Towards a Realist Philosophy of Historiography

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Abstract

In this thesis I argue for historiographical realism: that by and large, historians are able to provide accurate knowledge about a mind-independent past. I take a two-pronged approach to this task: by rebutting several 'standard' anti-realist arguments in historical theory as well as putting forward a positive account – based on a rational reconstruction of historical practice – of why we are entitled to take historical accounts as more often than not accurately reflecting what occurred in the historical past. The philosophical picture of historical practice presented here is designed to replace the old, discredited 'naive realist' theory of historical practice with a more nuanced and philosophically literate account.

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Dedicated to the memory of :

James Davies-Shuck 1978-2016

David Timmins, 1952-2019

I declare that this thesis is a presentation of original work and I am the sole author. This work has not previously been presented for an award at this, or any other, University. All sources are acknowledged as References.

Introduction

In this thesis, I will put forward a realist philosophy of history to replace what has come to be called the 'naive realist' view of historiographical practice which has been discredited in historical theory. In a nutshell, my view argues that by and large historians are successful in their task of producing accurate knowledge about a mind-independent past and that therefore philosophers of historiography should take as their starting point the fact that current historical practice is successful, and philosophically account for this.[1] This might seem to be a fairly common-sense position: but as we will see, it is not one generally assumed in the philosophy of history. In fact, it is not exaggeration to say that a majority of philosophers of history believe that the acquisition of accurate knowledge about the historical past is epistemically problematic at best and impossible at worst. Why they make these claims forms the first part of this introduction.

The Great Divide

With the exception perhaps of the early twentieth century (ironically, when there wasn't much philosophy of history to speak of) there has generally been a divide between practising historians and philosophers of history with regards to the success of historiographical practice. It is no exaggeration to say that the majority of practising historians are realists about the historical past; James Cacraft is correct when he stated in a recent paper that "historians could not be historians without an implicit commitment to what in modern philosophy as well as in everyday life is called realism" (Cacraft 2015, 54).[2] This would explain why the 'linguistic turn' in historical theory failed to have much of an impact on

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[1] One reaction to this might be: 'isn't this begging the question? How do you know that historiography is successful?' However, we are entitled to a presumption of realism here. To elaborate a little: if we have well-founded beliefs about X, then as a philosopher, this creates a presumption that the beliefs in question are true. Arguments against realism come in three forms: 1) General scepticism; 2) Denial that beliefs about X are in fact well-founded; 3) Denial that there can be truths about X. This thesis deals with credible versions of 2) and 3) – and indeed, Chapter 1 dismisses non-credible versions that fall into 1). It is methodologically acceptable to set aside general scepticism, and indeed this is what I will be doing.

[2] Later on in the same piece he writes that "historians are by the nature of their business what philosophers would call common-sense realists." (58).

historical practice; the approaches gathered under that umbrella generally denied that the historical past could be recaptured in any kind of authentic way. Little, if any, 'postmodernist history' was or has been written despite the anti-realist tendencies of historical theory for much of the past fifty-or-so years.[3] The fact is that to commit to being a historian – which entails, among other things, to spend hours wading through dusty archives engaged in what can be mind-numbingly tedious research – requires a belief that one does so because one is engaged in the process of finding out *what actually happened*.

As alluded to however, by contrast much philosophy of history from the early twentieth century onwards has tended towards idealism and/or anti-realism about our knowledge of the historical past. Even those philosophers of history who claim to be inclined towards realism have ended up producing accounts of historical practice that few practising historians would be inclined to assent to.[4] We will examine why this has been the case momentarily; but ultimately, the upshot of this divide has been a classic dialogue of the deaf – if indeed we can even use the term 'dialogue' – between historians and philosophers of history. To use an analogy of Jenny Teichmann's, just as bumblebees fly about daily, impervious to the entreaties of aerodynamic theorists who insist according to aerodynamic theory it cannot be done, so historians continue to produce accurate knowledge about the historical past, oblivious to theorists who assure them the past-in-itself can play no part in historical accounts (Teichmann, 1993, 59).

History as a discipline has always received something of a rough ride from philosophers: one can go all the way back to Aristotle's claims that history was inferior to poetry as marking the beginning of the troubled relationship between the two. The twentieth century provided "repeated instances of philosophy coming forward to serve as conceptual warden for historiography, uninvited and unappreciated by historians" (Zammito 2009, 64). From Hempel (1942) to Jenkins (1991), "philosophers have never felt the least hesitancy in explaining what historians were doing (or better still, *ought* to be doing) even if historians did not know it" (a strong form of externalism if there ever was one) (Zammito, 2009, 64).

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[3] Writing about semantic anti-realism, Timothy Williamson came up with a line that perfectly describes what happened with postmodernist historical writing: said postmodernist theorists "preferred to polish their formulations of the grand programme rather than getting down to the hard and perhaps disappointing task of trying to carry it out in practise" (Williamson, 2006,180).

[4] Frank Ankersmit and Aviezer Tucker are two names that immediately spring to mind.

It is unsurprising then, that historians have not felt particularly well-disposed towards philosophers, given some the treatment they have received. However, the blame for the divide between history and philosophy cannot entirely be placed in the lap of the philosophers. At the best of times, historians have a tendency to be suspicious of the philosophical (though perhaps not without reason. Recently Kerwin Klein wrote that many who identify as working historians "treat theory as a mysterious black box filled with occult instruments. From time to time, we might run over and pull out some specific theoretical tool and then scurry back to history to see if it can be applied" (Klein, 2011, 32). The comment is a little harsh, but not without truth. For many historians, speculations on the philosophy of history merely detract from time that could be better spent researching and writing it.

A more pertinent reason on the side of the historian to not engage with theory is that there is an asymmetry vis-à-vis what one needs to know to be a successful historian and a successful philosopher of history. A practising historian needs little or no philosophy of history in order to be successful at his or her task; for example, one does not need to have an opinion on the ontological status of colligatory concepts in order to write a good biography of Ted Heath. But the reverse isn't true: in order to write an account of historiographical theory that is to be of any relevance, a philosopher has to have some notion of how historical research and writing is actually practised.

Philosophers of history however, have tended not to see things this way. Not only are they confident that no knowledge of historical practice is needed to undertake historical theory, but they have sought to actively discourage historians in indulging in theoretical reflections upon historical practice.[5] One can understand why this would be the case, as even those who profess to have an understanding of actual historical practise tend to paint a picture that is far removed from what historians actually do. Of course, one can find these kind of asymmetries in other areas – before the historical turn in that area, philosophers of science rarely troubled themselves to look at the historical record to see what scientists actually did.[6]

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[5] A recent example is provided by Kuukkanen: "The point is that the job of historians, like that of scientists, is to find the best possible characterizations and constructions of their object world and not to ponder primarily what the relation of historiography and its cognitive products is to historical reality in general. That is a job for the philosophy of historiography" (Kuukkanen, 2015, 199-200).
[6] Imre Lakatos notoriously argued that the actual history of science should be replaced with a rational reconstruction of how science *should* have unfolded (Lakatos, 1970, 107).

The philosophy of history has thus become (and perhaps always was) something of a cottage industry. Hayden White is universally acknowledge to be one of the most influential historical theorists of the past fifty or so years; but influential where? Certainly not in historical practice, where the influence of White has been minimal; or at least, I have yet to come across a historian who has introduced their work by setting out which literary trope they have used to emplot their account with. To an extent it is reminiscent of pre-historical turn philosophy of science, where accounts proceeded on the basis of what philosophers thought science ought to be, rather how it actually was: prescription rather than description was the order of the day. Something like a 'historical turn' is desperately needed in historical theory like that which occurred in the philosophy of science – i.e. philosophers of history looking at the practice of history in the way philosophers of science started looking at actual scientific practice.[7]

How then, does the present thesis propose to remedy (or partially remedy in any case) the above state of affairs? In a nutshell, I propose to provide a philosophically respectable realist philosophy of historiography that takes as its starting point history as it is actually practiced. Thus, this thesis will provide a rational reconstruction of historical practice, and will also *philosophically gro*und this practice. I feel I am in a unique position to carry out this task, possessing degrees in both modern history and philosophy.

This invites the question as to what it to be a 'realist' about historical practice? Of course, 'realism' is a term that has many senses: there are so many variants of realism that a philosopher who announces that they are a realist without further explanation "has done little more than clear their throat" (Burgess & Burgess, 2014, 68). In order to avoid running through the various senses of the term, I shall stipulate here what I take historiographical realism to consist of. A realist about historical practice holds that our accounts of the historical past by and large accurately reflect or capture what actually happened; and furthermore that such a past plays a sizable role in constraining and constituting our historical accounts. When historiographical practice is successful, it provides accurate knowledge of the historical past. Furthermore, we can have different views of the historical past without it

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[7] The irony is of course, that just at the point scientific methodology turned to history, historical theory turned away from science and towards literary theory.

being the case that if we change our view of the historical past, this does not change *how things actually were* in that past.[8]

I thus concur with W.H. Walsh's remark that "if you are an opponent of historical realism you must be a supporter of historical idealism." (Walsh, 1977, 66). As with a definition of realism, one could devote an entire thesis to just what historical idealism consists of. A thumbnail definition to tide us over here runs as follows: historiographical idealism takes the form of stating that the past can play no part in historical accounts, and that said accounts are simply constructions from the present. The past can play no role in the construction of these accounts, because it is not available to perception and/or because it no longer exists. It follows from this – indeed, I do not see how it cannot – that whatever forms the content of our historical accounts, these accounts do not, and cannot, reflect what actually took place in the past. Needless to say I reject such a view, and at various points in the thesis some of these arguments will be examined and rejected.

Outline of the Thesis

The thesis will take the form of a rational reconstruction of historical practise. After an opening chapter briefly despatching some of the more outlandish idealist claims about historiography, we will start from the bottom up, looking at how historical facts are established, and then work our way up – progressing from issues dealing with facts to issues that come with the integration of these facts into historical explanations and accounts. The thesis will consist of, for lack of a better phrase, positive and negative aspects. The 'negative' will consist in rebutting anti-realist arguments with regards to our knowledge of the historical past. These are largely (but not exclusively) related to the form that historical accounts take – for instance, the idea that historical accounts are indecomposable entities that do not isomophically map onto the past; the notion that adopting a narrative form of writing automatically distorts the informative content of said narratives, and that historical accounts and their contents cannot be said to 'refer' to the past in itself.

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[8] As Charles Travis puts it in a different context, as realists we want our historical accounts to be answerable to how the past itself was; what the idea of 'answerability' demands is that whether a given stance on the historical past is correct or not "cannot depend on how, or what, or whether, *one* thinks about the way things in fact are" (Travis, 2008, 110).

The positive aspect will consist in a sketch of what historical practice actually entails: something that is almost entirely missing from current accounts of historiography. In particular, I will argue for a *two-stage* model of historical practice: that is, a distinction between the stags of historical research and historical understanding. This is in contrast to the increasingly popular 'holist' view of historiography – the idea that historical research and understanding are one and the same process; that historical facts are constituted at the same time as the historian pens his/her account – encapsulated by Goldstein's phrase "there are no leftover statements" (Goldstein, 1986a, 94). In contrast, I argue in Chapter 3 that (among other things) that there is a pool of verified historical facts in any given field that the historian can and does avail themselves of. This heads off the incommensurability that the holist account implies, and shows how there can be legitimate disagreement based on common ground between competing historical accounts.

Intermittently over the past few decades debate has broken out over whether there is a need for a 'philosophy of history' as such, or if historiographical epistemology can be subsumed under our epistemology of how we gain information about the present. Idealists about historiography seem unsure as to where they stand on this issue. My thesis argues that while there is a need for a 'philosophy of history', it does not need special pleading, in the sense that we do not (contra Ankersmit and the early Goldstein) need special categories such as 'historical truth', 'historical reference', and so on to apply to historiographical works. The philosophical tools we need to construct a realist philosophy of history are already at hand, so to speak – they have just perhaps not been utilised. In chapter 4 for instance, I develop a theory of reference for colligatory concepts[9] – an area that has been neglected by philosophers of language generally, as it is an area that really only concerns historiography.

Frank Ankersmit has argued that philosophy of history should take as its starting point history as it is actually practised (Ankersmit, 2012, 114). I wholeheartedly concur, and have followed this precept in the writing of this thesis. In instances where a realist explanation conflicts with historical practice – most notably in chapter 8, where the most straightforward 'realist' move would be to opt for a model of verisimilitude – I have chosen not to abandon the commitment to basing this work on what historians actually do. It would, quite frankly, have been easy to

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[9] For the uninitiated: W.H. Walsh coined the term 'colligatory concept' to describe terms that historians use to describe historical change: terms which relate a group of events by a common idea. Colligatory concepts thus identify a general relationship among singular events.

concoct a plausible version of verisimilitude; however, given that in large part that many of the problems of philosophy of history have stemmed from a failure to attend to what historians actually do, it would have been unforgivable to take this route here. In this thesis, historical practice always has veto.

Historical theory in general is in a strange place at the time of writing. Following the decline and fall of postmodernism in philosophy of history, no one key theme or idea has emerged to take its place. At the time of writing, a movement seems to be underway to revive the 'analytic philosophy of history' of the 1950s and 60s.[10] I am genuinely baffled as to why this is occurring; to paraphrase Virginia Woolf, one of the damn things is enough. I see this thesis as making a crucial intervention in historical theory at this time; a realist alternative is desperately needed to some of the current views of historiography. While I accept that the old 'kick the stone' empiricism which has generally constituted historiographical realism is bankrupt, this does not mean that realism is not a viable route for philosophy of history – only that a more philosophically respectable version is needed. This thesis thus provides a more sophisticated version of realism.

Chapter Outlines

Taken as a whole, this thesis divides into three sections. The first deals with what we might call the building blocks of historical practice; the ontology of the past, the facts which historians utilise in their accounts and how these facts are established/ verified. The final section looks at issues that relate to historical accounts as a whole as opposed to their sentential components; specifically how historiography can be said to progress, the epistemic status of the narrative form, and issues relating to holism. Bisecting these is a middle section which deals with some fairly technical issues relating to the reference and ontology of colligatory concepts. I have appended a brief note in-between Chapters 3 & 4 explaining why this brief detour is taken. As stated a moment ago, the thesis is structured to approach historiographical theory from from the ground up, as it were. Thus, we begin with the building blocks of historiography, historical facts, and then move on up to deal with issues

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[10] For an introduction to this, see the essays collected in Brzechczyn, 2017.

involving various aspects of the composition of historical accounts, and finally finishing the thesis with chapters devoted to issues concerning 'whole' historical accounts.

In Chapter 1, I undertake some Lockean rubbish-clearing by dealing with some common arguments against historiographical realism that can quickly be dismissed. These tend to rest on a conflation of the metaphysical past with the historical past. The two of course, are different things, and this conflation accounts for some of odd things that philosophers of history say, such as that historiography is problematic because the past 'changes', and so forth. I also deal with the old shibboleth that historiography is epistemologically hamstrung because historical events are not available to perception. This chapter's contribution to the overall realist thesis is that it quickly dismisses several anti-realist arguments that have been floating around historical theory in various forms for quite a while.

In Chapter 2, I outline an ontology of the past. Although it is arguably possible to practise history without having a clearly defined ontology of the historical past, a philosopher of history has no such excuse. I will therefore outline an ontology of history to the effect that historians investigate actions, events, and states of affairs. I will also tackle the problem of historical facts; the problem being that these are undoubtedly the backbone of historiography, yet remain woefully under-defined in terms of what they exactly are. I argue that, as historians use the term, a 'historical fact' is a true sentence reflecting a historical state of affairs (in other words, a non-standard usages of the term 'fact').The chapter's contribution to the realist thesis is that it outlines a workable and plausible ontology of the past within which historians can work within. This provides an alternative to the anti-realist view that there is no workable ontology of the past (and by extension, the present): that the past is a kind of amorphopus lump in which there are no joints to carve.

Chapter 3 is devoted to what, *pace* Carr, I have termed the club of historical facts. I argue that in any reasonably well-established historical research area, there is an established corpus of facts that historians working in the area are entitled to draw upon as 'free moves' (to use a term of Brandom's). Not only are these facts so well-established as to be taken as 'given' by the historian, but by and large they are also significant facts that the historian needs to include in any competent account of the subject in question. Moreover, it is the use of such facts that ensures the commensurability of historical accounts, and thus wards off any kind of global

incommensurability – and incommensurability is one of the main threats to historiographical realism.

Chapter 4 looks at the reference of colligatory concepts. Colligatory concepts are expressed by event names that tend to crop up only in historiography – things such as 'the Renaissance', 'the First World War', 'the French Revolution', and so on. By and large questions relating to the semantics of these names have been neglected by philosophers of language, due to the aforementioned fact that these are terms that generally do not crop up outside of historiography. This chapter offers some much needed clarification of these terms, with regards not just to how we secure the reference of events like the Renaissance, but also what semantic category these terms fall into – are they proper names or definite descriptions? (I argue that they should be classed as proper names with unorthodox features). This chapter contributes towards the overall thesis of historiographical realism by demonstrating that we can indeed secure reference to such entities.

Chapter 5 looks at the referents of colligatory concepts, in the sense of what kind of ontological status these events have. I argue that, contra anti-realists who state that there are no such 'things' as the Renaissance, French Revolution, etc, that these are events that have/had ontological existence, and thus there is nothing 'unreal' or 'fictional' about such entities. In a nutshell, I argue that macro-events (as I term them) such as these are emergent entities that result from state of affairs and smaller events, but are not reducible to them (in other words, this is an anti-reductionist position). The idea that colligatory concepts are 'merely' organising devices is a large part of the historiographical anti-realist case; thus, to show that they in fact have some kind of ontological existence works in the service of realism.

Chapter 6 moves onto issues relating to 'whole' historical accounts. In this chapter I address the issue of historiographical progress. Most historians would – correctly – argue that historical accounts progress, in the sense that later accounts of say, the First World War are better than their predecessors. To put it another way, we know more about the First World War in 2019 than we did in 1969. But what is it that motivates this progress? In this chapter I reject the notion of verisimilitude (a route that a realist might be expected to go down), on the grounds it is not consonant with historical practice, and instead put forward an institutional account of historiographical progress, upon which progress in historiography is guaranteed by

the epistemological standards set up by the institutional framework within which historiographical practice takes place. Progress is clearly a fact of historiographical practice; thus, any kind of realist account needs to explain how this comes about.

Chapter 7 looks at the issue of holism in historiographical accounts. I address two potential concerns vis-à-vis holism and realism in this chapter; the first stemming from issues relating to the somewhat non-standard version of holism that is prevalent in historiography, and the second relating to the standard linguistic view of holism. If followed to their logical conclusions, both have the potential to lead to incommensurable historical accounts, which would of course be a hammer blow for realism.[11] Fortunately this is not the case, and in this chapter I show how a holist-inspired incommensurability can be avoided.

Chapter 8, the final chapter, looks at narrative. In the past forty years or so – what in historical theory have come to be known as the 'linguistic turn' years – narrative anti-realism has been at the forefront of philosophy of history. In a nutshell, this is the idea that the narrative form is indispensable to historical writing; but this form *necessarily* distorts historians' attempts to convey what the past was actually like. As one would expect, I argue that there is no cause for concern here; that the narrative method of presentation – which in any case is not *essential* for historical accounts (though it has its merits) – need not give us any cause for epistemological alarm. We can still use the narrative form without entailing any kind of anti-realism.

A Brief Terminological Point

One final point: I use the term 'historiography' and 'historiographical theory' in this thesis to refer to what used to be called 'critical' philosophy of history. While I'm aware this is perhaps a slightly non-standard use of the term 'historiography' – which traditionally refers to the history of historical practise – the ambiguity of the word 'history' itself means that 'philosophy of history' is somewhat problematic these days. When one tells someone in a philosophy department that one is working in philosophy of history, one often gets a response along the lines of "ah, you mean Hegel and all that." This of course, is manifestly *not* what one means; so following Aviezier Tucker, I designate historical theory under the emblem

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[11] This is not to say that there *cannot* be incommensurable accounts; but incommensurability is not *necessarily* entailed.

'historiographical theory' – theory relating to the epistemic practices of historians (Tucker, 2004, 1).

1) The Past and the Historical Past

This opening chapter aims to undertake some Lockean clearing up, as it were, in dealing with a persistent source of confusion in writing in historical theory: this being a failure to make a distinction between the past and the historical past (which I will explain momentarily). On first blush one may think they are one and the same; and indeed, the historical past is contained within, or is part of, the past. However, there are important differences between the two. Furthermore, arguments for anti-realism tend to take the form of arguments that have their roots in metaphysical assumptions about the past as opposed to epistemological ones about the historical past.[12] Questions about the past - the spatio-temporal status of events and actions which have now perceptually passed from human purview - is a question for metaphysics. Questions about the historical past - how we recover information about past human actions and events and interpret this information - belong to the domain of epistemology. However, as we will see in this chapter, several historical theorists have based their arguments to the effect that we must necessarily be anti-realists about historical knowledge on the metaphysics of the past, as opposed to the historical past.

Of course, epistemology can never be entirely divorced from metaphysical and ontological questions: indeed, the very idea of realism involves a metaphysical thesis that there exist entities independent of our knowledge claims that can render our statements in a given subject area true or false. The historian – as we shall see in our next chapter – must possess some kind of idea of the ontology that he/she thinks the past possesses before they embark on historical practice. However, historians studying the historical past are quite entitled to be agnostic on many issues concerning the metaphysical past. One does not need to take a position on the merits of growing-block vs. presentist theories in the philosophy of time to be able to write a first rate biography of Lenin; indeed, one can subscribe to either and it have little effect on historical method.

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[12] As Raymond Martin wrote in 1993, philosophers of history (among which he included himself) "too often become preoccupied with questions, such as that about the ontological status of the past, that, however legitimate and interesting, are not responsive to the methodological problems that arise when one tries to find out what happened historically and what it means that it happened" (Martin, 1993, 29).

This chapter then, will outline some of the long-standing and misplaced anti-realist arguments for historiographical anti-realism that have their basis in exclusively metaphysical viewpoints. In the first section, I will further clarify the distinction between the past and the historical past, and in particular show how historical theorists have not always been clear on the division between the two. The remaining four sections will look at various arguments for historiographical antirealism that stem from taking such a view as their starting point: specifically, a) the metaphysical status of time, b) the fact that the past is not available to perception, c) the idea that the past changes, d) the nominalist view that the world (and hence the past) is an unstructured lump. I will argue that all of these views fall outside the purview of historiographical epistemology, as they pertain to the metaphysics of the past as opposed to the epistemology of the historical past.[13]

(i)   Historical Facts and Facts about the Past.

Historians are interested in the historical past; which is, by definition, a *human past*. It is no accident that the era of the dinosaurs – or indeed any of the periods before homosapiens walked the earth – is referred to as 'pre-history'. There are many facts about the past that historians are simply not interested in, as they not fall within the purview of the human past. For example, on the morning this chapter was first drafted, in its haste to reach a student feeding the ducks, a mallard (hilariously) flew into a builders cage situated near the lake where I live. While this is undoubtedly a fact, and furthermore a fact of the past, it is not a fact of history. True, we do speak of such things as 'the history of the universe and 'the history of the dinosaurs: but we leave the investigation of these to astronomers and palaeontologists respectively: we do not expect the 'ordinary' historian to deal with them. As W.H. Walsh puts it, "we can safely say that except where we are dealing with human beings there is no history proper"(Walsh 1942, 130).

Yet this is not a distinction that has always been appreciated in historical theory or by historians. Famously, E.H. Carr got into tremendous trouble for making the distinction between the past and the historical past in *What is History?* In the opening chapter of the book, Carr talked about the difference between facts of the past and 'significant facts' – i.e. the facts of history. As an example of the former, Carr remarked that "the fact that you

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[13] Of course, only some metaphysical issues are irrelevant to historians.

arrived in this building half an hour ago [the book started life as a series of lectures] on foot, or on a bicycle, or by car is just as much a fact about the past as the fact that Caesar crossed the Rubicon. But it will probably be ignored by historians" (Carr, 1962 11). Further on, he gave an example of how a fact of the past was transformed into a historical fact, thus gaining membership to "the select club of historical facts": the (now infamous) death of the gingerbread vendor at Stalybridge in 1850.[14] The point that Carr makes then, is that historians are only interested in facts about the human past; and furthermore, in *significant* facts at that.

How is it that a "mere fact" is transformed "into a fact of history"? In a set of memoirs, an eyewitness recorded that at Stalybridge Wakes in 1850, a gingerbread vendor was kicked to death by a mob. Was this a fact of history? A year prior [to his delivering the Trevelyan lectures], Carr would have said 'no', based on the original citation alone. However, in the intervening period, Dr Kitson-Clark cited it in his Ford lectures. Was this enough to transform it into a historical fact? On its own, no. The fact's present status "was that it has been proposed for the select club of historical facts", and that it now awaited "a second and sponsors." Carr remarked that

> it may well be that in the course of the next few years, we shall see this fact appearing first in footnotes, then in the text, of articles and books about nineteenth century England, and that in twenty or thirty years it may well be a well-established historical fact. Alternatively, no-one make take it up, in which case it will relapse into the limbo of unhistorical facts about the past from which Dr Kitson-Clark has gallantly attempted to rescue it (Carr, 1962, 12).

Which path the fact in question takes will depend upon "whether the thesis or interpretation in support of which Dr Kitson-Clark cited this incident is accepted by other historians as valid and *significant*" (Carr, 1962,12). (my italics) The status of the death of the gingerbread vendor as a historical fact "will turn on a question of interpretation. This element of interpretation enters into every fact of history" (Carr, 1962, 12-13).

Carr's position here infuriated many readers, who took him as saying that in deciding what constitutes a significant fact, that the historian in effect *creates* the past. Geoffrey Elton opined that

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[14] Coining the phrase "club of historical facts" undoubtedly raised the ire of commentators, as it was no doubt intended to. In *The Critical Historian*, Kitson-Clark wrote that "the techniques of historical criticism should be applied to any account of matters that have occurred in the past to which any importance is attached", and no-one batted an eyelid (Kitson-Clark, 1967, 1).

this really is an extraordinary way of looking at history; worse, it is an extraordinarily arrogant attitude to both the past and to the place of the historian studying it. A man was kicked to death in 1850; this is a fact, an event that took place and nothing now can either make or unmake….the event can be known, and that is all that is required to make it a 'fact of history'. Interpretation, or general acceptance of a thesis, has nothing to whatsoever to do with its independent existence (Elton, 1967, 56).[15]

Elton is correct; but his point misses the target because he confuses an epistemological thesis on Carr's part with an ontological one. Elton takes Carr as saying that something happened *only if* a historian says it happened; but this is manifestly *not* the point Carr wants to make. If, for whatever reason, the fact about the death of the gingerbread vendor "relapses into the limbo of unhistorical facts about the past", this is not to say that in some way it did not occur: it is still a fact about the *past*, but not a *historical* fact. Thus, while Elton is correct in saying that interpretation has nothing to do with the independent existence of a fact, to say so here completely misses the point, as Carr is making a historiographical point, not an ontological one.

However, one problem that Carr did not address is that what is classed as significant may well change over the course of time. On Caesar's crossing of the Rubicon, Carr remarks that "it is the historian that has decided for his own reasons that Caesar's crossing of that petty stream, the Rubicon, is a fact of history, whereas the crossing of the Rubicon by millions of other people before or since interests no-one" (Carr, 1962, 11). Two decades later, this was no longer the case: for example, "social historians would have been interested in precisely in those millions of ordinary people and the patterns of communication their crossings and recrossings of the river Rubicon revealed" (Evans, 1997, 172).

Facts then, can take on significance in hindsight that was not apparent at the time. What we may perhaps wish to say then, is that there is always the potential for a fact about the past to acquire some significance that will elevate it to a fact of history. To take my own example from earlier: if it turned out that the crashing of the mallard into the builder's cage distracted an academic who was cycling by, causing him to fall in the lake and drown, then the fact of said mallard crashing into the steel mesh would become a fact of history as opposed to a fact about the past. Indeed, here the death of Rinka the Great Dane in 1975 springs to mind; dogs

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[15] In a similar vein, Arthur Marwick wrote that Carr's passage was "amusing, rhetorically satisfying, but complete rubbish." Whether the death of the gingerbread vendor is fact of history depends on the reliability of the evidence, "not a lot of junk about being proposed and seconded for a club" (Marwick, 2001, 155).

die all the time, but the shooting of this particular one started a sequence of events that lead to Jeremy Thorpe having to resign as the leader of the Liberal Party.

Furthermore, it is not just in history where the idea of 'significant' facts applies. In his recent book on scientific realism, Anjan Chakravartty notes that the notion of 'truth' in science is slightly qualified: *by itself*, truth matters little, in the sense that via empirical observation, one may generate many statements that are true, but entirely trivial. Scientists are interested in "important truths, viz. ones that have scope and specificity that are appropriate to the inquiries they undertake, and that raise and answer questions of significance" (Chakravartty, 2007, 220). One might argue that there is something of a parallel to history here; there are facts about the world, and then there are scientific facts.

In sum then, there is a difference between a fact about the past, and a historical fact. As we have seen, historians thought that when Carr pointed this out, he was in effect saying that historians somehow 'create' the past; when in fact, he was merely pointing out that whether a fact is significant is rooted in epistemology, rather than metaphysics. Whether written about by historians or not, it remains a fact of the past that the poor gingerbread vendor at Stalybridge was kicked to death. Whether it becomes a significant fact however, is a question that falls within the epistemology of historiography; not metaphysics. Indeed, I will make use of Carr's conception of a historical fact in Chapter 3.

(ii) The Ontological Status of the Past

One of the reasons that historiography has always seemed epistemically problematic to some is that the events of the past that historians write about are not available to perception. The view that many anti-relaiusts take is that the past is dead and gone, but historians have a tendency to write about the past as though it still exists in some form or another. We can call this, for, lack of a better term, a 'spatialised' view of the past. For example, Richard Evans – responding to the debate about our aforementioned gingerbread vendor - remarks that "whether or not a historian has carried out [an] act of verification [of a fact] is irrelevant to its facticity: *it really is there* entirely independently of the historian" (Evans, 1997, 77) (my italics). Carl Becker – summing up a view he was about to attack – talked of the "comforting sense" that the past lies behind us "like a stretch of uneven country we have crossed; and it is

difficult to avoid the notion that one could easily, by turning around, walk back into the country of the past" (Becker, 1955, 327).

Such a view is seen as somewhat naive. John McDowell has argued that the realist view of "the reality of the past can described, with only the mildest caricature, as the idea of another place, in which past events are still occurring, watched perhaps, by God" (McDowell, 1998, 313). On the side of historiographical theory, R.G. Collingwood wrote that "we commonly suppose, in our more illogical and slipshod moments, that the past still exists and lies somewhere concealed behind us, and that by using appropriate instruments and methods we can discover it and investigate its nature" (Collingwood, 1946/1994, 384). In a similar vein, Louis Mink writes that "events don't withdraw from the present to the past as an actor withdraws from the stage to the wings", and that the past is not something that is "actual but separated from us by time, as Kilimanjaro is actual but separated from us by space" (Mink, 1987, 94). These are all anti-realist points of view.

Such views tie into Paul Roth's characterisation of the realist view of historiography, which he has dubbed the "woolly mammoth" view. Said view is based on an article that Roth read on the discovery of a fully-preserved woolly mammoth found in an Arctic region, embedded in the ice. The realist view of history see past events as analogous to this: "as past, events become forever locked into some fixed configuration, awaiting a historian to come along and chip away the excrescences of time so that "the past" can stand revealed in all of its original glory" (Roth, 2012, 314). But for Roth *et al*, the past is not a place that be revisited; the oft-quoted phrase that "the past is a foreign country" should not be taken literally. Mink characterises historians response to this as being to simply to carry on "with their work, humming Ranke under their breath" (Mink, 1987, 153).[16]

However, all of these statements of the non-existence of the past are made purely ad hoc, with little argumentation given to support them. It is 'obvious' that the past does not exist; if it did, we could see it; but we can't, ergo it does not. But we can make just as convincing a philosophical (and metaphysical) case that the past does in fact still exist, although it cannot – at least with current technological limitations – be revisited. Firstly, consider developments in twentieth century physics. One of the revolutionary implications of special relativity was that there was no objective difference between space and time. Thus, we are entitled to say

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[16] This refers to Leopold Von Ranke, the 'father' of the historical method.

that, for example, the Battle of Waterloo exists objectively in the same way that the fact that you are reading this does.

Philosophers of history on the whole though, still tend to proceed on the basis that there is an objective difference between space and time; what has been called the "widespread folk belief that time is dynamic, that the present is all there is, that the past is gone, and the future yet to be" (Dyke & Maclurin, 2015, 531).[17] This is the 'presentist' view: the view that "only the present moment, and hence present objects and events, exist" (Miller, 2015, 346). Ontologically, only the present exists, which is combined with a dynamical thesis that the present moves – which moment is the present moment changes (Miller, 2015, 346).

But there are other metaphysical views of time. Initially developed by C.D Broad, the 'growing block' model of time accepts the reality of the past and present, but holds the future is simply nothing at all. Nothing has happened to the present by becoming past "except that fresh slices of existence have been added to the total history of the world. The past is thus as real as the present" (Broad, 1927/2000, 66). On the growing block model, every event

> on the block is located in the past, except for those events that occur on the three-dimensional slice at the very end of the growing edge of the block. Events on that slice are in the objective present. Once the block grows and a new slice is added, events that were at the very edge of the block cease to be in the present and become part of the objective past (Miller, 2015, 346).

Various arguments for and against both growing-blockism and presentism can be adduced. The point is however, that none of the views briefly canvassed in the preceding paragraphs are to be found in any anti-realist accounts which question the efficacy of historical practice on the grounds that the past no longer 'exists'. It is simply asserted as 'self-evident.' But more importantly, the view that the past does not exist is a *metaphysical* one; and as such has little force as a club with which to beat historiographical realists, who can counter with an equally plausible metaphysical account of their own. And one can adhere to either view without it really affecting historiographical epistemology.

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[17] Again, Mink provides an example of this view: "the past isn't there at all. There's no there for it to be" Mink, 1987, 153).

(iii) The Perception of the Past

As noted above, one of the anti-realists' favourite shibboleths is to point out that the past is not available to perception, and this state of affairs has been taken by some to bolster the anti-realist argument in historiography. Leon Goldstein in particular pushes this line of thought.

> What we come to believe about the human past can never be confirmed by observation - can never be known by acquaintance - and so can never be put to the test of observation, the method of confirmation which is virtually the only one explicitly recognized by science and philosophy (Goldstein, 1976, xii).

Moreover, due to this lack of perceptual access, Goldstein concluded, "no examination of the actual character and procedures of historical study reveals a role for the real past to play, either in the formulation of historical hypotheses or in their confirmation" (Goldstein, 1976, xix) Writing later in response to a critique of the book, Goldstein stated that "a past event is rather more difficult to deal with than a present event. But it is because they are not available for observation that past events raise problems that present occurrences do not, not because of anything that has to do with the nature of the past as such" (Goldstein, 1977, 38). In this last sentence Goldstein seems to say that we can be agnostic about the metaphysical status of the past, but it is the fact it is *unobservable* that raises problems for the realist. And such unobservability is the grounding for Goldstein's infamous view that the real past plays no part in historiography (Goldstein, 1976,xx).

It is unsurprising that more than one historical theorist raised an eyebrow at such a claim. Even Frank Ankersmit – no kick-the-stone-realist himself - remarks that we cannot but fail to "be amazed by" accounts of the practice of history that effectively rule out any chance of accessing the object of historians investigations – "even in the most esoteric parts of contemporary theoretical physics such a state of affairs would be the cause for epistemological alarm." It may well be that the link between "theory and fact has become extremely tenuous and complicated in physics…yet even here the link between theory and empirical fact is never completely severed" (Ankersmit, 2005, 114).

The mention of physics provides us with a nice segue: given the increasing role that unobservables play in present-day science, it seems that arguments to the effect that access to facts about the past-in-itself is impossible due to the fact that it is unavailable to observation are on somewhat shaky ground. As noted a moment ago, research "at the frontiers of particle physics and cosmology deals with problems and theories for which observational or experimental test appears impossible" (Shapere, 2000, 153). If we are to hold to an acquaintance theory of knowledge, then we have to rule out most developments in physics in the twentieth century – one suspects this is a bullet that most anti-realists about historiography would not wish to bite.[18]

Moreover, it is unclear what exactly Goldstein means by 'observation' in the above quoted sentence. Indeed, a few paragraphs on in the same passage he switches to talk of 'perception', and generally sticks with that term for the rest of the book. This is not mere hair-splitting over semantics on my part; perception and observation are not the same. Not all things that are available to perception are available to observation: "we can note that even in common language observing something means more than just seeing or perceiving it" (Agazzi, 2000, 51). Perception is generally passive, whereas observation has a directionality or an intentionality to it; we often talk of having 'seen' something, but not 'noticed' it. Humans are able to make conscious decisions to observe; and this is due to possessing the ability to judge – this is "the typical factor that distinguishes perception from observation (in its fullest sense)" (Agazzi, 2000, 51).

As noted above, the majority of advances in science have been by *going against* what our senses tell us; the observations that have constituted a large part of the advancement of science has been via the use of instruments that permitted the observations of things like sunspots that are not perceivable with ordinary perception (Agazzi & Pauri, 2000, 2). Furthermore, many would argue that the real achievements of science have consisted in the "discovery of laws, considered as general ontological structures of nature that cannot be 'observed', but are 'discovered' through *intellectual* research" (Agazzi & Pauri, 2000, 2).

It is a fact of course, that in everyday life we tend to privilege perception as a source of information about our immediate world, and that therefore doubt is cast on the historical past because it is not the sort of thing that can be perceived. However, even in everyday life we do

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[18] Then again, perhaps not: witness some of Paul A. Roth's writings to the effect that science is seemingly bought into being by narrative accounts in the same way that history is. See Roth, 2013. I am grateful to Barry Lee for suggesting this to me after the viva.

not *soley* rely on our sense organs; we have all sorts of other sources of information about the world (witness the debates on the epistemology of testimony). In both science and history, "we rely upon other instruments for affirming the existence of certain referents" (Agazzi & Pauri, 2000, 2). Agazzi argues that the disciplines used in historiography – papirography, palaeography and numeristics for example – "are not intrinsically different from theories we need for correctly using a laboratory instrument in chemistry or physics" (Agazzi, 2000, 55). In disciplines such as historiography and science, the point is that, in many areas, we use the techniques we do in order to gain information that simply isn't available to the senses.

In sum then, there is no need for a historical fact to be presented to us in a foolproof way in order for us to ascertain that it actually obtains. Granted, it would be a good thing "to know what we know in a way that is minimally, if at all, subject to mistake by reason of a distortion from a medium of transmission" (Alston, 1997, 101). But if we take this to be a necessary condition for knowledge, then we fall back into the Cartesian requirement for certainty of the sort we are supposed to have left behind in the twenty-first century (Alston, 1997, 101). The fact that the past is not available to observation is not *necessarily* a barrier to historical knowledge.

(iv) A Changing Past?

Another argument for anti-realism concerns the idea that concrete knowledge about the historical past is unattainable because *the past itself* changes over time. The idea of a changing past seems to have originated with Arthur Danto's concept of narrative sentences. A narrative sentence is one that refer to two time-separated events in it, and specifically refers to the earlier event in terms of the later one; in Danto's words, "they refer to at least two time-separated events though they only describe (are only about) the earliest event to which they refer" (Danto, 1962, 146). A classic example is: 'the Thirty Years War began in 1618.' The time separation here comes from the fact that when the war began in 1618, no-one knew it was going to last for thirty years. We can give numerous examples of such sentences; for instance 'Harold Wilson began the first of his four terms as British Prime Minister in 1964' could not have been written until his retirement in 1976, and so on.

So far so good. At this point Danto introduces the notion of the 'Ideal Chronicler', who

> knows whatever happens the moment it happens, even in other minds. And he is to
> have the gift of instantaneous transcription: everything that happens across the whole

> forward rim of the Past is set down by him, as it happens, the way it happens. (Danto, 1962, 146).

Surely, if such an Ideal Chronicler existed, he/she would put the historian out of a job? Not so. For as we have seen, there are numerous true statements about event *t* that are only available *after* it has taken place. Thus – and this is the crucial point - the whole truth about "an event can only be known after, and sometimes only *long* after the event itself has taken place" (Danto, 1962, 154). And this has led some commentators – most notably in recent times, Paul Roth – to argue that the past *itself* changes.

The fact that we can only know some things long after the *initial* occurrence of an event however, while correct, should not seem particularly eyebrow-raising. Events by definition have temporal parts: it takes time for the whole event to unfold. We do not find it metaphysically puzzling that the half-time score of a football match is often different to the full-time score; or that more often than not the full-time result is something that no-one could have predicted at half-time. And so it is with history; the consequences of an event (or an action for that matter) often take some time to play out. Those who make use of the idea of a changing past would argue that because the future is always open, the potential consequences of a past event are never 'closed', and thus we are unable to say anything 'definitive' about the past. But this is surely nonsense; the fact that earlier events can be embedded in later events does not mean that every aspect of the earlier is open to re-interpretation.

Indeed, the notion of unintended and unfolding consequences is arguably why we have historians and history in the first place; for the historian can provide an account of events that contemporaries could not have; for events often unfold in a way that contemporaries could not and did not forsee. The historian however, knows exactly what happened, and is in a position to offer an understanding that was not available to said contemporaries. The historian is a person who typically "sees farther than the persons he wrote about and can describe their actions in light of their consequences as these persons could not" (Olafson, 1970, 274). A large part of historiography is concerned with unintended consequences – things that happened even though no-one intended to bring them about (as A.J.P. Taylor used to put it, the historiographical equivalent of traffic accidents).

Again though, this is an *epistemological* point: as historians we can attain knowledge that was not available to the contemporary actors because of our later spatio-temporal vantage point. There doesn't seem anything particular earth-shattering about this revelation: and yet

Danto's account of narrative sentences has resulted in a cottage industry of anti-realist accounts that argue that the end result of all this is that the past *itself* is indeterminate, and that as a result *the past can be said to change* (The locus classicus is Roth, 2012). Rather than being something fixed and immutable –like a woolly mammoth frozen in ice - the past is in fact plastic and mutable. The fact that the amount of true things we can say about the past accumulates as time goes on is taken to mean that when we say something new about the past, this is because the past itself has changed - the "past is unfixed or plastic because later events reshape not just what we know or how we describe what happened, but indeed what happened in the first place" (Weberman, 1997, 750).

In Weberman's paper on the changing past though, there is a particular line that gives the game away: while "Danto's claim was *epistemologica*l, mine is *ontological*." (my italics) (Weberman, 1997,750). At the risk of sounding repetitive: again, an *epistemological* state of affairs is being used to make a *metaphysical* claim about the structure and substance of the past. The fact that we can only *know* certain things about an event sometime after its initiation is conflated into the claim that this knowledge was not available because of some metaphysical indeterminacy on the part of the past.

The Danto thesis about narrative sentences then, is perfectly compatible with a fixed and unchanging past. The fact that we as historians can see things in the sense of things like unintended consequence is one of the strengths of historical study. The value of hindsight is that

> from its perspective it can reveal elements that augment the original [events]. Those who look back can assess the importance of unintended consequences ranging far beyond the perspectives and aims of the original participants (D Carr, 2008, 21).

One of the points that Danto makes is that narrative sentences are the reason why we will always need historians, even if by some miracle of bio-technology Ideal Chroniclers are bought into being. But it does not follow from the existence of narrative sentences that the past itself is mutable and unfixed. Our descriptions of the historical past may – and do – change, but events themselves, once they have occurred, do not.

Indeed, the irony here is that this type of thesis about historiographical antirealism is perhaps more extreme than some of those proffered by the postmodernists in the 1980s/90s. The hardcore relativism espoused by the postmodernists ultimately had its roots in epistemology –

bad epistemology, but epistemology nonetheless. The general thrust was that the gaining of knowledge about the past was so problematic that some advocated the abandonment of the practice of history.[19] However, none made the kind of metaphysical claims that the likes of Roth proffer.

Conclusion

In this chapter I have outlined a distinction between the past and the historical past, and argued that many anti-realist arguments in historical theory commit the fallacy of conflating the two; of running epistemology and ontology together. I began by clarifying the distinction between the past and the historical past, and showing how historians and historical theorists have not always been clear on the division between the two. The following four sections looked at various arguments for historiographical antirealism: specifically, a) the conflation of the past and the historical past; b) the metaphysical status of time, c) the fact that the past is not available to perception, and finally d) the idea that the past changes,. I argued that all of these views fall outside the purview of historiographical epistemology, as they pertain to the metaphysics of the past as opposed to the epistemology of the historical past.

With these preliminaries out of the way, in the next chapter, we will begin the look at the building blocks of accounts of the historical past, by providing a historiographical ontology.

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[19] See for instance, Jenkins, 1999.

2) Historiographical Ontology

This chapter will sketch out, for lack of a better word, an ontology of historiography. Although in the previous chapter I took aim at philosophers of history whose approaches take their lead from metaphysics, not all metaphysical issues are irrelevant to historiographical practice. Any kind of epistemology cannot be undertaken without possessing some kind of metaphysical picture of the world, and thus the past. One is reminded of Louis Mink's remark that "any decent regard for the history of Western Philosophy must reveal that at least from Plato to Wittgenstein the question has been not whether epistemology and metaphysics are related, but how they are." (Mink, 1987, 152). A historian cannot be an agnostic regarding the ontology of the world, and thus the historical past.[20]

Such an ontological inventory is, to be blunt, desperately needed in historical theory, for a glance at many primers on historical practice reveals that little has been done in the way of this. For instance: historical facts are generally seen as the building blocks of historiography, and the majority of primers on historical theory have a section on historical facts. Yet definitions of what facts actually are thin on the ground. In a similar vein, most historical accounts refer to events: but yet few, if any, primers include a definition of events. This is something that desperately needs correcting. When historians speak of facts, do they mean Russellian entities that are part of the ontology of the world, or sentences encapsulating past states of affairs? What exactly is an event? And so on.

Writing in the middle of the postmodernist wars in historical theory, Geoffrey Roberts remarked that what was missing from Arthur Marwick's hardline empiricist account of historical practice was

> a presentation and exploration of the metaphysical basis of historical practice. Like anyone else, historians have reasons for the kind of research they conduct and the type of statements and truth claims they make. The historical approach is not in

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[20] The reader might note the appearance of the word 'ontology' here, and may wonder what I consider the relation between ontology and metaphysics to be. The answer is that in a sense, ontology is prior to metaphysics. For instance, we can agree that there are things such as events, while disagreeing about their essential natures. See Varzi, 2011, 408.

opposition to the metaphysical approach: the historical approach is as metaphysical as that of any other discipline (Roberts, 1996, 225).

In this chapter then, I will put forward an ontology of the past. Said ontology looks like this: the world is made up of events, states of affairs, and actions; all of which historians recount using true sentences. Such sentences are referred to as 'facts', and these facts are the building blocks of any historical account; though as we shall see, facts alone are not enough for what is needed from historiography. Indeed, I will address the latter insight in the final section of this chapter, arguing that any theory/model of historiographical practice needs to recognise that historiography is a two-stage affair. That is, historical research – the establishment of historical facts - and historical explanation/writing are two distinct endeavours.

This chapter will proceed as follows. I first want to clarify the slightly non-standard use of the way historians use the term 'fact', before going on to argue that the past itself (and the present, for that matter), is comprised of states of affairs, events and actions. These entities act as truth-makers for our historical accounts. Finally, I will outline one of the key arguments for this thesis: that historiographical practice follows a two-stage process: the determination of significant fact, and the attempt to integrate this facts into a coherent framework in order to provide an understanding of the past. The fact that determination of fact is separate from understanding provides the common ground that is necessary in order for rational debate and disagreement to take place.

(i)   What are Historical Facts?

Although historians and historiographical theorists frequently talk about historical facts, one is hard-pressed to find a precise definition. Instead, an exemplar tends to be given: 'the battle of Hastings was fought in 1066' being a classic example of a historical fact. As Socrates would argue however, an exemplar is not a definition. To complicate things further, whereas some historical theorists talk of facts, others talk of "events under a description", "atomic sentences", "statements", and "event-descriptions". To muddy the waters further, Geoffrey Elton talks of "facts" and "events", but seems to conflate the two without giving a definition of either (Elton, 1967, 10).

A perusal of the literature leads one to conclude that when historians' use the term 'fact', what they actually mean is 'a true sentence about the past.' Yet of course, this is a somewhat non-standard used of the term. For the notion of a fact, in its standard philosophical use, is something that is part of the world: the fact that I am typing this sentence on an awful, muggy day is to be differentiated from the sentence 'I am typing this sentence on an awful muggy day.' Yet historians tend to conflate the two; Harriet Gillam wrote in the mid-70s that "In most historical contexts it is unclear whether fact refers to the deed itself or to a statement implying a judgment that the deed was done." This is a statement that still holds true today (Gillam, 1976, 233-34).

In contrast to the standard, Russellian definition of a fact, most historians tend to take their lead from has its roots in the fifteenth century when the practice of double-entry bookkeeping developed. In contrast to ancient facts, which were taken to refer to metaphysical essences, "modern facts are assumed to reflect things that actually exist, and they are recorded in a language that seems transparent". (Poovey, 1998, 29) Double-entry bookkeeping marked one of the earliest manifestations of this, privileging "both things in themselves (the objects and money the merchant traded) and a formal system of writing numbers that transformed representations of these things into usable facts" (Ibid).

What one takes historical facts to be seems (perhaps not unnaturally) to largely depend upon whether one is a realist or ant-realist about the past. For the realist - to use Mary Poovey's phrase - facts can be said to "exist in the world like pebbles, waiting to be picked up" (Poovey, 1998, 1). They are things that are independent of observation, "particulars, isolated from their contexts and immune from the assumptions (or biases) implied by words like 'theory,' 'hypothesis', and 'conjecture'." (Poovey, 1998, 1) Thus, although the historian cannot conduct experiments like the scientist can; he/she does have a way to test their hypotheses about the past; they can see if they fit with the facts (Passmore, 1958, 107).

For the anti-realist though, facts are "bits of evidence marshalled to persuade others of the theory one sets out with" (Poovey, 1998, 1). They are not found or discovered, but constructed or constituted: they are "manufactured and thus informed by all the social and personal factors that go into every act of human creation" (Poovey, 1998, 1). Far from being beyond interpretation, they are "the very stuff of interpretation, its symptomatic incarnation instead of the place where it begins" (Poovey, 1998, 1). Facts can never be immune from the assumptions that underlie theories; facts are always "evidence that has been gathered in the

light of - and thus in some sense *for* - a theory or hypothesis" – in other words, facts are never 'neutral', for lack of a better term, but always 'theory-laden' (Poovey, 1998, 1). As we saw earlier, for the anti-realist, facts cannot act as truth-makers outside of a particular theory: they only "appear as candidate truth-makers by virtue of their location within a particular framework" (Roth, 2012, 320).

Historical facts tend to be seen as foundational and atomistic; indeed, if they weren't, then they could not act as the building blocks for historical accounts. Ian Hacking remarks that the modern fact tends to be seen as "the tiny particle of information, the capsule, the nugget…something compact, robust, down to earth, neutral, bite-sized, the very opposite of theory" (Hacking, 2002, 12). Historical facts are also seen as being solid or brute; if we look at the language historians use to describe and refer to facts, we see historians talking "about 'hard facts' and the 'cold facts', about 'not being able to get around the facts'" (Becker, 1955, 327). Historical facts are "something solid, something substantial like physical matter…something possessing definite shape, and clear persistent outline – like bricks or scantlings; so that we can easily picture the historian as he stumbles about in the past, stubbing his toe on the hard facts if he doesn't watch out" (Becker, 1955, 327).

Here at least, the anti-realists are correct, in that historical 'facts' do not exist in the sense of having ontological Russellian existence; these facts are the true sentential statements about the past, and one cannot stub one's toe on a sentence. What historians think of as 'brute facts' are simply those that have been admitted to the club of historical facts, as we will see in the next chapter. What I will emphasise at this point is that none of this implies anti-realism about historiography. For although historians *use* historical facts, and historical primers invariably refer to them, if we examine almost any work of history, we will see that historians *use* facts to write *about* events. And events are things that we can figuratively stub our toes on. About events though, primers on historical theory have had little to say. Thus, we need to make an effort to partially fill this gap.

To conclude this brief section; when we henceforth speak of a historical fact, this can be taken as a true sentence about the past. Although this is a non-standard use of the concept of 'fact', it is one that has nonetheless evolved in historical practise.

(ii)   What There Is: The Constitution of The Past

In this section I am going to outline the ontology of the historical past as I take it to be. As ultimately this is not a thesis concerning the ontology of the past, but rather, one looking at historiographical epistemology, I will not be going into argumentative detail about the justifications for the definitions I cite here.

To begin with; one would perhaps assume that as a realist, I would be inclined towards the notion that facts are things in the world, and we use sentences to pick them out and individuate. However, I reject this Russelian account. I tend towards the view that facts, as

> entities distinct from the statements to which they are presumed to correspond, have no careers of their own…These ghostly copies of true statements cannot be independently specified, confronted or analysed: their reality is no easier to determine than the truth of their respective parent sentences (Scheffler, 1982, 26).

We can reject the notion that there are sentence shaped chunks of reality while retaining the notion that our sentences correspond with reality – it is just that this correspondence is not isomorphic. [21] The idea that sentences were isomorphic with reality formed the basis of the classic correspondence theory of truth. But there seems something idealistic about this: this isomorphic thesis reads sentential structure into the world, but this "can only be the case if the nature of sentences somehow determines the nature of the world, and that is an idealist thesis" (Morris, 2005, 51). Thus, I will reject this thesis, but as stated, this does not mean that we cannot have sentences that correspond to reality. From this point on then, when I talk of 'historical facts' I will use the term to refer to the true sentences we use to describe the historical past. [22] The counterparts of these sentences will be *states of affairs* and *events*.

The concepts of 'states of affairs' comes from the work of David Armstrong, and I will help myself to his account here. Armstrong argues that "the world, all that there is, is a world of states of affairs" (Armstrong, 2004, 1). On my reading then, building on Armstrong, a historical fact corresponds to a past state of affairs. To states of affairs however, I will also add to our ontology events. There are several reasons for this, but the most pressing is that historians, in their historical accounts, repeatedly refer to 'events.' While we are not obliged to accept that there are such things as 'events' on the basis that historians say there are,

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[21] I will come back to this later when discussing Frank Ankersmit's view of historical representation.

[22] This is important; a historical fact is by definition true – there can be no such thing as a false fact.

nonetheless, if we are to take historical practice as our starting point. As stated in the introduction, this thesis is a rational reconstruction of the terminology and epistemological strategies used by historians; thus, it will attempt to underwrite these features with a compatible and cogent metaphysics and epistemology. Fortunately, there are good reasons for thinking that there are such things as events other than the fact that historians refer to them.

On my reading, events are changes in states of affairs. More specifically, an event is composed of interrelated states of affairs. I will examine the precise nature of this relation in Chapter x; suffice it to say here though, that said relations are ontologically real. This is contrary to Hayden White's famous claim that

> histories, then, are not only about events but also about the possible sets of relationships that those events can be demonstrated to figure. These sets of relationships are not, however, immanent in the events themselves; they exist only in the mind of the historian reflecting on them (White, 1978, 94).

I will argue for precisely the opposite point of in this thesis.

To try and define the properties of events is probably (like historiography), an argument without end, at least if the history of the philosophy of events thus far is anything to go by. I will content myself here with the following; events are concerned with change in a way that states of affairs are not; or rather, an event embodies a causal process that motivates a change from one state of affairs to another. As Kim puts it, "the term 'event' ordinarily implies a change…Besides events, we also speak of 'states'. If 'events' signal changes, 'states' seem to be static things, "unchanges", to use a term of C.J. Ducasse's" (Kim, 1976, 312). This certainly ties in with how historians characterise events and states of affairs. If we think of prototypical historical events such as 'The French Revolution', 'The First World War', and so on, all are related to change – the state of affairs that prevailed at the end of the event was quite different to that which preceded it. Contrast these with states of affairs such as 'Anthony Eden was Prime Minister in 1956'; 'Abraham Lincoln was over 6 feet tall', and so on.

Lewis has argued – convincingly on my view - that any event has a causal history: "a vast branching structure consisting of that event, and all the events which cause it, together with all the relations of causal dependence among these events" (Lewis, 1987, 242). One of things that historians try to do in their historical accounts is to trace the relations between events and

to try and establish their causes and effects. As I will argue in Chapter 5, this is one of the things historical narratives are good at doing. Additionally, events are contingent; events occur, but it is contingent that they occur (Lewis, 1987, 243). This is why we are in the habit – particularly in historiography – of talking about events that *didn't occur*: as Lewis puts it, "non-vacuous counterfactuals about what would have been the case had a given event not occurred, as we must if we are to place that event in a history of causal dependence" (Lewis, 1987, 243).

Finally, to events and states of affairs I want to add actions, on the basis that I am convinced by Kent Bach's argument that actions and events are not always one and the same (Bach, 1980). Again, we do not need to go into the arguments here. But it strikes me that there are plenty of everyday actions that we would be loath to call events. Kicking a football is an action; would we really be inclined to call this an event? To push the football analogy further: a football match is an event; but would we want to say that every kick of the ball within it is an event? For these considerations among others, I propose to add actions to our ontological inventory.

In sum then: the past is composed of states of affairs, actions and events, which historians encapsulated with descriptive sentences hence referred to as historical facts. These events, states of affairs and actions act as metaphysical (but not epistemic) truthmakers for historical accounts (although not isomorphically, as discussed above). Before moving on truthmakers however, I quickly want to deal the notion of 'events under a description.'

(iii)   No Standardised Descriptions

Both Louis Mink and Paul A. Roth have made the case that because there exists no 'standardised description of events' (an arguable point in my view), and this contributes towards the fact that in a sense events have no existence prior to the historian's description – such events only come into being under a description. Such a view of course, has it's roots in Goodman's notion of 'worldmaking', which (very roughly put) argues that the world has no joints at which to carve (Goodman, 1978).

In "Narrative Form as a Cognitive Instrument", Mink wrote that there are serious problems with the notion of 'event' with regards to the "limits of application of the concept" (Mink,

1987, 199). For instance, are there such things as 'atomic' events – "events which are not further divisible into events"? At the other end of the spectrum, what is the "maximum complexity and span of time beyond which the application of the term is appropriate?" Is the Renaissance – which spanned centuries – an event? I will look at this issue further in Chapter 5.

Mink's real target however, is the idea that we can refer to events-in-themselves:

> it is clear that we cannot refer to events as such, but only to *events under a description*; so there can be more than one description of the same event, all of them true but referring to different aspects of the event or describing it at different levels of generality (Mink, 1987, 199-200).

The problem historians have then, is that they "have no idea whether there are minimal or maximal events, and no knowledge of any standard or preeminent descriptions of events" (Mink, 1987, 200). Mink contrasts science and history here: whereas science is able to determine the standard description of an event, history "reports on how descriptions change over time" (Mink, 1987, 139)

Paul A. Roth has also picked up on the idea that there are no 'standard' descriptions of events in historiography; like Mink, he compares history with natural science to bring out the contrast. Whereas a science such as chemistry has something like the periodic table which can serve to standardise things, "there exists no settled theoretical "recipe" in historiography regarding how facts should or could be put together to make an event and which events they make". (Roth, 2017, 44)[23] Events therefore, "exist only by virtue of humans who carve time in certain ways for certain purposes" (Ibid).[24] This is ultimately the constructionist view of historiography *par excellance*; the idea that the historian constitutes the past that he/she explains in the same process, and no distinction can be made between the two endeavours.[25]

Needless to say that the idea that we 'make up' events will sit uneasily with most historians, as indeed it does with me. Although Roth is at pains to point out that this view does not imply any kind of 'unreality', it is nonetheless reliant on the premises that the past is a kind of

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[23] Mink and Roth are assuming here is that in principle, there can in principle *never* be such a recipe for history. As was pointed out to me by Tom Stoneham, such a claim seems to be contradicted by the existence of something like Marxism, which posits *precisely such a recipe* for how facts should be put together. The problems with the Marxist view in general do not defeat the point that it is then, in principle, possible to do so.

[24] This is also Mink's view: at one point he writes "surely no-one believes…the Renaissance is a *datum*." (Mink, 1987, 152).

[25] The exemplar of historiographical constructionism is arguably still Goldstein, 1976.

ineffable lump which we may carve in any given way – there are shades here of Froude's famous remark that "history is like a child's box of letters from which we may spell any word we please" (Froude, 1888/1962, 1). I argue that there are indeed 'things' (for lack of a better word) called events, and these events can subsequently act as constraints on the historians accounts.

Roth states that there is no "recipe" – algorithm would perhaps have been a better term – that the historian can follow to construct events from. If he had stated that there is no recipe for historians to construct *historical accounts* from – that there is no all-encompassing formula to take a given set of facts and construct an account from them– most historians would be inclined to agree with this. But as we will see in Chapter 5, Roth conflates events with narratives; he thinks events only come into being with the construction of historical narratives. No narrative – no event. I argue however, that events have actuality beyond our accounts of them – like the proverbial tree falling in the woods, an event has past actuality even if no-one records it.

This is quite a strong claim, but I as will show in Chapter 5, we are entitled to make it. Mink correctly pointed out that historians have not given much thought to the ontology of events – what counts as an atomic event for example. Undoubtedly this is a question that needs to be answered (though not here, for the reason already stated). Although historians must undoubtedly lean on what philosophers of action/ ontology have said in this area, they cannot simply palm this task onto philosophers - unless of course, they are willing to accept the kind of division of labour I deemed unacceptable in the introduction.[26]

Ultimately, here we have the issue that separates historiographical realists from historiographical constructionists. The latter believe that although there was a past, it is now dead and gone, and thus only attains existence now when we attempt to reconstruct it. No description – no past – the past effectively is our description of it. This is to make the mistake of conflating the fact that we must necessarily use descriptions to write about the past (or indeed about anything) with the idea that the past does not exist independently of our descriptions.

(iv)   Truthmakers

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[26] In the same article quoted above, Roberts remarks that "historians prefer to practise their metaphysics rather than talk about them." If historians don't want to make do with the kinds of constructionist efforts foisted on them by philosophers, they had better start talking about them (Roberts, 1996, 225).

A brief word needs to be said here about truthmakers. Earlier I stated that events, actions and states of affairs in the past act as truthmakers for true sentences – i.e. historical facts – about the past. While we need not get into the metaphysics of truthmaker theory here, a brief précis is needed nonetheless.

As realists about historiographical practice, we want our statements about the past in be *grounded* in something. The notion of truthmakers is the idea that that the truthmaker for a particular truth is "some existent, some portion of reality, in virtue of which that truth is true" (Armstrong, 2004, 5). The relation is a cross-categorial one; "one term being an entity or entities in the world, the other being a truth" (Ibid). There is something that exists then, independently of the sentence expressing it, which makes a historical fact true. The sense of 'makes' in the previous sentence is not the causal sense of 'making'; rather, the meaning of 'makes' here should be seen as "in virtue of" (Ibid).

As Dodd and Beebee note in their introduction to an anthology of papers on the topic, if truthmaking is a genuine relation between entities, then we need to know

> something about the things on either side of this relation. What kinds of entities are fit to serve as truthmakers? And what are the things that are made true by such truthmakers? (Dodd & Bebee, 2005, 9).

The answer in this case has been given above: states of affairs and events are the entities that *serve* as truthmakers, and the things that are *made* true by these entities are sentences (i.e. historical facts). To give an example then; the historical fact "Neville Chamberlain uttered the words 'peace for our time' on September 30th 1938" is made true by an event which occurred on September 30th, 1938.

It is important to note that on this view then, there is a *relation* between the historical fact and its truthmaker, and that is this that ensures that truth is grounded in reality. Grounding is a relation, and relations link entities – ergo, the grounding relation links some entities to historical facts, which by definition are true (Beebee & Dodd, 2005, 25). The relation is an asymmetrical one – the fact "Hitler is dead" is true because Hitler is dead, but it is not the case that Hitler is dead because the sentence "Hitler is dead" is true. As realists about historical practice, we want our accounts to be answerable to the historical past itself. Truthmaker theory shows how this can be achieved. As this thesis is primarily concerned with epistemology, rather than metaphysics, we need not go into the metaphysics of this any

further. The point is however, that we can just as easily provide a metaphysical account that supports realism as can the ant-realists who invoke metaphysics to support their historiographical idealism.

(v)   The Two Stages

One of the main arguments of this thesis, insofar as the reconstructive aspect of it is concerned, is that historical practice operates on a two-stage model, as opposed to a holist one. By a 'two-stage' model, I mean that historical research and historical explanation/writing are two ultimately distinct endeavours. By contrast, a holist conception of historical practice argues that historical facts are constituted *within the process* of putting together an explanation/written account of the past; essentially, the explanandum and explanas are constituted at the same time.

This latter model is one favoured by constructivist historical theorists, with Leon Goldstein and Paul A. Roth being the two most prominent exponents. Needless to say, as a realist about historical practice I reject the constructivist view. This rejection is based on the fact that the holist view simply is not reflected in actual historical practice. For it is apparent to anyone familiar with such practice that historians who pen accounts of the same subject that differ in terms of the explanations they provide nonetheless use many of the *same* facts. In fact, there exists, in any reasonably well-established research area/tradition, a corpus of established facts which historians writing in this area can, and indeed to some extent must, draw upon and address in the course of putting together their accounts. Historians can take these facts as something akin to 'givens', in a sense to be explained further in the next chapter. In the following chapter I will also set out how it is that a fact gets accepted into what, following Carr, I shall call the 'club of historical facts.'

In *Meaning, Truth & Reference*, Ankersmit remarked (correctly) that the "distinction between historical research and historical writing – though always quite evident to historians when pondering the practice of their discipline – has lost all its popularity with historical theorists (Ankersmit, 2012, 60). Why is this? Ankersmit argues that it tends to be rejected "as a remnant of a crude nineteenth-century positivism that still maintains the possibility of strictly separating fact from theory" (Ibid, 61). There is probably something to this. Historiography tends to be around twenty to thirty years behind the times in appropriating concepts from

other disciplines, so it is perhaps unsurprising that the notion of 'theory-ladeness' tends to be thrown about quite a lot in current accounts of historical theory.

A holist view of course, goes hand in hand with the notion of theory-ladeness; which in turn is the idea that what is observed is in part determined by the observers' conceptual categories.[27] To give an example used by Norwood Hanson: if Kepler and Tycho Brae sit on a hill at dawn and watch the sun rise, Tycho sees the rising sun; Kepler sees the rotation of the earth. Both are *looking* at the same thing; but *see* different things. In historical theory, theory-ladeness is the idea that that there are no 'unconceptualised' facts, so to speak; historians approach the record of the past with their own concepts/prejudices, and hence the facts 'constituted' are 'theory-laden.' (A variation of this approach is to be found in Hayden White's tropological view of historical writing, in which historical events are reduced to a consequence of the particular tropological emplotment that the historian has selected).

That said, even prior to Kuhn the two-stage view had its critics. In *What Is History?*, Carr poured scorn on the old fashioned notion that the historian filled his/her notebooks with facts in the archives, then opened them up at his/her desk and wrote up their account. Carr used the analogy of preparing fish: the facts are like fish on a fishmongers slab, which the historian takes home and serves up as he/she pleases (Carr, 1962, 9). Instead, on Carr's view the facts are like fish swimming in a vast and more often than not inaccessible ocean. What type of facts the historian will net depends on what kind of (conceptual) net they use (Ibid, 23). Different nets presumably, catch different fish.

Carr was right to argue that a *strict* separation between research and explanation was problematic and many historians would agree with the picture of the historical research procedure that he set out in the same chapter:

> as soon as I have got going on a few of what I take to be the capital sources, the itch becomes too strong and I begin to write - not necessarily at the beginning, but somewhere, anywhere. Thereafter, reading and writing go on simultaneously. The writing is added to, subtracted from, re-shaped, cancelled, as I go on reading. The reading is guided and directed and made fruitful by the writing: the more I write, the more I know what I am looking for, the better I understand the significance and relevance of what I find (Carr, 1962, 28).

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[27] The locus classicus is Hanson, 1958.

There is much in this. We can still, however, allow for there to be a distinction between historical research and writing while allowing for a great deal of interaction between the two: to use one of Ankersmit's analogies, although it is hard to tell where the head ends and the neck begins, undoubtedly head and neck are ultimately two distinct entities.

In contrast to the two-stage view, a holist view of historical practice sees historical facts are being constituted within the process of historical explanation. We have seen in Chapter 1 the roots of Goldstein's constructionism: the fact that the 'real past' is inaccessible to perception.[28] Thus, the historical account is a construction which is entirely divorced from the past-in-itself, which plays no part – indeed *cannot* play a part – in said construction: there are no antecedently existing 'facts', as it were, prior to the historians activity. As Roth would put it, the historians constructs the explanandum as well as the explanas.

The problems with the holist account are numerous; but one in particular stands out. If each historian "has some frame of reference, each of which is self-justifying, then there are no possibilities of resolving disputes in historical inquiry when accounts from different frameworks conflict with one another (Hobart, 1989, 54). These situations have generally come to be characterised, since Thomas Kuhn's use of the term, as exhibiting incommensurability: people talking past each other because they are talking about different things. I will address this in more detail in chapter 7.

Thus, on the holist account, it becomes difficult to see how any kind of rational debate or comparison can take place about the past –if each historian constructs the events which he/she is to explain, then different historical accounts seemingly refer to different events. So what forms the basis for rational comparison between historical accounts? The fact that rational debate undoubtedly does take place between historians – we see few signs of historians talking past each other in the way Kuhnian incommensurability implies in historical debates (although this is not to say the phenomenon *never* occurs) – gives the holist account a further difficulty to contend with.

This is something that Goldstein in particular struggled with. In several places Goldstein approvingly noted the amount of consensus about the historical past that resulted from historiography.[29] Yet this is combined with an account of historical practice that struggles to

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[28] "The actions we know to have taken place in the historical past have a different epistemic status from those we observe around us" (Goldstein, 1986b, 104).
[29] See for instance Goldstein, 1971, 128.

account for how this comes to be the case. Responding to Nowell-Smith's essay on *Historical Knowing*, Goldstein complained that on the former's view

> Since I have dispensed with the real past, I ought to be left with historians subjectively constituting historical events, and subjectivism rendered inevitable because I do not have the real past to determine where the truth of the matter is....Thus, I would seem to be left with all manner of entities – reconstructed historical events –with no way of getting rid of them?..[but] Why should I be saddled with views such as these? (Goldstein, 1977, 42-4).

To the very end of his career Goldstein steadfastly denied that he was an idealist: one of those instances that makes us think twice about the idea that one has privileged access to one's own mental content. Even Roth, a big Goldstein fan, concedes that the worry about Goldstein's model of historical knowing is that it seems to permit "an unreasonable proliferation of historical "knowledge"" (Roth, 2012, 317; also 322-23).

Moreover, Goldstein provides no criteria by which to adjudicate two (or more) competing historical accounts with. In "History and the Primacy of Knowing", he states that

> in the course of work on intellectual problems, we come to increasingly preferred solutions, [and] that seems to be true of history as of any systematic discipline. *For whatever reasons*, some historical constructions will seem better to the community of scholars than the others that have been proposed (Goldstein, 1977, 43). (my italics)

"For whatever reasons" is not particularly inspiring from an epistemological point of view. And in general, constructivists struggle with providing some kind of evaluative criteria for historiographical assessment. Paul Roth's work also faces this problem: Roth has stated that, as a good Quinean, he subscribes to "inscrutability, indeterminacy, and ontological relativity" (Roth, 1986, 79). However, this does not apparently mean that "our theory of the world…is not a matter of free play on the part of the human mind" (Ibid). Yet it isn't exactly clear on the constructivist account how the past or the evidence left by it can constrain our accounts of it.

Although Roth's position shares a good deal of similarities with Goldstein's, a key difference is that whereas Goldstein argued that historical knowledge presented an epistemological problem in comparison to knowledge of the present, Roth argues that it is on a par with the present insofar as the world – and hence the past – is something of an amorphous lump. Roth

is highly influenced by – along with the work of his mentor Quine – the nominalism of Nelson Goodman and Ian Hacking. There are no joints to carve the world at, and the same applies to the past; different taxonomic schemes reveal different entities. Whereas Quine's holism meant that the unit of empirical investigation in natural science was the whole of science, Roth's view of history is something akin to 'everyman his own paradigm,'

(vi) Not By Fact Alone

Why do we need the second stage? In the 'golden age' of history – that is, the turn of the twentieth century, when the discipline's confidence in itself was never (and never has been) higher – it was argued that we didn't: facts were seen as the be-all-and-all of historiography. The historian's task was simply to gather together as many of the facts as he/she could, present them chronologically, and the meaning of events would take care of themselves. Historiography was seen "as a backwards extension of present experience", and historical facts were on the same level of "those of sense perception" (Walsh, 1942, 129). It was the business of the historians to simply "present these facts in their proper chronological order, to trace the exact and complete course of events; but beyond this [they] need not go" (Ibid).

We are now of course, all aware of Mr Gradgrind's musing on facts due to E.H. Carr bringing them to a wider audience; however, there were real-life Grandgrind's in the profession too.[30] At the first International Congress of Historians, held in 1900, one of the participants wrote that

> We want nothing more to do with the approximations of hypotheses, useless systems, theories are brilliant as they are deceptive, superfluous moralities. Facts, facts, facts – which carry within themselves their lesson and their philosophy. The truth, all the truth, nothing but the truth (Quoted in Novick, 1988, 38).

The motivation for this view of course, was the striving for objectivity. As Ankersmit puts it, if historians were not 'objective', then they would inevitably

> add something to the 'object' investigated by them, that is, the past, something that belongs exclusively to the 'subject', that is, historians themselves. And in this way the

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[30] Mr Gradgrind was a character in Charles Dicken's novel *Hard Times*, who famously intoned "what I want is facts…facts alone are what is wanted in life."

historian would distort the past itself by projecting something on it that is alien to it (Ankersmit, 2001, 75).

It soon became apparent though, that facts did not speak for themselves.[31] This is a pity, for the historian's job would be a lot easier if it consisted of nothing but ascertaining the facts.[32] Not that this task is easy by any stretch of the imagination; but as we will highlight in the next chapters, there is a large swathe of historical knowledge – historical facts – which historians are inclined to agree upon.

Indeed, if historiography were simply about finding out the facts in a given area, then we would indeed be moving towards a state of historical knowledge that Acton referred to as "ultimate history." [33] And indeed, there was a time when historians thought that it was enough to describe the facts; that they could (to use the words of a recent philosopher of explanation) "automatically contribute to the explanatory understanding of an event simply by finding out and describing facts about its causal history" (Ylikoski, 2009, 110). However, as A.J.P. Taylor remarked in one of his book reviews, we know more about the five weeks proceeding the outbreak of war in 1914 than any other five weeks in history - yet historical debate about the origins of the war, although it has calmed down considerably in the past fifty years or so, is still a matter for discussion. Actonian ultimate history came to be replaced over the course of the twentieth century by Pieter Geyl's dictum that history is "argument without end." Later on in this thesis I will outline how I think that historiography can be said to make progress.

The point to made here though is that in history we do not just want to know *how* things happened – although this is not to underrate the work that goes into establishing the facts. Facts alone though, are not enough. Alexander Bird has argued that science is in the same boat, and gives an analogy which also nicely illustrates the situation for historiography:

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[31] For instance, Novick recounts the increasing frustration in the American historical profession about the lack of a definitive account of the origins of the First World War in the years following the conflict, and how this lacunae could not be attributed to a lack of 'facts' available.

[32] "If historians thought their labours involved nothing but research, they would lead easier lives. Honest and thorough research can be exhausting and tedious. But honest and through writing will certainly be those things, and the agony of forcing thought into pattern and order should not be despised" (Elton, 1967, 81).

[33] In his report to the syndics of the Cambridge University Press vis-à-vis the *Cambridge Modern History* that he had undertaken to edit, Acton remarked that "Ultimate history we cannot have in this generation; but we can dispose of conventional history, and show the point we have reached on the road from one to the other, now that all information is within reach, and every problem has become capable of solution" (Quoted in Carr, 1962, 7).

imagine a team of researchers engaged in the process of counting, measuring, and classifying geologically the billions of grains of sand on a beach between two points. Grant this may add to scientific knowledge. But it does not add much to understanding (Bird, 2007, 84).

Similar complaints were made of historiography, particular in the early years of professionalization; Sir Walter Scott's nomenclature "Dryasdust" was applied to historians whose works were largely huge compilations of facts. Simply piling up facts about the past does not constitute historiography, although the facts historians recover undoubtedly provide the foundations for the next stage. As Mary Fulbrook puts it in her austere manner,

> factual accuracy of individual statements is an important, indeed vital, prerequisite to the production of any adequate historical knowledge; but an assertion that such limited, accurate, factual knowledge (which we might wish to call, perhaps, 'information') is possible is by no means a complete answer to the more fundamental questions about the nature of history as providing interpretation and/or explanation as well as a true compilation of factual knowledge (Fulbrook, 2002, 186).

We do not just desire to *know* about the past; we also want to *understand* it: and it is "one thing to know that something is the case; it is something else to understand why" (Lipton, 2009, 60).

Knowledge and understanding are equally key components in the historiographical enterprise: knowledge without understanding is largely pointless, as indeed is understanding without knowledge. Historical facts are crucial to the historiographic enterprise, yet as we have seen, by themselves they are not enough. Moreover, there is a reciprocal relationship between the two stages. The disclosure of facts leads to attempts to understand them, and attempts at understanding directs us to where other truths might lie, and thus we go back to the archives. Truth and understanding are the poles that historiography bounces between, as it were; there is a dynamic relationship between the two stages.

Conclusion

In this chapter I have undertaken some metaphysical housekeeping, as it were. I have set out the metaphysical picture of the past that underpins historical research. The past – which was present, and thus has the same structure – is composed of states of affairs and events, which historians describe in the present in the form of historical facts. Historical facts are the building blocks of historical accounts, but facts alone do not constitute historiography (though any work of history has to get the facts right.) Facts need to be explained. (The process of explanation will be examined in Chapter 6).

I have also argued for a two-stage conception of historiography; for a distinction between research and writing. This is not to say that writing does not inspire further research; the model of historians gathering all the facts they need, then sitting and writing them up without need for further research is one that must be jettisoned. While they may influence each other however, research and writing are ultimately two distinct processes. Furthermore, the historians' job does not – indeed cannot – end with the gathering of significant fact, as facts provide knowledge, but by themselves cannot provide the understanding we require from historical accounts.

A by-product of the view argued for in this chapter (and indeed the thesis as a whole) is that in any given field there is a certain stock of historical facts that are established to the satisfaction of most historians in the field, and that moreover all historians are free to draw upon this common 'pool' of facts. The next chapter looks at how a historical fact gains entry to this club.

3) The Club of Historical Facts

One of the key arguments of this thesis – and indeed, of historiographical realism – is that historians working in the same field who write accounts that propose differing explanations of the same subject matter nonetheless utilise many of the same facts – thus ensuring at least a partial commensurability between historical accounts. This is in contrast to holist accounts (explored in more detail in Chapter 7) which argue that different historical accounts use different facts, because the historical [ast itself is constituted in the very course of composing the account. The exemplar of this view is Leon Goldstein's discussion of A.J.P Taylor's revisionist account of the origins of the Second World War: Goldstein has it that Taylor was "surely not seeking to explain the outbreak of war in 1939 by selecting among the historical facts some overlooked by the usual interpretation and shunting aside, as not relevant, facts that those who subscribe to the usual interpretation prefer to emphasize" (Goldstein, 1976, 84). Rather, the situation is this: one side argues that the facts are thus and so, while the other side argues that the facts are this and that - Taylor's conception of the origins of the war in 1939 "involves historical facts which have no existence at all in the conception of his opponents: each side thinks the evidence calls for the constitution of different historical facts" (Ibid).

This is surely little more than a theory of conceptual relativity - the idea that a fact is a fact only within a conceptual scheme – facts do not hold independently of any way we have of conceptualising them. A quick glance at any almost any area of historiography will surely cast severe doubt upon such a thesis. To give an example; Alfred Cobban, in constructing his account of the French Revolution, "nowhere questioned the facts made use of by his Marxist opponents…stranger still, he made use of the *same* socioeconomic facts that had been dug up from the archives by his opponents in order to develop a powerful critique of their representation of the French Revolution" (Ankersmit, 2005, 52). Even in a field like the historiography of the French Revolution then – one notorious for having a proliferation of competing interpretations – there is nonetheless a large consensus over what the facts about the revolution are. As Elton put it in *The Practice of History*, "there is a large body of agreed historical knowledge on which no dispute is possible" (Elton, 1967, 80). (Even Goldstein,

oddly enough, is keen to emphasise and acknowledge the amount of agreement there is in historiography; though how this squares with his 'different facts' thesis remains to be seen.)

I argue in this chapter that there is a large corpus of agreed upon facts that historians then set about trying to understand and assign meaning to – in other words, there exists a distinction between fact and interpretation. This view has not been particularly fashionable in historical theory for a while now. As we saw in the previous chapter, Carr lampooned it in *What Is History?*, while more recently historical theorists are likely to bring up the notion of the theory-ladenness of observation developed in the philosophy of science. Not for the first time however, a cursory examination of historiography as it is practiced seems to show a gap between historical theory and historical practice with regards to this matter.

I submit then, that in any given area of historiography there is a corpus of agreed-upon facts (in the sense of fact that I laid down in the previous chapter – i.e. true sentences) which historians in that area are free to draw upon – to use Carr's infamous term, the select club of historical facts. In effect, these facts are treated as a 'given', even though they are not given in the sense that philosophers normally understand the term. Although facts have to be established, in the sense that they don't simply fall out of the archive into the historian's lap, the facts that we construct have "their status as *facts* just because they are *not* to be at the mercy of our theories, our expectations, and our beliefs" (Trusted, 1987, 39).

This chapter then, will be devoted to examining how facts come to be admitted to the corpus. I will begin from a philosophical standpoint, drawing on works by Gareth Evans, W.V. Quine, Robert Brandom and Robert Stalnaker to provide analogies with regards to how knowledge is established and discussed in historiography. I will establish that we have good reasons for treating certain facts as givens; and that it is only by doing so that any kind of major historiographical work can proceed at all. The chapter will proceed as follows: I will first draw analogies with the work of Brandom and Evans as to how certain facts are established beyond all reasonable doubt. I will then appeal to Putnam and Quine's work on the idea of revisability before finally sketching out how these agreed-upon facts provide the basis of rational debate, using Stalnaker's concept of common ground.

(i)   Evans and Brandom

This section will draw upon aspects of the work of Gareth Evans and Robert Brandom; specifically Evan's work on the reference of proper names, and Brandom's idea of the 'game of giving and asking for reasons'. Both of these accounts, although belonging to the field of semantics and the approach taken by pragmatism respectively, provide striking analogies with the kinds of practices that historians use to establish significant facts.

(a) Gareth Evans – Establishing Proper Names

In order to account for how reference is established in proper name-using practices, Evans developed the idea of 'producers' and 'consumers' of reference. Take, for example, the practice of using the name 'NN' to refer to x. The distinctive mark of any such practice "is the existence of a core group of speakers who have been introduced to the practice via their acquaintance with x" (Evans, 1982, 376). Members of this core group are 'producers'. They do more than merely use the name to refer to x - "they have dealings with x from time to time, and use the name x in those dealings – they know x, and further, they know x as NN" (Ibid).

This practice of using NN may or may not originate in a 'baptism'[34], but the expression cannot become a name for x unless it has a certain currency among those who know x. Any producer can make another person a producer by an introduction – 'This is NN' – but a formal introduction is not necessary; "the name may be picked up by observing the practice of other speakers (Evans, 1982, 377). Furthermore, there is no special importance with regards the initial encounter a person has with a name – "a practice of using the name will normally be continually reinforced by the manifestly harmonious practice of others, and subsequent acquaintance with the practice of others can override an erroneous introduction to the use of the name (Ibid). Consumers on the other hand, "are not able to inject new information into the practice, but must rely upon the information-gathering transactions of the producers" (Ibid). Consumers are not acquainted with x, but are introduced into the practice either by an explanation – 'NN is the ø' – or just by hearing sentences in which the name is

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[34] The term is Kripke's: in fixing reference, "an initial "baptism" takes place. Here the object may be named by ostension, or reference of the name may be fixed by a description" (Kripke, 1980, 96).

used. Producers have no need for consumers; "while the dependence of consumers on producers is absolutely plain" (Ibid).

One might argue that the analogy here does not hold exactly between using proper names. For instance, when we start using a historical fact as a consumer, it is clear that we 'know' something as a result of that usage. But in naming practices, the consumer does not know anything as a result of piggybacking on the producer; they do not have knowledge as a result of being able to correctly use a proper name to refer. However, jumping ahead to our later chapter on understanding, it can be argued that the producer can be said to possess an understanding of a fact in way that the consumer might not necessarily have. As Michael Strevans puts it, there is a difference between grasping a fact and knowing it, as the former involves "a more intimate epistemic acquaintance with the state of affairs in question than the latter" (Strevens, 2016, 41).

(b) Robert Brandom – Deontic Scorekeeping

We turn now to the work of Robert Brandom. In the third chapter of *Making It Explicit*, Brandom sets out his model of discursive practice as 'deontic scorekeeping'. For Brandom, "putting forward a sentence in the public arena *as* true is something one interlocutor can do to make that sentence available for *others* to use in making further assertions" (Brandom, 1994, 170). Furthermore, acknowledging the undertaking "of an assertational commitment has the social consequence of licensing or entitling others to *attribute* that commitment" (Ibid). By adopting this deontic attitude, "putting a claim forward *as* true is putting it forward as one that it is appropriate for others to *take* true, that is to endorse it themselves" (Ibid). The term 'assert', aside from its specifically linguistic use, has a normative use according to which we can assert our authority or rights: a part of this sense of asserting is the idea of "defending, championing or justifying" (Ibid, 172).

When we assert something linguistically then, we undertake a specifically *justificatory* responsibility for that which we claim. In asserting a sentence, "one not only licenses further assertions (for others and for oneself) but commits oneself to justifying the original claim" (Ibid). Here Brandom utilises Kurt Baier's notion of 'task-responsibility': this sort of responsibility requires the performance of a task of some kind for its fulfilment. In making an

assertational claim, "one undertakes the conditional task-responsibility to demonstrate one's entitlement to the claim, if that entitlement is bought into question. Justifying the claim when it is queried, giving reasons for it when reasons are asked for, is one way to discharge this obligation (Ibid).

When performers produce assertions, they are doing two things. In the first instance, they are authorising further assertions, in terms of both "concomitant commitments on their part (inferential consequences) and claims on the part of their audience (communicational consequences)" (Ibid) Secondly, in doing so they become responsible for the answerability of their claims – they are undertaking a task-responsibility to show "that they are entitled to the commitment expressed by their assertions, should that entitlement be brought into question" (Ibid).

Brandom argues that at the core of assertational practice lie three ways in which we can demonstrate our entitlement to a claim, two of which are relevant here. The first has been sketched out above: we can demonstrate an entitlement to a claim by justifying it – that is, by giving more reasons for it. Giving reasons for a claim "always insists in making more claims: asserting premises from which the original claim follows as a conclusion" (Ibid,174). Those interlocutors who accept "the reasons offered as a justification demonstrating entitlement to the conclusion" are concomitantly endorsing a certain inference. (Ibid)

The second way of demonstrating our entitlement to a commitment is to appeal to the authority of another asserter. The communicational function of assertions is to "license others who hear the claim to reassert it. Such a license gives those who "rely on it and reassert the original claim a special way of discharging their responsibility to demonstrate their entitlement to it" (Ibid). When B asserts something then, this means they are able to defer to A the responsibility of demonstrating entitlement to the claim – "B's responsibility can be discharged by the invocation of A's authority, upon which B exercised the right to reply. The buck is passed to A" (Ibid, 175). Communication then, involves both the sharing of commitments and the way that claims can be entitled by being inherited by the consumers of assertions from their producers. The authority of an assertional performance "consists in part in making available a new way in which those to whom it is communicated can discharged their *responsibility* for demonstrating entitlements to commitments they undertake" (Ibid).

As Brandom points out, this justificationary style of vindication threatens a regress on claim contents – that we may end up with an infinite regress of entitlements with no stopping point.

The solution to this is that we should take many claims as "innocent until proven guilty" – that is, "taken to be entitled commitments until and unless someone is in a position to raise a legitimate question about them" (Ibid, 177). Doubts sometimes need to be justified in "order to have standing to impugn entitlement to doxastic commitments. (Ibid). There are certain claims to which we are *prima facie* entitled to, such as "I have two eyes and a nose" for example. Elsewhere in *Making it Explicit* Brandom describes such claims as "free moves" by members of a speech community – "they are available to just about anyone any time to use as premises to assert unchallenged" (Ibid, 222).

That said, these claims are not wholly immune to questions about our entitlement to them; however, such questions "themselves stand in need of some sort of warrant or justification" (Ibid, 177). When a commitment is appropriately challenged – where the challenger is entitled to the challenge – the inferential and communicative authority of the corresponding assertions will be voided "unless the asserter can vindicate the commitment by demonstrating entitlement to it" (Ibid, 178). Thus, although there is no point fixed in advance where demands for justification can come to an end, "there are enough places where such demands *can* end that there need be no global threat of debilitating regress" (Ibid).

(c) The Club of Historical Facts

How then, does the above exegesis of Evans and Brandom translate in what occurs in historical practice vis-à-vis the club of historical facts? Here is a rough outline of the latter process. During the course of research, a historian comes across evidence that lends itself to the construction of a fact – such as for instance, the now infamous death of the gingerbread vendor in Stalybridge Wakes – and decides that it is a significant fact, and includes it in their work accordingly. In the Brandomian idiom, they are making this fact available to use in making further assertions. So historians might use this fact to, for example. support arguments about public violence in mid-Victorian England. Kitson-Clark then, by putting forward such a claim is licensing others to take it as true and use it. And if those users are challenged about the veracity of the gingerbread vendor's death in Stalybridge, they will defer the responsibility to justify upon Kitson-Clark. If Kitson-Clark himself is called upon to justify it, he will give reasons or make claims – that the evidence from which the fact was

constituted is reliable; that it coheres with what else we know about the period in question, and so forth.

The idea of producers and consumers from Evans's work also ties in here. A historian puts forward a candidate fact based on the evidence they use, and said fact is published as part of that historian's account in either a book or a journal. Other historians specialising in that area pick up on this fact, and they will be sufficiently *au fait* with the source material that was used to construct it to investigate the veracity of it for themselves. They then either accept or reject the fact.[35] If they accept it, then they start using it in their own works. This core group – the group that is intimately acquainted with the evidence – are the producers.[36] The consumers are those that are not familiar with the evidence, but take fact *x* as given because a prominent producer has used it, and has thus bestowed legitimacy on it – as Evans put it, they must 'rely upon the information-gathering transactions of the producers'. Take for instance, a fact such as "The Battle of Hastings was fought in 1066." I am aware of this fact, but am no expert in mediaeval history. If challenged about its veracity, I should point to the works that I have seen it cited in. In this instance, I am clearly a consumer, rather than a producer.[37]

It might be pointed out that this comes awfully close to Bosanquet's notion that historical facts are less true when uttered by a schoolboy than when they are uttered by a history scholar. Suffice to say I reject this – the fact itself is just as true when parroted by the schoolboy as when uttered by the well-respected historian. Where the two differ is not that the latter's pronouncement of it is "embedded in a more complex, coherent system of beliefs", as Bosanquet would have it; but rather, that the historian can provide a better justification for his or her utterance than the schoolboy can (Mosteller, 2014, 44). If asked why they think that 'the Battle of Hastings was fought in 1066' is a fact, the child will reply 'my teacher told me', whereas the historians reply will run along the lines of 'because I am intimately acquainted with the historical evidence available for England in the eleventh century.'

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[35] For an example of what happens when a candidate fact is rejected, see the so-called 'Storm over the Gentry"; a neat summary is provided in (Stone, 1972).

[36] This ties in with Carr's remarks we quoted in Chapter 1 – "it may well be that in the course of the next few years, we shall see this fact appearing first in footnotes, then in the text, of articles and books about nineteenth century England, and that in twenty or thirty years it may well be a well-established historical fact. Alternatively, no-one make take it up, in which case it will relapse into the limbo of unhistorical facts about the past from which Dr Kitson-Clark has gallantly attempted to rescue it."

[37] Indeed, this is something akin to Putnam's division of linguistic labour, which I will address momentarily.

Eventually, as Carr noted, certain facts become so well-verified or justified that they are added to the corpus of what Mark Bevir has called "generally accepted facts" (Bevir, 1999, 104) and Kitson-Clark called "public facts."[38] At this point, they become what Brandom calls "free moves" – facts that anyone is entitled to use without being challenged. Moreover, if someone does want to challenge the assertion that, say, the Battle of Hastings was fought in 1066, they will need good reason to do so. It will not be enough to simply say "well, it might not have taken place in 1066" – the individual will need to give compelling reasons for re-opening what we might call a closed case; new evidence having been discovered pertaining to that era which shed a new light on things would usually be the only reason to do so.[39]

One might see this as something like a 'critical mass' approach to the issue: something is an accepted fact about the past iff a majority of historians agree with it. The worry here is that, as we have seen, whether this reduces to an event having occurred in the past depending on whether or not there is a consensus among historians that it happened. However, this kind of reliance upon consensus is something that as a knowledge producing discipline historians are entitled to make use of. In chapter 6 I will look at the idea of the historical profession conferring legitimacy on a work being akin to the institutional theory of art. On such a view, a large critical consensus of the artworld seems sufficient to establish the arthood of an object: at a certain point,

> the segment of the artworld that confers arthood upon some object becomes so large that one is inclined to say that people who believe it is not art are mistaken about the use of the concept of art. Members of the artworld who disagree with the consensus of informed opinion lose their franchise (Young, 1995, 334).

Something similar can be said for historiography: the consensus that, for example, the battle of Hastings was fought in 1066 is so great that anyone who disagrees is inclined to be looked upon somewhat dubiously by the historical profession.[40]

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[38] "Putting aside the philosopher's right to doubt everything, there may be recorded incidents of which one may say with some certainty that that they certainly did occur. There are for instance what may be called the 'public facts of history, that is those facts which are so woven into the texture of human history that, unless human affairs are an illusion and all history is false, they are not false." (Kitson-Clark, 1967, 41).

[39] There is an analogy here with Kuhn's ideas on the function of dogma in scientific research. Kuhn argued that if scientists were constantly returning to first principles, they would never get anything done (he saw this as being the problem with the social sciences.) Hence, when a paradigm is adopting certain assumptions and presuppositions are ring-fenced, and only examined again when enough anomalies crop up to warrant doing so.

[40] Again, Kuhn makes a similar point in *Structure* regarding resistance to a new scientific theory: "Though the historian can always find men-Priestley, for instance-who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes illogical or unscientific. At most he may wish to say that the

(ii)   Quine and Putnam on Revisability

As noted, facts added to the corpus in a given research area are effectively treated as foundational givens. Many have argued against treating facts as such: historical facts (or the sentences that encapsulate them) are always defeasible: in the words of Goldstein, "surely points are never so settled - statements so irrevocably established - that they serve as the criteria for the truth of all subsequent statements. In history, as in all spheres of inquiry, what is established is always subject to revision in light of what may subsequently turn up" (Goldstein, 1976, 46). Collingwood said something similar in *The Idea of History*; when the historian describes historical facts as his/her data, all that they mean is "that for a certain piece of historical work there are certain historical problems which for the moment he proposes to treat as settled; though, if they are settled, it is only because historical thinking has settled them in the past, and they remain settled only until he or someone else decides to re-open them" (Collingwood, 1946/1994, 244).

I suspect that historians may well pay lip-service to such a view; perhaps citing Popper's doctrine of falsificationism as they do so. In practice however, they tend to take Quine's web of belief, rather than Popper's falsificationism, as their model here, for it is a brute fact that there are certain facts that it is hard to see historians ever being willing to give up. On Quine's reading, we do not test our sentences one by one, as it were – evidence (sensory evidence in Quine's case) "bears upon our entire system of beliefs rather than its individual elements (whence the phrase 'web of belief')" (Resnik, 2007, 413). There are certain statements which, although in theory are not immune to revision, in practice we take for granted. For instance, although Quine holds that revising something like our theory of mathematics is a "live option", much to the dismay of some, he would be the first "to emphasise how radical it would be to revise mathematics in order to save a scientific theory" (Ibid). In practice, "the various sciences take large blocks of theory for granted (Ibid, 420).[41] And so it is with historiography.

There is an important difference though between historiography and science: whereas in Quine's picture of science, large blocks of theory are at the centre of the web of belief, in history it is the other way around – empirical findings (i.e. facts) are to be found at the centre

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man who continues to resist after his whole profession has been converted has *ipso facto* ceased to be a scientist" (Kuhn, 1962, 159).

[41] David Armstrong has made a similar observation; in empirical science, "we should draw a distinction between the frontiers of science and the settled body of knowledge that is presupposed by those working at the frontiers." (Armstrong, 2004, 32).

of the web. In any given area of historiography, there is a corpus of facts which are considered, to all intents and purposes, unrevisable. Sir George Clark's remark that history might be seen as a "hard core of facts" surrounded by a "pulp of disputable interpretation" seems extremely apropos (Quoted in Carr, 1962, 2). To use an analogy from Lakatos in the philosophy of science, these facts form the 'hard core' of a given area of historiography. Lakatos' picture of science sees this hard core surrounded by a 'protective belt' of auxiliary hypotheses; it is this protective belt that bears "the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the thus-hardened core" (Lakatos, 1970, 133).

Without getting too bogged down in making the analogy match point for point, what would such a 'protective belt' consist of in historiography? One is tempted to say: simply the consensus of historians with regards to the veracity of fact *x*. That is; if a historian were to claim that the Battle of Hastings took place in 1076, an immediate storm would break out, and several historians would undoubtedly leap to the defence of the notion that the battle of Hastings took place in 1066. Indeed, given the emphasis that Goldstein places on the value of communal agreement among historians one suspects he would have to agree with this. However, a more epistemologically pleasing response is probably required; and it would probably take the form of the methods used to constitute/recover these facts. That is, if the methods utilises to recover the fact is sound, then by extension the fact itself is sound.

Hilary Putnam has also written on the idea of certain facts and truths being insulated at a given point. Writing on the notion of 'necessary truths' in physics and elsewhere on the idea of 'framework principles', after remarking, "there are necessary truths in physics, but they can be revised if necessary"[42], Putnam goes on to state that scientists are "perfectly correct to assign a special status to these statements" (Putnam, 1975a, 88). Holding such statements "immune from revision…is good methodology", and "it is the task of the methodologist to explain this special status, not explain it away" (Ibid, 92). This is what I have been attempting to do in this chapter for historical practice.

In the words of one of his commentators, Putnam accepts the potential fallibility of things like facts, but like Quine and Brandom also holds that "revisability only makes sense in practice when appropriate conditions for revision are provided" (Fine & Muller, 2005, 87). Where this is not the case, we take it for granted that the statements in question constitute

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[42] A laboured attempt at humour.

objective knowledge and, if they contain extralogical vocabulary, even knowledge of the most abstract structures of the world" (Ibid). When a statement is "necessarily relative to a body of knowledge, we imply that it is included in that body of knowledge and that it enjoys a special role in that body of knowledge" (Putnam, 1975a, 88). Furthermore, "one is not expected to give much of a reason for that kind of statement" – it is taken as a given to all intents and purposes. (Ibid) Those working with then cannot conceive their being false.

Putnam isolated a series of central assumptions that, during a practice of inquiry, participants take for granted when they make and exchange claims about facts. Firstly, "terms used descriptively in public practices of making and exchanging empirical claims refer" (Fine & Muller, 2005, 87). Secondly, "statements at all levels in a system of empirical knowledge state facts, and are taken as objectively true and revisable as long as they are in use" (Ibid). Thirdly, "descriptively used terms can preserve reference over dramatic differences in theory and belief" (Ibid). Finally, "there is a publicly shared environment in which applications of terms and theories take place" (Ibid).

Needless to say, these all apply to what most historians do in the practice of history. They believe that the facts that they use in their accounts are true; that the events they write about actually happened. Facts that have been admitted to the corpus are taken to be true, unless a good reason is given to cast this into doubt. And as I have been arguing and will argue across this thesis, these facts can be used in accounts which put forth very different explanations of what took place in the past. Finally, all this takes place in the various historical books and journals that are published, as well as at conferences and informal discussions.

Thus, although Goldstein is correct in stating that historical facts are not 'given' in the way that we would normally use the term 'given', many of them nonetheless come to receive the status of a *given*; and thus in this way they are able to provide a "natural stopping point" with regards to historians' explanations of the past. Of course, in principle any historical fact is defeasible – indeed, as realists, we hold that all knowledge is in principle defeasible. However, there are certain facts, which, if we are to challenge them, we must have extremely good reasons to do so. Revising the date of the battle of Hastings is akin to revising the canons of mathematics; it is a live option, but like pressing the nuclear button, is a last step which we would only wish to take in extreme circumstances.

(iii)   Common Ground

By now the importance of the corpus of shared fact that I had argued exists in historiography should be clear. Rational debate and disagreement can only take place upon some kind of shared foundation; what Robert Stalnaker has come to term 'common ground.' In this section, I will elaborate upon Stalnaker's ideas, and how they map onto historiography.

In his work on pragmatics, Stalnaker looked at presuppositions and how they enable conversation to take place. To presuppose a proposition "in the pragmatic sense is to take its truth for granted, and assume that others involved in the context do the same" (Stalnaker, 1999, 38). Since these presuppositions play such a large part "in determining what is going on in a linguistic situation, it is important that the participants in a single context have the same set of presuppositions if misunderstanding is to be avoided" (Ibid, 39). Such shared presuppositions are essential for any kind of communication. Any kind of communication, be it linguistic or otherwise, "normally takes place against a background of beliefs or assumptions which are shared by the speaker and his audience, and which are recognised to be shared" (Ibid, 48). To give an example from our field of historiography; in discussing the history of the Second World War, historians will take basic facts like 'Germany invaded Poland on Sept 1 1939'; 'Japan attacked Pearl Harbour on December 7th 1941', and so forth, for granted. That they can take such facts for granted makes communication more efficient – and unless historians could "reasonably treat some facts in this way, [they] could probably not communicate at all (Ibid, 39).[43] This then, is the common ground; in history, it is a set of facts that the participants in in a conversation at a given time can mutually assume to be taken for granted and thus not subject to further discussion.

The point of a conversational exchange – or at least a central kind of conversational exchange – is to exchange information (Ibid, 98). Participants in a conversation "begin with a certain body of information in common, and it is that body of information that the speech acts they perform are designed to influence" (Ibid). During such conversations, participants will sometimes make assertions; the content of such an assertion will be a "a piece of information, and if the assertion is successful, then that information will become part of the body of

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[43] Ibid, 49. There are parallels here with Kuhn's notion of the function of dogma in science; if scientists were forever returning to first principles they would never get anything done.

information that provides the context for subsequent [conversational exchange]" (Ibid). When a sentence is accepted by participants in a conversation, the common ground is successfully updated. To give a recent example; due to recent conclusive evidence that has come to light, it is now part of the common ground with regards to deceased Liberal politician Cyril Smith that he was a paedophile. Henceforth, in historical exchanges about Smith, this fact will now be taken for granted. As we have seen, in historiography, a historian will put forward a fact from their researches, and if historians with the requisite expertise decide that it should become a part of the corpus in that area – the background – then it will subsequently be elevated to the appropriate status.

Thus, the background knowledge in any given area of historiography will be constantly evolving. As Stalnaker puts it, "the context is…continually being updated in the course of a conversation" (Ibid, 155). When historical facts are added to the corpus, they are, to use a concept of David Lewis', 'accommodated' by the participants in the exchange. They are accommodated by "assuming that the appropriate things were common ground even if they previously were not, or that certain things were not common ground which a hearer previously thought they were" (Ibid).

Of course, not all assertions will be accepted or accommodated. Nonetheless, rejected assertions will still have an effect on the conversation: "actions (including the attempt to update the common ground inherent in an assertive speech act) always have effects even if their intended effect is thwarted" (Von Fintel, 2008, 139). Thus, the assertion of a potential fact that is ultimately rejected by other historians for membership of the club of historical facts "will not have the effect of updating the common ground in the intended way", but it will "have plenty of other effects on the conversation and even on the common ground: for example, it will be common ground that the speaker uttered the sentence and plausibly, that the speaker's intentions were thwarted, etc" (Ibid, 139-140).[44] It will become part of the historiography of a certain area that such a fact was put forward and rejected. This can also be applied to arguments and interpretations; 'in the discussion of topic X, historian Y put forth controversial thesis Z, but it was rejected for these reasons.'

Thus, even an individual who is "unmoved by the arguments of his interlocutor to change any of his beliefs about the subject matter of conversation…will still change his beliefs about the ongoing conversation itself: when something is said, he will come to believe that something

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[44] Again, the 'Storm over the Gentry' is a good historiographical example of this.

has been said" (Stalnaker, 2002, 708). To return to an example I used in an earlier paper of mine, historians who were unhappy with the thesis put forward. However, it was clear that there was a case to be answered; that a new factor had been introduced into the debate (Timmins, 2012, 102-103). This is what Stalnaker calls a *manifest event* – an event that, when it occurs, is mutually recognised as occurring. (Stalnaker, 2002, 708).

Stalnaker calls a context in which "each of the parties to a conversation presuppose the same things" a "nondefective context" (Ibid, 717). Is it the case that historians in a given area all presuppose the same things? Yes, and no: while they will differ on how the facts should be interpreted, they will in all likelihood presuppose the same facts in constructing their accounts (though will be the odd exception here and there.) Writer on the origins of the Second World War presumably all agree descriptively on the events and actions that constitute Neville Chamberlain's foreign policy; what said events and actions mean or how they are to be interpreted is a different matter however.

Stalnaker outlines cases where defective contexts can be repaired; a brief example would be Alice saying "how old is he?" to Bob when the latter has his baby daughter in his arms. In this instance, Bob would recognise Alice was taking something to be a common belief that he knew to be false, and would take steps to correct her. This is a case which is fairly easily remediable: "a case where one party not only recognised that the context was defective, but also recognised exactly how the presuppositions of the two parties diverged, and so knew how to repair or accommodate" (Ibid, 718). But as Stalnaker recognises, "not every defective context is so easily corrected: there are cases where accommodation is impossible" (Ibid). One can think of many examples in historiography where we can put our finger on how and why the presuppositions of two parties diverge, but the two cannot be reconciled. As A.J.P. Taylor put it in a different context, is there any document that could have convinced Macaulay that the Glorious Revolution had been a mistake; that would have led him to 'accommodate' this? (Taylor, 1967, 102).

I want to return at this point to the idea of accommodation in relation to the model of producers and consumers I sketched out earlier. As we saw, not all assertions are accepted without fuss into the common ground. Faced with an assertion, we can do one of several things. We can reject the assertion on the grounds that we may not have much confidence in the historian in question, or because our familiarity with the subject area has lead us to draw different conclusions. Here we would have a case of presupposition failure. On the other

hand, if we consider said historian reliable on this point; or undertake our own research into the matter and concur with them, then we might add this fact to our set of beliefs, and also to the common ground.[45]

With regards to producers and consumers; consumers will by and large accommodate without fuss assertions made by producers. If they seek to challenge these, then they will in effect have to become a producer, as in order to challenge the assertion they will need to become familiar with the core material. Mostly however, the consumer will be content to accept the producer's verdict. As Fulbrook puts it, "where there are gaps in the evidence, or problems of interpretation, the lay reader is most inclined to view the professional historian as the person who has been most engaged, most immersed in the material and whose judgement is thus perhaps most to be trusted" (Fulbrook, 2004, 154).

(iv)   The Division of Epistemic Labour

The appeal to a division between producers and consumers made earlier calls to mind a division of epistemic labour. The notion of lay individuals trusting experts was most famously made by Hilary Putnam, who coined the phrase 'the division of linguistic labour.'[46] In "The Meaning of Meaning", Putnam attacked the idea that the extension of a term could be determined solely by an individual speaker. Using the example of elms and beeches, Putnam wrote that

> the reason my individual grasp of 'elm tree' does not fix the extension of elm is not that the word is vague…The reason is rather that the extension of 'elm tree' in my dialect is not fixed by what the average speaker 'grasps' or doesn't 'grasp' at all; it is fixed by the community, including the experts, through a complex cooperative process (Putnam, 1975b, 265.).

A competent speaker in a linguistic community then, relies on "experts and on frontline, causally immersed observers and ostenders – the social and environmental context – to fix the extension of some terms" in his or her language, and this is the division of linguistic labour.

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[45] A point made in Simons, 2003, 260.
[46] Although at the time of writing we are in a period where expert knowledge is becoming increasingly disdained. See Nichols, 2016.

(Ibid). We could "hardly use such words as 'elm' and 'aluminium' if no one possessed a way of recognizing elm trees and aluminium metal; but not everyone to whom the distinction is important has to be able to make the distinction" (Ibid, 227).

Take a term like 'gold', for instance. There is a distinction/ division between those who *acquire* the term gold, and those who have acquired the method of *being able to recognise* if something is or isn't gold. The person who acquires the term is able to rely on the special subclass of speakers who can recognise gold in order to fix the extension. Putnam thus put forth the following hypothesis:

> Every linguistic community exemplifies the sort of division of linguistic labor just described: that is, possesses at least some terms whose associated ' criteria' are known only to a subset of the speakers who acquire the terms, and whose use by the other speakers depends upon a structured cooperation between them and the speakers in the relevant subsets (Ibid, 227-28).

Essentially then, as Diego Marconi puts it, there is a distinction between speakers who are "very competent (as competent as one can be) in the use of certain words, and less competent speakers, whose use of such words is – somehow – parasitic upon the experts' competence" (Marconi, 1995, 133).

A similar division exists with regards to historical facts though as stated, this is an epistemic division rather than a linguistic one. In historical research (as in many other areas), "it will be necessary to rely at some point on someone other than oneself" (Kitson-Clark, 1967, 119). One of the features of historiography – as well as other disciplines, most notably science – over the past one hundred and fifty years or so has been an ever-increasing degree of specialisation – what has been characterised as knowing more and more about less and less. It is not just the lay reader of history who must rely upon specialists. Although I have a solid working knowledge of the historiography of the origins of the Second World War, I must confess to never having looked at a single primary source on the subject. For my knowledge of the facts of the origins of the Second World War then, I am dependent upon those who have toiled in the archives – those on the front-line, as it were - in order to establish said facts.

Moreover, as Putnam notes, extension is a community affair. Recall the process by which a fact becomes a member of the corpus of historical facts. The fact that Kitson-Clark proposes

it is not enough (although Clark's standing in the field will count for something; a historian of lesser standing may perhaps simply be ignored); it needs to be examined and verified (for a lack of a better word) by others. We want the notion of competence to be associated with objective notions; "a sentence's truth value is not assessed on the basis of an individual speakers competence; it should be evaluated in the light of the best available theory concerning the subject matter that sentence is about" (Marconi, 1993, 140) [47]

Conclusion

In this chapter I have argued that there is a body of facts in any given area of historiography that historians are able to take for granted and draw upon. Furthermore, many of the same facts will be used in historical accounts that differ in terms of the meanings they ascribe to the events recorded by these facts. I have outlined some of the mechanisms by which said facts are added to the corpus – or not, as the case may be. It should be fairly clear by now that the holists' "different facts" thesis – which, if true would raise serious questions about the rationality of the historiographical enterprise – fails to get off the ground.

Taken together, the previous three chapters have dealt with what we might call foundational issues of historiographical theory. We have dismissed some common anti-realist arguments which need not be taken particularly seriously; we have established an ontology that realists and historians can be happy with; and we have shown how a corpus of facts exists that enables rational debate and disagreement to take place in historiography.

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[47] As Marconi puts it, even "if Bob believes that the word 'transvestite' applies to anybody wearing a uniform, we still do not want 'All American generals are transvestites' to be true when uttered by Bob" (Marconi, 1995, 140).

Interpolation

Before we move onto chapter 4, a brief note is required to explain the shift in tone between the preceding three chapters and the next two. The following two chapters deal with somewhat technical issues dealing with the philosophy of language. The main reason is that these two chapters provide a bridge between the three that have proceeded it and the three that succeed it. Before we can go on to look at some of the issues that are raised by 'whole' historical accounts – in other words, philosophy of historiography – we need to address some issues with the language that historians use.

Chapter 4 deals with a specific sub-section of reference, that being the reference of terms which are used exclusively in historiography: so-called 'colligatory concepts' such as 'the Renaissance', 'the French Revolution', and so on. This is an area that philosophy of language in general has ignored, while the efforts of historical theorists to deal with them have been, as we shall see, somewhat problematic. It is not the aim of this chapter to deal with the semantics of referring to events per se; only those events which crop up exclusively in historical accounts.

As noted, such events tend to be referred to in historical theory as colligatory concepts, and Chapter 5 investigates the reality of these events. I will argue that, contra Ankersmit, events like the Renaissance have as much 'reality' in their own way as tables and chairs. Again, this involves a discussion of notions such as supervenience, which in general do not crop up much in historical theory; and for this reason, among others, badly needs addressing.

Having dealt with these fine-grained technical issues, the remaining three chapters – 6,7 & 8 – deal with issues concerning the philosophy of historiography.

4) Reference

In this chapter, I will examine the issue of reference in historiography, specifically focusing on a special class of terms that crop up uniquely in historical study. These are terms such as the 'French Revolution', 'the Renaissance', and so on, which are known as colligatory concepts. I will examine the notion of a colligatory concept in more detail in the next chapter. In this chapter I will investigate how we establish the semantic value of these terms, which I will argue should be classed as descriptive proper names. Historiographical reference is a subject which has generally received little attention from philosophers, of both language and historiographical theory. It seems to be generally agreed by philosophers of language (with the exception of those inclined towards presentism perhaps) that we can refer unproblematically to individuals from the past – 'Aristotle' is the exemplar normally given in these instances - but little effort seems to have been made to examine how we secure reference to the aforementioned colligatory terms. This is something I will correct – or at least, make a start in correcting –in this chapter. As stated a moment ago, this chapter and the following one form a pair, in the sense that they are both concerned with reference and realism: the present chapter argues that when we talk of entities like the French Revolution, there *is* something to refer to in a non-trivial sense; the following chapter sets about examining how exactly we successful refer to such entities

This chapter will proceed as follows. I will first argue that event names, despite seemingly being orthographically unorthodox, should in fact be seem as a variant of a descriptive proper name, and that furthermore such descriptive proper names are not themselves semantically suspect. I then go on to examine how such event names are established, before going on to examine the worries that Ankersmit has vis-à-vis referring to macro-events. While the worries are legitimate, I argue that Ankersmit's solution is a classic case of the cure being worse than the disease. Finally, I will show how we can secure reference to events like the Renaissance, using Imogen Dickie's 'governance theory' as the base.

Before we begin: for the sake of clarity, by 'referring', I mean the following: a referring expression is one in which, by using it "in a simple sentence, a speaker represents himself as aiming to introduce an object…for the rest of the sentence to say something about"

(Sainsbury, 2005, 81). When we generally use words and sentences, we do so with the intention of referring to something; as Gareth Evans remarks, "the notion of the intended referent is rather like the notion of a target" (Evans, 1982, 317). To use a similar idiom, a referring expression is designed to 'pick something out in the world'. When I use a name or a sentence, my aim is that it – the referring expression – should *bring to mind* the same thing in your head. For once we have the same thing in mind, we can have a conversation about it safe in the knowledge that we are both thinking of the same person/object/etc.

(i)   Prelude – Why is the Reference of Colligatory Concepts Problematic?

To begin with: why is it that colligatory concepts pose a particular problem for reference? Some theorists (Ankersmit among them as we will see below) see reference to past events as problematic on the basis that as they are not available to perception we cannot point them out demonstratively, and can only refer to them via description. Thus, if we have two competing interpretations of a past event or set of events, we cannot check them against 'the past in itself' to see which one is correct. (As noted in Chapter 1, Goldstein has used a similar analogy to motivate his idealist theory of historiography).

However, this objection is easily countered by the fact that demonstrative reference does not always require perceptual contact. Often reference can be deferred: think of instance, of seeing a parking warden apply a ticket to a car, and our responding "he won't like that." Moreover, many of the things historians talk about were referred to at the time, and we have records of these references. There is a causal chain of reference that we can tap into. Think for instance, of a football match: West Bromwich Albion vs. Preston North End in the 1888 FA Cup Final was referred to by contemporaries, and thus we can be confident that such an event took place, and we can draw on these records and this chain of reference to characterise it.[48]

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[48] Incidentally, Preston asked to be photographed with the trophy before the game, so confident were they of winning the final. WBA won 2-1.

Colligatory concepts pose difficulties precisely because they refer to things which were *not* referred to at the time. The concept of 'the Renaissance', for instance, was coined in the 19th century, several hundred years after the events it refers to. A slight more recent and less heavyweight example is the genre of music known as 'yacht rock', whose hey-day was I the 1970s, but has only been identified retrospectively. In these cases, there is no deferred ostension which we can draw upon; our referential access to them is solely mediated by descriptions formulated after the event that do not have the causal connection we can normally help ourselves to. Thus, establishing the reference of concepts like 'the Renaissance' is a lot trickier than that of establishing the reference of say, Julius Caesar or the Battle of the Somme.

(ii)   The Semantics of 'The Renaissance'

One of the issues with colligatory concepts is that it is unclear whether they should be classed as definite descriptions or proper names. If 'the Cold War' were a definite description, then it would have to have been a war, and it would have had to have been cold, as definite descriptions are supposed to be semantically descriptive. So it seems such expressions as 'the Renaissance' have to be proper names: i.e., they are non-descriptive. However, the definite article at the beginning of 'the Cold War' is semantically active: it implies that the Cold War is unique, and proper names are not supposed to be unique.

Some philosophers have argued (not unreasonably) that such names should be treated as a hybrid of proper names and definite descriptions.[49] However, I argue that such historiographical terms are in fact descriptive proper names that utilise a Fregean semantics; they are descriptive names of a certain *kind*. They aren't definite descriptions, but nonetheless they are syntactically complex: with a name like 'the Renaissance', we have one part indicating uniqueness, the other saying it is a certain kind of thing. Thus, we can contest what type of thing it was without changing the referent. Suppose for some reason historians were to decide that the Cold War was not in fact a war. Nonetheless, we could still use the name 'the Cold War' to the period in question; whereas if it were a definite description, we would not be able to, as it would no longer describe a war. In sum then, such terms are descriptive proper names, but with two interesting features: non-uniqueness, and indication of type.

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[49] See for instance Rabern, 2015.

We can see at this point why we need a Fregean, as opposed to a Millian, semantics. On a Fregean semantics, the meaning of a proper name is given by or associated with a body of associated information: thus, the referent of the name is that which said information is true of. Certain information is semantically encoded in a name like, for instance, Bryan Danielson encodes the identifying properties: former Ring of Honor champion, master of the 'Cattle Mutilation' submission manoeuvre, and so on. This definite description is satisfied by a unique person or object in the world: i.e. Brian Danielson. This is something that a Millian semantics cannot accommodate; a Millian name is just a label, no more no less.

In "Descriptive Descriptive Names"(sic), Robin Jeshion notes that there are numerous kinds of referents for descriptive names; as well as objects, there are things that we are in the process of putting together: think for instance, of someone planning to construct a cathedral – they bestow a name upon the cathedral even though as yet they are still planning it in their head (Jesion, 2004, 608-609). Another example is mathematical objects or numbers with which we lack acquaintance – π for instance, was introduced to descriptively refer to the ratio of the circumference of the diameter of a circle. To this group, we can to add historical events, which are not 'objects' per ce, but nonetheless exist/existed and thus can be referred to.

However, while historical events are descriptively identified, and thus qualify as descriptive names, it seems that they differ slightly in terms of their introduction from something like 'Neptune' or 'Jack the Ripper.' In the case of these names, we were reasonably certain that there was a planet or an individual that the description picked out, but it could have been the case that we were mistaken; for instance, the murders we collectively attributed to Jack the Ripper might have been individually committed by different individuals. However, it seems slightly odd to say that we might have 'misidentified' the First World War; or to use a contemporary example, that we think we are going through the process of 'Brexit', but actually we might be mistaken.

How exactly are such events picked out? For many historical events, contemporaries involved in said events will have a hand in 'baptising' the event, as it were. Furthermore, what *kind* of event it is will also play a part in how it is identified. For those involved in what we now call the First World War as well as for contemporary observers, it was fairly clear by October 1914 that there was 'a war' going on in Europe; as indeed it would have been to

observers in Europe by mid-1940. In such cases, it only remains to append a prefix to the label war: initially, the war of 1914-18 was referred to as 'the Great War', until another 'great war' occurred between 1939-45, at which point it was rechristened 'the First World War'. There seems then, to be a closer connection between these events upon which descriptive names are bestowed than there was in say, the case of LeVerrier's discovery of Neptune.

As noted earlier, though, some historical events are identified retrospectively. The prime example is 'the Renaissance', which took place in the fourteenth and fifteenth centuries, but the descriptive name 'the Renaissance' was not initiated until 1858, when Michelet coined the term. Two years later, Burckhardt used the term, and from this point its use became firmly established. Again, the difference is that Michelet was sure that there *was* an event that he was to bestow the name 'the Renaissance' upon; he was not *speculating* that it existed, *ala* Neptune or Jack the Ripper.[50] In the case of the latter, an entity was invoked to account for a set of murders. It doesn't seem right however, to say that Michelet *invoked* the concept of the Renaissance to account for a sequence of events: rather, those events *are* the Renaissance, as it were.

That said, as we shall see, such descriptive names may be said to be *open-textured* insofar as we not have all of the information needed to *rigidly* fix the semantic content of the name at the moment of baptism. The notion of open-texture comes from Freidrich Waismann, who argued that when we introduce a concept in a natural language, it is never limited in such a way that there is no room for doubt as to its application. As an example,

> we define gold in contrast to some other metals such as alloys. This suffices for our present needs, and we do not probe any farther. We tend to overlook the fact that there are always other directions in which the concept has not been defined…In short, it is not possible to define a concept like gold with absolute precision, i.e. in such a way that every nook and cranny is blocked against entry of doubt. That is what is meant by open texture of a concept (Waismann, 1945, 123).

Indeed, Francois Recanati has argued descriptive names are "created only in the expectation that more information about the bearer will accumulate" (Recanati, 1993, 180). This is

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[50] There may be one or two equivalents to the 'Neptune' case in historiographical name-fixing – these would tend to involve categorisations that are disputed. For instance, Geoffrey Elton has argued that a political revolution took place during the Tudor era; however this is not universally accepted. At the moment then, we could say that we use the name 'Tudor Revolution' with an asterisk next to it; if the thesis gains a consensus in that subject area, the asterisk will be removed.

certainly the case for the names of historical events; although we know the outline of the event denoted, much historical research is still to be done in order to give us something like the knowledge and understanding we generally seek in historiography.[51]

A word needs to be said about who is licensed to introduce historical event names. Rebutting Evans's suggestion that there are no kinds of restrictions on introducing a descriptive name, Jeshion points out that amongst other things, one's social standing determines whether one is in a position to introduce such a name.[52] In the case of historiography, the standing of the historian will play a role in determining this. If the historian in question is seen to be an expert in the field, then many will defer to their expertise in terms of such matters. If however, a young scholar, with their first publication, decides that the First World War be renamed "the War of Franz Ferdinand", the chances are they will be ignored.[53]

To come back to the point I made at the start of this section: although historical event names are clearly descriptive proper names, it seems that in many cases, we have something like *de re* access to the events in question that appears to be lacking in the case of say, Neptune. One of the traditional arguments for the motivation of introducing descriptive names is that they are used to tag objects that we do not have *de re* access to – e.g. Neptune, the Unabomber, etc. However, it seems slightly odd to say that contemporaries of say, 'the Cold War' could not have been said to be *en rapport* with the Cold War, as it were (a more recent example would again be 'Brexit'). Granted, the Cold War is not the sort of thing that could have been denoted ostensively, but even so, our access to it at the time was not purely *de dicto*.[54]

(iii)   Ankersmit's Worries

As noted earlier, philosophers of history who have addressed the topic of reference have been few and far between. One of the few to do so has been Frank Ankersmit; however, Ankersmit

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[51] Particularly if we are inclined to argue, *pace* Geyl, that historical study is an "argument without end." (Geyl, 1970, 278).

[52] "For example, you are not in a position to name my son…Even if at the time of his birth you screamed at the top of your lungs, "I name him "Lester"!', you will not have done so" (Jeshion, 2004, 601.)

[53] Unless perhaps, a scholar whose name has more clout within the progression takes it up on their behalf. We might here invoke Evans' producers/consumers model – the producers are those with the requisite expertise in the subject area involved. Between them, it is they who perhaps ultimately decide these things, and the non-experts are parasitic upon their use/decision. See Evans, 1982, 377-379.

[54] Of course, this all largely depends on whether you admit events as part of your ontology or not.

has worries about the suitability of the concept of reference for historical events/ colligatory concepts, and his thesis on historiographical reference is ultimately a negative one. Ankersmit has argued that we should dispense with the very notion of reference in favour of the concept of 'aboutness' (See for instance, Ankersmit, 2012, 79-81). Some of Ankersmit's arguments against reference are, quite frankly, bizarre – for instance, the idea that reference "came to be looked upon with increasing suspicion and disinterest" post-Kripke (Ibid, 87). However, there is a legitimate concern within his writing on historiographical reference that should be addressed.

The worry that Ankersmit has about the establishment of historical reference runs along these lines. As noted earlier, any reference to past events or states of affairs must be mediated via descriptions: and as a result of this, Ankersmit is worried about how we can establish the semantic value of colligatory concepts. For it is a fact that, when it comes to events like 'the Renaissance', 'the French Revolution', and soforth, different historians give different descriptions, and to some extent *mean* different things by these terms (Jackson, 1998, 214). It seems therefore, that there is no semantic value of these terms, if by semantic value we mean a conventional or stable standing meaning.[55] Instead, all we have are various speaker meanings put forward by various historians. Thus, how can we refer to the Renaissance if the only way we can so is via description, and historians keep putting forward different descriptions of what is supposedly the same event?

In fact, I would go as far as to say that Ankersmit believes that historiographical reference is always attributive. Although Ankersmit is, to be blunt, none too familiar with the philosophy of reference, he does cite Donnellan's "Proper Names and Identifying Descriptions" at one point (Ankersmit, 2012, 88-90). Donnellan, of course, made a distinction between the referential and the attributive use of a definite description (Donnellan, 1970). A *referential* use is made if a speaker makes a reference to a particular object X while having in mind exactly what X is; an *attributive* use, on the other hand, occurs if the speaker has no such unique object in mind, but wants to predicate something of said object. For instance, we can say that "Smith's murderer is a violent man" even if we are unaware of the exact identity of said murderer.

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[55] The term 'standing meaning' comes from Richard Heck. A term which has a 'stable' standing meaning is one that "users using the term correctly will always refer to the same object or property."

Thus, when historians write about 'the Renaissance', they refer to it using definite descriptions in the attributive sense: they put forward an account (or a 'representation' to use Ankersmit's terms) of what they think the Renaissance was like. As we have no access to the past outside of these descriptions – i.e. we have no Archimedean viewpoint by which to compare our accounts of the past with the past itself – we can never make a referential use of a definite description on Ankersmit's account.[56] (This is granted a slight departure from Donnellan's view, in that for Donnellan, it is the referential use of a definite description that involves the user having 'something in mind', whereas for Ankersmit, different historians have different things 'in mind' when they use terms like 'the Renaissance'; thus they *mean* different things by these terms.[57])

In sum then, Ankersmit can be said to subscribe to the idea of speaker's reference, but not semantic reference. Terms such as 'the Renaissance' have no conventional meaning; each historian effectively provides their own definition of these terms. Such a view of reference is akin to what has been termed "subjectivist semantics" by David Kaplan (Kaplan, 1989). On this view, when historians use terms like 'the French Revolution' in their accounts, the reference of the proper name is *fixed anew* with each new account; that effectively, each historian *stipulates* the set of events/properties etc to which a term like 'the Renaissance' is to apply to.[58] There may be "*entities* that serve as possible meanings may be regarded as objective, in the sense that the same possible meanings are accessible to more than one person" (Kaplan, 1989, 600). However, "the *assignment* of meanings is subjective, and thus the *semantics* is subjective" (Ibid). Such a view is also similar to an *intention-based semantics*, on which the semantic properties of words are derived from what speakers normally intend to do with them.

Needless to say such a picture is deeply problematic for historiographical realists such as myself. On this account, language use amounts to little more than the "fortuitous coordination of private idiolects (each governed by the descriptive, constitutive intentions of a single speaker), with its own semantics and reference-fixing mechanisms" (Soames, 2007, 339). Such a view brings to mind J.A. Froude's remark quoted earlier about history being like a

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[56] Thus, Ankersmit is ironically close to the constructionist view of historiography that he repudiates; see Ankersmit, 2005, 114.

[57] There are issues with the notion of reference consisting simply of what someone 'has in mind' – for some instances, see Evans, 1982, 325-26.

[58] Indeed, at one point Ankersmit argues that all representations of the Renaissance are "true *by definition....each* historical account of the Renaissance is true, since it can be derived logically from how the historian in question proposes to define the Renaissance" (Ankersmit, 2011, 38). Italics in original.

"child's box of letters from which we may spell any word we please." Clearly this will never do. The onus is then, to show how we can secure what Joseph Almog has called the problem of "trans-mind cross-identification" – on what basis do we cross-identify two distinct minds, and "make them *co*-thinkers, minds *focused* on the same thing? (Almog, 2004). To return to Stalnaker's concept of common ground: what provides the 'common ground' between two historians who hold differing views of, say, the origins of the Second World War?[59] To this question we will now turn.

(iv)   Dickie's Governance Model of Reference

One of the issues that regularly crops up in the theory of history is whether historical knowledge is a sui generis form of knowledge, and if it is, does it need to appeal to some specifically tailored, non-standard criterion of truth, reference and soforth. My position is that historical knowledge is a *sui generis* form of knowledge – and thus we do need a 'philosophy of history' – but this need not involve any special pleading in the form of *sui generis* concepts such as "historical truth" and soforth (which is a move Ankersmit makes). As an argument for this, in this section I want to show how Imogen Dickie's 'governance theory' of reference – a theory developed without historiography specifically in mind – can nonetheless quite adequately serve as a method for establishing reference for events like the Renaissance.

In a nutshell, Dickie's view of reference is one that ultimately takes information as its base; when we fix the reference of something, a subject "receives a stream of information, and marshals it into a body of beliefs, attempting to secure cognitive focus on something outside the mind" (Dickie, 2015, 186). The idea is that these entities outside of the mind and the information we receive about them *govern* our referential representations of them – hence why it is termed a 'governance' theory.

In a situation where a hearer responds to the utterances of a proper name by forming beliefs about it, how is reference transmitted "from the beliefs the speaker expresses using NN to

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[59] "When speakers speak they presuppose certain things, and what they presuppose guides both what they choose to say and how they intend what they say to be interpreted. So much is obvious, but what does it mean to say that someone presupposes something… To presuppose something is to take it for granted, or at least to act as if one takes it for granted, as background information - as *common ground* among the participants in the conversation" (Stalnaker, 2002, 701).

those the hearer would use it to express"? (Ibid, 172). Say we are introduced to a proper name via testimony. We can think of careful uptake from testimony as involving three overlapping phases. The first involves "initial acceptance", which is fairly self-explanatory; the second involves "sensitivity to defeaters", in which an initial default tendency towards acceptance is potentially overridden by factors which undermine accepting the testimony (Ibid, 174).

The third phase is what Dickie calls "narrative construction." We organise the information that we receive concerning a given proper name by marshalling it into a narrative or story, and we thus manage future testimonial input by seeing how well it coheres with the picture we have built up. If the subsequent information that we receive coheres with the picture we have, as well as with our background beliefs, we will add it to the narrative. However, if an "incoming piece of information does not cohere with the existing picture, we either revise the picture, or reject the incoming information" (Dickie, 2012, 57). We can think of this narrative as a mental file[60], which starts off with a core of information, and then which once up and running, acts as filter "through which potential additions to the file are filtered, which is subject to internally-driven updating, and which informs speculation about likely future developments" (Ibid, 68; also Dickie, 2015, 174).

A distinction must be made here between the "developmental core" of a file on the one hand, and the "developmental periphery" on the other. Beliefs in the core are ones we are inclined to hold onto unless we are given an extremely convincing reason to reject. Of those on the periphery; say we are told something about X by a source whose reliability we are unsure of. We are unlikely to allow such testimony to effect the core; however, if I am convinced of X's trustworthiness, I will allow this into the core, and this may be (potentially) accompanied by widespread changes (Dickie, 2015, 178).[61] Thus, we can say that our beliefs about an object or individual are *governed* by that object or individual. If

> S forms a body of <NN> beliefs by careful uptake from a stream of NN testimony whose constituent utterances express beliefs about *o*. Then S's body of <NN> beliefs is itself about *o* if and only if it is governed by o.[62]

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[60] The concept of 'mental files' is Recanati's; the role of a mental file is as a vehicle to store information about a referent. Although Recanati initially specifies that mental files are there to store information gained by acquaintance (i.e. *de re*), he goes on to state that we can open such files in the absence of such an acquaintance, "provided one has good reason to do so" (Recanati, 2012, 167).
[61] Dickie, *Fixing Reference*, 178. There are analogies here with both Quine's notion of the web of belief and Lakatos' characterisation of a scientific research program having a 'hard core' and a 'protective belt.'
[62] Ibid, 179.

How exactly does this move away from the individualist picture of speaker reference I decried earlier? Dickie builds up on a notion introduced in the work of Gareth Evans I drew on in Chapter 3 – that of a division between *producers* and *consumers* of reference. In *The Varieties of Reference*, Evans argued that the distinctive mark of a proper-name-using-practice was the

> existence of a core group of speakers who have been introduced to the practice via their acquaintance with *x*. They have on some occasion been told, or anyway have come to learn, a truth which they could then express as 'This is NN'... Members of this core group, whom I shall call 'producers' (for a reason that will become apparent), do more than merely use the name to refer to x; they have dealings with x from time to time, and use the name in those dealings (Evans, 1982, 376).

At the introduction of a proper-name, all of the name-using participants of a name-using practice will be producers, but this will not be the case for long. Soon, others will be introduced into the practice of using this name to refer. Call such individuals 'consumers', "since on the whole they are not able to inject new information into the practice, but must rely upon the information-gathering transactions of the producer" (Ibid, 377).

In a proper name building practice then, the practice of using NN as a proper name is instigated by a group of producers who have thoughts about o, and use NN to express these thoughts to one another. An encounter with a producer who are using NN provides an opportunity to begin using the name oneself; so "participation in an NN-using practice may be passed from speaker to speaker until subjects joining as new members do so at many degrees of remove from the community of instigating producers" (Dickie, 2015, 184). It is in this way that we are able to think about individuals from whom "we are far removed in space and time, the intervening distance bridged by a series of *governance*-transmitting transactions" (Ibid).

In a footnote in *Fixing Reference*, Dickie remarks that "what I have said here leaves open the question of how producers' [reference] fixing works for proper names of places and events" (Dickie 2015, 185, fn 29). However, it can easily be applied to the naming of and reference to events, as we shall now see. Many historical theorists' accounts of historical practice tend towards a 'lone scholar' view of historiography, which implies that the historian works in

some sort of vacuum, isolated from his or her peers; "solitary thinkers, whose work begins with reflection upon discrete problems raised from scratch" (Dray, 1995, 196). Needless to say this is not the case. A historian is nearly always situated within a research tradition at a given point, and their engagement will stem from this accordingly. The historian will always start out as a consumer. Their entry into a given research tradition will always consist in reading secondary texts on the subject, most often as an undergraduate. If they choose to pursue this interest further and undertake original research in that particular field, at this point they will have the opportunity to become producers. They will start to engage with the original sources and evidence; their beliefs about X, which were previously parasitic upon the work of others, will now be 'caused' or 'governed' by the event itself. They will build up their own mental file; marshalling a plethora of information and converting it into a body of beliefs about X.

And ultimately, they will construct accounts reflecting those beliefs which will be put forth in the public domain for other historians to comment on. On Dickie's account, governance is established by 'producer's rapport';

> A full characterisation of producers' rapport would be an account of the kinds of relations to an object that a group of speakers must have if they are to be in a position to introduce and establish among themselves a practice of using *A* as a name for *O* (Dickie, 2012, 71).

It should be clear at this point how such an account maps on historiographical practice. The reference of proper names for events like 'the Renaissance', 'the French Revolution' and so on, are never fixed in isolation, as Ankersmit's account suggests. On the contrary, this is a communal practice. As Dickie puts it such a practice is the result of "a group of speakers, who have a characteristic kind of rapport with an object [in this case, gaining information about it via the surviving evidence] begin to use the name to collect information about that object that they have in virtue of this rapport" (Ibid).

As we have seen, very few historians will agree on everything concerning a given period or event. But there is nearly always a large core of facts that they will agree upon (I will return to this shortly): call this 'co-variance' among the developmental cores of their respective files on the name in question. One of the other worries that the Ankersmit account gave rise to was the spectre of incommensurability; as his account seemed to preclude any kind of meaningful

debate between historians.[63] Not only does this core provide the requisite common ground, but furthermore, as Dickie notes, communication requires that "each speaker be in a position to understand why the other takes various observations for claims of the form 'A is F', and why the other acts as he or she does having accepted a claim of form 'A is F' as true. (Dickie, 2011, 72).

Finally, while fixing the referents of things like 'the Renaissance' is a thus a proper-name producing practice in the Evans-Dickie mould, it should be noted there that it is a *temporally extended practice*, one that involves historians engaging with each other over time via each other's historical accounts as well as with contemporaries in the same field. Although there will inevitably be a 'baptism' of some form with regard to the proper name of an event – think of Michelet coining the term 'Renaissance' for instance – the reference is not *fixed* there and then; determining the precise referent will be a process that takes place over an extended period of time. (This is where the notion of open texture comes into play).

To recap then. Rather than think of reference-fixing in terms of a definite description, or a purely 'causal' link between the referent and ourselves, we should take as our starting point the fact that we inevitably associate certain information with a given referent. We have a certain body of beliefs about a given proper name – be it 'Napoleon', 'Germany', 'the Renaissance' - which we marshal to form a certain picture of the entity in question. Moreover, the entities in question are the information sources that govern the informational narratives that we construct. Moreover, in the case of historiography, reference-fixing is constrained not only by the causal source, but also by the writings of other historians.

We can see then, how the governance model cashes out in terms of the reference of colligatory concepts. It allows that two historians with radically different views about the nature of a given event or set of events can still be talking about the same thing. Thus, they use the same name even though they have different beliefs. It can also accommodate shifts in reference ala Evans' Madagascar example. For instance, it was once thought that 'Homer' was an individual who wrote the Odyssey and Iliad, whereas now it is believed that several different authors had a hand in the production of these works – but nonetheless, we still use the name 'Homer'.

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[63] Comparing accounts, I would argue, is not the same thing; the picture Ankersmit gives of the evaluation of historical works is somewhat akin to that of a beauty contest; they are all lined up side by side for a panel of judges to pick a winner.

Conclusion

In this chapter I have put forward a theory – that developed by Dickie – that ensures we can secure the reference of colligatory concepts, and incidentally shows that the worries and suspicion that Ankersmit has about reference are far from well-founded. I first argued that event names, despite seemingly being orthographically unorthodox, should in fact be seem as a variant of a descriptive proper name. I then went on to examine how such event names are established, before examining the worries that Ankersmit has vis-à-vis referring to macro-events. While the worries are legitimate, I argued that Ankersmit's solution is a classic case of the cure being worse than the disease. Finally, I showed how we can secure reference to events like the Renaissance, using Imogen Dickie's 'governance theory' as the base.

In the next chapter, I will look at the notion of colligatory concepts in more detail.

5) Colligatory Concepts

In this chapter I will look at the notion of colligatory concepts. As outlined earlier, colligatory concepts in historiography are terms such as 'the Renaissance', 'the Cold War', 'the French Revolution', and so on – terms plausibly referring to sequences of events that historians feel are interrelated in some way, and therefore can therefore pose questions in these terms of historical accounts. Anti-realists about colligatory concepts are that the events expressed in such concepts have no counterpart in the historical past. There is no historical 'entity' such as the French Revolution – this is merely an organising concept that historians utilise.

I will argue for colligatory realism in this chapter – that there are in fact historical entities such as the French Revolution. I will argue that things like the French Revolution, The Cold War and so are ultimately a species of event: they are macro-events, for lack of a better term: events of an extremely complex kind. On my view, 'the Cold War' is as 'real' as seemingly unproblematic entities such as tables or dogs.

The present chapter will take the following structure. In the first section I will briefly recount the origins and development of the idea of colligation in history, before outlining the main arguments put forth for being anti-realists about colligation. I will then refute these and sketch out an argument as to when we are entitled to be ontological realists about events such as the French Revolution. Finally, as a prelude to the following chapter, I will briefly outline a definition of concepts in general.

(i)   The Origins of Colligation

The concept – no pun intended - of colligation is generally ascribed to William Whewell, who argued that all knowledge "essentially involves the antithesis of two elements. One of them is given to us by pure observation, and the other is superimposed by ourselves upon what we observe" (Ducasse, 1951a, 58). For Whewell, colligation was a variant of induction that involved a "mental operation of bringing together a number of empirical facts by "superinducing" upon them a conception which unites the facts and renders them capable of

being expressed by a general law."[64] Whewell referred to this as the "Colligation of Facts", and this was to be applied "in every case in which, by an act of the intellect, we establish a precise connection among the phenomena which are presented to our senses" (Ducasse, 1951b, 218). Whewell's exemplar of this process was Kepler's discovery of the elliptical orbit of Mars; Kepler knew the various points of the Martian orbit, and colligated them using the conception of the elliptical curve.

Although colligatory concepts ultimately sprang from the mind, this is not to say they are merely invented - that they have no counterpart in reality. As Ducasse summarises

> Although the conception may indeed be *suggested* by the facts, it is no more to be described as itself an additional fact observed like the rest, than is the key to a cryptogram to be described as itself observed in the cryptogram in the same way as the letters of it. The letters are *perceived*, the key is *thought of*, even though both the letters and the particular order in which they make sense be facts of the cryptogram (Ibid, 219).

The historiographical notion of colligatory concepts was introduced by W.H. Walsh. Though his views evolved over the course of the thirty or so years during which he wrote about the term, the hinge for colligatory concepts always revolved around the notion of "trends", "developments", and "processes." When we take different events to belong together in a single development, "we do not think of them as being loose and separate in the Humean manner, but rather as having an altogether more intimate relation" (Walsh, 1974, 128). Pace Whewell, the role of such concepts is to group together different events "under appropriate conceptions" (Ibid, 133). Colligatory concepts can perform several functions: they can be used in the "interest of explaining something"; but they can also serve to "characterise and analyse" towards producing "understanding and enlightenment" (Ibid, 136-37).

What did Walsh think the relationship was between the concept used to colligate an event and the events themselves? To colligate is, "broadly, to organise, and it would be generally agreed that any acceptable scheme of organization must have a firm foundation in fact" (Ibid). Colligatory concepts must not be arbitrary: they must be "tailored to fit the facts rather

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[64] Snyder, Laura J., "William Whewell", *The Stanford Encyclopedia of Philosophy* (Winter 2017 Edition), Edward N. Zalta (ed.), URL = <https://plato.stanford.edu/archives/win2017/entries/whewell/>.

than a straitjacket into which the facts must be forced whatever their particular nature" (Ibid, 139). More specifically,

> For every authentic statement containing a colligatory concept it must be possible to produce a series of relevant and connected lower-level statements which count in it's favour, are framed, by comparison, in untheoretical terms, and about whose acceptability historians are generally agreed (Ibid).

The "logical status" of colligatory concepts, on Walsh's view, may be seen as being akin to that of "definite descriptions: they pick out what is to be taken as a single subject of discourse and predicate something of it" (Ibid, 142). And this ties in with the idea that the events that we colligate are not simply "aggregates of externally connected events", but rather, "unitary processes" (Ibid, 143).

In recent decades C.B. McCullagh has given the most clear-headed account of historiographical colligation. McCullagh – correctly – argued that although ideas play a role in the unification involved in *some* colligatory processes, "others are unified simply by the form of change in a subject which they constitute" (McCullagh, 1978, 271). Moreover, while some colligatory concepts may indeed be singular, others – such as for example, 'revolution' – are general, "since such processes occur more than once in history" (Ibid). Not all colligatory terms are general, but some are. To colligate an event is to "show it to be part of a collection of events which can be viewed as a whole", and "colligatory terms describe these wholes" (Ibid, 277). Such wholes must be more than the sum of their parts.

In later writings on colligation, McCullagh started to use the term 'pattern' when talking about colligation. In his entry on the subject in the *Blackwell Companion to the Philosophy of History & Historiography*, he writes that one of the reasons historians write history is not simply to answer why-questions, but to "display a pattern they have discerned among events in the past, a pattern which gives those events meaning and significance, and which sometimes helps to explain them" (McCullagh, 2009, 152). What separates colligatory patterns from regular patterns of cause and effect in historiography is that "they involve seeing individual events as forming a conceptual whole, such as the elliptical path of a planet….[etc]" (Ibid, 160).

Colligation then, can be summed up as a process by which the significant relations between a sequence of events are established by historians. But do these relations, and thus these entities, actually exist in the historical past, or are they simply a figment of the historian's imagination?

(ii)   Anti-Realism about Colligation

On an anti-realist view of colligatory concepts, terms/names such as 'the Renaissance', 'the French Revolution' and soforth, have no counterpart in the historical past; they merely act as organising devices for our information about a particular period. As Kuukkanan puts it, "they tie, group or join objects together. They are thus unifying expressions; but they are "subject-sided" as opposed to "object-sided", i.e. they are not objective (Kuukkanen, 2015, 107). These concepts then, originate from the historian, as opposed to originating in the past. Ankersmit has also been particularly vocal about this. While it might seem "entirely reasonable" that "there be *"something"* in historical reality that corresponds to…for instance, the Renaissance", this in fact is not the case (Ankersmit, 1983, 169). Quite the contrary; "no identifiable objects correspond to most of the terms we use in discussing the past" (Ibid, 175). The historian uses them simply to bring "unity and continuity" to the "chaos and disorder" of the past (Ankersmit, 2012, 45).

For Ankersmit, it seems that a concept like 'the Renaissance' is akin to what Hans Burckhardt calls a *conceptus subjecitivus*: a concept that exists independently of the consistency of its content (Burckhardt, 1991, 45). Historiographical discussions of the Renaissance "do not concern the *existence* of something in the past" (Ankersmit, 1983, 195); indeed, "we cannot mis-describe the Renaissance (because there is no such thing)" (Ankersmit, 1983, 219). The situation in historiography is that "the past is shown by means of entities that do not form part of the past itself and that do not even refer to actual historical phenomena or aspects of such phenomena" (Ibid, 87). Similarly, Kuukkanen writes that "there is nothing 'real' or 'natural' in the ontological sense in how the historian organizes historical phenomena into more general categories like the 'Renaissance' or the 'Cold War'" (Kuukkanen, 2015, 109).

It is somewhat ironic that Ankersmit chides Goldstein for his historiographic constructionism on the grounds that it posits a view of a discipline that is completely disconnected from its

subject matter: for one could argue that Ankersmit's view of the use of colligatory concepts motivates a very similar view.[65] In his review of *Meaning, Truth & Representation*, Branko Mitrovic points out that despite believing that they have no counterpart in the historical past, Ankersmit nonetheless believes that colligatory concepts are "indispensable for the rational discussion of history": but as Mitrovic points out, it is not clear how our knowledge of the past can be enriched by debates which are effectively about the reference of terms which do not exist (Mitrovic, 2014, 286). He also points out Ankersmit himself gives no "criteria by which one could measure the progress of the debate about this nonexisting something" (Ibid). (In contrast to Ankersmit, I believe that historiography does indeed make progress, and will argue for this in the next chapter.)

What motivates anti-realist views of colligatory concepts? It is clear that what lies behind Ankersmit's argument is the idea that a phrase such as 'the Renaissance' cannot be said to pick out a particular *thing*. In *Narrative Logic*, Ankersmit writes that states and revolutions differ from "normal things like dogs and snowflakes" (Ankersmit, 1983, 163). The difference which seems to be in the back of Ankersmit's mind is "that the meaning of terms which refer to "normal things" can be learned ostensively, whereas the meaning of "states," "revolutions," and so forth cannot, but can only be learned by reading about instances of them" (McCullagh, 1984, 398). However, there are many things that exist, but are not available to be picked out by ostension – for instance, one thinks of things like black holes, continents and oxygen.[66] (This of course, is an issue I addressed in Chapter 2.)

Ankersmit likes to contrast the fact that we cannot refer to things like the Renaissance with how we can unproblematically refer to individuals like Napoleon and Hitler. Why are we able to refer to the latter both not the former? The answer is that individual persons are objects that "possess a certain unity and continuity through time" (Ankersmit, 2012, 44). One would never guess from this statement that just what exactly guarantees personal identity and thus continuity through time is an extremely vexed question in the history of philosophy. We might be inclined accuse Ankersmit of indulging in something like a 'common-sense' metaphysics here.

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[65] "We cannot fail to be amazed by the fact that this account of historical practise effectively rules out the possibility of any experiences that historians might have of their object of investigation, that is, of the past itself" (Ankersmit, 2005, 114).

[66] Ankersmit seems to subscribe to the view that "intelligibility presupposes cognitive meaningfulness, which in turn always rests on objective observation. Then, supposing that only material objects (states etc) can be observed, only they can be intelligible, hence real" (Hagueland, 1984, 4).

As alluded to a moment ago, Kuukkanen also pushes the idea that colligatory concepts do not refer to a single 'thing' in the world. Take the Cold War for instance; what "would be a *particular* to which the 'Cold War' refers? The question is rhetorical - colligatory expressions do not seem to instantiate any individual – they do not seem to correspond to any singular object in the historical world" (Kuukanen, 2015, 107). A move that Kuukkanen makes (along with Ankersmit) is to draw a line between unproblematic concepts and colligatory ones. Kuukkanen for instance, contrasts colligatory concepts with kind terms, and argues that the former cannot do the job that the latter do. Colligatory concepts are not taxonomic, whereas kind terms are; "it is not possible to create taxonomic, genus-species, categories of the kinds of the 'Renaissance' in the way that taxonomies of the kinds of dogs, mammals, animals, etc. are created" (Ibid, 110).

What about things like revolutions and wars? Surely these are classificatory terms? Here Kuukkannen starts to backslide a little bit. It is true that in the case of revolution, "different 'revolutions' may perhaps be expected share some common features and differ in some other respects" (Ibid). However, Kuukkanen argues that 'revolution' is not a classificatory concept in way that for example 'planet' is. But it is not entirely clear what at this point separates 'revolution' from something like 'planet'. Although 'planet' "is a nominal kind, and thus has no natural essence, planets nonetheless have features in common; for instance, they all share the feature of traversing around the sun" (Ibid). Although we might baulk at calling 'revolution' a *natural* kind, it is not clear from Kuukkanen's exegesis what the difference is that marks off 'planet' from 'revolution' in terms of their both being classificatory concepts.

Kuukkanen's response here is that we

> should indeed expect that the phenomena that all are called 'revolutions' should be somehow similar, and the natural expectation is that they all designate fundamental changes of some sort. However, while the term 'revolution' is general, *each of these revolutions* is specific (Ibid, 111).

We can surely say the same about dogs and planets though? The term 'planet' is a general one, but Mercury, Mars, Earth etc all have unique features which set them apart from the other members of the set; the same can be said of dogs. Each dog is unique, just as each revolution is, but dogs and revolutions both share some common features.

It is not unreasonable to say that Kuukkanen is struggling here with the issues surrounding classification, and there is much that goes unaddressed. For instance, what about singular colligatory concepts? It is not a necessary or sufficient condition of a colligatory concept that it has to apply to more than one thing; indeed, this is true of concepts generally (think of 'the sun', for instance). There is also an issue with his use of kind concepts. Kuukkanen constantly adverts to the concept of natural kinds; yet not all kinds are natural. Tables and trees are *kinds* of things, but not necessarily natural kinds. Indeed, if History with a capital H describes and explains human actions, then it is hard to see how or why we should wish to get along without anthropocentric kinds! It is clear that the account that Kuukkanen presents in *Postnarrativist Philosophy of History* is confused at best and incoherent at worse, and struggles to get off the ground as a result.

(iii)   Why Realism About Colligation?

At this point the general reader might be forgiven for asking: why all this fuss about colligation? After all, there seems to be a fair amount of agreement between realists and anti-realists about colligation, in that both camps agree that colligatory concepts are indispensable to historical practice: why split hairs over the ontological status of them? The answer in a nutshell is that these events have causal efficacy; I will argue that these macro-events have properties over and above the individual events and actions that make them up. These macro-events then, have emergent properties: they are holistic entities. That said, they are not homogenous and undifferentiated, but are internally complex and have internal properties. They combine unity with a great deal of diversity.

What is it that links the events that we take to comprise the First World War together? Clearly those at the head of the countries involved were convinced that they were fighting in a war, and directed their countries armies and resources accordingly. The events that took place in July/August 1914 provided the basis for much of what occurred over the next four years; the French fought to remove the Germans from their territory (as did the Russians); the British fought because they wanted to check German hegemony, and so on. Arguably the point here is that one of the reasons that we are inclined to describe the events of 1914-18 as

a war is because the majority of the individuals involved certainly *thought themselves* that this what was taking place. We can find a contemporary example in the concept of 'Brexit'. I am tempted to call this something like 'first-order colligation': where colligatory concepts are used by *contemporaries* to makes sense of contemporaneous events.[67]

How do we deal though, with something like 'the Holocaust' or 'the Renaissance', both identified retrospectively?[68] Why do we need to utilise or postulate an event 'after the fact', as it were? Can we just allow that the events of the Holocaust – i.e. six million deaths – took place while denying that anything supervenes on them? This won't do though; for to deny the colligatory concept 'the Holocaust' is in effect to deny that the deaths in questions were interlinked as part of Nazi policy to eliminate European Jewry. And this in turn leaves the door open for a Holocaust denier to states that those six million deaths were largely unconnected – indeed, this is exactly one of the arguments that said deniers *do* use.[69] To use a slightly less emotive example: we would hardly be inclined to say that the millions of deaths or the concentrated destruction of land that took place in Europe between 1914-18 was a giant coincidence. Yet the anti-realist view of colligation seems to commit us to something like this view: that there is nothing to explain about the Holocaust because there was no Holocaust in the sense of a interrelated and unified set of deaths: only six million seemingly unrelated deaths.

In one of his papers on colligation, McCullagh writes that a process of historical change worthy of being colligated "cannot be understood as merely the summation of the events which compose it"; rather, when a colligatory concept is used, it indicates "the particular nature of that process and so point [sic] the particular significance of the events which compose it" (McCullagh, 1978, 271-272). For instance, a revolution is

> not *merely* the series of events which, in any instance, compose it. It is that series of events in virtue of the sort of change it brought about, and the events involved are significant in that they contributed to that sort of change (Ibid, 272).

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[67] Though of course, contemporaries referred to what we now call WW1 as 'The Great War."

[68] The Holocaust of course, has become a touchstone for historiographical realism. Ankersmit got into a terrible tangle trying to argue that the Holocaust had a concrete existence that the Renaissance didn't. See Ankersmit, 2012, 82-83.

[69] In his book recounting the David Irving trial, Richard Evans notes that one element in Holocaust denial is "a refusal to accept that the extermination of the Jews was systematic, organised or centrally directed" (Evans, 2002, 125-26).

We cannot then, simply colligate *any* sequence of events; the unification that binds them together must not be figurative. To paraphrase Max Black, to argue that bringing together a set of events is something achieved by some peculiar act of mind leads to the view that the human mind "can annihilate space and time in a most extraordinarily productive way" (Black, 1971, 620). As McCullagh puts it, "where there is nothing to unify the process, it does not form a unified whole in terms of which its parts can be colligated" (McCullagh, 1978, 284).

(iv)   Reductionism, Supervenience and Emergent Properties

In this section, I want to argue that colligatory concepts like the 'the Renaissance', 'the First World War', and so on, denote actual entities. They are collections of events and actions, but they are not simply aggregates of events and actions; they form something over and above the entities that compose them. This of course, takes us into the realm of emergence and supervenience.

It would not be unfair, I think, to associate anti-realism about colligation with a form of reductionism. That is that something like 'the French Revolution' is nothing over and above the individual actions, events, and states of affairs that make it up. As always, we should define our terms: the term 'reduction' in philosophy expresses

> the idea that that if an entity x reduces to an entity y then y is in a sense *prior* to x, is more *basic* than x, is such that x fully depends upon it or is constituted by it. Saying that x reduces to y typically implies that x is nothing more than y or nothing over and above y (Van Riel and Van Gulick, 2019).

Reduction has epistemological and ontological forms. An epistemological notion of reduction is arguably cashed out in explanatory terms: that an event or event expressed by a concept like the First World War needs to be explained in terms of its more basic elements. Thus, an epistemic reductionist about science holds that we are, or will be able to, reduce our current science to a more fundamental one. Ontological reduction argues that the world essentially reduces to a basic set of entities, although whether we will ever to able to epistemically

represent this is an open question. (As ever, epistemology and ontology turn out to be two sides of the same coin.)

An anti-realist about colligatory concepts then, is an explanatory reductionist in the sense that they believe that terms like the 'First World War' are merely heuristic concepts; we use these concepts to *talk about* various segments of the past, but ultimately these concepts reduce to more elementary individuals, namely events and actions. Thus we talk of the 'First World War', but in fact only a series of battles, political decisions, and so on that took place between 1914-18: (the notion of) 'the First World War' is itself epiphenomenal. (It is hard to say whether this view also entails ontological reductionism, given that (as we saw in Chapter 2), many historians are somewhat sketchy as to ontological commitments in general.)

The tool of many anti-reductionists has been the concept of supervenience. The starting point is the idea that the world is a system that has some kind of *structure*; it is not a "mere assemblage of unrelated facts, events, and objects" (Kim, 1984, 153). Central to the notion of interconnectedness is the idea of dependence: "things are connected to one another in that whether something exists, or what properties it has, is dependent on, or is determined by, what other things exist and what kind of things they are" (Ibid). Supervenience then, roughly put, is a relation between two collections of properties, one collection of which is at a higher level than the other. (Humphreys, 1996, 339). We say that the properties at the higher level supervene on those at the lower level. A strong formulation of supervenience can be put thus:

> A strongly supervenes on B just in case, necessarily, for each x and each property F in A, if X has F, then there is property G in B such that x has G and necessarily if any Y has G, it has F (Humphreys, 1997b, 10).

So we might be tempted to say that macro-events such as the Renaissance or First World War *supervene* on lower-level events and states of affairs. I want to argue here, however, against supervenience and for emergentism.

With regards to the former: despite the fact that supervenience is supposed to be an alternative to reductionism, there is "a strong residue of suspiciously reductionist terminology" within many accounts of supervenience (Humphreys, 1996, 338). In saying that 'A supervenes on B', we seem to be saying something akin to: because A's existence is necessitated by B's existence, all we need ontologically speaking is B (Ibid). Another problem is the fact that while supervenience has adopted the reductionist picture of levels, it does not appear to allow for upwards and downwards causation between them: thus

"supervenient causation is…no form of causation that a realist should accept" (Humphreys, 1995, 109). Finally, there is nothing in the definition of supervenience that explains why, given two sets of properties with one set supervening on the other, that the supervening set has an inferior ontological status to the subvening set (Humphreys, 1996, 338).

Instead, Humphreys argues that we should replace supervenience with *emergence*, and more specifically, *emergent properties*. As the editors of a recent volume on emergence put it, to claim that something is emergent

> involves asserting something about the relationship between the thing and its more fundamental parts….although a thing is dependent on its parts (that is, it could exist without them) it is also something novel with respect to them: it is something new and distinct (Gibb et al, 2019,1).

As alluded to above, I am advocating ontological emergence here rather than epistemic. Epistemic emergence is relatively uncontroversial; it is often convenient to represent the world using fictional entities. Ontological emergence however, implies "that the higher level will contain entities that are just as real as those found at the lower level" (Ibid, 1-2).

A rough characterisations of emergent *property* is one that 'arises' out of more fundamental entities, and yet are irreducible with respect to them. The property that emerges is not merely a new value of existing properties, but novel properties that are "qualitatively different" from the properties from which they emerge (Humphreys, 1996, 338). To give an example of this, Humphreys quotes G.L Sewell:

> Macroscopic systems enjoy properties that are qualitatively different from those of atoms and molecules, despite the fact that they are composed of the same basic constituents, namely nuclei and electrons. For example, they exhibit phenomena such as phase transitions, dissipative processes, and even biological growth that do not occur in the atomic world. Evidently, such phenomena must be, in some sense, collective, in that they involve the cooperation of enormous numbers of particles: for otherwise the properties of macroscopic systems would essentially reduce to those of independent atoms and molecules (Ibid, 342).

In sum then, supervenience does not provide us with any understanding of the relationships between ontological levels: for this we need emergence.

I am therefore, suggesting that macro-events like the First World War should be treated not as being reducible to the micro-events and actions that compose it; not that the lower-level entities subvene on the higher level ones. Rather, the macro-event emerges from the lower-level constituents - a 'fusion' (the term is Humphrey's) of the lower-level constituents - which produces an event which has certain properties not possessed by the constituents. (That said, macro-events are not homogenous and undifferentiated: they combine unity with a great of diversity.) Beckermann characterises the notion of emergence thus: "when the complexity of material configurations reaches a certain critical level, genuinely novel properties emerge, properties that have never been instantiated" (Beckermann, 1992, 15). Now consider the complex series of interlocking factors that conspired to produce the First World War: the assassination of Franz Ferdinand; Austro-Russian rivalry in the Balkans; the Schlieffen Plan; British foreign policy traditions…this series of states of affairs and events reached a critical level, and the outbreak of a European war was the result.

We can think of such macro-events as being akin to complex systems. Consider the following as the two poles of systems: at one extreme, we have systems in which a particular property "is exhibited simply because the parts of the system possess that property. In this case we are properly led down a level to focus our whole inquiry on the components" (Bechtel & Richarson, 1992, 265-66). At the other extreme, we have systems where their behaviour can be explained entirely by the way the parts are put together. An intermediate position

> allows for systems in which the parts do make distinctive contributions, but in which the way the parts are put together imposes interesting constraints on the actions of the independent parts and leads to unexpected behaviour in the system as a whole. In these contexts one may appropriately examine the component parts of the whole system, but also maintain that the behaviour exhibited by the whole system is novel and not predicted from what we know of the behaviour of the parts in isolation (Ibid).

Recall that earlier I stated that macro-events are not homogenous and undifferentiated, but are internally complex and have internal properties. The definition of an intermediate system given here reflects this. Emergence then, can be seen as a way of characterising relationships between complex entities and their parts.

Furthermore, one of the attractive features of taking an emergentist view of macro-events is that it chimes with the general conviction that the outcome of many historical events could

not have been predicted in advance, even with access to all the relevant facts/information.[70] (This of course, is one of the rocks upon which the Hempelian covering-law model of explanation foundered.) Now consider this aspect of emergence: emergent properties are novel not only in the fact that said property has "never been instantiated before, but also in the sense that it could not even have been predicted in advance" (Ibid). C.D. Broad – one of the early writers on emergentism – wrote that "the characteristic behaviour of the whole *could* not, even in theory, be deduced from the most complete knowledge of the behaviour of its components, taken separately or in other combinations, and of their properties and arrangements in this whole" (Quoted in Beckermann, 1992, 16). This is a sentence that could have been written by a historian; for there are numerous historical events that only a prophet or a fool would have predicted beforehand.[71]

Colligatory concepts then, are not simply heuristic devices, but denote macro-events which have genuine existence and properties, that should be considered as something akin to mereological wholes. These events emerge from the interaction of many thousands (sometimes millions) of micro-events and states of affairs; said macro-events are not simply aggregates or collections of events and affairs. Consider one final example. Take a football match (or "soccer", as Americans know it as). When we talk about football matches, we say things like "it was a tactical chess match", or "a fast and furious match in which defence was at a minimum." These are properties which we cannot get by simply enumerating all the individual actions from the match (x kicked the ball to y, it was intercepted by z, who dribbled the ball over the halfway line, and so on.) They are properties that emerge from the whole.[72]

To be sure, this is somewhat of a controversial view, in that what exactly the concept of emergence is and what it implies is still being worked out generally. Furthermore, applying the notion of emergence to events is, to the best of my knowledge, a fairly novel use of the concept: emergence tends to be used in trying to account for things like mind-body interaction, as well as characterising various processes in biology. However, I believe my

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[70] It might be argued that this actually isn't much of an improvement over the anti-realist view, which sees events as chaotic. However, we hold that an event could not have been predicted beforehand without this entailing that events are chaotic, in the sense that they are unintelligible – simply "one damn thing after another", as Elton would put it.

[71] To name two off the top of my head: The Bolsheviks gaining power in 1917; Margaret Thatcher surviving the Falklands War and the Miners Strike, but almost having to resign over the government's handling of the sale of a helicopter firm (ie Westland).

[72] A question remains though; in a case where we have alternative colligations, how do we choose between them? This is a question I hope to look into in the future, but in principle I believe that there is a rational way of choosing between them.

account of what we might call 'event-emergence' is prima-facie plausible; it certainly at this stage provides a plausible alternative to colligatory anti-realism.

(v)   Colligation, Unification, Understanding

In this section I want to briefly outline the role that colligatory concepts play in terms of our understanding the past. We can characterise the two-stage process I have argued for in this thesis between fact and interpretation as something like a divide between knowledge and understanding. Historical facts provide us with knowledge, but on their own cannot provide understanding: and it is arguably the point of historiography to provide understanding about the past. This is where, in part, colligation comes in.

It can be argued that the historians use of colligation and colligatory concepts can be seen as something akin to the role that unification plays in science. In his paper "Explanation and Scientific Understanding", Friedman argued that science provides understanding by the unification of general phenomena. On his view, the understanding provided by science was "global rather than local" Scientific explanations

> do not confer intelligibility on individual phenomena by showing them to be somehow natural, necessary, familiar, or inevitable. However, our over-all understanding of the world is increased; our total picture of nature is simplified via a reduction in the number of independent phenomena that we have to accept as ultimate. (Friedman, 1974, 18).

The objection to Hempel's covering law model then, comes on the grounds that "contrary to Hempel's assumption scientific explanations rarely deal with singular events. Far more often science is concerned with explaining general regularities or patterns of events" (Faye, 2015, 5).

For instance, it was once thought that celestial laws were different from those that governed the Earth; however, when Newton united both realms under a single set of laws, our understanding of nature increased as a result. Here is a case then, where understanding accompanied "the realisation that a great number of seemingly different and unconnected

phenomena can be accounted for in terms of general principles involving only a fewer number of basic entities" (Ibid, 8).

We can say the same about historiography. Historians are concerned with not just with singular events; they are concerned with large sequences of events, which is why we use colligatory concepts like the French Revolution, the Renaissance, and so forth. A fact in isolation is a tautology in terms of its meaning: "Hitler declared war on Poland in 1939" means nothing more or less than "Hitler declared war on Poland in 1939." This hold not just for historiography; in general, "no phenomenon out of a given context of inquiry points to anything beyond itself" (Ibid, 61).

Recent work on the idea of understanding has stressed the role of seeing how things 'fit together.' As Wayne Riggs puts it, a key sense of understanding is that of "an appreciation of order, pattern, and of how things 'hang together'" (Riggs, 2007, 217). Call this 'understanding as coherence'. In this sense of understanding, there is "an important difference between merely believing a bunch of true statements within a subject and having an understanding of M (or some part of M) is that one somehow sees the way they 'fit together'" (Ibid, 218). Jonathan Kvanvig also characterises understanding in this way; understanding requires "the grasping of explanatory and other coherence-making relationships in a large and comprehensive body of information. One can know many unrelated pieces of information, but understanding is achieved only when informational items are pieced together by the subject in question" (Kvanvig, 2004, 192).

Ultimately this is a chapter about colligatory concepts, not understanding, yet we need to talk a little about what such concepts enable us to 'do' in historiography. Colligatory concepts – which provide understanding – also play a key role in explanation. We need not go through the tortured history of explanation in historiography here – i.e. the whole covering-law debate– nor indeed theories of explanation in general.[73] What I will simply note here is that the divide between 'explanation' and 'understanding' that was paradigmatic of much thought and writing from the beginning of the twentieth century onwards has in recent decades

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[73] A book really needs to be written looking back on the whole 'covering-law period'. As it stands, one may as well start with Hempel's original paper, "The Function of General Laws in History"; the classic anti-Hempel response is W.H. Dray, *Laws and Explanation in History*.

collapsed. Explanation and understanding – although still ultimately distinct concepts – are now seen as two sides of the same coin.[74]

Hempel (among others) accepted that understanding came with explanation, but that the latter was of no real epistemic value. Now however, we can see the "fact that understanding comes with explanation is not a contingent feature of explanation, but is the purpose of making explanations" (Faye, 2015, 2). We use colligatory concepts to help us pick out the various interconnections between certain events and states of affairs, and the understanding that these concepts provide in turn helps to provide the explanations that we seek. As I argued in Chapter 2, there is a difference between knowledge and understanding in history; and as we have seen, Walsh ascribed a key role in colligation to producing understanding.[75]

A feature of colligation is that, although concepts such as revolution are rooted in the events themselves, as we have said, that there was a revolution in France in the late eighteenth century is not just 'another fact', available to inspection, as it were. To illustrate what I mean, take Kepler's discovery of the elliptical orbit of Mars. One could not observe the orbit *itself*; but only the various positions of the planet Mars. As Whewell said, "the path is something contributed by the mind of the scientist, and in fact it took Kepler a lot of work to discover it" (McCullagh, 1978, 156). But it is no less 'real' for the fact that the mind needs to make this contribution. Similarly, the physical events that constitute something like the French Revolution were observable and real, but the "constitutional revolution that they achieved, from a feudal to a democratic form of government, is something that historians discover by a process of reflection" (Ibid).

One of the interesting things about colligatory concepts is, as with narrative – which I look at in Chapter 6 – both realists *and* anti-realists are agreed upon the centrality of colligatory concepts in terms of providing an understanding of the past. I spoke earlier of the difference between knowledge and understanding; this is a point that has also been made by Louis Mink (although he sometimes talks of 'comprehension' instead of understanding). Mink agree with Walsh (and myself) that "comprehension is not knowledge, nor even a condition of knowledge." In a (somewhat purple) passage he writes that comprehension is

> the human activity by which the elements of knowledge are converted into understanding, it is the synoptic vision without which (even though transiently and

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[74] See for instance, some of the essays in De Regt et al, 2009.
[75] "The whole object of the colligatory exercise is to increase understanding" (Walsh, 1974, 140).

partially attained) we might forever pass in review our shards of knowledge as in some nightmare quiz show where nothing relates "fact" to "fact" except the fragmented identity's of the participants and the mounting total of the score (Mink, 1987, 55).

However, (as we shall see in more detail in Chapter 7, Mink combines this with a thoroughgoing anti-realism about historiography.

Colligation then, plays a key role in attaining historical understanding, which is one of, if not *the*, primary aim of historiography.

Conclusion

In this chapter I have investigated the reality of historical entities such as 'the Renaissance', 'the French Revolution' and so on. I have argued that we are entitled to be realists about such colligatory concepts, as these concepts actually denote macro-events; and that these events have as much reality as 'common-sense' entities like chairs and tables. These events taken as a whole possess emergent properties which we cannot get by simply aggregating their parts.

The reality of events such as the Renaissance, the Cold War and so on, helps not only to explain why we gain understanding through the deployment of colligatory concepts, but also contributes towards historiographical realism. Colligatory concepts play an indispensible role in historiographical practice because they possess referents. They function not as heuristic devices (although they may perform this function as a by-product), but they enable to trace the relations between events and thus aid in explanation and understanding.

6) Historiographical Progress

In this chapter, I want to examine the issue of historiographic progress, and in particular to account for what enables this progress to take place. I will argue for what we might call an 'institutional theory' of progress; that the disciplinary standards that were put in place when history became a professional discipline in the second half of the nineteenth century provided mechanisms which, while they do not guarantee progress, make it much more likely to occur. I will also investigate an additional aspect of historiography which I feel has been key to aiding progress, that of specialization. I will argue that this progress consists in increased knowledge and understanding of the historical past, while rejecting the notion of verisimilitude and the idea that we are approaching 'ultimate history'.

What does this have to do with historiographical realism? As I will demonstrate momentarily, progress is a fact of historical practice; and as this thesis has taken as it's starting point historiography as it is actually practised, then we need to account for this progress. Furthermore; most historians would concur with Pieter Geyl's famous remark cited in Chapter 4 that historiography is 'argument without end.' On the face of it, this might seem to point towards scepticism: if this is indeed the case, then, historiography is, to paraphrase Elton, simply 'one damn historical account after another', with no cognitive direction of improvement. Thus, it is necessary to show that this is not the case; that historians are not simply spinning their wheels, as it were.

This chapter will proceed as follows. I will I will outline how historiography became professionalised, as this development is key to the institution of standards. I will then show how progress emerges from these professional standards, and that the institutional framework in question allows for an increased historiographical understanding. I will then focus on a key aspect of the progressive mechanism in historiography, that of specialisation. I will conclude by outlining why I reject the notion of 'truthlikeness' as a model of historical progress, as such a model might seem the obvious route for a realist about historiography to take.

(i) Prelude: Historians and Progress

The pendulum has swung from one extreme to the other as far as historiographical progress was concerned in the twentieth century. At the start of the twentieth century, any who asked if historiography made progress would have been rewarded with an unhesitant 'yes'. Given the fact that history had recently become a professional discipline – with an established method, journals, and so on - it was surely inevitable historiography could only make progress from hereon in. Lord Acton famously proclaimed the time would come when we would have "ultimate history" – full and final accounts of a given historical subject.[76] After two World Wars and the Holocaust humanities faith in progress in general was considerably damaged; and this had a knock-on effect on the idea of progress in historiography. Acton's clear-eyed confidence at the start of the century was replaced by a despairing outlook at the end of it, exemplified by Peter Novick's despairing conclusion that "there was no king in Israel."[77]

However, despite the fact that no-one would now seriously countenance the idea of 'ultimate history', there are many who are still quite happy with the idea that historiography can in fact be said to exhibit/make progress. To give a brief cross-section: E.H.Carr – no copper-bottomed historiographical realist - wrote that "the historian of the 1920s was nearer to objective judgement than the historian of the 1880s, and…the historian of today is nearer than the historian of the 1920s; the historian of the year 2000 may be nearer still" (Carr, 1962, 130). This suggests a fairly linear view of progress. In contrast, Eric Hobsbawm (another Marxist incidentally) felt that "history has made progress this century [i.e the 20th], in a lumbering and zigzag manner, but [still] genuine progress" (Hobsbawm, 1998, 97). Frank Ankersmit – again, no card-carrying realist - too believes that historiography progresses: "nobody will doubt that there is progress in the discipline of historical writing: we know far more about the past than ever before" (Ankersmit, 2012, 84-85).[78] (Note that Carr

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[76] In the preface to the *Cambridge Modern History*, Acton wrote that "Ultimate history we cannot have in this generation; but we can dispose of conventional history, and show the point we have reached on the road from the one to the other." (Quoted in Carr, 1962, 4).

[77] See Novick, 1988, Chapter 15. The phrase comes from C.I. Lewis, commenting on the development of multi-valued logic: "First there was one logic, then there were two, then there were several; so that now . . . the state of logic is "that of Israel under the Judges, every man doeth that which is right in his own eyes" (Quoted in Novick, 1988, 136).

[78] Ankersmit wasn't always this sanguine about the idea of historical progress however; compare this with Ankersmit, 1989.

talks about "objective judgement" and Ankersmit about knowledge; the two are connected however; objective judgement helps to produce knowledge).

Before we look at how this is achieved, how are we defining the term 'progress'? The concept of progress undoubtedly implies change; but change alone is not enough. After all, there "is change as the tide comes in and goes out, but that is hardly progressive. Progress implies that there is change in a certain *direction*. You must be going somewhere to have progress" (Ruse, 1996, 19). As J.B. Bury put it in his seminal work on the subject, the idea of human progress is "based on an interpretation of history which regards men as slowly advancing- *pedetemtim progredientes* - in a definite and desirable direction" (Bury, 1920, 5).

Direction alone however, is not enough either. If I set out to lose weight, and I gain a stone, there is change and direction, but not progress. Progress, "more than anything, implies direction toward an improved state" (Ruse, 1996, 19). A distinction needs to be made at this point between *absolute* progress and *comparative* progress. As Ruse puts it, if one "arrives at the Heavenly City, one has made absolute progress. If one makes a bigger and better atom bomb, one has made comparative progress" (Ibid). Thus, progress is a "a normative or goal-relative - rather than purely descriptive-term" (Niiniluoto, 1980, 427). To say that the move to theory B from theory A constitutes progress is to say that B is an *improvement* on A in some respect. The term 'progress' here can be contrasted with a more neutral one like 'development' (Ibid, 428).

(ii)   An Institutional Model of Historiographical Progress

Prior to the nineteenth century, historical practice and writing was largely an individual affair, with no agreed-upon standards or methodology. However, this changed in the middle of the nineteenth century with the institutionalisation of historiography, the result of which saw history become a professional discipline. What do we mean by 'professional'? Siegrist defines professionalization (as summarised by Lindelbach) as an ideal type that encompasses the following: an existing activity (in our case history) becomes

> a specialized profession requiring an ever greater standardization, formalization, and thus homogenization of an education based on fixed curricula; the knowledge acquired is accredited through exams and confirmed by certificates; the profession

increasingly defines standards of education, observing their compliance, and thus also controlling access to the profession itself; and the education produces experts, endowed with esoteric skills, who then monopolize the supply for their services (Lindelbach, 2014, 78).

What about 'institution'? The term encompasses several dimensions. On the one hand, it refers to "the material dimension of an organisation with its structures, committees, staff and funding"; on the other, it "encompasses a social and symbolic dimension as institutions set norms and rules and provide orientation" (Ibid). They are also places for processes of social negotiation, work, and communication structures", in addition to shaping the "cognitive structures of activists that take place within in them" (Ibid).

As Lindelbach notes, institutions of higher learning (which have tended to be universities[79]) were "crucial for the development of the historical discipline because they shaped the process of professionalization" (Ibid). The first move towards the standardisation of historical practice was the introduction of the seminar in universities; unlike lectures, seminars introduced the principle of dialogue, such as discussion of research projects and results, as well as attempts to decipher primary sources. Seminar-based teaching "significantly contributed to the methodological standardisation of the discipline, as subsequent generations of historians were introduced to the critical assessment of primary sources as well as to the writing of scholarly papers" (Ibid). Additionally, the establishment of academic journals enabled the development of social networks among historians (ie: historians started communicating with each other instead of working in isolation), as well as consolidating academic standards. Such journals published the results of research, and their reviews and bibliographies disseminated information "and served to disseminate and consolidate disciplinary standards" (Ibid, 84).

Institutionalisation was crucial for the development of modern historiography. History as it is currently practiced rests on a framework of institutions: "without libraries, archives, and learned societies, the study of history would become little more than a survey of noble ruins" (Fussner, 1962, 60). To give the example of Britain: the establishment of a journal (*The English Historical Review* in 1886), societies (the Royal Historical Society in 1868 and the

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[79] But not always – see the *grandes ecoles* in France for instance.

Historical Association in 1906) and research institutions (the British Academy in 1902 and the Institute of Historical Research in 1921) were all important steps towards putting historiography in Britain on a professional footing (Parker, 1990, 88).

The institutional framework which historiographical progress rests upon is akin to intuitionalist theories of art. Such theories in aesthetics take as their starting point an attempt to define what exactly a work of art is. Perhaps the exemplar of such a theory is that put forward by George Dickie. Dickie's institutional theory evolved over time, with his later version centred on the idea "that the status of being art is not something that is conferred by some agent's authority, but instead derives from a work being properly situated in a system of relations" (Stecker, 2005, 147). Central to this is the notion of the 'Artworld' (which Arthur Danto's writings on aesthetics also heavily lean on): a work of art is something upon which "some group or subgroup [i.e. the Artworld] of a society has conferred the status of candidate for appreciation" (Dickie, 1969, 254).[80]

Something similar can be said for historiography. In theory anyone can write a work of history, in the same way that anyone can construct/constitute a candidate for being a work of art. However, in order for it to be taken seriously as a work of history, it has to meet certain criteria set out by the historical profession. I will set out some of these standards below; the point to be made here is that works that do not meet the standards set up and maintained by the historical professional will not be taken seriously as "proper" works of history. The age of the internet has seen an explosion of works of amateur history; but these works rarely trouble the pages of professional historical journals, usually with good reason.[81]

Indeed, it can be argued that an 'institutional' theory of historiography works better than an institutional theory of aesthetics. While Dickie's institutional theory has struggled in terms of providing a definition for what a work of art is (see for instance, the criticism of Stecker, 1986), an institutional theory of historiography seems to work quite well. A book/article is a *bona fide* work of history if and only if it conforms to the standards laid down by the historical profession at the time of its composition. While "unanimity in the artworld is rather

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[80] The classic exemplar of this is perhaps Duchamp's Fountain: what makes this a work of art in a way that say, the urinal in my office isn't? The answer is that "Duchamp's act took place within a certain institutional setting and makes all the difference" (Dickie, 1969, 255).

[81] A lot of these works tend to be concerned with conspiracy theories: who shot JFK, did we land on the moon, etc. The other major source of amateur history is concerned with local and/or family history.

rare" with regards to what counts as a work of art, by contrast we find very few disputes in historiography over what counts as a bona fide work of history (Young, 1995, 332). To give a somewhat hackneyed example, the works of David Irving are now not seen as constituting acceptable works of history, due to the methodological issues (i.e. deliberate falsification) that pervades his works.[82]

(iii)   Standards and Progress

How exactly then, does the institutionalisation of historiography ensure progress? The process is relatively simple. As a result of professionalization, historical works are expected to meet certain standards.[83] They are expected to adhere to broadly Rankean criteria in terms of citing sources and indeed making use of original source material, as well having mastered the relevant secondary literature in the field. Professionalization establishes epistemic standards, and these standards give us confidence that we are correct in what we are saying. Of course, historical accounts are defeasible – any kind of realism has to be committed to some form of defeasibility – but these standards ensure that we are entitled to make a certain move at a given time.[84] This of course, adverts to some of the ideas I set forth in Chapter 3 in terms of making use of Brandom's pragmatism, and in particular the notion of the game of giving and asking for reasons.[85]

I want to take a brief tangent at this point with regards to having to having mastered the secondary literature in the field; historiography may be said in this sense to be *backwards* looking: but not in a pejorative sense. As Kuhn noted, the work of scientists is always undertaken with one eye on their predecessors, as opposed to looking *forward* something. In

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[82] Another more interesting and perhaps more borderline case might the recent phenomenon of historians like Simon Schama and Niall Ferguson – historians who utilise teams of researchers to do their legwork for them. There are epistemological questions over this practise that have yet to really be fully explored. This is another topic for another day though. (And in fairness to both, before they became "superstar" historians, for lack of a better word, both wrote respectable works of history).

[83] As Eckhardt Fuchs puts it, "the professionalization of history and the establishment of the 'research ideal' was tied, by professionalization, to objective and systemised knowledge, or in other words, factual knowledge. This expert knowledge could be verified, mastered, and extended only by specialists who had acquired the requisite theoretical and methodological standards by a through education" (Fuchs, 2000, 247).

[84] One thinks here of Peirce's view of inquiry, where "truth is conceived as the property that we can hope to steer our enquiry to home upon: the beliefs that inquiry furnishes to us are beliefs that it is rational for us, however fallibly, to persist in until specific grounds for doubt present themselves" See (Wiggins, 2004, 119).

[85] Kuukkannen's *Postnarratvist Philosophy of Historiography* makes a similar use of pragmatist notions; indeed, Brian Fay argued that it should have been called *Pragmatist Philosophy of Historiography*. As we saw earlier though, my views differ considerable from Kuukkannen's.

The Copernican Revolution, he wrote that the "extent of the innovation that any individual can produce is necessarily limited, for each individual must employ in his research the tools that he acquires from a traditional education, and he cannot in his own lifetime replace them all (Kuhn, 1958, 183). Change in science is most intelligibly understood as being a response towards *existing problems*, rather than at aiming towards some undefined truth.

Thus, when scientists construct theories, they do so with their immediate predecessors in mind. Wray gives the example of Newton: on the notion that science is aiming towards some kind of 'ultimate truth', it follows that Newton would had to have been thinking about particles that moved at the speed of light. Of course, Newton never dreamt of such a thing: "his concerns were continuous with the concerns of his predecessors: understanding the behaviour of falling bodies, pendulums, motion propagated through fluids, projectiles, and orbiting planets and satellites" (Wray, 2012, 113-114). The acid test for scientists theories is always that they are better in some way that the ones that immediately preceded them.

This does not entail any kind of anti-realism however; it is simply to say that the notion that we compare our scientific theories 'with nature' is incoherent. But this is not to say that nature plays no role in the construction of said theories, or that scientific theories do not successfully mediate nature. On the contrary, nature provides the *content* of scientific theories: in *Structure*, Kuhn famously talks of scientists having to "beat nature into line" to get it to fit their theories – a remark that makes it hard to construe Kuhn as the idealist some have claimed him to be (Kuhn, 1962, 134).

Historiography operates in a similar way. In particular, when a historian undertakes a piece of research in a given field, they will have one eye on their predecessors.[86] For example, the current standard biography of Hitler is that written by Ian Kershaw; 'standard' as in that this the standard that a successor will have to meet in terms of scholarship (perhaps "state of the art" might be a better term than "standard"). Future biographies of Hitler will be reviewed and judged upon how well they compare to Kershaw's; do they say anything new, or do they refute anything put forward in the former work?[87] Kershaw's book has raised the bar in terms

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[86] Collingwood's thought ran along these lines when he talked of 'history of the second degree' (ie historiography) being just as important as 'history of the first degree' (Dray, 1995, 294).
[87] To turn to a different subject; Martin Stannard's biography of Evelyn Waugh is often at pains to point out where Waugh's previous biographer, Christopher Sykes, has made errors of fact. Similarly, Robert Skidlesky

of Hitler scholarship; should someone meet or even exceed the standards set, then progress will have been made. Indeed, we cannot have progress *without* standards; that is, without some sort of benchmark to either match or surpass. [88]

And just as in scientific theories, where nature plays the role of determining whether a theory is successful or not, in historiography it is the past-in-itself which provides the content of historiographical works, even though such works can only be evaluated with respect to each other. Understanding is a *cognitive* endeavour; it always involves an individual subject. Thus, when we say that historical accounts must be compared with each other in determining if progress has been made or not, this is not because historical accounts are 'logical entities that have no counterpart in the past' - as Ankersmit would have it - but rather, because they are endeavours which necessarily involve human cognizers, while facts that they contain can be checked – indirectly - against the past in itself. [89]

To return to the idea of standards and how they increase the odds of making progress: as the epistemological bar rises, historians have to keep up. What was acceptable for Macaulay is no longer acceptable now. [90] There are a couple of analogies from biology we might use here; the Red Queen hypothesis argues that species have to "run" in order to stay in the same place – i.e. avoid extinction. [91] Similarly there is Stephen Jay Gould's 'drunkard's walk' analogy; that evolution is a one-way process: there is a limit on the simplicity of organisms, and this limit "moves forward", as it were – Dawkins has referred to this as 'the ratchet' (Dawkins, 1997, 1017). Similarly we might say that the minimum standard for historiography 'ratchets forward', as it were. To give an example from the historiography of the Soviet Union: classics like Robert Conquest's *The Great Terror* had to glean what they could from fragmentary sources. Now however, the (albeit partial) opening of the Soviet archives have rendered much of this extant literature obsolete, however ingenious the guesswork it employed (Hobsbawm, 2000, 94). In other words, what was once a state of the art work may now not even count as 'proper' history, if it were written now.

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often notes where Keyne's previous biographer Roy Harrod got things wrong – and thus why both are an improvement over their predecessors.

[88] And as A.J.P Taylor once wrote, the mark of greatness is that it can survive changing standards. Shakespeare was great to his contemporaries; great to Coleridge; great to us.

[89] In *Narrative Logic*, Ankersmit makes a similar pint, but in an idealist way. Ankersmit adverts to Poincare's remark that "there is no time over and above the various clocks we have. We can compare one clock to another clock; but we cannot compare any clock to time and it makes therefore no sense to ask which of the many clocks we have is correct. The same is true of….historical narratives" (Ankersmit, 1983, 81-82).

[90] This is not to diminish Macaulay's greatness however.

[91] The Red Queen is of course, from *Alice Through the Looking Glass* – "Now, here, you see, it takes all the running you can do to keep in the same place."

There is also something to be said for the view put forward by Collingwood to the effect that historical knowledge "can only grow" out of historical knowledge, in the sense that the "more historical knowledge we have, the more we can learn from any given piece of evidence; if we had none, we could learn nothing" (Collingwood, 1946/1994, 247). This also partly explains why history is 'argument without end'; a historian who visits a piece of evidence in sat the year 2000, can potentially get more out of it than someone who viewed it in 1900, on the basis that the former has a hundred years more historical knowledge available to him/her than the latter did. A realist about historiographical practice can accept the idea that historiography is 'argument without end' without entailing scepticism; that we are simply spinning our wheels as far as the writing of history goes.

Finally, one of the benefits that an institutional framework brings is that it increases the likelihood of yielding increased historical understanding; the institutional framework is a sufficient, but not necessary condition. Recall that in chapter two I argued that the two-stage process roughly corresponded to a knowledge-understanding divide, although there was mutual reciprocation between the two. One of the things that the institutional model does is make increased understanding more likely. It does not guarantee it: it is perhaps arguable that no-one has achieved a greater understanding of the Roman Empire than Gibbon, who of course falls outside the institutional era. A historian like Gibbon comes along once in a blue moon however; the rest of us need some help, and the institutional setting provides this. Progress can be seen as the 'ratcheting' increase of historiographical standards, and this generally increases understanding; it is important to note that it does not *guarantee* increased understanding, but it does make it more likely.

(iv)   Specialisation and Complexity

In this section I want to examine one of the key mechanisms by which historiographical standards are bought about and maintained: that of specialisation.

This specialisation can be seen as analogous to that of speciation in biology. To turn once again to the work of Thomas Kuhn: Kuhn wanted to get away from the notion of science as

'aiming at truth': but he did not want to give up on the idea of scientific progress.[92] In the Postscript, he famously stated that he could see "no coherent direction of ontological development" in the succession of physical theories from Aristotle to Einstein (though he did hold that there was an *epistemological* improvement.) (Kuhn, 1962, 206). Kuhn argued this picture should be replaced by one of "a process of evolution *from* primitive beginnings – a process whose successive stages are characterised by an increasingly detailed and refined *understanding* of nature" (Ibid -my italics).

This increasingly detailed picture has been largely achieved by the increasing specialisation of science; of sub-disciplines breaking off from the parent branches. This proliferation of specialities is bemoaned by some on the grounds that the fragmentation of scientific study is/was a bad thing.[93] Kuhn though, felt that "specialization and the narrowing of the range of expertise now look…like the necessary price of increasingly powerful cognitive tools" (Kuhn, 2000, 98). With regards to complaints that it affected the 'unity of science'. Kuhn replied that "such unity may be in principle an unattainable goal, and its energetic pursuit might well place the growth of knowledge at risk" (Ibid). Accompanying specialisation is – to use another biological analogy – niche formation. As Hull sketches out the concept in his reconstruction of the process of science, when several scientists collaborate, they form an audience for their own publications: thus, the "demic structure of science provides conceptual niches for the development of new ideas" (Hull, 1988, 395).[94]

It is clear from viewing the history of historiography that a similar and recurring process of specialisation has taken place. By the end of the nineteenth century, the genres of social and economic history had established themselves alongside the traditional political histories of events/institutions that had previously formed the subject of historical works. In the interwar period, the *Annales* historians pioneered - among other things - demographic studies and the

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[92] "Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievements the extent to which it brings us closer to that ultimate goal?" (Kuhn, 1962, 171).

[93] As Wray puts it, "philosophers are inclined to think of the creation of new specialties, and especially the resulting communication barriers they create, as a temporary state on our way to developing a unified science" (Wray, 2012, 126).

[94] To the best of my knowledge, the origination of the term 'deme' is to be found in Gilmour & Bishop-Harrison, who define a deme as a term "denoting any group of individuals of a specified taxon." (Gilmour & Bishop-Harrison, 1954, 152). Hull gives a fuller definition of the term: "Ideally a deme consists of organisms in sufficient proximity to each other that they all have equal probability of mating with each other and producing offspring, provided they are sexually mature, of the opposite sex, and equivalent with respect to sexual selection. To the extent that these conditions are met, the organisms belonging to a deme share in the same gene pool" (Hull, 1988, 433).

history of 'mentalities'. Nearly two decades into the twenty-first century, we have, among other things, feminist history, environmental history[95], and even the history of smells as sub-disciplines of historiography.

We also have niche formation: historians specialising in certain areas can be said to occupy a niche with others who share this specialisation. Here, 'sheltered', as it were, from other areas of historiography, they can pursue this speciality in greater detail than would otherwise be possible. For instance, with the development of specialist journals in historical theory, those who write on the subject do not have to fight (figuratively) to get their articles published in general historical journals; the chances of being published in the latter being fairly slim.

However, we need to be a bit clearer as to what we mean by 'specialisation' here, for there are different kinds of specialisation. The first is what we might call, to use Le Roy Ladurie's phrase, a move towards truffle hunting at the expense of parachuting. To use British historiography as an example again: the professionalization of historiography led to the increased production of "specialised studies of limited topics…highly professional in terms of their technical competence in handling sources, but part of no general thesis or interpretation of history" (Parker, 1989, 88). As has been the case in science, historiography has progressed by coming to know more and more about less and less.

The second is what we might call 'skill specialisation.' The historian who studies the fourteenth century, for example, needs special skills in deciphering the printing and handwriting of the archival sources, as well as a good working knowledge of languages like Latin. These are skill-sets that are not needed by say, twentieth century political historians, nor do said skills transfer across.

The third kind of specialisation is arguably the kind that is the motor of increased understanding (although this is not to say that the second kind does not play a part – I am minded here of Geoffrey Eton's comment on Mommsen.)[96] That is, when historians ask *different* questions about the *same* period. The early definition of social history of course, that

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[95] Or at least, the interaction of humans with the environment: the point being this development does not contradict my assertion that the historical past is concerned with humans.

[96] Elton responded to Carr's remark that Mommsen's greatness rested on his History of Rome rather than his corpus of inscriptions by remarking that such a complaint showed "no sign that he [Carr] grasps what Mommsen achieved when he laid the solid foundations of Latin epigraphy…the sheer leaning and expository skill displayed in these works exemplify historical insight and achievement of the highest order" (Elton, 1967, 12-13).

it was "history with the politics left out", as inspired by the third chapter of Macaulay's *History of England*. In these sorts of specialities, we can legitimately say that historians are looking for different facts in the same area: a political history of England in the nineteenth century will largely be composed of political facts and an economic history will be composed of economic facts (though there will be some overlap). Some specialisations are more motivated by values than others: feminist history is the obvious example; Marxist history would be another.

Is it the case that all of the findings of these different sub-specialities are ultimately commensurable? In science, Kuhn thought that speciation helped to account for his later, weaker notion of incommensurability. Others though – particularly in physics – tend to view specialisms as looking as small parts of what is ultimately one big picture. In history, there has been much more scepticism about this sort of thing, with many historical theorists following Louis Mink in rejecting the idea of 'Universal History.'[97] On the other hand, Frank Ankersmit has recently argued that perhaps historiography can be seen as a cumulative enterprise in this sense. My own view is one of cautious optimism. Not all of the pieces – i.e. historical works – will fit together but a good many will. We can see this by the fact that political historians can lean on the findings of economic historians, economic on social, and so on. In particular, many journal articles are synthesised or incorporated into larger book length works.[98] Furthermore, more and more seminars are becoming interdisciplinary vis-à-vis the second sort of specialisation.

How does specialisation aid in the development of historical understanding? It is clear from a look at the historical record that historiography in general has become more complex over time, in terms of things like the explanations that historians make getting more complex over time. One can give numerous examples of this. If we look at accounts of the origins of the Second World War, we have come a long way from the 'Hitler was a bad man aided by cowardly appeasers' accounts that were first proffered to explain the war's origins.[99] Or take Cold War historiography; initial accounts of the conflict felt that the answers the questions about the conflict were to be found solely "in the government documents, and foreign

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[97] I will come back to this in Chapter 8.
[98] Moreover, we can look to the fact that many historians are participating in interdisciplinary activities.
[99] See for instance, Wheeler-Bennett, 1948; also Namier, 1949.

ministry files in particular" (Hopkins, 2007, 914). Few would accept such an approach nowadays however.

This raises a couple of questions, most namely: is this increasingly complexity of subject matter a good thing - as if it must be if we are to argue that this constitutes progress. Secondly, can we account for *why* this increase in the complexity of historiography has taken place?

With regards to the second question, there has been a general realisation within historiography that the past itself is a complex phenomenon that thus requires complex explanation. For instance, the historiography of the origins of the First World War has moved away from the idea that there is a *singular* cause of the war to be found, or that blame can be entirely apportioned to one single country. Or to return to the example of Cold War studies: this area has moved on from the idea that we can explain the war solely on the basis of what statesmen or diplomats said to each other as being able to provide a satisfactory explanation. There has been a realisation then, that *the past itself* is in fact a lot more complex than was once thought.

Furthermore the idea that simplicity is supposed to be a virtue in the sciences is somewhat of myth. As Bunge notes

> Since the 17th century scientists have not striven for epistemological simplicity but on the contrary, have been inventing more and more transcendent (transempirical) concepts, more and more theoretical entities, and with the sole restrictions that they be part of theories, scrutable (not necessarily observable) and fertile…In short, *the progress of science is accompanied by epistemological complication* (Bunge, 1963, 74).

This is not to say however, that science strives for epistemological profligacy either: a decreasing degree of epistemological simplicity is forced on us by, among other things, "the complexity of reality" (Ibid).

Turning to the first question – how does increasing complexity signal a proxy measure of progress – here we come back to the idea of increasing standards. As the quality of historical accounts rises, the bar is set higher in terms of what is acceptable in a historical account. For instance, Alan Bullock's 1952 biography of Hitler omits virtually any mention of the

Holocaust.[100] Given the state of the study of the Holocaust at the time, Bullock could be forgiven for this odmission. A biography that did this in the 21st century however, would almost certainly be dismissed as almost fatally flawed; as we now believe that in order to understand this key part of Hitler's life, one needs to understand the mass killings he set in motion. Furthermore, it is not merely enough to say something like 'the Holocaust was a result of Hitler's loathing of the Jews.' In a sense of course, it undoubtedly was; but an explanation of how and why the Holocaust was able to occur now needs to encompass aspects such as bureaucracy in Nazi German, the attitudes of the German people towards Jews, and so on.

 The historiographical process then, does not *aim at* complexity for it's own sake; nonetheless, this increasing complexity is integral to historiographical progress. The more we discover about the past, the truth about that past becomes more complicated, hence the need for specialities.[101] Again, there are parallels here with scientific progress; "a complication of initially simple assumptions may well be available in the face of an increasing incompatibility with a body of empirical evidence" (Bunge, 1963, 75).

(v)   Truth & Progress

As we have already noted, most historians reject the idea of 'ultimate history' – that we will ever reach a point where we have a final and unrevisable account of any given area of the historical past. The obvious move for the realist to make here would be to would be to appeal to the notion of verisimilitude or truth-likeness; the realists could still reject the idea that we can ever achieve 'ultimate history' – i.e. an absolutely true account of any given subject or set of events in the past – but could argue that historical accounts can be said to approach an absolute truth, even if we know we will never actually reach it. The problem I have with such an account is that, for reasons I will now explain, it seems to me ultimately incoherent.

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[100] To be precise, the book devoted two paragraphs to the subject (Fulbrook, 2004, 115).
[101] As Lytton Strachey famously wrote in the preface to *Eminent Victorians*, "the history of the Victorian Age will never be written: we know too much about it. For ignorance is the first requisite of the historian – ignorance, which simplifies and clarifies, which selects and omits." Although he was understandably pilloried by historians for this remark, as Carr noted, there is more than a grain of truth to it.

The modern notion of truthlikeness – or verisimilitude as it often termed in the literature – was coined by Karl Popper. Popper's notion of scientific method of course, entails that although we can never conclusively verify a scientific theory, we can nevertheless falsify it. This seems somewhat thin gruel: on the Popperian picture "the best we can do is to show theories are false and discard them, and even if we did have some truth this could never be known, then things look pretty gloomy" (Oddie, 1986,23). On a hardcore falsificationist view, a scientific inquiry "could never contribute anything *positive*" (Ibid).

Popper's move to try and brighten up his picture then, was to argue that although we may only assert negative statements/propositions about reality, nonetheless by the elimination of false conjectures, we still come closer to the truth about the world, on the basis that some conjectures and theories are more truthlike. Thus, although we may have theories that strictly speaking are false, one may nonetheless contain more true statements or axioms than the other. We are inclined to say that Newton's physical theory was an improvement on Aristotle's, and that in turn relativity theory and quantum mechanics are an improvement on Newtonianism. (Indeed, Newton's theory can still be used for middle-sized dry goods, even though strictly-speaking it is 'false.') Successive theories can thus be said to be better approximations to the truth, and thus the rationality of science is preserved.

Oddie sums up neatly the role that truthlikeless is supposed to play in the philosophy of science:

> The realist takes the aim of an inquiry to be the truth of some matter, but reluctantly concedes that many inquiries fall short of actually achieving this aim. What is worse, in many inquiries, it is extremely unlikely that the aim will ever be achieved. Nevertheless, the realist wants to claim that even if the aim is not achieved, progress towards it is possible: one theory may be closer to the truth (or more truthlike) than another, even if *both* fall short of the whole truth (Oddie, 1986b, 243).

The issue of truthlikeness in science has been debated for the better part of the past fifty years or so now; with Popper's original formulation being found wanting in some respects and alternative proposals being put forward. We need not review the vagaries of the debate here. The question we must ask is: is there something like a viable version of truthlikeness that the historian could avail him/herself of?

Reading through the debates on verisimilitude in science, it is clear that we cannot simply boilerplate the scientific conception onto historiography. In order to make a rigorous comparison of two scientific theories, "it is necessary to be able to be able to present the information conveyed by a theory in standard form" – that form usually mathematics. (Oddie, 1981, 243). This is something we do not need to do in order to compare historiographical works; indeed, it is questionable if a work of history could be paraphrased into first order logic; and even if it could, it would be hard to see what this would exactly achieve.

One of the main problems with the concept of truthlikeness can be traced back to an objection made by (of all people) St Augustine's objection: we cannot reasonably know if the son resembles the father unless we know what the father looks like. Similarly, how we supposed to judge how closely a thesis resembles the 'absolute truth' if we have no idea what the latter looks like? This is a point Rorty has made in his own arguments about the problem of science having truth as a 'goal' – generally speaking, in order to achieve a goal, we need to know what it is we are aiming for. But if we don't know where the journey ends, then how are we to judge if we have made progress in reaching our destination? (Rorty, 1995, 298).[102]

Another issue with using the notion of truthlikeness for historiography is that it is not clear that this concept has any traction with regard to being able to evaluate two competing historical accounts. One of the things we established early on in this thesis is that historical accounts which put forward competing views of the same period may nonetheless utilise many of the same facts. Thus, counting the number of true statements that each account contains is not likely to do us much good. Indeed, including mostly true statements is arguably a necessary, but not sufficient condition of producing a good historical account - many of the historical accounts of the late nineteenth century – the 'dryasdust' efforts, as they came to be known – were bursting with historical facts, and yet due to the lack of understanding they provided, are now seen as extremely deficient in terms of what we want from a work of history.[103]

The mention of Rorty a moment ago is apposite, for there is much in the view of progress that I have put forward that has much in common with the pragmatist view of scientific progress (though we need not weld it to a pragmatist account of truth.) The account I put forward

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[102] One suspects that Kuhn would also concur with this conclusion.
[103] This is one of the things that arguably sets historiography apart from science: recall the Taylor remark made earlier – it is hard to imagine such a situation in science. In science, the worry is always that we never have *enough* facts.

however, is to be distinguished from the recent attempt to utilise pragmatism by Jouni Kukkannen. Kuukkanen's appeal to the notion of warranted assertibility is largely motivated by his rejection of colligatory realism, as we saw in Chapter 5: that historical accounts cannot have truth-makers because they are not structurally-similar or isomorphic with the past itself (Kuukkanen, 2015, 142). (And this move of course, is ironically *unwarranted*).

However, a correspondence theory of truth is arguably not appropriate for historical accounts not because they have no truth-makers; but rather, because they aim ultimately at *understanding* about the past. One of the central themes of this thesis has been a commitment to a two-stage model of historical practice. The first stage, as we saw, concerned the establishment of historical knowledge, typified by the establishment of significant fact. And knowledge, it is generally accepted, involves truth (Alston, 1997, 248). But knowledge alone cannot provide understanding. That said, knowledge is a necessary condition for understanding, but not a sufficient one. And – without getting bogged down in a detailed definition of understanding – it is clear that understanding largely requires coherence (Riggs, 2007, 218). But coherence alone is not enough: "a coherent body of predominantly false and unfounded beliefs does not constitute an understanding of the phenomena that they purportedly bear on" (Elgin, 2007, 35). There is a requirement then, that understanding be *factive* in some sense.

The upshot of all this then, is that truth is inherently involved in understanding, and progress is achieved via increased understanding of the past. Thus, truth plays an integral role: but we don't need to explain progress in terms of truth: the two are ultimately independent of each other. We are also beginning to see how the two-stages that encompass the production of historical knowledge are inter-linked. Understanding clearly requires the establishment of significant fact; hence the first stage described in Chapters 2-3. In turn, increased understanding gives us an idea of where to look for facts. Indeed, this helps to account for why historiography continues even if optimal understanding were to be achieved. Suppose for instance, it was felt that Gibbon's account of the fall of the Roman Empire showed a depth of understanding that was unlikely to be surpassed. Nonetheless, such understanding would still need to be factually supported. Furthermore, any subsequent factual discoveries would need to be incorporated into such an account.

I argue then, that an account of historiographical progress needs to be developed that is sensitive to the nuances of historiographical practice. One of the numerous faults of with anti-realist accounts of historical theory is that they seem to believe that most historians still adhere to some sort of theory of ultimate history. If this were indeed the case, then the point would be well made – the idea of a view of progress in historiography akin to the 'book of nature' view in science is problematic at best. But historians do not cling to a view of 'ultimate history', though as we have seen, they still adhere to the notion of historiographical progress.

Conclusion

In this chapter, I have laid out a theory of historical progress and the mechanisms by which it is achieved. To begin with, I outlined how historiography became professionalised, as this development was key to the institution of standards. I then showed how progress emerged from these professional standards, before focussing on a key aspect of the progressive mechanism in historiography, that of specialisation. I concluded by outlining why I rejected the notion of 'truthlikness' as a model of historical progress, as such a model seemed the obvious route for a realist about historiography to take.

7) Holism & Incommensurability

In this chapter I will look at some of the issues that holism poses for historiography. It should be stated at the outset that, as we have seen in other areas, the term 'holism' in historiography tends to be used in something of a non-standard way. Philosophers who normally deal with holism in terms of things like semantics and beliefs might be slightly surprised to hear historical theorists talking about holism in terms of the 'indecomposability of texts' and so forth. So as with our chapter on reference, I will begin by quickly dealing with the historiographical use – or perhaps that should be misuse – of the term, before moving onto a more substantive worry about holism and historiographical realism – that of the type of global incommensurability postulated by the early Kuhn. I argue that while global incommensurability fails to take hold, a more local form of incommensurability is present in historical writing; but that this local form is of no threat to historiographical realism.

The chapter will proceed as follows. First I will recap the issues surrounding holism in historiographical theory, before arguing why these issues are really non-issues. I will then outline a more realistic threat to historiographic realism, couched in terms of a global incommensurability between historical works, before going onto to argue that if this were the case, the rational debate which historians clearly engage in would not be possible. As historians clearly do engage in said rational debates, global incommensurability is clearly not an issue.

(i)   Historiographical Holism

The three main variants of historiographical holism can be summarised as follows: Ankersmit's 'representation' view, Mink's 'non-detachability' theory, and Kuukkannen's isomorphism argument.

1) Ankersmit's Representationlism

When historical theorists' think of holism, the conception that they tend to have revolves around the idea of the indecomposability of historical texts. This might been seen as akin to what is known as 'sematic indecomposability' in the philosophy of language.[104] Exactly how this form of holism came to be seen as relevant for historiography will require a quick detour into the history of the philosophy of history. The 1950s and 60s in historical theory were dominated by what has come to be known as the 'covering law' debate. This was inspired by a paper of Carl Hempel's to the effect that historiography, if it wanted to be taken seriously as a science, needed to make use of deductive-nomological laws in its explanations in the same way that the physical sciences did (Hempel, 1942). Somewhat predictably, historians did not take too kindly to this. One of the reactions against this – combined with a general 'linguistic turn' in historical theory – was an increased examination of the role of historical narrative. The result was that from the early 1970s onwards historical theory developed a "philosophical fascination with whole linguistic texts" (Gorman, 1997, 56).

As a result of this interest in narrative, the idea quickly gained traction that the historical text was something qualitatively dissimilar from the elements that it was composed from; historical accounts were not simply compilations of facts, but are greater than the sum of their parts. Thus, those arguing against the covering-law model of explanation made the point that although historical accounts might contain covering law explanations as *parts* of a historiographic text, this did not account for the "nature of *the historiographic text as a whole*" (Ankersmit, 2009, 199).[105] The explanation given in a narrative whole is not simply the sum total of all the explanations of individual events given in it; it is something over and above these, and thus the "cognitive message" of a given historiographical account is to be found in "the text *as a whole* rather than in *its constituent parts*" (Ibid).

Ankersmit has provided his own particular spin on the narrativist turn, with his argument that historical texts should be seen as *representations*.[106] On Ankersmit's view, singular facts are lower-order entities, while narratives are higher-order ones. Thus, "theories of representation

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[104] This is the idea that "there is some whole with semantic significance that has the priority of the semantics of the parts" (Pagin, 2008, 220)

[105] Italics in original.

[106] Though as Tom Stoneham has pointed out to me, Ankersmit is using 'representation' in a non-standard way – what Ankersmit means by 'representation' is probably closer to 'depiction.'

are, essentially, theories about how the whole of a historical text is related to the past that it is a representation of – and this is a problem that cannot be reduced to how a historical text's individual statements relate to the past" (Ankersmit, 2005, xiv). For Ankersmit, historiographical representations are akin to paintings, in that among other things, we cannot distinguish between a subject and predicate in a painting like we can in a sentence. In a sentence, "the subject-term of the statement identifies and refers to a thing in reality whereas its predicate term attributes a certain property to it" (Ibid). This cannot be done in a representation though; "if we look at a painting or a photograph we cannot distinguish between components that refer and those that attribute" (Ibid). Ankersmit also uses the analogy that reading a work of history is like viewing a portrait: "we do not experience it as a composite of bits of information…but rather as a representational whole" (Ankersmit, 2012, 98). Moreover, in a historical work "none of the statements which constitute the text is…irrelevant to the text's presentation of the past" (Ankersmit, 1995, 225). Thus, to change one sentence in a work composed of maybe a million sentences is to affect the work as a whole.

(2) Mink and Narrative Understanding.

A slightly different form of historiographical holism is put forward in the work of Louis Mink. In several of his essays, particularly in "Narrative as Cognitive Instrument", Mink has argued for holism in terms of what he calls the 'non-detachability' of the conclusions in historiographical accounts from the facts/evidence that support them:

> Historians generally do not adopt one another's significant conclusions unless convinced by their own thorough inspection of the argument; unlike scientists in general, they *must read one another's books* instead of merely noting their results (Mink, 1987, 77).

How does this entail holism? Mink argues that the reasons that detachable conclusions are available in science is because scientific concepts have a "uniformity of meaning", whereas historiographical conclusions can never be detachable because "not merely their validity but their meaning refers backwards to the ordering of evidence in the total argument" (Ibid, 79).

The conclusions that a work of history make are ingredient in the argument itself, "not just in the sense that they are scattered throughout the text, but in the sense that they are *represented* by the narrative itself. As ingredient conclusions they are exhibited rather than generated" (Ibid). A historical narrative explanation cannot be "said", but must be "shown" (Ibid, 198).

Once again, we have a notion of the indecomposability of historical texts. Although there are differences between Mink and Ankersmit's views – Ankersmit leans heavily on his construal of representation while Mink focuses on the cognitive function of narrative – both argue for the idea that historical accounts cannot be 'decomposed' in any way: although individual statements may be used in their construction, the composition of the historical account *synthesises* these statements into an indissoluble structure. As Mink writes, a narrative cannot, "in especially successful cases…be divided into self-contained or relatively independent parts" (Ibid, 172). Historical accounts have to be 'swallowed whole', as it were.

Mink's idea of holism is tied to the notion that historiography employs a specific mode of comprehension. That is, that historical understanding is the kind of understanding that "consists in thinking together as a single act, or in a cumulative series of acts, the complicated relationships of the parts which can only be experienced *seriatim*" (Ibid, 150). The cognitive function of a narrative is not simply that it organises the historian's research; rather, it is "the narrative history itself which claims to be a contribution to knowledge" (Ibid, 168).

(3) Kuukkanen – The Isomorphism Argument

A third anti-realist argument vis-a-vis historiographical holism is the idea that historical accounts – as wholes - are morphologically dissimilar from the past they purport to represent, and thus cannot be said to reflect the past as it was due to this structural dissimilarity. In *Narrative Logic*, Ankersmit argued that given the morphological or structural difference between the past and the historical narrative, how could any kind of translation rules "be expected to link them together? Projection or translation rules can exist only where there are two corresponding spheres of structural similarity" (Ankersmit, 1983, 82). The most recent espousal of this position however, is to be found in Kuukkanen's *Postnarrativist Philosophy of Historiography*. There Kuukkanen writes

there is a *morphological* or *structural* difference between the historian's presentation and historical reality, which explains why any idea of copying or matching between the two is fundamentally misconceived. One simply cannot make two structurally totally different entities *correspond* with each other. Elephants cannot be made to correspond with butterflies due to obvious structural differences. The historian's narrative is verbal and textual, while historical reality is non-narrative and non-verbal in nature (Kuukkanen, 2015, 42).

Kuukkanen a short while later states that it would "be a category mistake to suggest that arguments [i.e. the arguments about the past put forth in historical accounts] correspond to anything in historical reality; the past cannot be thought of as being structured like an argument with premises, conclusions and their relations" (Ibid, 67). Thus, because historical accounts are formally dissimilar from the past they represent, a realist view of historiography fails to get off the ground.

(4) Rebuttals

Although these arguments all tend to be seen as credible within historical theory, in fact they are all arguably pseudo problems at best. To take Ankersmit first: if Ankersmit's view of historical texts were true, then historiographical realism would indeed be in trouble. Fortunately, however, it is not. To begin with, there are problems with Ankersmit's view of representation *qua* aesthetics, before we even begin to use the analogy for historiography. Is it really the case that we are unable to experience a portrait in any other way than as a representational whole? Surely one can, in viewing a portrait, choose to focus in on a single aspect or area of the portrait if one wishes – it is not a case that the viewer must experience it as a whole or not at all. Furthermore, some of the recent writing on depiction seems to contradict Ankersmit's (largely unsubstantiated) assertion that picture do not or cannot refer.[107] Conversely, there are things which historical narratives achieve which depictions

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[107] As the editors of a recent collection on depiction put it, "although there are many interesting issues concerning the aesthetics of pictures at least as many non-aesthetic issues fall with its domain [i.e. philosophy of depiction]...the philosophy of depiction draws on research in other areas of philosophy, most notably the philosophy of mind and language" (Abell & Batinaki, 2010, 1-2). Thus, even if for the sake of argument

clearly cannot: for instance, historical narratives contain referential terms which 'pin' them to real objects and events in a way that is not achieved in pictures.

Turning to Mink's brand of holism, three points need to be made here. The first is that the idea that we need to swallow a narrative 'whole', as it were, in order to fully understand the events it sets out seems very problematic. It seems to posit historical understanding as an absolute concept; we either possess understanding of a historical text or we don't, and the only way we can acquire such understanding is to read the whole text. This seems to imply that, if we get three-quarters of the way through a work of history and then break off from it, we will possess no understanding of the work in question. And this seems wrong. Understanding is available in degrees, as anyone familiar with the literature on concept possession will be aware. Furthermore, here we arguably have a difference between narrative in fiction and narrative in history; if we were to leave off reading say, Dickens' Bleak House ¾ of the way through the book, there is an important sense in which it could be said that understanding of that work could be denied to us. But we could get halfway through reading, say, William Shirer's *Rise and Fall of the Third Reich*, and be happy that we have understood what we have read, even though we have not traversed the book to its conclusion.

Secondly, is it really the case that a narrative cannot be dissolved into relatively independent parts? I have in front of me a copy of P.M.H. Bell's *Origins of the Second World War*. If I were to distribute Chapter 8, dealing with the Munich crisis, to a university class as part of a seminar on Chamberlain's role in the run-up up to WW2. According to Mink the group would not be able to understand or draw anything from the book: as one cannot understand the part taken away from the whole. Again (and fortunately for pedagogy), this just seems wrong. One can agree with certain parts of a historical account while disagreeing with the overall thesis it puts forward (if it does put one forward) – Taylor's *Origins* is a classic example of this.

Finally, is it really the case that when scientists look at journal articles and/or write-ups of experiments, they simply look at the results, as Mink would have it, and don't bother with the methodology of experiments whatsoever? I am not sure too many scientists would agree with this. To give just one counter-example; one of the reasons that the fabricated results of Jan Hendrik Schön's experiments in microelectronics came to light was because rather than

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historical accounts are depictions, it does not follow – as Ankersmit claims – this does not mean that historical accounts fall outside the domain of epistemology. (Ankersmit, 2001, 12).

simply noting the results (as Mink claims they do), scientists were interested in the methods by which he had achieved them.[108]

More generally, both Ankersmit and Mink appeal to the notion that historical accounts are something over and above the sum of their parts; but little attempt is made to substantiate this claim. As Gorman puts it, it is fairly self-evident that there is a difference between a statement like 'Hitler's Germany invaded Poland in 1939' and a whole book on the Second World War; and furthermore "to note that the epistemological acceptability of the one is independent of the epistemological acceptability of the other" (Gorman, 1997, 408). But beyond this, it is not entirely clear *how* exactly a text has an autonomy relative to its constituent parts. This is not to say that the whole of something cannot be more than the sum of its parts: only that Ankersmit and Mink have not shown us how this is the case in historiography.[109]

Turning finally to Kuukkanen's isomorphism: if the only criterion we had on the table for truth was that of correspondence-as-congruence, or what tends to be known these days as an identity view of truth, then historiography would indeed be struggling. Fortunately, this is not the case. Branko Mitrovic has admirably dealt with Kuukkanen's argument in his review of *Postnarrativist Philosophy of Historiography*: it is hard to see "why one should expect *any* kind of morphological or structural similarity between a set of propositions expressed in a text on the one hand, and the events, objects or situations in the world that make it true on the other" (Mitrovic, 2016, 3-4).[110] Moreover, Kuukkanen seems to be of the same mind as Ankersmit in feeling that the isomorphism argument does not affect singular sentences in the way that it does historical texts. But it is hard to see what separates the two: why is it that a sentence about an elephant need not resemble an elephant; but presumably a book about elephants does need to resemble an elephant if it is to accurately reflect what happened in the past? One cannot have it both ways.

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[108] See Reich, 2009.

[109] Moreover, as David Armstrong has written in relation to another topic, we can have unities that do not unify. (Armstrong, 408 1997).

[110] This is a point also made by Peter Kosso; when we read an account of the past by say, Herodotus, the words of Herodotus….do not physically resemble the objects and events of interest. Nonetheless, the information in the text is evidence of the past in the sense that an information-bearing signal has been conveyed through a series of interactions, beginning with the events themselves, through Herodotus and his informers, and eventually to us" (Kosso, 1992, 32).

Given some of the counterintuitive consequences of historiographical holism, philosophers with little experience of historical theory mat well wonder how such notions ever got off the ground in the first place. This is something that we can trace back to the aforementioned linguistic turn. It would only be a slight exaggeration to say that during this period the past dropped out of history, to be replaced by 'texts', as literary theorists assailed the discipline of history in the 1980s and 90s (I will return to this in the conclusion). But while these variants of historiographical holism may be easily dealt with, there are more substantive worries about holism for historiography, to which we may now turn.

(ii)   More Substantial Challenges

As we have seen in the previous section, the arguments for holism that are to be found in the existing literature on historical theory can be dealt with fairly easily. There are however, two more substantial arguments which have not been considered, but are potentially more formidable than those outlined above. In this section and the next I want to outline these arguments, before showing that they too can be faced down. The first revolves around the kind of global holism that Thomas Kuhn implicitly put forward as part of his earlier notion of incommensurability. While Kuhn was later to disown this earlier conception of incommensurability in favour of a much more localised version, it could be argued that while incommensurability holds no threat for science, it could well be a problem for historiography. The second argue centres on what we might call a standard reading of holism (as opposed to the non-standard view of the term that is generally used in historical theory), and centres around holism as relates to semantics and meaning.

(1) Kuhnian Incommensurability

In *The Structure of Scientific Revolutions*, Kuhn argued that "the differences between successive paradigms are both necessary and irreconcilable" (Kuhn, 162, 103). It was not just that successive paradigms told us different things about the universe; for paradigms "are directed not only to nature but also back upon the science that produced them" (Ibid). They also comprise the methods and standards of solution accepted by the scientific community at a given time. Thus, when we have a paradigm shift, there is necessarily a methodological

shift that accompanies it. Kuhn gives several examples of this, one being that of the chemical revolution: as a result of it, chemists "set up previously unimagined experiments and searched for new sorts of reactions", experiments that would have been unthinkable without these new concepts being in place (Ibid, 106). Thus, "changes in the standards governing permissible problems, concepts, and explanations can transform a science." (Ibid). Call this aspect of Kuhnian thought methodological incommensurability.

A second aspect of incommensurability is that of *meaning* incommensurability. The example that Kuhn gives in Structure is that of Newtonian and Einsteinian physics, where he asks the question, "can Newtonian dynamics really be *derived* from relativistic mechanics" as is commonly supposed? (Ibid, 101). The answer is no, because even though both theories employ concepts like 'space', 'time' and 'mass', the "physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name" (Ibid, 102). When a paradigm shift takes place, "old terms, concepts, and experiments fall into new relationships one with the other…To make the transition to Einstein's universe, the whole conceptual web whose strands are space, time, matter, force, and so on, had to be shifted and laid down again on nature whole" (Ibid, 149).

The third aspect of incommensurability has generally been characterised as 'world change', based on Kuhn's claim that when the historian of science looks at the record of past science, he or she "may be tempted to exclaim that when paradigms change, the world itself changes with them" (Ibid, 111). What Kuhn precisely meant by 'the world itself changes' is not entirely clear.[111] Indeed, Kuhn himself was unable to precisely put his finger on it: "In a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds" (Ibid, 150). In one world, space is flat; in another, it is embedded in a curved matrix. In one world, solutions are compounds; in another, they are mixtures. And so on.

The above is a rough outline of Kuhn's early construal of incommensurability. As a result of the conjuction of these three strands, we have global incommensurability; and the result is that the proponents of "competing paradigms are always at least slightly at cross-

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[111] Paul Hoyningen-Huene has made the most comprehensive attempt to try and clarify Kuhn's position; however, the problem is that it relies on Kuhn's post-*Structure* identification with Kant – a move arguably made in order to gain favour with philosophers. See Bird, 2003.

purposes…[and thus] The competition between paradigms is not the sort of battle that can be resolved by proofs" (Ibid, 148). Incommensurable theories seem to be incomparable in terms of content. Rather, the choice between competing paradigms is one that for the individual scientist, must be taken on something akin to a leap of faith.[112] As one commentator has put it, the worry about incommensurability then, is that "it suggests a view of science on which communication failure is rampant, theory choice utterly irrational, and scientific progress a myth" (Sankey, 1994, 3).

In his taxonomy of incommensurability, Simmons characterises global incommensurability as concerning a scientific thought-style understood as a total conceptual system or network of concepts: "global incommensurability theses concern rival thought-styles viewed as "global" wholes, in contrast to the multifarious "local" elements embedded within the thought-styles" (Simmons, 1994, 120). Because there is no substantive conceptual overlap between globally incommensurable thought-styles, there are "no substantive standards of evaluation by appeal to which their [the respective scientific communities] disagreements could be adjudicated definitively" (Ibid).

Such a construal might be a touch alarmist: even Kuhn made it clear in his later work he did not think that this was the case. But the issues involving incommensurability and holism in science tie into some of those I have briefly touched upon at earlier points in this thesis vis-à-vis what I have termed the constructivist view of historiography. If all theories within the same domain are incommensurable, then it is hard to see how any genuine conflict between them can take place. If we replace the word "theories" with "historical accounts" in the paragraph below, Sankey neatly encapsulates the point for us

> In the absence of genuine rivalry between a pair of theories, the rationale for seeking grounds to decide between them is obscure. But even were the attempt to make such a choice not misguided, it is unclear how the choice could be made on rational grounds. For if theories neither agree nor disagree, it is not as if one might be shown to be a better account of the same phenomena. There is simply no point at issue between

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[112] Scientists must "have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith" (Kuhn, 1962, 158).

them. Such theories cannot be compared by means of a detailed comparison of their consequences with respect to a shared body of evidence (Sankey, 1994, 3).

It is worth here reprinting a passage from Goldstein I initially quoted in Chapter 3; Goldstein argued that Taylor's book on the origins of the Second World War involved "historical facts which have no existence at all in the conception of his opponents: each side thinks the evidence calls for the constitution of different historical facts." How then, could Taylor's account be said to conflict with those of his predecessors if they were talking about *different things*? This type of holism undoubtedly poses a much more serious threat to historiographical realism.

(2) Meaning Holism

Perhaps one of the ironies in recent historical theory (of which there are many) is that while some anti-realist historical theorists have been quick to latch onto of some of the developments in post-positivist philosophy of science – specially the notion of 'theory-ladenness' – the idea of meaning holism, which seems to pose a legitimate problem for historiography, has been largely ignored – with perhaps one exception. In an early Paul Roth paper, develops the notion of explanations as paradigms; something akin to 'everyman his own paradigm.' In particular, he states that "there is no separating the analysis of explanation from attention to examples, to cases which are taken to instances of [qua Kuhn] exemplary problem solving" (Roth, 1989, 469). This ties in with Roth's later writing where he leans heavily on the idea that historians constitute the explanandum as well as the explanans.[113] Such a view mirrors Kuhn's idea that different paradigms postulate different entities and words have different meanings; and there is more than a whiff of this in Ankersmit's work too – recall our discussion in chapter 4 over the reference of 'the Renaissance.'

There is a legitimate worry about meaning holism in historical works if the semantic holism argument in general is correct. A fairly rough and ready definition of holism runs thus: words are things that only get their meaning within the context of a sentence; and by extension, sentences are things that get their meaning from their place in a language. Meaning holism

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[113] See in particular Roth, 2017.

then, is the notion that "only whole languages or whole theories, or whole belief systems, *really* have meanings, so that the meanings of smaller units….are merely derivative." (Lepore & Fodor, 1992, x). This is largely a result of developments in twentieth century philosophy of language: Quine, Davidson and Rorty (among others) have all argued in their own way that the semantic properties of a symbol are determined by its role in a language.[114] The upshot of all this is that in order to know the meaning of a word you need to know the meanings of all the other words in a language, then our general theory of how communication takes place appears to be in jeopardy. For such a picture generally relies upon the idea the "linguistic and theoretical commitments of speaker and hearer can overlap partially" to some degree (Ibid, 10). This would seem essential to the notion that language has an inter-personal, social existence. and holism poses a threat to this picture.

We can see then, how this general problem in the philosophy of language poses a problem for historiographical theory. As we have seen, the basic motivation behind holism is that no meaning is an island; that the meanings of a sentence in a system necessarily depends upon its relation to other meanings/sentences in said system. In order to understand the meaning of, for example, the sentence "For Bismarck only had to maintain an existing settlement; Stresemann had to work towards a new one", we need to know the meanings of all the other sentences in the book in question, in the same way that (on the holist view) in order to understand *f=ma* we need to understand the system of physics within which it is embedded (Taylor, 1961, 79). Where the historiographical version departs arguably is that in something like science seemingly holism operates at a disciplinary level; so that while the practitioners of a science can communicate having all accepted a certain theory, in contrast historiographical holism seems to be a linguistic variant of "every man his own historian", with each historical account sharing no kind of overlap with any others, where the meaning of a given term in a book is determined by the meanings of all the other terms in said work.[115] Thus, historical works seemingly concerned with the same subject matter become incommensurable.

We can use an analogy with an exemplar that Henry Jackman gives vis-à-vis holism in the sciences. On the holist view, the Greek view of astronomy - that stars are very nearby and that they ride around the heavens in crystalline spheres is "actually *not contested* by our view

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[114] Although in fairness to Davidson, he argued against significant inscrutability of meaning between languages.
[115] See Roth, 1989, where he seems to suggest that each historical account is a 'mini-paradigm', as it were.

that the stars are very far away and don't ride around the heavens at all" (Lepore & Fodor, 1992, 10). In fact, "strictly speaking, it would follow that the Greeks didn't *have* any views about *stars;* we can't, in the vocabulary of contemporary astronomy, say what, if anything, Greek astronomy was about" (Ibid). Take for example, a Marxist view of the English Civil War which would be incompatible with a non-Marxist account of the Civil War, as they are based in very different belief systems. Thus, they are not talking *about* the same set of events at all. On this view, it seems we are reduced to the Kuhnian strategy of having to 'learn different languages' in order to compare historical explanations. However, I believe we can avoid such a global incommensurability – ironically, by using some insights that Kuhn developed in his *later* works.

(iii)   Holism Defanged

As we have seen, the global meaning holist argues that the meaning of one word or sentence in effect infects all of the others within a system of words or sentences. In contrast, a molecularist about holism is happy to concede (or I am, in any case) that the meaning of some sentences is tied to the inferential role they play in a system, but not *all* sentences are affected. In historiography, this is cashed out thus: the view that one takes on a given state of affairs or event will entail certain views on other events and states of affairs in the relevant period or subject, but not on all of them. In historiography then, meaning is related to causal relations; on a global view of causation, everything effects everything else. The molecular view rejects this however; indeed, we might well say that what it is to write history is to determine the extent of the effect of an event – the extent of the relations that are going to effect the event. I will return to this momentarily.

As stated, the molecular view I put forward here is largely inspired by Thomas Kuhn's later writings on incommensurability. Kuhn's initial portrayal of incommensurability in *Structure* seemed to imply a global, wide-reaching holism which in turn entailed a relativist view of natural science. As was his wont in these situations, Kuhn claimed that he had in fact been misinterpreted, and in his later work on incommensurability in the 1980s, reformulated his views. The later Kuhn argued that in two successive scientific theories – i.e. in a domain where one theory succeeds the other, most of the terms

common to the two theories function the same way in both; their meanings, whatever those may be, are preserved; their translation is simply homophonic. Only for a small subgroup of (usually interdefined) terms and for sentences containing them do problems of translatability arise. The claim that two theories are incommensurable is more modest than many of its critics have supposed (Kuhn, 2000, 36).

Kuhn repeatedly argued that, for example, terms like 'force' and 'mass' must be acquired together. In learning the meaning and application of something like $f=ma$, one cannot learn 'mass' and 'force' independently "and then empirically discover that force equals mass times acceleration. Nor can one first learn 'mass' (or 'force') and then use it to define 'force' (or 'mass') with the aid of the second law. Instead, all three must be learned together, parts of a whole new (but not a wholly new) way of doing mechanics" (Kuhn, 2000, 44).[116]

This type of incommensurability has been termed by Simmons (among others) as "local" incommensurability. Local incommensurability theses "concern relationships between particular elements embedded within rival thought-styles, rather than the thought-styles themselves viewed as total systems" (Simmons, 1994, 122). On this view, the two thought-styles "have some substantive conceptual overlap and thus would not be globally incommensurable" (Ibid). Thus, the global holism that characterised Kuhn's earlier work has been replaced with a 'local' holism; which entails that "a thing (property, relation) is individuated by its relations to other things, properties or relations" (Peacocke, 1994, 243.) What it to be that thing, to have that property or to have that relation cannot be properly explained without bringing in other things/properties/relations. In order to understand a certain expression then, involves being simultaneously given "an account of what is involved in understanding (or at least possessing a concept of) the other" (Ibid, 244).

Let us see how this works in historiography. Take two works looking at the origins of the Second World War. Said works take different views on the significance of the Hossbach memorandum. One author states that this was clearly the enunciation of a long term plan to dominate Europe; the other argues that Hitler was merely letting off steam, and was essentially a short-term opportunist in matters of foreign policy.[117] Naturally, if one thinks of Hitler as a short-term opportunist in diplomacy, then one is forced to draw certain other

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[116] As Peter Hare points out, there is a certain irony in the fact that Newtonian physics seems to have inspired holism, given that Newton's theory is arguably a "prime example of atomistic, nonholistic thinking; classical physics defines a whole as the sum of its parts" (Hare, 2007, 52).
[117] The latter, of course, was AJP Taylor's view; which led Hugh Trevor-Roper to protest: "was Hitler really just a more violent Mr Micawber sitting in Berlin?"

conclusions with regards to the meaning of other events. Similarly, if one believes that Hitler had a long-term plan in mind, one will need to interpret certain events in a way which will maintain and maximise the coherence of the overall work/interpretation. However, both interpretations will still leave a significant amount of meanings untouched. We will be able to agree on many aspects of things like the Anschluss, the Munich Conference, the occupation of Bohemia-Moravia, and so on; although we may disagree on others.

What perhaps makes meanings in historiographical texts less susceptible to a global holism than other languages or theories is that meaning in historical accounts is largely (although not exclusively) concerned with the meaning of particular events; and events of course, stand in temporal relations with each other. And of course, historians are largely concerned with the *relations* between events. Due to the structure of historical accounts (and the past for that matter), the domain of eligible facts that are germane to a given event or set of events will be restricted.

To give an example of what I mean; say one is writing a book on European diplomacy between the wars. It is unlikely that say, that one's interpretation of, or the meanings one assigns to, something like the Rapallo conference of 1922 will impact upon something like one's view of the Rhineland Crisis of 1936. This is because the consequences of Rapallo will have exhausted themselves, as it were, and thus their impact on the later events will be virtually nil. Thus, two historians could quite easily hold differing views on Rapallo while agreeing over the Rhineland crisis; or vice-versa.[118] There are many events within a given period between which there are no causal, explanatory or inferential connections; they can in effect, be compartmentalised (Faye, 2014, 55).

Thus, in historical accounts, semantic meaning is determined by causal relations and the consequences of events; what historians are using language to do in historical accounts is to map out the consequences of events; thus, consequence relations determine the meanings of words, and the relations of words to other words in the text depends on these consequence relations. While the global holist thinks that the meanings of certain terms effects everything in the text, the molecularist, on the account I have given, can quite plausibly deny this: consequences of events become exhausted, thus what happens in Chapter 1 of a book on the origins of the Second World War may not necessarily effect what happens in Chapter 8.

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[118] Temporal distance does not always exhaust meaning though; think for instance, of contemporary British politicians invoking a desire to avoid 'appeasement' for instance.

One of the worries about a molecularist take on holism is that it appears to revive something akin to the analytic/ synthetic distinction, in that a non-meaning constitutive belief would seem to be analytic, while a meaning constitutive belief would appear to be synthetic. However, this all depends on whether we take meaning to have just one sense. Historical facts arguably have meanings in two senses. The first sense is what we might call a notion of meaning needed for a minimal understanding of a sentence; that of the component words and of them taken together. Any meaning properties here are arguably purely referential. So the meaning of the sentence/fact "Hitler became Chancellor of Germany in 1933" is literally just that: that we know what all the words/connectives etc. in the sentence mean. Call this sense of meaning 'wide meaning', in that every historian who writes on the subject of German history in the first half the twentieth century would be happy to assent to this (indeed, this is why the fact is admitted to the club).

The second sense of 'meaning' in historical works concerns what we might call consequences or significance of a historical fact or event. Think of how we use the word 'means' in this instance. 'Russian mobilization in 1914 almost certainly meant a European war'; 'defeat at Manchester City meant West Brom could no longer avoid relegation', and so on. Here 'means' is taken to signify consequences: the 'meaning' of an event is what it led to. There are slight problems with terming this 'narrow' meaning, on the basis that it seems to imply that the assessment of consequences of events is an entirely subjective affair which no two historians could agree on. And this clearly not the case; two historians are quite capable of agreeing that, for instance, that the 281-200 majority in the House of Commons confidence vote in 1940 meant the end of Neville Chamberlain's spell as British Prime Minister. But what splitting meaning into something wide and narrow meaning does do however, is show that a degree of overlap/ consensus can – and indeed does – occur between competing historical accounts without invoking something like an atomistic conception of meaning.

Conclusion

In this chapter I have looked at the issues surrounding holism in historiographical theory. I have argued that the non-standard take on holism that debates in historical theory have generally centred around what appear to be pseudo-problems at best, and I have suggested

precisely why these are pseudo-problems. I have furthermore argued that a more viable threat to historiographical realism lies in the form of the kind of global incommensurability postulated by the early Kuhn, as well as issues teeming from the more 'standard' version of holism. However, it is clear that an analysis of actual historical practice shows that global incommensurability fails to get off the ground. A form of local incommensurability is present, but this poses no threat to historiographical realism.

8) Narrative & Realism

In this final chapter, I will examine the epistemic status of historical narratives. Over the course of this thesis I started from the building blocks of historical practice – historical facts – and then worked up to issues concerning whole historical accounts, and thus we finish here, with the epistemic status of the narrative form, which historians more often than not utilise in presenting the results of their researches.

The best part of the past fifty or so years of historical theory have been marked by two claims that appear somewhat at odds with each other. The first is that the narrative form is indispensable to the writing of history – in other words, a form of essentialism about narrative. The second is that the narrative form inescapably distorts the past; narrative was "an autonomous realm that [is] somehow at odds with everyday life", and thus when we apply it the past, we inevitably distort the latter (D Carr, 2018, 73). We can rephrase these two strands of thought in terms of two questions. The first is: does the narrative form *necessarily distort* historical accounts by projecting onto the past a structure that is not found within the past itself? The second is: does the narrative form make a legitimate "contribution to knowledge", or is it merely a "literary artifact"? (Mink, 1987, 168). I argue in this chapter that the narrative form does make a contribution to historical knowledge, but not at the expense of realism - we can quite happily use narratives without be committed to anti-realism. Moreover, I want to outline some of the positive features that the narrative form brings to the table

Any philosophical account of historiography would be incomplete without addressing the subject of narrative: for it is undoubtedly the case that the narrative form occupies a significant place in historiography. As Dray put it nearly fifty years ago, narratives are an "admissible and prominent, although not universal, aspect of historiography" (Dray, 1971, 157). That is still largely the case; in 2017 Brian Fay wrote that narrative "is the preeminent form in the discipline" (Fay, 2017, 18).[119] That said, I reject the notion of narrative essentialism – for alternative reasons to be given momentarily. However, this does not mean that narrative does not have an important role to play in historiography, as we will see.

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[119] In 1973, Murray G Murphey predicted that "in recent years…there has been marked increase in the number of non-narrative historical studies. One may, I think, confidently predict that this trend will continue in the future." There is a lesson here for anyone thinking of predicting historiographical trends (Murphey, 1973,124).

It is particularly apposite to look at the epistemic status of narrative at the present time, for two reasons. Firstly, philosophers of science have recently started to take an interest in narrative as an explanatory form: moreover, they have been able to so without getting into the kind of anti-realist tangles that philosophers of history do when they try to philosophically account for the epistemic status of narrative.[120] Instead of worrying about what kind of 'entities' narratives are, philosophers of science instead seek to "defend the epistemic viability and productivity" of narrative construction – an approach this thesis will certainly try to emulate (Currie & Sterelny, 2017, 14). Secondly, Jouni Kuukkanen's recent *Postnarrativist Philosophy of Historiography* has argued that we need to move on from the 'narrativist' era in historical theory. As I hope will be proved by the present chapter, narrative has much to offer historians, and attempts to write its obituary, as Kuukkanen seeks to do, is premature.

Prelude: What is A Narrative?

It seems like the thing to do at the outset of this chapter would be define exactly what we class as a 'narrative'. This however, is something of a fool's errand. As Paisley Livingston (sic) puts it, with regards to a definitive account of *the* essence of narrative, "bald assertion and unacknowledged stipulation are all too pervasive in the literature", and as a result the author recommends "provisional agnosticism" on the matter (Livingston, 2009, 28). Certainly we do not wish to rush in where angels fear to tread. However; I concur with Marie-Laurie Ryan's stance questioning the need for a bulletproof definition in the first place. Instead, we should see narrativity "as a scalar property rather than a strictly binary one, and narrativity as a fuzzy set allowing different degrees of membership, but centred around prototypical cases that everyone recognises as narrative" (Ryan, 2006, 193).

Ryan gives the following list of criteria for a narrative

(1) Narrative must be about a world populated by individuated existents.

(2) This world must be situated in history and undergo changes of state.

(3) The changes of state must be caused be external events, not by natural evolution (such as aging).

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[120] See for instance, the special issue of *Studies in the History and Philosophy of Science* devoted to narrative in 2017.

(4) Some of the participants in the events must be human or anthropomorphic agents who have a mental life and react emotionally to the states of the world.

(5) Some of the events must be purposeful actions by these agents, i.e. the agents must be motivated by conflicts and their deeds must be aimed toward the solving of problems.

(6) The sequence of events must form a unified causal chain and lead to closure.

(7) Some of the events must be non-habitual.

(8) The events must objectively happen in the story world.

(9) The story must have a point (Ibid, 194).

It should be said that none of these are necessary features of a narrative; but most narrative will generally contain at least one or two of these. Ryan's list is arguably more criterial than necessary. The conclusion that we can draw from all this is that narrative is not easily definable, although determinate – something akin to a vague concept. All that said, with a couple of tweaks – for instance, changing point 8 to "the events must have objectively happened in the historical past" – the above provides a useful framework to work with when discussing historical narratives.

(i)   What Narratives Do Well

As stated above, although there are good reasons to utilise the form of narrative in historiography – indeed I am about to outline some of them – nonetheless, I reject the idea of narrative essentialism. This being that either a) works of history *must* take the form of narrative, or b) that there is no way for a narrative explanation be recast in a non-narrative way that carries equal explanatory force.[121] The fact that we reject narrative essentialism however, does not mean that narrative is a mere garnish placed on top of historical research: a serving suggestion, as it were. To use Mink's words, narratives are not "imperfect substitutes for more sophisticated forms of explanation and understanding, nor are they the unreflective first steps along the road which leads toward the goal of scientific or philosophical

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[121] As David Carr, who has written extensively on the topic of narrative puts it, "I have never understood why narrative and non-narrative history could not be considered two perfectly compatible and complimentary approaches to the past" (D Carr, 1986, 170).

knowledge" (Mink, 1987, 59-60). In the words of a recent literary theorist, narrative can be "an interesting and sometimes indispensable feature" of historiography (Klauk, 2016, 135).

In this section I will outline three ways in which the narrative form can make a unique contribution to historical understanding. The first is that narratives are good at enabling us to display contingencies and counterfactuals, which play a central role in historical explanation. The second is that they enable us to familiarise the unfamiliar, which also plays a key role in explanation and understanding. The third is that narrative can provide the kind of 'common ground' discussed in earlier chapters.

(1) Contingencies and Counterfactuals

To begin, let us look at contingencies and counterfactuals. John Beatty - a philosopher of biology - argues that "narratives are especially good for representing contingency and accounting for contingent outcomes." (Beatty, 2016, 34). Take the classic example of a micro-narrative: 'the queen died, then the king died of grief.' This is a narrative, but only just, because

> there is no *process* here. A process must have more than one step because it involves tracing possible futures…The sense of process, the activity of tracing possible futures from a given past, is essential to narrative. (Morson, 2003, 61).

Here Gary Morson coins the term 'presentness' – in a narrative, present moments must matter, and what gives a moment presentness is 'open time.' For a present moment to matter – for it have "real weight" – more than "one thing must be possible at the next moment. We may define open time as the excess of possibilities over actualities (Morson, 2003, 61-62).

Contingency is something that is arguably built into the historical process. There are very few historical events that are necessary; perhaps none in the grand scheme of things.[122] Even if we are inclined to say that some events were indeed inevitable in a sense – a prime example would be that some sort of revolution was inevitable in Russia in the early decades of the twentieth century – it was by no means settled when precisely such a revolution would occur or what the circumstances of it would be. Indeed, perhaps one of the peculiarities of historical

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[122] A.J.P. Taylor always argued against the use of the word 'inevitable' – "nothing is inevitable until it has happened.'

explanation is that it is more often than not just as concerned with what *didn't happen* as with what did occur – to continue with the current example, why was the Russian monarchy able to withstand the protests of 1905, but not those of 1917?

When we construct a narrative then, we embed what *actually* occurred within the context of what *could possibly* have occurred. Of course, we are primarily concerned with what did happen; but a narrative of what did occur "does what a narrative does best when it acknowledges the branching tree of possibilities in which it is embedded" (Beatty, 2016, 36). Another example: in the run-up to the outbreak of the attack on Poland, and thus the launching of the Second World War, Adolf Hitler hesitated at the last minute and tried to gain more time to negotiate with the British. Any account of the origins of the Second World War will surely refer to this hesitation and what might have resulted from it (i.e. the aversion of war); certainly any narrative account must do so – it must "indicate or allude to non-actualised possibilities and their consequences" (Ibid).

Key here is the notion of "turning points", which is used as a technical term in recent narrativist literature. Gerald Prince defines a turning point as "the act of happening that is decisive in making a goal reachable or not" (Quoted in Beatty, 2016, 38). This definition is problematic though; to stay with our historiographical example of European diplomacy between the wars; the majority of historians would be inclined to assent to the fact that the reoccupation of the Rhineland was a turning point. Was this event decisive in "making a goal reachable or not" though? One could argue that it marked the last favourable chance that France and Britain had to stand up to Hitler; certainly war was more likely after it than before. But again, I am not sure this ties into the notion of reachable goals. Or take the Italian invasion of Abyssinia in 1935 and the reaction of the various European Powers to it. We can state easily enough what the consequences were; it destroyed the Stresa Front and the League of Nations, while pushing Italy towards an alliance with Germany. But again - is talk of goals really appropriate here?

I should state that I am not arguing against the notion of 'turning points' or "hinge events" – I believe they exist. However, Prince's definition is clearly inadequate. Instead, I will substitute this one in its place; *pace* Beatty, we should speak of turning points as events or action that *all but rule out* certain possibilities or paths and making others more likely. The signing of the Nazi-Soviet pact in August 1939 for instance, made the outbreak of war within the next few weeks extremely likely (but not inevitable – remember Hitler's vacillations at

the end of August). Seymour Chatman uses the term 'kernels'; these are "narrative moments that give rise to cruxes in the direction taken by events" – they are "nodes or hinges in the structure, branching points which force a movement into one of two (or more) possible paths" (Chatman, 1978, 83).

We can bring this point out by asking: what would be the point of narrating something without any contingency; that is, of narrating events in an entirely deterministic world? The example Gary Morson gives is that of a narrative of Mars's orbit around the sun. This orbit can be specified entirely in terms of astronomical equations, but we could also narrate it: "in March, Mars was here, and then in May it was there, and in June…we saw Mars at yet another place, and so on" (Morson, 2003, 61). As we saw earlier, narrative requires presentness – "the present moment must matter" – it cannot be a "mere derivative of earlier events or dictated by later events, that is, by the structure of the whole" (Ibid).[123]

Narratives then, enable historians to focus not only on what happened, but also what might have happened. As Ricoeur puts it, historians "must struggle against the tendency to consider the past only from the angle of what is done, changeable, and past. We have to reopen the past, to revivify its unaccomplished, cut-off – even slaughtered possibilities" (Ricouer, 1990, 216). A historical narrative needs to raise the readers' awareness that what happened was by no means inevitable. Morson has coined the term 'sideshadowing' to account for this. This is contrasted with foreshadowing, where the narrator indicates that something will happen in the future. With sideshadowing, the sense is conveyed "that actual events might not have happened…Alternatives abound, and more often than not, what exists need not have existed" (Morson, 1995, 117-18).

(2) Action and Understanding

We saw momentarily that a narrative about something such as the orbit of Mars around the Sun would be a fairly pointless affair, and this spoke to the fact that narratives are suited to charting the development of contingent events. But this also testifies to the fact that narratives are suited to the description and explanation of agency and action; things that generally

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[123] It is not necessary that "all" moments in a narrative have such presentness, but some must.

cannot be subsumed under general laws. (This of course, was one of the hinges on which the debate over Hempelian 'covering laws' turned upon – there are no general laws of action.)

This point has been developed by both David Carr and Alastair MacIntyre. I argued in Chapter One that the past we are interested in is the human past, and thus to a large extent we will be concerned with human actions and thought processes. Carr characterises a narrative account as one that recounts actions by starting from their meaning for those involved; their "initiation in a perceived set of circumstances", their execution according to those plans, and finally the outcome of the actions in question (D Carr, 2008, 21). A quick recourse to the history bookshelves reveals that this certainly rings true; a book on the origins of the First World War will examine the actions of the various statesmen of the countries involved; an account of British politics in the 1960s will examine politicians and their attempts to guide events and their responses to other events, and so on. Even the Annales historians could not entirely eliminate human actions from their accounts, though not for lack of trying.

MacIntyre argues in a similar vein in *After Virtue*; to identify an action is to enable us to see it "as flowing intelligibly from a human agent's intentions, motives, passions and purposes" (MacIntyre, 1981, 209). In order to do this, we need to place the agent's intentions

> in causal and temporal order with reference to their role in his or her history; and we also place them with reference to their role in the history of the setting or settings to which they belong….Narrative history of a certain kind turns out to be the basic and essential genre for the characterization of human actions (Ibid, 208).[124]

How though, do such narrative accounts explain? More often than not, we find certain historical actions somewhat puzzling, particularly given that we have the gift of hindsight. Given that almost no-one expected a lengthy conflict when war broke out in 1914, why did the resulting war last for four years? Why did Hitler stop the German army advance in 1940, allowing the British to evacuate most of their men from Dunkirk? If we have a puzzling action, we can construct a narrative about it that will "place that action in its temporal continuum, relating it to previous actions and events that led up to it" (D Carr, 2008, 21). Furthermore, said narrative also places the action "into relation to a future scenario or set of possible futures" (Ibid). A narrative then, on Carr's view, works as explanation by making

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[124] MacIntyre goes onto claim that in trying to identify and understand what someone is doing, "we always move towards placing a particular episode in the context of a set of narrative histories." Needless to say I reject this claim.

the unfamiliar familiar – it "performs the function of placing the puzzling action not only in a temporal context, but also in a familiar repertoire of actions, emotions and motives" (Ibid, 22).

The above ties into what we might call Carr's narrative phenomenalism. For Carr (responding in large part to Hayden White's view that narratives are imposed not found), "storytelling obeys rules that are embedded into action itself, and narrative is at the root of human reality long before it gets explicitly told about" (Ibid, 29). Thus, there is a closeness in terms of structure between "human action and narrative" that means "we can genuinely to be said to explain an action by telling a story about it" (Ibid). An agent's awareness of a situation, their desire to reach a certain goal, and their choice of means to achieve it, all form part of an action - whether they tell anyone about it or not.

Historical narratives are arguably all about actions, and again, we come back to the notion of counterfactuals: historical narratives abound with things like "unfulfilled expectations, unwarranted beliefs, failed attempts, crushed hopes, suppositions and false hopes, errors and lies", and so on (Prince, 1988, 3). Hence why the narrative form is particularly suited to the representation of them.

(3) Narratives As Providing Common Ground

A potential third reason – one that has not been addressed in the existing literature to the best of my knowledge – relates to what we might call the pragmatics of historiography. It has been almost axiomatic ever since history became disciplinised that, in contrast to the specialised knowledge provided by the sciences, historical accounts are more often than not aimed at the general reader. We will come on to the notion of the 'non-detachability' of explanations momentarily; but it is worth noting here that one thing a majority of historical accounts aim to do is to enable the reader with virtually no prior knowledge of the subject to be able to read and follow the account nonetheless. We might say then, that historical accounts seek to provide readers with information in order to follow the explanation.

This can be seen as a variant of the notion of common ground referred to in Chapter 3. A narrative account is able to provide the common ground necessary for the reader with little or no knowledge of the subject to follow the explanation/argument/thesis the author is putting

forward.[125] This is in contrast to specialised disciplines like the sciences, where it is taken for granted that, in picking up something like *The Journal for Quantum Chemistry* (if such an organ exists), one will have the requisite background in quantum chemistry in order to glean anything from the articles within.[126] (However, it might be pointed out that there also exist popular accounts of science that are written in the same way to enable readers with little or no scientific training to follow them: the most famous example probably being Stephen Hawking's *A Brief History of Time*.)

That being said however, one clear difference between natural science and historiography that at the cutting edge of scientific research, the sole vehicle of publication is the journal. This wasn't always so of course – one thinks of the *Almagest,* the *Principia*, and so on. For the majority of the twentieth century and the current century though, the full-length work in science does not play any role at the frontier of research. By contrast, book-length works can and do contribute to advances in historiography as well as articles published in journals.

What is it then, that the book length narrative work can do that the journal article cannot? The most obvious difference is one of scope. A recent biography by Mark Ormrod on *Edward III* springs to mind as an example where a narrative is needed. This is a work which due to its scope could not be accommodated in the confines of a journal (it is not an easy read though, by any stretch of the imagination, and thus cannot be said to be squarely aimed at a 'popular' audience – the point holds however). By contrast, articles in historical journals are necessarily specialised, in that they address a very small segment or esoteric area of the past.[127] (I sketched out why this occurs and the benefits of it in Chapter 6).

Indeed, I am inclined to say something akin to this: that we can see book length works and journal articles in historiography as working on two different levels. At the level of the historical journal article, historians are largely engaged in establishing the facts (and thereby adding to the club of historical facts), trying to achieve the kind of consensus I wrote about in

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[125] And conversely, non-narrative accounts often make no concessions to the reader; to come back to Lawrence Stone's *The Causes of the English Revolution*; the reader who comes to it with no knowledge of the events of the period in question will not take much away from it.

[126] Although readers of a historical narrative will need some knowledge in order to be able to follow the narrative, such as knowledge of human psychology. Such knowledge is arguably tacit, although still necessary.

[127] Clicking on a random edition of the *English Historical Review*, we find the following titles: "Confronting Grotius' Legacy in an Age of Revolution: The Cornets de Groot Family in Rotterdam, 1748—98"; "The Pershore "Flores Historiarum": An Unrecognised Chronicle from the Period of Reform and Rebellion in England, 1258—65"; and "Locked in a Dusty Cupboard, neither Accessible on the Policy-makers' Desks nor Cleared for Early Publication: Llewellyn Woodward's Official Diplomatic History of the Second World War." *English Historical Review*, Vol.127, Dec 2012.

Chapter 3. (The work done in journals can also been seen as akin to Kuhnian puzzle-solving, a point I have addressed elsewhere (Timmins, 2012). The book-length work, by contrast, tries to integrate the kind of detailed work done at the level of the academic journal into a wider context. Both are indispensable: to use LeRoy Laudrie's famous phrase, we need parachutists and truffle-hunters.[128]

If there is one lesson to be taken from the above, it is that any attempt to provide a homogenous account of the mechanics of historical writing will ultimately be fruitless. In many cases, narrative is the apposite form for presenting the results of historical research; yet it doesn't *have* to be. This neatly leads us on to the notion of narrative essentialism.

(ii)   Rejecting Narrative Essentialism

As stated earlier, I reject the notion of narrative essentialism: which is, in the words of one the recent proponents of such a view, that "narrative explanations prove to be *uniquely* suited to answer certain explanatory questions" – specifically those raised by historians (Roth, 2017, 42). The reason for my rejection of this is plain enough – there are numerous examples of historical works that have made an essential contribution to a subject area that do not utilise the narrative form – the aforementioned work by Lawrence Stone for instance.

That said however, we need not go to the other extreme, as Jouni Kuukannan recently has, and suggest that narrative should be jettisoned completely. One of the goals of Kuukkanen's recent *Postnarrativist Philosophy of Historiography* (the clue is in the title) is "to replace it [narrative] with something quite different" (Fay, 2017, 13). As Kukkannen states in the introduction, "I intend to establish that it is more fruitful to see historiography as reasoning for theses and points of views and the products of historiography as complex informal arguments than as narratives" (Kuukkanen, 2015, 10). Essentially the thrust is to try and revive the old analytic philosophy of history: historiographical works should be seen as "rationally warranted claims and argumentative interventions" (Ibid, 150.

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[128] I analysed the data from the 2014 Research Assessment Exercise, and this showed that on the submissions history departments made in the UK, 22% of the total submissions were book length works.

But as Brian Fay pointed out in his review of said book, the picture of narrative that Kuukkanen presents "neglects or undersells the many additional insights that narrativists have developed regarding the way narratives work in historical explanation" (Fay, 2017, 18). Here is a classic example of an either-or dichotomy: narrative has failed to prove that it is "the necessary or basic form that historiography must take", ergo it must be seen as a failure and replaced with something else (Ibid).

However, we can reject narrative essentialism without rejecting narrative. If we are to take as the starting point for historiographical theory history as it is actually practiced, then it is clear that the fact historians have largely (but not always) resorted to narratives is telling us something – namely, that it is a form suited to expositing knowledge and information about the past. Kuukkanen uses E.P. Thompson's *Making of the English Working Class* an exemplar for his argument/evidence model; but the irony is that *Making* is itself *a work of narrative*. As Fay elucidates, the reason that *Making* adopts the kind of structure it does is because "it is concerned to reveal the particular historical process of a particular historical phenomenon in which something new occurred in English history" – and this is something that narrative is uniquely suited to do, for many of the reasons set out in the second section of this chapter (Ibid, 19).

(iii)   Pseudo-Problems for Narrative Realism

The key question from our realist point of view is this: is the intelligibility that narratives provide something we project upon the past from without, or can it be said to be inherent 'within' the past? In this section I examine and rebut two anti-realist views of narrative. The first is the Mink/White view, in which narratives have/ ascribe qualities to the past which are not to be found in the past itself, while the second is Paul A.Roth's account of narrative which ties into his 'irrealist' view of historiography. Both views can be easily rebutted, and I see no reason to take either seriously as an impediment to a realist view of historiography.

(1) The Fictionality of Narrative

Both Mink and Hayden White have been the most prominent progenitors of what we might call the 'narratives are fictional' theory: both adhere to the notion that there are no stories in real life. In his famous essay on historiographical comprehension, Mink concluded that

Stories are not lived, but told. Life has no beginnings, middles or ends…There are hopes, plans, battles and ideas, but only in retrospective stories are hopes unfulfilled, plans miscarried, battles decisive, and ideas seminal (Mink, 1987, 60).[129]

Similarly, Hayden White states that in general

there has been a reluctance to consider historical narratives as what they manifestly are: verbal fictions, the contents of which are as much *invented as found* and the forms of which have more in common with their counterparts in literature than they have with those in the sciences (White, 1978, 82)[130]

Needless to say, I take issue with this, and will set out the problems with the White/Mink account and show that it poses no threat to our theory of historiographical realism.

One of the main themes of the 'lived not told' thesis is the idea that narratives project an artificial coherence onto the past that is not to be found in the past-in-itself. White argues that narrative "imposes a discursive form upon…events", agreeing with Barthes that the function of narrative is not to represent, but rather, to constitute a spectacle (White, 1987, 42-43). Narrative *adds* something to the events it portrays. As Carroll (summarising White's view) puts it, this narrative coherence is *conventional* rather than realistically motivated – "this inventing, distorting, constructing, imposing, constituting, meaning-making (signifying), and convention-applying activity are all acts of the imagination (in contrast, one supposes, to some more literal information assimilating process)" (Carroll, 2001, 137).

Mink takes a similar view, stating that narratives are "a product of imaginative construction" which could not defend their claims to truth "by any accepted procedure of argument or authentication" (Mink, 1987, 199). The upshot of this view is that insofar as "historical narratives represent the lives of the past in story form, they do not correspond to what existed in the past and are, therefore, fictional" (Carroll, 2001, 141). Narrative "is constitutionally unable to represent "life" (the real events and actions of the past) because of the form of narrative itself…the very form of historical discourse undermines its epistemic intentions" (D Carr, 1986, 10-11). Mink's view coincides with that of Sartre put forward by the character Roquentin in *La Nausee*; to present a human life in the form of an intelligible narrative is

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[129] Mink reiterated this in a later essay: "Only by virtue of such form [i.e. narrative] can there be a story of failure or success, of plans miscarried or polices overtaken by events, of survivals and transformations which interweave with each other in the circumstances of individual lives and the development of institutions" (Mink, 1987, 198).
[130] Italics in original.

necessarily to falsify it – "human life is composed of discrete actions which lead nowhere, which have no order" (MacIntyre, 1981, 214).

Yet this is simply not so. To begin with, we have argued above that what historical narratives do is (to use Carroll's term) "track courses of events" (Carroll, 2001, 142). As stated in Chapter 2, events have a real existence, and if they are "admissible ontologically", then they are "there to be discovered and represented" (Ibid). Moreover, the use of 'imagination' is not synonymous with 'making-up' or 'inventing', as Mink seems to take for granted. The notion that imagination is soley used to 'make things up' is completely with odds with recent developments in the philosophy of mind, particularly in what is known as simulation theory.[131]

Although there are divergent views among simulation theorists, the central commitment is "a belief in the existence of states of recreative imagining, their role in our everyday understanding of minds, and their capacity to reduce the amount of theorising that we need to attribute to people in explaining their mentalizing capacity" (Currie & Ravenscroft, 2004, 51). In our everyday lives we rely on the ability to "project ourselves in imagination into the situations of others", specifically those "whose behaviour we want to predict or understand" (Currie & Ravenscroft, 2004, 51). Thus, we use imagination in practical, everyday tasks such as buying train tickets or interacting with others. Imaginative projections involve can thus involve "the recreation of practical inference: we imagine ourselves in this situation and then, in imagination, we decide to do something" (Currie & Ravenscroft, 2004, 19-20).

In order to predict someone's behaviour by imagining, three elements need to be present. Firstly, we need to be able to acquire some knowledge, or at least some beliefs, about their situation; secondly, we must be able to place ourselves imaginatively in that situation to what we would decide, and thirdly, we must be able to draw a conclusion about what the person in question would do (Currie & Ravenscroft, 2004, 54). It is not hard to see how we can and do extend this to individuals and situations in the past. (Indeed, this is arguably what Collingwood was driving at with his re-enactment thesis, only naturally he lacked many of the concepts available to the simulation theorists.) In sum then, the idea that because

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[131] John Lewis Gaddis also has a nice line here; "we do not normally think of research in the "hard" sciences as an imaginative act. Where would Einstein have been though, without an imagination so vivid that it allowed experiments with phenomena too large to fit not just his laboratory, but his galaxy?" (Lewis Gaddis, 1997, 77)

narratives are 'imaginative constructions' they somehow lack epistemic warrant is decidedly dated.[132]

A corollary of White's thesis is that there are no such things as 'turning points' or significant events in the past itself; these things are not immanent in events themselves, but are projected onto them by the historian. White calls these the 'meanings' of events. One of the mainsprings of White's argument seems to be that the same event can have different meanings or a different significance in different narratives. The Wall Street Crash of 1929 can take its place in a narrative about economic history and in one about political history, and it will be significant in each for different reasons. Mink takes the same view: the same event, "under the same description or different descriptions, may belong to different stories, and its particular significance will vary with its place in these different – very different – narratives" (Mink, 1987, 198).

Implicit in this view is the notion that if there were such things as 'real stories', then they would have to be what are called "absolute stories" – that is, "for any series of events, an event emplotted in a narrative structure that is immanent in the historical process will have one and only one fixed significance" (Carroll, 2001, 148-49). This is what Mink referred to as the belief in Universal History – "the claim that the ensemble of events belongs to a single story" (Mink, 1987, 190). On such a view, a variety of historical narratives on the same subject should be compatible with each other and should aggregate into a more complex whole: "insofar as they make truth-claims about a selected segment of past actuality, they must be compatible with and compliment other narratives which overlap or are continuous with them" (Ibid, 196).[133] However, this is not the case on the anti-realist view, because narrative history "borrows from fiction the convention by which a story generates its own imaginative space", and this conflicts with the idea of the "past actuality as a singular and determinate realm" (Ibid, 197).

This is another of example of "either-or logic": either an event has a single meaning/significance, or else events have differential meanings in different contexts, and are

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[132] Although in fairness to Mink, simulation theory largely post-dates his time of writing. Contemporary historical theorists have no such excuse, however.

[133] Interestingly, Ankersmit has recently argued that such histories could indeed aggregate – somewhat of a *volte-face* from his earlier work. (Ankersmit, 2012, 107).

therefore fictional.[134] However, the fact that different events can be used differently in different narratives does not mean that therefore events are without inherent significance, or that any imputation of significance must be a fictional imposition. Again, to cite Carroll's critique of White,

> the presumed disjunction that either there is one real story or a multiplicity of fictional ones fails to accommodate the fact that courses of action intersect and branch off from shared events, which intersections and branches can be found or discovered (Carroll, 2001, 143).

We can allow that events can have different meanings/significance in different causal chains without coming to the conclusion that such meanings are entirely imposed on events.[135] We are quite entitled to speak of things such as turning points, which are not simply literary embellishments.

Finally, White draws on an old shibboleth of historiographical criticism that has been in play ever since Descartes – that of selection (i.e. that the historian cannot include everything that is known about a given period in a historical account, and therefore some facts must be left out). White writes that the fact that historical writing does not/cannot provide a perfect replica of the past "is usually construed as a simply *reduction* by selection, rather than as the *distortion* that it truly is" (White, 1978, 111). This has its origin (in more recent times) in Barthes's famous phrase that "art knows no static." This is to say that "in a story, everything has its place in a structure while the extraneous has been eliminated, and that in this art differs from "life" in which everything is "scrambled messages"" (Carr, 1986, 13-14). We see the same kind of thinking here that cropped up in the previous chapter: the historian's account is not an exact replica of the past, and therefore must be fictional. Once again, we have the Whitean dilemma, and as before, it can be easily rejected – we can "maintain that historical narratives are not, and in fact should not be copies [of the past] in the mirror sense while also maintaining that this does not make them fictional" (Carroll, 2001, 145). The fact that the historian cannot include everything in a historical account need not entail any kind of anti-realism.

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[134] The phrase "either-or logic" is taken from Richard Bernstein, and cited by Chris Lorenz in his article on White and Ankersmit. On a view based on either-or logic, "arbitrariness and chaos constitute the only alternatives for a firm foundation" (Lorenz, 1998, 313).

[135] Carroll gives an example of this: in a political or military history of Russia, the Battle of Stalingrad can be seen as an extremely significant event; not so much in an architectural history of Russia (Carroll, 2009, 137)

(2) Roth – Narrative Constructivism

Paul A. Roth has written on and off about historical narrative for the past twenty years or so, but recently returned to the fray with an article entitled "Essentially Narrative Explanations" (Roth, 2017). Unsurprisingly given the title, Roth here adheres to what we earlier described as 'narrative essentialism': in his own words, "narrative explanations prove to be *uniquely* suited to answer certain explanatory questions." (Roth, 2017, 42). The problem from our realist point of view is that Roth's exhortation of narrative ties in with his overall irrealism about historiography in general. The task for this section is to explain why Roth's irrealist view is problematic without getting too bogged down in his overall view of historiography, which among other things confuses the ontological with the epistemological.

Recall that in Chapter 1 we looked at Arthur Danto's concept of narrative sentences, and noted that philosophers have drawn all sorts of strange conclusions from the fact that there are certain things about events that can only be known after the fact. Roth is one such philosopher. Roth argues that narrative sentences can only be explained by narrative explanations. This in itself is not particularly problematic; however, it is quickly tied into Roth's view that "historical events exist only under a description", and that this description, "in turn, makes it possible to formulate truths about that event" (Roth, 2017, 42). Narratives can thus be said to "create" what they explain – not because they "make things up", but because "only by this means does a historian provide meaning and structure to a morass of details that otherwise has neither." (Roth, 2017, 42) The last sentence is a nod to Hayden White's view that the sets of relationships that historians set out as existing between events are not "immanent in the events themselves; they exist only in the mind of the historian reflecting on them" (White, 1978, 93).

To an extent this comes back to the idea of linguomorphism discussed in the previous chapter; that in order for us to be realists about our descriptions and explanations of the world (and by extension the past), the structure of the world must mirror the structure of sentences. If it does not, then we must necessarily become irrealists. Roth is influenced by Nelson Goodman's nominalism; that there are no properties or classes, but "terms or predicates, construed as, or constituted by, tokens of one sort or another" (Scheffler, 1996, 162)[136] Thus, features of the world (and the past) "will be dependent on the saying – that is, bought forth by the process of token production" – whatever we say "will in this sense be dependent upon the

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[136] I believe Schleffer's summary of Goodman's position here is a charitable one.

saying (Ibid)" But as Hilary Putnam correctly notes, "why should the fact that reality cannot be described independently of our descriptions lead us to suppose that there are only the descriptions?" (Putnam, 1996, 189). The fact that we cannot describe the world (or the past) without using descriptions does not mean that our view of the world is *necessarily* compromised *because* we must use such descriptions.

If we reject Roth's irrealism, are there any grounds for his arguments for narrative essentialism? I see no reason to revise my view that we need not be narrative essentialists based on Roth's views. Let us give an example of how a narrative sentence can be explained non-narratively. Take the following narrative sentence:

> The season started well for Tony Pulis' West Bromwich Albion side, as they won their opening three games; however, their subsequent failure to win in the following ten games resulted in Pulis' sacking on November 6th.

A narrative explanation of WBA's failure to win since August 22nd would presumably run along the lines of the following: "they conceded a late equaliser at home to Stoke, then lost 3-1 at Brighton, before throwing away a two goal lead at home to Watford…" and so on.

But is this *the only way* we could explain this state of affairs? I think not. We could construct an analytic explanation which could do the job just as well; one which did not need to refer to the individual games played in a chronological sequence, but that would instead to refer to things like the ultra-defensive way Pulis set the side up to play, the team's inability to score goals, the players and supporters increasing disenchantment with Pulis, and so on.

We are clearly in the realm here of Mink's view of the supposed non-detachability of the explanans from the explanandum in narrative, which I touched on in the previous chapter. . However, here we come back to what I talked about earlier vis-a vis common ground: whether one gives a narrative or an analytical explanation will to an extent depend on one's audience. If I am writing the above piece for a publication dedicated to WBA fans, then it will be unnecessary to tell them what happened in the matches this season, as one assumes they will already know – the necessary common ground will already be present. If I am writing for say a national newspaper however, then it will be necessary to provide some common ground for readers.

Roth would reject the notion that narratives provide background or common ground; arguing instead that the reason they include the content that they do is because there are no 'standard

descriptions' in historiography; an argument also made by Mink. Historians' narratives constitute the description of the phenomena to which they provide an explanation. However, is it really the case that there can be no 'standard' description of an event? Take for instance, the First World War. It is quite conceivable that we could put together a description of it, including all the relevant battles etc, in value-free language that would satisfy most historians. Such a description wouldn't be particularly enlightening, for the reasons we outlined in Chapter 2; but it is possible in any event. The fact that it is possible to put forward a value-laden description of an event; indeed, even of a fact ('Britain's stout resistance in the face of the underhanded Argentinian invasion ensured that the Falkland Islands remain in British possession') does not mean that standard descriptions are not possible.

Conclusion

In this chapter I have argued that we should take a realist view of historical narratives. Much recent philosophy of history – i.e. White, Mink, Roth – has tied narrative exposition in with anti-realist accounts of historiography. Yet we are not obliged to concur with such views; in particular, the idea that narrative form is something completely alien that is imposed upon events. I have shown in this chapter a view of narrative that is perfectly compatible with a realist philosophy of history. In the first section I outlined the things that narrative excels at showing, in particular using insights from recent philosophy of science. I then argued that we can be realists about narrative without resorting to or being narrative essentialists. I examined the notion of narratives making the past intelligible, before going onto rebut some of the recent anti-realist accounts of narrative that have been put forth.

Conclusion

In this concluding chapter I will briefly recap the chapter content of this thesis, before going to make some closing remarks out the current state of and future of historical theory.

Taken as a whole, the thesis divided into three sections. The first dealt with what we might call the building blocks of historical practise; the ontology of the past, the facts which historians utilise in their accounts and how these facts are established/ verified. The final section looked at issues that relate to historical accounts as a whole as opposed to their sentential components; specifically how historiography can be said to progress, the epistemic status of the narrative form, and issues relating to holism. Bisecting these two sections was a middle section which deals with some fairly technical issues relating to the reference and ontology of colligatory concepts. I appended a brief note in-between Chapters 3 & 4 explaining why this brief detour was taken.

In Chapter 1, I undertook some Lockean rubbish-clearing by dealing with some common arguments against historiographical realism that can quickly be dismissed. These tend to rest on a conflation of the metaphysical past with the historical past. The two of course, are different things, and this accounts for some of odd things that philosophers of history say, such as historiography is problematic because the past 'changes', and so forth. I also dealt with the old shibboleth that historiography is epistemologically hamstrung because historical events are not available to perception.

In Chapter 2, I outlined an ontology of the past. Although it is arguably possible to practise history without having a clearly defined ontology of the historical past, a philosopher of history has no such excuse. I outlined an ontology of history to the effect that historians investigate actions, events, and states of affairs. I also tackled the problem of historical facts; the problem being that these are undoubtedly the backbone of historiography, yet remain woefully under-defined in terms of what the exactly are. I argued that, as historians use the term, a 'historical fact' is a true sentence reflecting a historical state of affairs.

Chapter 3 was devoted to what, *pace* Carr, I termed the club of historical facts. I argued that in any reasonably well-established historical research area, there is an established corpus of facts that historians working in the area are entitled to draw upon as 'free moves' (to use a term of Brandom's). Not only are these facts so well-established as to be taken as 'given' by the historian, but by and large they are also significant facts that the historian needs to include in any competent account of the subject in question. Moreover, it is the use of such facts that ensures the commensurability of historical accounts, and thus wards off any kind of incommensurability which threatens historiographical realism.

Chapter 4 looked at the reference of colligatory concepts. Colligatory concepts are event names that tend to crop up only in historiography – things such as 'the Renaissance', 'the First World War', 'the French Revolution', and so on. By and large questions relating to the semantics of these names have been neglected by philosophers of language, due to the aforementioned fact that these are terms that generally do not crop up outside of historiography. This chapter offered some much needed clarification of these terms, with regards not just to how we secure the reference of colligatory terms like 'the Renaissance', but also what semantic category do these terms fall into – are they proper names or definite descriptions? (I argue that they should be classed as proper names with unorthodox features).

Chapter 5 looked at the referents of colligatory concepts, in the sense of what kind of ontological status these events have. I argued that, contra anti-realists who state that there are no such 'things' as the Renaissance, French Revolution, etc, that these are events that have/had ontological existence, and thus there is nothing 'unreal' or 'fictional' about such entities. In a nutshell, I argued that macro-events (as I term them) such as these are emergent entities that result from state of affairs and smaller events, but are not reducible to them (in other words, this is an anti-reductionist position).

Chapter 6 moved onto issues relating to 'whole' historical accounts. In this chapter I addressed the issue of historiographical progress. Most historians would – correctly – argue that historical accounts progress, in the sense that later accounts of say, the First World War are better than their predecessors. To put it another way, we know more about the First World War in 2019 than we did in 1969. But what is it that motivated this progress? In this chapter I rejected the notion of verisimilitude (a route that a realist might be expected to go down), on the grounds it is not consonant with historical practise, and instead put forward an

institutional account of historiographical progress, upon which progress in historiography is guaranteed by the epistemological standards set up by the institutional framework within which historiographical practise takes place.

Chapter 7 looked at the issue of holism in historiographical accounts. I addressed two potential concerns vis-à-vis holism and realism in this chapter; the first stemming from issues relating to the somewhat non-standard version of holism that is prevalent in historiography, and the second relating to the standard linguistic view of holism. If followed to their logical conclusions, both have the potential to lead to incommensurable historical accounts, which would of course be a hammer blow for realism. Fortunately this is not the case, and in this chapter I showed how a holist-inspired incommensurability can be avoided.

Chapter 8, the final chapter, looked at narrative. In the past forty years or so – what in historical theory came to be known as the 'linguistic turn' years – narrative anti-realism has been at the forefront of philosophy of history. In a nutshell, this is the idea that the narrative form is indispensable to historical writing; but this form *necessarily* distorts historians' attempts to convey what the past was actually like. As one would expect, I argued that there is no cause for concern here; that the narrative method of presentation – which in any case is not *essential* for historiographical (though it has its merits) – need not give us any cause for epistemological alarm.

The Way Forward

In this final section, I want to tie a bow on this thesis by looking ahead to various projects that I hope to undertake in the future. While I hope that this thesis will kickstart something akin to a 'realist turn' in historical theory, clearly much work remains to be done, on my part and by the discipline as a whole.

At the top of the list is arguably the question of historical understanding. The topic of understanding is a complex one, and I have skirted around it in this thesis on the basis that I would have been unable to do justice to it. I noted in the introduction that there was a movement afoot to revive the old 'analytic' philosophy of historiography; this seems to me to be a backwards move, literally and figuratively. One of the reasons for this is that the 'covering-law' period of the 1950s and 60s that constituted analytic philosophy of history

was narrowly focused on explanation, in line with Hempel's view (a view generally accepted in philosophy of science at the time) that understanding was a purely psychological phenomenon that contributed little in terms of telling us about the world.

As it turned out of course, not only was Hempel's deductive-nomological model not really suitable for history, but as positivism declined, it was seen as problematic with regards to science. In the past fifty years or so, the role of understanding in science has been increasing explored, with understanding and explanation largely being seen as two sides of the same coin.[137] Surely an adequate exploration of historical explanation needs to take this into account; thus, it is hard to see what exactly revisiting the historical theory of the 1950s and 60s is going to achieve.

I am also interested to further explore the areas highlighted in the two 'technical' chapters, being chapters 4 and 5. There is much work to be done on the semantics of terms such as 'the Renaissance', 'the Cold War', and so on – as Rabern has referred to them as, 'definite descriptions that have grown capital letters'. I argued in this thesis that such terms should be classed as proper names with unorthodox features, and this is something that needs to be developed in a journal paper.[138] I want to develop this insight, as well as exploring what exactly is meant by 'meaning' in historiography – an area which desperately needs to be properly addressed. I also want to investigate both the ontology and semantics of colligatory concepts; as we have seen in this thesis, I believe that we are more than entitled to think of something like 'the Renaissance' as an event; and if so then we are entitled to impute as much reality to this as our best friend's wedding.

As noted in the introduction, over the best part of a century, the pendulum of historiographical theory has swung from one extreme to the other; the clear-eyed optimism displayed at the start of the twentieth century giving way to extreme pessimism at the start of the twenty-first. What is needed is an account of historiographical theory that avoids both extremes: one that highlights the success of historians in recovering information about the past, while at the same time acknowledging that 'ultimate history' is, and probably always was, a pipe dream.

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[137] A good introduction to the topic can be found in Grimm et al, 2016.

[138] It is perhaps understandable why little attention has been paid to what we might call historiographical reference. As this is something that only applies in large part to historiography, it is unsurprising that philosophers of semantics have not shown much interest in the issue. On the other hand, most historical theorists lack the expertise in linguistics to make a decent fist of the task – Ankersmit being a case in point.

A realist account of historiographical practice is particularly vital at the present moment: in an era which is being described as the 'post-truth' era, a time where politics – in Britain and America in any case – has become the realm of the irrational. We need the integrity of professional historical practice, and we need a philosophy of history that reflects this. Historians and others have debated for centuries over what exactly the point of historiography is; this is another of those questions that needs the best part of 80,000 words to answer. I personally tend towards Hobbes' view that "no man can have in his mind a conception of the future, for the future is not yet. But of our conceptions of the past, we make a future." Now of course, whether we can in fact learn anything from history is up for debate: but once we start crafting our accounts of the past to suit present needs, or start making mischief about 'irrealism', the game is lost – a game we can perhaps. ill afford to lose.

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