprise”. Nevertheless, we may agree for the sake of the argument that it is proper to identify this level, and if we do so then we can – and indeed Goldstein must – allow that a historical account can be rightly conceived, at this level, as a selection of atomic fact-expressing sentences, since we, *as readers of the account*, can understand it that way if we choose to “analyse” or break up the account into its constituent parts. Equally, the range of facts, so understood as atomic, appear to the reader as “synthesised” into the coherent whole account.

Earlier we observed that the historiographical account characteristically presents facts in a unified way, and, in so far as that unification is factually appropriate, it is, as a whole, in consequence factual itself, and we remarked also on the complexity of that “in consequence”. However, this “superstructure” level is for Goldstein of no epistemological interest. When we think of the whole account as factual, as opposed to considering as factual the constituent sentences considered severally, we are to suppose that nothing of epistemological significance has been added. We are to see this level as merely literary, plainly conceiving it as a matter of superficial presentation.

⁵⁹⁰ Leon J. Goldstein, *Historical Knowing*, Austin: University of Texas Press, 1976.

⁵⁹¹ *Op. cit.*, pp. 140ff. Goldstein’s epistemology of history underwent development, with an early realism replaced by a later antirealism. See Luke O’Sullivan, “Leon Goldstein and the epistemology of historical knowing”.

⁵⁹² Leon J. Goldstein, “Impediments to epistemology in the philosophy of history”, p. 82.

Recently, Aviezer Tucker accepts this hoary position, which long predates the Hempel of 1942, saying “it is impossible to reconstruct the epistemology of historiography from studying its superstructure... Since this book pays little attention to the superstructure of historiography, it pays even less attention to the debate whether it has the structure of a narrative or not”.⁵⁹³ The supposition that literary presentation is epistemologically irrelevant has often angered historians. J.H. Hexter, for example, thought that certain literary elements, which he called the “rhetoric” of history,⁵⁹⁴ were themselves an essential way of expressing historical knowledge. The rhetorical side of historical writing, which may be seen as the ordering or “emplotment”⁵⁹⁵ of sentences in a narrative, has an explanatory and truth-telling use. Only a “fancy view of historical truth,” Morton White once claimed without argument,⁵⁹⁶ would permit such a thing.

We recognise that we cannot be sure from the output writing alone, or “superstructure”, that a particular account is a *historiographical* account, since it might not have been produced in the approved historiographical way. That is, the very same selection of sentences could have been achieved in a proper historiographical way, or in, say, a “scissors-and paste” way. Even history and fiction might not, from the reader’s perspective, be distinguishable, and Goldstein rightly says, “It is not difficult to imagine that entire sentences – perhaps even paragraphs – identical in every linguistic respect might appear both in some historical novel and in some proper work of history”.⁵⁹⁷ But this identity is only *apparent* from the reader’s point of view. As Goldstein says of the historiographical account and the historical novel, “their statuses would be different”.⁵⁹⁸ His point is that the genesis of one is entirely different from the genesis of the other, but he importantly adds that the point he wants to make does *not* depend on the difference between fact and fiction. Two identical *factual* sentences could be warranted in different ways, one for example by

⁵⁹³ Aviezer Tucker, *Our Knowledge of the Past*, p. 7.

⁵⁹⁴ J. H. Hexter, “The Rhetoric of History”.

⁵⁹⁵ This is Hayden V. White’s word: see *Metahistory*.

⁵⁹⁶ M. G. White, letter, *New York Review of Books*, 23 March 1967, p. 28, attempting to clarify his position in *Foundations of Historical Knowledge*, New York: Harper and Row, 1965.

⁵⁹⁷ Leon J. Goldstein, *Historical Knowing*, Austin: University of Texas Press, 1976, p. xviii, a reference I owe to Luke O’Sullivan, “Leon Goldstein and the epistemology of historical knowing”, p. 210.

the observation of witnesses and the other by historiographical means. It is, in the same way, the *epistemic status* of the sentences which makes them different.

Goldstein uses this argument as a reason to object to philosophical attention to the superstructure, and Tucker does the same: “it is impossible to reconstruct the epistemology of historiography from studying its superstructure”.⁵⁹⁹ However, this argument is too strong, for if the superstructure is epistemologically irrelevant then it is so in its entirety. That entirety consists of a synthesis of atomic fact-asserting sentences. It is mere unfounded dogma to insist that the individual fact-asserting sentences count epistemologically but that the synthesis does not. If it is possible for the atomic sentences in the superstructure to have appropriately sound epistemic status and epistemological relevance, then – given the absence of any other reasons one way or the other at this point – so can the synthesis. It is not proper to conclude that the superstructural presentation of sentences is irrelevant to the kind of epistemic status which it is possible for those sentences to have, merely because the epistemic status of those sentences is not apparent from the superstructural presentation *alone*.⁶⁰⁰ The individual sentences have an appropriate epistemic status despite that fact. The same argument displays the possibility of the synthesis itself having epistemic status and epistemological relevance.

Goldstein continues by distinguishing the superstructure from the infrastructure as follows: the *infrastructure* is “that phase of historical research during which historians apply the techniques and methods of their discipline, and by thinking historically – whatever that proves to mean – about historical evidence come to some conclusion about what it is most reasonable to believe took place in the historical past”.⁶⁰¹ With this distinction clarified, we may now return to the question whether Goldstein is correct to think of historical account construction as essentially a single-stage operation. Is the account synthesis – whatever it is that unifies the facts in the

⁵⁹⁸ Leon J. Goldstein, *Historical Knowing*, p. xviii.

⁵⁹⁹ Aviezer Tucker, *Our Knowledge of the Past*, p. 7.

⁶⁰⁰ In any event, the superstructural presentation of a work of historiography does typically display material relevant to its epistemic status, namely, the presence of footnotes and the like.

⁶⁰¹ Leon J. Goldstein, “Impediments to epistemology in the philosophy of history”, p. 82.

outcome presentation – necessarily a part of, an essential outcome of, the source analysis, or can it properly be a separate stage?

Either the account synthesis is essentially involved in the source analysis or it is not. If whatever it is that unifies the facts in the final superstructural synthesis is *not* essentially involved in the source analysis, then, against Goldstein, there can indeed be two stages here: first, the “infrastructure” stage, analysing sources to achieve factual outcomes; second, the separate “superstructure” stage of associating facts in some unifying synthesis which reports the factual knowledge already achieved. However, Goldstein cannot consistently deny this two-stage process, for, quite apart from saying at one point “There are, of course, interesting things to be done with a past already emerged. One could explain it; or one could interpret it”,⁶⁰² he thinks that philosophical attention to the unification of facts in the *expression* of historical knowledge is a typical philosophical error: “Philosophical writings on history tend almost always to take their point of departure from something in the superstructure”.⁶⁰³ “The actual character of the constitution of the historical past is lost sight of when one’s point of departure is the finished account which may be analyzed ...as a collection of atomic statements. What is lost sight of is precisely that part of historians’ work in which knowledge is brought into being, where considerations of truth and falsity are at issue, and when issues of epistemological interest are at stake. ...epistemological questions come into play only where knowledge is being *acquired*, not *reported*”.⁶⁰⁴

But Goldstein’s conceptual distinctions between “infrastructure” and “superstructure”, between “acquiring” knowledge and “reporting” it, map on the distinction between source analysis and account synthesis. Goldstein here is thus committed to understanding the historiographical situation as involving two conceptually distinct operations in order to say as he does that one operation is *epistemologically fundamental* and the other operation is *epistemologically superficial*. They cannot be the *same* operation, having as they do contradictory qualities. The upshot is that

⁶⁰² *Op. cit.*, p. 83.

⁶⁰³ *Ibid.*

⁶⁰⁴ Our emphasis. *Op. cit.*, pp. 94-95.

Goldstein cannot consistently claim both that there is (a) only one stage in account construction and also that (b) the second stage is epistemologically superficial. For a moment, it seems that he is best understood as intending the second (b) of these two claims, holding that, while there are indeed two stages, there is only one *epistemologically important* stage, that is, the infrastructure.

But, on the other hand, if the account synthesis – that which unifies the facts – is necessarily involved in the source analysis, then we may indeed accept Goldstein’s first claim (a) that there are not two stages here. However, in order to understand what is going on in source analysis, we then need an understanding of the historiographical unification involved at that source analysis level, a unification which is, *ex hypothesi*, apparent to us in the finished account product precisely because, as we are now taking Goldstein to be arguing in the first claim (a), this is *not* a different stage of the historiographical process: “The account hangs together because its statements are not atomic, logically independent in the manner favored by logicians, but intimately intertwined in their genesis and function”.⁶⁰⁵ Despite the conclusion of our previous paragraph, Goldstein clearly means this. Consequently, it *cannot be* a philosophical error to attend to what Goldstein calls the “superstructure” precisely because that is, on his own say-so (a), not to be separated from the epistemologically significant source analysis.

In fact, the more there is one historiographical process rather than two, then the more essential the connection between account synthesis and source analysis; and the more essential the connection between account synthesis and source analysis, the more relevant is philosophical attention to the so-called “superstructure” as being essentially connected to the so-called “infrastructure”, that is, the more relevant is attention to the *expression* of historical knowledge to understanding the constitution of the historical past. Whether the synthesising element in this single stage is *epistemologically* insignificant (for it is certainly *historiographically* significant), essentially connected to source analysis in account construction as it is, will have to depend on quite different arguments which will inevitably involve attention to the “superstructure”, to the

⁶⁰⁵ *Op. cit.*, p. 99.

finished historical account. On this single stage approach, there is now some justification (which is not apparent on Goldstein's view) for some historians to review as they do the "superstructure" of other historians' works, while it is in any event not true that the superstructure is "typically consumed by those readers who are not themselves historians".⁶⁰⁶

⁶⁰⁶ *Op. cit.*, p. 82.

A fancy view of truth

Says Goldstein, “It may be argued that...attention to the way in which historians select in order to produce coherent accounts of some swath of the human past is precisely to pay attention to the way in which new knowledge is acquired. And that would make attention to the way in which narratives are formed – something which belongs to the superstructure of historical work – part of the epistemology of history, which seems to be contrary to what I have been saying”.⁶⁰⁷ That is indeed what we are here arguing needs to be considered. However, Goldstein does not take this 1986 speculation (nor the earlier work of others⁶⁰⁸) seriously, following it immediately with the remark “In due course, I shall try to suggest that historians’ selection is largely a myth”, a position which we have now examined and discarded. In the previous section we showed that historians organise reality in terms which go beyond atomic sentences:

historiographical truth involves a unified synthesis of atomic facts. Historical reality is counted as such in terms of whole accounts. At this point our own argument recognises that if it is possible for the atomic factual sentences in the historiographical superstructure to have appropriately sound cognitive support and so be epistemologically relevant, then these things are also possible for the synthesis. Goldstein has given us no good reason to deny this.

We have seen that, in historiography, the facts involved in factual judgement are not characteristically seen only in terms of short or atomic sentences, but in terms of selections of such sentences. We need next to examine examples of selections of factual sentences to observe the main cognitive features which a selection might have. Against the background of these examples, we will be able better to understand historiographical synthesis. In particular, we understand that synthesis as being at what Goldstein called the “superstructure” level, and so, at this point, we are neutral as to how that synthesis came about. There is thus no *a priori* view as to the process by which this synthesis was achieved. There are as many syntheses of factual sentences as there are works of historiography, and only very abstracted examples will suit the present stage of the argument. The examples we will use are as follows, which refer

⁶⁰⁷ *Op. cit.*, p. 83.

to what might be called, with apologies to Murphey, a well-known event in English history:

(A) “Joyce was born in Ireland, according to the application for a British passport made by him in 1933. He became a Fascist, for whom Sir Oswald Mosley, the ‘Bleeder’, was too moderate. In August 1939, a few days before the outbreak of hostilities, he went to Germany and offered his services to the German ministry of propaganda. He was the Germans’ principal English broadcaster (known as ‘Lord Haw-Haw’ from his manner of speaking). He was executed as a traitor”.

(B) “Joyce was born in New York. His father was a naturalized American citizen. Joyce never made a formal request for British nationality, though he spent most of his life in England and was regarded as patriotic. In September 1940 he became a naturalized German. Joyce attracted to himself the mythical repute of Lord Haw-Haw. These legends were the manufacture of war-nerves. He was executed as a traitor”.⁶⁰⁹

These are brief, but that is sufficient for the argument. It is not apparent from the superficial text whether these short accounts are fictional or historiographical, but that is as it should be. What does matter is that they *purport to be factually true*. How they were constructed is at this point irrelevant, although they may be regarded as “scissors-and-paste” examples, and were constructed from sentences selected from historians’ works. The issue which we address here is what historians characteristically mean when they think of an account being true. There is no doubt that a part of this is that the individual sentences of the account should be severally

⁶⁰⁸ See Luke O’Sullivan, “Leon Goldstein and the epistemology of historical knowing”, pp. 218-219.

⁶⁰⁹ The sources are A.J.P. Taylor’s “pro-Joyce” *English History 1914-1945*, Harmondsworth: Penguin, 1965, and *Chambers’ Encyclopaedia*. I developed this material for my 1973 Cambridge PhD thesis, supervised by Bryce Gallie and Mary Hesse. The construction is explained in my “Objectivity and truth in History”, *Inquiry* 17, 1974, pp. 373-397 (reprinted in *History and Theory: Contemporary Readings*, eds. Brian Fay, Philip Pomper and Richard T. Vann, Oxford: Blackwell, 1998, pp. 320-341, where I tried to analyse the rational standards which would be required to make accounts “objective” in their selection. Further lengthy analysis was given in my *The Expression of Historical Knowledge*, Edinburgh University Press/Columbia University Press, 1982, which used

true, but that is not enough. An exact analysis shows that every sentence in both of the two accounts, (A) and (B) above, is indeed individually true, or (for those with realist assumptions) at least accepted by historians as historiographically warranted.⁶¹⁰ There is, of course, highly tendentious selection – why leave out of an account of Joyce reference to his having been Lord Haw-Haw, and include only a reference to false stories about Lord Haw-Haw (and there were some)? – but it is precisely this need for the *selection* to be truthful which we are addressing here.

Given that “whole accounts” like these are the characteristic way for historians to count reality, then the arguments in our previous sections beginning with “Postmodernism” apply as much to “whole accounts” as they do to atomic sentences. The initial postmodern suggestion is then that historiographical accounts can configure reality in many different ways, analogous to the claim that we can believe what we like. Following Quine while including his assumption of consistency, we can only vary our configuration of historical reality if we make enough adjustments elsewhere in our system (which is no longer to be understood as a system merely of atomically understood beliefs, but, while including those, is a system including whole accounts, including also all our evidence for those accounts). Adjustments are then required when we have to decide between some historiographical accounts and other historiographical accounts when they conflict *at the whole account level*.

The relevant point for us now is that the (A) and (B) accounts are indeed, in some way still to be understood, *inconsistent* with each other. Briefly, we might wish to say that (A) is anti-Joyce and (B) is pro-Joyce. However, while “anti-Joyce” and “pro-Joyce” are adequate brief characterisations, they suggest that the conflict between (A) and (B) has essentially to do with our or the historian’s approval of Joyce or his situation. We might well think that the accounts should not be politically tendentious or “trouble-making”, a comment often made about A.J.P. Taylor, one source of the character of

Michael Dummett’s understanding of antirealism. This missed what is here provided, a developed understanding of the pragmatic conditions involved.

⁶¹⁰ The justification includes analysing the “conversational implicatures” involved. Readers who wish for detail are directed to *The Expression of Historical Knowledge*, Edinburgh University Press/Columbia University Press, 1982.

the (B) account above.⁶¹¹ However, to say this would be to hint too quickly at the following possible epistemological conclusion: that *factual* selection in historiography is *evaluative*, or even – what does *not* follow from that – that historiographical selection cannot be factual just because it is evaluative. The essential step at this point of our argument is to recognise that the selection of sentences makes, as a whole, a factual claim which is more than the sum of its parts, and we can see this just in so far as we recognise that it cannot be the case that both (A) and (B) succeed in telling us what *the facts* – not just “some facts” – are.

When we read (A), it makes more obvious *factual* sense to us, than does reading (B), how it came about that Joyce was executed as a traitor. That is so whether or not we approve of Joyce, or approve of his being executed. Hence, despite their consistency at the level of their constituent atomic sentences considered severally, the two Joyce accounts are, each conceived as a whole, factually inconsistent with each other. They purport to count the reality in question in opposed ways. These examples display the difference between truth at the atomic sentence level and at the whole account level, and also display whole account level inconsistency as distinct from atomic sentence level inconsistency. To recognise the inconsistency is to recognise that the conflict between the accounts needs to be resolved: it is not possible to accept both accounts as *the* truth about the Joyce affair.

Earlier we noted Goldstein’s view of Wertenbaker’s and Washburn’s accounts of the Bacon rebellion as presented by Murphey: “The statements in both accounts cannot all be true. They are not mutually compatible or consistent. Bacon’s Rebellion on Wertenbaker’s account of it is not compossible in the historical world that contains Washburn’s account”.⁶¹² We can now see the ambiguity in this position. We may well accept that “Bacon’s Rebellion on Wertenbaker’s account of it is not compossible in the historical world that contains Washburn’s account”, but it does not follow that “The statements in both accounts cannot all be true. They are not mutually compatible or consistent”, *considered severally*, which is how Goldstein conceives the

⁶¹¹ See Kathleen Burk, *Troublemaker: The Life and History of A.J.P. Taylor*, New Haven: Yale University Press, 2000.

⁶¹² Leon J. Goldstein, “Impediments to epistemology in the philosophy of history”, p. 99.

matter. Of course they *can* all be true, considered severally, even if there is inconsistency at the “whole account” level; this is precisely what occurs in the Joyce accounts.

Whether in the Bacon rebellion case they *are* all true, considered severally, is a different question: with respect to Wertenbaker, Murphey says “Bacon is portrayed as a hero, Berkeley as a rogue”,⁶¹³ whereas “Washburn’s book presents Berkeley as the hero and Bacon as the rogue”.⁶¹⁴ Plainly a sentence in Wertenbaker saying “Bacon is a hero” and a sentence in Washburn saying “Bacon is not a hero” would yield inconsistency at the level of the atomic sentence. But that is not the situation. As Murphey presents Washburn, “Bacon emerges as a hot-tempered, avaricious man chiefly interested in destroying the Indians so that he could claim their land”.⁶¹⁵ That Bacon is a rogue according to Washburn is Murphey’s *summary* of the *whole* of the relevant passages. The *emergence* of the character of Bacon takes time, and takes many sentences to narrate. We may understand Murphey to be correct in his summary of Washburn at this point, without supposing that the sentence here quoted from Murphey itself appeared in Washburn.

Similarly, Murphey said of Wertenbaker’s, Washburn’s and Bailyn’s accounts that all three interpretations “seek to provide a causal explanation of events. ... Yet these three interpretations of the same event are strikingly different: they differ in their use of narrative, the significance which they attribute to events, and even the facts which they discuss”.⁶¹⁶ It is possible that these conflicts lie in atomic sentences in the different accounts, such as “the cause is *p*” and “the cause is not *p*, but *q*”, but it is at least as plausible to suppose that the factual descriptions of “causes” in the three accounts are a matter of *narration*, which is conceived as a “whole account” notion. Nothing in Goldstein’s presentation of Murphey supports what is effectively Goldstein’s contention, that consistency and inconsistency are only to be assessed at the level of the atomic sentence.

⁶¹³ Murray G. Murphey, *Our Knowledge of the Historical Past*, p. 104.

⁶¹⁴ *Op. cit.*, p. 105.

⁶¹⁵ *Op. cit.*, pp. 104-105.

Notice that, assuming for a moment a categorical distinction between fact and value,⁶¹⁷ even if there were independent reason to hold that the selections in accounts (A) and (B) were as a whole evaluative and – it might then be thought – *therefore* not factual, the individual sentences in those two accounts are (given the categorical distinction) in any event factual and not evaluative. None of the individual sentences in the (A) and (B) accounts are essentially pro-Joyce or anti-Joyce sentences. The relevant holistic character of each account is thus still more than the sum of its parts. The familiar suggestion that *all* “facts” (thus whether expressed in atomic sentences or in whole accounts) are “evaluative” is not relevant to our argument here about the factual centrality of selection in historiographical accounts. Evaluative issues have, as earlier argued,⁶¹⁸ to do with *choice*, and we will address that shortly.

Part of what we characteristically want of historiographical writing is then *the truth* about something, not just “some truths”. A *true* story would make it clear to us as readers whether Joyce was justly or unjustly executed, meaning by that not some moral judgement on our part or on the part of the historian, but rather whether Joyce was proved or otherwise to be a traitor according to proper legal process at the time. There is simply not enough material in the (A) and (B) accounts to enable us to grasp the historical *reality* here. The accounts fail to give *the truth* about the matter. And yet, neither account is false in the following sense: every sentence in each can be examined for truth, and will pass that atomic test. It is in this way that we think of the whole being more than the sum of its parts. This is a familiar and important idea. Thus earlier,⁶¹⁹ we noted an ambiguity between historians conceived as individuals and historians conceived as a group. We observed that, in order to understand the character of historiography *as a discipline*, it would not be enough merely to list the different views of different historians. We needed views sufficiently shared to amount to a *consensus* about the rules specifying the character of the discipline, which would enable historians to count themselves and others as contributors to the discipline. This is an analogy for our present situation: we need to recognise that historians think of the expression of historiographical truth not as a mere list of truths but as a “group” of

⁶¹⁶ *Op. cit.*, p. 109.

⁶¹⁷ See the section “Justification in the second-order context: Kuhn”.

⁶¹⁸ See the section “Postmodernism”.

truths. The historical account typically has some unifying feature on the basis of which there is some historiographically shared understanding – which may on occasion be somewhat indeterminate – about which facts are relevant and which are irrelevant.⁶²⁰

The selection of sentences understood in terms of group-truth rather than individual- or atomic sentence-truth is factual just because *this is how historians count historical reality*. History is what historians count it to be, and this is how they do it. Hence, when historians write historiography they write history. In the first section “Respect for historiography” we distinguished “historiography” as, among other things, what historians write from “history”, which is what they write about. We avoided at that point committing ourselves to a realist interpretation of “history”. Now, presupposing the antirealist pragmatic approach, we may think our distinction no longer appropriate. Many historians will, in any event, find artificial our continual use of “historiography” rather than “history”, and historians commonly avoid the use of the word “historiography” except in certain specialist contexts. Usually, they use the word “history” to reflect both what historians write and what they write about, thus displaying to philosophical *cognoscenti* their antirealist presupposition. That “realism” which is supposedly characteristic of historians’ theoretical attitudes is belied by their language.

Counting reality in terms of group-truth rather than individual- or atomic sentence-truth is essential to our factual understanding not just in historiography but in our everyday lives.⁶²¹ Thus the point is important not just for the understanding of historiography but for more general philosophy. “Whole account” truth is *not truth-functional*. If we imagine a historical account as a selection or set of factual sentences p, q, r, \dots , each of which is true, then we would usually in philosophy regard that as entailing that the whole account is true. In effect, we are treating the commas

⁶¹⁹ In the section “Our primary sources”.

⁶²⁰ “Any kind of narrative, assuming there were kinds of narratives, would require and presuppose criteria of relevance in accordance with which things would be included and excluded”, Arthur C. Danto, *Analytical Philosophy of History*, , p. 140.

⁶²¹ This is the point at which it is appropriate to reflect on the ways different cultures use stories, dance and music to structure their lives.

between p , q and r as if they replaced the word “and”, so that the whole account is a *conjunction*. And it is essential to the nature of a conjunction that it has the entailment mentioned. If the truth-functionality of the whole *fails* then we will seek some other connective or operation which governs the selection. It is elementary to do this, for example with the following three sentences:

All crows are black

Fred is a crow

Fred is black.

These are not arbitrarily conjoined sentences. Suppressed is the expected word “therefore” before the third of them. Given this, the selection may be taken to be a deductive argument. There is an “analogue” for “whole account truth” here, namely the *validity* of the argument, and validity is not truth-functional, because an argument can be valid whether or not all of its subordinate sentences are true. Validity is, however, only a poor analogue for “whole account truth” because arguments have as a whole only a *hypothetical* status: an argument is valid just when *if* the premises are true then the conclusion must be true. Arguments are not a way of counting reality. *But historical accounts are.* There is nothing logically odd in suggesting for historiographical and indeed other contexts that the whole is more than the sum of its parts; on the contrary, suggesting this is merely drawing on the point that conjunction is not the only linguistic connective, and making the additional point that conjunction is not the only *cognitively relevant* linguistic connective. As we noted earlier, the historiographical account characteristically presents facts in a unified way, and, in so far as it does this, then it is, as a whole, in consequence factual itself. That “in consequence”, we remarked, is complex, and that this is so should now be apparent. “Whole account truth” – *truth* – is not based on mere conjunction, not since Herodotus, and no doubt long before.⁶²² We should not assume that atomistic conceptions of truth, whether developed by Aristotle for his purposes or by Alfred Tarski for his, should alone be used. The development of modern science has already privileged atomistic conceptions of truth quite enough.⁶²³

⁶²² Truth needs a history. A beginning is Felipe Fernández-Armesto, *Truth: A History*, London: Black Swan, 1998.

⁶²³ “Whole-account truth” is not held here to be an *emergent* concept.

It is now clear that we should understand a historical account as characteristically making a truth claim *as a whole*. That individual sentences within it can be conceived as making what might be called atomic truth claims may well be, contingently, a part of that. But, in a parallel way to the approach now explained, we also should not assume that the occasional failure to assert truth at the atomic level should lead to the whole account being counted false, as would occur if it were a conjunction. That was not true even of Herodotus: Cicero had no difficulty describing him both as the “father of history” and as a “notorious liar”.⁶²⁴ Indeed, it is not even clear that historiographical truth is determinate at the atomic level: to repeat⁶²⁵ Bradley’s holist empiricist point, “It is a very common and most ruinous superstition to suppose that analysis is no alteration, and that, whenever we distinguish, we have at once to do with divisible existence. It is an immense assumption to conclude, when a fact comes to us as a whole, that some parts of it may exist without any sort of regard for the rest”.⁶²⁶

⁶²⁴ “The place of Herodotus in the history of historiography”, A.D. Momigliano, *Studies in Historiography*, pp. 127-f128.

⁶²⁵ In the section “Commonsense and experience: Hume”

⁶²⁶ F.H. Bradley, *The Principles of Logic*, vol. I, , p. 95.

Holistic choice

Can we, at the whole account level, believe what we like about reality? As we have seen,⁶²⁷ the postmodern position suggests freedom of choice in the context of what to believe about reality, and hence it is contingent what people understand reality to be. The claim is that reality is “multiply configurable”. Adopting an empiricist approach, we first analysed postmodernism in terms of Quine’s view that our reality-expressing beliefs are holistically related to each other.⁶²⁸ Quine, we saw, claimed three things: first, that we can hold true any statement; second, that, if we are to hold true any statement, then we must make sufficient adjustments for the purpose; third, that we can make sufficient adjustments for the purpose. We noted that, on this view, adjustments were required when some beliefs conflicted with other beliefs, and we had to decide between them.⁶²⁹ We saw that such conflict had to be resolved in a holistic and not atomistic manner, which meant that any required adjustments to our existing belief system had to be completed so as to minimise recalcitrant experiences by comparison with it. Quine’s third point was then wrong because, contingently, in many cases we cannot meet the cost of adjustment. Many factual beliefs are thus pragmatically inescapable and so in practice indubitable.

However, we saw that both Quine’s argument and our response to it presupposed the availability of standards of consistency and so did not allow for postmodernism in a form which denied that.⁶³⁰ We examined this denial, and argued that, from the first-person singular point of view, the judgement of consistency or inconsistency is pragmatically to be understood in terms of our ability or inability to believe things we contingently judge to be complementary or conflicting as ways of counting reality. Shared standards of consistency arise in the same way when we, contingently, share with others what we recognise as conflicting and, contingently, also share the recognition that the conflict needs to be resolved in so far as we seek to share our reality. This pragmatic approach in which we share an understanding of the need to avoid conflict in how to count reality gives us sufficient shared standards of

⁶²⁷ In the section “Postmodernism”.

⁶²⁸ In the section “Quine as postmodernist”.

⁶²⁹ In the section “The costs of belief”.

⁶³⁰ In the section “Quine not postmodern enough”.

consistency to drive our earlier conclusion: that the requirement for a holistic resolution of conflict makes many factual beliefs pragmatically indubitable.

In the section “Narrative truth” we showed that historians organised reality in terms which go beyond atomic sentences: the truth involves a unified synthesis of atomic facts. Historical reality is counted as such in terms of whole accounts. Given that “whole accounts” are the characteristic way for historians to count reality, then the arguments in the earlier sections “Postmodernism”, “Quine as postmodernist”, “The costs of belief” and “Quine not postmodern enough” apply as much to “whole accounts” as they do to atomic sentences. This earlier argument applies to decisions about what to count as true, and so applies whether our expression of that counting, that is, our expression of what we claim to be historical knowledge, is conceived in terms of atomistic factual beliefs or in terms of whole account selections. The initial postmodern suggestion is then that historiographical accounts can configure reality in as many different ways as we like, analogous to the claim that we can believe what we like.

Repeating our earlier point, and assuming standards of consistency, we can only reconfigure historical reality if we make sufficient adjustments. Adjustments are required when we see conflict between some historiographical accounts and others at the whole account level. The Joyce examples, in the section “A fancy view of truth”, displayed the difference between truth at the atomic sentence level and at the whole account level, and also displayed whole account level inconsistency as distinct from atomic sentence level inconsistency. To recognise the inconsistency was to recognise that the conflict between the accounts needed to be resolved: it was not possible to accept both accounts as the truth about the Joyce affair.

This historiographical resolution would again have to be in a holistic and not atomistic manner. Thus, in so far as historiographical research developed a better account than the (A) and (B) accounts, then such an account would be “better” because it would “minimise recalcitrant experiences” by comparison with them. In other words, it would square better with our overall system of historical knowledge, as that is expressed in other more established historiographical accounts and factual assertions

including our beliefs about what the evidence is. With respect to the Joyce affair, historiographical methods are in fact available to resolve the conflict between the (A) and (B) accounts and develop an improved account. Contingently, while such an improvement is easily available in respect of the Joyce accounts (tendentious scissors-and-paste as they are), the situation is different with respect to more established accounts, which is, of course, why we think of them as “established”. Here alternative accounts which meet the necessary holistic test would characteristically not in practice be available, so that attempts at revising received historiography about many historical states of affairs (if not the Joyce affair itself) would fail. Many historical accounts would thus be established in their truth-telling status: they would be pragmatically indubitable in factual terms. A proper understanding of the pragmatics of holistic empiricism would then reinforce the broad reliability of our ordinary understanding of historical reality, even though that understanding is characteristically expressed at the whole account level.

However, given the earlier arguments with respect to atomic sentences, we need to question once again whether it is appropriate to use standards of consistency at the whole account level, since postmodernism has a form which denies such standards. Paralleling the earlier argument, we saw that to recognise the inconsistency was to recognise that it was not possible to accept both the (A) and (B) accounts as the truth about the Joyce affair. Again following the earlier argument, such understanding of inconsistency is readily made intelligible from the first-person singular point of view, for the judgement of inconsistency is pragmatically to be understood in terms of a person’s inability to accept two things that that person contingently judges to be conflicting ways of counting reality. This solution does not work where different people believe conflicting things, and here we recognised that being able to see ourselves as contesting with each other arose precisely because and in so far as we seek to share our reality.

Our shared language, operating successfully as it does at the short sentence level, involves this contingent commitment to a shared consistent reality. In parallel to the earlier argument, shared standards of consistency arise which are pragmatically suitable for our purpose when we, contingently, share with others the following: some view

which counts reality in such a way that we can share some part of it as our subject matter; a desire to share with others a view as to how that element of what we count as reality is to be fully expressed, that is, a desire fully to share the reality in question; a recognition of what we count as conflicting ways of counting or conceptualising that shared reality; and a recognition that this conflict needs to be resolved, just because and in so far as we wish to share the reality in question. When we thus share standards of consistency, the requirement for a holistic resolution of conflict between whole accounts again makes many historiographical accounts pragmatically indubitable.

A consistency-denying postmodernist, seeking to overturn the *establishment* of historiographical accounts, for example a state-based “master narrative”, is already committed to some of these elements which give rise to standards of consistency. Thus there is little of interest in the postmodernist merely claiming that there are many ways of “counting reality”. Indeed there are: an infinite number. An account of the Joyce affair is one way, the latest theory in physics is another, and “grass is green” is a third. We may count as true the claim that my computer keyboard is 15 centimetres from my computer screen, and also as true the claim that it is 16 centimetres from a point one centimetre the other side of my screen, and so on indefinitely. These examples are uninteresting because they are *consistent* with each other: there are indefinitely many atomic truths, and indeed indefinitely many historiographical accounts. Postmodernism gains no purchase from one historian writing on Napoleon, and another on Charlemagne.

The postmodern position may seem, at first, to involve saying that there is no *right* way of counting reality, or no *one* right way of doing so. Expressing the position in this way, however, suggests a correspondence theory, and this would involve committing the postmodernist to a realist approach. Instead, here we continue with our explicit assumption that the approach is antirealist rather than realist, and moreover that we are not in a position to defeat an antirealist approach. Reality is essentially reality for us: it is what *we*, contingently, count it to be. We cannot be “incorrect” in counting reality, in any realist sense of that word. We can only have a failure of *shared* counting. That there are indefinitely many atomic truths and historiographical

accounts, all consistent with each other, is not the postmodern position. Can the postmodernist, as a way of insisting on some failure of shared counting, merely assert that there just is no shared counting? This would amount to saying that *we* do not count reality at all, and that, in effect, only a succession of “I”s do so. But this offends against the antirealist presupposition of the argument: reality is what *we* count it to be, and there is a minimal sharedness in that alone: in any given case, there has to be some shared prefiguration of that subject matter which is, supposedly, configurable in so many ways. The postmodernist, in accepting this antirealist presupposition, has to think in terms of factual *conflicts* being irresolvable or, if solvable, arbitrary in the plurality of their resolution. The postmodernist thus relies on, that is, *needs* the possibility of conflict or inconsistency in the way we count reality, in order to express the position. But once standards for judging consistency and inconsistency are available, the holistic empiricist position overcomes this approach, we have argued.

Not all historiographical accounts, or for that matter individual facts, are of course well established. Historians often disagree, and often it is the lack of complete evidence which leads to the indeterminacy between conflicting accounts. Assuming that, while it is limited, all the available evidence is taken into consideration by both sides of such a disagreement, and recognising that the evidence, too, is part of the holistic web, then the approaches of both sides may in such circumstances be understood to square equally well with the rest of our historical understanding. The disagreement nevertheless arises, and persists in the absence of the evidence required for resolution. It arises just because the historians concerned share with each other the view that they are each seeking, in their different ways, to describe or count the same reality, and they recognise that they conflict in doing this, and they seek – if in vain – for resolution of that conflict. They share the appropriate standards of consistency. Often, they share a view as to what sort of evidence, if available, would resolve their disagreement. With regard to the use of language to share reality at the atomic sentence level, it is characteristic for all of us, historians included, to share these things.

For much (and we can disagree how much) of historiography, there are many ways of configuring historical reality at the whole account level, and many historiographical disagreements. However, not all such disagreements are to be understood in the

above way. It is not always the lack of evidence which leads to indeterminacy. In so far as a disagreement arises and persists, then the historians concerned characteristically share with each other the view that they are seeking, in their different ways, to describe or count the same reality and they recognise that they conflict in doing this. If evidence were available to determine the issue, then historians would characteristically use that evidence to resolve it. However, the presence or absence of evidence may in some instances be irrelevant, for in practice historians, like the rest of us, and as we saw earlier,⁶³¹ only seek to share reality with others up to a point.

Much of our argument has stressed the contingency of the pragmatic processes involved in resolving factual conflicts. While it is contingent when shared standards of consistency arise, then, given that they do arise, it is a requirement rather than a contingency that we share with others some view which provides an initial prefiguring of reality so that we can share our subject matter, and it is a requirement rather than a contingency that we share a recognition of what we would count as conflicting ways of counting or conceptualising that shared reality. It is, however, a contingency rather than a requirement that we desire fully to share the reality in question and so need to resolve any factual conflict which we recognise. It is, in other words, a contingency whether we would be worried by any inconsistency between the accounts offered by different historians. In fact, it would be characteristic for a historian, reading the (A) and (B) accounts above, to wish to overcome the conflict between them and determine by historiographical means what the reality was, so that we could see whether or not Joyce underwent the proper legal process to execution. However, it would also be characteristic for a historian, contingently, to take a different attitude, and not to think that every factual disagreement is a worry to be overcome, and this does contingently occur at the whole account level (although characteristically not at the atomic sentence level). Says Niall Ferguson, expressing a widespread view, “As with all history, the same events can be narrated in at least two mutually contradictory ways”.⁶³² This is not expressed as an epistemological complaint, suggesting a problem to be overcome; rather, the historiographical accounts concerned are supposed to be, as a whole,

⁶³¹ In the section, “Quine not postmodern enough”.

⁶³² Niall Ferguson, “The old order is defeated – prepare for the old disorder”, London: *The Sunday Telegraph*, 7 January 2007, p. 23.

factually inconsistent with each other, so conflicting in counting the same reality, but the need for resolution of that conflict is not taken seriously. In other words, the need to share the reality in question is not recognised.

The implication is that the “contradictory” accounts which result, while inconsistent, do not give rise to an inconsistency which is necessarily to be overcome. It is characteristic of many historians, particularly those influenced by “postmodernism”, to believe precisely in the philosophical point which is now made, which is that whole account truth is how historians characteristically count the world, and that there can be conflicting ways of counting reality, and we can allow that we just do not have to resolve the perceived inconsistencies, quite apart from the question whether there is an agreed way of doing it. We might just *tolerate* the positions alternative to our own.⁶³³ Indeed, freedom requires this, we might well think if we follow Foucault. We might, contingently, think that history is essentially pluralistic rather than monistic; and our view might change. Maybe only belief in God kept it monistic in the past, and without God it would lose any monistic character.⁶³⁴ Says Koselleck, “Our contemporary concept of history, together with its numerous zones of meaning, which in part are mutually exclusive, was first constituted towards the end of the eighteenth century. It is an outcome of the lengthy theoretical reflections of the Enlightenment. Formerly there had existed, for instance, the history that God had set in motion with humanity. But there was no history for which humanity might have been the subject or which could be thought of as its own subject. Previously, histories had existed in the plural – all sorts of histories which had occurred and which might be used as exempla in teachings on ethics and religion, and in law and philosophy”.⁶³⁵

If there is no consistency constraint, as there is not, in pragmatic effect, if inconsistencies at the whole account level do not require resolution, then historians, like the rest of us, can in practice “narrate” what they like, even if they cannot in practice “believe” what they like. It is only of passing interest whether Ferguson really believes that *all* historiography is indeterminate like this, and that there are, as a matter

⁶³³ See Jonathan Gorman, *Rights and Reason*, chap. 10.

⁶³⁴ “even Godless truth was seen as eternal”, we remarked in the section “Historiography of historiography”.

of contingent fact, no established positions. Many historians – Richard J. Evans, for example, makes his opposing views apparent⁶³⁶ – will feel that much historiographical output is, as we earlier expressed it, pragmatically indubitable. Nevertheless, a concern with the effect of postmodernism on historiography will still regard it as problematic if *any* events can properly be “narrated in at least two mutually contradictory ways”. If it is acceptable for judgements of inconsistency at the whole account level to be made and yet also ignored, even if only in some cases, then the plurality of ways of counting historical reality will make that reality, even if only in part, inherently indeterminate, since reality is what we count it to be. If we deliberately count it in an inconsistent way, inconsistent is what it is. The historical world, as counted by historiography, is not shared, necessarily. In principle, we might not wish to share with others the same world. Just as our world is contingently limited by, say, our territory, so may it be by our historiography, or even by our time.

The postmodern position may well be thought to be reinforced by some historiographical methods, which often involve interpreting both documents and events by empathising with the individuals involved, which amounts to understanding reality from the particular points of view involved. Remarks Koselleck, “The method of *Verstehen*...functions only at the level of the source”.⁶³⁷ Since the use of this method involves recovering the many points of view involved, and since there may have been in fact conflict between these points of view, it will always be open to the historian not to seek to resolve that conflict, for fear of imposing an anachronistic consistency or spurious objectivity. On this approach, the past may be seen as constituted by the intersubjective intersection of different past points of view, warranted by the way evidence is empathised with. It will then be contingent how far that past “reality” was a consistent reality for those past individuals concerned, and contingent how truthful it would be for historians to impose a consistent reality on those concerned by anachronistically attempting to resolve the disagreements.

⁶³⁵ Reinhart Koselleck, *Futures Past*, p. 200.

⁶³⁶ Richard J. Evans, *In Defence of History*.

⁶³⁷ “Even explanatory models employed, for instance, in the interpretation of long-term economic change, escape the method of *Verstehen*, which functions only at the level of the source”. Reinhart Koselleck, *Futures Past*, p. 154.

We may imagine today that, for those past individuals, the imagined inconsistencies were allowable at what was for them the whole account level, and not at the atomic sentence level, just as it can be for us; but it is nevertheless a contingency when we agree and when we do not. Past individuals may not share our practical distinction between the whole account level and the atomic sentence level. They may have been less *atomistic* in their understanding than we are. Past individuals may for example have fought⁶³⁸ to decide between conflicting *stories* (with perhaps, for example, a religious content), when we would see those same stories simply as conflicting points of view, with no requirement for resolution. By contrast, the need for agreement at the atomic level may have been less apparent to them. It is thus open to us to imagine a past (or a present alien) culture with a language which has a rich capacity for narrative expression yet which is rudimentary in that there is not complete agreement on language at what is for us the atomic sentence level. Their sharing of their world is minimal in such detail; they are not well placed to develop scientific knowledge as we understand that. But we may lack their certainty at the whole account level.

Some historians think that historical reality can be narrated in inconsistent ways which do not require resolution, so that factual inconsistency at the whole account level can be historiographically acceptable. Other historians disagree. Those who disagree may hold to the ancient metaphysical position that reality is inherently consistent. Antirealistically interpreted, they hold the “factual” to be just that with respect to which inconsistencies have been, and have to be, overcome. They then insist that conflicts between accounts be resolved, if those accounts are properly to be thought “factual”. Factuality comes about through resolving inconsistency. If the same events can be narrated in contradictory ways, then those narrations cannot be factual. Factual inconsistency at the whole account level must then be resolved. Against this, there is *choice* over the metaphysics here. The world is what we count it to be, and the “we” here is not wholly determinate. “We”, contrasting ourselves with some others, can contingently choose not to share with those others how the world is to be counted, while insisting on consistency for those accounts we share with our own people. We may or may not choose to tolerate what we judge to be inconsistent

⁶³⁸ Think, war; or think, peaceful search to reconcile the recalcitrant.

alternatives which others provide. Not tolerating alternatives might involve war, or it might involve historiographical methods.

One way of objecting to the postmodern position here may seem to be by denying that factual inconsistency at the whole account level can exist at all. Those who object in this way may wish to run our argument in reverse,⁶³⁹ and use the difference between our readiness to insist on resolving factual inconsistency at the level of atomic sentences and our hesitancy completely to do so at the level of whole accounts as a way of expressing what is, for them, the crucial difference between atomic sentences and whole accounts with respect to the counting of reality. Whole accounts, for them, are not to be seen as a way of counting reality at all precisely because of the room that leaves for tolerating inconsistent factual alternatives. But then, on this approach, only the atomic sentence is allowably factual. A whole account, with respect to factuality, is wholly reducible to the factuality of its constituent sentences. Whole accounts will then be no more than a set of factually disconnected atomic facts, which would return historiography to a state long before that in which Herodotus found it. This amounts to a *reductio ad absurdum* of the objection.

Historians who disagree with the multiple configuring of reality which postmodernism claims will need to find ways of overcoming factual inconsistency at the whole account level. They will have to find ways in which the historiographical syntheses of atomic facts in whole accounts make determinate the selection of facts in question, so ruling out alternative selections. With alternative selections ruled out, so is factual inconsistency overcome at the whole account level. Next we will investigate ways in which a whole account synthesis might make determinate the selection of facts, and so determine the choice between factually inconsistent accounts. Historical judgement requires an understanding of the synthesising factual choices available at the whole account level, and what their pragmatic limits are.

⁶³⁹ As often said by philosophers, one person's *modus ponens* is another person's *modus tollens*.

Structuring factual synthesis

Factual judgement in historiography is characteristically seen in terms of the factual expression of the whole account. We can analyse whole accounts into their constituent factual sentences, but treating these sentences as if they were factually independent of each other is characteristically a mistake. While in some given case the account may appear to be a conjunction, for we think it true overall, and its constituent factual sentences are all true and plainly contribute fully to its overall truth, the account's overall truth is never *just* that. If it were, the account would remain true overall by substituting for any factual sentence the arbitrarily chosen true sentence "the Coronation of Queen Elizabeth II occurred in 1953 and Joyce was born in New York and $2 + 2 = 4$ " regardless of that sentence's relevance to the subject matter of the account. In fact, the overall truth of the account is a function not just of the truth of the constituent sentences but also of their *relevance*.⁶⁴⁰

As remarked earlier, there are as many syntheses of factual sentences as there are works of historiography. There were, in all these works, *choices* of what to include and what to exclude. The historians concerned with all these works *judged* the relevance of what to include and what to exclude. While some historians are more successful at this than others, none judged arbitrarily. We all of us share an ability to judge relevance, and this has some connection with our natural capacity to attend to some part of our world to the exclusion of other parts, and with our ability to draw the attention of others to it. While, again, some of us are better at this than others, this ability is a feature of our natural rationality. Our ability to *summarise* is closely related to this ability of ours to judge relevance. Contingently, judgements of relevance may seem to us sometimes to be determinate. At one level, every historical account involves its own judgements of factual relevance, and readers in any one case may judge the selection involved to be sound. However, it is also the case that, as with what "reason" requires in other contexts, we can disagree about what is relevant and what is not. By attending to historiographical disagreements we will be able to display alternative modes of judging relevance and this will enable us to seek the

⁶⁴⁰ This is analysed at length in my *The Expression of Historical Knowledge*.

implicit or explicit criteria involved. The various structures of historiographical writing display disagreements in plenty. Our concern will be with the limits of choice with respect to such structures.

Herodotus' narrative or narration was "irreproachably comprehensive",⁶⁴¹ and we earlier referred to "whole account truth" as "narrative truth". But the word "narrative" covers a range of approaches. It might be stipulated merely to mean "the whole account", without regard to how the synthesis associated with that account is to be characterised. However, the word "narrative" is often taken as another word for "story".⁶⁴² Stories are often understood to have beginnings, middles and ends, with the whole presented in chronological order. If "narrative" is understood in this simple sense, then historical accounts can certainly be narratives, so often as to make illustration superfluous. Even the maps in the *New Cambridge Modern History Atlas* are ordered chronologically.⁶⁴³ However, many chronologically ordered works, for example Jacques Gernet's *A History of Chinese Civilization*, contain also a different mode. Apart from the obviously chronological such as "The first great military exploit of Ch'in after its reorganization by Shang Yang was its victory over the nomads of the north in 314 B.C. This victory was followed in 311 by...",⁶⁴⁴ it contains also a temporally neutral mode, such as "Two kinds of troops were to be found near the frontiers: farmer-soldiers, known as soldiers of the irrigation canals (*he-ch'ü-tsu*) or soldiers of the granaries (*k'u-tsu*), and soldiers on garrison duty in advanced posts. Look-out duty, patrols, and training occupied a considerable part of the time of troops serving in the first lines of defence".⁶⁴⁵ Yet this undated ordering, which we may call "analytical" without commitment to what that involves, is clearly subordinate to the chronological.

⁶⁴¹ J.B. Bury, *The Ancient Greek Historians*, p. 45.

⁶⁴² M.H. Abrams, *A Glossary of Literary Terms*, Fort Worth: Harcourt Brace, 1993, entry on "narrative and narratology". See also Lawrence Stone, "The revival of narrative: reflections on a new old history".

⁶⁴³ *The New Cambridge Modern History Atlas*, eds. H.C. Darby and Harold Fullard, London: Cambridge University Press, 1970.

⁶⁴⁴ Jacques Gernet, *A History of Chinese Civilization* [1972], 2nd edn., trans J.R. Foster and Charles Hartman, Cambridge: Cambridge University Press, 1996, p. 103.

⁶⁴⁵ *Op. cit.*, p. 123.

By contrast, a work may be ordered entirely analytically rather than chronologically: J. Huizinga's *The Waning of the Middle Ages* has 23 short chapters, each with a temporally neutral title such as "the vision of death" or "art and life".⁶⁴⁶ Fogel's and Engerman's *Time on the Cross: The Economics of American Negro Slavery* is organised analytically in terms of categories partly appropriate to economic theory and partly appropriate to the source material "involving thousands of man and computer hours".⁶⁴⁷ These compare with R.W. Southern's *The Making of the Middle Ages*, which is similarly temporally neutral in organisation, but contains a small amount of explicit chronological ordering, such as the section headings "Before Gregory VII; Gregory VII; After Gregory VII".⁶⁴⁸ Again, Jacob Burckhardt's much longer *The Civilization of the Renaissance in Italy* is in six parts, with temporally neutral titles, although certain sections (such as "the Papacy and its dangers") involve some chronological ordering, but ordering which is again clearly subordinate to the overall analytical structure (in this case, Part 1, "The state as a work of art").⁶⁴⁹ Other historians seamlessly operate with both chronological and analytical approaches, such as J.J. Lee in his *Ireland 1912-1985: Politics and Society*: "The industrialisation drive after 1932, which might at first sight have seemed conducive to the spread of the performance principle, did little in practice to generate a new ethic. Protection guaranteed possession of the home market to the new firms within very relaxed performance criteria. The discipline of the market was generally kept at a discreet distance. 'For more than thirty years our many state and private enterprises have produced managers by accident rather than managers by design', complained *Irish Management* in 1957..."⁶⁵⁰

But while Simon Schama's *Citizens* is mostly ordered in a traditional chronological fashion, it begins with a Prologue which looks *back* on the French Revolution and its

⁶⁴⁶ J. Huizinga, *The Waning of the Middle Ages: A Study of the Forms of Life, Thought, and Art in France and the Netherlands in the Fourteenth and Fifteenth Centuries* [1924], trans. F. Hopman, Harmondsworth: Penguin, 1955.

⁶⁴⁷ Robert William Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery*, London: Wildwood House, 1974, *passim* and dustjacket.

⁶⁴⁸ R.W. Southern, *The Making of the Middle Ages*, London: Hutchinson, 1967, p. 6.

⁶⁴⁹ Jacob Burckhardt, *The Civilization of the Renaissance in Italy* [1860], trans. S.G.C. Middlemore, revised and ed. Irene Gordon, New York: Mentor, 1960.

⁶⁵⁰ J.J. Lee, *Ireland 1912-1985: Politics and Society*, Cambridge: Cambridge University Press, 1989, p. 393.

succeeding decades.⁶⁵¹ This kind of reverse chronological structuring is factually superficial because it is clear that Schama could have used his Prologue to end the book rather than begin it, without affecting the book's factual content. The example shows us a more general point: chronological ordering is always, in factual terms, only a presentational matter, for chronology is not a way of selecting facts. It merely orders facts already selected, facts that are dated (implicitly or explicitly) *prior* to the presentation. Here is a chronological "narrative" to make this obvious point: "The first great military exploit of Ch'in after its reorganization by Shang Yang was its victory over the nomads of the north in 314 B.C".⁶⁵² "Under Adrian VI (1521-1523), the few and timid improvements carried out in the face of the great German Reformation came too late".⁶⁵³ "The industrialisation drive after 1932, which might at first sight have seemed conducive to the spread of the performance principle, did little in practice to generate a new ethic".⁶⁵⁴ Mere chronological ordering does not synthesise.

As Arthur Danto points out of a similar example, such a "correctly ordered sequence of true statements is not a chronicle if, by 'chronicle', we mean an accredited way in which some professional historian delivers publicly the narrative of his discoveries".⁶⁵⁵ In fact, "chronicle" does not characteristically mean that for historians. Historians commonly distinguish chronicles from historiography proper: thus the *Anglo-Saxon Chronicle*, a collection of medieval manuscripts, presents "facts" broadly in chronological order, but a historical "narrative" is characteristically more than that. On the other hand, while a mere "chronicle", the *Anglo-Saxon Chronicle* is not "merely chronological". Although the facts selected may often appear somewhat arbitrary to us, the scribes concerned were writing that which they or those around them wished to record as in some way significant.

⁶⁵¹ Simon Schama, *Citizens*.

⁶⁵² Jacques Gernet, *A History of Chinese Civilization*, p. 103.

⁶⁵³ Jacob Burckhardt, *The Civilization of the Renaissance in Italy*, p. 116.

⁶⁵⁴ J.J. Lee, *Ireland 1912-1985*, p. 393.

⁶⁵⁵ Danto, A.C., "Mere chronicle and history proper", *Journal of Philosophy* 50, 1953, 173-182 at p. 174.

W.H. Walsh distinguishes firmly between “plain” and “significant” narratives,⁶⁵⁶ where “plain” refers to the purely factual and “significant” *explains* the facts involved. In effect he offers what we earlier described as a two-stage historiography: first, you find the facts; second, you explain them. However, against this, the “purely factual” plain narrative must still have passed the test for the *relevance* of its constituent factual sentences to its subject matter. Walsh’s assumption that explanation is essentially different from the factual, which has to mean that explanation is essentially different from the *relevantly* factual, forces us to separate what are most plausibly seen as closely related: when historians explain, they are also *selecting*. Explanation can be a major way of determining factual relevance and can thereby itself be factual. Conversely, the presentation of facts relevantly synthesised with each other can just *be* the explanation of those facts.

Lying behind Walsh’s assumption that stating facts is one thing and explaining them is another is a view, widespread among philosophers at the time he was writing, that explanation is essentially *causal*, and that causation is to be understood at least in broadly Humean terms and, more attractively for many, specifically in terms of Hempel’s 1942 article “The function of general laws in history”.⁶⁵⁷ It was on the basis of causation mainly understood in his way that Arthur Danto and Morton White analysed narrative in the 1960s.⁶⁵⁸ For White and Danto, narratives are essentially presentations of historical facts which are causally linked. For Hempel, as we saw earlier, explaining an event consists in indicating its causes, and to say that some events have caused the event *E* to be explained “amounts to the statement that, according to certain general laws, a set of events of the kinds mentioned is regularly accompanied by an event of kind *E*”.⁶⁵⁹ There is for him no difference between displaying the cause of an event and showing that scientific laws cover that event. On this basis an explanation has to look like a deductive argument. Certainly the requirement for a

⁶⁵⁶ Walsh, W.H., *Introduction to Philosophy of History*, p. 31.

⁶⁵⁷ See our earlier section “Modelling a discipline: the truth of historical theory”, which refers to Carl G. Hempel, “The function of general laws in history”.

⁶⁵⁸ A.C. Danto, *Analytical Philosophy of History*, Morton G. White, *Foundations of Historical Knowledge*.

⁶⁵⁹ C.G. Hempel, “The function of general laws in history”, p. 345.

deductive argument to be valid can operate as a constraint on the selection of its constituent sentences, and to some extent determine relevance.⁶⁶⁰

Relevance can be determined in the following way. We think of the argument as Hempel did:

Whenever $C_1...C_n$ then E ;
 $C_1...C_n$;
 Therefore, E .

$C_1...C_n$ are fact-asserting sentences independently warranted by appropriate historiographical means, but they are only some of many. E is a fact-asserting sentence independently warranted by appropriate historiographical means, but it is only one of many. $C_1...C_n$ are selected as relevant because they, unlike the other practically available facts, can be shown to warrant the factual conclusion of the argument. Here E is imagined to have primacy in the account, as the event to be explained, but that need not be the case. $C_1...C_n$ and E are associated by the account with each other, so they become mutually relevant: to some extent they stand or fall together as relevant (although not, of course, as true). It is the primary subject matter of the account which determines whether we are interested in those facts at all. By contrast, the sentence “Whenever $C_1...C_n$ then E ”, while *factual* and supposedly well confirmed by the empirical evidence, is not in fact warranted by historiographical means. More importantly for our overall concerns, it and its like do not characteristically appear in historical accounts. Such laws are not, in fact, among historians’ grounds of selection.

More importantly still, we simply do not have, as a matter of contingent fact, a range of scientific laws which will cover the events characteristically dealt with in historiography. Perhaps such laws do not exist; they are certainly not known by historians. Hempel knows this, which is why he presents historians as offering only “sketches” of the necessary arguments. Relevance for him is essentially *causal* relevance, and, given the lack of suitable historical laws, historians’ judgements of relevance are equivalently poor. Historians’ use of causation typically displays its

⁶⁶⁰ Whether classical logic is *good enough* at determining relevance is a question which we shall not address here. See S. Read, *Relevant Logic: A Philosophical Examination of Inference*, Oxford: Basil Blackwell, 1988.

indeterminacy in the historiographical context. For Carr, “the study of history is a study of causes”,⁶⁶¹ but for Elton “causation is merely one of several ways in which historical events may be “linked and rendered comprehensible”.⁶⁶² As we saw earlier in Murphey’s characterisation of Wertenbaker’s, Washburn’s and Bailyn’s accounts of Bacon’s Rebellion, all three interpretations “seek to provide a causal explanation of events. ... Yet these three interpretations ... differ in their use of narrative, the significance which they attribute to events, and even the facts which they discuss”.⁶⁶³

While we do not have scientific laws suitable for historiography, and it is widely accepted that these are unavailable, the possibility of these has been more philosophically controversial than it is now,⁶⁶⁴ and historians have taken different views about their importance for historiography. There has been a range of so-called speculative or metaphysical⁶⁶⁵ philosophies of history and other large-scale “clues”⁶⁶⁶ to history, not all of them obviously causal in form, which historians have used. They can *choose*, for example, to adopt (although they would no doubt have to reinvigorate them first) progressive approaches such as Hegel’s or a challenge-response view such as Toynbee’s or a cyclical view like Spengler’s,⁶⁶⁷ or explain at a collectivist and macroeconomic rather than individualist level by drawing on, say, Marx’s or Marxian approaches. Such theories will determine large scale outlines of what historiographical work should look like, and so to some limited extent which facts are relevant to an account.

However, regardless of how metaphysical, scientific or otherwise factually meritorious their theoretical source may be, an associated problem with the use of causal links to structure a whole account is that, even if particular causes of particular events are,

⁶⁶¹ E.H. Carr, *What is History?*, p. 81.

⁶⁶² G.R. Elton, *The Practice of History*, p. 23.

⁶⁶³ Murray G. Murphey, *Our Knowledge of the Historical Past*, p. 109.

⁶⁶⁴ See Karl Popper, *The Open Society and its Enemies*, vol. 1, *Plato*, and vol. 2, *Hegel and Marx*, and his *The Poverty of Historicism*.

⁶⁶⁵ Here a derogatory term, as is characteristic among historians although not philosophers.

⁶⁶⁶ This is Kant’s term. See his “Idea of a universal history from a cosmopolitan point of view” [1784], in Gardiner (ed.), *Theories of History*, pp. 22-34.

⁶⁶⁷ G.W.F. Hegel, *The Philosophy of History* [1837], trans. J. Sibree, New York: Dover, 1956. A.J. Toynbee, *A Study of History*, London: Oxford University Press, 12 vols., 1934-1961. Oswald Spengler, *The Decline of the West* [1919], trans. C.F. Atkinson, London: George Allen and Unwin, 1928.

untypically, completely clear, the overall selection problem has still not been solved, for that would require that there be a continuous – completely unbroken – chain of cause and effect throughout. Such a chain is in practice unavailable, not least because the Hempelian model of explanation is also a model of prediction. If we know that “Whenever $C_1...C_n$ then E ” and “ $C_1...C_n$ ”, then we can *predict* E . With a causal chain similarly understood, then we would be able to predict the entire course of events. This is indeed the outcome of some speculative philosophies of history, such as Marx’s reference to “the inevitable victory of the proletariat”.⁶⁶⁸ But, to the best of our understanding, the world is just not that deterministic. While historians have the advantage of hindsight, successful hindsight grasp of causes would also give them, because of the generalisations alleged to be involved, an extraordinary foresight, which they do not have. In practice historians have sufficient understanding of causation, which is in any event an everyday notion which is atomistic or particular rather than long-term or synthesising in its application, to offer causes of some historical events or states of affairs. The notion of “cause” involved, however, is then not of a kind which would have enabled those events or states of affairs to have been predicted, which suggests that generalisations should not be central to our analysis of causation.⁶⁶⁹

A related problem affects W.H. Dray, who developed a traditional line of anti-positivist argument in his criticism of Hempel’s approach. Dray holds that in historiography we characteristically seek to explain human actions by using “rational explanations”.⁶⁷⁰ In particular, as we saw earlier, we seek “a reconstruction of the agent’s calculation of means to be adopted toward his chosen end in the light of the circumstances in which he found himself”.⁶⁷¹ We may wish to accept Dray’s view here, and indeed we can do

⁶⁶⁸ An inexact, but familiar, quotation from Karl Marx and Friedrich Engels, *The Communist Manifesto* [1848], ed. Gareth Stedman Jones, London: Penguin, 2002. See Mark Cowling (ed.), *The Communist Manifesto: New Interpretations*, Edinburgh: Edinburgh University Press, 1998.

⁶⁶⁹ Needed are theories of causation different from Hempel’s Humean “regularity” conception. See, for example, R.G. Collingwood, “On the so-called idea of causation”, *Proceedings of the Aristotelian Society* 38, 1937-38 p. 85; D. Gasking, “Causation and recipes”, *Mind* 64, 1955, p. 479; J.L. Mackie, “Causes and conditions”, *American Philosophical Quarterly* 2, 1965, p. 245; J.L. Mackie, “The direction of causation”, *Philosophical Review* 75, 1966, p. 441; David Lewis, “Causality”, *The Journal of Philosophy* 70, 1973, pp. 556-567; David Lewis, “Counterfactual Dependence and Time’s Arrow”, *Noûs* 13, 1979, pp. 445-476; David Lewis, “Causation as Influence”, *The Journal of Philosophy* 97, 2000, pp. 182-197; essays in T.L. Beauchamp (ed.), *Philosophical Problems of Causation*, Encino: Dickenson, 1974.

⁶⁷⁰ W.H. Dray, *Laws and Explanation in History*, p. 124.

⁶⁷¹ *Op. cit.*, p. 122.

so without committing ourselves to a view on the conflict we examined earlier,⁶⁷² between positivists and their opponents, because it may be that such a calculation can be understood in causal terms, as Donald Davidson suggests.⁶⁷³ Perhaps “reason” should be understood in terms of cost-benefit analysis, and those historians are correct who choose to concentrate on individual action and use neoclassical microeconomic theory as the bases of their explanations.⁶⁷⁴ But even if such views are correct, we cannot use them to structure a whole account because there cannot plausibly be a “calculation” (however interpreted) which would structure the whole account. Here it would be the past individual who would be imagined to have extraordinary foresight, quite apart from remarkable power.⁶⁷⁵ Once again, such modes of explanation are broadly atomistic rather than synthesising in their application.

While causation and rational calculations of various kinds have their place in historical writing, a full understanding of the structures of historiographical synthesis would tell the historian just *when* a cause or a rational calculation is relevant. Historical figures often (although not always) “calculate” in some sense, if not Dray’s, because people are characteristically rational, and the field of explanation is plausibly exhausted if we add to this approach the view that whatever remains is causally determined. The field is equally exhausted if *every* event has a cause. From such massive oversupply of “facts” and their explanations historians must still *select* (not necessarily as a two-stage operation).⁶⁷⁶ Historians, like the rest of us, characteristically and successfully judge relevance independently of whether they have a well worked out theory of causation or a philosophy of individual action, and they characteristically do so without some speculative philosophy of history which specifies large-scale causal or rational structures which might determine the relevance of particular facts.

What is required is a theory appropriate to historiographical practice which enables historians to judge relevance and to place causal and other modes of piecemeal

⁶⁷² In the section “Modelling a discipline: the truth of historical theory”.

⁶⁷³ See Donald Davidson, “Actions, reasons and causes” [1963], reprinted in Alan R. White (ed.), *The Philosophy of Action*, London: Oxford University Press, 1968, pp. 79-94.

⁶⁷⁴ See Jonathan Gorman, *Understanding History*.

⁶⁷⁵ Hegel’s “World Spirit” might answer the requirement.

⁶⁷⁶ A parallel argument prevents Gallie’s use of necessary conditions from being effective. See W.B. Gallie, *Philosophy and the Historical Understanding*, London: Chatto and Windus, 1964.

explanation in an account-structuring context. A theory which seeks to do this is Hayden White's, initially presented in his "much-discussed but little-imitated"⁶⁷⁷ *Metahistory*.⁶⁷⁸ White sees certain works of nineteenth-century European historiography as representative forms of historiographical reflection, and provides in *Metahistory* an "analysis of the deep structure of the historical imagination".⁶⁷⁹ Consistently with our own approach, which holds that historiography is to be understood in terms of the *standards* accepted by the profession, which thus specify how historiography *ought* to be undertaken, White holds that "historiographical disputes on the level of 'interpretation' are in reality disputes over the 'true' nature of the historian's enterprise".⁶⁸⁰ His approach thus presents different historians' judgements about what historiographical explanations, and other characteristic features of historiography, should be. Again consistently with our own approach, which argues for a pragmatic difference between truth at an atomistic level and truth at a whole account level, White says "I am not speaking here of the kinds of disputes which arise on the reviewers' pages of the professional journals, in which the erudition or precision of a particular historian may be questioned. I am speaking about the kinds of questions which arise when two or more scholars, of roughly equal erudition and theoretical sophistication, come to alternative, though not necessarily mutually exclusive, interpretations of the same set of historical events...".⁶⁸¹

In a way in which, we have earlier argued, many historians do not, White has the fullest sense that historiography needs to be understood in terms of its own historical context, so that he sees himself as writing a history of historical consciousness, and he takes the same view of philosophy: "the evolution of philosophy of history – from Hegel, through Marx and Nietzsche, to Croce – represents the same development as that which can be seen in the evolution of historiography from Michelet, through Ranke and Tocqueville, to Burckhardt. The same basic modalities of conceptualization

⁶⁷⁷ Richard J. Evans, *In Defence of History*, p. 354.

⁶⁷⁸ Hayden V. White, *Metahistory*. The introductory section is particularly important. See also his *Tropics of Discourse: Essays in Cultural Criticism*, Baltimore: The Johns Hopkins University Press, 1985, *The Content of the Form: Narrative Discourse and Historical Representation*, Baltimore: The Johns Hopkins University Press, 1987 and *Figural Realism: Studies in the Mimesis Effect*, Baltimore: The Johns Hopkins University Press, 1999.

⁶⁷⁹ Hayden V. White, *Metahistory*, Preface, p. 1.

⁶⁸⁰ *Op. cit.*, Introduction, p. 13.

appear in both philosophy of history and historiography, though they appear in a different sequence in their fully articulated forms”.⁶⁸² However, White also recognises the *contingency* of connection between philosophy and historiography, arguing that “the philosopher of history represents a greater threat also, because philosophy of history is characteristically a product of *a desire to change the professionally sanctioned strategies by which meaning is conferred on history*”.⁶⁸³ His expression “professionally sanctioned strategies” is exactly appropriate to those rules of the profession of historians which we have been seeking to elucidate.

We have in our own argument been working with the antirealist assumptions involved in holding that reality is what we count it to be, and this places language at the centre of our understanding of reality, and also suggests that we should, as White does, take into particular philosophical consideration what Goldstein called the “superstructure”, although without taking that as categorically distinct from the “infrastructure”. This so-called “linguistic turn” means that all the resources of our language are available for our purpose of counting reality, and indeed for any other purpose appropriate to historiography in our understanding of that. Among these resources are “tropes”, or literary devices, and White’s analysis argues for their centrality in historiography, beginning with the view that a characteristic piece of historical writing has the form of a narrative prose discourse, with a structure which is poetic and not narrowly literal. “The theory of tropes provides a way of characterizing the dominant modes of historical thinking which took shape in Europe in the nineteenth century. And, as a basis for a general theory of poetic language, it permits me to characterize the deep structure of the historical imagination of that period considered as a closed-cycle development. For each of the modes can be regarded as a phase, or moment, within a tradition of discourse which evolves from Metaphorical, through Metonymical and Synecdochic comprehensions of the historical world, into an Ironic apprehension of the irreducible relativism of all knowledge”.⁶⁸⁴

⁶⁸¹ *Ibid.*

⁶⁸² *Op. cit.*, Introduction, p. 42.

⁶⁸³ *Op. cit.*, chap. 7, p. 276; White’s emphasis.

⁶⁸⁴ *Op. cit.*, Introduction, section *The Phases of Nineteenth-Century Historical Consciousness*, p. 38.

The deep structure of narrative prose discourse is also the deep structure of historical consciousness, and White believes himself forced to postulate conceptual strategies which historiographical work presupposes, strategies in terms of which historians seek to explain and present their material. These conceptual strategies have an *a priori* status relative to any given historical work, so reminding us of Kant's position.⁶⁸⁵

Earlier we argued that, in so far as reality is what *we* count it to be, there must be a minimal sharedness: in any given case, there has to be some shared prefiguration of that subject matter which is, supposedly, configurable in so many ways. This prefiguration is not White's, for it is for him a *poetic* act which prefigures what he calls the historical "field", and historians can differ about which such act to engage in. Such prefiguration need not be shared, unlike our own. White's is then a second stage of prefiguration after our own, and it is this prefiguration which creates what he calls the historical "domain". It is this domain to which the historian brings to bear, in a third stage,⁶⁸⁶ the specific theories he will use to explain "'what was really happening' in it", "that is to say, constitute it as an object of mental perception".⁶⁸⁷

That historiography has a poetic foundation has a long tradition. As we observed earlier, Herodotus owed prose historiography to Hecataeus, who developed the work of logographers who put into prose poetic mythical traditions. Repeating part of an earlier quotation, "In one sense, history writing for the Greeks began with Homer. In another more formal sense, history was not only a new literary genre but a radically new kind of genre when Herodotus and Thucydides began to write in the fifth century B.C."⁶⁸⁸ But theirs were *additions* to the poetic traditions, not *replacements* of them.⁶⁸⁹

⁶⁸⁵ In Immanuel Kant, *A Critique of Pure Reason*.

⁶⁸⁶ The counting of stages here is intended to be neutral with respect to the question how one stage is related to another. In fact White implies, probably inconsistently with his overall position, that the stages are temporally distinct; Hayden V. White, *Metahistory*, p. 5. See Jonathan Gorman, "Reality and irony in history", *Storia della Storiografia* 24, 1993, 59-69, from which the summary of White's position is here derived.

⁶⁸⁷ Hayden V. White, *Metahistory*, Introduction, section *The Phases of Nineteenth-Century Historical Consciousness*, p. 30.

⁶⁸⁸ Carolyn Dewald, "Practical knowledge and the historian's role in Herodotus and Thucydides", p. 47.

⁶⁸⁹ Herodotus wrote in prose. But this is hindsight conceptualisation. One difficulty, so far as his self-understanding is concerned, is that this is what some later historians have described him as doing, rather than what he describes himself as doing. We may recall Molière: M. Jourdain: ...is that prose?

Continuing our outline of White's position, we see that according to him the poetic act of prefiguration takes one of a number of the following forms, forms which are classifiable in terms of their dominant linguistic modes: Metaphor, Metonymy, Synecdoche, and Irony. These are the four principal modes of historical consciousness, for White. For him, the poetic act is precognitive and precritical, and it is indistinguishable from the linguistic act which makes the field ready for interpretation. The poetic act precedes the formal analysis of the field. The field must be construed as a ground inhabited by distinguishable entities, entities classifiable as having certain kinds of relationships with each other. The poetic act is also constitutive of the concepts the historian will use to identify these entities and their relationships. The act is thus, in White's terms, constitutive of the verbal model which the historian offers as a representation and explanation of what really happened. It determines the type of conceptual strategy used to explain. Once the field is prefigured, historians, according to White, have three strategies of explanation,⁶⁹⁰ and for each strategy, White identifies four possible forms of expression by which the historian can gain a specific kind of explanatory affect, so totalling a Kantian twelve.⁶⁹¹

Richard J. Evans says of White and others with related views, "we have a paradoxical situation in which arguments are overwhelmingly advanced...about the nature of the historian's enterprise as a whole, on the basis of a reading of the subject's practitioners writing a century or more ago".⁶⁹² However, here it is necessary to clarify the status of White's position. His theory is not *derived* from the work of nineteenth century

Philosophy Teacher: Yes, Sir.

M. Jourdain: Good heavens! For more than forty years I have been speaking prose without knowing it.

Molière, *Le Bourgeois Gentilhomme* (1670), II, iv, source reference owed to *The Oxford Dictionary of Quotations*, Oxford: Oxford University Press, 1979.

⁶⁹⁰ These are: by emplotment, by formal argument, and by ideological implication. These and similar details will not be analysed here.

⁶⁹¹ Explanation by emplotment involves archetypes of Romance, Comedy, Tragedy, and Satire. Explanation by formal argument involves the modes of Formism, Organicism, Mechanism, and Contextualism. Explanation by ideological implication involves reference to Anarchism, Conservatism, Radicalism, or Liberalism. Each of these twelve "categories" is complex in application and they are only to a limited extent self-explanatory, and greater understanding is best achieved by attempting to sort a wide range of historians by using them. Such detail, however, is not relevant to our present argument.

⁶⁹² Evans, Richard J., *In Defence of History*, p. 71.

historians, but rather purports to express, in general, what is *necessary* to understand those historians. His argument has a transcendental status, and is Kantian at least in that respect.⁶⁹³ That is, it presents what is judged necessary for *White*, and if he is right the rest of us, to understand those historians. White is strongly self-reflective in his presentation of this understanding, and recognises that he himself has to structure that nineteenth-century historical field which is his subject matter in a way which is consistent with his own theory. He himself does so as a *twentieth-century* historian, not as a nineteenth-century one.

White sees certain works of nineteenth-century European historiography as *representative forms* of historiographical reflection. Just because, as Evans himself says, White's work has been "much-discussed but little-imitated",⁶⁹⁴ there is little to suggest that a similar study of twentieth-century historians would not, at least in principle, yield similar results, particularly as White applies his theory to his own work. In effect, if correct, it does not need to be repeated. Moreover, his theory includes an understanding of the place of empirical work in historiography, work which Evans thinks particularly characteristic of twentieth-century historiographical developments. The situation is not paradoxical; on the other hand it may well, although perhaps not in White's terms, be *ironic*, for it is the very postmodern force of White's "nineteenth-century" approach which has influenced many twentieth-century historians and which worries Evans into the twenty-first century.

Hans Kellner sees more clearly, than it sometimes seems does the White of *Metahistory*, the Kantian implications of the position: "The paths from *Metahistory* will be quite divergent: on the one hand, the more canny historians will naturalize the elements of the quadruple tetrad, and incorporate them without difficulty into the tradition of professional discourse; on the other, the deconstructors will trope the turns and turn the tropes, unfolding their texts until they have arrived at their nondestination. What will *not* happen is a close approximation of White's own

⁶⁹³ See our section "Respect for historiography".

⁶⁹⁴ Richard J. Evans, *In Defence of History*, p. 354.

way”.⁶⁹⁵ White’s historiographical structuring is *safe*, for Kellner (if not for Evans); White refuses the “‘absurdist moment’ lurking within his system”.⁶⁹⁶ Kellner quotes Derrida: “This stratum of ‘founding’ tropes, this layer of ‘first’ elements of philosophy (let us suppose that scare-quotes are sufficient precaution here), cannot be subsumed. It will not allow itself to be subsumed by itself... there would always be at least one metaphor that would be excluded, or, to cut the argument short, the metaphor of metaphor”.⁶⁹⁷ In other words, White’s self-reflexiveness is not carried far enough, for his tropes, characterised by White himself as *foundational*, are themselves the outcome of a literary approach which *seeks to create the foundational*. Says Kellner, “in the face of this abyss, in which the trope of trope has always escaped one’s grasp, the electorate of affinities becomes clear: *it is White’s choice*”.⁶⁹⁸ “The key question has to do with the origin of tropes. Are they somehow ‘*a priori*’ or even ‘natural’ structures of understanding (as Kant or Freud would have it), or merely cultural ‘conventions’ (as Hayden White or Roland Barthes would assert)?”⁶⁹⁹

We began the present section recalling that there are as many syntheses of factual sentences as there are works of historiography, and said that our concern here is with the limits of choice with respect to such structures. Just how much room for choice, and so historical judgement, is there, on White’s approach? While White, as Kellner rightly concludes, is in the end best interpreted as holding that tropes are “conventional” rather than “Kantian”, there is nevertheless scope for the alternative reading.⁷⁰⁰ While the tropes and other categorisations in White’s position can be in dialectical tension with each other, their very identification as *particular* modes of categorisation which are primarily *distinct* from each other suggests, not just that they have a “foundational” function in historical writing, but that they are foundational in the sense of being *unalterable*. White seems not committed to this – indeed, he had best not be, if he thinks that they are cultural conventions – because while his

⁶⁹⁵ Hans Kellner, *Language and Historical Representation: Getting the Story Crooked*, Madison, Wisconsin: The University of Wisconsin Press, 1989, p. 225.

⁶⁹⁶ *Op. cit.*, p. 224.

⁶⁹⁷ *Ibid.*

⁶⁹⁸ *Ibid.* Our emphasis.

⁶⁹⁹ *Op. cit.*, p. 191.

categorisations are argued to arise as necessary for understanding certain nineteenth-century historians, they may be contingent if they vary with which historians are to be understood. In principle, those categorisations may then be seen as time-bound. Yet nothing else about his theory suggests the mere contingency or time-bound nature of such tropes. White's position shows no sign that it might be, in Karl Popper's terms, *falsifiable*, which is an important sign of contingency. White's categorisations are clearly intended by him to cover *representative* forms of historical writing, and so historiography *in general*. If this is so, then they do not undergo cultural change. They then mark our limits: they are not themselves a matter of choice.⁷⁰¹ Postmodern reconfigurability would end here.

Yet once we *know* – if we do – that we as historians characteristically write in terms of White's or similar approaches, then apparent to our consciousness are those fundamental historiographical categorisations which White has now historiographically recovered for us. Being conscious of them, we should now be able to criticise them, and change them for alternatives if we think fit. Yet here we face a main point of our earlier argument: to do this, *alternative categorisations must be available*. White's *detail* is no doubt revisable and there are many literary theorists with alternative views, but the overall approach in which we structure our shared experience in fixed rhetorical and storylike modes may be correct. Pragmatically, there just may not be an alternative to this. If that is our situation, then White's or similar categorisations contingently function, for all us human beings, as *a priori* ways of determining general relevance, in so far as they are beyond *our* present choice. They then are, in pragmatic effect, Kantian; at least for now.

Can we find alternative categorisations? If we think that making sense of the past-present continuum of our world is best understood as a form of art, then we will characteristically think that the generation of alternative ways of seeing and presenting that world is an essential part of the discipline of historiography, just as it is for other

⁷⁰⁰ Richard J. Evans, describing Russell Jacoby as one of White's "most acute critics", reports Jacoby's comment that White's rhetoric reveals "the language – and cadence – of aggressive science". *In Defence of History*, p. 69.

⁷⁰¹ It should be noted that we are here recovering the implications of the argument; it is no longer relevant to discover White's "actual" view on this.

forms of art. Postmodernism, with its assertion of the multiple reconfigurability of historical reality, gives historians the freedom to do this. We can expect to generate and so find alternatives; perhaps historiography as currently understood will entirely stop. On the other hand, if we think we cannot find alternatives to categorisation in terms of fixed storylike modes, then we may well think that these modes are foundational for us. We may then think of this categorisation as being less like Kant's *a priori* categories, with respect to which, with historical hindsight, we may now think ourselves to have alternatives, and more like Kant's *a priori* intuitions, space and time. Here, whatever our physics says, it seems impossible in practice for us to experience our everyday world other than in the light of those dimensions, and history is continuous with such understanding. Experiencing in terms of narratively organised time may be fundamental. That historiography, in its structures of writing, inherits ancient *poetic* mythical traditions and continues to be fundamentally of that kind may then be a function of unchanging human nature itself.

“Human nature” may not be merely a general name for that shadowy place into which we, in our ignorance, cast that which we find primitive or mysterious in our understanding and self-understanding. Rather, we may think that the limit of our narratively organised time-bound perception of the world is a proper subject for science. We may suppose that our structuring ability here is epistemically primitive because this is how the human brain works. We may speculate about a future in which cognitive science, which already recognises the importance of narrative, has given us the answers. It is, indeed, very early days for that discipline: internet search engines still largely work through key words, for example, rather than modelling our own sophisticated conceptions of relevance, but we can expect advances here. Again, the science of memory has to take into consideration the effectiveness of repetition in storylike form as an aid to memory, and “Frederic Bartlett showed over seventy years ago that what people remember best about stories are not the words themselves, but the ‘gists’ of narratives”.⁷⁰² Our future may include a science of gists.

⁷⁰² Harvey Whitehouse, research paper, “Cognitive science and the art of historiography”, p. 7, published as “Cognitive historiography: where science meets art”, *Historical Reflections/Reflexions*

We now find that we are in the same uncertain position as Kant, for whom, in the *Critique of Pure Reason*, human nature was unchanging, but for whom, in the “Idea of a universal history”, human nature could change. We earlier argued that the factuality of a whole historiographical account should not be understood as wholly reducible to the factuality of its constituent sentences. If whole accounts were no more than a set of factually disconnected atomic facts, we remarked, that “would return historiography to a state long before that in which Herodotus found it”.⁷⁰³ Maybe there never was that earlier state. More likely, we may now think, is that Herodotus developed his “irreproachably comprehensive”⁷⁰⁴ account using innate storytelling abilities, shared with us all, to structure the shared everyday world and that historical reality which is continuous with it. Our understanding is then continuous with his. “Human nature” displays itself as much in our historiography as in our history, and we have found little evidence of fundamental change as we moved from Herodotus through Ranke to the present. An earlier conclusion we drew was that it is not then true that the past is a foreign country where they do things differently. But is *that* true? It is an outcome of how *we have counted that history*. It was our decision. Did we have an alternative?

It is contingent how the human brain works, and it is contingent how we understand it to work. Even a developed cognitive science is not to be understood as enabling us to bypass our own reconfigurable conceptualisations of reality, for it is itself subordinate to our *decisions* what to believe. Moreover, perhaps that future science would be historiographical rather than atomistic in form. Our question in this section has been with the limits of choice with respect to synthesising factual sentences in historiography. It is our art, more than our science, which is most likely to make those limits apparent to us.

Historiques 31, 2005, pp. 307-318, referring to Bartlett’s *Remembering*, Cambridge University Press, 1932.

⁷⁰³ At the end of the previous section, “Holistic choice”.

⁷⁰⁴ J.B. Bury, *The Ancient Greek Historians*, p. 45.

Moral judgement in historiography

The section “Historians’ self-understanding” concluded that historiography had continually been structured by questions about historical truth and rhetoric, comprehensiveness of factual description, and of moral judgement. In this final section we outline the limits of the last of these. Once, that historiography was primarily for moral teaching was taken for granted. Says Bentley, “The view that history was fit only for ‘philosophy teaching by example’ did not originate in the eighteenth century: it occurs in classical writers and Renaissance writers rediscovered it. But Lord Bolingbroke’s *Letters on the Study and Use of History* (1752) gave the concept a contemporary cachet and few authors of his day avoided giving a patina to their text that was intended to elevate the mind of the reader or bend it towards a particular conclusion”.⁷⁰⁵ Here is Kelley on a sixteenth-century example: “Toward history itself Le Roy ostensibly took a naïvely utilitarian view, expressed by the Ciceronian (and Budaeian) representation of history as a repository of examples (*historia plena exemplorum; histoire plein d’exemples*). ‘The memory and knowledge of the past’, Le Roy declared, ‘is the instruction of the present and the warning of the future’”.⁷⁰⁶

“Power tends to corrupt, and absolute power corrupts absolutely”, said Lord Acton in a letter of 1887. Since before Herodotus, historians have been active in the debates of their own time, and often, particularly in the nineteenth century, seen as authoritative in virtue of their historical understanding. Here Acton is commenting on a then current issue, and Acton’s moral judgements were more often of the present than of the past. Nevertheless, it is he who is widely understood to be paradigmatic of the view that moral judgements should inform historiography itself.⁷⁰⁷ Yet Butterfield observes that, following discussions with Ranke and years of reflection, Acton “condemns the ‘exceeding vividness’ of moral judgements in Macaulay and Thomas Carlyle, and in men like Michelet and Taine”.⁷⁰⁸ It is apparent that Acton *agonised* about this matter.

⁷⁰⁵ Michael Bentley, *Modern Historiography*, p. 4, referring to A.D. Culler, *The Victorian Mirror of History*, New Haven: Yale University Press, 1985, p. 4.

⁷⁰⁶ Donald R. Kelley, *Foundations of Modern Historical Scholarship*, p. 81, referring to Louis le Roy, *La Vicissitudes des choses* (Paris, 1584), f. 247^r, 254^r.

⁷⁰⁷ Herbert Butterfield, *The Whig Interpretation of History*, particularly the last chapter.

⁷⁰⁸ Herbert Butterfield, *Man on his Past*, p. 92.

Present-day historians characteristically disagree about it. The Royal Historical Society is “mindful” of an increasing concern with ethical issues.⁷⁰⁹ Edward Said thinks that all discourse, and particularly historiography, is inherently ideological.⁷¹⁰ Our situation in the modern world informs many historians’ attitudes. Here is Chairman Mao: “You can’t solve a problem? Well, get down and investigate the present facts and its past history! When you have investigated the problem thoroughly, you will know how to solve it”.⁷¹¹ “‘Facts’ are all the things that exist objectively, ‘truth’ means their internal relations, that is, the laws governing them, and ‘to seek’ means to study”.⁷¹² “The mistakes of the past must be exposed without sparing anyone’s sensibilities; it is necessary to analyse and criticize what was bad in the past with a scientific attitude so that work in the future will be done more carefully and done better”.⁷¹³ Mao here follows ancient Chinese tradition, as presented from Beijing: “for instance, on the tragic end of Xiang Yu who lost his battle to Liu Bang, Sima Qian [historian, 145-90 BCE] commented: ‘While Xiang was about to die, he was still not aware of his own mistakes, and instead of blaming himself he was lamenting, ‘It is Heaven that fails me, but not my own fault in battle’. Isn’t it an absurdity?’ To place the interpretation of history on the humanistic basis rather than on Providence – here lies his remarkable insight rarely found in ancient historiography”.⁷¹⁴ Butterfield understands such claims, and has a moral objection: “since moral indignation corrupts the agent who possesses it and is not calculated to reform the man who is the object of it, the demand for it – in the politician and in the historian for example – is really a demand for an illegitimate form of power”.⁷¹⁵ Moreover, “moral judgments on human beings are by their nature irrelevant to the enquiry and alien to the intellectual realm of scientific history”.⁷¹⁶

⁷⁰⁹ Royal Historical Society Statement on Ethics, <http://www.rhs.ac.uk/>, [December 2004], accessed 2 December 2006.

⁷¹⁰ Edward Said, *Orientalism*, New York: Random House, 1978.

⁷¹¹ Mao Tsetung, *Quotations from Chairman Mao Tsetung* [“The Little Red Book”, 1966], Peking: Foreign Languages Press, 1976, p. 233.

⁷¹² *Op. cit.*, p. 231.

⁷¹³ *Op. cit.*, p. 262.

⁷¹⁴ He Zhaowu, Bu Jinzhi, Tang Yuyuan and Sun Kaitai, *An Intellectual History of China*, revised and trans. He Zhaowu, Beijing: Foreign Languages Press, 1991, pp. 197-198. See also “Forum: Chinese and Western historical thinking”, ed. Jörn Rüsen, *History and Theory* 46, 2007, pp. 180-232.

⁷¹⁵ Herbert Butterfield, “Moral judgments in history”, in *History and Human Relations*, p. 110.

⁷¹⁶ *Op. cit.*, p. 103.

Here is one of Elton's views: "It is all very well to regret the day when history was 'philosophy teaching by examples', the day when historians thought themselves the moral preceptors of a ruling class and, aping Plutarch, used their science to instil high principles in their pupils. We cannot return to the attitude which produced a *Mirror for Magistrates* to show, by using historical instances, how those who offend against the divine order always come to a bad end".⁷¹⁷ And here is Elton with another: "The professional [historian], truly understanding an age from the inside – living with its attitudes and prejudices – can also judge it; refusal to judge is quite as amateurish a characteristic as willingness to judge by the wrong, because anachronistic, standards".⁷¹⁸ "A good many critics demand that historians should leave the shelter of their muniment rooms and libraries in order to play their part in creating a general intellectual climate; but do they know that they are only repeating the arguments of Voltaire and the eighteenth-century philosophical historians against the antiquarians whose researches they despised?"⁷¹⁹

Acton, Butterfield and Elton wrote when historiography had been disciplinised and was largely seen, following Ranke, as a factual discipline rather than one which was written for moral instruction. Here we will outline the limits of moral judgement in the light of Ranke's views of historiography. We recorded earlier that Ranke, writing the "Preface to the first edition of histories of the Latin and Germanic nations" says "to history has been given the function of judging the past, of instructing men for the profit of future years. The present attempt does not aspire to such a lofty undertaking. It merely wants to show how, essentially, things happened".⁷²⁰ We noted, too, Iggers' and von Moltke's reliable summary of Ranke's position here: "the factual establishment of events does not yet constitute history. The historian is not a passive observer who merely records the events of the past but, rather like the poet, he actively recreates a situation."... Ranke "assumes that every individual, institution or culture

⁷¹⁷ G.R. Elton, *The Practice of History*, p. 59.

⁷¹⁸ *Op. cit.*, p. 30.

⁷¹⁹ *Op. cit.*, p. 16.

⁷²⁰ Leopold von Ranke, "Preface to the first edition of histories of the Latin and Germanic nations", p. 137.

constitutes a meaningful unity”.⁷²¹ “Ranke reminds us that history is concerned not merely with the collection of facts but with understanding those facts. But this understanding proceeds only from the intuitive contemplation (*Anschauung*) of the historical subject matter. Such contemplation for Ranke requires that the historian consciously avoid projecting his subjectivity into the subject of inquiry”.⁷²² “The ability to portray the forces of history without interjecting one’s own set of values is the core of objectivity. The historian himself will have value positions. History centers around values. Yet Ranke, like Weber, calls for a value-free understanding of these values”.⁷²³ Ranke says that objectivity is always impartiality, because they alike involve observing distinctive individuals and forces in the unique relationships which characterise them.

In the earlier section “Rival historiographies of science”, we said that where there is choice, there is judgement, and where there is judgement, there is discretion. But the exercise of discretion is not necessarily a *moral* exercise.⁷²⁴ Throughout this book we have philosophically characterised historiography in terms of pragmatic choices. We have also argued for the limitation of such choices: in many factual matters alternatives are not in practice available, so making determinate the factual decision. It will be recalled that the “factual” here not a matter which is to be categorically contrasted with the “evaluative”, for we have also argued that true descriptions characteristically involve the meeting of criteria and standards and so involve the affirmation of those criteria and standards as appropriate to the context in question. The criteria and standards themselves form part of the holistic web, just because they are not categorically to be distinguished from “purely” factual judgements, if sense is to be made of that expression. Where those criteria and standards are recognised as expressing the rules which constitute a profession or a discipline, they have implications well beyond the context in question. We partly argued this, in the section “Justification in the second-order context: Kuhn”, on the basis of material from the philosophy of law, and noted that the self-understanding of legal practitioners might be

⁷²¹ Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*, p. xlii.

⁷²² *Op. cit.*, p. xlvii.

⁷²³ *Op. cit.*, pp. xlviii-xlix.

⁷²⁴ See H.L.A. Hart, “Positivism and the separation of law and morals”.

such that law is in part *constituted* by moral considerations, by roles and practices which meet appropriate evaluative standards. If it is so constituted, that is a *contingency*.

Ranke, like the rest of us, is not a “passive observer”. When Ranke “establishes” facts, he “recreates” facts and he “understands” facts, and this understanding proceeds from an “intuitive contemplation” of the historical subject matter. He tries to see the *essence* of the matter (hence the importance of the word “essential” rather than “actual” in the translation of “*wie es eigentlich gewesen*”). When asked what states are, he says that they are “individualities, analogous to one another, but basically more independent of each other. Instead of the passing conglomerates which the contractual theory of the state creates like cloud formations, I perceive spiritual substances, original creations of the human mind – I might say, thoughts of God”.⁷²⁵ Much confusion might be stimulated by these words, particularly if we share Butterfield’s view that “we do not deny the importance of morality in life any more than we deny the hand of God in history”.⁷²⁶ To “perceive” a “substance” seems to be to observe something outside oneself. On the other hand, that something seems to be our own *creation*, and so perhaps not an external matter at all.

We have here what may seem to us as a confused Rankean mixture of Kant’s epistemology and Hegel’s spiritually idealist metaphysics. While Ranke was himself Hegelian in his belief that reality was made up of spiritual substance, something in which we all, as conscious beings, share with the World Spirit’s consciousness, it is plain that it is not this element of Ranke’s teaching which informed later generations of historians. By contrast, his words “original creations of the human mind” suggest that the human mind, in “intuitive contemplation”, makes some *input* to that reality which it comes to know and understand. This is a Kantian view: reality is what it is, as we *know* it to be, partly in virtue of the *a priori* structures which we bring to it. So, again, Ranke, like the rest of us, is not a “passive observer”. When Ranke, like the rest of us, “establishes” facts, he brings *a priori* structures – *categories* or

⁷²⁵ Leopold von Ranke, “A dialogue on politics” [1836], trans. Theodore H. Von Laue, in Georg G. Iggers and Konrad von Moltke (eds.), *Leopold von Ranke, The Theory and Practice of History*, pp. 102-130 at p. 119.

presuppositions – to his understanding. In terms of our own argument, this is to be given a pragmatic reading in terms of a holistic empiricist position. Reality is what it is, as we *count* it to be, partly in virtue of the pre-existing views which we bring to it. We have argued that it is a contingency how much choice Ranke, Hayden White, or the rest of us, really have in the structures which we bring to our understanding.

Ranke effectively thinks that the structures are *fixed*, but not just in a Kantian way, where the rational structures of the mind operate at a very general level. Rather, like Hegel, he thinks that they are fixed in a Hegelian way, involving contingent factual historical detail in a particular epoch. In the “Preface to the first edition of histories of the Latin and Germanic nations”, he expresses what he calls, without any show of difficulty, his “viewpoint”: “I regard the Latin and the Germanic nations as a unit. I reject three analogous concepts: one, the concept of a universal Christendom (which would embrace even the Armenians); two, the concept of the unity of Europe, for since the Turks are Asiatics and since the Russian Empire comprises the whole north of Asia, their situations could not be thoroughly understood without penetrating and drawing in the total Asian situation. Finally, my point of view also excludes the almost exactly analogous concept, that of a Latin Christendom. Slavic, Latvian, and Magyar tribes belonging to the latter have a peculiar and special nature which is not included here. The author remains close to home with the tribally related peoples of either purely Germanic or Germano-Latin origin, whose history is the nucleus of all recent history, and touches on what is foreign only in passing as something peripheral”.⁷²⁷

This is breathtaking, for us, as an account of a historically “objective” point of view, with the “Magyar tribes” already in the European Union, Turkey trying to join in a context of Middle Eastern instability, and the European Science Foundation funding a research program, “Representations of the Past: The Writing of National Histories in Europe”, introduced as follows:

⁷²⁶ Herbert Butterfield, “Moral judgments in history”, p. 103.

⁷²⁷ Leopold von Ranke, “Preface to the first edition of histories of the Latin and Germanic nations”, pp. 135-136.

“National history is central to national identity. A sustained and systematic study of the construction, erosion and reconstruction of national histories across a wide variety of European states is a highly topical and extremely relevant exercise for two reasons: firstly, because of the long and successful history of the national paradigm in history-writing; and, secondly, because of its re-emergence as a powerful political tool in the 1990s in the context of the accelerating processes of Europeanization and globalization. National histories form an important part of the collective memory of the peoples of Europe. National bonds have been, and continue to be, among the strongest bonds of loyalty. A genuinely trans-national and comparative investigation into the structures and workings of national histories will play an important part both in understanding the diversity of national histories in Europe and preparing the way for further dialogue and understanding among European nation-states.”⁷²⁸

So, must Ranke be incorrect in thinking that his “viewpoint” is objective? To regard the Latin and the Germanic nations as a unit, as a spiritual substance or a thought of God, is, most people will no doubt think, just plain wrong. However, this is for us an easy position to adopt, for, unless we are ourselves full-blooded Hegelians, it is the *metaphysics* which is just plain wrong, and that metaphysics, knowable as Hegel presents it as being, is inconsistent with the holistic empiricism adopted in our argument. We will, however, typically object to more than this: aware as we are of the subsequent course of European history, we may anachronistically read forthcoming horrors into the expression “Latin and the Germanic nations as a unit”, particularly if we recall that, for Hegel, “Germanic nations” includes we English-speakers. With hindsight, we see the *outcome*, we see what for Ranke could – at most – only be a *possible* future. How would Ranke have reacted, had he known this future? In outline, at least, perhaps in a less tolerant fashion than we would wish:

“Carl: You seem to favor a military tyranny.

Friedrich: How could a magnificent position ever be acquired without the voluntary and perfect cooperation of all citizens? By the invisible activity of unifying ideas the

⁷²⁸ accessed 1st June 2004 from:
http://www.esf.org/esf_article.php?section=2&domain=4&activity=1&language=0&article=363

great communities are gradually formed. Fortunate if there is a man of genius to guide them. He would never have the power to command them".⁷²⁹

As Evans says, Ranke "was a profoundly conservative figure".⁷³⁰ Some may now read Ranke, understanding his approach in terms of his view that the Latin and the Germanic nations form a unit, as bringing to bear some proto-Nazi political attitude which would be paradigmatic of a *failure* of historiographical objectivity. But it should also now be clear that the question whether Ranke's approach is objective is itself a contingency. From the point of view of his historical future, his insistence on the unity of the Latin and Germanic nations is thick with controversial and contentious moral and political attitudes. By contrast, it is plain that Ranke was able, in his own time, to *take for granted* that his readership would not find morally or politically controversial or contentious his view that the Latin and the Germanic nations form a unit for the purpose of his history, as opposed to working with a universal Christendom, the unity of Europe, or a Latin Christendom. The historian owes a duty to his or her readers to provide what he or she claims to offer, and may offer value-free historiography.⁷³¹ Yet the historian ought to be aware of the likely reader's overall understanding, which will characteristically include a grasp of moral and political matters. The historian engages with the reader, and what counts as value-free will vary with that reader. Ranke is not writing for us. It is *Ranke's* past that counts here, not *ours*. He could stress the unity of the Latin and the Germanic nations for his purpose merely a page away⁷³² from stressing *wie es eigentlich gewesen*, at the same time developing a reputation such that Heinrich Leo saw his work as involving a "timid avoidance of personal views".⁷³³ Butterfield remarks of Acton, "It disturbed him that Ranke refrained from any condemnation of the Inquisition".⁷³⁴ Other historians *counted* Ranke as writing with passionless detachment. They noticed it, and indeed

⁷²⁹ Leopold von Ranke, "A dialogue on politics", p. 118.

⁷³⁰ Richard J. Evans, *In Defence of History*, p. 16, quoting from a 1973 Indianapolis edition of Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*.

⁷³¹ See Jonathan Gorman, "Historians and their duties", *History and Theory, Theme Issue 43*, "Historians and Ethics", 2004, pp. 103-117.

⁷³² Recall that Cicero gave Herodotus the two descriptions "father of history" and "notorious liar" and did so in the very same sentence. It is hindsight, not historical context, which makes these juxtapositions problematic.

⁷³³ G.P. Gooch, *History and Historians in the Nineteenth Century*, p. 98.

⁷³⁴ Herbert Butterfield, *Man on his Past*, p. 92.

were not particularly influenced by it, despite accepting his critical approach to sources. As we saw earlier, historiography as a discipline in Germany *contrasted* with Ranke's detached European approach as other historians developed the self-understanding of German nationalism.⁷³⁵

Historiographical understanding proceeds from bringing to bear on the historical subject matter the presuppositions of the historian's holistic web of beliefs. The "contemplation" which this involves for Ranke requires that he consciously avoid projecting his *subjectivity* into the subject of enquiry. Repeating, Ranke says "to history has been given the function of judging the past, of instructing men for the profit of future years. The present attempt does not aspire to such a lofty undertaking. It merely wants to show how, essentially, things happened".⁷³⁶ Ranke's "subjectivity", those personal moral preferences and attitudes which others of his own time might find contentious or controversial, he eschews. "To introduce the interests of the present time into the work of the historian usually ends in restricting its free execution".⁷³⁷ Butterfield also had a similar practical objection to moral judgement in historiography: "moral judgments must be recognised to be an actual hindrance to enquiry and reconstruction; they are in fact the principal reason why investigation is so often brought to a premature halt".⁷³⁸ Historians are also not very good at it: "Much of the benefit which is supposed to result from the whole practice is nullified by the deplorable fact that the moral judgments of historians are so often taken at a low level".⁷³⁹

Moreover, Ranke eschews those attitudes of his own time which, with hindsight, he recognises are not appropriate to understanding the past he is dealing with. He *chooses* not to judge the past, and he *chooses* not to present what he finds as morally instructive for later generations. His choices are characteristic ones for historians, long before and long after Ranke: earlier we noted and will repeat here Anthony

⁷³⁵ Charles E. McClelland, *The German Historians and England*, pp. 63-64; Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*, p. lxii.

⁷³⁶ Leopold von Ranke, "Preface to the first edition of histories of the Latin and Germanic nations", p. 137.

⁷³⁷ Leopold von Ranke, "Preface to History of England", p. 156.

⁷³⁸ Herbert Butterfield, "Moral judgments in history", p. 103.

⁷³⁹ *Op. cit.*, p. 114.

Grafton's presentation of the choice facing humanists, with "two different notions of classical scholarship in conflict. On the one hand,... pedagogical: to produce well-behaved young men who could write classical Latin. ...On the other hand, ...scientific: to offer exact knowledge about minute details of ancient culture and to transmit sophisticated techniques for resolving difficulties in the ancient sources".⁷⁴⁰ "One set of humanists seeks to make the ancient world live again, assuming its undimmed relevance and unproblematic accessibility; another set seeks to put the ancient texts back into their own time, admitting that reconstruction of the past is difficult and that success may reveal the irrelevance of ancient experience and precept to modern problems".⁷⁴¹ It is clear what Ranke's choice was.

Iggers and von Moltke commented that "Ranke's philosophy of value, teaching that every individual and state must be understood in terms of its own standards and that 'every epoch is immediate to God', is no longer credible to many historians after the political catastrophes of the twentieth century".⁷⁴² On the contrary, historians later to us, looking back on us looking back at these catastrophes, and plausibly using Ranke's philosophy of value, will understand why *our* own standards might not allow us to escape judging the wickedness of, most obviously, the Holocaust. We might think, for example, that the Holocaust was a matter of such evil that it would be morally wrong to adopt a dispassionate view of it. On the other hand, if our understanding of the discipline of historiography is such that it requires the historian to adopt a dispassionate distance, then we might well think that historians should not deal with the Holocaust at all. As earlier argued, the discipline of historiography does not have history all to itself, and if it is conceived as a dispassionate discipline then it is just as well that that is so. Another approach or discipline would be required; although perhaps "there is no morally honorable way of writing about the Holocaust, since all

⁷⁴⁰ Anthony Grafton, *Defenders of the Text*, pp. 25-26.

⁷⁴¹ *Op. cit.*, pp. 26-27.

⁷⁴² Georg G. Iggers and Konrad von Moltke (eds.), Leopold von Ranke, *The Theory and Practice of History*, p. lxviii.

writing about it will enlarge our universe of moral depravity”⁷⁴³. Historians later to us might think differently. They can judge us, *if they choose*.⁷⁴⁴

The argument so far given allows the *possibility* of Ranke’s kind of objectivity and impartiality – “unnerving” impartiality, as Veronica Wedgwood put this approach.⁷⁴⁵ There is no need to assume that Ranke’s dispassionate distance requires that some objective “view from nowhere” is available which defeats the claim that all discourse is ideological. There is no need to assume that a “view from nowhere” is required if one is to avoid making moral judgements. Moreover, there is no need to assume that a “view from nowhere” is required if one is to make moral judgements properly. Ranke’s “dispassionate distance” requires, not a “view from nowhere”, but a view from *somewhere else*, and this is readily available to historiographical hindsight. Moral distance, seen as dispassionate, is readily achieved by what is the contingent discounting of one’s passions over time,⁷⁴⁶ whereas historians also have hindsight knowledge of consequences later to the situation being judged. Historians are in a peculiarly privileged position for moral judgement. Ranke is factually right in holding that history gives historians “the function of judging the past, of instructing men for the profit of future years”.⁷⁴⁷

Should historians turn away from this privileged position? What these arguments do not show is whether Ranke was *right* to do so. The issue is not so much a philosophical question whether historians are somehow obliged to judge, or whether historians inevitably, if unknowingly, impose their subjective evaluations. Rather, it is

⁷⁴³ F.R. Ankersmit, “The ethics of history: from the double binds of (moral) meaning to experience”, *History and Theory, Theme Issue 43*, “Historians and Ethics”, 2004, pp. 84-102 at p. 97.

⁷⁴⁴ Earlier we quoted from Simon Schama, “Historians have been overconfident about the wisdom to be gained by distance, believing it somehow confers objectivity, one of those unattainable values in which they have placed so much faith”. Realistically understood objectivity is no doubt unavailable, but historical hindsight may be as good a source of wisdom as one might have. Simon Schama, *Citizens*, p. xiii.

⁷⁴⁵ Veronica Wedgwood, “Miss Mangnall of the Questions”, in *The Collected Essays of C.V. Wedgwood* (London: Fontana, 1987), 92-95 at 93.

⁷⁴⁶ Says F.H. Buckley, “I feel less angry about the [Maryland, 2002] snipers than I did a year ago, much less upset about O.J. Simpson than I did ten years ago. And I feel positively benign about Bluebeard the Pirate”. F.H. Buckley, “Are emotions moral?,” *The New Criterion* 22, January 2004, printed from <http://www.newcriterion.com/archive/22/jan04/emotion.htm>

⁷⁴⁷ Leopold von Ranke, “Preface to the first edition of histories of the Latin and Germanic nations”, p. 137.

a *moral* question whether historians ought or not to judge. This presupposes that they have a choice here, and it is plain that they do. But what may seem also problematic is whether that moral question is one to which an absolute or universal answer, true for all time, is to be given.

Against this, *what we count as a moral issue is a contingency*. Consistently with the pragmatic holistic empiricist position, it can change over time. Consistently with this, we can *refuse to allow* it to change over time. We can *count as absolute* certain moral standards, and write our historiography accordingly. We can, in principle, *decide* whether, with Ranke, all times are of equal value in God's eyes, and so should be for the historian. We can, indeed, "decide" whether God exists, but note that that would not necessarily mean that we "created" God, for perhaps there is, for us, *no alternative* which is holistically supportable. Said Acton, "The men who plot to baffle and resist us are, first of all, those who made history what it has become. They set up the principle that only a foolish Conservative judges the present time with the ideas of the past; that only a foolish Liberal judges the past with the ideas of the present. ...History, says Froude, does teach that right and wrong are real distinctions. Opinions alter, manners change, creeds rise and fall, but the moral law is written on the tablets of eternity".⁷⁴⁸ Or we may accept the pluralist persuasion of Isaiah Berlin that it is wrong, totalitarian, to unify different moral answers into a single moral point of view.⁷⁴⁹ There are no tablets of eternity, we may think. We can *decide* whether the past should be understood as those who lived in it understood it. We can *decide* whether, in judging the past, we should use the morality of those contemporary with the events or our own moral standards. We can even decide not to decide. The only constraints upon our decisions are moral and social so, in all these things, recognising the importance of continuing to share our world, we must ensure the moral and social responsibility of our historical understanding.

⁷⁴⁸ Inaugural lecture on the study of history, 1906.

⁷⁴⁹ Berlin's position involves both factual and moral pluralism. A helpful explanation is James Cracraft, "A Berlin for Historians", *History and Theory* 41, 2002, 277-300.

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