Postnarrativist Philosophy of Historiography

Jouni-Matti Kuukkanen

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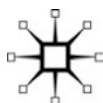
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Jouni-Matti Kuukkanen
University of Oulu, Finland

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Preface and Acknowledgments

Writing this book has been like trying to climb to the top of a mountain to get a clear view of the terrain below. Many monographs in the humanities are actually collections of articles published elsewhere, supplemented by an introduction and a conclusion. While I do not deny that some article collections result in balanced accounts, I pointedly wanted to create a connected and progressive chain of reasoning, which takes the reader from the starting point to the end so that the path, the turns on it and the reasons for those turns become evident. Obviously, what the reader has in her or his hand is not the documentation of all the twists and turns of reasoning, but the outcome of this activity. I hope that the book functions like one coherent and comprehensive piece on its topic and that all its parts appear justified. Achieving something like this has been my aim ever since I entered academia, although this dawned on me only much later.

It has not been easy to reach the mountaintop. There have been many false awakenings and attempts to force a path through impenetrable terrain. On a number of occasions I have realized that the elevation on which I was standing, and which I thought was the top, was actually only a small hill next to a much bigger one. And it has even happened that I have had to go back down and try another side after finding that an approach would not provide that clear vision that I was after. Now I believe that I am on the top of *a* mountain and can see far. I am not claiming that this is the only mountain, the highest peak or the only clear view to be had. Far from it. There are innumerable potential visions to be laid out and it is my belief that every author attempts to provide one.

The metaphor has its limitations too, as I do not think that the terrain below is or can be ordered only in one way. It is composed of philosophical ideas after all. Perhaps it would be more appropriate to think of the ground below as Lego blocks, which can be ordered and combined in innumerable ways. Some arrangements certainly are more robust than others and some reach higher and are more creative than others. But once there is order, there is clarity. Perhaps someone can arrange the blocks better, or use blocks that I have ignored, but now I have finished my job and am ready to offer my view to the reader.

The planning of this book began implicitly when I decided to teach a course on the philosophy of history and historiography at Leiden University in 2009, which was then repeated the following year in modified form. My participation in the research project 'Philosophical Foundations of the Historiography of Science' in 2008–2011 in Leiden prepared my mind for reflection on historiography. Actual research and writing were made possible by two academic grants. The Emil Aaltonen foundation provided a Young Researcher's Award for 2011–2012 and I was the recipient of a EURIAS fellowship at the Helsinki Collegium for Advanced Studies in 2013–2014. I would like to thank both funding bodies for their generous support. Parts of my paper 'Representationalism and Non-representationalism in Historiography', published in *Journal of the Philosophy of History*, 7(3): 453–479, are reproduced in Chapters 3 and 4. I am grateful for Koninklijke Brill NV for permitting the reuse of this material.

There are a number of people with whom I have had discussions on the theory and philosophy of history and historiography or whose comments on my texts have helped to draft the final manuscript. I warmly thank all of them and especially the following people: Frank Ankersmit, Aviezer Tucker, Herman Paul, Lisa Muszynski and James W. McAllister have all read some chapters or plans for the book. I am grateful to Kalle Pihlainen, who tirelessly went through the entire manuscript and helped to make the text more readable. I also thank Erika Servin who designed Figure 10.2 in Chapter 10 for me. Jorma Kalela deserves a special mention as he first formulated the idea of historiography as argumentation, which subsequently provided the central inspiration for this book.

1
Introduction: The Narrativist Insight

Imagine that you go to a bookshop near you and seek out the history shelf. What you have in front of you are the main scholarly products of the discipline of history. What are they? Naturally, they are books, often with illustrated covers and, typically, hundreds of pages of writing. But is this the main scholarly product? Ink on paper? Of course not. Ink on paper amounts to sentences, and the sentences in these books express statements about the past. Now choose some page from the middle of the book and select a sentence there. Is it possible to conclude that some particular individual statement that the sentence denotes in the middle of the book is the main result of that book? We cannot say that either. The book in your hand is not a collection of unconnected sentences and statements they express. It is a text. If you turn the book and read the blurb, you may get an idea of what it actually 'says', although only reading the whole text reveals the message fully. History books include integrative views, theses or claims, and all the hundreds of pages and their sentences and statements are designed to explicate and ground those. This is what I call the *narrativist insight*.

Most narrativists naturally call the integrative unit 'narrative', but the essential claim is that books contain some content-synthesizing entity. The narrativist insight may appear self-evident, that is, that the books of history articulate views on the past, but this insight had not been properly analyzed before the emergence of the narrativist philosophy of historiography in the early 1970s. (Stay with me for the moment; I will explain this terminology below.) Philosophically the main problem is to spell out what kinds of objects these synthesizing entities are and what their epistemic status is. What are they composed of? Do all statements in a book contribute to the synthesizing entity? In what role? Could a synthesizing view be true? How can we choose between different

views? And so on. The book now in your hand purports to answer these questions.

It is clear that answering these and other similar questions means taking a stand on the status of historiography itself. Many of the readers are probably familiar with the comparison of historiography to literature by the narrativists, and as a consequence, with identifying the central synthesizing entity as a literary product. It can be said up front that this is not how I see the matter. The traditional, Rankean, proposal is that historiography should be seen as a 'scientific' discipline. This is not feasible either, because the narrativist insight and truth-functionality are not compatible for reasons that are outlined briefly below (and at length later in the book). The view in this book is that historiography is a form of rational practice. Why this is so requires a great deal of explanation, of course. All I can say now is: please read on.

'If we reject, as I think we should, the ridiculous idea that in historical studies *anything goes*, then an essential and central part of our philosophical task is to determine what *stops* a historical interpretation; or more modestly, in case nothing stops an interpretation cold, then our task is to discover what *slows one down*'. So suggests Raymond Martin (Martin 1993, 32). Indeed, this is the problem: to find criteria that can be used to rank different historical interpretations, accepting that no interpretation is absolutely correct, but also insisting that it is neither the case that anything goes. From one perspective, this book could be situated between 'objectivism' and 'relativism'. 'Objectivism' is understood here (preliminarily) as a claim that narratives about the past are objectively given (by the past), and 'relativism' as an epistemic thesis that any evaluation is always relative in such a way that it perniciously erodes the epistemic authority of any historical thesis.[1] It could also be said that this is a book that attempts to solve Frank Ankersmit's translation problem – that is, how to translate the past or traces of the past into a narrative of historiography (Ankersmit 1983, 76–82, 190, 216–217). Although my judgment is that this problem is unsolvable when 'rule' is understood as an algorithm that would yield a uniquely correct account of the past, I nevertheless suggest that there are rules of thumb and other criteria in a looser sense that can guide our construction and that enable the ranking of alternative interpretations in terms of their cognitive qualities. This book attempts to establish a way in which it is possible to reject absolute truth-functional standards and replace them with a cognitively authoritative rational evaluation without implying that there are absolutely correct interpretations. It will be necessary to return to the definitions of 'objectivism' and 'relativism' in more detail,

but this characterization of the message of the book is sufficient for the moment. In general, I develop my reasoning progressively in this study. Later discussion builds on what has been earlier explicated. Where no deeper elucidation is possible due to limitations of space, I try to offer further references and literature for the benefit of the reader.

I have one request for the reader. This book should be read on its own merits. Although my starting point is in the contributions of the narrativist philosophy of historiography, which explains my title *Postnarrativist Philosophy of Historiography*, the book should not be seen as propagating any single existing tradition. The view in the book is an eclectic mix of theoretical influences and the aim is to produce a coherent, if not exhaustive, account of what historiography philosophically is. I would specifically ask the reader not to jump to conclusions on the basis of mere terminological or other resemblances to certain (in)famous philosophical positions. In this sense, the reviewing process of my book proposal taught me a valuable lesson. One reviewer understood the proposal as representing detrimental postmodernism and relativism; another saw it as an attempt to show how narratives can be true, and yet another read it as an unfair caricature of 'narrativism' and an attack against 'straw men'.

I am grateful to all the reviewers, but the reviews also prompt me to caution the reader. Specifically, I urge readers to be attentive to my take on some often-used concepts. At the top of the list are 'postmodernism' and 'relativism'. I do not know, which of these is more notorious, but I tackle the former first. Often a mere accusation of being 'postmodern' is enough to dismiss an account from being worth taking seriously. There might of course be proper 'postmodern accounts', and some thinkers have indeed outlined programmatic postmodern visions in various fields. The most famous of them is Jean-Francois Lyotard's *Postmodern Condition* (1984), but also F. R. Ankersmit attempted to formulate one in the 1980s (e.g. 1989a) and Keith Jenkins still commits himself to postmodernism (e.g. 2003). The latter kind of postmodernism will be discussed in Chapter 7. The problem is however that, in most cases where one uses the term 'postmodern', it is not clear what is meant by it and what is being objected to. The most natural reference point would be modernism, in relation to which 'postmodern' would then be defined, but this is not a great advancement, as it is too often equally unclear what precisely 'modernism' stands for. Typically the epithet 'postmodern' is attributed to accounts that allegedly suggest that nothing is 'real' or that 'anything goes'. It should become clear that this book does not represent 'postmodernism' in either of these senses.

In this book I specifically make a commitment to the notion of rationality and rational evaluation, something that is hardly compatible with the stereotypical 'postmodernist account'. The book could of course be postmodern in some other sense, if (and only if) the term is given an appropriate content. 'Postmodern' is only a term, after all. On the other hand, if categorical concepts are needed, I suggest that we look for new ones. I have chosen to call this book 'postnarrativist'.

Relativism is another bugbear in scholarly discussions. As with postmodernism, an accusation that someone is a 'relativist' is often reason enough to halt the analysis and brand the view indefensible. This is an ironic stance in philosophy, as philosophy should be the field of reflection, and reflection should make it evident that the concept in itself does not have much content. As James McAllister has pointed out, relativism expresses only a relation. Relativism concerning a property P means that statements of the form 'Entity E has P' are ill-formed, while the statements of the form 'Entity E has P relative to S' are well-formed. Naturally, this does nothing to reveal what E, P and S are. And as McAllister states, 'innumerable forms of relativism are entirely unobjectionable', such as the property of utility: a tool or an instrument, such as a hammer, is useful only relative to a purpose. What would 'absolute utility' be? Some forms of relativism may be difficult to accept, such as relativism about truth, which states that truth is always relative to some S, such as a culture or a language. My point is that these and other terms should be used with caution, and if they are judged appropriate, they should be applied only after the sense is understood and explicated. In the philosophy of science and epistemology, the property P in statements of relativism can be, for example, truth, rationality, evaluation, knowledge or reality. It is unlikely that one would find an account that is relativist in all these senses – and it can be categorically stated that this book does not represent such an account.

What is the field of this book more specifically? Nancy Partner makes a distinction between the traditional philosophy of history and historical theory in *The SAGE Handbook of Historical Theory* (Partner 2013). According to Partner, the philosophy of history refers to attempts to 'discern the shape and direction of very large scale changes in human collective life over long stretches of time' (Partner 2013, 1). Partner identifies Thucydides' cyclical repetitions, medieval Christian millennial ideas and Marxist and Hegelian dialectical history as examples of such 'philosophy of history'. Another relevant feature, Partner writes, is that 'history' as an ontological entity was assumed, not investigated, in this traditional philosophy of history. 'Historical theory' instead focuses

on 'history' itself, asking questions concerning the kinds of representations that are offered as 'true information' of the past, the ways in which description works and the operations that produce the intelligible linguistic structures of 'events-in-time' (Partner 2013, 2).

It is clear from the above that what Partner understands as 'philosophy of history' is what used to be called 'speculative philosophy of history'. Her term is problematic, as it overlooks the so-called 'analytic philosophy of history' that flourished in the decades after the Second World War and emerged as a critical reaction to the 'speculative philosophy of history'. This analytic philosophy of history certainly did not attempt to find large patterns of history itself. And it would be wrong to suggest that analytic philosophers of history took 'history' as given. Further, although the analytic philosophy of history was interested in questions similar to those of Partner's 'historical theory', subsuming the former under the latter would obscure the fact that analytic philosophers did not attempt to build any kind of 'theory', but practiced philosophy in a manner similar to (general) analytic philosophers of the time. That is, they analyzed concepts.

Partner is of course correct to point out that the old terminologies need clarification and updating, but the distinction between 'philosophy of history' and 'historical theory' is too coarse. Fortunately, there are more apt definitions available. To begin with, one needs to distinguish history clearly from historiography – something that is done increasingly often, although not as often as is desirable. Let *history* refer to past events and processes, that is, historical phenomena, themselves. *Historiography* can then signify the results of inquiries about history – which almost always take a textual form. If one wanted to express *historiography* briefly with a non-technical term, it would be the writing of history or simply history writing (see Tucker 2009; see also Tucker 2004, 1–6). The first half of the definitional task has now been accomplished. Let us turn to the 'philosophy' part.

Aviezer Tucker has suggested that we abandon the old discipline-designating terms, that is, *critical* or *analytic* philosophy of history as opposed to *substantive* or *speculative* philosophy of history. On the basis of the definitions above, it can already be seen why these terms won't do. Namely, it is very useful to make a difference between objects of investigation but much less so between styles of investigation. I am mainly interested in the scholarly 'knowledge' products of history as presented by historians and not in the nature of the past and its processes as such. The latter would require metaphysical investigations while the former, if historical presenting is narrowed to scholarly historiography, constitutes

a sub-field of the philosophy of science. Further, arguably, one could philosophize either speculatively or analytically (whatever these mean precisely) about both history and historiography. Finally, the distinction is also value-laden and contains an obnoxious statement concerning the worth of the philosophers who attempted to find large-scale syntheses of history. As Tucker puts it, '*Speculative* philosophy is essentially a term of abuse' (ibid. 4).[2]

Following this distinction between history and historiography, the two central areas for investigation are the *philosophy of history* and the *philosophy of historiography*. Philosophy of history is an examination of the nature of the past and its phenomena themselves, including such topics as the contingency, meaning, and directionality of history. Philosophy of historiography, by contrast, is the philosophical study of the results of inquiries about history, including history writing, the investigation of evidence and other epistemic questions (that may precede writing) as well as the central concepts and other structuring elements of historiographical presentation. There may be cases in which one has to violate this border, but this book is a study in the philosophy of historiography before anything else. Its focus is on scholarly historiography or on historiography as an academic discipline, not for example oral or folk historiography, which are guided by more lax norms and criteria. As expressed above, this means that the book is in effect a philosophy of science book. It is worth adding that I still use the term 'analytic philosophy of history' to designate that school and those practitioners who were writing mainly in the 1950s and 1960s.

But what do I mean by the 'narrativist philosophy of historiography'? Who are the 'narrativists'? For the sake of clarity, let me begin once more with the term 'historiography'. It should be emphasized that *historiography* does not mean here 'the history of history writing', as the term is sometimes understood. My focus is not on the historical development of narrativist theorizing on history, although certain philosophers or theoreticians of narrative are in a central role in this study. Provided that *historiography* means history writing and the *philosophy of historiography* the philosophical study of history writing, the *narrativist philosophy of* historiography is the philosophical study of history writing from a narrativist perspective. That is the target of *my analysis* in this book but – excluding the narrativist insight, which I endorse – it is *not* my view of historiography. There is a reason why this book is titled ***Post**narrativist Philosophy of Historiography.* While I fully accept and build on the narrativist insight, there are good reasons to consider going beyond narrativism in historiography.

In *Our Knowledge of the Past: A Philosophy of Historiography* (2004), Tucker questions focusing on the narrativist philosophy of historiography. He relies on Leon Goldstein's distinction between the 'superstructure' of historiography ('the finished product of historiographic research ... usually in narrative form') and the 'infrastructure' of historiography (invisible engagement with evidence and other research activity) (2004, 6–7). He criticizes narrativists for their emphasis on the finished product in the manner of logical positivism and empiricism before the historical philosophers of science transformed philosophy of science. Because the finished product does not reflect the actual research activity, it cannot, according to Tucker, reflect 'the historiographic process of inquiry' and be our guide into the epistemology of historiography. My view, to be specified in this book, is that the distinction between superstructure and infrastructure is not solid, because presentation is a part of the justification of a historiographical work and therefore must be a subject of historiographic epistemology. Further, the investigation of 'actual processes', if meant literally, would require studying historiography empirically as scholars in science studies and in the history of science have done in recent decades. Tucker's volume is rather a rational reconstruction of what historiographical reasoning could be. It is necessary to add that I have no problem with rational reconstruction (my evaluative dimensions are such) and Tucker's characterization may well contribute to what will be called (in Chapter 8) the cognitive justification of historiography. Still, the distinction also calls for the kind of study that investigates the actual reasoning directly in practice, which goes beyond the hypotheses and examples mentioned in his book. Lastly, and this relates to the first point, epistemological evaluation requires taking a stand on what the main cognitive product of historiography is and on what its epistemic standing is. The main cognitive product is not any singular evidential inference but something more synthesizing than this. This, my endorsement of the narrativist insight, is arguably the main difference between Tucker's project and the topic of this book.

Scholars whose primary interest is historiographical practice may wonder who the narrativist philosophers of historiography that I am talking about are. This is a good question. Yet this question also exposes a tension that exists between historical and sociological analyses on the one hand and philosophical approaches on the other. My claim is that 'narrative' as a notion and the focus on 'narrative' as an object of analysis are widespread and emerged largely due to theorizing on the role of 'narrative' in fiction and history writing in the 1970s. In this sense 'narrativism' could be seen as a dominant theoretical approach in the theory

and philosophy of history and historiography today. Further, 'narrativism' has, as a heterogeneous theory orientation, become almost global in the humanities and certainly goes much beyond the theory and philosophy of historiography (cf. Hyvärinen 2006). Chapter 2 prepares the ground for the explicit analysis of the narrativists' philosophy by charting the awakening of awareness to the narrativist aspects of history writing among the analytic philosophers of history. The chapter also briefly introduces the two main narrativist philosophers, Hayden White and Frank Ankersmit.

It is important to emphasize that this is primarily a philosophical study and not one in intellectual history. Philosophy is abstract by nature and philosophizing is bound to lead to abstractions, not because abstractions are necessarily desirable as such but because an abstraction is thought to express some fundamental concept or principle in its bare essence and with the highest degree of clarity. My approach to the narrativist *philosophy* of historiography is premised on two further assumptions. One of them is normative. That is, the narrativist philosophy of historiography is the most sophisticated and developed comprehensive philosophy of historiography within contemporary theory and philosophy of historiography. This refers to the scholarship of Frank Ankersmit and Hayden White above all. This is my assessment, but I believe that it is on a firm basis, and something that this book also testifies to. One consequence of this commitment and of my endorsement of the narrativist insight is that the first part of the book (Chapters 2 and 3) concentrates on the philosophy of narrativist thinkers. Some themes that emerge in this discussion have wider significance, but a more detailed philosophical analysis will be left for subsequent chapters. The other assumption is that this narrativist *philosophy* can be analyzed and its essence reduced, without any significant loss, to a number of central concepts and principles. It is *not* claimed that all theorists of narrative *explicitly* commit to this view. However, it is claimed that at least the best part of narrativist philosophy of historiography *implies* these concepts and principles and, further, that this analysis is also quite likely to apply much more widely to the narrativist theory tradition. In other words, the analysis expounded in Chapter 3 is my suggestion toward what the essence of narrativism is, something that has not been previously explicated in sufficient detail and with sufficient philosophical sophistication. Even if Ankersmit or White would not put the matters quite in the terms as I do, my claim is that the central concepts and principles as analyzed in this book, *representationalism*, *constructivism* and *holism*, are implicit in their work. My aim is not to repeat what they say, but to express what the things said by them philosophically amounts to.

The oft-heard criticism of philosophical analyses that they first construct a 'straw man' only to deconstruct him is beside the point here. Martin remarks:

> Having done one's descriptive homework, there is no reason why a philosopher (or anyone else) should refrain from prescribing some confirmational strategies as better than others. Of course, this is risky business for philosophers. They are standing with one foot on somebody else's turf. But those who would articulate a *philosophy* of historical methodology, unless they are professionally both philosopher and historian, will be standing with at least one foot on somebody else's turf. (1993, 31)

Indeed, philosophy of historiography is a risky business, because one should know both historiography and philosophy, just as the philosopher of science should know both science and philosophy. The fact that I have studied both subjects gives me some confidence in this task. More importantly, my view is that if one either manages to show what an account – such as a 'narrativist' entails, or that a coherent and compelling view can be constructed out of the elements of that account, the analysis provided should be taken seriously. Having said all this, I take some pride in the fact that I attempt to both reconstruct the narrativist philosophy of historiography crisply but fairly and to also illustrate my philosophical claims with references to actual historiographies. Although this is thus a *philosophy* book, I aim to exemplify my central philosophical claims with concrete examples and analyses, the approach that is most clearly visible in Chapters 5, 6 and 7.

After the initial exploration of the terms of the narrativist philosophy of historiography in Chapters 2 and 3, Chapter 4 assesses one of the key concepts of this school, representationalism. It is my suggestion that it would have been more advantageous and logical for the narrativists to have taken one further step and moved beyond representationalism altogether. That is, provided that one of the central tenets of narrativism is that narratives cannot mirror historical reality and refer to corresponding entities in the past, it would have been better to give up the idea of narratives as *re*-presenting something given. Sticking to representationalism requires inventing abstract objects to which historians' synthesizing presentations refer, and this approach is prone to making philosophical matters more complicated than necessary. The chapter also introduces the prospect of non-representationalism in historiography, something that will be further developed in later parts of the book.

As will be emphasized many times, I single out the suggestion that the books of history produce synthesizing views of the past as the most important contribution of the narrativist philosophy of historiography. Exaggerating slightly, one might say that everything else follows from this, much like the problem of representationalism mentioned above. Chapter 5 analyzes the presentational structures of historical works. The key questions here focus on what 'narrative' is and whether historiography is essentially 'narrativist'. To put it differently, should one assume that works of history necessarily amount to narratives? I investigate the structure of two books at length: E.P. Thomson's (1980) *The Making of the English Working Class* and Christopher Clark's (2012) *The Sleepwalkers: How Europe Went to War in 1914*. I could have chosen some other books, of course. My main criterion for choosing these is that they represent good historical scholarship and that they exemplify the kind of research that is commonly practiced in historiography. There is also one further reason for the choice. They both appear to be drafted very 'narratively', which offers a good testing ground for the examination of 'narrativity' in historiography. It is difficult to strike a balance between empirical adequacy and philosophical clarity, but these are the two chief virtues that have guided my approach in this book. It is for the reader to evaluate how well I succeed. By analyzing these histories 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. 'Informal' signals here that 'argument' should not be understood in the rigid formal way of logic and argumentation theory, but that the techniques and forms of reasoning can come in diverse modes, including narrative persuasion.

If Chapter 4 questions the tenability of representationalism, Chapter 5 calls another key concept, holism, into question. The examples from scholarship make it easy to appreciate that the books of history cannot reasonably be considered as constituting indivisible wholes. While the entire content of a book matters, of course, it is not the case that texts could not be analyzed into smaller elements without losing the most valuable knowledge contribution of historiography. My suggestion is that it is possible and reasonable to separate the *meaning* of the central thesis argued for from *evidence* for it in a work of history.

One of the greatest philosophical challenges and consequences of narrativism is the problem of epistemic evaluation in historiography. The narrativist philosophers have suggested that only moral and aesthetic criteria are available in evaluation. Chapters 6, 7 and 8 deal with this question from different angles and try to show that cognitive evaluation

can play a role in historiography. Chapter 6 deepens the analysis of historiographical language and focuses on the nature and role of colligatory concepts. The main example in the discussion is the concept of the 'Thaw', which has been used to describe the period of Soviet history from the mid-1950s to the early 1960s. What is the appropriate attitude to this kind of language, which is very common in historiography? One option would be to try to banish it as being too obscure. However, my suggestion is that the philosophers of historiography ought not to attempt with historiographical language what was once tried with scientific (theoretical) language by the logical positivists – that is, attempting to reduce a well-functioning discipline-specific language to some kind of simple expressions. It is less hubristic and more challenging for the philosophers of *historiography* to attempt to form a positive theory for the evaluation of colligatory expressions. In Chapters 6 and 7 I investigate whether colligatory concepts could be seen as justified (1) representationally, (2) referentially, (3) inferentially from historical evidence, and also (4) verificationally as empirically uniquely correct. It turns out that all these attempts fail. Chapter 7 suggests how one could nevertheless order and make choices between colligatory notions by relying on such epistemic values as exemplification, coherence, comprehensiveness, scope and originality.

Chapter 8 initiates the search for an alternative framework of evaluation in historiography, although it takes it that the ground for it was prepared already in Chapter 5 where it was suggested that books of history should be seen as *informal* arguments. This suggestion entails that historiography as a field can be located in the domain of rationality. Chapter 8 considers, first, what kinds of options and problems we have at our disposal with regard to epistemological judgment and with regard to rational evaluation more generally. This consideration raises the question of *epistemic authority*. That is, what is it that guarantees the epistemically authoritative status of an object of knowledge? I argue that whatever it is, it cannot fundamentally be a community in the Rortyan sense, but that proper epistemic authority requires community-transcendence of some kind. The problem is that truth, the traditionally favored option for acquiring epistemic authority, does not work in this context. When 'truth' is identified as correspondence, which I think is the most intuitive option, one is faced with the problem that synthesizing historical theses and colligatory expressions do not have truth-makers, that is, entities that would make them true or false, in the past. This is not to claim that there are no theorists of history who wish to see historiography evaluated truth-functionally also with regard to

synthesizing expressions. The fact that there are such scholars is indeed one of my reasons for searching for a way in which we could accept both the essential insight of the narrativist philosophy and yet leave room for epistemic or cognitive[3] evaluations. The existing attempts either do not take the narrativist insight sufficiently into account or they fail to adequately address the problems that emerge from the insight – namely that the synthesizing elements are the main cognitive historiographical contributions. In brief, my claim is that – despite some interesting proposals – no one has so far successfully managed to meet the evaluative challenge posed by the narrativist philosophy of historiography.

One has to find some other way to ground the epistemic authority of the central knowledge contributions of historiography than the truth-functional one. The explication of the evaluative framework by reliance on the concept of *rational warrant* is the task of Chapter 9, which is the culmination of this book. The stakes are thus high to the end. The notion of rational warrant is the governing concept here, but it can be further divided into three sub-components. A work of history can be evaluated in terms of its *rhetorical dimension* (the quality of reasoning); its *epistemic dimension* (in terms of epistemic values); and its *discursive dimension* (success as an argumentative intervention in the relevant argumentative context). The key point is to realize that all these aspects contribute to the overall cognitive and rational warrant of a historical thesis, the aim of which is to make the acceptance of the thesis *compelling* in its context of appearance. This suggestion resembles Quentin Skinner's idea to view texts as speech acts with which one intends to make a point in some context. The argumentative context and the notion of argumentative intervention receive a further empirical illustration in the form of the debate on the origins of the Great War.

In Chapter 10, the final substantial chapter, I will discuss a number of problematic philosophical concepts, such as constructivism and objectivity, which emerged in the preceding discussion. One of the key questions is that of when could we take an object to be 'real' and whether a constructed object can ever be seen as being 'real'. What does 'real' mean? Further, I suggest that the problem of objectivity and subjectivity is a case of a sliding scale and that *practically* all historiographical works should be seen as located somewhere on this axis, with almost all of them containing some degree of subjectivity and some degree of objectivity.

It is possible to summarize the conceptual landscape of this book with the help of three concepts – representationalism, holism and constructivism – which are introduced as the central tenets of the

narrativist philosophy of historiography in Chapter 3. As mentioned earlier, Chapter 4 questions the commitment to representationalism and Chapter 5 the commitment to holism. Chapters 6, 7, 8 and 9 agree about constructivism with the narrativist philosophy of historiography, but detail reasons, problems and solutions more precisely than the narrativists have done. Chapter 6 deals with the main constructivist entities, colligatory notions, in historiography. Chapter 7 initiates the search for an answer to the problem of evaluation in terms of epistemic values. This search continues with the problem of truth in Chapter 8, and the comprehensive solution to the problem of evaluation is finally outlined in Chapter 9. It is important to understand that constructivism does not mean that 'anything goes' but in fact the concept of rational warrant enables one to make epistemically authoritative and rationally principled choices between historical theses. As argued in Chapter 10, the choice that a historian has to make is how much subjectivity he or she will tolerate. While it may be possible to achieve a high degree of objectivity in historiography, it comes at the expense of significance and originality: the less the historian is prepared to say, the more objective but less interesting the result is likely to be, and vice versa. Popper once said that significance in science requires bold conjectures. This, I believe, is the case in historiography too.

2
From Analytic Philosophy of History to Narrativism

The philosophy of historiography looked very different half a century ago. Narrativism is currently the dominant school, but then it was the analytic philosophy of history, whose interests and problems were rather different. In order to appreciate the transition to narrativism and understand the change of perspective that accompanied it, it is instructive to begin with an exploration of the analytic philosophy of history.

Although Maurice Mandelbaum had published his analytic treatise *The Problem of Historical Knowledge: An Answer to Relativism* already in 1938, Carl Hempel's article *The Function of General Laws in History* in 1942 really kick-started the scholarly discourse that later became known as the analytic philosophy of history. This paper and the themes it raised came to dominate the discussion in the philosophy of historiography for the next thirty years.

Hempel was an arch logical empiricist, who formulated the (in)famous covering law model, that is, the suggestion for the formalization of (all) scientific explanation. This intellectual orientation is clearly apparent also in his article on historiography. Hempel's concern in this text is that the discipline of history does not seem to employ anything like general laws in its explanations because it studies individual phenomena. As a consequence, historiography did not seem to deserve the status of science. Historiography was, for logical empiricism and Hempel, thus a kind of borderline science and an unusually hard case to bring in from the cold of pseudoscience or non-science to the warmth of the family of sciences. Hempel is at pains to show that despite all the problems, 'general laws have quite analogous functions in history and in the natural sciences' (1942, 35).

Soon, two sides emerged. There were those, such as Mandelbaum, who together with Hempel, defended some form of the view that the same

kind of explanation can be found in historiography as in the sciences. And there were those influenced by Collingwood's philosophy, such as William Dray, who argued that specifically human or historical understanding or other field-specific explanation is used in historiography.[1] In general, the analytic philosophy of history was practiced under the shadow of logical empiricism, and the debate centered on the theme of the unity of sciences. Typical topics of interest were explanation, causality and understanding.

The analytic philosophers of history shared a mutual respect for a clear and argumentative style and an inclination towards conceptual explication with other analytic philosophers. However, sometimes it is even more enlightening to define a philosophical school by what it is not rather than by what it is. The analytic philosophy of history was not primarily interested in the writing of history. To put it in other words, it was only remotely concerned with the question of how historical findings are communicated by historians. Analytic philosophers did not analyze historical texts and their structures, that is to say, the elements that present historical knowledge.[2] They were oriented towards implicit historiographical explanatory patterns, such as the covering law model (e.g. Hempel 1942) and rational action theory (e.g. Dray 1982), the preoccupation with which practically narrowed analyses to individual claims, or at most to very short specific segments of historical texts. By implication, this negative characterization offers us a preliminary idea of what narrativism is concerned with. Its focus is on history writing and text as the end result of the writing process. Narrativists are interested not so much in the generation of historical knowledge and explanation as in the forms in which it is presented.

The shift from the analytic philosophy of history concerned with atomistic statements and explanations about the past to the text-oriented philosophy of historiography accords with the self-understanding of the narrativists, although Ankersmit contrasts the concern with 'the philosophical problems of historical research' to that with 'the narrative writing of history' (Ankersmit 1986, 14). The former is identified by an interest in such questions as 'what are historical facts?', 'how can facts be explained?' and 'how do values influence the accounts given of historical facts?' By contrast, the 'narrative philosophy investigates the question of how historians integrate a great number of historical facts into one synthetical whole' (Ankersmit 1983, 16).[3]

The story of rupture and the change of focus in the philosophy of historiography is certainly not the only account that could be provided about the history of philosophy of history and historiography, but it

appears to be a rational reconstruction in broad terms. Nevertheless, this tale needs some balancing. We should not forget that there were some scholars, whom we could see as participants in the discourse of the analytic philosophy of history, who were interested in and analyzed narrative aspects of history, but who still did not belong to the subsequent narrativist philosophy of historiography. The analytic philosophy of history in general and the debate on the covering-law model in particular have been discussed in earlier research, but the 'early narrativists' have not received much attention in previous studies.[4] They however initiated many themes and argued many points that were later adopted by the narrativists. Knowing what they wrote helps us not only to understand the narrativist philosophy of historiography, but also to formulate a framework for the postnarrativist philosophy of historiography later in this book. I will next examine their thinking in more detail, most of which appeared just before the publication of Hayden White's *Metahistory* at the beginning of the 1970s.

Early narrativists

As in the discussion on explanation, there were two sides in the debate of the early narrativists. A number of scholars, such as Arthur Danto, W. B. Gallie, Louis Mink, and Morton White suggested that narrativity is characteristic for historiography and distinguished it from the sciences. On the non-narrativist side, opposing the view that takes narrative as an essential feature of historiography, we find Mandelbaum and Behan McCullagh and a few others supporting their line of argumentation.

One of the earliest noteworthy suggestions is Danto's idea of 'narrative sentences' that amount to 'a differentiating feature of historical knowledge' (Danto 1962, 146). These are sentences that refer to at least two time-separated events although they describe or 'are about' only the earlier event. A simple example offered by Danto is: 'The Thirty Years War began in 1618'.

Danto shows that, because of the narrative form of historical knowledge, historical presentations cannot be made to correspond to the past. Danto defines the 'full description' of an event E as a set of sentences that 'state absolutely everything that happened in E' and says that there is an isomorphism between the full description and the event of which it is true (Danto 1962, 151). Then Danto imagines an Ideal Chronicler, who knows everything that happens, including people's minds, in the moment it happens, who is also able to write down exactly how it happened (Danto 1962, 152). Danto notes that there is

a class of descriptions of any event under which the event cannot be witnessed, and which are therefore necessarily excluded from the Ideal Chronicle produced. They are the kinds of descriptions that contain a future-reference or other associated post-dated significance that cannot be observed at the time of occurrence. No one could have observed that The Thirty Years War began in 1618, because no one could have known at the time that it would last 30 years. Or to take another example of Danto's, suppose that two scientists A and B at two different points of time t_1 and t_2 discover a certain theory T, but that the first scientist A never published his results and that his discovery is found much later, after B has already been given the credit for the discovery. A historian of science might describe the situation saying that 'A anticipated at t_1 the discovery by B of T at t_2'. 'Anticipation' is something the Ideal Chronicler cannot even in principle witness at the time of an original event (Danto 1962, 154–5, 158–159).

Danto's claim is that historians use narrative type of descriptions, which attach retrospectively such valuations and significations to the events that are not part of the events themselves. Any event can be placed under different descriptions and seen from the angle of a later one. The condition for the Ideal Chronicle is then that the Ideal Chronicler should be able to know not only future events but also the minds of future historians – that is, what future historians are interested in and what later events they will relate to the earlier ones. Danto thus demonstrates that historical accounts cannot be reduced to their object, the past, in any simple way, but that historical interpretation implies unavoidable subject-sidedness; it involves 'an inexpungible subjective factor' (Danto 1968, 142).

Gallie suggested that story or narrative is essential to all history-writing and peculiar to historical understanding. What is more, story is not merely an essential, defining characteristic of historiography, but 'every genuine work of history…is *a species or special application of the genus story*' (Gallie 1969, 169; my emphasis)[5]. Louis Mink drafted an interesting response and a development of Gallie's contribution. Mink does not criticize Gallie's emphasis on narrative, but writes that it is a mistake to think that the essential feature of narrative is following a story, whose conclusion the reader does not know. The 'configurational mode' as typical for historical comprehension treats a number of separate things as 'elements in a single and concrete complex of relationships' so that they are 'just in balance' (Mink 1970, 551). So a letter may be related to a story or narrative that relieved a misunderstanding at a crucial moment. Further, Mink is convinced that 'stories are not lived

but told' (Mink 1970, 557), more specifically that only stories – and not life – have beginnings, middles and endings that weave together separate images of recollection. His idea is thus that the narrative qualities are transferred from art to life. This is something that came to be endorsed by Hayden White in the years that followed, but was intensely disputed by the phenomenological narrativists such as David Carr (1986).

What is particularly interesting in Mink's account is the suggestion, in contrast to almost all other scholars at the time (with the possible exception of Danto), that although narratives are conceived of as stories, time is not of the essence. It is rather that the techniques of narratives are 'instruments for facilitating the comprehension of the story as a whole' (Mink 1970, 555). The actions and events of a story are 'connected by a network of overlapping descriptions' (Mink 1970, 556) that goes beyond a story and temporal sequences. Danto had claimed earlier that narrative imposes a structure upon events, grouping some of them together with others and ruling some out as lacking relevance (Danto 1968, 132; 140). And he saw narrative itself as a form of explanation (Danto 1968, 141; 237). In this way also Danto differs from most other discussants, who doubted whether narration can entail a structure and be explanatory.[6]

Another major figure who wrote favorably on the narrative aspects of history is Morton White.[7] M. White[8] writes that narration is 'the typical form of discourse employed by the historian' (M. White 1965, 4) and that narration must have a *central subject* of which the narrator gives 'a connected account of the development' (M. White 1965, 221). The fundamental problem for M. White is the question of how historians evaluate each other's works, provided that these evaluations need to go *beyond the truth and falsity* of the statements that compose the works. In other words, it is possible that all competing histories are true in the sense of containing only true statements, which means that on this basis one cannot make a difference between them (M. White 1965, 225). According to M. White, this issue comes down to the question of what reasons the historian has for including some statements rather than others in his narration. Or, 'what do historians mean when they say that one history of a given subject is better than any other?' (M. White 1963, 8). M. White argues for 'a pluralistic view of admissibility', which says that there can be various reasons for inclusion. Sometimes statements are included because they are assumed to present the typical features of a given period or because of their 'colligatory power' (M. White 1965, 257, 263–264). But on other occasions the choice reflects the historian's interests and value judgments about what is historically important or worth remembering. The message is that the historian is allowed to

choose 'his facts with eyes on all kinds of considerations, so long as he writes true and connected narrative' (M. White 1965, 259).

Mandelbaum (1967) was the first to react to the line of argumentation that places narrativity in a central role in historiography. Symptomatically with regard to the early discussion on narrativity, Mandelbaum treats stories, narratives and 'connected chronicles' largely as synonymous. His main point is that the historian is not 'engaging in an activity which is best represented by the model of telling a story', not even in a case in which constructing a 'sequence of occurrences' is the main aim (Mandelbaum 1967, 414). According to Mandelbaum, the problem is that 'the sequential story' does not amount to a proper analysis of the complex contextual factors that resulted in the outcome and neither does it amount to a satisfactory explanation as to why the events occurred as they did. Mandelbaum accuses the early narrativists of relativism because they allegedly hold that some facts and their relationships are regulated at least in part by the historian's story and are not given independently.

Also McCullagh (1969) joins the critical chorus and casts doubt on whether a narrative presentation could amount to an adequate historical explanation. The problem is, as he puts it, that: 'To explain how a situation changed is not the same as explaining why the change occurred' (McCullagh 1969, 258). The narrative explanation for McCullagh is describing the steps of change in a temporal chain, but the proper historical explanation has to be predictive. This makes him conclude that the narrative style has only dramatic value in that it may help the reader to experience the same kind of surprise as the historical figures themselves did when the events unfolded.

What kind of conclusions can we draw from the early narrativists' discussion? The early narrativists highlighted the narrativity of historiography and narrativity entails two important ideas: (1) there is a lower and higher level of cognition in historiography; and (2) the truth-values of the lower level statements cannot be translated into a justification (or falsification) of higher-level cognition. This is the central premise in the subsequent narrativist philosophy of historiography. They also shed light on the constructivist and colligatory character of historiography, seen in Danto's subject-sided 'narrative sentences', Mink's 'configurational mode' and M. White's 'central subject'. Further, with the exceptions of Danto and Mink, 'narrative' was fairly straightforwardly equated with 'story'. It was seen to designate temporality, the before–after structure, and sequential order. The third conclusion is that the analysis of narrativity focused quite narrowly on chronological or temporal structure. In

other words, no early narrative analyst paid attention to actual historical texts or narratives, but they all focused on short abstracted sentences at best. It is just, as Richard Vann writes, that the analytical philosophy of history tended to dissect historical discourse into its smallest intelligible units, such as two-sentence narratives, and as a consequence ignored the questions of genre, plotting and the fundamental organizing principles of history (Vann 1995, 61).

'Narrativity' in this early stage specifically did not address any kind of literary features embedded in historical texts. The predominant question was not what kind of narrative structure is characteristic of historiography – as a historical text was not the given object of analysis – but whether historical presentation requires narrative form and whether narrativity is essential for historiography. The question that preoccupied the minds of the early narrativists was whether historical explanations rely on a general explanatory scheme and whether narrative explanation lowers the degree of generality and scientificity of historiography. This concern related to the debate on the subjectivity and objectivity of historiography. The early narrativists, such as Danto, Gallie, Louch, and M. White, argued that the narrativity of historiography means that historiography inevitably contains subjective elements, while the critics tried to counter the claim by questioning the view that historiography is fundamentally a story or narrative.

The whole discussion took place under the shadow of logical positivism and empiricism, that is, a debate focused on causality and explanation, with specific reference to the covering-law model. As Vann points out, philosophical problems of *historiography* were often discussed by writing about cars with burst radiators or the indigestion that afflicted Jones after his ingestion of parsnips (Vann 1995, 41). Those who objected to viewing historiography as essentially narrativist typically contrasted explanation to narrativity, implying that a (mere) sequential ordering does not amount to or cannot be translated into a proper explanatory account of historical events. It was often suggested that a narrative account cannot involve contextual explanations, facts external to a sequence of events, or other explanatory principles. And even the early narrativists, who took a positive view of narrativity in historiography, referred to historical narration as a peculiar kind of explanatory account. Narrativity was seen as a 'distinct kind of explanation' (Louch 1969, 58), as 'self-explanatory' (Gallie 1964, 108) or itself as 'a form of explanation' (Danto 1968, 141, 237, 251).

Despite all the often very enlightening analyses of historiography by the early narrativists, disciplinary externalism characterizes their point

of view. In other words, concerns and questions emanated more from general philosophy of science than from the field-specific problems of historiography. This is indicated most obviously by the early narrativists' preoccupation with the notion of explanation and by their sensitivity to the unity-of-science theorizing on historiography. The emergence of narrativism in the beginning of the 1970s reset the focus of the theoretical discussion in the philosophy of historiography.

Narrativism

There is no doubt that Hayden White's *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (1973) transformed the landscape of theory and philosophy of history and historiography. A telling indicator of his influence is the praise that has been heaped upon him and his *Metahistory,* both at the time of its appearance and retrospectively by many eminent scholars in the field. Soon after the publication of *Metahistory,* Mink said, quite correctly from the contemporary vantage point, that it was 'the book around which all reflective historians must reorganize their thoughts on history' (1987, 22). Ankersmit has called *Metahistory* the 'most revolutionary work on philosophy of history' (Ankersmit 1986, 18). Brian Fay, writing at the end of the millennium, in turn suggests that 'an important shift in philosophy and theory of history occurred twenty-five years ago' when *Metahistory* appeared (Fay 1998, 2).

There are many ways in which one could characterize White's revolutionary impact as well as many different aspects in his thinking that one could highlight. Nevertheless, one of the most fundamental changes that White brought about is arguably the shift of focus from individual statements about the past onto entire texts of history. This perspective on historiography is absolutely crucial if one wishes to understand what the discipline is about. Take any book of history into your hand and open it; you will notice that it is indeed a text. Studying and reading it a bit more closely reveals that it contains lots of sentences, but also chapters. Typically, the book has a beginning, middle sections and a conclusion. If one is studying a monograph on history, by far the most common book type in historiography, we soon realize that all these elements, the text in its entirety, seem to function as a whole to form a view or views of the past. If one is very busy, one could read only the blurb on the back, which typically provides an overview in a few sentences. Perhaps one would read the conclusion as well, but if one really wants to understand what is argued and how, one has to read through all or most of the

text in the book. The text encapsulates the most important product of historical scholarship.

In order to illustrate the narrativist perspective, I mention an example. Fritz Fischer's *Germany's Aims in the First World War (1967)* (*Griff nach der Weltmacht*) is an eminent study of the origins of the Great War and contains a wealth of information and statements about the past. On page 310 it is claimed that Austro-Hungary's aim in the war was to 'get the greatest possible increase of power and security when things are re-arranged'. To take another random example, on page 510 we are told that Soviet Russia recognized the independence of Finland on January 4 following personal negotiations between Svinhufvud and Lenin. However, Fischer's book is not about the war aims of Austro-Hungary or the independence of Finland or the Soviet policy at the end of the war. It is about the aims of Germany in the Great War and implies a substantive thesis: the First World War was a premeditated German bid for power. That is *the* central historical view of the book and something that the reader of the book should not miss. In other words, theses like the one in Fischer's book are the most important contributions of historiography to our understanding of the past. One can of course be interested, and often is, in some singular claim included in a book and may wonder how the historian justifies it, but such claims are clearly subsidiary and integrated to (the argument for) the general point put forward.

Frank Ankersmit's *Narrative Logic: A Semantic Analysis of the Historian's Language* from 1983 amounts to another landmark publication in contemporary narrativism. It is a book that developed narrativism into new and philosophically more explicit directions and can be said to have opened a whole new discursive level in the philosophy of historiography. Similarly to White, Ankermit suggests that we understand historical works as producing holistic literary or linguistic theses, such as the idea that there was an industrial revolution at the end of the 18th century, that the 17th century was an age of crisis or that there was a cold war between the superpowers after the Second World War. These kinds of theses are compared to 'comprehensive, panoramic interpretations of large parts of the past' (1983, 7, 15). Indeed, as already mentioned previously, the absolutely fundamental function of narratives is, according to Ankersmit, 'how historians integrate a great number of historical facts into one synthetical whole' (1983, 15). He points out that the study of narrative is something that the earlier philosophy of history left out (2001, 53), which is largely but not entirely true, as we saw above. In brief, 'Narrativism is that view which requires the historical theorist to

focus on the whole of the historian's text and not its constituent parts (for example, its individual constative or causal statements)' (Ankersmit 1995a, 155).

Ankersmit stresses that, if we omit the narrative level of historiography, we ignore the feature that is most characteristic of historiography. This idea is expressed powerfully in the following quotation:

> In the first place, it cannot reasonably be doubted that these narrative sentences are paradigmatic of all historical writing: historians state facts for no other purpose than to relate them to other facts. So take away from historical writing these narrative sentences and what they effect, and you have transformed historical writing into a corpse without a heart. (1987, 68)

Historical writing without narrative sentences is thus a body without its heart. Who would like to reduce such a flourishing and dynamic practice to a corpse?

The difference with the analytic philosophy of history is clear as analytic philosophy did not seek to understand and analyze the nature of historical texts and the comprehensive historical theses that they contain. The analytic philosophy of history was too much driven by its normative ambitions to link historiography to the family of sciences. Even the early narrativists in all their well-intentioned attempts at studying the narrative aspects of history-writing ended up examining contrived 'two-sentence narratives'. This neglect makes the contribution of the narrativist philosophy of historiography extremely valuable since it highlighted an overlooked narrative aspect of historians' practice and analyzed the products that result from this practice.

Both the name 'narrativism' and the discussion above suggest that the central theoretical notion of the narrativists is that of 'narrative'. It is something that organizes textual material into one intelligible form. Now, it is of course a contentious issue what, exactly, narrative is. We saw above that most of the early narrativists understood narrative as a story, typically containing a beginning, a middle and an end. But note that Hayden White actually says very little about narratives as such in his *Metahistory,* despite the fact that he is given the role of 'founding father' of contemporary narrativism. On the other hand, Ankersmit discusses the nature of 'narratives' at length in his *Narrative Logic.* And it can be said that 'narratives' and equivalent technical terms 'narratios' and 'narrative substances' are certainly not stories to him. He writes that 'whenever in this book the terms "narration" and "narrative substance"

are to be used, all associations with the *belles-lettres* and with a story-telling kind of historiography should be avoided' (Ankersmit 1983, 16; similarly 1986, 2). The function of narrative is to propose points of view on the past. Metaphysically they are 'primary logical entities in histo-riographical accounts of the past' (1983, 94) and amount to 'the third logical entity' in addition to the subject and predicate already known in propositional logic (1983, 95).

Now, it is not important to nail down here what 'narrative' is. Much more important is to recognize that White and Ankersmit both paid attention to the unifying structures of historiographical studies and attempted to define them in more precise terms. Even if we assumed that a historical study necessarily contains a beginning, a middle, and an end, it is obvious that there is much more to say about how histo-rians present their most important research results. In other words, the unifying structure is much more complicated and much more philosophically involved than the talk about 'story' implies. This is the issue that the two central narrativists have pursued in their own ways. Ankersmit's strength is that he has detailed the philosophical implica-tions, which we discuss in the following chapter. White's take is more elaborate when, in *Metahistory*, he describes the structural components that result in concrete 'stories' or 'narratives'.

Reading Hayden White and Frank Ankersmit

This chapter is designed to outline the transition from the concerns prevalent in the analytic philosophy of history to those of the narra-tivist philosophy of historiography. If this transition has to be reduced to only one issue, it is the shift of focus from atomistic linguistic analysis to the whole texts of history. And while it is not suggested that the concerns of analytic philosophy of history are or were unimportant, the narrativist perspective on historiography certainly is important and crit-ically increases our understanding of historiography. In other words, the traditional philosophical questions and conceptual studies on causality and explanation, for example, have to be an integral part of the philo-sophical examination of historiography also in the future. But the same must be said of whole works of history and their texts, which arguably constitute the central historiographical contributions.

Narrativism as a scholarly phenomenon is diverse and widespread and not limited to Hayden White and Frank Ankersmit. In addition to histori-ography, the notion of narrative has 'successfully travelled to psychology, education, social sciences, political thought and policy analysis, health

research, law, theology and cognitive science', as Matti Hyvärinen notes in his survey of the concept (2006, 20). Naturally, my intention is not to include all this diversity in this one study. It would be too broad an undertaking, and it is also questionable whether all branches of narrativism in fact have something in common. But even in a more limited sense, as a philosophical orientation, narrativism has more than one face. My argument in this study is that White and Ankersmit collectively amount to the most developed and comprehensive philosophy of historiography that there is. And although they are different kinds of philosophers, and their styles are quite dissimilar, my claim is that they have enough in common to be treated as sharing a certain core common philosophy.

Sometimes White and Ankersmit have been categorized as linguistic narrativists because they understand narrative as a linguistic condition of historical presentation and because they are above all interested in the configuration that leads the historian to construct a narrative out of historical 'raw material' (Kalela 1990, 90).[9] There are also other narrativist philosophers such as Paul Ricoeur (1990) and David Carr (1986) whose orientation differs from that of H. White and Ankersmit. The name phenomenological narrativism is applicable to the former, as Ricoeur and Carr treat narrativity as a fundamental condition of human experiencing in general and have taken direct influence from phenomenological philosophers, such as Husserl and Merleau-Ponty. And in the case of Ricoeur, time and temporality could well be said to characterize his interests as much as narrativity. As he once put it, 'narrative [is]… a guardian of time' (Ricoeur 1990, 241).

While phenomenological narrativism will occasionally be discussed in this book, it does not constitute the core subject. These thinkers are discussed only when they seem to directly contribute to the issue under consideration, but as a philosophical school their differences to the linguistic narrativists are at least as significant as the similarities. The most important difference is that the former are primarily preoccupied with a metaphysical question, which is beyond the main research interest of this book. My aim is not to take a stand on the transcendental limit of human experiencing or to take part in the debate on the nature of time. My focus in this book remains squarely on historiography; specifically, on the nature of historiographical construction and configuration. Furthermore, just like Mink and Mandelbaum above, I am skeptical as to whether time and the sequential before–after structure is essential for historiographical construction and presentation. Tucker has more recently expressed similar doubts (Tucker 2004, 139). My own reasons will become obvious in the chapters that follow.

Commentators often emphasize that Hayden White is an elusive character. Even more, despite all respect and interest that White's scholarship has attracted, it is not at all easy to decipher White's exact 'philosophical position'. Some say that his scholarship is focused on creativity and productivity at the expense of consistency and systematicity. And he uses the essay style to provoke, 'to try things out', and always moves on to new topics. Vann has said that any consideration of his *oeuvre* should therefore begin with the question 'Which White?' (Vann 1998, 144; 145); 'extracting from him – or imposing upon him – a systematic philosophy of history is impossible' (Vann 1998, 161). In his biography of White, Herman Paul similarly claims that the latter does not have any philosophical 'position' in the sense of a well-grounded system of philosophical beliefs (Paul 2011, 7).

This warning of over-interpreting White or imposing a rigid systematic philosophy on him is undoubtedly appropriate. Sensitivity to shifts in thinking must be one of the most valuable virtues especially in biographical writing, in which one tries to achieve a comprehensive and fair picture of someone's thinking. It is by no means rare that scholars change their minds and interests throughout their careers; therefore, one should be wary of the illusions of the career-length coherence of thinking. However, in this book the aim is not to create an intellectual biography of any one person. I make no claim to offer a 'full picture' of Hayden White, Frank Ankersmit or any other thinker who appears in this work. What is more important to note is the fact that not even in the case of White can we avoid the conclusion that his writings are philosophically loaded. While trying at all costs not to misrepresent White, I take his words at face value when he writes about the problems of historical knowledge and place his claims under philosophical scrutiny.

Paul specifically warns that reading White as an anti-realist risks missing the point that his epistemological anti-realism was inspired by moral reasons: 'although he [White] tended to accept Mink's and Ankersmit's epistemological anti-realism, his own reason for treating the past as meaningless in itself was a moral consideration' (Paul 2011, 116).[10] Granting this point about the moral rationale in White's philosophy, Paul's reading of White nevertheless implies that he was a (philosophical) anti-realist of some kind. White might have been motivated by moral considerations, but he ends up saying something epistemologically significant. The good news is that the existentialist interpretation does not seem to contradict the anti-realist reading; rather, they seem complementary. We can take the former as detailing a personal motivation and driving force in his career, which led to a rich variety of

essays and philosophical views, including anti-realism regarding historical knowledge.

With Ankersmit we are not faced with the problem of struggling to pin down deliberate argumentation for philosophical positions. Indeed, Ankersmit is often philosophically more amenable as an object of analysis than White for the simple reason that he is philosophically more explicit. However, Ankersmit also changes direction, as he clearly indicates at least on one occasion in his career (see below). This potentially poses a problem for the interpretation of his writings.

When *Narrative Logic* is read together with the papers written mostly in the 1980s, we find a principled and coherent view of historiography. The ten years from the beginning of the 1980s to the early 1990s constitutes the early period in Ankersmit's philosophical thinking and, in his view, more generally in the new narrative philosophy of historiography. According to Ankersmit, the attempt to formulate the transcendental condition of historical knowledge characterizes this early period. The introduction to a collection of his papers *History and Tropology: The Rise and Fall of Metaphor* is revealing in this respect. He says that the book attempts to 'break the spell of Kantian, transcendentalist patterns of argument' (1994, 17). He writes that four chapters out of the seven in the volume 'still operate on the basis of Kantian assumptions' (1994, 19), but the other three already attempt to move beyond it: 'If the collection could be said to tell a story, it would be the story of how to move from a metaphorical, transcendentalist conception of history to the Aristotelian–Freudian conception of historical writing' (1994, 28).[11]

Indeed, it seems that in the early 1990s Ankersmit became more interested in the question of how we experience our personal and cultural past, which subsequently led to such publications as *Sublime Historical Experience* (2005). Icke describes the change of focus in Ankersmit dramatically as a 'lost "historical" cause', and further calls the early Ankersmit 'The Good Ankersmit' as opposed to the later Ankersmit, whose goodness began to 'melt away' and who, as a result of this, succumbed to the 'highly subjective world of mysticism' (Icke 2010, 1, 8). It should, however, be noted that although Ankersmit in this later period devotes more attention to different questions than in the early stage, there is also detectable continuity and consistency in his career. First, questions regarding language, truth, knowledge, and historical representation have occupied him from his first writings. For example, historical representation was a theme of interest already in *Narrative Logic* (although the *term* itself does not appear there). The book that emerged twenty years later was tellingly entitled *Historical Representation* (2001). And the

same topic plays a central role in his latest work: *Meaning, Truth, and Reference in Historical Representation* (2012). Even in *Sublime Historical Experience* he emphasized that, although that book was about sublime historical experience, it did not question whether or not we could make true statements regarding the past or explain the past in terms of texts representing the past (2005, 14).

Further, there is a programmatic aim that interweaves different periods together. From the beginning, Ankersmit has been very explicit that he wants to create a new kind of analytic philosophy and extend the examinations of the philosophy of language from the level of singular propositions onto the level of texts. First, although Ankersmit says in *Narrative Logic* that his aim is to formulate a 'synthetical philosophy of language', the style of the book is remarkably analytic. In a way, *Narrative Logic* is a book of analytic philosophy that argues for a holistic approach to historiography. In *Historical Representation*, Ankersmit suggests that analyzing historical representations 'add[s] a new and important chapter to contemporary philosophy of language' (Ankersmit 2001, 283). More precisely, 'Philosophers of language have over the last century and a half closely scrutinized the notions of truth, of reference, and of meaning in order to clarify the relationship between the true statement and that of which it is true. But they have never ventured upon the problem of the text, and surely this is an important aspect of our use of language…philosophy of language will remain a mere theoretical torso unless it takes seriously the kind of problem that is addressed in philosophy of history' (Ankersmit 2001, 283; similarly 2008a). Furthermore, the analytic approach of *Narrative Logic* and *Historical Representation* resonates well with the agenda for *The Journal for the Philosophy of History*, written thirty years after *Narrative Logic*. The agenda is co-signed by Ankersmit, but one feels his touch more strongly in it, perhaps unsurprisingly since he is the chief editor. The agenda calls for clarity and rigor in the philosophy of history. It suggests that the philosophy of history may both learn from and contribute to the philosophies of language and science (Ankersmit et al. 2007). And the same desire to contribute to philosophy of language, to 'add a new chapter' to it, can still be found in Ankersmit's most recent book (e.g., 2012, ix). Also the break and discontinuity with Kantian philosophy is in doubt as he confesses in *Meaning, Truth, and Reference in Historical Representation* that, despite his resistance to it, transcendentalist discourse has inspired a large part of the book (Ankersmit 2012, 46).

However, perhaps a more important question than the continued development and rethinking of some particular issues is whether he has

changed his mind in some other important respects. For example, after reading *Narrative Logic* and the early papers, in which the view that a historical representation (and a text) cannot be true of the past plays a prominent role, one may be surprised to find the following statement in his most recent book: 'we are justified in speaking of historical truth. Not only are a historical text's individual sentences typically true of the past…the same can be said of that text *as a whole*' (Ankersmit 2012, 124). So much for the postmodernism that has sometimes been attributed to Ankersmit!

There certainly have been modifications in the views and in their groundings as well as changes in emphases and terminology in his career. For example, with regard to the question of truth above, the understanding and use of 'truth' is different in these two contexts, which makes the seemingly contradictory assertions compatible. But it is again necessary to add a caveat. As in the case of White, this book is not a biography of Ankersmit, not even an intellectual one.[12] My intention is not to force Ankersmit's whole career into a 'box of coherence' – but I do believe that there is a certain coherent and articulable core philosophical position to be found in his scholarship. If he has other scholarly interests and preoccupations – as most scholars do – these are beyond this study. This means that I will not be commenting on all of the changes of emphasis that his career may contain.

What I do claim in the next chapter is that three concepts, *representationalism*, *constructivism* and *holism*, are adequate to describe and summarize the central philosophical position of these two narrativist philosophers of historiography. The purpose of this chapter has been to set the stage for later investigations and offer an overview of the development in the philosophy of historiography in the decades after the Second World War. The next one aims to spell out the *philosophy* of the narrativist philosophy of historiography.

3
Three Tenets of Narrativist Philosophy of Historiography

This chapter provides an in-depth analysis of narrativist philosophy of historiography. The two philosophers whose thinking was already introduced in the previous chapter are discussed in detail here: Hayden White and Frank Ankersmit. I hope to be able to show that despite some differences, they share a certain core philosophy between them.

The aim, in other words, is to spell out what this *philosophy* of narrativism is. My suggestion is that the kernel of it can be captured by three concepts: *representationalism*, *constructivism*, and *holism*. The emphasis in this chapter is on analysis, not on judgment. In subsequent chapters the evaluative function is more prominent. However, it would be impossible to analyze philosophical reasoning and claims without also providing some assessment of these three tenets of narrativism.

Representationalism

'Historians almost naturally opt for what one might call "the copy theory of historical representation." They believe that there has been a past that they should "copy" as well as they can in the language they use for writing about it. All that they say about the past should have its exact counterpart in the past itself – and language should not add anything to this. For that would be a distortion of the past *wie es eigentlich gewesen,*' writes Ankersmit in his recent book (Ankersmit 2012, 45). The quotation expresses the copy account of historical representation against which Ankersmit has argued practically throughout his career. It is noteworthy that the famous German statement by the 'father' of modern (scientific) historiography Ranke grounds this view theoretically and historically. Along similar lines, White suggests that one should not think of a historical account as a model in the way of a scale model of an airplane or a

ship, a map or a photograph (White 1978, 88). It is typical for White to refer to 'a scientific kind of representation', an allusion to the Rankean type of historiography, in his critical comments on the kind of historical theory that treats narrative as a realistic portrayal of the past (e.g., White 1984, 26).[1]

The message in this section is that both Ankersmit and White assume that representations and their production are fundamental for historiography, a position that is called 'representationalism' in this book. Representationalism appears in many ways and on many levels in the philosophies of both Ankersmit and White. It is to be seen in the criticism of Rankean historiography and in the general characterizations of the nature of history writing. And in the case of Ankersmit, it is also apparent in the development of an alternative representationalist account to remedy the problems of modern historiography. What is more, representationalism also limits the options available in this corrective endeavor.

For a more detailed analysis of representationalism it is good to start with Ankersmit's *Narrative Logic*. The possibility of historical *re-presentation* is the main sticking point between two central interlocutors, the narrative idealist and the narrative realist. One of the three pillars on which Ankersmit's philosophy, narrative idealism, rests is the statement that 'narrations contained in works of history are not images or pictures of the past' (Ankersmit 1983, 7). By contrast, the narrative realist is someone who regards *narratio* as a picture of the past and assumes that there is a correspondence between these two similar to that between a photograph or a picture and the reality depicted in them. Ankersmit says that the narrative realist understands historical *narratio* as something akin to a verbalization of individual images depicted in a film, and an even better analogy is the one between a machine and its blueprint (Ankersmit 1983, 75–76). Photographic, cartographic, and copying metaphors are very common indeed for both Ankersmit and White. They are used when the authors characterize the philosophical views that they oppose.

The interesting issue is that despite their critical perspective on representational realism, both White and Ankersmit see representations and the creation of representations as essential to historiography. White assumes that we have an inherent desire to *project* the qualities of narrative – such as coherence, integrity, fullness and the closure of 'an image of life' – onto real events, although the former features can only be imaginary (White 1984, 24–25). According to White, any historian has to face this problem of how to *represent* and re-create the past (White

2005). White talks about 'specifically historiographical representation', dubs the nineteenth-century classic historians whom he analyzed in *Metahistory* 'great narrativizers of historical reality' and suggests that (also) literature is interested in 'representing reality realistically' (White 2011, 392, 395, 398). Tellingly he calls the need to propose representations 'the decease of representationalism', thus indicating that realistic representation is something we are bound to attempt, but that it is an activity in which we will ultimately fail (White 2005).

Similarly to White, Ankersmit sees that creating representation is *the* central function of historiography. In spite of the fact that Ankersmit's first pillar in *Narrative Logic* rejects that *narratios* are images or pictures of the past, his second pillar suggests that (nevertheless) 'narrative historiography proposes "panoramic" interpretations, points of view or theses of the past' (e.g. Ankersmit 1983, 7). The representationalist language again catches the eye here. 'Point of view' implies that we are looking at some object from a certain location, which defines the 'point' from which we gaze at it. And 'panorama' is typically a comprehensive image of a landscape. Further, in some later publications, Ankersmit is very explicit as to what kind of role representation assumes in historiography. He writes that 'historical writing gives us representations of the past' and that '*all* historical writing aims at a realist representation of past reality' (Ankersmit 2001, 11, 25; my emphasis). Provided that this is the aim of all historical writing, it is only natural that 'the central problem of historical theory' is 'the problem of how the historian accounts for or represents past reality' (Ankersmit 2001, 68). Finally, the following statement in Ankersmit's recent book is perhaps the most direct expression of the intimate and necessary relation between historiography and historical representation: 'My main thesis will be that there can be no historical writing outside historical representation' (2012, 47).

Ankersmit is convinced that visual and optical metaphors also offer us the correct language for understanding the nature of historical text (1995b, 223). Indeed, without them we might never succeed in this theoretical challenge: 'The relevant secrets of the nature of historical writing can only be discerned if we see the historical text as *a representation* of the past in much the same way that the work of art is a representation of what it depicts' (Ankersmit 2001, 80). In so explicitly endorsing the analogy between visual arts and historiography, Ankersmit takes a step further down the path that began with White's comparison of historiography to literature. This path, paved with representationalist vocabulary, leads Ankersmit, and slightly less also White, closer to modern historiography than has been recognized. One thing is that Ankersmit now

clearly says that the representationalist account of Ranke and Humboldt was essentially correct although in need of updating (Ankersmit 2012, 1). On the other hand, many historical theorists bent on historical realism, unlike Ankersmit and White, nevertheless accept a similar characterization of the historian as a portrait painter, or other creator of representations, attempting to portray the past in his or her literary depictions (Ankersmit 1994, 145). For example, Croce understood historiography as a type of artistic representation that has the real event as its object (Croce 2012, 500). One might say that a commitment to representations, and more, to representationalism, that is, the view that historiography necessarily *represents* some kinds of objects, is a reversal of the radical legacy of narrativism, when it departed from the research-oriented analytic philosophy of history and from the traditional copy theory of historical reality.

But what is the kind of representationalism that White and Ankersmit advocate? I will begin with White. In the subsequent discussion on representation, he stays more in the background because, although he understands the basic representational function in history writing in very similar terms to Ankersmit, he has very little to say on the specifics of representation.[2]

White suggests that, even if we give up on the idea that the past can be represented directly, as if 'narrative' were just a 'neutral "container" of historical fact', we can still try to form representations of the past (1992, 37). His idea is that we use 'intransitive writing', in which the relationship to the events described is 'in the middle voice'. This can be understood as an attempt, not to portray the object as it really is, nor as a suggestion that we are imprisoned by our subjective point of view and language, but to describe (real) historical events through our own experiences. It is remarkable in this light, however, that White still wishes to cling to the idea of realistic representation: 'This is not to suggest that we will give up the effort to represent the Holocaust realistically, but rather that our notion of what constitutes realistic representation must be revised to take account of experiences that are unique to our century and for which older modes of representation have proven inadequate' (White 1992, 52). Although White thus regards the dream of copying the object in the historian's language as impossible to realize, he nevertheless hesitates in taking a step further and denouncing that the object is forever unreachable (although some of his Kantian inspired comments go some way towards this conclusion, as will be shown below). His middle voice is a compromise between these two extremes. This shows that White's theoretical thinking and options are seriously

limited by the subject–object dichotomy. Just as the visual and representational metaphors suggest, the basic constellation is that between the historian as the subject (attempting to describe) and the past as the object (to be described).

Ankersmit has developed his account of historical representation especially in his later writings. The central publications in this respect are unsurprisingly *Historical Representation* (2001) and *Meaning, Truth, and Reference in Historical Representation* (2012). The clearest expression of what the representationalist and visual analogy means can be found in the latter. This is that the relationship between the historical text and historical reality is 'aesthetic in the same way that this can be said of the work of art' (Ankersmit 2012, 62).

The new representationalist account is meant to replace the old one implied by modern historiography and shown to be untenable by its critics. What emerges is a new technical notion of historical representation designed to overcome the problems that the traditional concept is marred with. Another name for the old copy theory is the resemblance theory of representation. The idea in this theory is that representation should resemble what it represents. So if the resemblance theory was correct, historical representations, that is, historical texts, would resemble the past or the part of the past that they represent.

The problem with the resemblance theory derives from the nature of representation itself. First, Ankersmit suggests that we distinguish between the terms 'reference' and 'representation'. They both stand in *a* relationship with reality, but while description may refer to reality via its subjects terms, a representation can only be said to 'be about' reality (2001, 41). An example is the representation of a black cat. According to Ankersmit, only in descriptions can we distinguish between a referring object, that is, 'cat', and its predicate, 'is black'. In pictures, and in historical representations, this kind of distinction cannot be made. That is, Ankersmit suggests that one cannot differentiate those parts of the picture that refer to a 'cat' from those that attribute 'is black' to it. Similarly, he says that one could not determine a reference of the 'Renaissance' and properties attributed to it in a historical text.[3]

In other words, visual and historical representations form wholes that cannot be decomposed into their constituent parts. According to Ankersmit, the problem is that the relationship between representations and past reality cannot be determined. We may well begin from an intersubjectively determinable level, such as the physical features of a model in a portrait painting and the sum of descriptions in a historical text. However, this is just the surface. Both types of representations

(visual and historical) go deeper than that and also contain 'personality', something that cannot be detected intersubjectively and will always remain unstable and unfixed. Representations themselves have layers that make reality opaque. Further, Ankersmit adds a metaphysical difficulty that hampers any attempts to reduce representations to their putative objects, that is, to the historical reality. The problem is that only representations, and not reality, can be 'coherent' or 'consistent'. White made very similar remarks above (p. 45). Further, reminiscent of *Narrative Logic*, *Historical Representation* characterizes this position as idealist and uses Kantian language in saying that the linguistic level of representation 'determine[s] what we shall find on the second level' (Ankersmit 2001, 39–48).

Now, what is to be done if the resemblance theory of representation is hopeless, but one still wishes to speak about historical *representations?* The answer is to change the way in which 'representation' is understood. Indeed, Ankersmit proposes that the 'substitution theory of representation' captures what historical representation is about. While 'resemblance' is the key concept and resemblance determines whether a representation represents what is represented in the resemblance theory, the central idea in the substitution theory is that a representation *makes* the represented *present again* – it *re*-presents it. The same conclusion seems to emerge, if one analyzes what the term 'representation' implies: 'The etymological meaning of the word "representation" already compels us to [consider the notion of "presence"]...representation is a making present of...something that is absent' (Ankersmit 2012, 157). And Ankersmit's intriguing claim is that such re-presentation can be achieved via the historian's writing: 'the past is categorically absent from the present, but it can be made present again by means of a textual representation of it by the historian' (Ankersmit 2012, 57, 159).

The substitution theory tries to achieve by means of identity what the resemblance theory attempts to do in terms of resemblance (Ankersmit 1998, 52). When we compare a representation and what it allegedly re-presents we compare a 'thing' to another 'thing', and not a linguistic entity to something non-linguistic as in the resemblance theory. This is to say that both representation and what it represents belong to the same ontological category; we replace a thing (the past) by another thing (the representation) (Ankersmit 1998, 48–52). Ankersmit writes that 'Since what is being represented is part of reality, the same must be true of its representation...*the ontological status of being part of reality is...transferred from the represented to its representation*' (2012, 56). An example of how this kind of substitution might work is the case of iconoclasts, initially

introduced by Baudrillard. Iconoclasts feared that the worshipping of God is transferred to the reverence of the simulacra or image of him, because God is invisible and unreachable. Similarly, historical reality itself is beyond our reach and therefore appears to be substituted and known only by its representations.

Nevertheless, the idea that a historian's text would make the past itself present may stretch credulity in the minds of some. Would a book about the Holocaust really bring the Holocaust to the reader? My point is not to deny that some books of history may succeed in moving the reader emotionally and give the reader *a* sensation of what life in a concentration camp might have been like. Yet, while this may (in the absence of the past itself) be called substitution, the substitute is arguably not the same thing as that which it is a substitute for. The actualized past when it happened was something real and tangible, and not a sensation. To put it bluntly, it is quite a different thing to read about the Holocaust in one's armchair, no matter how moving the account is, than to live through the Holocaust.

Now, Ankersmit's latest articulation of his theory of representation develops the substitution theory further. The idea that something substitutes, or stands for, something else is the same, but Ankersmit feels the need to specify what it is that substitutes that which is absent (see 2012, 78). He introduces a new concept, 'aspect', and a new tri-partite account of representation. First we have a *representation* (such as a text), which offers us the *aspect* or the *presented*[4] of *represented reality.* The crux of the matter is that one should not equate what a representation represents with its object in the world. If one were to think of a representation of Napoleon by historian X, we should say that the historian is not representing Napoleon himself, but presenting an aspect of him, which is specified in the book.[5] Perhaps Napoleon is presented as a heroic soldier or as an arrogant ruler. And the same goes for paintings. The portrait painter's representation gives some aspect of a person, not the person him- or herself, and the aspect or presented functions as a substitute for the actual object.

Ankersmit claims that knowing a text's or a painting's presentation – what they are about or what their 'presenteds' are – is an absolutely fundamental requirement for understanding its meaning (2012, 53). In this sense, he says, aesthetics is logically prior to hermeneutics. He even talks of 'aesthetic truth' after admitting that there is something like historical knowledge and historical truth (2012, 59). One line of his argumentation connects to sublimity and historical experience. Because the substitution theory requires making the past present in the form of a

presented or an aspect, we can see how the notion of presence is related to these two concepts. If the historical text really succeeds in its task, then the reader is experiencing historical reality although the reader is not in the past. However, here this theme will be largely sidestepped, although it will later require some further remarks, when I discuss the concept of truth.

Constructivism

Nothing could thus be clearer than that the narrativists reject the picture or copy theory of representation. The bad news is that we stand to lose a great deal in terms of historiographic epistemology. Namely, if the past was like the photographer's object waiting there to be immortalized in a representation, one could speak of discovering it and capturing it as it (really) is or was.[6] All that one needed to do in that case is ensure the accuracy of our depiction.

Unfortunately there is no 'discovering' the past. A more appropriate metaphor here is that of 'construction', as becomes clear in the following:

> The 'historical landscape' is not *given* to the historian; he has to *construct* it. The narration is not the projection of a historical landscape or of some historical machinery, the past is only *constituted* in the narratio. The structure of the narratio is a structure *lent* to or *pressed* on the past and not the reflection of a kindred structure objectively present in the past itself. (Ankersmit 1983, 81)

There is more than one possible reading available concerning the sense of saying that the 'historical landscape' is not given and requires construction: sociological, epistemological, and metaphysical.

The first option deals with actual historiographical practice. The past as a 'historical landscape' is not available to the historian, as mountains and seashores are for the cartographer. One cannot compare and model one's representation to any tangible and observable object. For this reason, the historian has no other option but to construct a *narratio* in the most concrete terms. Historiography is thus in a trivial sense a constructivist endeavor as opposed to research that discovers or finds that which exists there prior to any investigation. This is the sense in which the historian constructs a *narratio*, which is 'lent' or 'pressed' onto the past, and we might say that only then does the past become intelligible.

I call this first reading of constructivism *sociological* in the sense that it relies on an (arguably correct) observation of the constructivist nature of *historiographical practice*. If we were to send a sociological observer, the kind of figure in Latour and Woolgar's (1979) *Laboratory Life: The Construction of Scientific Facts*, to observe how historians work, the observer would certainly not find that they copy and compare their representations to some pre-modeled past in front of them. Indeed, it is safe to assume that not even the most committed narrative realist would object to sociological constructivism. Realists are prone to common sense views and it would just be too fanciful to suggest that the ready-structured past somehow floats into view in front of the historian's eyes, in order that it can then be mimicked in a representation.

However, realists are likely to try to turn this state of affairs to their advantage. An example of how this could be done is Geoffrey Elton, a dominant figure together with E. H. Carr for decades in seminars on the theory and methodology of history and historiography. Elton's point is that the historian's research object, the past, is strictly independent of the historian's inquiry and therefore unmodifiable. In this sense, historiography is in a better position than many natural sciences! For example, in biomedical sciences scientists interfere with their research object and thereby jeopardize its independence of them. Elton writes, 'Just because historical matter is in the past, is gone, irrecoverable and unrepeatable, its objective reality is guaranteed: it is beyond being altered for any purpose whatsoever' (1967, 53–54). Practical difficulties in reconstructing the past may boil down to the uncertainty and the insufficiency of evidence in establishing facts. For Elton there is always truth to be discovered although not necessarily always to be found in practice.

This discussion has brought us into the domain of historiographic *epistemology*: how can we acquire historical knowledge? Given that the past is not accessible in any direct and unmediated way, perhaps one could nevertheless reconstruct it and its meaning with the help of traces left from the past? Indeed, Elton and historical realists imply that we can bridge the gap between historians and the past they investigate. Further, we might say that much theoretical discussion in modern historiography has focused on formulating methodological advice on how to deal with source material, that is, what kind of source criticism would guarantee epistemically warranted – and even beyond this, true – conclusions. Elton writes that the 'Historical method is no more than a recognized and tested way of extracting from what the past has left the true facts and events of that past' (1967, 65). And it is worth remembering Ranke's injunction that historians should extinguish themselves,

in order to prevent their subjective beliefs and assumptions from being projected onto their reconstructions of the past. It is much better on this view to let the facts found in documents speak for themselves.[7]

The narrativists represent the opposite end to modernist historiographers, at least when it comes to narratives and other synthesizing interpretations. Using Ankersmit's terminology, we might say that a narrative realist, like Elton, believes that there is something akin to 'translation rules'[8] that govern the relationship between the past as given and the representation of the past as represented by the historian. Most admit that there are no direct rules of correspondence, but many insist that indirect rules, such as appropriate reading of source materials, exist. By contrast, White abandoned the narrative realists' attempt to understand the past itself and their belief that there are such translation rules that show what historical reality really is like. In White's theory, his famous four tropes (Metaphor, Metonymy, Synecdoche and Irony) assume the role of translation rules, which are culturally embedded in our way of making sense of the past. However, they do not reveal the true nature and shape of historical reality, but function like a Kantian 'transcendental deduction'; in this sense, the tropes are needed to make historical knowledge possible in the first place. Ankersmit commits to the same idea of the transcendental limit of historical knowing. The difference is that he is clearer that the translation rules in this sense are not proper translation rules of the past at all. They reveal to us, not what the past is like, but only show the logical structure of the narrative accounts of the past (Ankersmit 1983, 77). Ankersmit writes that 'Whatever concrete content we may give to the translation rules, they will never be more than *arbitrary* selection rules, acceptable to some historians but to be rejected by others' (1983, 81; my emphasis). Or simply, 'there are no translation rules' (1983, 87; similarly 216, 226).

The statement that 'there are no translation rules' is a clear denial in the context of epistemology that one could have (even) indirect epistemic access to the past, which would show it *wie es eigentlich gewesen*. However, the debate between the realist and the idealist does not stop on the epistemic level either, which is thus the second sense in which the 'historical landscape' is not given and has to be constructed by the historian. The quotation above (p. 37) continues as follows:

> The past is by no means like a machine: it does not possess some hidden mechanism whose working the historian has to trace. Nor is the past like a landscape that has to be projected onto the linguistic level with the help of projection or translation rules. ... We should

> reject "the idea that there is a determinate historical actuality, the
> complex referent of all our narratives of 'what actually happened', the
> untold story to which narrative histories approximate" (Mink). ... All
> this means that the past as such has no narrative structure – narrative
> structures occur only in the narratio. (Ankersmit 1983, 81)

Because there is no narrative structure or any other 'untold story' in
the past, there is nothing to tell and nothing to discover, even if we
had the 'access'. The past only becomes narratively structured through
the imagination and the hand of the historian, who imposes order and
meaning there.

It is notable that both White and Ankersmit assume that the indi-
viduation of historical facts is unproblematic, an example of which is a
chronicle. As the latter expresses it, the correspondence between indi-
vidual statements of a narratio and historical reality is 'beyond doubt'.
Ankersmit writes that 'Saying *true* things about the past is easy [on the
level of individual statements] – anybody can do that' (Ankersmit 1990,
278).[9] But everything is different with historical texts. There is no story
'within the welter of facts' (White 1975, 59). 'The sets of relationships'
that the historians postulate are not 'immanent in the events them-
selves; they exist only in the mind of the historian reflecting on them'
(White 1978, 94). Yet more pointedly, no historical event is intrinsically
tragic, comic, and so on (cf. White 1978, 84). We can understand White
here, first, making a negative point about historical reality. Whatever
the past is, it does not inherently possess any narrative or story forms in
which historians present their accounts. This is a comparable position
to Ankersmit's narrative idealist, who thinks that 'the past as such has
no narrative structure'.

But there are some indications that both go yet further than this and
maintain that the past has *no structure whatsoever*. The most obvious
point of reference is the theoretical chapters in *Metahistory*, where
White writes that the historian has to prefigure the field of investi-
gation before it can be a possible object of knowledge. Prior to that
constitutive act, neither objects nor the relationships between them are
constituted. This also is the reading of White that Ankersmit builds on
in his writings. According to Ankersmit, White understood the past as
'a meaningless myriad of facts, states and events, an amorphous chaos
of data' (1983, 78), which is an idea that re-appears in Ankersmit's later
books. He writes that historical reality remains a chaos as long as a
representation has been singled out to bring order into this chaos (2001,
45) and that the historians' concepts create continuity and unity in the

field that is in the state of chaos and disorder before their postulation (2012, 45).

The stronger interpretation of their position is thus that White and Ankersmit not only state that the past is not narratively structured but also deny historical reality any inherent order or structure whatsoever. Ankersmit's words, above, that there is no 'determinate historical actuality' at all might be read in this light. This is to claim that they take a metaphysical stand on the nature of reality, which could perhaps be described as nominalism.[10] Perhaps White and Ankersmit should be understood as advocating the unorthodox form of nominalism, something like that which Ian Hacking brings forward in his *The Social Construction of What?* Hacking uses the term 'nominalism' to individuate a position that rejects, above all, that nature has any given natural kind *structures*. Hacking calls the opposite view 'inherent-structurism', that is, the belief that the world comes with an inherent structure (Hacking 2001, 82–84).[11] The writings of both undoubtedly contain ingredients for this kind of interpretation, but one should note that the idea of the past as chaos is related to the historian's practice. This suggests a slightly different reading, which deals not with the nature of historical reality as such but with the historian's role in making sense of this reality, saying that whatever the true nature of reality is, for us, and for historians, it *appears* chaotic before we order it with our concepts and narratives. In other words, Ankersmit and White may be talking about the 'chaotic' order of historiographical data and not about the past as such.

The claim that narratives are not found in the past but imposed on it reveals yet another fundamental aspect of narratives in the narrativist philosophy of historiography. White has famously stated that historical narratives are 'verbal fictions, the contents of which are as much *invented* as found' (1978, 82) and that the historical work is 'a verbal structure in the form of a narrative prose discourse' (White, 1973b, 2). White used these remarks to justify his comparison of historiography to literature. Further, White also writes that the qualities of narratives are only something that people seek and value, and that they do not have any correspondence in the past:

> This value attached to narrativity in the representation of real events arises out of a desire to have real events display the coherence, integrity, fullness, and closure of an image of life that is and can only be imaginary. The notion that sequences of real events possess the formal attributes of the stories we tell about imaginary events could only have its origin in wishes, daydreams, reveries. Does the world really present

itself to perception in the form of well-made stories with central subjects, proper beginning, middles, and ends, and a coherence that permits us to see 'the end' in every beginning? (White 1987, 24)

Here emerges the position that Ankersmit has expressed philosophically in less ambivalent terms: 'Given this *morphological* or structural difference between the past and the narratio, how can translation rules ever be expected to link them together? Projection or translation rules can exist only where there are two corresponding spheres of structural similarity' (1983, 82; my emphasis). Ankersmit refers in this context to Poincaré, who said that we can compare one clock to another, but not to 'time' itself, because there is no time as such. The same reasoning is said to apply to historical narratives. 'We cannot glimpse at history. We can only compare one book with another book' (1983, 81–82).[12]

In other words, 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. We come to the conclusion that constructivism is not a forced option merely because of practical problems of copying the past, either sociological or epistemic, but due to a yet deeper, metaphysical, philosophical problem. White and Ankersmit wish to point out that whatever reality is like, the features of historical representations are not part of it. This is what Ankersmit's and White's constructivism fundamentally boils down to.

The gap between us and the past world is bridged by the historian's representations, which unfortunately results in the morphological divergence between the representation and the represented. This takes us directly to a common theme underlying White's and Ankersmit's philosophy, already briefly mentioned earlier in this book: Kantianism.

In the last chapter we saw how Ankersmit understands both philosophy of language in general and White's and his own early scholarly works as subprojects in the larger Kantian program. All philosophies of language allegedly have in common the idea that language is the principal condition for the possibility of all knowledge and meaningful thinking (1994, 2; similarly 2008b, 84). In White, tropes function like Kantian categories of understanding, as preconditions of meaningful historical knowledge.[13] And in the early Ankersmit, this view comes

down to a conviction that it is the metaphor that organizes historical knowledge and makes the unfamiliar familiar.

More specifically, in *Narrative Logic* Ankersmit made it clear that his approach in that book resembles Kantian philosophy in that it aspires to answer the question of how narrative knowledge of historical reality is possible. He says that it is an attempt to develop 'a Critique of Historical Reason', an obvious reference to Kant's *Critique of Pure Reason*.[14] And although Ankersmit indicated that his intention was to move beyond Kantianism in *History and Tropology* (see chapter 2), he remains Kantian also in his later publications. In *Meaning, Truth, and Reference in Historical Representation*, Ankersmit again tries to resist the Kantian discourse, but succumbs to it under its appeal and pragmatic worth: 'I would rather avoid transcendentalist vocabulary … this nevertheless is to a large extent the view inspiring the remainder of the book' (2012, 47).

Further, Ankersmit writes that historians, in their belief of finding correspondence between the past and historical language, are blinded 'to the fact that the unity and continuity supplied by historical language is *the transcendental condition* for the possibility of historical knowledge' (Ankersmit 2012, 46; my emphasis). Their poor vision might lead some, such as the French *Annales* historians, to prefer an 'incoherent raw mass of information' to more developed and succinct accounts (2012, 46). But it is the historical representation as a Kantian transcendental condition that in Ankersmit's view brings order into chaos: outside historical representation there is no historical writing (2012, 46).

Finally, it is worth remembering how White, cited above, claimed that no historical event is intrinsically tragic. The story form encodes the facts of history into an intelligible narrative. Consistency, coherence, and 'illuminative power' as qualities of the historical account stem from the historian's vision and choice of the mode of presentation (1973b, 4). The same set of story elements that looks tragic from one point of view (through one story form) may be comic from another. Michelet construed the French Revolution as a drama of Romantic transcendence, but Tocqueville emplotted the same phenomenon as an ironic tragedy. White even calls the potential elements of story value neutral; meaningless events in serial order that are transformed through plot structures into meaningful structured stories (White 1978, 84–85; 1987, 44; 1975, 59). His words have a familiar Kantian ring: 'The implication is that historians constitute their subjects as possible objects of narrative representation by the very language they use to describe them' (1978, 95).

The status of historical accounts as 'possible modes of historical representation or conceptualization does not depend upon the nature of the

"data" they used to support their generalizations' (1973b, 4). Indeed, White thinks that the mode of presentation is an a priori choice, not to be judged empirically at all. These modes are reconceptualizations that make historical narrative and knowledge possible, without which historical knowledge would remain in the state of an incoherent heap of data. Story forms or tropes thus add something to the past that does not exist independently there prior to the historian's creative act (cf. White 2011 395, 397). And, because there is in his view a limited set of story modes available to the historian, the historian imposes on historical reality not only one's own personal vision but also a (Western) culturally conditioned encoding more generally.

Holism

Now we arrive at the third tenet of narrativism: holism. The natural starting point here is the narrativist demand to treat and study works of history as wholes, that is, not to cherry-pick historical claims from a historiographical text, but to try to understand what kind of story, message or thesis a work of history as a whole amounts to. White emphasized that historians produce unified texts, and *Metahistory* is a case in point. In it, many classics of Western historiography and philosophy of history receive a comprehensive reading in an attempt to uncover what tropes and story modes govern them as whole texts. White's talk of the structures of texts and the relationships between story elements is also symptomatic. Cutting stories or narratives into smaller pieces, except as an analytical exercise, means that they lose their primary identity and communicative function.

   The same idea of historical texts and theses being distinct and qualitatively dissimilar from the elements out of which they are composed characterizes Ankersmit's thinking. 'History and historical debate is holistic in that the universally shared assumption in historical writing is that only the whole of the text conveys the historian's cognitivist message, and to which the parts only contribute', writes Ankersmit (2008b, 92; similarly 2012, 159). But while White is content with a structural examination of texts, Ankersmit analyzes their philosophical status at length. In *Narrative Logic*, Ankersmit makes an important link between three closely related theoretical notions: images/pictures (of the past), colligatory concepts, and narrative substances. *Narratios* or narratives thus amount to images or pictures of the past that a historical text articulates (conceived of as proposals about how to view the past and not as being copies of the *actual past*). Ankersmit writes that the term 'colligatory

concept' is well suited to replace the terms 'image' or 'picture' of the past, but that it in turn is subsequently replaced by an even more fitting term: 'narrative substance' (1983, 93). Although the concept narrative substance came to be replaced by other notions in Ankersmit's later writings, the idea of the holistic nature of historiography remains the same. In what follows, I characterize narrative substances in more detail in order to understand the narrativist view of the holistic nature of historiography.

The first important observation is that there are two kinds of narrative entities that need to be kept separate: narrative *subjects* and narrative *substances*. The previous are an integral part of the narrative realist's world-view, and also accepted by the narrative idealist, while the latter belong only to the narrative idealist's universe. Suppose we have a biography of Napoleon, containing a large number of statements concerning him and his life. The narrative subject in this case is the historical Napoleon, the person who lived between 1769 and 1821 and became the emperor of France. Ankersmit's narrative idealist sees that, *in addition* to the narrative subject, there is also a narrative substance, which is the '*image*' or '*picture*' that the author presents of the life of the historical Napoleon, and which is an outcome of all individual statements of the *narratio combined* (Ankersmit 1983, 90; cf. 2001, 61).

The difference between narrative subjects and narrative substances is like flipping the perspective on individual statements of a *narratio*. If the *narratio* is seen merely as an enumeration of singular statements, then we are talking about narrative subjects and derivatively of 'real' entities in the past to which these statements refer. But if we view the *narratio* not just a list of these statements but as the *totality* that these statements altogether *amount to*, and importantly, as a specifically ordered set, then we are talking about narrative substances.

If the claim is that 'Napoleon conquered Russia', the primary function in historiography is to make a particular statement about the past, that is, that Napoleon conquered Russia. In principle, it seems that we can decide whether this is true or false. Its truth requires that there was a person called 'Napoleon' and that this Napoleon actually conquered Russia. To the best of my knowledge, the French Emperor Napoleon did not succeed in his attempt to conquer Russia, and therefore, the statement is false. In its second function, this very same statement contributes to the image of Napoleon that is detailed in a work of history. The statement is part of the *whole* picture provided (and a 'property' of a respective narrative substance). Ankersmit remarks that it would be easier to differentiate which entity is in question if we were to reserve

the name 'Napoleon' for the historic Napoleon and distinguish it from images offered by historians by using such locutions as, 'my Napoleon' or 'the Napoleon of the historian H' (1983, 95). And the following would be even better expression: 'Louis XIVh_1, narr', 'Renaissance h_2', 'the emergence of a new social élite in the 19th century h_3', and so on, where 'h_1', 'h_2', and 'h_3' denote different historians' interpretations or different works by those historians. There is also an addition 'narr' in the case of Louis XIV in order not to confuse the narrative substance with a narrative subject Louis XIV, as otherwise the danger is that we multiply the number of historical Louises (1983, 124–125).

Further, narrative language is self-referential. The name the 'Cold War', for example, refers to the the Cold War$_{Ns}$,[15] which expresses the narrative meaning of the whole but is composed of a set of statements. If the narrative substance itself is seen as a name of something else, it refers to that set of statements it is composed of, that is, to itself. Further, it is necessary to express the narrative meaning in the form of 'N_1 is p...N_1 is p_n', and not just as a sequence of individual statements 'p_1...p_n', because otherwise one misses the most important feature of historical texts, which is that they bring forward some general picture or thesis of the past, identified as narrative substance and here expressed as 'N_1'. We thus construct this new *holistic entity*, narrative substance, in order to see and recognize this aspect of historiography (see Ankersmit 1990, 278–281).

Ankersmit writes: 'The thesis that all statements expressing the properties of Nss are analytical is, perhaps, the most fundamental theorem in narrative logic' (1983, 127; similarly 1988, 220; also 1995b, 225–226).[16] In light of what has been said of narrative substances as holistic entities, this thesis is now entirely understandable. All the parts of a whole belong to it inherently; we might say that they are necessarily parts of it, and are true of the entity due to this membership alone. It would be possible to express the analyticity of narrative substances as follows. If a specific narrative substance N_1 is composed of the set of statements, s_1, s_2, and s_3, then to say, for example, that 'N_1 is s_1' does not bring any new information, but is necessarily true due to the definition of this narrative substance. The requirement to make a complete enumeration of all properties of Ns as the only way to individuate (e.g., 1983, 110) can also be explained by the idea that narrative substance is a whole composed of all its parts. And as a further consequence, the identity of an analytically defined or true entity requires that all parts remain unchanged: 'Whichever Ns we may choose, none could ever be different from what it is, without ceasing to be the Ns it is' (1983, 213). If there is any change

in its constituents, it will be a different entity: 'as soon as one statement is omitted or added we have to do with a different Ns' (1983, 213).

A consequence of this position is that a narrative cannot misdescribe its object, such as 'Renaissance', because it creates it: Renaissance$_{Ns}$ 'is nothing more and nothing less than what individual historians tell us that it is' (1983, 201). And there is, for example, no such thing as the 'Fall of the Roman Empire', strictly speaking. Narrative substances are not 'shorthands' that enable us to speak about things in historical reality. As a consequence, narrative historical knowledge is not knowledge proper, but 'an *arrangement* of knowledge' (1983, 227). There is no fact of the matter regarding what the 'Fall of the Roman Empire' is, but its identity is totally up to the historian's stipulation.

Although there is a gradual terminological change from 'narrative substances' to 'representations', all the elements of holism are still an integral part of the later Ankersmit's philosophy. In his latest writing, holism is applied to 'representation' in the three-place representation schema of representation, presented and represented reality. In general, one may say that three central features characterize holism in the narrativist philosophy of historiography: *undecomposability*, *analyticity* and *unfalsifiability*.

Undecomposability means that an entity cannot be decomposed into its constituent parts without losing its identity. Undecomposability can be derived from the meaning of 'whole' or 'holism' itself, but is seen also in claims like '*none* of the statements which constitute the text is…irrelevant to the text's presentation of the past' (1995b, 225; my emphasis) or earlier, in the one pointing out how no statement can be omitted. It amounts in the later writings to the idea that one cannot make a distinction between the attribution of predicates and reference, an example of which was the image of a black cat above, where 'cat' and 'is black' constitute one unified representation. Or compare the following view of portraits: 'We do not experience it as a composite of bits of information about hair, nose, color, form of the eyes, etc. (all of them corresponding to statements about the sitter's hair, nose, and so on) but rather as a representational whole' (2012, 98).

Analyticity is a very consequential feature of historical representations: all that can be said of representations derive from their definitions. If claims are true, they are necessarily so, because they constitute the very meaning of those representations. For example, think about historians writing about 'Renaissance'. They all come up with a different definition of it, and as a consequence, all that they say about their respective 'Renaissances' is analytically true of them and can be deduced from their

meanings: '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…what is then said about fourteenth- and fifteenth-century Italian civilization is, admittedly true *by definition* – but true it is' (Ankersmit 2001, 38).

We notice then that each representation is strictly unique, like an individual person. In actuality, representations are even more unique than human beings. An individual could lose a hand or a leg and still be the same person. By contrast, if any part of a holistic entity is changed or removed, it is necessary to talk about a different object altogether, about a different representation.

The third characterizing feature of holism is *unfalsifiability.* Given that representations are wholes, one cannot try to falsify or corroborate any part of the narrative but instead needs to focus on entire narratives. But narratives are definitionally and analytically true, which means that they are immune to any empirical challenges. The classic example of an analytically true sentence in philosophy is 'A bachelor is an unmarried man'. One cannot possibly falsify this linguistic stipulation by empirical information on the numbers of married and unmarried men.[17] It is naturally possible to come up with an alternative definition, but this has nothing to do with empirical standing. Whenever linguistic definitions, constituent parts of representations, are changed for whatever reason, a new entity is defined and created, but the old one is not falsified. Alternatively, its epistemic status remains unchanged. Compare White's words on this: 'history progresses by the production of classics, the nature of which is such that they cannot be disconfirmed or negated.…It is their nondisconfirmability that testifies to the essentially *literary* nature of historical classics' (1978, 89). This has to be so, because the classics create their own 'worlds' and their own analytically true linguistic creations. How could anyone hope to corroborate or falsify novels? From a logical point of view the message is that historical 'facts' do not yield precedence to any specific narrative or to any narrative form. They are all equally justified and immune to falsification. Further, representations cannot be compared to each other in terms of their empirical adequacy.[18] That is, since they are autonomous and 'analytic entities', they cannot be ranked in terms of their fit with empirical evidence. There may be of course other criteria with which they could be ordered, such as their originality or aesthetic/literary appeals, but empirical adequacy is not one of them.

My discussion on holism later in this book (Chapter 5) focuses on the question: how feasible is the assumption that historical theses cannot

be undecomposed? That is, I will examine some such theses and their contexts of origin and see whether they could be seen as composable. In this chapter, I hope to have shown that three concepts: representationalism, constructivism, and holism characterize and define the philosophy of narrativism. In the following chapter, I concentrate on evaluating the tenability of one of them: representationalism.

4
Representationalism and Non-representationalism

What kind of a subject is historiography? Is it a science, an art form, a craft or a unique practice of its own kind? And what is the point of doing historiography? These questions are important because the answers in part determine what historians should aim at producing and achieving. Philosophers and theoreticians of history and historiography have given various replies, although two have undoubtedly dominated the discourse. The majority of interlocutors have seen historiography either as a science or as a form of art. Both answers are problematic in some sense, and most of the debate has consequently focused on figuring out a solution to the problems identified. I begin this chapter with a brief outlook at some of the answers and solutions that have been provided. This exposition is far from exhaustive. It is illustrative and designed to pave the way for an analysis of representationalism in the narrativist philosophy of historiography and, subsequently, for outlining a non-representationalist account of historiography.

Art or science?

In his paper 'History Brought under the General concept of Art', Benedetto Croce defended the view that historiography is art, in contrast, for example, to his contemporary Johann Droysen, who thought that historiography was definitely a science. The fundamental problem was that historiography had typically been assumed to study particular individuals, singular facts or particulars. Croce writes that, by contrast, science investigates types or 'the general – what exists in all the individual objects' (Croce 2012, 492). As a result, historiography as a science would be a science of the individual, which would seem to entail a contradiction, as Schopenhauer had put it. Indeed, it is the subsumption

under the general that often defined science and the representation of the particular that characterized art in nineteenth-century discourse.

Croce writes that while some forms of art represent 'the possible', 'history may be defined as that type of artistic production which has the real event as the object of its representation' (Croce 2012, 500). Perhaps surprisingly for the modern reader, Croce thinks that both artists and historians aim at accurate representation and work in the spirit of observation: 'as the artist cannot lapse into the false, so the historian cannot lapse into the imaginary' (Croce 2012, 500). The only rationale for historiography is 'to tell the facts', and telling the facts means narrating. In passing it may be noted that, for this reason, some authors had suggested that historiography was a special kind of non-explanatory science, that is, a descriptive science (Croce 2012, 492) – but for Croce historiography was a special kind of art that deals with what is real and of historical interest.

In accordance with Croce's view, Daston and Galison (2010) show in their book *Objectivity* that subjectivity has not always been seen as an obstacle to acquiring truth in science. The norm that guided scientific investigation in the eighteenth-century, which the authors call 'truth-to-nature', required the scientist's (subjective) eye and the artist's (subjective) skill to discern the true essence of natural objects without any of the deficiencies of the actual perceivable things – to correct nature's imperfect specimens. This requirement is similar to the demands on Croce's historian, although such a historian is faced with a yet more challenging situation in the sense that constructing 'a complete narrative' is, by Croce's own admission, only rarely achievable, yet represents an ideal that the historian nevertheless has to try to reach.

While Ranke may have emphasized the subjective, intuitive skills of the historian more than has generally been realized, the philosophical school *Rankeanism* stressed the need to remove subjectivity entirely from historical narration.[1] The historian was asked to 'extinguish oneself' in order to pin down *wie es eigentlich gewesen,* or 'how it really was'. Rankean historiography may also be dubbed a scientific historiography[2], which relied on the idea that by using critical source methods it is possible to acquire an objective and true history. 'Objectivity' here means neutrality, and thus excludes any literary, poetic and other speculative elements from the historian's narrative. One might say that, in the Rankean paradigm and in its implied historical realism, the rationale of historiography derives directly from its epistemological standing. To acquire knowledge and truths about the world amounts to a self-justified rationale for historiographical practice.

G. R. Elton is an exemplary modern advocate of scientific historiography and historical realism. He writes that 'like all sciences, history, to be worthy of itself and beyond itself, must concentrate on one thing: the search for truth' (Elton 2002, 44). That is, the search for truth, is the whole and self-evident point of doing history. Elton also believed that historical method provides a means of 'extracting from what the past has left the true facts and events of the past' (Elton 2002, 59). Provocatively he states that the historian can only 'discover', and in this way prevent the tendencies of the observer and 'experimenter' being reflected upon the subject matter (Elton 2002, 49). Or even closer to the modern day, we may listen to Arthur Marwick, who says that historians work in the same spirit as natural scientists ('always working from the evidence, always basing their generalizations, interpretations, or theses on the evidence (not on metaphysical speculation)'): 'history should be judged as a scientific activity' (Marwick 2001, 248, 249).

The previous chapter demonstrated how the narrativist philosophy of historiography questioned historical and narrative realism, something present in one form or another in all of the above attitudes. The narrativists specifically dispute that the narratives of complete works of history could, even in principle, be true and that the truth-making qualities of narrative could be 'discovered', because the past does not possess such properties. It follows that the function of history cannot be anything like 'accurate representation' or the 'search for truth' with regard to its central scholarly products. The narrativists also likened historiography to art, arguing initially that historiography resembles literature and subsequently that it should be compared to visual art. This suggests that the worth and the rationale of historiography is to create artistic products with aesthetic value, without any entailment of their reality or truthfulness.

Now, the postulation that historiography is art, or science, or something else, is one thing, and the actual possibility of creating the kinds of entities produced in these disciplines is quite another. For example, one may wish that historiography be a science and that it produce true narratives. But if it so happens that a 'true narrative' is an unattainable goal, as the narrativists have argued, then attaining the status of science on this basis becomes an impossible desire. The narrativists have stated that historiography necessarily creates representations. Commitment to the notion of representation and representationalism is interesting because it cuts across various philosophical orientations. Croce wrote about representations. And similarly to the anti-realist narrativists, historical realists typically think that historiography creates representations, the

difference being that they believe those representations can represent the past as it was. In the previous chapter, I began the analysis of what historical representations are representations of, provided that they are not 'copies of the past'. Now this investigation continues. How tenable is the narrativist theory of representation? And how necessary is it for historiography?

The problems of representationalism

In this section, my aim is to discuss some of the problematic consequences that representationalism and the substitution theory of representations are associated with. In the subsequent section my claim is that we can construe an alternative suggestion, which manages to give a satisfactory account of historiography and its main knowledge contributions, but which avoids the problems of representationalism.

Before commencing a detailed critique, it is instructive to take a few steps back and consider the concept of 'representation' in general. What does it mean? On the most fundamental level, 'representation' is a two-place relation, creating a link between two variables: one that represents the other that is thus represented. There are several ways in which to understand this relation as we have already seen. The following are arguably the most obvious ones: (1) 'representation' can imply that one object is a copy of another in some sense; (2) 'representation' can be taken to mean that one object is a substitution of another; (3) 'representation' can be understood yet more widely to state that one object stands for or symbolizes another even though the former is not a replacement of the latter. In the case of historiography, the idea that a historical text could be a copy has been dismissed by the narrativist school. I concur with this. The text seems to have qualities and structures that historical reality lacks, something that White emphasized (White 1984, 24–259). Ankersmit aptly spoke about a 'morphological or structural difference' between the past and a narration (1983, 82).

Ankersmit's and White's argument that representation in the form of resemblance and morphological similarity does not work is well in tune with recent debates in the philosophy of science. One might say that the theories that rely on similarity and isomorphism are indeed the ones that try to capture the celebrated and criticized intuition that A is a representation of a target B if and only if A constitutes a *mirror image* of B, and it appears that the concept of representation in terms of similarity and isomorphism is in deep trouble also more generally in the philosophy of science. Mauricio Suárez (2003) argues that 'no theory

that attempts to reduce scientific representation to similarity or isomorphism will succeed' (2003, 241).[3]

There are of course many suggestions for how the concept of representation should be modified. The pragmatically oriented philosophers share the belief that what seems initially attractive in the similarity and isomorphism accounts, that is, that the representation depends on the facts of the world alone and guarantees in this way the objectivity of scientific representation, is untenable.[4] Their idea is that it is necessary to include the purposes, views or interests of enquirers in the account. Suárez speaks about the 'essential directionality of representation' and suggests that 'a necessary condition for A to represent B is that consideration of A leads an informed and competent inquirer to consider B' (2003, 237).[5] It is notable that even the modified accounts of representation entail a one-to-one relation between the representation and the represented.[6] This is challenging in the case of historiography. If A is a text on the Renaissance, B should be the historical phenomenon Renaissance. The point of the narrativist philosophy of historiography is that that the text constructs the historical phenomenon on the narrative level, which is thus something that cannot be discovered. In this case then, A constitutes B and therefore A can only refer to B, that is, to itself. And further, if B is accepted as a colligatory concept, it is does not seem clear whether it is possible to consider it as an independently existing single entity that can be referred to in the way that a name refers to a person, for instance. Now, it can still be maintained that A, the text, leads to a consideration of B, although A constructs B. Namely, A can lead one to think about events colligated under B, such as some paintings, furniture, ideas or scholars of the period, etc. The crux of the matter is that the text cannot lead to a consideration of the pre-constructed historical phenomenon, because no higher-level phenomenon, such as the Renaissance, that is constructed of lower-level objects is independent of the historian's act. The discussion concerning the nature and reality of constructed phenomena will be left for Chapters 6 and 10, but it can already be said that the reality of either the Renaissance or the events it colligates are not in doubt. Further, the judgment on whether a representation could be isomorphic with or similar to its target object naturally depends on both what is identified as a 'representation' and what is seen to be the 'target object'. The subsequent chapters consider both these questions in detail.

We thus come to Ankersmit's idea that representation is substitution, something that is suggested by the meaning of the word 're-presentation'. His idea differs from that of the pragmatically oriented philosophers

of science. According to Ankersmit, every painting or historical representation contains something unique, some 'aspect(s)' or 'presented(s)', which was also (in *Historical Representation*) called their 'personality'. Further, the postulation of an 'aspect' turns the two-place relation into a three-place relation. It is true that Ankersmit is not alone in suggesting that a simple two-place relation between 'representation' and the world as given fails to explicate the concept of representation adequately. The difference is that while most other suggestions would include something like purposes or agents (scientists) in the relational schema (e.g. Giere 2004), Ankersmit adds an extra object or object-like entity into the equation.

On first sight, Ankersmit's 'presenteds' or 'aspects' appear to be a cunning solution for retaining the representationalist account when it is clear that resemblance, similarity and isomorphic theories of representation fail. Provided that historical representation cannot be directly about the past, the problem is to know what these aspects are that historical representations are about. What kind of entities they are? Are they additional abstract objects in their own right? Does their postulation help us to understand historical texts better than normal interpretational practices?

One line of interpretation suggests that 'presenteds' are indeed independently existing entities and that they reside in their own abstract world. Consider the following paragraphs:

> All objects of interpretation drag along with them the roots they have in the (imagined) reality they represent. (Ankersmit 2012, 52)

> There is always an (imagined) world or reality that representations, whether texts or paintings, are "about" and of which they are more or less "true" (where I take the word "true" here in the vaguest possible way); and whoever interprets texts and paintings without taking this into account will inevitably be like a sailor without a compass. (Ankersmit 2012, 52)

It is worth noting that the reality of the past itself is not at stake here, despite the connotations associated with the word 'imaginary', since historical reality is one part of Ankersmit's tri-partite account. One should thus avoid attaching any psychological connotations to this notion of 'imaginary', If we read these passages at face value, they would seem to suggest that the world of 'presenteds' forms an abstract world of its own. One might see this world as parallel to the realm of Platonic heaven, Fregean propositions or a Popperian Third World, that is, entities

that really exist but in a non-material and an ahistorical form. Perhaps Fregean propositions and concepts provide the best analogy for 'aspects' and 'presenteds' as they also connect linguistic entities and the external world. Frege claimed that one should not assume that our subject terms refer directly to entities in the world or that their meaning is given directly by their external references (in contrast to the causal theorists of reference, such as Kripke (1980) and Putnam (1975)). For it seems entirely possible that the same person believes that the 'evening star' is not the same as the 'morning star', even though both refer to the same object, that is, the planet Venus. Frege thought that the 'senses' (or 'concepts') of these expressions are different although their reference is not. We can make similar observations in the context of belief ascriptions. Jack may well believe that 'Clark Kent didn't save the world' but that 'Superman did save the world' without any contradiction if we assume that there are two different propositions to which these sentences refer and that Jack does not know that Clark Kent is Superman. The world of senses and propositions as a fully disembodied world outside history would seem to help us to make sense of the said between our language and the actual embodied material world.

Now, perhaps Ankersmit can be seen as joining this venerable tradition of the philosophers of language with his suggestion that between textual representations and historical reality there are similar disembodied 'aspects' or 'presenteds'.[7] Frege's suggestion was that these abstract entities are needed to make sense of the said when the content of the said differs, but when the references in the external world remain the same. The appropriate test for this analogy would then be to see whether the postulation of a 'third world' has similar benefits for the intelligibility of our historical discourse. Namely, in the case that two historians refer to the same historical (material) world with different kinds of representations, one could not make sense of a historian's interpretations, the said, without postulating two 'presenteds' as the corresponding abstract entities. I must admit that I find this interpretation interesting. Further, it may perhaps be seen to link to a relatively recent discussion on abstract social ontologies.[8]

However, the 'two-world' suggestion is not without problems either philosophically or as a textual interpretation of Ankersmit. To begin with the philosophical problems: a metaphysical implication is that the postulation of 'aspects' duplicates historical ontology. That is, we would have, first, the world of representations and, second, the world of 'presenteds'. There would thus be two levels and two ontologically separate worlds. On one occasion Ankersmit writes that:

> Each representation, then, carries its own represented or aspect[9] along with itself – much in the way that we are each accompanied by our shadow on a sunny day – and each of these representeds is indissolubly linked to one, and only one, particular representation corresponding to it. (Ankersmit 2012, 72)

In the ontological interpretation, this would seem to mean that we view ourselves and our shadows as objects of equal standing. Therefore, if we think that both 'representation' and 'presenteds' are objectively existing entities, we introduce a two-tier ontology (in addition to actual historical reality, the third tier). It thus seems that the 'being about' postulation entails that there is a (perhaps linguistic) level of representations and that there is a separate level of objects, presenteds that the former are about. Each historiographical representation would give a birth to a new independent abstract object. In brief, the 'being about' stipulation forces us to duplicate and inflate our ontology.

The upshot is that although the substitution theory of 'representation' is a theory about substitution, or the identity[10] of the past and present, it unwittingly suggests some kind of correspondence relation between subject and object, that is, between the one that re-presents (such as language, text or 'representation') and the one that is re-presented. The analogy between bodies and shadows mentioned above entails that there should be such a relation of correspondence. This suggestion is perfectly intelligible when we pause to consider this relation. A shadow can be expected to be isomorphic with the body; it reflects the two hands, one head, two legs, etc. that the body possesses. This analysis provides us another view onto the question of how and why the *language of 'representations'* inadvertently returns us to the realist 'language game' of mirroring, even though the world of objects mirrored is not the 'real' human-independent world of the realist but the imaginary world of historiographers.[11] An ontological interpretation of the abstract world would commit us to some kind of realism in idealism: it requires one to postulate a real world – albeit that this real world is dependent on the human mind and its creations, at least until they are created.[12] Perhaps, after their creation, 'presenteds' do gain independence from the human mind but even then they would not exist in material or physical form but as a world of thought products.

It may be necessary to simply bite the bullet here despite such metaphysical complications if the postulation of an extra realm is required to make sense of historiography and its content. In this case one would expect clear practical gains from the postulation. I leave the discussion

of the pragmatic value for the following section. Now it is necessary to return to the textual interpretation of Ankersmit's philosophy and ask whether he actually commits himself to the existence of an abstract independently existing (imaginary) world of 'presenteds'. Some of the passages displayed above seem to suggest so, but in actuality Ankersmit denies this and, in the end, puts forward a 'one-world solution'. He states that 'aspects' or 'presenteds' have the same ontological status as past reality itself: 'a representation's presented is an aspect of things and hence part of the world itself' (Ankersmit, 2012, 105). Ankersmit's view that they belong to the same 'inventory of the world' and that there is no 'ontological hierarchy' was already mentioned earlier (p. 48; Ankersmit 1998, 50). An analogy that clarifies the status and meaning of 'aspects' is that of the front and back of a person, which are both part of one and the same person.[13] It is striking that this suggestion entails the cumulative nature of historical knowledge. If all historical books, say, about Napoleon present us with an aspect of Napoleon, can we think that all the aspects increase our knowledge of Napoleon in such a way that we gradually move toward a more complete picture of Napoleon? The 'complete picture' can of course be understood as an ideal limit, perhaps never actually reached. Nevertheless, there would thus seem to be genuine accumulation of historical knowledge in the form of collecting more aspects or presenteds of some historical phenomenon. Some expressions of the later Ankersmit indicate that this is indeed possible. Note the following illustration concerning the role of aspects through reference to a geographical discovery: 'Think of the first explorers of the American continent in the early 1500s: each of them discovered only an aspect of the continent, but then mapmakers pulled all their individual discoveries together; only then did a new thing come into being, namely, the American continent' (2012, 155). Might one thus view historical objects as undiscovered continents that we come to understand more and more in the course of time and by virtue of diligent exploration? Ankersmit says that 'nobody will doubt that there is progress in the discipline of historical writing: we know far more about the past than ever before'. Even more revealingly, he indicates that one day we might reach 'a universal consensus' on historical phenomena such as the Renaissance. This would be the situation in which 'the historical (propositional) truth about the Renaissance would have been discovered' (2012, 84–85, 228).

The prospect of the accumulation of narrative knowledge in this sense is surprising and in stark contrast with what Ankersmit argued in his earlier texts. At the time when Ankersmit still identified himself as a

'postmodernist', he compared the increasing pace with which historical interpretations are produced to 'intellectual alcoholism' (1989, 138) and agreed with Jonathan Culler's (1983, 90) statement that in historiography, 'paradoxically, the more powerful and authoritative an interpretation, the more writing it generates' (Ankersmit 1986, 25). He thus did not expect that historical writing would bring an end to historical debate and result in consensual clarity and definitive knowledge regarding some historical phenomenon (Ankersmit 1985, 25; similarly White 1978, 89). The later Ankersmit is clearly more optimistic about this. If 'presenteds' are inherent parts of an object presented, that is, 'aspects', then adding more of them arguably yields a more accurate account of the object itself. Indeed, Ankersmit defines 'representational truth' 'as what the world, or its objects, reveal to us in terms of its aspects' (Ankersmit 2012, 107).

We come to the following picture. Representations are about 'presenteds' or 'aspects', which themselves belong to the inventory of the historical world, that is, the represented reality, itself. I must admit that I feel uneasy with the realist and cumulativist undertones that this constellation implies. First, if aspects are part of historical reality itself, this would seem to be a reversal of the early Ankersmit's commitment (in *Narrative Logic*) to the colligatory nature of historiography. Second, it is not clear in what sense aspects could be part of the past itself. What does it mean to say, for example, that an aspect of Napoleon (as re-presented in a book of history) is a part of the historical world itself? Was it not that an aspect is a constitution and colligatory contribution by the historian? It seems to me that any particular interpretation owes more to the 'subject-side' than is recognized here, as I will explain in Chapters 6 and 10. And it is not clear how a historian could discover a 'part-of-the-world aspect'. In relation to this, one is puzzled as to whether the number of aspects is limited. Further, as Ankersmit expresses it,[14] if representations are about 'aspects' then it is difficult to say what they are 'aspects of'. Does one need to commit to a pre-given set of objects (in the historical world) about which all historiography and their interpretations (via their aspects) are? The cumulative view creates an expectation that historiographical inquiry comes to an end and that all aspects of an inquiry are re-presented in that end. Finally, although there is no commitment to the 'third world' of presenteds, the constellation reproduces the subject–object dichotomy, that is, the basic conceptual opposition between representation and represented. For Ankersmit, represented reality is 'objectively given' and an interpretation is 'an activity of the subject' (Ankersmit 2012, 51). I won't go further into the metaphysical intricacies of what these objects might be,

but it is obvious that much more philosophical groundwork is needed to make the view viable. At the very least one would expect clear practical benefits to emerge from this kind of problematic postulation.

In the next section I outline a principled alternative to the representationalist account. Before that there is still one more task ahead, however. At the beginning of this section I suggested that one possible sense of 'representation' is that one object stands for another. An example of 'standing for' could be a member of parliament, who 'stands for me' in the parliament, but is nevertheless not a substitution of me. Another way to express this idea is to think of two religious groups and their symbolism. Typically an image of a saint stands for the saint in the absence of the real thing, but it cannot arguably be said to be its full-bloodied substitution. One still needs and craves the real saint. By contrast, the urge to destroy the images of God by iconoclasts may be said to reflect the fear that the images would take the place of God as a full substitution. Baudrilland writes that iconoclasts predicted the 'omnipotence of simulacra': 'that God himself was never anything but his own simulacrum' (1994, 4). They thus feared the death of divine referential and that the worshipping of God is transferred to the veneration of the simulacra or image of him (cf. Ankersmit 1994, 189–192).

Could we apply the 'standing for' sense of representation and see historiography in representationalist terms in this fashion? Could we say that a historical text 'stands for' the past? Undoubtedly we could, but this is too easy a solution at the same time. Namely, anything can be said to 'stand for' anything, if we just stipulate so. The meaningfulness of that stipulation is another matter. If I state that my coffee mug stands for an omnipotent being, my claim would be understandable linguistically, but would probably be taken as senseless nevertheless. I accept that a historical text 'is about' the past in some loose manner, but it does not make good sense to claim that a historical text stands for the past. In any literal sense, a text is a rather different kind of thing to the past, which makes it an unsuitable symbol of the latter. Most importantly, this postulation of 'representation' does not in any case change the substantial issue, which is that a historical text cannot be isomorphic with historical reality. They are structurally very different, as they are by their qualities. How could non-representationalism help us here?

Towards non-representationalism

For Ankersmit, the problems with the traditional account of representation strengthen his resolve to stick to the concept of representation

and find an alternative formulation for what representation is rather than seek a replacement for it. I agree with him that the problems with representation pose a challenge but, to me, the challenge is to go yet one step further and ask what historiography and its central knowledge contributions are, without making the assumption that they must be representations. In the previous chapter I argued that a commitment to representationalism unites the philosophies of Ankersmit and White. And the beginning of this current chapter discussed how representation-alism (in various formulations) has a long history and goes well beyond these two thinkers, including many who are otherwise extremely critical of Ankersmit and narrativism.[15] My view is that it is essential to consider giving up on the pre-analytic representationalist intuition that historical writing is necessarily about creating representations and that the mean-ingfulness of historical discourse requires that these representations are about some specific (abstract or concrete) corresponding entities that are *re-presented*.

There are two topics to consider here. One concerns my reasons for claiming that representationalism is a problematic commitment. The second involves the task of signposting a route to an alternative non-representationalist account of historiography. In other words, I wish to outline an alternative way for understanding what historical theses or narratives are – one according to which they are not about any kinds of target objects. The latter topic also revisits the question of what the rationale for historiography is. Both of these themes will be continued and the analysis deepened in subsequent chapters.

According to Ankersmit, the suggestion that each representation is paired with one aspect or presented stems from the logic of historiog-raphy. The idea that there is such a logic implies that these notions are needed to understand what historiography is and/or what it produces. The postulation of 'representation' may of course help in this task, but the question is whether the Ankersmitian notion of representation offers the least philosophically problematic and pragmatically most useful account available. Some philosophical problems that a commitment to representations brings were already discussed above. On the other hand, Ankersmit claims something more than that representationalism is pragmatically worthwhile – namely, that without a representational account one cannot provide an adequately comprehensible view of historiography. Representationalism is thus allegedly a necessary posi-tion on historiography in the philosophy of historiography. Is it?[16]

We are faced with the question of the pragmatic value of representa-tions. How is one able to identify an 'aspect' and ensure that we are

talking about the correct 'aspect' of a representation? Identification is important when we try to pin down what historian X's representation actually is. How could one check whether a representation 'is about' the correct corresponding 'aspect'? It is instructive to remember Wittgenstein's argument against private language, in which he said that a private language would be unintelligible not only to others but ultimately to its originator as well, as no-one could establish the meaning of its signs due to their non-public nature. There needs to be a possibility of being wrong or right about these aspects, and the criteria cannot be private.

It is notoriously difficult to spell out a historian's, or indeed any writer's message or thesis, and thus disagreements about what that message is are not uncommon. If we imagine that different readers come up with different prima facie justified 'aspects', what then is the status of all these 'aspects'? Are they all wrong, except perhaps one? There does not seem to be any interpretation-independent and other intersubjective way of identifying an 'aspect' except to read the written presentation and try to figure out what its 'presented' is, which takes us back to the original problem. Or would one choose the other horn of the problem, that is, that all interpretations are proper and justified 'aspects' of a representation? This option would render the notion of 'aspect' redundant because one would simply be just offering different interpretations of one representation.

To repeat, Ankersmit has correctly rejected representation as a two-place predicate according to which representation more or less directly reflects historical reality itself. The problems with that are obvious. 'Narratives' or 'representations' of history contain qualities that the past does not have. Further, it seems that they do not refer to any uniquely identifiable entities in the past. The central problem with two-place representationalism is clear. The past and its representations are so different in terms of their qualities that it is senseless to try to pair the two together. Now, it is not entirely clear why one would need the middle variable of the 'presented' or 'aspect' between 'representation' and the past if it has been admitted that representations cannot reflect the past. This variable does not seem to bring any practical benefits in terms of criteria for representational content. This casts doubt on whether the correct logic of historiography has been explicated: if 'presents' or 'aspects' do not help to decipher the meaning of a historiographical text, then they do not seem to be necessary postulations in textual interpretation. Further, philosophically, and particularly with respect to ontology, the added middle layer appears entirely superfluous, and is bound to create

additional philosophical puzzles with regard to its ontological status, location, identification, etc. In this respect, my suggestion is to apply the widely accepted maxim of Occam's razor: all things being equal, one should favor a simpler solution over more complex ones. And it is more straightforward and 'logical' to say that historiography is a 'presentational' rather than a 're-presentational' activity once the mirroring metaphor has been rejected. A work of history presents something, or constructs a thesis, but it is not necessary to assume that these products have independently existing counterparts either in a real or in an imaginary world.

The concept of representation appears to imply a capacity to *present again* something that has a determined form before any representational act. And if it is the case that representation via isomorphism, resemblance and similarity all fail and, further, that there is no determinate object to be re-represented, the concept of representation fails. It is true that many 'revisionist' accounts are more relaxed with regard to isomorphism, resemblance and similarity, but they require a target, an object, nevertheless. If we may stipulate target objects as tolerantly as Suárez (2004) suggests and only require that one be able draw informative inferences, any historiographical interpretation would turn out to be a representation of the 'past'. However, it is obvious that this re-definition makes the notion of 'representation' almost empty of its original meaning. The object category of the 'past', for example, is informationally too broad, whereas more fine-grained stipulations, such as the Renaissance, raise the question of whether the target object is given or is itself historiograpically constructed. Suárez makes a worthy proposal, but it is ultimately an attempt to rescue the concept of representation by removing it from its customary and productive sense. This kind of deflationarinism or minimalism has been fashionable in recent years with regard to truth (e.g. Horwich 1990; Wright 1992), knowledge (e.g. Williams 1996) and meaning (e.g. Horwich 1998), for example, but makes one wonder whether it would instead be more advantageous to reject the idea that such concepts are universally applicable. Why is a particular notion seen as so valuable that the conditions of application need to be stretched to such lengths? It is worth remembering that Nelson Goodman thought that fictional objects – such as the picture of a unicorn – have a 'null denotation'. He argued that such expressions as 'picture of' and 'represents' may be misleading because they have appearance of two-place predicates even in cases when they should be understood as one-place predicates: '"picture of Pickwick" and "represents a unicorn" are better considered unbreakable one-place predicates…from

the fact that P is a picture of or represents a unicorn we cannot infer that there is a something that P is a picture of or represents'(1976, 21). My view is that it is preferable to limit the scope of application, as in the case of Goodman's null denotation, rather than to attempt to make 'representation' an all-inclusive concept.

On one occasion Ankersmit comments on a similar proposal. Assuming that the copy theory of representation is untenable, Ankersmit notes that the 'postmodernist' solution would be to talk only about a presentation that, as such, does not refer to and is not about independent reality – and thus also not about re-presenting anything: 'We cannot properly speak of historical representation at all. For, the term representation requires the presence of an independently given (historical) reality which is, next, represented in and by historical writing. Consequently, as post-modernists often argue, the postmodernist notion of the simulacrum is essentially a going "beyond" or against representation' (Ankersmit 1994, 191–192; cf. Jenkins 2003a, 41). He goes on to say that in this case we would only speak of 'presentation'. However, as we have seen, although Ankersmit aptly analyzes the implications of representationalist vocabulary, he is not ready to sever the one-to-one link between representations and the historical reality that they are about.

The scare-word 'postmodernism' popped up in the quotation and requires some comment. To say that neither knowledge nor historiographical interpretation reflects reality is not to deny that there is something real or a reality. It only says that there are no structural or other (relevant) similarities and no simple referential relation between these. Assuming the risk of making the matter even more complicated, one can still say that historical interpretations are 'about' the historical world in some loose sense. Equally, it is reasonable to say that the phlogiston theories of eighteenth-century chemistry are 'about' the natural world although it is futile to look for the 'phlogiston' that would make statements about it true or false. The claim is thus *not* that 'presentations' could not refer beyond themselves or beyond a text; it is rather that they fail to refer to any unique corresponding entities. Further, neither does this position lead to a situation in which we could never prioritize different constructions on a principled basis. And certainly this does not lead to an 'anything goes' attitude. I cannot detail now why and how this is so, and therefore, I ask patience from the reader to continue on to the subsequent chapters.

The idea that historiography produces presentations, and not re-presentations, takes one directly to non-representationalism. The main advantage of this move can be seen immediately: it provides

liberation from a rigid subject–object dichotomy that forces one to look for clear and determinable objects that historiographical constructions are about. And this in turn enables one to direct attention to philosophical concerns other than those relating to the ontological status and nature of these putative objects. Michael Williams has written that the idea of the 'representational' is symptomatic of Cartesian philosophy and implies that a representation is about or of something: a unicorn, Paris, or the square root of three, (William 2009, 19). It may be better to look for an alternative model to the kind that Dewey called the 'spectator account', at the centre of which representationalism lies. The spectator account implies that 'the thing to be known is something which exists prior to and wholly apart from the act of knowing' (Dewey 1929, 205). It might be said that 'narrative substances' are prime examples of the kinds of entities that do not exist prior to historians' construction. Similarly to Dewey, Richard Rorty defines 'antirepresentationalism' as a position, which 'does not view knowledge as a matter of getting reality right, but rather as a matter of acquiring habits of action for coping with reality' (Rorty 2011b, 1). Emphasis is thus on the process of construction and not on a reflection of what is given in subject-independent reality.

However, I would not characterize the position of this book as 'antirepresentationalist', even though my intention is to outline a non-representationalist account of historiography with regard to its most important scholarly products. Further, my intention is not to play the role of a general critic of representationalism and the concept of representation aside Rorty (although I also do take the mirroring metaphor to be unhelpful). My aim is more modestly to suggest that it is not reasonable to apply representationalism in the context of historiography with regard to its main knowledge products. To try to make sense of historiography through a rigid scheme of representationalism would be misleading and will only take one further from the central observations: interpretations are inferentially born and are constructions by nature. Although many modifications of representationalism are possible, one should not try to save the concept of representation at all costs, provided that there is a more straightforward 'presentational' option available.

In other words, I do not intend to outline an alternative comprehensive metaphysics and philosophy of language. And I have no general problem with representations, which may indeed work perfectly well in some contexts. My argument is that representationalism is not a reasonable commitment in the case of historiographical theses and interpretations. As a matter of fact, even on Rortyan pragmatist grounds, it seems

permissible to continue to use the concept of 'representation' in some contexts – when it appears pragmatically justified and useful language for a particular context.

A potential problem is that if we give up on the idea of 'aspect' or 'presenteds' that are represented in historical writing, we also lose the central disciplinary contribution of historiography, that is, the 'views' or 'messages' that it produces – the narrativist insight of Chapter 1. If a book on Napoleon portrays him as an arrogant ruler, which would be its aspect, then it seems that we miss this and will be left only with the shells without the content, that is, a set of separate descriptions of Napoleon and the actual historical Napoleon, with nothing in between. This worry is understandable but nevertheless groundless. The non-representationalist solution does not do away with the central messages or theses of historical works. The essence of this suggestion is not to create additional imaginary worlds or assume that there are other kinds of objects that historians' theses must directly refer to. Provided that historiographical theses do not correspond (refer) to historical reality, it does not bring clarity to retain the representationalist language and the (quasi)realist approach, which forces us to match our representations with their 'shadows' in some abstract world. In other words, it is possible to accept that the books of history contain meaningful theses or messages, but that these theses or messages do not refer anywhere.

What is the nature of historiography then? How should we characterize its main products of knowledge if not in representationalist terms? The chapter began with a discussion of the different views of what historiography is and two suggestions were focused on. Many have seen historiography as a science but many others have taken it to be a form of art. I do not position my proposal on this axis because what matters are the discipline-specific features, not disciplinary status as such. I agree with Goldstein that historiography is a way of knowing and that the historian's account must be justified in terms of cognitive and not literary criteria (1976, 176; 181–182). The mistake that Goldstein (and also Tucker 2004) makes is that he thinks that historiographical 'constitution' (Goldstein 1976, 212) could be carried out merely on an evidentiary basis. 'Attending evidence' is certainly required, but it is not enough by itself. The non-representationalist suggestion of this book is that historiography is about reasoning for some theses and that the main contribution of a work of history is to provide an informal argument for or against a given thesis.

That a work of history contains synthesizing theses is something which I agree with the narrativists on, and it is indeed a valuable

observation. While Ankersmit and others have suggested that we should see representation as a three-place relation, and some have even proposed a four-place operation (Giere 2004), my proposal is to go in the other direction. I wish to move from the two-place postulation to a flat, one-place proposal with regard to the main cognitive products of historiography. My view is that we can give up on the assumption that there has to be an object that that makes a 'presentation' of history true or false, or an object to which a presentation 'refers' to, or 'is about'. Historical writing contains arguments, or, to say it somewhat differently, a historical presentation in total amounts to an argumentative intervention.

The reader probably already senses how this step changes the vocabulary, and how it re-orients one in theorizing. Historiography, with respect to its higher-order knowledge contributions, is being identified as a discursive practice and not in relation to objects to be portrayed. By the same token it is a *rational* undertaking. For the narrativist, the historian is a kind of descriptivist storyteller. In my view, the historian is a *critical reasoner*. An argument (in general) can be said to be comprised of premises and their relations, intermediate conclusions and, ideally, the main conclusion that follows from the reasoning that precedes it. It would be a category mistake to suggest that arguments correspond to anything in historical reality; the past cannot be thought of as being structured like an argument with premises, conclusions and their relations, just as it cannot have narrative qualities, as White pointed out. Instead, arguments function like interventions in historical discourses. In this sense, we might see historiography producing argumentative speech acts with which the author tries do something, which takes me closer to Quentin Skinner's theorizing (e.g. Skinner 2002) than I would have expected a few years ago (cf. Kuukkanen 2008). The 'doing' of history involves bringing about a change in the existing historiographical discourse – perhaps to make people reject a certain thesis or accept it in some modified form, or to persuade them to endorse an altogether different thesis. One should note that the ways of persuasion and reasoning can be very diverse and may also include 'narrative persuasion'. This, in brief, is my suggestion regarding what historical works produce and what their main rationale is. The remainder of the book is intended to convince the reader of this view and provide a detailed account of it. Next, I will analyze how reasoning is manifested in the books of history, contrasting the narrativist view with an argumentative one.

5
Reasoning in Historiography

The paradigmatic change that narrativism brought about in the theory and philosophy of history and historiography, as already emphasized a number of times, is that it turned our attention to what the books of history as more or less autonomous entities are. After all, they are the main knowledge contributions of historiography. Since books are composed of a large number of sentences organized in a handful of chapters, and perhaps subchapters, it is only natural to ask what it is that binds all these elements together. There has to be something that ties together the chapters and, ultimately, the content in those chapters. What is it?

The short answer is that historiography produces texts. For this reason, the analysis of historiography *as a text* and the examination of textual qualities have taken centre stage in narrativist examinations. Ankersmit, much like other narrativists, claimed that no other type of philosophy has studied texts as objects of analysis in the philosophy of language, which is how he thinks they should be approached (e.g. Ankersmit et al., 2007). Further, as readers are well aware by now, narrativism has also attempted to define the theoretical entity that binds all the textual elements together more precisely. It has suggested that there are specific synthesizing structures that weave sentences and chapters into a comprehensive view of the past and that are more abstract than material sentences or texts on paper. The synthesizing structures have been given various technical names, such as 'story', 'plot', 'narrative', 'narratio', 'narrative substance', 'trope' and 'representation'.

In this chapter, attention is shifted to the structure of historical works. This means dealing with one of the key concepts of the narrativist philosophy of historiography, that is, holism, as identified earlier. Do the books of history or the views that they contain amount to indivisible

wholes? Or should the internal structure of those books be understood in some other way?

It is worth repeating Ankersmit's main reason for regarding narratives or representations[1] as holistic entities. In his philosophy, there is an absolutely essential distinction between higher-order entities, 'narrative' or 'representation', and lower-order entities – that is, all the 'singular' statements or 'facts' that are subordinated to the higher order. On one occasion he spells out this distinction clearly, if provocatively:

> Theories of representation 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 … one can quite well be (as I happen to be myself) an adherent of positivist and empiricist accounts of historical writing for what takes place in the historical text on the level of the statement while being, at the same time, and adherent of a theory of historical representation for the text as a whole. (Ankersmit 2005, xiv; my emphasis)

The argument is that if one forgets this distinction and assumes that a historical text amounts only to a set of lower-order statements, one loses the 'view' that the text as a whole articulates. Indeed, the 'view' is *all* those statements *combined*, which constitute or give birth to another kind of entity on a higher level, as it were, with emergent qualities and features of its own. The resulting narrative or representation is a new individual entity defined by all the statements that it contains. To put it another way, all the statements are analytically true, that is, true by virtue of meaning, of the narrative or representation.

Representation as a whole is related to a discussion concerning the meta-views of historiography initiated in the previous chapter, that is, to the question of what kind of a practice historiography is and what types of products it creates. The narrativists compare representations to artistic creations, initially to literary narratives, and later predominantly to visual artistic products, such as paintings. When art is the analogy for historiography, it is understandable that the outcomes of historiography are seen as holistic entities. A painting is typically what it is only as *one*, and not, for example, as two separated halves (when it would arguably be one or two new artistic creations) or as a thousand sliced pieces of the original. But if one were to shift the comparison to some other kind of practice, the problem would change or disappear. If historiography were viewed as a science, we would not conclude that its products are wholes,

or at least not on the same grounds as when its products are compared to artistic creations. If the function of historiography were just to report 'facts', as might be the case when fitting it into a scientific framework, its product would arguably not be a whole but a list of factual statements instead. Similarly, if the role of historiography were seen as being *to explain* in the mode of a Hempelian covering-law scheme, its explanations would not amount to holistic entities. These are, of course, not the only options available. Towards the end of this chapter, my view on what historiography is and how its main scholarly products should be identified becomes clearer as I identify historiography as a rational and argumentative practice.[2]

In what follows I will first consider the central knowledge contributions of historiography from a practical angle. Can we identify something like a narrative or central thesis in works of history? What would concrete examples of this be? I will mention a number of historical examples. The virtue of these concrete analyses is that they make subsequent philosophical exploration of this topic more fruitful. I consider the kinds of entities that such central theses actually are. More precisely, I will examine whether they can be divided into smaller components or whether they are indeed wholes. What is the relation of the narrative or the 'central thesis' to the entire content of a book? Do all the possible statements that a text of history contains also define the historiographical thesis? The upshot of my view is that we need to distinguish the *meaning* of a historiographical thesis from the *evidence* for the thesis. This raises many challenging philosophical issues and also takes us to a more detailed examination of reasoning in historiography and the structure of its textual products. The structure will again be explicated with reference to concrete historical examples.

Narrative skepticism and narrative essentialism

It is worthwhile to outline the overall conceptual landscape before delving into a detailed analysis of sample historiographies. The narrativists have identified 'narrative' as the synthesizing element of historical works. It is now time to discuss in more detail what 'narrative' is, what a commitment to it entails and whether it is an inherent feature of any historiographical presentation. Even before that, however, it is useful to take a step back and briefly consider what it would mean to question the narrativist insight; that is, to think that a proper historical account could be devoid of any synthesizing elements.

Much of Ankersmit's early philosophy is based on criticism of the earlier philosophy of historiography for its failure to recognize and analyze the textual and other integrative features of historiography. In other words, as already discussed in the preceding chapters, he chided the philosophers of previous generations for focusing on statements as atomistic units and for not adequately differentiating between the historical accounts as a set of atomistic statements and as narratives composed of an ordered set of those statements. Questioning the narrativist insight would thus amount to thinking that a set of statements without connections or integration would pass as an acceptable form of historiography. What would such histories look like? Are there any examples?

The most obvious example is undoubtedly to be found in medieval annals that seem to merely record a year and an event or events without offering commentary or links between events. Another example would be the chronicle, which similarly lists events in a chronological order, although some see it as already being more structured in that it contains a 'central subject' (e.g. White 1980). In the historiography of science, we find a modern programmatic effort to 'merely describe' without imposing any pre-empirical structures on phenomena. Latour and Woolgar's (1986) anthropological studies of science require observers to empty their minds of any preconceptions regarding what science is and pretend ignorance with respect to it. Rather than seeking any larger frameworks or other integrative principles, anthropologists should 'just describe the state of affairs at hand' (Latour 2005, 144). According to Latour, 'the name of the game is to go back to empiricism' (Latour 2005, 144). Historiography based on anthropological studies of science would thus aim at only localized descriptions of social practice purified of all preconceptions of the described practice – something that has indeed found some applications in Latour and others.[3] However, it is doubtful, whether even radical anthropology can avoid imposing any generalizing structure on its textual material. Latour and Woolgar's *Laboratory Life* itself is certainly not merely a sequence of statements without any commentary that would connect the statements. It seems that advocates of radical anthropology rather commit to an *ideal* of descriptivism that promises the possibility of avoiding narrative or other synthesizing features without actually implementing it themselves.[4]

Are we then left only with annals as a candidate for an account of history devoid of any integrative elements? There are two things to say. White has suggested that even annals imply some minimal narrativist

plotting. I will discuss this suggestion below. A more important issue is that annals cannot be said to provide us with a proper historical account. No book of history is just a collection of descriptive statements and no student could pass a course in history if the final course work amounted to a random list of claims. Modern historiography requires coherence, consistency, integration and putting forward some views or theses on the past.

Let us next imagine a narrative skeptic and a narrative essentialist. They both accept that a work of history has to contain some kind of integrative element. The former is skeptical as to whether narrative form is necessary for historiography, implying that a non-narrative historiography is possible and reasonable, while the latter thinks that narrative is essential for any proper historical account. The narrativists are typically narrative essentialists, but here we can distinguish at least two different views, already mentioned in Chapter 2. Phenomenological narrativists, such as Paul Ricoeur and David Carr, assume that human experiencing itself must take a narrative form. This implies that narrativity is a transcendental condition of any kind of human experiencing and historical reality (as a human reality) always takes narrative form. Linguistic narrativists like Hayden White and Frank Ankersmit commit to a weaker claim that historical reality itself has no form, but that its intelligibility requires imposing a narrative order onto it.[5] Further, Mink famously stated that to claim that 'the qualities of narrative are transferred to art from life seems a *hysteron proteron*. Stories are not lived but told…it seems truer to say that narrative qualities are transferred from art to life' (1970, 557–558). Now, those who claim something about the conditions of human experiencing engage in a metaphysical debate, which arguably also links to the concerns of evolutionary biology and cognitive psychology (how was it that humans developed such an alleged transcendental condition of experiencing? How is this condition rooted in human cognitive capacities?). That debate is beyond the scope of this book and my interests, as the focus here is on the disciplinary nature of historiography. Narrativists of both kinds are nevertheless united in their belief that historiography necessarily requires narratives and is inherently a narrativist kind of practice. It is necessary to examine these claims.

Narrative essentialists form a large group in the theory and philosophy of history. We saw in the previous chapter that Croce thought that historiography necessarily takes a narrativist form that requires 'telling the facts.' Peter Gay states that 'historical analysis without narration is incomplete' (Gay 1974, 189). White called narrative a 'metacode'

for transmitting transcultural messages (see note 5). Historical theorist Nancy Partner says that 'history is narrative in form, virtually by definition, because narrative is what brings the seriatim stream of time under control for intelligible, meaningful comprehension' (Partner 2013, 2, cf. Mink 1970, 547). Even Ankersmit thinks that books of history typically adopt a narrative mode (2005, xiiv).

White mentions Tocqueville, Burckhardt, Huizinga and Braudel as examples of scholars who have attempted to draft a non-narrative or even an anti-narrative form of historical knowledge. They are thus potential narrative skeptics. However, White's claim boils down to a view that, although they did not 'tell a story with well-marked beginning, middle, and end phases' (1980, 6), they nevertheless 'narrated' their accounts in some sense. The hardest case for the narrative essentialist seems to be annals and their practice of mere recording mentioned above, because there does not seem to be any 'necessary connection' (White 1980, 11) between the events in their bookkeeping. And although it is possible to deny that their 'history writing' amounts to proper historical accounts, White argues that in fact even the annals contain 'surely a plot – if by "plot" we mean a structure of relationships by which the events contained in the account are endowed with a meaning by being identified as parts of an integrated whole' (1980, 13). For White, the registration of the events in the list of dates confers coherence and fullness, and in this way also 'meaning' to them (White 1980, 13). Further, he says that even a singular entry in the annals can be seen as a narrative. For example, the event of 1056, 'The Emperor Henry Died; and his son Henry succeeded to the rule', is a narrative according to White (1980, 18). Thus, although the annals and the chronicle fail to 'narrativize reality adequately', even they are not totally devoid of narrative elements.

How should we then understand 'narrative' itself? There is no universal definition, but a number of features are typically associated with it. One point of confusion is that 'narrative' is sometimes equated with 'story' (cf. Chapter 2), which was also Ankersmit's reason to reject the notion of 'narrative' in favor of 'representation.' It is clear in any case that 'narrative' for the narrativist philosophers of historiography is not 'just a story'; it is something more abstract.[6]. White writes that 'by common consent' narrativity is an essential attribute of 'history proper', which requires that 'the events must not only be registered with the chronological framework of their original occurrence' but also be 'revealed as possessing a structure, an order of meaning, which they do *not* possess as mere sequence' (White 1980, 9; original emphasis).[7] While

narrativity itself does not require chronological ordering for White, a chronological sequence is a condition for the narrativization of a set of events in historiography. We can also consider Ankersmit's 'narrative realist's' idea of narrativity as depicting a consecutive set of events like in a film (see Chapter 3). And this same seriatim principle with regard to narrative may be said to characterize the 'narrative idealist's' mode of presentation. Further, the early narrativists, such as Danto (1962, 1968) and Mandelbaum (1967), also linked narrativity with chronological ordering.

However, if the chronological presentation of real events were enough by itself, then the annals and chronicles would count as *proper* narratives, which they do not. Yet opinion seems to be split as to what other features narratives must possess. It is nevertheless clear that the narrativist philosophers of historiography, particularly White and Ankersmit, require that a narrative must reveal events 'as possessing a structure, and order of meaning, which they do *not* possess as mere sequence' (White 1980, 9). This structure and meaning thus constitute features that the past itself does not have and which is imposed by the historian. Further, it seems clear that White, Ankersmit and the early narrativists more generally (see Chapter 2) require that there is some kind of central subject (such as some 'narrative substance' or 'presented'), which is the principle that provides importance or significance to events and around which the narrative is organized. White thinks that even chronicles require a central subject: 'the capacity to envision a set of events as belonging to the same order of meaning requires a metaphysical principle by which to translate difference to similarity. In other words, it requires a "subject" common to all of the *referents* of the various sentences that register events as having occurred' (White 1980, 19).[8] Finally, some have also suggested that a narrative (or 'plot') must include causal links between events (e.g. M. White 1965, 223–224; Foster 1927, 60; Carroll 2001, 126).

The narrative essentialist's commitment to narratives and their central subjects has a philosophically significant consequence: holism. We just saw how 'plot', according to White, amounts to an integrated whole that yields a specific meaning to the events subsumed. In accordance with this, Partner writes that the 'plot' must be 'intelligibly connected, *every component* standing in some logical relation to the others' (Partner 2013, 503; my emphasis; similarly 502). The tenet of holism in general was discussed in detail in Chapter 3, where it became clear that this commitment entails some philosophical problems (undecomposability, analyticity and unfalsifiability). Now it is time to move closer to the

actual books of history. It will soon become obvious that there is a price to pay also on the practical level.

A consequence of the union between narrativism and holism is the belief that everything in a narrative defines the narrative and the narrative amounts to historical knowledge only as an irreducible whole. The claim is thus that, without seeing all the events as part of one whole (narrative), understanding of a historiographical thesis is not achievable and, further, that historical knowledge is the whole written narrative from the beginning to the end.[9] Let me first say that this view sets the bar very high for the historian. The historian is expected to have the skill of a novelist and to design a book so that it amounts to one comprehensive narrative in which everything mentioned in the narrative finds it place. But this conception is very demanding with regard to the reader as well. In order to comprehend the historian's narrative and acquire historical knowledge, the reader should be able to know the whole narrative and all the events that are part of it. All the events and statements of those events are definitionally part of the narrative: without all the statements of events there is no narrative, and without the entire narrative there is no adequate historiographical comprehension of those statements.

We are ready to state, then, what the double commitment to narrative and holism by the narrative essentialist entails. With regard to narrative, the commitment implies that:

1. The only acceptable form of presentation in historiography is the narrative form, which implies at least chronological ordering and the holistic endowment of meaning to all parts of the narrative.
2. Historiography takes a fundamentally descriptive mode in the sense that it attempts to order, link and give meaning to a sequence of events in the object world of historical reality.

With regard to holism, the commitment implies that:

A. Every event or a statement of the event has a definitional role in the narrative.
B. No historian's cognitive message can be understood without understanding the whole narrative. That is, no part of a narrative, statement, etc., can be ignored.

Next, I test these presumptions of narrative essentialism by examining some actual historiographical examples. This takes the discussion onto a more tangible level and in this way provides ingredients for abstract

evaluation at the same time. It might also be said that the following discussion is a search for two kinds of answers. On the one hand, the questions deal with the nature of historiography: What kind of practice is it fundamentally? How should it be characterized? Or simply, what is the point of historiography? On the other hand, this involves the relations of different elements in a book of history: What is the relation between lower-order statements of knowledge and the higher-order historical thesis? Is a work of history a whole? Can some parts of it be understood without understanding some others?

Historiographical theses

Eric Hobsbawm was perhaps the most productive generator of historiographical theses of his generation. In a review of the posthumous collection of Hobwbawm's articles, Richard J. Evans suggested that one reason for his global appeal was the extraordinarily 'enormous, astonishing fertility of his historical imagination', which resulted in 'a whole shedload' of concepts: the 'General Crisis of the 17th Century', the 'dual revolution' (the French and Industrial revolutions, the formative events of modern times), the 'invention of tradition', 'primitive rebels', 'social banditry', the 'long 19th century' (1789–1914), the 'short 20th century' (1914–1989), etc. Evans' character assessment of Hobsbawm passes well as a characterization of an ideal historian: 'ability to see the big picture and devise a framing concept to sort out the diverse and unruly detail of history' (Evans 2013). At least some of these concepts also amount to the theses of entire books. *The Age of Extremes: The Short Twentieth Century 1914–1991* (Hobsbawm 1995) springs to mind above all.

Another example of a historical thesis can be taken from E. P. Thompson's *The Making of the English Working Class* (1991). Thompson's book is a classic in labor and social history. What is the view for which Thompson argues in this monumental text of 958 pages? The main thesis of the book is that the English working class was born between 1780 and 1832, and further, that its birth was an active process, that is, that the working class was not made but that it made itself. As Thompson eloquently expresses this view at the beginning of the book: 'The working class did not rise like the sun at an appointed time. It was present at its own making' (Thompson 1991, 8). Thompson's book has great depth and contains a wealth of information on the period, some of which I will introduce and discuss below. But it is this thesis above that provides the topic and angle for our analysis, as none of the details is central in the book. Alternatively expressed, all the details are

subservient to the main thesis, which thus synthesizes the book and its informational content.

Christopher Clark's book on the origins of the First World War *The Sleepwalkers: How Europe Went to War in 1914* is another book of history that will be studied in depth. This is a recent book that hit the book-shelves in good time for the 100th anniversary of the Great War. What is the central view of this book? It is already included in the title, as often is the case in good historiography. The claim is thus that European decision-makers were like sleepwalkers who moved step by step towards a goal without being fully aware of or without fully understanding what the end station of this process would be.

It would be possible to go on mentioning examples of equivalent historical theses almost endlessly, but these are enough to give concrete-ness to this task. These represent first-class scholarship in historiog-raphy and that is the main reason for their inclusion here, but I could have chosen innumerable others. Like so much of Hobsbawm's work, Thompson's volume has also acquired the status of a classic. R. J. W. Evans (2014) calls Clark's volume in turn 'the most consistently subtle, perspicacious, and thought-provoking', referring to the wave of publica-tions on the event of the 100th anniversary of the Great War. In what follows, I analyze how the central theses can be identified in these two books and what form they take.[10]

The narrativist suggestion is thus that one understands a literary historical thesis only in the situation in which one knows all the sub-clauses that define it or are somehow related to it. Ankersmit writes that 'none of the statements which constitute the text is...irrelevant to the text's presentation of the past' (1995b, 225). In other words, it would be possible to understand the thesis only if one knew the entire content of the book, because it is assumed that the book forms an integrated *whole*. Alternatively, if a book is irreducibly *one*, then the understanding the book requires knowing what this *one* is, and this in turn requires knowing all the components that define the one.

Holism thus presents a very strict criterion for both the identity of narratives and for the understanding of them. Books contain thou-sands of sentences and they *all* constitute what a narrative is, that is, its identity. An omission of even one meaning-defining sentence changes the object, that is, meaning, to something else. This kind of semantic holism[11] has many unintuitive consequences, such as that very few, if any, would understand the main thesis of Thompson's *The Making of the English Working Class*. How likely is it that even the best expert in social history is able to memorize or have some other kind of mental access to

all the sentences on 958 pages in Thompson's volume? Even if one had read the book from cover to cover, it is hardly realistic to expect one to keep all the details in the mind.

If one now admits that it is not necessary to know all the sentences in order to understand Thompson's main thesis, then one either admits that all the sentences do not define the thesis or that we can draw a distinction between understanding and identity or meaning so that understanding a thesis does not require knowing its meaning. The first option would amount to abandoning holism and the second remains unclear without some further theory on the relationship between understanding and meaning.

It is therefore important to ask, what defines the meaning or identity of a specific historical thesis. Or simply, what constitutes a specific historical thesis? The assumption about the holistic nature of historical theses is best assessed by considering it on the level of a concrete historiographical example. Is the claim that Earl Fitzwilliam was removed from his Lord-Lieutenancy for protesting at the massacre of Peterloo in 1818 (Thompson 1991, 751), for example, *a constitutive part* of the meaning of Thompson's main historical thesis that the English working class was born more or less spontaneously between the years 1780–1832? Or is the claim that the publication *The Political House that Jack Built* sold 100,000 copies (Thompson 1991, 743) a part of its identity? Or that the London Correspondence Society called a great demonstration in Copenhagen Fields in Islington on 26 October 1795 (Thompson 1991, 157)? All these details are part of the 'representation' or 'narrative' if it is conceived of holistically. Is it the case that, if a person does not know one of these sentences, one does not understand what Thompson claims in his book? Let us assume that two extremely intelligent and competent readers know two respective sets of sentences that are almost complete, but that differ with respect to one sentence (that is missing in one's set or is replaced by another in the set). Should we in this situation assume that the theses they understand are different? If holism is correct, we should, no matter how insignificant the difference or the sentence is.

It is unreasonable to assume that all these details amount to meaning-constituting parts of the historical thesis in such a way that knowing them is required for understanding Thompson's main claim. Thompson could have dropped the mention of how many copies this particular underground journal sold and still be seen to make the same case for the birth of the English working class. Needless to say, it would be possible to mention literally hundreds of other minute 'facts' found in the almost 1000 pages that do not seem central to the main historiographical thesis.

In addition to the negative case, that no one (including the author) would understand historical theses and all would in practice understand a different thesis, it is possible to make a positive case (below) that one can as a matter of fact understand what the historian is saying without knowing all the details and claims that the book contains.[12] These considerations suggest that we should make a distinction between identity or *meaning* on the one hand and *evidence* on the other. The difference can be put as follows. Ideally all the material in a work of history supports the main thesis (and in this sense the narrativists are correct that the whole literary work matters), but all the details mentioned do not define the meaning of the thesis, that is, what the thesis is. Minute details about minor agents and their actions and movements provide evidentiary support for the thesis but the understanding of the thesis does not require knowing all of them. The case is rather that the more of this kind of information there is, the stronger the evidence for the thesis. The bits and pieces should naturally be appropriately connected to provide effective support.

Distinguishing between meaning and evidence means rejecting semantic holism, as the distinction establishes a border between meaning-constituting and non-constituting parts. This kind of distinction is very traditional and familiar from the history of the analytic philosophy of history. It may appear to be a risky commitment because it resembles the analytic and synthetic distinction derided by some of the finest philosophers of the twentieth-century, such as Quine (1951). It is necessary to make clear that my intention is not to re-establish the analytic–synthetic distinction.[13] The main issue is that that the distinction between meaning and evidence appears useful, and more, necessary, if we are to make sense of the structure of the studies of history; this is my suggestion for the 'logic' of historiography. The reasonable approach in this context is not to get bogged down in a debate on the nature of meaning as such but to consider what the understanding of (the meaning of) a thesis practically requires. It is worth remembering that we are dealing with something that emerges through the *writing* of history. With this point in mind one needs to direct attention to the question of what it takes to *understand* a historical thesis. The rationale for history writing is obviously to *communicate* something about the past to contemporaries, and this places the question of understanding in a central role. In what situation can the readers of a historical book be said to understand what the book is arguing for? The key problem is not what the 'meaning' is but when that 'meaning' can be said to be understood. And the answer is that it is understood when a sufficient number

of appropriately related beliefs or claims, as well as the relations between them, are known. Consequently, what is not required for understanding (a thesis) is *evidence*, the role of which is to convince the reader that the thesis is tenable.

Even if there is no sharp boundary to be drawn between meaning and evidence it does not follow that there is no distinction. Although there is no sharp distinction between the bold and the non-bold either, this does not mean that all are bold or that no-one is bold. I am well aware that the philosophical problems of semantic holism and its alternatives are extremely challenging. One might say that Quine's argument that there is no principled way of drawing the analytic–synthetic distinction is still the chief reason to advocate semantic holism in some form. And although the problems with semantic holism are recognized, it is difficult to outline alternatives to it without endorsing equally problematic semantic atomism. Fodor and Lepore mention two attractive ways of reaching a middle ground. One could try to rely on the notion of meaning or belief *similarity* instead of identity. Or one could be a 'molecularist' who thinks that 'there are other beliefs that we must also share if we are to share the belief that P, but...denies that *all* our other beliefs have to be shared' (1992, 31).[14] However, they argue that in the first case, all notions of similarity must ultimately rely on the notion of identity and that molecularism does not hold without the adoption of the analytic–synthetic distinction. Now, as already stated above, the challenge that I take on in this book is not to sort out this interesting philosophical dispute but to find a credible and practically functioning way of using the notions of meaning and evidence in the context of historiography. Nevertheless, it would be possible to characterize my suggestion both in terms of molecularism and similarity about meaning content, as will be explained later.

Meaning in historiography

When one rejects the holistic account one thus also abandons the assumption that *all* the statements in a work of history are meaning-constituting. The polar opposite position is to assume that a historiographical thesis or its components are punctuate or atomistic entities, whose understanding does not require knowing any other meanings and their relations to the thesis studied. This is not feasible in our historiographical case (and more generally). since one needs to be aware of a fair amount of information expounded. For example, in order to understand Thompson's claim that 'The English working class was "present at its own

making"' in 1780–1832, one needs to be aware that Thompson under-
stands 'class' as a process and not as a static sociological entity. More
importantly, it is necessary to also know how the process of 'making' is
understood and what its central constituent parts are. The first link in
the process is made evident in first part of the book, 'The Liberty Tree'.
It says that in the years and events before the eighteenth-century the
seeds for the later emancipatory working class movement were planted.
These 'seeds' are composed of various (often religious) forms of dissent
and the idea of 'The Free-born Englishman'. The next main section of
the book, 'The Curse of Adam', claims cultural and political continuity
from the late eighteenth-century and describes how the productive rela-
tions and working conditions of the Industrial Revolution changed the
life of field laborers (Chapter 7), urban artisans (Chapter 8) and hand-
loom weavers (Chapter 9). Thompson's conclusion is that 'by 1840
most people were "better off" than their forerunners had been fifty
years before, but they had suffered and continued to suffer this light
improvement as a catastrophic experience' (Thompson 1991, 231) and
that this meant 'the reduction of the man to the status of an "instru-
ment"' (Thompson 1991, 222). Finally, it is necessary to understand the
point in the third main section of the book, 'Working-class presence'.
Radicalism remained defensive in the years after 1815 and was often
driven underground. And although there is very little (and distorted)
information left on the underground movement, Thompson claims that
there is a clear continuity of 'making' from pre-1815 years to the 1830s.
In order to see where Thomson's reasoning leads to, it is best to take a
few direct quotations from him:

> At the end of the decade [1820s]…it is possible to speak in a new
> way of the *working people's consciousness* of their interests and of
> their predicament as a class…there is a sense in which we may
> describe popular Radicalism in these years as an intellectual culture.
> (Thompson 1991, 781; my emphasis).

In these years, working people learnt to see themselves

> as part of a general history of conflict between the loosely defined
> 'industrious classes' on the one hand, and the unreformed House of
> Commons on the other. From 1830 onwards a more clearly defined
> *class consciousness*, in the customary Marxist sense, was maturing, in
> which working people were aware of continuing both old and new
> battles on their own. (Thompson 1991, 781–782; my emphasis)

Further, the final actual (sub-)chapter declares that, in the years
1831–1835:

> there is a sense in which *the working class is no longer in the making, but
> has been made*. To step over the threshold, from 1832–1835, is to step
> into a world in which *the working-class presence* can be felt in every
> county in England, and in most fields of life. (Thompson 1991, 887;
> my emphases)

The meaning of the thesis can be said be to be constituted of these kinds
of central elements that constitute the process, which results in the birth
of English working class. Understanding meaning implies that one is
able to link these elements together.

It is worth noting that meaning and evidence naturally become
entangled as a deeper understanding of the meaning results in a better
awareness of evidence. Again, despite the fact that this conceptual
distinction is not clear-cut, there is nevertheless a distinction to be
made, the main claim being that most of the factual elements in the
book are not necessary for the understanding of the main historical
thesis. The deeper one delves into factual evidence, the clearer it
becomes that one has crossed the border beyond what is needed for
the understanding of the thesis for any practical purposes. The distinc-
tion between meaning and evidence is thus gradual and a matter of
degree. If one tries to analyze what a historian claims in a book, one
eventually normally realizes that that one not only understands the
meaning of the central thesis but has also become aware of at least
some of the grounds for believing it. In other words, the meaning of
a historiographical thesis depends on its historiographical context
but *not on the whole context*. Therefore, it could be said that meaning
is narrowly holistic or narrowly context-dependent, without a sharp
boundary concerning what needs to be understood. Or, using Fodor
and Lepore's terminology, the account is molecularist in the sense
that understanding the main historical thesis requires knowing many
beliefs, but not all of the beliefs put forward in the book. Alternatively,
if one can make a sufficient number of *inferences* or possesses a suffi-
cient number of relevant beliefs, then one can be said to understand
Thompson's historical thesis. The question of how much is sufficient is
not important because that depends on the level of specificity required.
The central claim is that when one goes beyond the meaning-context,
one enters the area of evidence, the knowing of which is inessential for
the understanding of meaning.

What about Clark's thesis of Europe sleepwalking towards the First World War? What is its meaning? This thesis is illustrated with some very telling anecdotes in the book. For example, in the autumn of 1913 Serbian Prime Minister Nikola Pašić and the Austrian Foreign minister Leopold von Berchtold met. The recent Serbian occupation of Albania cast a shadow over the meeting and Berchtold intended to raise the issue in discussions. However, he was so overwhelmed by the 'warmth of Pašić's overtures' (Clark 2013, 98) that he 'forgot' to express Vienna's strong objections to Serbia's occupation of Albania. It is worth letting Clark describe the situation:

> It was agreed that he would broach the Albanian Question with the Serbian leader that evening when the two men were both expected to attend the opera. But when the foreign minister arrived a little late to take his seat in the royal box, he found that Pašić had already retired to his hotel, where he was supposedly in bed fast asleep. The Serbian prime minister left Vienna early next morning without any further meeting having taken place. Berchtold went back to his desk and spent the small hours writing a letter that was taken round to the hotel courier so that it reached Pašić as he was leaving the City. But since it was scrawled in German script (not to mention Berchtold's notoriously inscrutable hand) Pašić was unable to read it. Even when the letter was deciphered in Belgrade, Pašić supposedly found it difficult to see what Berchtold was getting at. And the people of the Austrian Foreign Office had no idea either, because Berchtold had not thought to preserve a rough copy of the text. This comedy of errors…is in no doubt in part an indictment of Austrian disarray…Above all, it conveys a sense of the paralysing awkwardness that had settled over Austro-Serbian relations by the eve of the First World War. (Clark 2013, 98)

This is of course only a small episode in the saga that took the world to its first total war, but the 'comedy of errors' and miscalculations of this episode exceptionally vividly exemplifies the sleepwalker thesis concerning the First World War. The main players in the pre-Great War drama were not in full control and lacked full understanding of the consequences of their deeds.

Clark gives several reasons for this 'sleepwalking'. One is that there was a 'chaos of competing voices' both between the allies and within the decision-structures of the European powers. For example, there was inherent uncertainty and lack of clarity in the monarchical decision processes. If a king failed to perform an integrative function (and

they often did) in the power relations of the monarchical structure, the system remained unresolved and potentially incoherent. 'In this sense, kings and emperors could become a source of obfuscation in international relations' (Clark 2013, 184). In general, the international system was marred by a relatively poor understanding of each other's intentions, and it did not improve the situation that confidence and trust (even within the respective alliances) was low, combined with high levels of hostility and high paranoia (Clark 2013, 240). The key foci are the 'fluidity' and 'fluctuations' of power structures and influences, which manifested in uncertainty and in a high degree of contingency in the system.

One way in which Clark's reading differs from various other interpretations of the Great War is that he does not look for single culpable actors in the crisis, and nor does he put particular blame on Austria-Hungary and Germany. In his book, Austria-Hungary provides a prime example of 'sleepwalking' because its decision-structures conditioned the walk towards the unknown. Clark compares Austrians to 'hedgehogs scurrying across a highway with their eyes averted from the rushing traffic' (Clark 2013, 429). The decision-makers in Vienna discussed the possibility of Russian mobilization and a general European war, but these scenarios were never properly weighed and assessed in the process of policy-making. A central reason was 'that the hive-like structure of the Austrian-Hungarian political elite was simply not conducive to the formulation of decisions through the careful shifting and balancing of contradictory information' (Clark 2013, 429). A tendency to miscalculate the intentions of negotiating partners and underestimate the seriousness of the situation was widespread. For a long time, and on many sides, there were no indications that the leaders wanted war. There were, for example, according to Clark, no signs at the end of July 1914 that British Foreign Secretary Grey wanted war, and all the major British newspapers viewed a European war with 'distaste' (Clark 2013, 492).

Another important reason for 'sleepwalking' was the lack of historical experience itself. Clark asks whether the protagonists understood how high the stakes were. 'Europeans subscribed to the deluded belief that the next continental conflict would be a short, sharp cabinet war of the eighteenth-century type; the men would be "home before Christmas", as the saying went' (Clark 2013, 561). Clark notes that people had no experience of a total war before 1914 and thus lacked a substantial understanding of what might follow, that is, the 'wisdom' that later probably helped prevent an open nuclear war between the superpowers

after 1945. All in all, Clark points out that although before 1914 people realized the dangers hidden in the international system and they indeed had some understanding of the horrors of war, they didn't really grasp them fully: 'The protagonists of 1914 were sleepwalkers, watchful but unseeing, haunted by dreams, yet blind to the reality of the horror they were about to bring into the world' (Clark 2013, 562).[15]

In sum, understanding Clark's thesis requires grasping the kinds of issues mentioned above. What precisely it is that needs to be known cannot be accurately stated because the border between meaning and evidence is porous. Nevertheless, many of the details in the book clearly fall on the evidence-side of the border in any normal inquiry. It is quite possible to understand Clark's view on the Great War without being aware of the way in which the Serbian King Alexander and Queen Draga of the Obrenović dynasty were assassinated and, for example, that it was king's first adjutant Lazar Petrović, who led the assassins through the 'darkened halls' in the hunt for the king (cf. Clark 2013, 3–5) – although knowing this incident illuminates the tumultuous nature of Serbian politics at this point and may help better appreciate how precarious the situation in European international relations actually was. Undoubtedly, Clark could have used different examples to make that point.

Again, it cannot be explicitly stated what belongs to the meaning component and what to the evidence component. Rather, it is a question of a sliding scale in which there are, on one hand, such linguistic meanings and central historical claims without which understanding would hardly be possible and, on the other hand, there is some numerical and other semantically inessential information. One can always go from the most central elements to more marginal ones and understanding of the thesis improves and becomes more nuanced in the process. But there is a point at which the student of the book is entitled to say: 'Now I understand what Clark means' without linking every evidential detail to the central claim.

Finally, I would like to further suggest that the blurred line between meaning and evidence is a strength of historiography and not a weakness. Given the twentieth-century philosophy of language and philosophy of science, it would not be wise to re-awaken the debate of whether there is a distinction between analyticity and syntheticity. And it would not be reasonable to commit to full-bloodied holism, which makes everyday communication a mystery. Some distinction is needed, but it is best to keep the border between meaning and evidence porous in the spirit of pragmatism.

Evidence, reasoning and argumentative structure

Now it is necessary to consider in more detail the role of those parts of a historiographical presentation that cannot reasonably be said to constitute the meaning of a thesis. In the background, there is the question of how presentations are structured. To put it differently, if the previous section rejected holism, now it is time to pay attention to the *form* of historical presentation. The most traditional suggestion is that historical presentations are narratives or take a narrative form, and it is often remarked that the narrative form can be found already in the works of Thucydides and other ancient writers of history. 'Narrative' was identified above as a structure that minimally entails chronological order and holistically endowed meaning. The latter feature has now been questioned with regard to the entire historiographical content. How about the chronological form of presentation? Is it necessary? More importantly, is it what historians must be committed to?

  If the requirement of creating an all-inclusive narrative or plot elevated historians unrealistically to the league of master novelists, the expectation of necessary chronological ordering of one's presentation would seem to demean the skill of the historian. The narrative form in this traditional sense implies something like a 'descriptive mode' of presentation. The idea is that historians first describe events and then connect them to each other; they thus show what happened first, what happened next, and so on, ending with a final event at the end of the book. The descriptive mode relies on there being such 'events' in the object world, which are merely transferred to a chronologically ordered set of linguistic descriptions to produce the historian's narrative presentation. Naturally, a 'plot' could not be derived from the events themselves, but apart from that historical writing would seem take to adopt a realistic mode and the standpoint of an observer. As discussed in the previous chapter, it is instructive to remember that, despite their criticism of the copy theory of representation, both White and Ankersmit think that historiography necessarily creates re-presentations of the past, terminologically suggesting that the historian's narrative is a re-description of pre-given set of events in some way. White even stated very clearly that both historiography and literature are by necessity committed to 'representing reality realistically' (White 2011, 398).[16] On another occasion, he even lamented that 'it is the historians themselves who have transformed narrativity from a manner of speaking into a paradigm of the form which reality itself displays' (1980, 27).

In this way we come back to the question of what kind of an activity historiography fundamentally is. Are historiographical presentations best characterized as being realistic narratives by virtue of their form? Is the structure of historical works like a temporally advancing set of events? It is important to emphasize here that the question deals with what historiography *primarily* is or what the *governing function* of historical presentation is.

My view is that it is degrading to suggest that historians merely report what happened first, what happened next, and thereafter, and still further on, etc., even if it is assumed that the chronologically advanced 'story' manages somehow to endow surplus meaning to the events. Some decades ago, Goldstein concluded that although the historian's presentation may take a narrative form, it 'need not and does not always' do so (Goldstein 1976, 141; see also 176). As an example, Goldstein argues that Ronald Syme's *The Roman Revolution* does not amount to a story that necessitates and makes all the events intelligible in followable order. Syme rather employs 'the method of accumulating examples' (Goldstein 1976, 172). Goldstein argues that Syme wrote his book in order to support the *thesis* that 'without a party a statesman is nothing' (Goldstein 1976, 178). Further, he writes that 'even a good deal of the more traditional products of historiography are not really narratives in any recognizable sense of that form' (1976, 177). Goldstein further thinks that 'it cannot be reasonable that the essential nature of the discipline is defined by the literary form' (Goldstein 1976, 142). Also Tucker has questioned narrativity as the defining feature of historiography (Tucker 2004, 139). However, although Goldstein and Tucker do thus not commit to narrative essentialism, they largely agree that typically historiography is presented in the narrative mode. By contrast, my point is that the nature of the 'superstructure', to use Goldstein's terms, is misunderstood. Further, the distinction between 'superstructure' and 'infrastructure' to distinguish the presentational mode from reasoning and the evidentiary mode is not firm.[17] The presentation itself is a form of reasoning and part of the overall justification of a historical work.

I argue that historians use their critical and reasoning faculties more than is typically recognized. *The American Heritage Dictionary of the English Language* defines 'reasoning' as the 'use of reason, especially to form conclusions, inferences and judgments'. This is what historians do in their books: they present a view or views and reasons to accept it/them. Further, the concept of 'narrative' mischaracterizes the nature of knowledge production in historiography, which, in actuality, results in something more structured than just a set of descriptions of singular events. It

also obscures the fact that the choices that the historian must make are far from self-evident. What is more, the narrative form of presentation would impose a kind of iron-cage model on historiography, implying that historians have to present their works temporally or chronologically. It is not too difficult to find examples of historians who do not use narrative plotting in any obvious sense, such as the US-style social science history (e.g. Fogel and Engerman 1974) and the French Annales School (e.g. Braudel 1996).[18] It is not my intention to pass judgment on how successful these specific attempts are, but to merely mention them as some well-known cases of non-narrative historiography. And if they do not present their books narratively, then it is clearly not necessary to commit to a narrative-chronological presentation of history. However, it is more interesting to reverse White and others' approach to narrativize all historiography and ask whether even those who seem to subscribe to a narrative–chronological model can in fact be said to do so. In order to find out whether this is the case, the focus is shifted again to actual written historiographies.

Reasoning in Thompson's *The Making of the English Working Class*

I suggested above that historical presentations contain specific theses concerning the past and that it is possible to differentiate between the meaning of these theses and the evidence for them. But what is the nature of evidence in a work of history then? Is evidence narratively structured or not? And, once more, what is the relation between meaning and evidence in a work of history?

The point of evidence is to give a reason or reasons to accept a historiographical thesis. The primary mode is to bring forward factual evidence that supports the thesis, but the historical data should naturally be so connected as to lead to the conclusion defended in the book. This connected evidence should in turn create a structure that makes a *case* in favor of one's claims, as Stephen Toulmin has usefully suggested. Although Toulmin's frame of reference is jurisprudence, he extends his analogy to historiography. If a historian's 'case' is to defend the character of Tiberius, for example, we can challenge his case by drawing attention to 'the grounds (backing, data, facts, evidence, considerations, features)' for making that case (Toulmin 1958, 11).

What factual evidence is there in Thompson's book that is not necessarily required for the understanding of the historical thesis but supports it, for example? In this case, if ever, it has to be 'for example'

since the 'factual basis' is so copious. I will mention several very useful details with regard to the main thesis. One key point of Thompson's book is that when a market for the labor force was created, it increased the insecurity of the workers enormously. Thomson provides very illuminating authentic descriptions of this state of affairs: '"If there comes a frost they discharge them," said one overseer. "When the season opens they come to me, and take 'em back again. The farmers make my house what we call in our trade a house of call." Wet weather created a "surplus": harvest a "shortage"' (Thompson 1991, 248). Through the documented words of this overseer one becomes a little more convinced and understands slightly better why the workers' situation is said to have deteriorated despite improvement in material living conditions in general. Furthermore, in several chapters Thompson gives other very interesting details on the ways in which the working people's predicament became worse: field laborers lost their common rights and the vestiges of the village democracy, artisans lost their craftsman's status, weavers experienced the loss of livelihood and independence, children lost opportunities for work and play in the home and while many workers' real incomes improved they too felt the loss of security and leisure as well as a deterioration of the urban environment (e.g. Thompson 1991, 487).

All these details provide evidence for Thompson's claim that the working people's situation became more insecure and, more importantly, add information on how this happened. This claim in turn is one reason for why working people became radicalized, which can then be taken as a pre-condition for the creation of the shared awareness of a class condition and, furthermore, seen as a definitional criterion for the birth of the English working class. I hope the reader has now acquired some sense of what the argumentative nature of historical books means. A chain of reasoning connects these various claims, and they are only some of the claims and the relations presented in support of the main conclusion. However, many of the details above are not necessarily required for understanding the main thesis regarding the emergence of English working class. It is perfectly possible to understand what Thompson means by his main claim without knowing what kind of exploitative opportunities a particular overseer saw. And one may not even necessarily need to be aware that the field laborer lost the vestiges of village democracy, although this statement appears fairly important. But if one knows these details one both understands the thesis better and realizes why Thompson has advanced it. There is no necessary end-point to this informational and evidential deepening.

It is worth taking note of how Thompson himself characterizes his treatise: 'This is a group of studies, on related themes, rather than a consecutive narrative' (1980, 11), much like Goldstein's 'accumulating examples'. I will next characterize the main argumentative line in the book in some more detail and examine the roles of three main sections of Thompson's argument.[19] How do these 'studies, on related themes' support the main thesis?

In the *first* main section, Thompson establishes that in the years and events before the eighteenth-century the seeds for the later emancipatory working class movement were planted. The *second* section adds another element that is needed in order to make the conclusion reasonable. Thompson argues that the working class was doomed to hardship and struggle. They were, apparently, carrying the 'curse of Adam', as the second part suggests. Thompson also shows that the conditions for unskilled manual labor and in industries subject to the outwork system reflected a regime designed by employers, legislators and ideologists to cheapen human labor in every way (Thompson 2009, 346). As a result of these years of struggle, the '"average" English working man became more disciplined, more subject to the productive tempo of "the clock", more reserved and methodical, less violent and less spontaneous' (451). This happened in part through the influence of disciplining industrial practices and in part through the influence of Methodism and other religious forms. Still, the working class community was the product of 'neither paternalism nor of Methodism but in a high degree of conscious working-class endeavour'(457), which is to say that the working class community had their collectivist values and their own moral code. The *third* and final main section of the book studies the underground years, when working class radicalism was a 'defensive movement', and asks whether there was a discontinuity with the early years of radicalism.

There are many sub-sections, which occasionally seem to advance the main argument very little and which appear as factual 'diversions' in a positive sense into the investigations of some related historical phenomena. A case in point is Luddism, which is an integral part of the working-class radicalism of the early nineteenth-century and simply cannot be ignored. Luddism was a transitional movement towards a fully conscious working class and grew out the illegal tradition before 1811 as an eruption against industrial capitalism, which violated the accepted paternalistic legal code among artisans (601; 658). It is typical for historiography that the main argument is given depth by *descriptive* and *factual* parts of some independently interesting historical phenomenon. Furthermore, although my suggestion is that historians argue for points

of views, factual discourse is and remains an important part of historical discourse. If a historian makes a significant claim, designed to play a role in a chain of reasoning, the historian usually has to back up this claim with reference to empirical data. For example, when Thompson asserts that the roots of English working class lie in the dissenting movement of the late eighteenth-century, he has to describe and give information of what these movements were. Historiography is an empirical discipline after all.

Further, as already discussed, the last section of the book makes the point that the English working class developed underground gradually and was 'made' by the mid-1830s. Because of the often secretive nature of the working-class movement, this section contains information on various related phenomena, such as the work of spies, provocateurs and working-class leaders as well as court trials in these decades. It is now the right moment to jump to the postscript of the book, which summarizes retrospectively the main argumentative line followed:

> What happened in this 'making' was twofold. First, there was a shift in the whole background, as well as in the minority foreground, of popular dispositions…second, from 1816 onwards…men were putting themselves into a new stance in relation to other social groups and were developing new solidarities. (938)

When I write about the argumentative nature of historiography, the point is not to suggest that historians use formal argumentative strategies and that their main mode of writing resembles explicit arguments of the type made by analytic philosophers. The point is, however, to suggest that they nevertheless advance a central thesis and that such theses are made reasonable through the reasoning displayed in their books, which almost invariably contain long descriptive sections. If the descriptive sections are called 'narratives', one runs a risk of seriously devaluing the work of most historians since the connotation of that is that historians merely report given chronological events.[20] Before the historian finds the final organization of a book, a great deal of analytical work is required. As Marwick observed, 'devising such a structure, and carrying it through successfully, is one of the most difficult tasks of the historian. This structure – finally represented in the table of contents, the organization of chapters and sections of chapters – determined the special form that a piece of historical writing takes…any substantial piece of historical writing will have to have more than just organisation or a plan – a "structure"' (Marwick 2001, 207–208).[21] The use of the

terminology of 'narrative' may be seen acceptable as long as one understands and explicates that 'narrative' amounts to a certain cognitive and synthesizing structure in the book. Nevertheless, I do not think it is the best term to describe the structure of historical works.

If I now needed to summarize the argument of Thompson's *The Making of the English Working Class* in its bare bones, I would compose the following list of premises leading to the conclusion: (1) Certain dissenting movements and the traditional idea of the 'Free born' Englishman pre-conditioned the sense and need for activism among workers at the end of eighteenth-century; (2) The industrial revolution made the living conditions of most working-class people insecure; (3) External economic pressures and previous activism molded the feeling of shared class consciousness; (4) The consciousness of a working people's shared predicament continued to develop during the underground years and working-class culture assumed several separate forms in those years. (Conclusion) By the mid-1830, English working people had developed a fully mature working-class awareness of their place in the battles of society.

Reasoning in Christopher Clark's *Sleepwalkers*

Finally, let us see, what we can learn of reasoning and the argumentative structure from a close reading of Clark's book. One could say that the rationale for writing *The Sleepwalkers* is to persuade its readers of the thesis that the main players in the development leading to the Great War did not understand the consequences of their acts or that singular acts formed a chain of ultimately unintended consequences. This chain was not, say, a result of conscious policy decisions by one or another of the great powers in the war. The question is, how does Clark attempt to persuade his readers of the appeal of this thesis?

On the most general level, it can be said that the entire book constitutes evidence and grounds for believing the thesis. But it would be erroneous to claim that his book is essentially a narrative, if by 'narrative' we mean a presentation of events in a temporal succession (with or without causal links between them). This is easily seen by studying the structure of the book, which reveals that the book contains many non-narrative parts. This is most evident, for example, in the way that Chapter 3, 'The Polarization of Europe, 1887–1907', is organized. The central claim in the chapter is that the polarization of Europe's geopolitical system was a precondition for the Great War, but that the bifurcation into two alliances did not itself cause the war. On the contrary, Clark claims that

polarization into two blocs both muted and escalated the conflict. The chapter is designed to explicate how the polarization came about and this explication is realized by answering four interlinked questions: 'Why did Russia and France form an alliance against Germany in the 1890s? Why did Britain opt to throw its lot in with that alliance? What role did Germany play in bringing about its own encirclement by a hostile coalition? And to what extent can the structural transformation of the alliance system account for the events that brought war to Europe and the world in 1914?' (Clark 2012, 123). The chapter is thus systematically organized and the subchapters answer these questions collectively. There is no narrative structure here, if that means describing events in terms of what happened before and after. The subchapters go back and forth in time in their treatment of different questions and countries. And the past itself does not automatically raise these questions either.

I will mention another example. Chapter 4, 'The Many Voices of European Foreign Policy' investigates where the real decision power lay in pre-war Europe by studying each main world power in turn. Clark investigates whether power was in the hands of monarchs, ministers, the military or the press and public opinion. And this builds on some earlier suggestions in the book and other literature that there were some fateful decisions or other underlying factors involved, such as Germany's decision to build a navy or general anti-German feeling, which channeled Europe to the path to a global war. The fact that Clark deals with these kinds of questions, thus anticipating criticism, reveals *patterns of reasoning* in his book.

All in all, these examples show how a book contains non-narrative parts. It would be possible to bring forward various others. For example, there is no 'narrative' or other necessity to deal with questions about the nature of Europe's geopolitical system or power structures and bases. These are not descriptively forced by the temporal order of events, and further, the exact set of power factors is not determined by anything independent of the historian. Clark's set of choices does not contain, for example, an 'economic sub-structure', something that a Marxist would certainly add to the list of potential factors. In brief, they are *argumentative choices* made by the author.

This is not to say that the book would not contain descriptive, or narrative, parts too. It clearly does and some are fairly long descriptions of a certain sequence of events. Yet the crucial question is, what is the governing function of the book? Is it just a report on a certain sequence of events in the realist mode or is it designed to 'make the case' for a particular thesis? Since the thesis that Europe's main players were

'sleepwalkers' is not written in the historical sources and even less in the events themselves in any direct, unmediated and evident manner, it cannot be just a matter of reporting what 'there is'. More importantly, when one analyses the various components, one realizes that the narrative parts are there, because they play some kind of evidential role as evidence to believe the main thesis. The narrative–descriptive parts are in this sense subservient to the central thesis.

Although narrative and argument have been often seen as incompatible (see Chapter 2), some others have suggested that 'narrative form itself is a highly persuasive mode of argumentation', implying that there are other 'modes of argumentation' available (Partner 2013, 503). My proposal is to view narrative(s) as an *explanatory part* in the main argument. Why, for example, does the book contain the 'narrative' and factual description of the meeting between Serbian Prime Minister Pašić and the Austrian Foreign Minister Berchtold mentioned above? It is not simply to report 'how things happened' or to 'tell a story'. The purpose is to illuminate both the personalities and the precarious nature of international relations and decision-making, especially Austria's, in pre-war Europe. And these characters *exemplify* the state of affairs of 'sleepwalking' uncharacteristically well. One of the points in Clark's book, which may separate it from its various predecessors, is its assumption that individual agency and personality played a crucial role in the developments that resulted in the First World War. He writes that 'this book…is…saturated with agency…the outbreak of war was the culmination of chains of decisions made by political actors with conscious objectives, who were capable of a degree of self-reflection, acknowledged a range of options and formed the best judgements they could on the basis of the best information they had to hand' (Clark 2013, xxvii). Provided this is so, it makes sense to describe some personalities and their lives at length, as with Nikola Pašić (16–19), or to narratively depict the key events of some negotiations in detail, such as the meeting between the President of France Raymond Poincaré and the Czar Nikolai II (e.g. 438–451). In other words, the description of these incidents adds to the persuasive force of the sleepwalking thesis.

Conclusion

It is true that books of history typically imply an underlying time-dimension, but it does not follow that such a book necessarily has to be organized chronologically. Often books of history have to contain and mention a series of important dates and events, such as Thompson's

description of the Luddites, but no chronological narrative or a set of events is self-evident or dictated by the past itself. To repeat, they are *argumentative choices* made by the historian and it is my suggestion that the *choice is made in light of the main thesis and its defense*. Naturally, books of history come in many forms and with varying degrees of perfection. Some are more explicitly reasoning and argumentative than others and some are more successful than others in tying and justifying different elements together. The fact that in some cases it might be difficult to decipher the structure and the links between different parts is not a counter argument to the suggestion that the main rationale is to reason in favor of and make argumentative cases for historiographical theses. The problematic cases may be samples of bad implementation or the books in which the ratio of 'narrative' or 'reasoning' parts may be have been stretched to one extreme.

A central lesson here is that premises or grounds in historiography are more diverse than those of a standard understanding of these concepts in courses of logic and reasoning. Historiography is about argumentation in a looser sense than that of a clear set of premises and conclusions. It is about proving or giving reasons to accept certain general points or theses. It is about establishing points about the past. There can be many kinds of reasons to accept a thesis: reasoning from premises, a (narrative) description of the state of affairs, exemplification, statistics, etc.

It is time to take stock of what has been said in this book so far. I have argued that the narrativist insight that the written accounts of history contain some kind of unifying structures is correct. However, my view of these structures is crucially different in two respects. First, the central theses that synthesize a study of history do not amount to indivisible wholes. Most importantly, the structural analysis of historiography does not support the implication that all the statements in a work of history define the thesis. In addition, holism would make the standard picture of understanding and language-learning impossible, implying that understanding, sharing thoughts and language-learning require a total overlap of beliefs and meanings. I consequently rejected holism and suggested that it is necessary to distinguish the meaning of a thesis from the evidence for it. Nevertheless, this is *not* to say that the whole of a book of historiography does not matter. Sometimes holism is defended by throwing the ball back to the critic of holism by asking the question, why would anyone either write or read entire historical volumes, if the point argued for can be expressed in condensed form (e.g. Ankersmit 1990, 286–287; cf. Mink 1970)? The answer is that the central point

is indeed normally understandable without taking all the elements of the book into account, but this does not mean that the non-meaning-constituting elements have no role to play. They have an evidential role and that is why the whole books and texts matter and, further, why the whole book is the primary cognitive unit in historiography. Many of the elements are there to support the main thesis in some way and therefore, if one wishes to know what the thesis is and what the grounds are to support it, one has to read the entire book. Second, the fundamental organizing principle of written historical accounts is not narrativity but the argumentative support provided for the central thesis. In my view, the rationale for writing books of history is to persuade readers to accept the view that is put forward in the book, not to merely report historical events in a chronological sequence. In other words, historiography is reasoning for a point or a point of view. This thought takes us once again to the question on the nature of historiography.

Rejecting holism means in effect abandoning the suggestion that historiography creates products akin to artistic artefacts. The proposal that the main rationale for historiography is to provide argumentative support for the main thesis entails that historiography is a form of rational practice. Some might see this as a suggestion that historiography is a scientific activity. In some ways this may be a correct impression, although it is too early to form a definitive judgment as there are important differences too. In any case, it is clear that the framework of rationality is different from the narrative–descriptive account that, if understood in the realistic mode, could form a ground to claim that historiography is a science of the particular, that is, a subject that provides as accurate descriptions of particular events located in time and space as possible. However, in order to acquire a more satisfactory answer to what historiography is, it is necessary to consider how historical theses can be evaluated. How to decide whether a rational–argumentative practice is successful? Traditionally, an *argument* is taken to be composed of several connected claims for a conclusion or conclusions that seem to allow truth-functional evaluation. Is this the case also in historiography? Can the central theses in historiography be true if we operate in the argumentative framework?

6
Colligation

This chapter focuses on one of the central themes of the narrativist philosophy of historiography, constructivism, as outlined in the previous chapter. More specifically, the analysis centers on ontological constructivism, that is, the question of whether historiography creates and adds something that is not given in historical reality. With regard to this, the most relevant and interesting type of historical knowledge is colligatory – knowledge which collects and integrates first-order information under unifying expressions.

It should be said at the outset that I agree with narrativism that historiography is constructivist in the above-mentioned sense. Having said this, it is important to again remind the reader that I do not agree with narrativist views regarding the central function of historiography and the role of unifying expressions in historiographical texts. In other words, my disagreement with narrativism concerns the suggestion that texts of history form undecomposable wholes and that they should be characterized as representations. These two themes, representationalism and holism, were already studied in the previous chapters.

Colligatory concepts in the philosophy of historiography

One unifying thread shared by the pre-narrativists and narrativists is their attention to the synthesizing expressions used in historiography. In Chapter 2 many suggestions regarding these were examined. William Walsh's *colligatory concepts* were briefly mentioned (in footnote 5). Danto wrote about *narrative sentences*, which involve 'an inexpungible subjective factor' (Danto 1968, 142). In addition, Danto suggested that the historian creates *temporal wholes* that connect temporally separate objects (Danto 1968, 248, 255). Mink spoke of *configurational modes*

that create historical comprehension by presenting separate things as 'elements in a single and concrete complex of relationships' (Mink 1970, 551). And Morton White claimed that narration must have a *central subject* of which the narrator gives 'a connected account of the development' (M. White 1965, 221). M. White also wrote about the *colligatory power* of the statements included in a narration (M. White 1965, 257, 263–264). Further, Ankersmit linked together three notions that all have some kind of integrative function in historiography: images/pictures (of the past), colligatory concepts and *narrative substances*. They amount to a synthesizing thesis or view of the past contained in a book of history. Finally, Hayden White's *tropes* unify texts and give them a meaningful plot. Various examples of synthesizing expressions have also already been presented, such as the 'Cold War,' the 'Renaissance' and the 'Industrial Revolution'. It would be easy to expand the list. For example, Behan McCullagh mentions David Hackett Fischer's thesis that the central theme of American history is that it is about the growth of liberty and freedom over the centuries (McCullagh 2008, 152). The notion of 'Jacksonian democracy' (e.g. Benson 1961, 329), which refers to a specific political movement towards democracy in the early decades of the 1800s, led by Andrew Jackson, is an equally good example. And so is the 'sense of imperial mission' that was used to describe the unarticulated governing purposes and goals of Victorian Britain (Walsh, 1958, 61–62).

The main idea behind all these notions of the pre-narrativists and narrativists is that they integrate units of information to form something new and to thus create novel historiographical information, which cannot be thought to have existed before this act of creation. The question that I am primarily interested is two-fold. Can such expressions be conceived of as true in any representational sense of historical reality? Do they refer to some entities in the past, for example? Can they correspond to the historical reality? Could we see them as natural classifications of the historical world? Secondly, can such integrative expressions be justified? And, more specifically, would it feasible to see them as justified even if they could not be viewed as true or referring? These are important questions since it is naturally desirable that historiography would not be just about arbitrary figments of literary imagination, and that historiography not be a field that lets imagination reign totally free and unconstrained without any cognitive constraints.

I will adopt the terms 'colligatory concepts' and 'colligation' from now on to designate the synthesizing expression in historiography. 'Colligation' appears to be most fitting as it means 'to tie, group or join

together'. It is also sufficiently technical and specific a term to be used in the discussion on historiography without the danger of overt generality. Next, I will study in more detail what colligatory concepts are. This begins with an examination of William Walsh's writings on colligatory concepts since the term originates in his writings in the philosophy of history and historiography. After that examination, I will introduce and study two examples of colligatory concepts in historiography, those of the 'Thaw' and the 'Christian expansion.' After the analyses of these colligatory concepts, I will evaluate, whether they could be true and justified due to their referential capacity and as classifications of the historical world. My conclusion in this investigation is negative, which will provide an incentive to consider other empirical and non-empirical ways for justifying colligatory notions in the following chapter.

W. H. Walsh on colligatory concepts

The term 'colligation' derives from the first self-identified philosopher of science William Whewell. In his *Philosophy of Inductive Sciences* (1847), Whewell develops a more sophisticated account of induction than that of simple enumeration, prevalent in the years since Francis Bacon. In this process 'colligation' has a central role. 'Colligation' is an 'act of thought', which brings a number of empirical 'facts' together by 'superinducing' upon them a conception that integrates and makes them in this way capable of being expressed by a general law. Colligation provides the 'true bond of Unity by which the phenomena are held together', writes Whewell (Whewell 1847, 46–48; see also Snyder 2012). An example could be the known data points of the orbit of Mars and their colligation under the conception of an elliptical curve. We might also note that while Whewell saw that one may choose among several colligatory concepts, he insisted that their choice is not arbitrary. Colligations are chosen through a 'special process in the mind' that involves some kind of inference or inferences (Snyder 2012).

W. H. Walsh borrowed the term 'colligation' from Whewell and applied it in the philosophy of history and historiography. The earliest paper that Walsh wrote about colligation in historiography is his 'The Intelligibility of History' from 1942. The process of colligation is part of an attempt to discover intrinsically related coherent wholes from historical 'facts'. Similarly to Whewell's conception, the historian '"colligates" different events according to "appropriate conceptions"' (1942, 133), the functioning of which can be illustrated by historical phrases such as the 'Industrial Revolution' and the 'Enlightenment'. In his *Introduction*

to Philosophy of History, Walsh explicitly relates the concept of colligation to the issue of explanation in history. When the historian is asked to explain a specific phenomenon, such as the British general strike of 1926, the historian tries to trace connections between that event and others with which it stands in an 'inner relationship'. The underlying assumption is thus that different historical events can be seen as 'going together to constitute a single process, a whole of which they are all parts and in which they belong together in a specifically intimate way' (Walsh 1958, 23; similarly 59–62).

In his subsequent critical comments in 'Colligatory Concepts in History', Walsh remarks that his earlier use of 'colligation' was part of the attempt to find a plausible version of an idealist theory of history. The concept was used to integrate historical events that are not 'externally connected' (Walsh 1974, 134; but cf. Walsh 1942, 134) via an explanation that sees an agent or a group of agents pursuing a long-term policy over a long period of time, creating an 'internal link' between diverse phenomena. 'Colligation', says Walsh now, refers to an essential part of the historian's *interpretative* process in which the historian is confronted with a mass of material that seems unconnected at first sight – but the historian 'goes on to show that *sense* can be made of it by revealing certain pervasive themes or developments' (Walsh 1974, 136; my emphasis).

How can a historian reveal 'pervasive themes or developments'? According to Walsh, the historian specifies what is significant in events by identifying those aspects that *point beyond* themselves and connecting them with other events 'as phases in a continuous process' (Walsh 1974, 136). Colligating is to organize, and Walsh suggests that this is something that any type of historiography just has to do (Walsh 1974, 137).

The recognition of the subject-sidedness of colligation is important. Especially in his later writings, Walsh emphasizes the interpretative and subjective nature of colligation, as opposed to Whewell, who seemed to think that facts can be colligated correctly and naturally, that is, objectively, on the basis of induction. For Walsh colligation and the principles it implies mean that the historian adds something 'non-objective' to the historical reality.

Two examples: The 'Thaw' and 'Christian Expansion'

Many examples of colligatory concepts, as seen in the literature in the philosophy of historiography, have already been mentioned: the 'Renaissance', the 'Cold War', the 'Enlightenment', etc. While the 'Cold

War' seems to be the favorite of Ankersmit, I have chosen the 'Thaw' as my first detailed example of a colligatory concept in historiography.

The 'Thaw' as a historiographical concept refers to the period in the Soviet History from the mid-1950s to the early years of the 1960s, when the Soviet leader Nikita Khrushchev initiated the process of de-Stalinization. It is often seen to have 'officially' begun after Khrushchev's 'secret speech' in 1956, in which he denounced the personality cult of Stalin as well as Stalin's policies. The 'Thaw' is seen as a period in which politics on many fronts and especially the cultural atmosphere of the Soviet Union in general changed and warmed from the 'freeze' of Stalin to Khrushchev's 'Thaw'. The 'Thaw' is characterized by various features in historiographical discussions, such as the easing of repression and censorship in publishing, the release of prisoners from the Gulag labor camps, the politics of peaceful co-existence with the West, the improvement of relationships with China and Yugoslavia, the creation of cultural contacts with previously hostile countries and economic reforms. Sometimes, also the symbolically significant event of removing Stalin's body from Lenin's mausoleum is understood as a manifestation of the 'Thaw'.

The term 'thaw' itself derives from Ilya Ehrenburg's novel *The Thaw* (1966). Given that it gave the name to an entire historical period, it is remarkable that the book itself is a story of private lives in the post-war Soviet Union. It is also noteworthy that Ehrenburg was a Stalin prize-winner and had become famous for his role as a wartime propagandist. The book culminates in the spring thaw that transforms the freeze of the preceding winter. It thus symbolizes transformation and change, although the primary reference is to a change in emotions, from repressed feelings to a new openness and the expression of love. At the end of the book, the factory chief designer Sokolovsky undergoes this kind of change in mood:

> Everything was all at once alive and resonant.
>
> Funny thing: now Vera will come in, and I'm not even thinking of what I'll say to her. I won't say anything. Or I'll say: 'Vera, the thaw has come'. (Ehrenburg 1966, 164)

Several pages later two youngsters, Tanechka and Volodya, are shown wondering what has happened:

> Anything can change the mood. Any nonsense. I saw Sokolovsky yesterday. Do you remember I used to tell you about him? A gloomier guy I've never met. Well, yesterday I arrive and there he is, laughing,

joking, talking. I even asked him what has happened to him. He said: 'Nothing. It's the spring'. He must be close to sixty. How many times has he seen the spring? If that's what you call a miracle, then I believe in miracles.

No, I'm not talking about the weather. It can go much deeper than that. You'll meet somebody, and you really fall in love. Or you'll begin to work and find that you're absorbed in it. (Ehrenburg 1966, 169–170)

Ehrenburg's *The Thaw* thus operates primarily on the symbolic level. However, it also contains elements that made it daring and tested the limits of censorship under Stalinism, as seen for example in its veiled reference to the 1930s mass deportations and to the anti-Semitic 'doctors' plot' when Jewish doctors had allegedly been planning the assassination of Stalin. The factory manager Zhuravlyov can also be seen as representing Stalin, and his removal in the novel as referring to the start of the historical period of the 'Thaw' – although it is necessary to add that Zhuravlyov is a rather mild version of the Soviet ruler. He is described as a bureaucratic and colorless leader with a mildly suspicious mind rather than a despotic dictator with chronic paranoia. Nevertheless, he is the one, as is Stalin in Soviet culture, who prevents the arrival of the spring thaw in the atmosphere of factory and in the emotional life of Zhuravlyov's wife Lena.

It is useful to look more closely at how the 'Thaw' is used in the discourse of Soviet history. The following is of course only a small sample of all the relevant discussion on the topic. Despite this, it is enough to illustrate how the 'Thaw' has been understood and applied.

The widest application of the term is adopted in general historiographical discourse of the post-Stalin Soviet Union. For example, a Wikipedia entry on 'Khrushchev's Thaw' at the time of writing begins with the words: 'Khrushchev's Thaw…refers to the period from the mid-1950s to the early 1960s, when repression and censorship in the Soviet Union were reversed and millions of Soviet political prisoners were released from Gulag labor camps, due to Nikita Khrushchev's policies of de-Stalinization and peaceful coexistence with other nations'. While a Wikipedia entry naturally does not amount to professional historiographical writing, it reflects well the general historiographical discourse and understanding of the 'Thaw'.

Professional historians tend to be more limited in their scope of application. In his book *Russia: A Complete* History, Peter Neville writes that, 'Under Khrushchev, the second phase of the *cultural* thaw began' (Neville 2003, 220; my emphasis). The first 'thaw' occurred immediately after the

death of Stalin. Although John Keep, in his *Last of the Empires: A History of the Soviet Union 1945–1991,* does not explicitly categorize the period after the death of Stalin as the 'Thaw', he prefers a close relative: 'de-Stalinization' (Keep 1995, 48), and he also uses familiar 'thaw'-language to characterize the Soviet culture and its transformation at that time. Stalinist political culture is described as 'the post-war cultural freeze' (Keep 1995, 24). Indeed, Keep writes that there were 'several successive "*thaws*" and "*freezes*" in the field of literature' (Keep 1995, 122). Further, the attributes given to Khrushchev's post-Stalin era have a familiar ring: 'In some way the period 1953–1964 was an optimistic era, marked by a relaxation of police terror and an improvement in living standards [and] … educated society was just beginning to recover from the ravages of Stalinism' (Keep 1995, 2; similarly 63). Stalinism is characterized as the period of closed archives. According to Keep, the historians of that period were little more than propagandists (Keep 1995, 30). It was a time when literature and the arts were permitted only to convey the message that Party and People were 'unbreakably united in a mortal struggle against wily foes' (1995, 27) and the culture of fear was common in politics (Keep 1995, 34). Succinctly, Keep calls Stalin's last years 'The Dark Ages'. If one pays attention to the kinds of issues that are mentioned concerning Khrushchev's optimistic era, one finds a very diverse list. To name but a few: the appearance of the controversial poem, 'Stalin's Heirs', by Yevtushenko in Pravda, the publication of Solzhenitsyn's *One Day in the Life of Ivan Denisovich* (mentioning the Gulag) (Keep 1995, 60), the emergence of new styles and genres in literature (Keep 1995, 120), the 'civilizing' of Soviet Government (Keep 1995, 64), the release of prisoners from the Gulag and their rehabilitation (Keep 1995, 76), the birth of a (partially) consumerist economy (Keep 1995, 85), the decentralization of industrial management (Keep 1995, 91) and the devolution of authority in agriculture (Keep 1995, 105).

The primary category of post-Stalinist historical reality that Peter Kenez leans on in his *A History of the Soviet Union From the Beginning to the End* (1999) is 'The Age of Khrushchev'. In agreement with the two authors discussed above, he uses the terminology of the 'Thaw' to refer to the transformation of Soviet intellectual life after the death of Stalin, while leaving the term 'de-Stalinization' to designate wider societal changes. According to Kenez, during the period of the 'Thaw', 'The Soviet Union ceased to be a totalitarian society' (1999, 191). More generally, 'October 1964 marked the end of a period of relative optimism, a period during which many people inside and outside of the Soviet Union believed that the flaws of the system could be remedied' (1999, 212–213). Beyond

this, Kenez associates many customary features and events with 'the Age of Khrushchev,' including the fact that 'millions returned to their homes…in most cases receiving rehabilitation' (1999, 193), the creation of 'decentralized' economic ministries (1999, 202) and the emergence of a new theoretical underpinning of 'peaceful co-existence' in foreign policy (1999, 203).

There are thus two uses of the term 'Thaw': broad and narrow. While the 'Thaw' has broadly come to signify a period of hopefulness and reforms after Stalin's death in general historical discourse and perhaps also in some professional historiographies, the narrower understanding of the 'Thaw' as a cultural liberation seems more common among professional historians. It is safe to conclude that the 'Thaw' has in any case become part of the standard historiographical language used to describe the period after Stalin's death, or aspects of it. In actuality, its ontological *status* has solidified so much that it can nowadays even be talked about as a causal factor triggering other developments and changing the minds of people: 'The thaw appears as a catalyst that mobilised and temporally fulfilled the young generation's inner expectations. The warm winds of liberalisation created a sense of purpose in a destabilised world' (Petrov 2008, 184).

I will introduce my other example of a colligatory concept, that of 'Christian expansion',[1] more briefly. The concept of the 'Christian expansion' is itself a colligatory concept that refers to the expansion of Christianity in the early decades and centuries of the first millennium, which has given birth to many other interesting colligatory concepts. In the background, as a kind of default option, there is Edward Gibbon's 'demystification' of the Christian expansion. He contradicted his predecessors who had explained the success of the expansion with reference to the superiority of Christian revelation. Gibbon lifted the veil of supernatural mystery by distinguishing five reasons for this success: the 'intolerant zeal' of the Christians, the promise of immortality, (alleged) miracles, Christian morality and the Christians' superior organizations. Many others have continued this historiographical discourse on the same basis and suggested alternative ways of understanding the Christian expansion. One way might be called the 'flashlight conversion' model, which explains the expansion through 'a sudden reversion of character through a completely new understanding of one's role in the universe, achieved as if in a flash of blinding light' (Drake 2005, 5). Another alternative uses the idea of social networking and yet another the metaphor of the marketplace to give an account of why Christianity was so successful as a rival against other religions and pagan

groups. The metaphor of the market place compares churches to 'religious firms', which raises new questions and a need for studies on the comparative advantages of Christianity. Further, Drake highlights the nature of Christianity as a mass movement, which in effect emphasizes that Christianity was heterogeneous, had low entrance requirements and was unstable. This brief study shows that one generally accepted colligated historical phenomenon, the Christian expansion, generates a number of alternative further colligations to account for the nature and causes of that phenomenon. Drake concludes that models likes these are meant to be useful for 'their ability to refocus thinking in potentially fruitful ways, and not because they are more "real"'(2005, 7). Indeed, the propensity of colligatory notions to 'refocus' thought is central; they open new avenues for investigation and provide new 'meanings' and ways of understanding the past.

What is remarkable in the historiographical use of colligatory notions is that they manage to colligate seemingly very diverse phenomena under one label. In the case of the 'Thaw', such historical phenomena as the publication of the magazine *Amerika* in the Soviet Union, the release of prisoners and Khrushchev's visit to China are subsumed under it. Now I move to the question of how a single concept can do this. What justifies colligatory practice?

Reference and colligation

The central question in this section is whether a colligatory concept can be an accurate representation of historical reality. The concept 'accurate' implies something like a faithful representation or a correspondence to facts. In Chapter 4 the notion of representation was already discussed at length. This discussion showed how Ankersmit has argued against the copy or resemblance theory of representation, and I think he has been correct to have done so. However, while he bases his argument on the nature of the central theses (that is, narrative substances) of history books, my focus is on colligatory concepts, which however leads to the same overall conclusion about the status of historiography: historiographical (re)presentation cannot be a faithful copy of historical reality. My argument for this conclusion can be summarized as follows: (1) historiography cannot do without colligatory concepts; (2) colligatory concepts are not objectively given and do not refer to corresponding entities in historical reality; (3) the truth of a statement in the sense of correspondence requires reference; (4) therefore, historiography cannot be true in the correspondence sense. It is time to spell out the premises.

Why do we need colligatory expressions? The pre-narrativist philosophers highlighted many indispensable functions that these expressions serve in historiography, as discussed above. To suggest eliminating them amounts to requesting a fundamental renewal of historical language. This smacks of arrogance to say the least. With what right could philosophers ask historians to change their language, which appears to be well-functioning and warranted? (cf. Tucker 2004, 138). The excessively ambitious project of the early logical positivists springs to mind; they wished to do something similar with the language of science and reduce it to directly observational expressions. More importantly, removing colligatory concepts from historiography would neuter its language and mean losing the most interesting and powerful features of historiography. Historiography is full of colligatory concepts: 'Antiquity', the 'Renaissance', the 'Baroque', the 'Enlightenment', the 'Second World War', 'Finlandization', the 'Cold War', the 'Industrial Revolution', the 'Scientific Revolution', etc. Concepts like these are loaded with meaning and are most colorful and useful in our attempts to make the past intelligible. Historiography would be much impoverished without them. They are an inherent part of historiographical discourse, as Walsh stated. L. B. Cebik writes that colligation 'is simply the way historians…go about making assertions about events and other sorts of things' (Cebik 1969, 57). It is reasonable to conclude that currently it is unavoidable, and more, even desirable, to accept colligatory language as historiographical language. To suggest otherwise would amount to little more than philosophical hubris.

It is somewhat more difficult to show why colligatory concepts are not objectively given and cannot be true in the sense of correspondence. I will begin from the idea that they could be literally true of the historical world. It is clear from the outset that this is not a promising approach. For example, it would be fanciful to suggest that at the time of Stalin's rule the world was literally frozen and began to thaw only when Khrushchev assumed power. I am sure that Soviet citizens experienced many warm summers, as well as cold winters, during the years of Stalin's reign! Colligatory expressions are typically metaphorical, or at least not self-evidently descriptive.

In order to consider the problem above in a less figurative way one may inquire whether colligatory terms (terms that denote colligatory concepts) refer to some entities in the external world. Let us consider a statement containing a colligatory expression: 'The Cold War was dangerous'. Does the 'Cold War' in the sentence refer? It seems very odd to think so. It is worth clarifying that 'reference' is understood here as in the case of proper names, which refer to individuals, and thus

provides a kind of default understanding also in discussions that focus on theoretical terms in the philosophy of science. The name 'Barack Obama' refers to one individual only, namely to the person who is the president of the USA in 2015. What would be a *particular* to which the 'Cold War' refers? Colligatory expressions do not seem to instantiate any individual – they do not seem to correspond to any singular object in the historical world.[2] As discussed earlier, colligatory concepts seem to be like shorthand for organizing historical data. They tie, group or join objects together. They are thus unifying expressions. If this is so, it is necessary to ask where the organizing principles that underlie colligations come from?

A useful way to address this question is to ask whether an organizing principle is object-sided or subject-sided.[3] More conventionally, one could ask whether an organizing principle is objective, thus of the object, or subjective, thus of the subject. Can the principle be somehow reduced to the historian-independent historical world? Can we see colligatory organizations as being 'natural' in the sense that perhaps elements in the periodic table are? Is a principle, such as a certain shared quality, contained in the objects themselves, as is the case with 'natural kinds'? Often membership in a natural kind category is seen to be determined on the basis of possessing certain essential qualities. For example, samples of water have to have a certain molecular structure (H_2O) in order to qualify as 'water'.

It seems obvious that this strategy is not going to work with colligatory notions and with instances they subsume in historiography. The objects that the 'Thaw' subsumes under it can be very different, such as the publication of Solzhenitsyn's *One Day in the Life of Ivan Densovich*, greater tolerance for humor in what is said and published, and the release of prisoners from the Gulag. It is difficult to see anything 'natural' in putting this group together in exactly this way to suggest that only the 'Thaw' can colligate them correctly. There are no essences or even any obvious shared qualities. Now, one might be tempted to suggest that, if a definition of colligated objects by a set of shared necessary and sufficient conditions (that is, possession of exactly the same set of properties) does not work, then perhaps one could try a family-resemblance definition. The idea is, in other words, that although colligated objects might not have any properties in common, they resemble each other. However, this does not seem to help either. If yet a few more objects that may be potentially subsumed under the 'Thaw' are added, such as economic reform and foreign policy visitations, the reason becomes evident. All these objects are very different and it would baseless to claim that they

all resemble each other due to some given set of object-sided qualities. It is worth adding that even if one were to spot a shared property among all the objects under a certain colligatory concept, one could not from this infer that it provides the only correct 'natural' classification of the objects. The lack of essences, similarities and differences between objects form, in principle, an endless array and source for categorizations. This is to say that many colligatory arrangements are possible without anyone them being uniquely privileged.

Colligatory classification

It may be argued that colligatory classification is characteristic to historiography in general, that is, also in cases in which there is no obviously identifiable colligatory term or corresponding colligatory concept. It is worth quoting Goldstein at length on this. The question he ponders is whether one could see the contested conclusion and event that Norsemen reached North-America in pre-Columbian times, in 1362, as reflecting the 'natural order' of the past:

> The pieces of evidence which, at the close of the inquiry, we see belonging together – the stone, artifacts found in Minnesota and in Scandinavia, all texts bearing on the interpretation of the linguistic material, the documents bearing on the Paul Knutson expedition and the dissatisfaction with King Magnus Erikson in Norway – are brought together not because they naturally belong together, that any suitably trained scholar could see that they belong together, but by the nature of the investigation as it is pursued to its proper conclusion. Should some scholar find reason to dispute the conclusions to which [Hjalmar] Holland [in *Norse Discoveries and Explorations in America, 986–1362*] comes, we would most likely find in his work a somewhat different ordering of evidence: presumably some of Holland's evidence would be grouped with other evidence not deemed by Holland to be relevant to his purpose, and others of it in other ways. That is, there would likely *not* be some natural ordering of the data to which all sides of the dispute might appeal for impartial judgment, but, rather the dispute of the scholars and the rivalry of constituted historical events would involve, as part of the very nature of the dispute, disagreement over the arrangements of the evidence. (Goldstein 1976, 59; similarly 131)

Cebik expressed colligatory organizing as follows: 'The colligation of events (and/or conditions) x, y, and z *as* a Q allows one to see x, y and

z as one could not see them before, that is, logically prior to the colliga-tion. Colligation adds something but not new empirical information. Rather, it adds...a conceptual framework, a kind of discourse' (1969, 45; cf. Dray 1959, 406). The considerations above convey the message that the *organizing principles* of colligatory concepts are not 'object-sided,' not 'natural,' which suggests that they must 'subject-sided,' imposed by the historian. Perhaps we might say that colligatory concepts form 'nominal categories' in the sense that the concept (or associated term) is not much more than a nametag attached to objects. This would imply that histo-riography and colligatory concepts entail nominalism, an idea that has indeed been put forward by Ankersmit on various occasions.[4]

The debate between nominalism and its opposite, universalism (and essentialism, as it is sometimes seen), has a long history in philosophy and a parallel (but much younger one) in the philosophy of science. The classical nominalist commits to the view that the world is a world of individuals or particulars only, and therefore, denies the existence of the universals (and in some versions the existence of abstract objects) that the universalist thinks are needed to explain our talk of *kinds* of objects and properties. The universalist by contrast claims, for example, that red things are red by virtue of their being instantiations of a universal 'redness' and gold things gold due to their being instantiations of the natural kind 'gold'. According to the nominalist, the classification and use of (natural) kind terms do not require invoking any other entities beyond the individuals that fall in the classes and kind categories.[5]

It appears 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.' Is historiog-raphy thus nominalist due to its colligatory language? This is a correct conclusion insofar as 'nominalism' means nominal *postulations,* that is, that colligatory arrangements are not natural, given or provided by the object (the past). However, the discussion of nominalism and realism often focuses on *kinds*, a central aim being to decipher what concepts are natural kinds, and on what grounds. The problem with regard to colliga-tory concepts is that it is far from clear that it would even be correct to see colligatory concepts as being any kind of *kind concepts*? Should we say that the 'Thaw' is a category or a set whose extension covers all 'thaw'-like objects? Alternatively expressed, are the objects subsumed under the 'Thaw' *kinds of* the thaw?

If some of the examples of colligatory concepts are considered, a crucial difference to kind concepts emerges. First, colligatory concepts are not taxonomic, while kind concepts are. A certain individual animal is a

German Shepherd (and a dog) because it is a kind of 'German Shepherd' (and 'dog'), on the basis that it shares some, perhaps essential, features with other kinds in that category. And the same taxonomic principle applies to 'planets'. Although 'planet' is a nominal kind, and thus has no natural essence, planets are nevertheless *kinds* of planets. They all share the feature of traversing around the sun. German Shepherds are kinds of dogs and the Earth and Mars are kinds of planets, but a certain painting and a book are not kinds of the Renaissance. 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.[6] This would make sense only in second-order cases, in which we classify colligatory concepts themselves, such as different concepts of the 'revolution': the 'Bolshevik revolution', the 'French revolution' and the 'English revolution'. In that case, different 'revolutions' may perhaps be expected share some common features and differ in some other respects. However, one has to rely on the ready-made and understood category 'revolution'.

There is also another difference to kind concepts, the importance of which cannot be exaggerated. The point is that colligatory concepts are not general concepts, but *individuals* in themselves, which regardless organize and subsume other individuals (events, objects, people) under them,[7] as in the case of 'Renaissance' paintings, sculptures, practices, scholars, etc.[8] Tucker is incorrect in suggesting that there is no difference between the use of colligatory notions and theoretical concepts (Tucker 2004, 138). The 'Renaissance' and the 'Cold War' name two unique periods in history, and thus have clear and restricted temporal and spatial references, while theoretical concepts apply to a large set of phenomena, which is perhaps even infinite in some cases. In the first case, the suggestion is that there was a period called the 'Renaissance' in history, with some beginning and end (1400–1700), within a certain geographical area (Europe), and which was manifested in various ways in cultural products, practices and thinking. As such, it was unique; there is no other 'renaissance' in history (secondary meanings such as 'neoclassicism' refer to separate unique events). This is philosophically peculiar, as general concepts are normally assumed to do such organizing.

McCullagh has nevertheless argued that colligatory concepts could be seen as general, not as particular, which indicates that all subsumed objects have some common features.[9] His examples are 'revolution', which entails that some form of radical change occurred in the historical phenomena, and the 'Renaissance', 'which refers to a collection of events inspired and directed by a set of ideas and values ... of a general

kind' (1978, 272). McCullagh is correct in claiming that 'revolution' seen in this way is general, implying that the 'French revolution', the 'English revolution' and the 'Bolshevik revolution' all have something in common. Yet, as discussed above, these are 'second-order' categorizations, that is, the categorizations of historian's language, once that discourse is first in place. One 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. The essential question is whether 'revolutionary change' can be inherent in the events themselves that are colligated to form a specific whole, and revolution. Let Cebik provide an answer to this. He argued that x, y and z, which are colligated under Q, lack a common feature: 'painting, inventing, sculpting, writing, *et al.*, in no way equal a renaissance, nor do any of the actions have a *discernible* feature we might term "renaissant"' (Cebik 1969, 46–47). Indeed, what would such an inherent feature of 'renaissant' or 'revolution' be in all the parts of a colligated whole? I believe that to claim that the subsumed historical phenomena all share such a common feature in the case of a specific revolution would commit one to some kind of teleological conception of history where the parts with a specific inherent feature determined and pre-figured development towards a telos. In Finnish historiography, seeing all the pre-1917 governmental events (such as Finland's autonomy within the Russian empire, its own money, its own postal system, etc.) as a preparation for independence, in this sense as sharing an 'independence feature' realized on December 6, 1917, has been aptly called 'key-hole' historiography (Jussila 2004, 15) because the past is perceived from a narrow retrospective perspective.

McCullagh is thus concerned with the use of certain general concepts, already colligated, and not with the way in which colligatory concepts are constructed and applied to historical *data in the first place*. If one thinks about historiographical language, it is of course true that common nouns, proper names and many other types of expressions are used. However, the issue at stake is how a historian constitutes and justifies colligations and what the relation of a colligation to the historical data is. Although the postulation of revolution implies some kind of change, and more specifically, that the events colligated amount to a revolution, it does not follow that there was some kind of 'revolution property' inherent in and shared by each event. Colligation is a synthesization by the subject-sided historian and the result of the historian's reasoning.

Ryan Shaw aptly says that there is no template that historians could use to determine whether a certain set of phenomena should be called a revolution or something else (2013, 1094), although, when the judgment is made, the historian postulates that those phenomena colligated under 'revolution' amount to some form of radical change. The 'magic' of colligation is exactly that it enables one to put together a diverse set of events under one concept.

It is instructive to pay attention to Walsh's talk about 'parts' and 'wholes' when he analyzes the relationship between colligatory concepts and the events they colligate. It follows that the events and phenomena that a given colligatory concept organizes are not *instances* or *members* of it in the conventional kind-category sense. It is more appropriate to talk about the entities (events, phenomena, objects) as *constituting* the 'Renaissance' in this particular interpretation. Insofar as there is the 'Renaissance' they form it. On one occasion, Walsh compares colligatory concepts to Hegel's idea of the *concrete universal*, understood as the thought of something as 'a unity in diversity' and a 'complex particular' (Walsh 1974, 143–144). Both are excellent expressions.

An important qualification is required before moving on. It is reasonable to say that although members of a colligatory concept category are not kinds of that colligatory concept, they need to *exemplify* it or its sense. There is thus this one feature or principle that has to apply to all subsumed entities in order for the colligatory category to be meaningful. This is where the hand of the historian and her valuation is felt, as the historian chooses what to illuminate. The highlighted aspect forms an invisible thread that keeps the entities of the category together, even though the feature or the category itself is in no sense 'natural'. It is possible to view all objects forming this new holistic entity as a Venn diagram or a circle drawn around objects. The objects themselves may not have anything else in common beyond the sense imposed on them, as we notice if we consider the entities subsumed under the 'Thaw'. They are very dissimilar, which explains why the notion of family resemblance did not help either. Colligations are not based on similarity and dissimilarity postulations in any obvious sense.

Colligatory concepts provide an entirely new approach in comparison to traditional theorizing on concepts and kinds in the philosophy of science. This approach, if not unique to historiography, is in any case characteristic of it. The term nominalism in the classical sense is not directly applicable in the context of colligatory concepts. The thought behind the postulation of colligatory concepts is not to deny the existence

of universals. It is true that historiography deals with individuals and is therefore compatible with nominalism, and also that colligations are nominal postulations without an assumption of natural essential qualities, but the main point is not to argue for nominalism against universalism. With colligatory concepts we are dealing with something that does not have a direct predecessor in the debates of the philosophy of science (with the exception of Whewell).

Another qualification to be added is that even if colligatory concepts cannot be true of historical reality, this does not mean that some other statements could not. This only means that historiographical language and its most interesting part cannot be true in the sense of correspondence. Secondly, there are other conceptions of truth that might be employed instead of the correspondence notion. I will discuss this theme more specifically in Chapter 8, but two points are worth mentioning already. If one swaps the correspondence notion of truth to, say, an epistemic concept, one changes the subject, and this does not alter the point that the language of historiography and the historical reality are incongruent. Moreover, my view is that it is more fruitful to speak about *epistemic authority* than truth in this context since the idea of correspondence is only one way of attributing epistemic authority to a claim or a view.

Conclusion

It is now time to draw some conclusions from the above discussion. The best way to do so is to first provide a definitional summary of what colligatory concepts are. Colligatory concepts: (1) *organise* lower-order data into higher-order wholes; (2) categorize *without* any necessary *shared features* or resemblance among sub-ordinated entities; and (3) are *particular*, that is, deal with phenomena restricted to a specific time and place.[10]

It was concluded that colligatory concepts cannot refer directly to the historical world since they do not have corresponding counterparts there. And 'counterpart' should here be understood widely, as covering objects, entities, processes, structures and tendencies. In other words, colligatory concepts do not 're-present' any given aspect of the past or refer to unique individual entities. One way to express this conclusion is to say that if we identify colligatory concepts as potential truth-bearers, they do not have truth-makers in the historical past, and cannot therefore be true or false. Further, we might state that there cannot then be a congruence or isomorphism between colligatory concepts and

historical reality. On the level of colligatory concepts, anti-realism rules. In Chapter 8, I will continue the discussion of the problems of truth and correspondence in a slightly more formal way.

Before moving on, it is worth noting in passing, however, that I do not accept the idea shared by Ankersmit and Hayden White that there is a qualitative difference between singular truth-functional statements and narrative non-truth-functional ones. The full explanation for this has to wait until next chapter, but the central reason is that my analysis of what historiography fundamentally is differs from theirs. In my view, the presentations of history do not form holistic units, although I do accept that whole books cannot be reduced to a set of singular state-ments either. My suggestion is that there is no clear demarcation line but instead a sliding scale, according to which all works of history can be located somewhere on an axis of the subject-sidedness and object-sidedness. Actual historiographies typically contain elements from both sides and the 'objectivity' of historical knowledge depends on the specific combination of these kinds of elements. For example, colliga-tory concepts and colligation entail constructivism and anti-realism with respect to historical knowledge, and this is what they share with nominal kinds. But, as noted earlier, this does not necessarily apply to lower-level language and expressions. I am thus not arguing for a full-blooded semantic anti-realism in historiography either. Non-colligatory expressions are subject to the same semantic problems and possibilities (in terms of their reference, predicates, vagueness and truth-values) as any 'literal' and straightforward statements about the world. This is an issue that needs returning to later in the following chapter.

This takes me back to the issue addressed at the beginning of this section: do we need colligatory concepts? Or rather could we do without colligatory concepts? I already argued that colligatory expressions amount to the most interesting and useful type of historiographical language. It is possible to find agreement regarding this even among scholars who adopt an otherwise very different approach. Arthur Marwick, who understands historiography as a 'purely empirical' (2001, 4) discipline and firmly believes that it produces cumulative knowledge, nevertheless arrives at the following conclusion: '"Periodisation," the breaking up of the past into manageable epochs or periods, is simply an analytical device: the periodization that is useful for political history may well differ from that useful for economic history, and once again from the periodization that is useful for social and cultural history.' He adds that it would be 'ridiculous' to treat these 'analytical devices' as

having 'some ineluctable materiality' or 'inherent reality' of their own (Marwick 2001, 9–10; 53; similarly 207). I agree. Despite (or perhaps due to) their constructive nature, historiography purified of colligatory expressions would be much poorer, and much less expressive. It would resemble a chronology of low-level observational statements, which arguably could not be said to fit our idea of proper history writing.

7
Underdetermination and Epistemic Values

'There is no higher knowledge, or hot-line to the gods, which tells us that the Renaissance, or for that matter the Enlightenment, actually took place' (Marwick 2001, 67). These words of Marwick might be said to capture the message of the previous chapter. It was concluded that colligatory concepts are constructions without counterparts in historical reality. From this it follows that it is impossible to justify their construction, and thus to credit them with epistemic authority, on the premise that they merely reflect what there is in the past. And this is so both in terms of reference and in terms of categorizing, 'carving nature at its joints', as Plato is said to have put it. Does this mean that colligatory concepts are mere random figments of the imagination? No, it does not. It is one thing to say that we cannot justify their existence in a historiographical discourse in the framework of (ontological) realism and quite another to claim that they have no cognitive warrant whatsoever. The challenge is therefore to spell out what it is that could justify the construction of a colligatory concept, provided that colligatory expressions are an indispensable part of historical scholarship. This is the task of the present chapter. First, I will consider whether it is possible to empirically justify colligatory concepts as uniquely correct either inferentially from data or as correct in light of data *after* the construction of a colligatory concept. Again, the answer will be negative. For this reason, most of the chapter concentrates on finding another kind of rational justification with the help of epistemic values.

Empirical vindication?

Although a colligatory concept is not a copy-like *re-presentation* of historical reality, it could still be taken to be justified if one could reduce its

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content to historical evidence. However, it is immediately clear that this brings one to a dead end since colligatory concepts go beyond empirical data. And this is their most important feature. Colligation is a uniting of seemingly separate data into a whole, and the principle that unites is not inherent in the data. The concept of the 'Thaw' subsumes heterogeneous material under it. and the organizing principle – the atmosphere of relaxation and 'thawing' in comparison to Stalin's freeze – cannot be said to be found in historians' empirical evidence in any unmediated manner.

Imagine a historian collecting evidence from the period of 'Khrushchev's Thaw': the historian might be reading archive material on the numbers of Gulag prisoners released and notice that many were freed – certainly more than when Stalin was in power. He or she might also note that books and magazines that would most likely have been censored earlier were allowed to appear. The historian might find some notes concerning discussions in the Politburo regarding the limits of what can be published and remark that Khrushchev favored a more liberal line. Then he or she might browse newspaper stories about the Soviet leaders' trips to previously hostile countries like Yugoslavia and the USA as well as about return visits by cultural delegates from these countries. And so on.

There is no prospect that the historian would ever come across the concept or idea of the 'Thaw' in this very diverse material[1] and certainly no possibility that she would stumble upon a unifying link between phenomena in the source material itself. The dream of such direct empirical inference resembles Baconian naïve inductivism. We can safely conclude that the 'Thaw' does not emerge 'from below' in any direct or unmediated manner. Detecting a specific 'pattern' in historical data and colligating them under the 'Thaw' requires some extra-evidential factors. Indeed, the idea of the 'Thaw' seems to imply another metaphor, the 'freeze' of Stalin. On the basis of that, exactly as the idea of colligation implies, the historian may construe the unifying principle of 'thawing' and impose it 'from above' on historical material.

What if one considers the question of empirical justification from the other side? In other words, assuming we already possess the concept of the 'Thaw', is it reasonable to think that historical data vindicates it as a uniquely applicable colligatory concept? This also seems unlikely. First, there are strong general grounds to doubt that any theoretical construct or abstraction can be uniquely determined on the basis of empirical evidence alone. The thesis of the underdetermination of theory by data is one of the key lessons from the twentieth- century philosophy of

science (Quine 1953). It is worth paying attention particularly to what has become known as 'contrastive underdetermination' rather than to 'confirmation holism' or 'holist underdetermination'.[2] The former says that a body of evidence that confirms or justifies a theory can equally well confirm a number of alternative theories. In other words, one cannot be expected to choose and find the correct theory from among a series of alternatives on the basis of empirical evidence alone. Contrastive underdetermination is sometimes illuminated by the following abstract example. Imagine a data set of points drawn on a surface. How many possible lines could we draw between them? The answer is that it is possible to connect the points in infinitely many ways. If we add more data points, we eliminate many curves, but infinite possibilities always remain.

Contrastive underdetermination means that there is always a logical possibility of alternative theories: although we may not be aware of an alternative theory, it is always logically possible to construe or imagine an empirically equivalent theory. In other words, even if we did not have an alternative colligatory concept to 'Thaw' for the period after Stalin's reign, this does not mean that no other relevant colligatory concept is conceivable and equally warranted in light of the historical evidence. One might object that the 'data' in history research are rather different to the 'points on paper' of the analogy, however. Almost any historical 'data' contain a wealth of information and meanings, and are thus not as radically open as 'data' in the physical sciences. But I think this conclusion would be a red herring.

If one thinks of an archival document concerning the release of prisoners, the books and magazines published in the 1960s, the discussion of the members of Politburo or even actual newspaper stories from the 1960s, one is reminded that they all are loaded with information and suggest practically unlimited lines of interpretation. Indeed, the abundance of information only emphasizes the problem of underdetermination since unlimited lines of interpretation remain open even if many of them seem implausible. One might say that, with respect to any one interpretation or colligation, historical evidence is *radically underdetermined*: due to the nature of historical evidence and the historical imagination, it is always possible to construct an infinite number of alternative interpretations.[3] The openness of interpretation can be illustrated with the example of a list of prisoners in the Gulag, a very simple document in comparison to many others in historiography. What can be concluded on its basis? Or more generally, in what kinds of reasoning can it be used as evidence? Arguably in an endless array of inferences: the number of

prisoners, the sex of the prisoners, the nationalities of the prisoners, the nutrition of prison camps, the quality of ink used by prison guards, the management of prison camps, the system of archiving, the politics of the Soviet regime, the 'Thaw', etc. There is really no end to interpretative lines. And this further supports the central idea of and need for colligation: in order to make sense of historical evidence and view the past as containing meaningful patterns, a historian needs to highlight some aspects and link historical phenomena into colligated wholes.

In contrast to the Quine–Duhem thesis on underdetermination, Tucker (2004) argues that it is possible to use evidence to determine theories, save perhaps in some stubborn historiographical cases. Further, Tucker suggests that we distinguish between *determinists* ('historians infer from evidence with historiographic theories and methods a single historiographical "output"'), *indeterminists* ('whatever consistency and regularity we find in historiographic judgment result from political, ideological, or socio-historical factors that influence groups of historians') and *underdeterminism* ('historians are constrained by the evidence and their theories to choose among a finite range of possible historiographies') (Tucker 2004, 9). This requires some comment. First, Tucker does not address 'determinism' *per se*. Neither does he talk about 'evidential determinism' – that is, that evidence alone determines historiographical interpretations, which one might expect in a discussion of the underdetermination thesis. He is interested in what might be called 'cognitive determinism', as it is both evidence and theoretical matters in the fashion of good old internalism that determine the 'output'. I think that evidential determinism does not hold and I am also agnostic with respect to cognitive determinism, but do think that it would valuable if one managed to develop a theory of cognitive determinism. Second, one does not have to be exclusive with respect to cognitive factors and extra-cognitive factors. It may be the case that they both have a role to play in scientific (and historiographic) decision making, and this state of affairs would be a disaster only for the 'Old Rationalist' (See Bird 2000, 3–9), that is, the one who believes that strong algorithmic kinds of rationality principles alone determine the content of science. If both types of factors have a role to play, it is not possible to develop an algorithm to show how theory choices are determined, but we would have a set of rational and other criteria at our disposal nevertheless. This means that 'cognitive indeterminists' do not have to think that cognitive factors, such as evidence, have no role whatsoever in theory decisions. They only need to think that solely cognitive factors do not determine the output. Furthermore, Tucker's indeterminists are not indeterminists if

they think that political, ideological and socio-historical factors determine output. This is a common misunderstanding among philosophers of such schools in science studies as the Sociology of Scientific Knowledge (e.g. Bloor 1991), which leans strongly towards determinism and talk of causality but denies that evidential and cognitive factors decide outcomes (they also leave room for both social and observational input). Third, a more appropriate understanding of underdetermination would be to say that historians are able to choose among (logically) infinite, not finite, possible historiographies, although this stretches the meaning of 'choice', as all historiographies are not available in the same already-formulated manner.

Tucker also claims that there is a discrepancy between the actual history of science and the kind of history that the Quine–Duhem thesis would lead us to expect because scientists have agreed to prefer some theories over others. On this basis, he concludes that 'the evidence…from the historiography of science is sufficient to disprove Duhem–Quine's underdetermination thesis' (2004, 145). This is a hasty conclusion, however, as the core of the underdetermination thesis is that empirical equivalence between various theories with respect to shared evidence is compatible with there being other rational or non-rational reasons for preferring some theories over others. It is strange that Tucker draws his conclusion because he also recognizes both empirical equivalence as the essence of the Duhem–Quine thesis and the possibility of extra-empirical criteria in theory-choice. Tucker admits that some 'parts of historiography are underdetermined' because some disputes have remained unresolved for a long time (2004, 151). But it is similarly odd that he both bases this conclusion on empirical evidence and suggests determining the extent of underdetermination in historiography empirically. The underdetermination thesis is a logical claim regarding the relation between theories and evidence and empirical investigations can, at best, make it more or less plausible. Naturally, if one theory appears to be more strongly supported than its rivals and manages to widen its evidential scope more than any other, for example, this provides a pragmatic reason to choose it in that situation. This state of affairs does not show the underdetermination thesis to be right or wrong, however. To show that it is wrong, one would need to prove that there is logically only one possible theoretical construction from a given set of evidence. But so far philosophers have not managed to find any such proof. In other words, if Quine and Duhem are correct, even if scientists had only one theory, this does not show the thesis to be wrong, since there may be yet unimagined empirically equivalent and more justified theories.

Consensus may simply reflect a lack of imagination in a community or perhaps the existence of repression (which Tucker has well analyzed).

The justificatory role of epistemic values

It is important to realise that *empirical* equivalence in the form of the underdetermination thesis does not mean *evidential* equivalence, as there might well be other rational, non-empirical means to choose between alternative constructions. Nor is it the case that the historian's proposal is either undisputably justified or unjustified (cf. Cebik 1969, 54). I suggest that justification in historiography is something more subtle than such polarity entails. Next, I will outline a theory of justification in historiography, which is modeled on the justification of colligatory expressions and suggests some ways in which one could make rational choices between them.

Above we considered the construction of colligatory concepts from a logical point of view. In other words, the question was that of whether a colligatory concept can be uniquely justified with respect to historical evidence, and the conclusion was that this is not possible. Whether this tells us anything about the actual construction of colligatory concepts is another issue. More specifically, do historians actually construct colligatory concepts on the basis of 'historical data'? Or are they constructed in some other way?

This contrast between the logical point of view and actual construction resembles the classical philosophical distinction between the context of justification and the context of discovery. Popper famously remarked that mysteriously appearing or dreamt ideas – such as Kekulé's dream of snakes biting their tails as representing the structure of benzene – may well turn out to be correct despite their odd context of origin. In other words, it is one thing to ask whether a construction is justified with regard to evidence and quite another to ask how someone arrived at such a construal.[4]

White, Ankersmit and the early narrativists all suggest that synthetizing expressions, tropes, narratives substances, colligatory concepts, etc., are imposed on empirical data. The temporal order runs from construction to application to data. And they have good reasons to say this. When one considers how colligations or other synthesizing expressions emerge, one notices that the marching order is typically from the historian and historiographical discourse to source material, and not the other way round. Michael Oakeshott observed that 'History [that is, historiography] ... begins not with a collection of isolated particles of

data, nor with a universal doubt, nor with a blank and empty conscious-ness, but with a homogenous world of ideas...And the work of the historian consists in the transformation of this world...in the pursuit of coherence' (Oakeshott 1966, 98). Above, I suggested that the concept of 'Thaw' must have been constructed against the idea of Stalin's 'freeze' because otherwise 'thawing' would not make sense. And how was the concept the of 'Cold War' born? Was the term coined and applied by a single historian on the basis of historical evidence acquired while in the archives? This is not the case. The term 'Cold War' appeared in a news-paper article by George Orwell (1945) at the end of the Second World War, and thus before the beginning of the Cold War, which is often viewed as having commenced in 1947. Walter Lipmann's book *The Cold War* (1947) made the term more widely known, after which it spread to general political and historiographical discourse and came to nominate the whole historical period. This shows that the choices of language and interpretation take place within a framework of the existing social situa-tion and discourse. Further, the concept of 'Renaissance' was by no means inferred from the historical data, but constructed by Michelet (1855), after which the term has become part of *parlance* of historiography.

It would not be wrong to suggest that historiography *resembles a priori* plotting of the past in one specific sense: integrating expressions are not outcomes of empirical investigations of source material but precede their application to that material and are thus *prior* to empirical work. Would this mean that historiography loses its status as an empirical discipline that studies what actually happened in the past? This is a worthwhile query. The problem is that if a colligatory concept implied by the histo-rian or inferred from historical discourse bears no relationship to histor-ical data, historiography really begins to look like the feared random figments of imagination. On the other hand, one might note that the context of creation ('discovery') does not necessarily have bearing on the context of justification. In the remainder of this chapter I wish to argue that *a priori type* of plotting and empirical justification are in no way incompatible.

This is a good moment to return to Ilya Ehrenburg's novel *The Thaw*. Given that it is the birth context of the colligatory concept with the same name, it is clear that the concept did not emerge as a result of direct consultation of historical evidence. Its context of origin is in literature. The 'Thaw' illustrates well the *a priori* nature of colligatory concepts with regard to historical evidence. It really is imposed on data and used to colligate data from above. Nevertheless, it has been successfully applied in factually based historiographical discourse. One might even

say that the 'Thaw' has become an integral part of the general historio-graphical talk concerning Khrushchev's post-Stalin period and that it is considered to be a meaningful expression on empirical grounds. What, then, can justify its application to empirical material? And, more generally, how can we decide how appropriate and fitting a chosen concept is? I propose that the five criteria and conditions below can be used in deciding the issue.

Epistemic values

Walsh writes that there are two conditions that govern the choice of using a particular colligatory concept. First, the concept must be 'tailored to fit the facts rather than a straightjacket'. This comes down to a requirement that generally accepted facts or statements about historical facts must be seen as supporting a colligation. A small number of disagreements do not show, according to Walsh, that a colligatory concept is defective. What matters is the overall support for the interpretative framework involving the colligatory concept (Walsh 1974, 139). The second condition is the question of how a concept *illuminates* the facts, which is the issue of how its 'use makes the past real and intelligible *to us*' (Walsh 1974, 140; my emphasis). The idea is that, given that the 'facts cannot speak for themselves', the historian has to organize the past and make it understandable through some synthesizing interpretations and concepts. Remember that earlier Walsh talked about how a set of facts become intelligible *in the light of* or through colligatory concepts by constructing a 'significant' narrative of the events (e.g. Walsh 1958, 62). In brief, the historian has a double duty: do justice to evidence and to readers. Colligatory concepts must be chosen in such a way that neither of these aspects is forgotten. Presentations of history should not be mere lists of factual statements without any communicable coherence, nor vulgar abstractions without any factual support. I will elevate the idea of casting new light or illuminating historiographical *data*[5] to the status of the first criterion:

1. **Exemplification**: The descriptive content of a colligatory expression has to exemplify the historical data it subsumes.

In other words, the historian may use a familiar set of data as material for the colligation and, in this way, give a new meaning to this part of the past. What did Ehrenburg's metaphor of the 'Thaw' communicate? The idea of a thaw and its contrast to freezing implies the thought of

a warmer and happier period. Indeed, in the novel, Ehrenburg's 'Thaw' refers to people becoming happier and freer due to a change in the intellectual climate that enabled them to think and act more spontaneously without constraints and a need for pretending. This seems to transfer well to the history of the Soviet Union. The 'Thaw' translates to people gaining greater freedom to think and act as well as to the process of relaxation of control in various arenas of life in post-Stalinist society. The historian wishing to use the concept 'thaw' would naturally therefore refer to events and phenomena that exemplify this sense, as already discussed above.

The implication of this is that the descriptive content of a colligatory expression must appear as appropriate with respect to the historical data, which entails a minimal requirement of truth. The colligated statements describing historical data should be assumed to be true, unless they are colligatory statements themselves. On the other hand, what appears as appropriate is relative to what each feature is seen to represent in the historian's own time. If we understand the release of prisoners as a form of liberation, and the release really happened (truth-requirement), then the 'Thaw' may be said to describe well what took place in the Gulag in the Khrushchev years. And if we interpret the Soviet leaders' visits to the West as friendly gestures, and they actually made such visits insofar as we can tell, these can similarly be used to illustrate the 'Thaw' in Soviet history. To emphasize this point, the sense of a 'warming climate' is not inherent in the historical data, but the imposed sense must nevertheless be seen to illuminate that particular episode.

The idea of 'exemplification' can also be elucidated by an illustration from McCullagh (2008). Although I think he is wrong to claim that the historian could discover the 'French revolution',[6] he provides a useful example. McCullagh writes that Lincoln's outlawing of slavery as well as the Congress' outlawing of some forms of racial segregation and granting the right to vote can be seen as a process of increasing the freedom for African-Americans. Would any contemporary historian dispute that? But this is nevertheless not an automatic and natural judgment. For example, we need to assume a certain sense of freedom and think, for example, that voting increases freedom. It is contentious whether voting in the Soviet Union increased the freedom of workers in the same, or indeed even in any, sense. The fittingness of the colligation thus depends on the analysis and judgement of the historical material. Given our cultural conceptions, this specific freedom hypothesis seems justified when applied to American history.

It is also important to recognize that there is always a vast amount of historical source material that could be selected and colligated:

notebooks, archived public records, various types of printed material, public monuments, memories, material artefacts, etc. The historian can naturally select only a small number of all the potential data to examine and report, even at the best of times. As a consequence, it is almost always possible to find contradictory material for a given colligatory concept in any period investigated. For example, it was not the case that all unjustly sentenced prisoners were freed in the post-Stalin era. And at least some of Khrushchev's seemingly liberal acts with regard to publishing were performed for tactical reasons: to underline the difference to the 'criminal' era of Stalin while upholding and encouraging a sympathetic interpretation of the Soviet state and its leaders. Would it be appropriate to describe the internal climate as a 'thawing' if the seeming liberalization was just a tactic to support the new regime and keep a tight grip on the population? In 1957, the Communist party reminded historians that it had no intention of tolerating 'liberal interpretations' (Mazour 1958, 244). Kenez notes that 'the writing of history was [still] strictly supervised' to evaluate the Stalinist past correctly, that is, to examine the past critically but not too critically in order not to delegitimize the post-Stalinist government (1999, 190). It is worth noting that the symbol of Khrushchev's 'Thaw', Dudintsev's critical *Not by Bread Alone*, was denounced as anti-Soviet in 1956, that is, in the early years of Khrushchev's reign. Further, Soviet tanks rolled onto the streets of Budapest with chilling effect in 1956. Would these phenomena be enough to freeze the thaw in historiographical *parlance*? We might also consider an entirely different colligatory concept, that of 'scientific revolution'. It makes sense to claim that scientific thinking changed distinctively in the roughly two hundred years beginning from the mid-sixteenth-century, when supernatural explanations were rejected in favor of natural ones and reason and experimental method replaced faith and dogmatism in science. However, as many historians of science have pointed out, this period demonstrates much continuity too, in some fields more than in others, and particularly in the form of experimental practice and instrumental application. Does this mean that this concept, originally applied to rotating wheels and to the wheel of fortune, is inapplicable as a colligatory concept to describe scientific change in these years?[7]

The point is not that it should be possible to find only one correct colligatory concept, or that no concept is apt, but that the historian has to make an interpretative choice that is intelligible in light of historical evidence. It is as Thomas Kuhn wrote that 'there is no such thing as research without counterinstances' (1970, 79) and as Imre Lakatos

provocatively put it, any theory 'at any stage of development, has unsolved problems and undigested anomalies. All theories, in this sense, are born refuted and die refuted' (1978, 5). Another way to express the point is to say that there is an infinite amount of potential historical data, but that there is no given, epistemically privileged set that a historian necessarily has to take into account. One might say that the selection of data is itself the historian's interpretative choice. Walsh proposed that some historical material could be allowed to contradict a colligatory concept, if that concept otherwise enjoys 'overall support'. This is correct, but it would be unwise for the historian to concentrate on highlighting contradictions or gaps any more than a scientist puts effort into highlighting known anomalies when proposing a new theory. The second criterion governing the application of colligatory concepts is therefore that the material highlighted should form as a coherent whole as possible, which thus entails that one should avoid contradicting the descriptive content of a colligatory concept.

2. **Coherence:** The material highlighted has to be chosen and constructed so that it forms a maximally coherent set.

It is important to realize that some colligations are more fitting and appropriate applications to historical data than others (without the expectation that they be absolutely correct). If some were not, it would be hard avoid the conclusion that all colligations are merely random figments of the imagination. The coherence condition requires that one try to maximize the 'fit'. That is, to show that there are inferential connections between the elements of data or even that they constitute a unified whole. This is related to another virtue: an attempt to colligate as much historical data under a concept as possible. Thus the third criterion of application is:

3. **Comprehensiveness:** The concept that applies to a larger amount of historical data than its rival on the assumed historical phenomenon is preferable.

If a historian suggests that it would better to call Khrushchev's period the 'Cold Snap' or the 'Big Chill,' the historian would be confronted with and be required to explain a vast amount contradicting historical material, despite being able to adduce some supporting evidence as well. These concepts would be able to colligate less than the 'Thaw' and there would be more contradicting material to make them unfit as colligatory

concepts for this period. What if someone proposed that we call what is now known the 'Industrial Revolution' the 'Agricultural Revolution'? It would not be difficult to draw attention to the documentary traces of factories, industrial policies, products, workers, etc. in the historical record to question the applicability of this suggestion. This is not to say there may still not be better ways to account for this historical phenomenon or that there are no other potential 'analytical' ways, to borrow Marwick's term (2001, 4), by which to divide the period into some other segments.

Yet another standard that can be used to prioritize between colligatory expressions is the scope of application – not to evidence of a given historical phenomenon but to historical phenomena themselves. If a concept applies to, colligates, or makes intelligible a large area or amount of historical phenomena, making the events and objects appear as one coherent item, the colligatory expression has a large scope of application. A large scope in turn makes it a powerful colligatory concept and can be a reason to prefer it over other concepts.[8] This gives us the fourth criterion.

4. **Scope**: Everything being equal, a colligatory concept with a larger scope of application to historical phenomena is preferable to one with a more limited scope.

This is a virtue that cannot be applied in isolation from the previous three, since a large scope cannot compensate for a colligatory concept if it only poorly exemplifies the historical evidence it is intended to highlight, for example. Furthermore, a large or narrow scope of application cannot be seen as an unambiguous reason for preferring a particular colligatory notion because that choice ultimately depends on the historiographical rationale of a given work. Nevertheless, colligatory expressions often have a very wide scope of application, which increases their appeal, as with the 'Enlightenment'. What is gained through a wide scope of application may, however, be lost in specificity since a very broad colligatory concept may explain some local phenomena quite poorly even when it makes a large part of the past comprehensible (cf. Tucker 2004, 148; 152). The fact that different cognitive values and virtues may be in tension with each other has been recognized in philosophy of science from the beginning by Thomas Kuhn and others.[9] It is not possible to construct an algorithm to determine the steps to take to reach a uniquely correct justification; instead, there is always a trade-off between different epistemic values, depending on the aims of the historian.

Scope of application is related to the final criterion: originality. Although it is difficult to spell out how to judge some expressions as being more innovative and original than others, these attributes are undoubtedly virtues in historiography. While one should not expect to pin down any point-by-point criteria for innovativeness and originality, they have to do with the cohesiveness of the views on the past preferred. This judgment is also tied to our cultural norms and valuations in the framework of historical discourse. Given that it was already customary to talk about the 'Cold War', the concept of the 'Thaw' appeared intelligible in the existing framework, while it was also refreshing at the same time. The final, fifth rule is thus:

5. **Originality:** Everything being equal, a more innovative and original concept should be preferred to a more customary one.

As above with the fourth criterion, this fifth criterion cannot compensate for the problems with application to historical data itself. Chapter 10 continues the discussion on the significance of originality in historiography and its relation to objectivity.

These five criteria amount to a theory of justification, which outlines how one can judge and choose appropriate colligatory concepts. To repeat, these criteria do not yield us an algorithm that could be used to *determine*, with certainty, what to construct and accept and what not. Neither can they be used to identify uniquely correct concepts, but they do, nevertheless, amount to both empirical and extra-empirical criteria to be used in choosing and ordering colligatory concepts. Indeed, we are not dealing with absolutes but a ranking between more and less appropriate concepts. What is needed here is a change of discourse from truth-functional language to the vocabulary of comparativity and rational ordering. These five criteria are rules of thumb that can be used to compare and understand which colligatory expressions are fitting and justified in historiography in the absence of rules of correspondence between the historian's presentation and the past.

Conclusion

In this chapter, as in the previous one, I have examined colligatory concepts, which are arguably the most interesting feature of historiography and which integrate both historical data and historical phenomena into powerful and meaningful synthesizing views. My conclusions are, first, that colligatory concepts cannot be true of historical reality in the

sense of correspondence. Second, they cannot be seen as natural categorizations of historical reality. Third, colligatory expressions do not emerge from the historical record; nor can they be uniquely correct regarding any given historical data. Further, choices between them cannot be determined solely on empirical grounds, even though any colligatory expression has to be supported by empirical data. Finally, it is nevertheless possible to form judgments between rival colligatory expressions on the basis of empirical and extra-empirical rational criteria: exemplification, coherence, comprehensiveness, scope and originality.

Before moving on to the next chapter, it is necessary to briefly return to the problem of holism and ask whether historical texts amount to colligatory constructions. It was concluded above that colligatory concepts are wholes composed of lower-order entities. If texts were colligations in this sense, they would thus also be holistic entities. Also, it was shown in the previous chapter that the narrativists typically thought that a historical text is a narrative whole. Ankersmit would identify a text as a holistic colligatory construction.[10] Some scholars following the narrativist philosophers of historiography formulate the relation between colligatory expressions and historical texts even more directly. For example, Shaw writes that 'the text [e.g. *Decline and Fall of the Roman Empire*] is assigned the subject Roman Empire not because the grammatical subjects of its individual sentences refer to some thing called Roman Empire, but because the text as a whole constructs a representation that its creator has named Roman Empire…Each history expresses a unique colligatory concept' (Shaw 2013, 1097–1098).

I already questioned in the previous chapter the claim that texts are undecomposable wholes, which means that they could not be colligations either. Only rarely is the central message of a historical text that one should adopt a new colligatory notion. This is of course possible, and something that may have happened in Michelet (1855), when he suggested that the historical period after the Middle Ages should be called the 'Renaissance'. Notice, however, that it does not follow that the text in its entirety has to be colligated under one covering concept even in a case like this. Nevertheless, it is the case that historiographical texts *contain* colligatory expressions. Colligatory language is deeply embedded in the practice of historiography and constitutes some of the most interesting and expressive concepts in historiography.

Although I have provided some criteria that could be used in the evaluation of colligatory concepts, there nevertheless remains a challenging situation in terms of evaluation when historical statements include colligatory expressions. The good news is that a commitment

to historiography as having an argumentative structure does not constitute similar problems as to the commitment to 'representation', because the former can be seen as being composed of distinguishable statements and not of one undecomposable whole. But one has to also answer the question of how the whole text could be evaluated, given that it is the central cognitive unit in historiography. It is now time to move to the chapters in which a comprehensive solution for evaluation in historiography is suggested. A dimension of this solution is justification via epistemic values, as explained in this chapter.

8
From Truth to Warranted Assertion

The challenge in the remaining chapters is to outline a comprehensive theory of evaluation of books in historiography. However, before a positive theory can be laid out there is still more groundwork to be done and many problems need to be solved. I ended the last chapter by saying that a text is the main cognitive unit in historiography. It is therefore necessary to spell out how texts can be evaluated. It should be clear that I am talking about their cognitive evaluation, that is, the evaluation of historiographical texts as products of knowledge (cognition), and not, for example, with regard to their aesthetic qualities.

Now, the notion of a 'cognitive unit' is admittedly rather vague. It has been made clear that I do not regard a historiographical text as one whole. Instead, I suggested that they can be perceived as manifestations of reasoning and decomposable arguments for historiographical theses. A historiographical argument can be divided into the meaning component of a historiographical thesis and the parts that play an evidential role for that specific thesis. Both components are naturally cognitively important, but the distinction means that it is the theses defended in the works of history that matter most. An even more important consequence is that the reasoning (argumentation) component and the conclusion (thesis) component of a historiographical argument assume different epistemological roles, as a consequence of which different epistemic standards apply to them: one should not inquire whether a historiographical argument can be true since arguments are not expected to be true and false but are, rather, judged according to their form. In logic and the theory of argumentation it is asked whether arguments are valid or invalid, sound or unsound, etc. In the case of historiography, these requirements must be relaxed, but the focus should nevertheless be on the success of historiographical arguments as forms of reasoning.

The actual problem with regard to truth-functionality is the question of whether historiographical theses, the conclusions of historiographical argumentation, can be true. If not, why not? This provides us with the first two specific questions to tackle in this chapter. What does the truth of a statement require? And do historiographical theses possess the required qualities? My focus is on the correspondence theory of truth and the theory of truth-makers. After considering these questions, I introduce the notion of epistemic authority. At the end of the chapter, I explain what warranted assertion means. It is my view that successful historiographical theses amount to warranted assertions. The emphasis in this chapter is on the history of philosophy and specifically on what philosophers in the pragmatist tradition have said on these topics.

The correspondence theory of truth and truth-makers

The correspondence theory of truth is arguably the most venerable and oldest theory of truth. By some accounts it goes back all the way to the roots of Western Philosophy, to Aristotle and Plato. Most epistemologists, such as Nicholas Rescher (who nevertheless develops a coherence theory of truth as a 'criteriological theory') (Rescher 1973, 9), for example, agree that it is also the most intuitive theory for expressing the meaning of truth. David Armstrong writes that 'it is entirely natural to think that a proposition is true or false according as it corresponds or fails to correspond to an independent reality' (1997, 128). Further, Mandelbaum thinks that the correspondence theory is presupposed by all works of history (1938, 184).

We can understand the correspondence theory of truth as saying that a proposition or a statement is true if and only if the state of affairs stated prevail; in this way a true proposition or the statement corresponds to 'facts'. When I say that the statement 'there is a cup on the table' is true, it requires that there is indeed a cup on the table. But what does 'correspondence' mean more specifically in the correspondence theory of truth? Without going into all metaphysical intricacies, 'correspondence' could in general be understood as the intuitive idea that some factual elements in the world correspond to true propositions and thus *make them true*. In this sense, the correspondence theory seems naturally connected to the truth-maker and truth-bearer theory. Armstrong has suggested that 'the correspondence theory tells us that, since truths require a truth-maker, there is something in the world that corresponds to a true proposition. The correspondent and the truth-makers are the same thing' (1997, 128). He also claims that 'anybody who is attracted

to the correspondence theory of truth should be drawn to the truth-maker' (1997, 14).

The truth-bearer and truth-maker vocabulary is very useful for expressing what is epistemologically and metaphysically at stake in trying to establish the truth of a historiographical thesis. The general idea is that the truth of a historiographical thesis requires that there is a relation between a truth-bearer and a truth-maker so that the latter makes the former true. Further, the notion of 'truth-maker' captures the idea that the truth of something depends on how things are in an independently given reality. In other words, truth-maker T is in some sense in the world, 'a portion of reality' (Armstrong 2004, 5–6), in virtue of which '*that T*' is true. The intuition in Armstrong's words is that

> It seems obvious that for every true contingent proposition there must be something in the world … which makes the proposition true. For consider any true contingent proposition and imagine that it is false. We must automatically imagine some difference in the world. (Armstrong 1973, 11)

Now it can be understood why the idea that colligatory notions correspond to the past is problematic. The reason is not that the criterion of correspondence could not be applied to the actual practice of history, as many have argued (e.g. Goldstein 1976, 41). An epistemic problem does not mean that the correspondence theory could not capture what is at stake with truth-clauses in historiography. The problem is that there does not seem to be *any one thing* that would make a colligation true. Colligation is an arrangement, a construction, which does not have an independently given corresponding object. This is to say that even if a colligatory expression could be regarded as a potential truth-bearer, and I think it can, it does *not have a truth-maker that would make it true*.

However, perhaps I am advancing too quickly here. One might say that all the individual states of affairs combined make the thesis true. That is, if all the statements that are colligated under a colligatory expression are true in virtue of *their* truth-makers, then the colligation, a higher-order expression, is true. All these states of affairs together would function as a collective truth-maker for the colligation. In this way, it might be possible to retain the intuition that the truth depends on something external to it, yet avoid the problems associated with the correspondence theory. However, the problem with this suggestion is that even if all the statements were true and all the states of affairs described consequently prevailed, it would not be possible to derive a historiographical

thesis without some additional elements; without some subject-sided imposition that links all the descriptive statements together. To express this another way, the links, the relations between phenomena or entities and their significance, are not objectively given unless one is prepared to accept that the reality of truth-makers admits logically complex facts in the early Russellian sense.[1] I suggest that the metaphysical problems associated with this view are not insignificant. To see this, it is enough to consider what kind of 'complex fact' might make Clark's 'Sleepwalking' thesis concerning the First World War true.

But could one not see the lower-order statements used by Clark as directly referring and being true in the truth-functional way? Perhaps this is possible, but it does not change things since the problem is still that the truth of lower-order statements does not guarantee and, moreover, does not enable one to infer the truth of higher-order historiographical theses. The narrativist insight is in part based on this observation. And we have seen that especially Ankersmit underscored the qualitative difference and the absence of 'translation rules' between these two levels. Assume that all the statements that describe the route to war were true. This includes the statements describing the meeting between the Serbian Prime Minister Nikola Pašić and the Austrian Foreign Minister Leopold von Berchtold in 1913, British obliviousness of the proximity of war, the 'beehive' structure of Austrian decision making, miscommunication between the Kaiser and German officials, the idiosyncratic behavior of French ambassador to Russia, etc. This set of statements would not allow one to infer the 'Sleepwalking' thesis regarding the First World War. Only when the thesis is suggested can one come to appreciate that all the states of affair are *reasonably integrated* under it.

What an advocate of a truth-maker solution needs is an entailment from lower-order entities to the higher-order entity of colligation so that if lower-order entities are true they *entail* the truth of the higher-order entity. This does not seem to be available. There are no such inferential relations from descriptive statements to their colligation, as the discussion of the empirical determination of colligatory notions has already showed. To see this yet more clearly, let C be a colligated expression, which colligates a large number of statements describing historical events, such as p, q, w, z, etc. The simplest expression of this state of affairs is to say that C creates a world in which it is the case that p, q, w, z....n. The problem is to infer C from p, q, w, z....n, which is exactly the original problem of colligation, as expressed by Whewell and others: how to derive general or other integrative concepts from a set of data describing particular states of affairs? Without some subject-sided

imposition this does not seem feasible. It would, of course, be possible to go the other way round, from C to p, q, w, z....n; from the truth of a synthesized thesis C to the truths of statements that it entails. But this does not take one anywhere because the problem is that of how to establish that C is the case.

Where does this leave us with regard to historiographical theses? They are identified as conclusions of historiographical arguments displayed in the books of history; they are something for which the historian argues. A historiographical thesis is a statement in terms of its form and, therefore, does not raise similar problems with to regard to truth-functionality as a narrative, for example. That is, a narrative is typically considered to be some kind of holistic entity that does not allow decomposition into its component parts or the determination of the referring relations of those components. A statement, at least in principle, allows this. However, it is important to pay attention to what kinds of claims historiographical theses typically make. Let us consider the thesis that Europe and its great powers went to war like sleepwalkers. It should be evident that sleepwalking is a metaphor, and hence we should not expect it to have a truth-value. Instead, it is a suggestion for how the development towards the war should be seen. Further, describing the process as sleepwalking is a colligatory expression that subsumes a large number of lower-order statements, which describe various kinds of occurrences prior to the war.

The situation is still more problematic than this. First, it is not tenable to make a clear-cut distinction between a higher-order thesis and lower-order factual statements. Colligated statements themselves may contain colligatory expressions, thus making them non-referring as well. For example, the suggestion that Austrian decision-making was organized like a beehive with unclear hierarchical relations is a metaphor. Further, the meeting between the Serbian Prime Minister and the Austrian Foreign Minister does not appear to be a neutral description of a state of affairs, but conveys a sense or meaning too. It could perhaps be sub-colligated in terms of something like the 'meeting of the deaf', because communication between the two repeatedly failed despite numerous opportunities. Second, it is conceivable that another historian would draw a different conclusion from the same set of statements, provided that the two somehow ended up with or were given the same set of evidence (as that set itself is naturally the result of many selections). The narrativists emphasized the importance of *ordering* 'singular statements' in a consecutive or other narratively fitting order. In the case of 'sleepwalking', the statements take no specific consecutive order, except that they all support the 'sleepwalking' hypothesis. It would be better

to speak of the emphases and valuations of some descriptive statements, although even then inference from a lower-order set to a historiographical thesis is impossible.

If a historiographical thesis can thus be seen as a colligatory or/and containing colligatory expressions, it seems as if it amounts to a whole or is at least 'molecular' in Fodor and Lepore's sense (see Chapter 5). However, it is very important to make some qualifications. Colligations try to reach beyond the text to the past itself, even if they do not instantiate any reference there. One can say that a text makes the case for seeing the past in some specific fashion. Further, the text does not normally amount to one colligatory notion but, rather, specifies the meaning of the thesis and evidence for it. In other words, everything mentioned in a book does not necessarily fall under any one colligation; instead, different parts and statement play different roles. If the historian rebuts, for example, a potential objection to an interpretation or rehearses the possible issues that a reader should bear in mind, this does not make them part of the colligation. The structure of the text is different from the colligatory power and the function of the specific (including colligatory) expressions it contains.

Colligation should not be seen as *defined* by its lower-order elements. While it is true to say that the latter compose the former, the talk about definitions has a connotation of something like analytic meaning, whose identity requires the presence of one particular set. This is not the case with colligatory expressions. A historian may subsume the meeting between the Serbian Prime Minister and the Austrian Foreign Minister under the sleepwalking thesis in one case and not do so in another, and yet commit to the same colligation in both cases anyway. Or let me take the more mundane example of the colligation 'all TV-programs that I like'. It is possible that those programs have nothing obvious in common except the fact that I like them. This kind of colligation does not require that some specific programs are definitionally included. If a new program is launched on TV that I specifically like, and it thus ends under category 'all TV-programs that I like', the sense of the colligation has not changed. In this case, the TV-programs that I like compose the category but do not define what 'all TV-programs that I like' means. The same is true of the 'Renaissance', which may or may not subsume some specific paintings or thinkers and still retain largely the same sense. It may be the case that many of these categories are simply *inherited* from previous historians and by-and-large retain their meaning and functioning despite the fact that the borders of the subsumption are porous.

Epistemic authority and truth

The problem with truth-makers and truth-bearers is of course not necessarily the end of the road for someone who insists that we ought to view integrative historiographical theses as true. Perhaps 'truth'-claims can be explicated without them. One might think that truth consists of something other than correspondence. For example, it might be possible to take 'truth' as an epistemic notion and think that 'ideally justified' is the same as 'true'. Or maybe a deflationary definition can save truth. I discuss a pragmatist attempt to redefine 'truth' in more detail in the next section. Let me, however, suggest now that what is fundamentally at stake in these discussions is the *epistemic authority* without which scholarly historiography would not make much sense.

There is no doubt that the notion of 'truth' serves a pivotal function in many scholarly and non-scholarly discourses. This becomes painfully evident whenever it is suggested that something cannot be true in an absolute sense. Often these kinds of claims trigger accusations of 'postmodernism', 'nihilism', 'relativism', or an 'anything-goes' attitude. It is noteworthy that these accusations are mostly meant not as an opening for further philosophical analysis of what is at stake, but as signals of a disapproval of the view. It seems that this is also, in reverse, the case with claims that include the term 'true'. That is, truth-claims convey the message 'believe this', 'accept this', 'this is belief-worthy', etc. And the claim that is attributed with 'truth' is deemed epistemically authoritative and expected to be promptly assented to. Consequently, the person or institution that is seen to deliver truths is given an epistemically authoritative status. For example, Raymond Martin argues that it is the discovery of truth that historians are after. But then he says that 'discovering truth' means deciding among competing interpretations on the basis of reasons and evidence. What Martin does is to try to find a way to attribute an epistemically authoritative status to a historiographical interpretation, but he does this without providing any substantial content to 'truth'. The source of the epistemic warrant in Martin's case is in the rational warrant that an interpretation possesses (he writes about 'relevant reasons' and 'evidence') (Martin 1993, 29).

Epistemic authority is something that can be attributed to a cognitive entity such as a theory, a belief or a historical interpretation. To say that P is true is one way in which one can attribute such authority. To say that P is true because P corresponds to a fact would be one specific truth-functional way of providing epistemic authority to P. Epistemic authority thus

yields a principled reason to accept an entity as epistemically trustworthy and compelling. Tucker (2004) has suggested that belief formation should be 'uncoerced'.[2] Indeed, appropriate epistemic authority attributed to a belief should be compelling due to cognitive qualities without external coercion to accept the belief.[3] If all candidates were on a par, or none had a higher epistemic authority than another, it would not make any difference what to endorse. This would indeed be a case of the dreaded 'anything goes' attitude. In actual historiographical practice not all interpretations are taken as equally worthy. And, while it might be possible to discriminate between different interpretations also by other than epistemic means, the focus now is on cognitively principled discrimination, whether the principle can be provided truth-functionally or not.

It is important to make a terminological distinction at this point. The notion of epistemic authority has not been broadly discussed in philosophy but, insofar as it has, it typically refers to someone's personal authority, that is, to a person that is seen as an authoritative source to rely on in forming one's beliefs (cf. Zagzebski 2012). This kind of person is often called an 'expert' and discussions have led to a consideration of the role of testimony in epistemology. I do not refer to people when speaking of 'epistemic authority' in this book; or rather, reference to people is at most a sub-category of the wider meaning. 'Epistemic authority' refers to any property that is attributed to an epistemic entity in order to provide it with epistemically authoritative status. In some cases this can of course be personal authority, but that is not the typical case.[4] I am more interested in the kinds of qualities that an assertion itself should possess in order to be believed in historiography. If the epistemically authoritative property is that of truth, then we need to ask what 'truth' means in such cases.

The correspondence theory has been criticized innumerable times and for various reasons in the past. I hope that my reason to reject it in the case of synthesizing historical theses has become clear. It should be mentioned in passing that this does not necessarily mean discounting it in all contexts and in all functional roles (see below). What about attempts to redefine 'truth'? Could 'truth' be taken to mean something other than correspondence?

Pragmatism and the meaning of truth

One of the most interesting suggestions for replacing the correspondence theory stems from the pragmatist tradition. William James wrote about truth as a dynamic property (James 1998, 97):

> The truth of an idea is not a stagnant property inherent in it. Truth *happens* to an idea. It *becomes* true, is *made* true by events. Its verity *is* in fact an event, a process: the process namely of its verifying itself, its veri-*fication*. Its validity is the process of its valid-*ation*. (James 1998, 97)

And the following sentence looks very much like a redefinition of truth: 'Truth for us is simply a collective name for verification-process' (James 1998, 104).

I agree with Richard Rorty (2011b, 127) and A. J. Ayer (1998, xxiv) that it was a mistake for James to attempt to offer a positive theory of truth, that is, to redefine 'truth'. Beyond this, Rorty provides a very charitable reading, in which he has James to stick to a 'negative point' about truth. According to Rorty's reading, James thought that no theory of truth had managed to explain the relation between language and the world satisfactorily and, therefore, it would be best to understand 'true' as 'a term of praise used for endorsing, rather than one referring to a state of affairs' (Rorty 2011b, 126–127). On one occasion, James envisions 'truth' as 'a name for all those judgments which we find ourselves under obligation to make by a kind of imperative duty' (James 1998, 109). He seems to be suggesting here that the role of 'truth' is to give epistemic authority to our beliefs and statements. Truth-judgements appear as tokens of a very specific kind of speech activity, asking others to believe and accept what is stated. Although I agree about the imperative role of truth, I think, for the reasons that will be explicated below, that James should have been less categorical with respect to the meaning of truth in general. When James writes that '"The true," to put it very briefly, is only the expedient in the way of our thinking, just as "the right" is only the expedient in the way of our behaving' (James 1998, 106), it would have been more advantageous, had he only said that 'the true' is expedient, but not that it is 'only the expedient'.

John Dewey introduces an even more interesting idea when he suggests that one could replace the notion of truth with that of 'warranted assertability'. This notion seems to convey the message that our claims could have rational warrant, and thus epistemic authority, without correspondence. Dewey defined 'warranted assertability' as the end state of an inquiry that has removed the doubt that existed at the beginning of that inquiry. Epistemic authority would thus seem to stem from a satisfactory termination of inquiry. But Dewey goes further. He posits that 'knowledge' simply means 'warranted assertability' (Dewey 1938, e.g. 1–23). This is an interesting suggestion, but the definitional

link with knowledge is prone to create problems since knowledge is, in epistemology, traditionally defined as a true justified belief and Dewey's postulation prompts the question of whether 'warranted assertability' implies 'is true' in some sense.

In general, the idea that our knowledge-inquiries always begin with doubt or with a problem fits well with historiographical practice. In the following chapter I elaborate on this when I introduce the concept of argumentative context, according to which a historian always takes a stance for or against a view in an already-existing discursive field. However, the problem with Dewey's definition of 'warranted assertability' from the perspective of this book is that it is too categorical in its problem orientation; put differently, the way in which it specifies rational warrant is not nuanced enough. Although the idea of problem-generated inquiry thus agrees with the practice of historiography on a general level, there are cases that require a more detailed explication of the rational qualities of historical assertions.

My view is that Dewey takes a misstep, like his esteemed predecessor, when he uses the kind of language that suggests a definition of truth via 'warranted assertability'. It appears that 'warranted assertability' turns ideal circumstances into a definitional feature of the truth itself, which also extends the re-definitional approach to the 'founding father' of pragmatism, Charles Peirce. As Dewey writes in a footnote of his *Logic: The Theory of Inquiry*:

> The best definition of *truth* from the logical standpoint which is known to me is that of Peirce: 'The opinion which is fated to be ultimately agreed to by all who investigate is what we mean by the truth, and the object represented by this opinion is the real.' *Op. cit.,* Vol. V, p. 268 (Dewey 1938, 345; ft 6)[5]

Further, in an earlier text Dewey, like James, also discussed truth and falsity as '*properties* only of that subject-matter which is the end, the close, of the inquiry by means of which it is reached' (Dewey 1988, 205, my emphasis). And he even put forward an alternative pragmatic definition of the correspondence theory as operational and behavioral, with 'the meaning, namely, of answering, as a key answers to conditions imposed by a lock, or as two correspondents "answer" each other; or, in general, as a reply is an adequate answer to a question or a criticism; as, in short, a solution answers the requirements of a problem' (Dewey 1988 207). These kinds of statements, and specifically his reference to Peirce's

'definition of truth,' have provided a reason for many to conclude that Dewey attempted to characterize truth in terms of assertoric correctness. Peter Pagin writes that common to Dewey, Michael Dummett (1976) and Hilary Putnam (1981) is that they all think that there cannot be anything more to truth than being supported by the best available evidence (Pagin 2012).[6]

Dewey's effort to draw our attention to the conditions in which our assertions can be said to be rationally warranted is nevertheless fruitful. One way to understand the act of asserting would be to think that to assert something is to present it as true. The idea is, thus, that when one asserts something, one presents a proposition as having the property of being true. The crucial question here is whether *assertion* necessarily commits one to the truth of the proposition or to the property of being true. Rorty suggests that we should view pragmatism as something that entails 'the dissolution of the traditional problematic about truth, as opposed to a constructive "pragmatist theory of truth"' (Rorty 2011b, 127). What if truth-clauses are just for the purpose of endorsing, as James in Rorty's reading advocates? In that case, 'is true' in the assertion *that p is true* would not add much content but would serve a social function. In other words, saying that 'the historian X's interpretation is true' asks the listener/reader to accept the interpretation, but does not give the interpretation any additional epistemic quality.

Rorty's reading appears sensible with regard to the pragmatists' argumentative rationale despite some obvious interpretative difficulties. It is also compatible with Cheryl Misak's interpretation of Peirce. Misak writes that Peirce's critique of the correspondence theory of truth boils down to a conviction that the correspondence definition is nominal or trivial. According to her, Peirce's view was that the correspondence theory is pragmatically empty and philosophically unsatisfactory. Misak emphasizes that 'the analytic definitions' of truth, such as the correspondence theory, make 'truth' a useless word from the Peircean pragmatist perspective since the pragmatist is not attempting to put forward a definition but is interested in the practical import of a true hypothesis or belief (Misak 1991, 38–43). Misak writes that a pragmatic elucidation of truth is a specification of what one can expect from a true hypothesis, that is, that it would not, in the end, be overturned by experience. Because the pragmatists' approach to truth is 'in principle detachable' from the analytic definition, the pragmatic expectation of a true hypothesis and various analytic definitions, such

as a 'Tarski-style definition,' for example, can in principle co-exist (Misak 1991, 43; 129).

In this regard, Nicholas Rescher offers a very useful distinction: one can take either the definitional or criterial route to truth. In the first case, one is interested in the *meaning* of truth and thus attempts to provide a *definition* of truth. In the second case, one examines the conditions for the *application* of the concept of truth and in this way aims to provide a *criterion* for truth. Rescher also expressed the criterial route as an attempt to provide a 'warrant' for applying the characterization 'is true' to a given proposition (Rescher 1973, 1–3). It may be said that pragmatism has been more successful in the explication of criterial conditions than of definitional ones.

To sum up, there seems to be something very intuitive about the correspondence theory in a primitive sense, even if that intuitive correctness is something entirely different than specifying what 'correspondence' itself means, answering the question of whether it captures the meaning of all kinds of 'true-clauses' or explaining what 'truth-makers' and 'truth-bearers' are. Since this is not a book about truth, I will not analyze different theories of truth and their problems further.[7] I accept the point that the correspondence theory is intuitively appealing as an expression of the *meaning* of truth, but I have already explained the reason why it does not apply to historiographical theses or other statements that contain colligatory or synthesizing notions: the lack of truth-makers. If someone insists on redefining 'truth', say, as an epistemic notion, that is of course entirely possible. I might even agree concerning the substance but suggest that different terminology be used. I think that some redefinitions are simply unintuitive. For example, if the principled grounding of 'truth' is (ideal) justification, then why not to talk about 'justification' directly? It would be more intuitive. The bottom line is that an advocate of an epistemic theory of truth does not disagree about my analysis of the fundamental problem with the truth-claims of historiographical theses, that is, that they cannot be in isomorphic to, or structural similarity relations with, the past, due to the lack of truth-makers. As expressed above, the most important thing is that our claims can be credited with some kind of epistemic authority, not that this epistemic authority is necessarily 'truth'. Now, the situation is that the most intuitive *theory* of truth, the correspondence theory, is judged as unsuitable for providing the needed epistemic authority for the most important knowledge contributions of historiography, which are the synthesizing historical theses about the past.

Warranted assertion

I suggest returning to the idea of warranted assertability without the implication of the *truth* of assertions. A warrant is a form of justification. When our assertion is warranted, we either have appropriate justification for stating it or are in an appropriately authoritative situation to assert it. And this idea seems natural enough in the context of historiography. In Chapter 5, I suggested that historiographical works should be viewed as manifestations of reasoning and informal arguments, which implies that the historian provides support through literary work for some specific claim about the past. In other words, by the end of a successful historical study, the historian should have a rational warrant for his or her assertion. But, if my analysis about the colligatory nature of synthesizing historical theses is correct, assertions cannot be true.[8] To repeat, the reason is that, provided that the correspondence theory of truth expresses the meaning of truth and that the correspondence theory requires the existence of truth-makers in the past, the theses cannot be true since there are no truth-makers for the colligatory expressions employed.

   The implication of all of the above is that we need to speak of the justification of historiographical theses without the presumption of their truth. Against the background of traditional epistemology this may appear problematic because the point of justification has typically been seen to consist of its relation to truth. Richard L. Kirkham claims that justification *must* be defined or analyzed with reference to truth or, alternatively expressed, that the concept of justification presupposes the concept of truth. Laurence Bonjour in turn surmises that, if truth were somehow directly accessible to us, as it may be for God, then the theories of justification would not be very interesting. But 'we have no such immediate and unproblematic access to truth, and it is for this reason that justification comes into the picture' (Bonjour 1985, 7). This intimate link between truth and justification seems to lurk behind the whole Western epistemological project:

> Because the motivation for epistemology is concern over whether and how our beliefs can be *justified as true*, it is the truth of beliefs with which an epistemologist is ultimately concerned. (Kirkham 2001, 47; my emphasis)

But what if we are not interested in truth but only in justification? What would justification without truth be? While this may be a problematic

orientation in traditional epistemology, it may also provide a fresh and ground-breaking perspective on historiography.

Wilfrid Sellars believed that all knowing, including sense experience, presupposes both concept formation and the understanding of the conceptual space in which the knowledge claim is located. According to Sellars even the use of color concepts, such as 'looking green', implies awareness of the kinds of circumstances, both physical and linguistic, in which the concept can be appropriately used. Thus even a report on our inner experiences is irreducibly intersubjective and the competent use of all concepts is *built on* and *presupposes* their role in intersubjective discourse. Sellars summarized this thought in the following famous sentence:

> The essential point is that in characterizing an episode or a state as that of *knowing*, we are not giving an empirical description of that episode or state; we are placing it in the *logical space of reasons*, of justifying and being able to justify what one says. (Sellars 1997, section 36; my emphases)

In other words, all 'descriptive knowledge', including reports on experiencing, are assertions in the (social) space of reasons and endorsements of specific claims (Sellars 1997, section 16).

Sellars' idea that all knowledge claims are assertions in the logical space of reasons resembles that of Dewey's 'warranted assertability', but is even more useful. As many have noted, knowledge claims have a normative dimension. That is, they are endorsements or promises to defend and give grounds for one's claim. And their specific location in the 'space of reasons' largely defines the kind of grounds that must be given in defense. Rorty interprets Sellars' sentence of what knowing implies as a position that knowledge is inseparable from social practice and specifically from the practice of justifying one's assertions to one's fellow-humans (Sellars/Rorty 1997, 4). Knowledge comes into being through this practice.

One of those who has developed Sellars' account into an interesting direction is Robert Brandom. Developing Sellars' account of knowledge, Brandom suggests that one takes assertions as inferential moves in the 'game of giving and asking for reasons'. At the core of this discursive practice is the notion of 'discursive commitment'. That is, when making an assertion, one is engaged in a social practice and makes an 'assertonial commitment'. Thus in the 'game of reasons', assertion can serve as

a reason for another assertion or it can itself stand in need for further reasons:

> Uttering a sentence with assertonial force or significance is putting it forward *as* a potential reason....Assertions are essentially *fit* to be reasons. The function of assertion is making sentences available for use as premises in inferences. For performances to play this role or have this significance requires that assertonial endorsement of or commitment to something entitles or obliges one to other endorsements. (Brandom 1994, 168)

The interesting thing with this account is the idea that in assertions one undertakes a responsibility, a commitment, to 'vindicate the original claim by showing that one is entitled to make it' (Brandom 1994, 171). According to Brandom, this kind of discursive commitment implies a broader normative use through which one can assert authority. In other words, the responsibility to defend one's claim and give reasons for it, if and when requested, is *justificatory.* Brandom writes:

> In asserting a sentence, one not only licenses further assertions (for others and for oneself) but commits oneself to *justifying* the original claim....Specifically, in making a claim, one undertakes the conditional task responsibility to demonstrate one's entitlements to the claim, if that entitlement is brought 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. *If the commitment can be defended, entitlement to it demonstrated by justifying the claim, then endorsement of it can have genuine authority*, an entitlement that can be inherited. (Brandom 1994, 172; my emphases)

I hope the way in which justification and epistemic authority can be acquired through rationality is slowly becoming clearer. Asserting is a normative sort of social practice that authorizes certain sorts of inferences and makes the asserter responsible for giving reasons for the assertion. When one manages to draw appropriate inferences, other assertions, for the main claim, one can be said to be entitled to that claim or be said to possess appropriate authority for the claim. What is it that yields this authority or warrant in the 'game of giving and asking for reasons'? It is, precisely, the successful practice of providing assertorial inferences. This practice forms the reason or reasons for the

claim. To put it differently, in the situation in which the respondent manages to give reasons, his or her assertion can be said to be rationally warranted or justified. *The inferential practice of giving reasons is thus itself a form of justification.*

We might say that *justification lies in the inferential act of rationality itself* and not, for example, in the copying of prior states of affairs or in referential relations. Or, as Dewey said, 'the value of any cognitive conclusion depends upon the method by which it is reached' (1929, 200). Despite some differences in how 'truth' and 'warranted assertability' are understood, my proposal fits well with the pragmatist notion of justification. According to Dewey, in the 'traditional conception' the thing to be known is something that exists prior to and wholly apart from the act of knowing whereas in the new conception '*knowing is a form of doing*' (1929, 205; my emphasis). Dewey further complained that the traditional conception implied that discursive knowledge always had to involve reflection on what is immediately known in order to be validated. Validation could not be seen to 'bring its credentials with it and test its results in the very process of reaching them'. According to Dewey, this meant that the 'old conception' implies that knowledge reached through inferential conclusions is simply a 'matter of restatement' of what pre-exists (1929, 181–182). In the next chapter, I continue this line of reasoning and attempt to show how epistemic credentials can emerge through the actual practice of articulating a thesis in historiography without any need for mirroring, *re*-presenting or *re*-constructing reality 'as it is'.

Conclusion

Where does this leave 'truth' in the 'game of asking and giving reasons' for stated historiographical theses? Provided that redefining the notion of truth by reference to some novel semantic or epistemic entities or processes is discounted and that the notion of epistemic authority is taken as a more fundamental concept than that of truth, I do not see why the 'truth' would be necessary in the game of asking and giving reasons. That is, if we manage by some other means to attribute to our assertions the kind of epistemic authority that compels a *rational* being to accept the assertions, reference to 'truth' is superfluous. And epistemic authority can be seen to derive from the inferential practice of providing reasons itself. Brandom suggested that Wittgenstein's substitution of his earlier question 'What are the facts?' with the question 'What are we entitled to say?' leads to the 'de-emphasis of the notion of truth' (1976, 138).

Indeed, if putting forward a claim as true is to endorse it, to ask others to accept it, endorsement can be carried out by providing reasons for its defense in the discursively regimented 'space of reasons' (cf. Brandom 1994, 170). Because of the problems with the correspondence theory in the case of historical theses, no deeper substance can be provided to 'truth'-clauses than the endorsement itself. Notice, however, that the truth of some subordinated statements may be required, as discussed in the final chapter of this book.

9
The Tri-partite Theory of Justification of Historiography

I have built this book on the contributions of the narrativist philosophy of historiography although I have also departed substantially from its model. It is now possible to reformulate the central contribution of the narrativist school as the insight that historical knowledge in synthesized form – regardless of whether this is taken to mean historical theses and/ or colligatory expressions – is the most important and interesting kind of knowledge that historiography produces. This to say that philosophical analyses should pay particular attention to this kind of knowledge. The fundamental problem with narrativism is that it cannot provide an epistemologically or otherwise cognitively meaningful evaluative framework for this kind of higher-order historical knowledge. Now, it is time to explain how we can keep the essence of the narrativist insight but to also formulate a cognitively meaningful approach to evaluation in historiography. The answer provided is in part based on the conclusions of earlier chapters and in part a continuation of the reasoning that led to them.

Before discussing the 'positive' theory of historiographical evaluation, it is useful to clarify my relation to postmodernism because narrativism has often been seen as postmodernist. After that is done, I will introduce three dimensions of historiographical evaluation: the epistemological, the rhetorical and the discursive. The focus in this chapter is specifically on the discursive dimension, which has not yet been studied extensively. In relation to this, a particular point of interest is Quentin Skinner's theory of speech acts in intellectual history. The next step in this chapter is to explain what argumentative context and argumentative intervention are through an example of the First World War. At the end, I will draw all the evaluative aspects together for a comprehensive theory of justification in historiography.

Postmodernism

Postmodernism in historiography, as seen for example in the writings of Keith Jenkins and Alun Munslow, is one descendant of 1970s and 1980s narrativism (e.g. Jenkins 2003a, 7–8). It should be remarked that I am not using 'postmodernism' here as any kind of abusive term but as a description of an intellectual orientation to which these authors are explicitly committed. Hayden White has undoubtedly been its main influence, but also Ankersmit's early writings have contributed to its formation. Although the argumentation and conclusions differ to some extent, it is fair to say that these two intellectual schools, postmodernism and narrativism, share many assumptions. In what follows, I will analyze the postmodernist philosophical position. This is important in order to understand the relation in which postmodernism stands to narrativism, but also in order to situate my project with respect to the former. Keith Jenkins' key writings are my foremost guide to postmodernism in historiography.

Postmodernism in historiography accepts and endorses the fundamental distinction between lower-order and higher-order entities of knowledge, as outlined by the narrativist philosophy of historiography. That is, it commits to the distinction between 'facts' and 'narratives' and to the difference in epistemic status between them. In a preface to Jenkins' *Re-thinking History*, Alun Munslow writes that Jenkins takes 'his cue' from White and Ankersmit that history is first and foremost a literary narrative about the past, a literary composition of data into *a* narrative where the historian creates *a* meaning *for* the past (Munslow 2003, xii). Indeed the idea of there being *data*, which are used to create *meaning* is central both to narrativists and to programmatic postmodernists, which leads both to historiographical constructivism. Jenkins writes:

> The historian can then begin to organise all these elements in new (and various) ways – looking to that longed for 'original thesis' ... Here the historian literally re-produces the traces of the past in a new category and this act of trans-formation – the past into history – is his/her basic job. (2008b, 27)

Occasionally postmodernists make the same problematic commitment to historical 'facts' as the narrativists rather than to a less loaded notion of 'data', which has sometimes led to a critique of their implied positivism.[1] For example, Jenkins indicates that the claims that the First World

War happened between 1914 and 1918 and that Margaret Thatcher came to power in 1979 are historical facts. Ultimately 'facts' are deemed insufficient for the construction of proper historical knowledge, because 'such facts, though important, are "true" but trite within the large issues historians consider' (Jenkins 2008b, 40). And 'large issues' refers to the historian's interpretative task and to the historian's attempt to make the past meaningful, a job which requires the historian to consider the weight, position, combination and significance of the 'facts'.

Ankersmit (1983) declared that there are no translation rules that would tell us how to translate the past or the traces of the past into a narrative, and Jenkins is certainly in agreement about this (Jenkins 2003, 5). In Jenkins' language, the narratives or theses that input meaning into the past are the historian's *referents* (Ankersmit would talk about 'presenteds' or 'aspects'). However, the historian's 'referent' is not out there but 'the product of their *inference'*. Jenkins plays down the importance of 'facts' in inferences and attributes a significant role to personal and professional interests and concerns (2008a, 66). The idea that historiographical interpretations are inferences is promising because it suggests that interpretation is explicable and rule-bound. Nevertheless, Jenkins agrees with Ankersmit that there are no logical rules that would regulate historical construction. Jenkins even hints that the presumption of, or perhaps the illusion that there exist, rules or methodologies to determine historiographical interpretations is harmful. He thinks that adherence to rules and methodology precludes choices and responsibility, and would thus ultimately be unethical (Pihlainen 2013, 244).

Jenkins makes clear, just like the early Ankersmit did, that the 'empirical/epistemological element' can operate on the level of singular statements but not on the level of narrative historical representations because these are of an 'aesthetic kind' (Jenkins 2008a, 69). But what is Jenkins' fundamental reason for saying this? His talk of narrative representations as aesthetic kinds and figures (Jenkins 2008, 69) would perhaps suggest that the problem has to do with the lack of isomorphism between the past and these kinds of entities. That is, he would perhaps mean that a qualitative difference makes it impossible to match them. In actuality, the onus of Jenkins' argumentation is on epistemological problems. First, Jenkins confesses to being a minimal, or 'fig-leaf' realist to use Michael Devitt's language (Devitt 1997, 23), in the sense that he accepts that there is mind-independent 'material stuff' out there, even if we may never be able to describe it accurately (Jenkins 2008a, 60). The fundamental problem of the 'empirically/epistemologically' driven 'non-radical historian' is that his or her aim to 'establish assured historical

knowledge … cannot ever be met' (Jenkins 2008a, 64), implying that the difficulty is of the epistemic practical kind. More revealingly, Jenkins writes that '[w]e shall never know what History/history "really is" – that will remain a secret like the name and the face of God'.

The problem thus is not that there is no 'what history/History really is' but that we can never know it. Jenkins describes our perspective as anthropomorphically limited (Jenkins 2008a, 60). Here emerges a crucial difference to my analysis of the situation, by which I may ultimately come to appear more radical than the 'radical historian' of the postmodernist. In my analysis, not even God could know what history 'really is', because there is 'no real' history in the sense that the past would have an inherent and given shape. I agree with Jenkins that 'the truth-full reconstruction of the past', at least on the synthetizing level, 'is … an impossible "myth"' (Jenkins 2008a, 63). Equally, Jenkins is on the right track when he states that 'History is about something that never did happen *in the way in which it comes to be represented*' because 'representations' are constructions (Jenkins 2008a, 67; my emphasis). My technical reason for this conclusion is that there are no truth-makers for the integrative theses on history. But I rush to point out that this does not mean, and here I again disagree with Jenkins, that all historians' construction are equally 'arbitrary' (Jenkins 2008a, 64). Jenkins seems to follow White in that there are no epistemological and empirical grounds to choose one interpretation over others.[2]

Postmodernism and narrativism (specifically in its earlier formulations) infer from the correct conclusion that there are no uniquely, absolutely, correct historiographical interpretations the erroneous one that no interpretation is cognitively more justified than another. First, 'constructed' does not automatically mean 'unreal'. The semantics of 'real' needs its own treatment (in Chapter 10) since it is frequently used in argumentation both by postmodernists and realists. Second, neither does 'constructed' mean 'unjustified', as there is room for many kinds of comparative and rational evaluations.

The mistake that Jenkins makes is that he equates historiographical interpretation with 'meaning', and 'meaning' with values. This assumed, Jenkins then relies on a traditional Humean principle that it is impossible to derive value judgments from facts (or 'ought' from 'is'), and concludes that one cannot infer historical 'meaning' from historical 'facts'. The conclusion is thus that, because the past has no intrinsic value, history can be '(logically) anything you want it to be (the fact-value distinction allows this …)' (Jenkins 2008b, 13; similarly 2003, 43). In other words, because we are free to choose our values, we are

also free to choose the meanings of history (cf. Pihlainen 2003, 243). Further, Munslow's explication of 'meaning' is misleading, if meant as a general 'meaning' notion in historiography, since he seems to conflate the 'meaning of the past' with the meaning of a past text (e.g. Munslow 2007, 100–101). Whatever 'meaning' the past can take, it can arguably be a meaning of something non-textual too. Martin proposes another way of understanding 'meaning' in the context of historiography, which reduces it to a cognitive notion. For Martin, to provide an answer to what meaning or historical importance is comes down to showing a coherent and intelligible pattern, such as making a case for the importance of the defeat of the Spanish Armada (e.g. European nations concluded that the God is not on the side of Spain and that religious unity was not to be reimposed by force) and, in this way, justifying the interest in the episode (Martin 1993, 44–47).

Perhaps the most important difference between my analysis of historiography and that of the postmodernists deals with how the central theses of historical works are understood. The assimilation of 'meaning' with values is problematic not only because it puts them beyond any kind of cognitive judgment but also because it threatens to exclude them from rational evaluations altogether. Jenkins states that 'history remains inevitably a personal construct' (Jenkins 2008b, 14) and it is personal tastes that make us choose one approach over another. Jenkins rhetorically asks: 'is it not likely that in the end one chooses, say Thompson, because one just likes what Thompson does with his method?' (Jenkins 2008b, 18). Munslow writes that 'It is the function of the reader to determine for herself or himself why some views of the past are plausible, satisfactory and convincing and others are not' (Munslow 2007, 116). Now, if the whole point is to make emancipatory, material differences to and within the present, and if interpretations are subjective choices entirely, it would seem that there is no room for other evaluative criteria than those that matter to the individual, no matter how ill-informed the interpretations may appear to be for others. In other words, there would be no role for rational judgments and corrections that go beyond the individual's tastes and preferences.

Although Jenkins' propagates 'antirepresentationalism', he nevertheless succumbs to the same kinds of problems with 'representationalist' vocabulary as Ankersmit.[3] Jenkins' 'antirepresentationalism' boils down to the view that 'historians' representations ... are *always* failed representations,' and thus 'fictive' in his parlance (Jenkins 2008a, 65; 68; similarly Jenkins 2003, 5). One wonders why he talks about 'representations' since the term connotes that one is re-presenting in historiographical

language something that exists there prior to any constructions (just like it is with the term 're-construction'), as discussed earlier in the book. And this is not just a harmless linguistic entailment but a commitment that channels argumentation into a particular direction and exposes it to certain problems, ultimately even resulting in discursive incoherence. In other words, the talk of 'representations' is vulnerable to all the Cartesian epistemic and skeptical problems of getting it right and having 'access to the actualities of "stuff"' (Jenkins 2008, 60) that is there, but which remains beyond our epistemic reach. But if interpretations do not have references, then why get stuck with this kind of discourse? Why not to say, as Pihlainen expresses it, that 'the past need not be *represented* at all' (Pihlainen 2013, 239; my emphasis).

I now return to the question of rules and methodology. The first thing to say is that, on the sociological level, it would simply be wrong to say that historiographical discourse is arbitrary since the communities of historians certainly evaluate and control what is accepted. While it is reasonable to suggest that one cannot achieve 'closure' in interpretation, this is not a case of 'interminable openness' (Jenkins 2008a, 65). Now, Jenkins of course means 'openness' in an epistemological sense, and not in general, since power and political interests still govern and restrain historiographical discourse. Like any 'knowledge', interpretation in historiography is used to legitimate power and material interests (e.g. Jenkins 2008b, 31). This provides a principled grounding for the evaluation of different 'histories'. That is, contemporary moral considerations take priority: 'radical historian don't *[sic]* work on behalf of the people who lived in the past: they work for us' (Jenkins 2008a, 64). According to Jenkins, all historiographical accounts should be directed towards emancipation and liberation, or they should make emancipatory and material differences in the present (Jenkins 2008a, 71; 2008b, 81).

His view that the existence of rules is incompatible with choice and ethical responsibility is an overreaction. It would be correct only if rules are of an algorithmic kind, strictly determining the outcome. In this situation, the only 'choice' left for the individual would be the correct following of rules. But there is currently a consensus among scholars in science studies and the history of science that this kind of strong rationality does not apply anywhere in the sciences, not even in the physical natural sciences. We might say that the key lesson of several decades of historical philosophy of science, the historiography of science and sociological science studies is that a multitude of cognitive, observational, personal and various kinds of social factors have a role to play in theory decisions, and none of them alone determines the outcome. This is to

say that Jenkins could have been more circumspect and expressed his position through the thesis of underdetermination of theory by data, which is in effect expressed in the following: 'clearly there are all kinds of limits controlling the knowledge claims that historians can make' and 'sources may prevent just anything at all from being said, nevertheless the same events/sources do not entail that one and only one reading has to follow' (Jenkins 2008b, 12, 15).[4] Elsewhere Jenkins indeed admits that '"the past"…is so very obviously underdetermining in relation to its endless appropriations' (although he also jumps to the conclusion that the past can be 'read at will') (Jenkins 2003, 10).

A still more important a point is that ethical responsibility itself requires some kind of rule-boundedness. Ethical choices cannot be random. Ethical responsibility arguably implies the existence of or commitment to ethical principles or maxims in some form, which instruct (but do not determine) about right and wrong behavior in this or that situation. Further, choices cannot be totally 'free' either, unless freedom refers to freedom from physical coercion. 'Freedom' is arguably not synonymous with 'arbitrary'.

Despite Jenkins' inclination towards individualistic subjectivism with regard to evaluative judgments in historiography, this position would not sit well with his other commitments. It is possible to maintain that aesthetic, moral and ethical considerations entail their own rational standards. And as just discussed above, social and ethical responsibility imply the existence of some kinds of behavior-guiding rules and principles, which entails that they are inter-subjectively applicable. It is, as Rescher writes, that 'the idea of rationality is in principle inapplicable where one is at liberty to make up one's rules as one goes along' (1988, 158). Specifically, the 'social' aspect of historiographical interpretation takes one beyond individuality, and therefore entails normative requirements. Further, it is far from clear that we should accept the assimilation of historiographical interpretation into value judgments, if one accepts that value-laden interpretations can be subsumed under rational (aesthetic, ethical or moral) considerations. A weakness in theoretical discussions on historiography is often the lack of specificity and of concrete examples. We need to ask whether historiographical theses are like values.

Thompson's thesis concerning the origins of the English working-class or Clark's 'sleepwalking' interpretation of the causes of the First World War are ultimately cognitive claims; they are suggestions regarding what the past was like, how it should be seen. To say this does not mean denying that they relate to certain political discourses, as we have seen;

indeed, argumentation takes place in a politicized context. Nevertheless, they are knowledge claims which, however, cannot be evaluated truth-functionally, as I have argued, but which may be evaluable by other cognitive criteria. Jenkins' approach closes down this option before alternative rational and cognitive criteria have even been considered, which is ironic because a cognitive approach may be closer to his position than Jenkins has noticed. Consider how he suggests that inferences in historiography are 'always arguments' (Jenkins 2008b, 67). Jenkins hits the nail on the head when he writes that 'arguments are never true of false; arguments can only be valid or invalid...*All* histories [are] always neither rigorously true nor rigorously false; at best it can have to recommend it "a certain appearance in its favour"' (Jenkins 2008a, 67–68). This is indeed so, but argumentation is a rational practice and arguments can be evaluated by considering how strong is the evidence that they provide for the main thesis. With this move we are firmly in the domain of rationality and cognitive assessment.

Three dimensions of evaluation

The governing idea of the tri-partite theory of historiographical justification is to see historiographical theses as rationally warranted claims and argumentative interventions. More precisely, my solution to the problem of historical knowledge is three-fold. The evaluation of (synthesized) historical knowledge can be divided into three dimensions or sectors with interrelated connections: (1) the epistemic dimension; (2) the rhetorical dimension; and (3) the discursive dimension. I will first introduce all three briefly. After that I will discuss them in more detail as appropriate. All of these dimensions can be subsumed under the concept of *rationality*, which will be further discussed in the penultimate chapter of this book.

It is worth reminding the reader about the specific challenge that we are faced with here. Higher-order historical knowledge cannot be true in the sense of correspondence since it contains subjective-sided elements that have no counterparts in historical reality. To put this differently, what is subjective does not have a truth-maker to make its potential truth-bearer true or false. And yet: not all colligatory expressions or historiographical interpretations seem to be on a par. If 'Khrushchev's Thaw' is a good colligatory notion, 'Khrushchev's Big Chill' cannot be equally acceptable about the same historical period and material. And if we are persuaded that the nineteenth-century was the era of 'Industrial Revolution', that seems to cancel out the possibility that it was an 'Agricultural Revolution'

at the same time.[5] The appeal to 'truth' is out of the question, provided that the meaning of 'truth' is correspondence. We have to find another principled explanation for why some synthetizing historical expressions are to be prioritized over others. What is more, this should be done in a manner that is cognitively meaningful. 'Cognitive' may be understood broadly as signifying relevance to knowledge and knowing. This commitment thus means that a principled grounding cannot rely merely on other kinds of (possibly rational) criteria, such as moral and aesthetic ones, although they could of course also play an additional evaluative role. I will next explain in more detail what this means.

1. *The epistemic dimension.* 'Epistemic' or 'epistemological' is a close relative of 'cognitive'. A brief terminological explication is thus in order. Larry Laudan (1984) distinguishes between epistemological values that are indicative of truth and cognitive values that may be valued for other reasons. Laudan states that 'many, and arguably most, of the historically important principles of theory appraisal used by scientists have been, though reasonable and appropriate in their own terms, utterly without epistemic rationale or foundation' (1984, 16). I will not adopt Laudan's definition of 'epistemic' but the distinction between epistemic and cognitive is useful, as is the idea that there are principles that are reasonable and appropriate in some other sense than a truth-functional one. 'Cognitive' here signifies any appropriate and *reasonable* criteria that make a historiographical thesis or expression concerning the past – that is, as a knowledge claim – *compelling* to accept. 'Epistemic' is here a sub-concept of cognitive and a more restricted notion referring not to truth-conducivity as it typically does (cf. Heather 2014), but to the *relation* in which a historical presentation stands *with* its *objects of research* (the past) and *with evidence* directly. The epistemic dimension of historiographic evaluation thus points to something that underlies the actual historiographical presentation, to the epistemic values that may be implicit in a presentation and may be explicated through a rational reconstruction. Epistemic values form a familiar set of such virtues as exemplification, coherence (including consistency), scope, comprehensiveness and originality that a notion should possess to make it epistemically valuable. The functioning of epistemic values was already discussed at length in Chapter 6 and therefore does not require further discussion here.

2. *The rhetorical dimension.* The second dimension is *rhetorical*. I have chosen the term 'rhetorical' because the point is that every work of

history attempts to persuade its readers to accept its central historiographical thesis. It is important to notice that we are not talking about just any kind of persuasion, but of a specific form of argumentative persuasion that relies on informal argumentative strategies and reasoning. For this reason, the second dimension could equally be called 'argumentative'. In Chapter 5 I have already discussed the sense in which a work of history can be seen to form an argument and hence will not initiate another detailed discussion about this dimension here. Discussion of the rhetorical dimension is kept on a methodological and theoretical level in this chapter, as is done also with the epistemic dimension.

The argumentative rhetorical dimension could be said to be 'internal' in the sense that this term has been used in the philosophy of science since the 1960s. That is, it refers to the internal textual and argumentative qualities of a text, as if the text formed an autonomous unit of rationality. Science was seen, and is often still seen, to advance according to its own 'internal logic' of reasoning and experimentation, which allegedly alone determines scientific theory choices. It should be obvious that while I do not wish to claim such autonomy for the works of history as textual pieces, the rhetorical dimension forms one evaluative dimension nevertheless. All the three dimensions I discuss are related. The first dimension (epistemic values) represents an abstraction of the theoretical principles embedded in historiographical argumentation, while argumentation (rhetoric) itself is manifested textually and makes a direct appeal to the readers, which constitutes the third, discursive dimension.

3. *The discursive dimension*. In the traditional terminology of the philosophy of science, a discursive dimension amounts to something 'external' because it refers beyond the text itself to the historiographical *argumentative context*. It is evident that no historical work appears as a self-contained piece from an intellectual vacuum but emerges, instead, inevitably as molded by existing historical knowledge and historiographical arguments. This is what I mean by the 'argumentative context' of historical works. The argumentative context itself has been shaped by various kinds of intellectual, political and other interests. It is my claim that a proper justification of a historiographical argument requires adequate accounting of the existing knowledge and arguments, and an appropriate intervention in a relevant argumentative context.

The idea of the argumentative context is to provide an account of the historiographical setting in which any historian has to situate his or her historiographical argument. Why is it necessary to do so? Ignoring the existing discussion would amount to disregarding prevailing historical knowledge. One would in effect be re-inventing the wheel, and most likely end up being excluded from the community of historians (because of scholarly omissions). More seriously, it would be very difficult to evaluate this kind of historiographical contribution because it would not relate to what is generally seen as justified and problematic in the research field regarding some specific topic. Perhaps one thinks that this kind of neglect of prevailing knowledge would not matter if one were to 'reconstruct' a historical episode from scratch, directly from historical sources. However, as has been discussed previously, especially in connection with the phenomenological narrativists, it is simply impossible to write history in a vacuum. That is, there is no thinking and writing of history without the existing historiographical consciousness and discourse, molded by various kinds of social and political factors. All historians simply have to locate themselves in the discursive field of historical thinking. History writing that begins from scratch and operates beyond *some* argumentative context is impossible.

It may be added that a professional historian should not be content with the general historical discourse but be aware and critical of the state of affairs in scholarly historiography. Finally, and to repeat my claim, the situation in the existing field of historical argumentation and the historian's response to it in part determines the degree of justification of a historiographical argument.

Quentin Skinner's theory of speech acts

An attentive reader may have noticed that the idea of argumentative context and argumentative intervention in that context has some affinities with Quentin Skinner's theory of speech acts in intellectual history. It is useful to have a brief look at Skinner's theorizing.

Skinner has famously suggested that the author is doing something when the author is speaking or writing; there is some point or intended force behind saying and writing something. A text for Skinner is a linguistic act. And more generally, any writer, according to Skinner, is engaged in an 'intended act of communication' (Skinner 1988, 63). An example could be Machiavelli's statement that 'Princes must learn when not to be virtuous'. Skinner thinks that the point or force behind

this particular claim is to 'challenge and repudiate an accepted moral commonplace' (Skinner 1988, 86).

Skinner follows J. L. Austin's speech act theory and calls the intended force behind a text 'illocutionary force', in distinction from its 'locutionary' or propositional meaning. One might summarize the central methodological problem as follows: 'The essential question which we therefore confront, in studying any given text, is what its author, in writing at the time he did write for the audience he intended to address, could in practice have been intending to communicate by the utterance of this given utterance' (Skinner 1988, 63).

I accept Skinner's idea that texts may be seen as speech acts with which authors intended to do something and, if successful, effect some change in their context.[6] This appears to be very fruitful in the context of historiography and I have already characterized historical texts as argumentative speech acts or argumentative interventions *for* a specific *point*. Next I am going to explore Skinner's 'theory' of illocutionary force and his methodological advice on how to identify 'force' in text.

On some occasions, Skinner writes about 'illocutionary force' as external to the actual 'meaning' of an utterance. For example, in reference to Austin, he writes that 'what a given agent may be *doing* in uttering his utterance is not a question about meaning at all, but about a force co-ordinate with the meaning of the utterance' (1988, 61). However, on other occasions, he states that 'illocutionary force' is part of the meaning of an utterance. In other words, without understanding 'illocutionary force' one would not be able to understand (the meaning of) the utterance or the text in question. Consider the following the statement: 'The concept of illocutionary force simply describes an aspect of the meaning of utterances' (Skinner 1988, 274). Skinner is thus saying that 'meaning' has several aspects, although he is not quite consistent on what these components are. Sometimes 'meaning' is split into three different components (e.g. Skinner 1988, 70), sometimes only two (e.g. Skinner 1996, 7–8).[7] In both cases, illocutionary force would in any case be a component of the whole meaning of an utterance or a text. Interestingly, Skinner has recently returned to the view that the meaning of a text is separate from the speech act of that text (Skinner 2007). For my purpose, it is not crucial to determine what Skinner's actual view is, however. In accordance with what I already argued in Chapter 5, I assume that historiographical text contains a thesis with a specific identifiable meaning but also that a text (in separation from this meaning) functions as reasoning and an argument for a specific point of

view in a given argumentative context. In my view, the argumentative intervention by a text is not part of its meaning.

Skinner offers some further advice on how to identify the illocutionary force of an utterance. First, one needs to elucidate the meaning of the utterance. 'Meaning' refers here to the conventional non-contextual understanding of meaning, as in the study of sense and reference. Secondly, it is necessary to determine what other utterances it relates to in the context. It is Skinner's belief that, if one characterizes the context in sufficient detail, one can eventually 'read off what the speaker or writer in whom we are interested was doing in saying what he or she said' (1988, 275). This approach can be illustrated with Machiavelli's claim that 'mercenary armies always undermined liberty'. Provided that the reader understands the 'conventional' meaning of this sentence, the reader should consider why Machiavelli said this, which requires studying the context of this statement. Depending on the conventions of the time, the intended illocutionary force can be judged to be different. If this belief or statement was frequently expressed, Machiavelli was merely repeating, upholding and agreeing with a generally shared opinion. If the belief was no longer generally accepted, but had been at some earlier time, he could be seen as restating, reaffirming or recalling that statement. If the sentiment in the statement was not typically accepted at all, then perhaps Machiavelli's point was to correct and revise some generally accepted belief. And so on. If the second step maps the terrain of possible communicative performances, decoding the actual illocutionary intention requires yet an additional step that traces the relations between the utterance and the linguistic context (Skinner 1988, 64–64; 275).

Occasionally Skinner characterizes context-related illocutionary force in a way that resembles the approach I have outlined in this book, or vice versa. I quote Skinner here at length:

> The types of utterance I am considering can never be viewed simply as strings of propositions; they must always be viewed at the same time as arguments. Now to argue is always to argue for or against a certain assumption or point of view or course of action. It follows that, if we wish to understand such utterances, we shall have to identify the precise nature of the intervention constituted by the act of uttering them…We need to see it not simply as a proposition, but also as a move in argument. So we need to grasp why it seemed worth making that precise move; to recapture the presupposition and purposes that went into the making of it. (Skinner 1988, 274)

Furthermore, Skinner's idea that 'any act of communication always constitutes the taking up of some determinate position in relation to some pre-existing conversation or argument' corresponds with my suggestion for understanding historical works as argumentative interventions in an argumentative context. This requires that one identify the exact position that has been taken up, as Skinner says.

It should be made clear that my interest in illocutionary intentions and forces is more limited than Skinner's. I am interested specifically in what might be called *disciplinary illocutionary intention* and *force*. Briefly put, the illocutionary intention of historiography, and thereby of practicing scholarly historians, is to persuade their peers and the wider audience to accept their historiographical theses. And the successful implementation of this intention, the disciplinary illocutionary force of historiographical argumentation, means that the audience accepts the theses advanced and modifies their existing knowledge accordingly. This means that the immediate intellectual context, argumentative context, is composed of existing historiographical conceptions and knowledge. In historiography, the point is more rarely to agree with existing historiographies than it is to disagree with and correct them.

The argumentative context of the Great War

It has been estimated that at least 50,000 titles have been published on the Great War (Winter and Prost 2005, 1). The Carnegie Endowment for International Peace alone published 132 volumes on almost all the belligerent and some neutral countries of the war (Winter and Prost 2005, 8). It would be a daunting task to go through even a representative part of the debate even in a much larger project than this one; as Winter and Prost put it, reading the existing literature would take 'several working lives' (Winter and Prost 2005, 1). My discussion of the First World War is intended to illustrate and explain what 'argumentative context' and 'argumentative intervention' in the context of this debate are. An especially useful book in this regard is John W. Langdon's *July 1914*: *The Long Debate, 1918–1990*. It provides an overview of the argumentative debate until 1990.

The first issue is to outline the original foundation for historiographical discourse on the Great War. A historical event, the Treaty of Versailles, which finally ended the Great War, provides the benchmark. It laid the war guilt and assigned reparation payments almost solely on Germany, and thus set the foundation for historiographical debate until today. This account should be seen as the default position for and

against which most historians have positioned themselves in subsequent decades.

Langdon suggests that debate can be structured around six key points regarding the First World War (1991, 8–18).[8] For example, a critical objective of the main German revisionists (e.g. Alfred von Wegerer and Max Monteglas) between the world wars was to argue that Russian mobilization was crucial for the outbreak, given the numerical superiority of the France–Russia alliance (Langdon's key point 6). In other words, they claimed that it was not Germany that had aggressive plans but Russia, which was interested in the Bosphorus, the Dardanelles and the Balkans. This shows very clearly how German revisionists positioned themselves and had to situate their argument in the existing argumentative field. That is, they pointedly argued against the dominant conception of German war guilt. Sean McMeekin (2011) is a contemporary historian whose thesis resembles that of the German revisionists in that he shifts the main blame to Russia's imperial ambitions. There were also many non-German revisionists who weighed in with their own argumentative interventions. We find, for example, a precursor of Clark's 'sleepwalking' thesis in Wegener, Sidney Fay and Harry Elmer Barnes, who argued that 'the nations involved were prisoners of chance, doomed to play their tragic roles in the absence of real control over their own destinies' (Landgon 1991, 23).

On the other side, many anti-revisionists in countries like Germany, the USA, the UK, France and Italy responded in kind. Perhaps the most famous of them is A. J. P. Taylor, specifically in *The Struggle for Mastery in Europe, 1848–1918* and *War by Timetable*. The main thesis of the former is that Germany prodded Austria into starting a war with Serbia with full awareness of the consequences. In *War by Timetable,* Taylor construed the outbreak of war in terms of railway timetables, the message of which can be expressed in the words of Winter and Post: 'Once the wheels were set in motion, they could not be stopped. However peaceful the leadership of the country was, the army had to be ready for war on time' (Winter and Prost 2005, 43). Similarly, in *The Struggle for Mastery in Europe*, Taylor states that 'The Austro-Hungarian declaration of war on Serbia was the decisive act; everything else followed from it' (Taylor 1954, 523). Nevertheless, despite the automatic trigger built into the international political and technological system, the finger points in the end at Germany: 'When cut down to essentials, the sole cause for the outbreak of war in 1914 was the Schlieffen plan. … yet the Germans had no deliberate aim of subverting the liberties of Europe. No one had time for a deliberate aim or time to think. All were trapped by the ingenuity of their military preparations, the Germans most of all' (Taylor 1969, 121).

Taylor's *The Struggle for Mastery* was the last major historiography before Fritz Fischer's *Griff nach der Weltmacht* in 1961 (*Germany's Aims in the First World War*). Fischer argued that the Great War was a result of Germany's premeditated struggle for power in Europe. The following quote illustrates well where Fischer directs his fire and how he makes his move in the game of argumentation: 'As Germany willed and coveted the Austro-Serbian war and, in her confidence in her military superiority, deliberately faced the risk of a conflict with Russia and France, her leaders must bear a substantial share of the historical responsibility for the outbreak of general war in 1914' (Fischer 1967, 88). Fischer's construction of his thesis of Germany's expansionist foreign policy shows well the argumentative layers of his work. Namely, it was based on his specific interpretation of German policy, that is, that the German government was convinced that Britain would remain neutral in any Austro-Serbian war. This interpretation appears to stem from the reading of Kaiser Wilhelm's notes in the margin of documents to the effect that Britain would remain neutral, indicating a refusal to believe that Russian Foreign Minister Sazonov was right (Sazonov had insisted that Britain would object, cf. Langdon 1991, 69). Fischer also had another unique point of critique to direct against the previous scholarly discussion: most of the preceding discussion concentrated only on political and diplomatic history almost to the complete exclusion of domestic economic and social factors. Fischer questioned this and emphasized the primacy of domestic policy.

Fischer's main opponent was 'the dean of German history', Gerhard Ritter. Both read the same material but emerged from their reading with different conclusions. While Fischer saw the generals and statesmen in Germany as united, Ritter emphasized their differences. For Ritter, German history in the twentieth century is a classic case of tragedy and he saw fit to speak about 'disaster' and 'blindness'. Fischer, by contrast, detected only 'intent' and 'premeditation' behind the course of events. Further, the evidence, which proved for Fischer that Germany was striving for world power and was possessed by a collective megalomania, was interpreted as showing only the face of new, more marginal, illiberal, conservative and militarist German nationalism by Ritter (Langdon 1991, 106–107).

Fischer's account gradually became the new consensus, the effect of which is still felt today. The debate has continued for decades and it would be possible to mention hundreds of other cases and arguments, such as those of Mommsen, who thought that the Great War was not the consequence of Germany's lust for power but a response to an

unwelcome and unanticipated crisis and hence a preventive war, which could nevertheless be used to redraft the power constellation to favor Germany (Langdon 1991, 121). In summary, much of the literature can be positioned on the war guilt axis, as Clark has observed (Clark 2013, xxvii–xxviii). Langdon remarks that since 1980, historians have moved beyond 'the limits of [the] Fischer' controversy, trying to blend together diplomatic, political, economic, social, cultural, and psychological insights (Langdon 1991, 155). And while it has often been argued that, after the initial set of events, the war was inevitable, some recent historians have even questioned this assumption and claimed that the war was actually improbable until it happened (see Afflerbach and Stevenson 2012).

And the debate continues. William Mulligan (2014) has analyzed and reviewed ten books that have appeared since 2010. He notes that the role of the international economy, businessmen and bankers in the origins of the war has previously received very little attention. There is an air of paradox because the historians of globalization have argued that the interdependence of the international economic system makes war unlikely between interdependent states, and still the relatively globalized world went to war in 1914. Indeed, new literature suggests ways in which the interdependent world economy offered new possibilities for the exercise of power and why interdependence created vulnerability for the sovereign states. Nicholas Lambert (2012), for example, has argued that Britain planned for an economic offensive against Germany by way of blockades and proactive economic warfare. The title of his book is worth noting – *Planning Armageddon: British Economic Warfare and the First World War* – since 'planning Armageddon' suggests quite a different view of what went on prior to the outbreak of war than does Clark's 'sleepwalking'.

I believe that this discussion is sufficient for illustrative purposes. The historiographical debate on the Great War shows how each historical text can be seen as an argumentative intervention in the discursive field of historiography. Each text puts forward some specific point, which typically tries to correct some conceptions in the existing discursive field. Relying on argumentative resources requires that the historian provide reasons for his or hers in the Sellarsian 'logical space of reasons' formed by the existing historiographical discourse. Alternatively, the historian makes an inferential move in the Brandomian 'game of reasons' and makes a commitment to provide premises in the defense of the assertions made. Or perhaps one prefers the Skinnerian (very similar) locution, according to which the historian not only puts forward propositions

but makes 'moves in argument' (i.e. in argumentative discourse), which requires explicating the worth and purposes for making a particular claim. A successful intervention and defense give epistemic authority to the historian's claim.

The plausibility of a historical thesis depends on its impact within the argumentative field. To put it the other way around, a rationally well-formulated historical argument cannot be seen as well-justified if, for example, it completely ignores some widely accepted historiographical thesis on its topic, such as Taylor's or Fischer's above. Success depends on how a new argumentative intervention manages to pinpoint weakness or insufficiencies in the existing accounts or to add something new to them. For example, Fischer was very successful because he exposed the narrow focus of earlier historiographies on diplomatic sources.

It is equally important to realize that historiographical reasoning takes place in a specific cultural setting molded by various kinds of social and political interests. This is uncharacteristically easy to see in the case of the Great War debate as it was, and perhaps still is, unusually closely connected to political interests. The presumption was that if the understanding of the war guilt could be changed so that it did not rest on Germany alone, then the demand for reparation payments by Germany as defined in the Treaty of Versailles would ease. Indeed, prior to Fritz Fischer's *Griff nach der Weltmacht,* the revisionist school in Germany and the USA tried to show that a degree of guilt lay on all parties to the war. All governments also produced their own 'colored books' of key documents (white for Germany, yellow for France, orange for Russia, red for Austria-Hungary, blue for Britain and Serbia, gray for Belgium) to establish their innocence.

Since no historical argument is formed on the basis of source material and the 'internal' logic of reasoning alone, it cannot be evaluated by only paying attention to its internal argumentative qualities and to its relation to historiographical evidence. The prevailing argumentative field and the conceptions it holds, as well as the social and political interests that have shaped them, determine in part the nature and success of historiographical argumentation. A different argumentative field would have produced and required different kinds of interventions. There is no inevitability, convergence of historical debate or expectation of consensus in historiography.

As mentioned above, the argumentative field with its social and political shaping would – in the old terminology used in the philosophy of science – have amounted to 'external factors', and reasoning or the argument itself to something like 'internal logic'. However, I would

like to suggest that justification in historiography goes beyond the external–internal dichotomy or, alternatively, cuts through it. That is, although social and political interests shape the frames of the discursive field, historians' argumentative interventions are rational by nature. They may or may not be explicitly politically motivated but, due to the nature of scholarly historiography, they are in any case required to critique existing conceptions on rational grounds, to make a successful intervention and defense in the logical space of *reasons* – if they intend to be successful. The qualities of good argumentation are by and large shared by internal and external arguments, but their rational nature does not change the situation that they presuppose a politically loaded and socially shaped argumentative field. In this way, historiographical justification and historiography as argumentation crosses the internal/external dichotomy. An argument is successful if it answers existing theses well, if it shows them to be, for example, inadequate, inaccurate, too narrow, biased, and so on. In brief, historiographical discourse takes place in a politicized context, but on rational grounds.

Conclusion: justification in historiography

It is time to draw the whole evaluative framework together. In this chapter, the focus has been on the discursive dimension, that is, on historiographical texts as argumentative interventions in argumentative contexts. Chapter 5 examined the rhetorical dimension, that is, historiographical texts as reasoning for specific points of view. And Chapter 6 discussed the epistemic dimension, that is, the role of epistemic values in choosing and ordering colligatory expressions. These three dimensions together amount to the *cognitive justification* of historical works and specifically of the arguments that they contain (Figure 9.1). 'Cognitive' refers here to criteria that are relevant for historiography as a form of knowledge. However, as has been discussed previously, synthesizing historical theses should normally not be evaluated truth-functionally. Instead, historiography should be seen as making assertions concerning the past, and when these assertions are successful, they are warranted. The type of cognitive justification in historiography is thus that of warranted assertion. This view is in accordance with seeing historiography as a form of rational practice and as operating in the domain of rationality.

So, what is a 'warranted assertion' in historiography? It is a case in which the historian has managed to construct a rationally persuasive argument for some specific point or conclusion. In addition, this
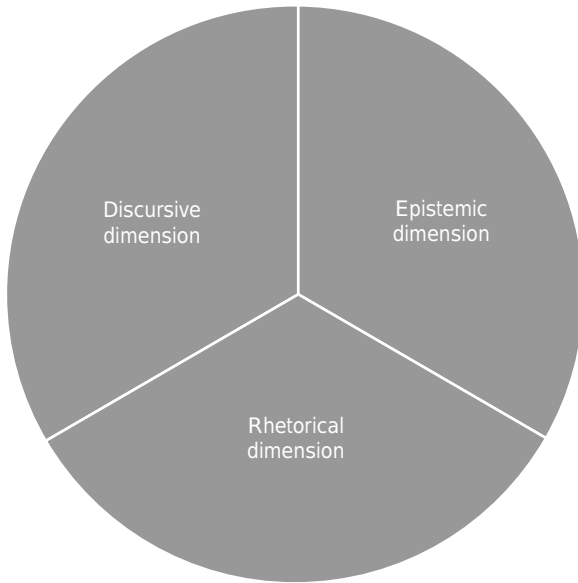
Figure 9.1   Cognitive justification in historiography

conclusion and its colligatory notions suggest an insightful way of making sense of the past and exemplify historical data – that is, provide a comprehensive account of the data available that is also internally coherent and subsumes the wide scope of phenomena under it. Further, the argument of the text also makes a successful argumentative intervention with regard to prevailing conceptions, questioning and correcting them in some way, while defending its own approach by reference to the argumentative resources presented in the historian's book.

Now it is time to make the transition to the last substantial chapter of the book, Chapter 10, which explores some broader philosophical topics in connection with the tri-partite justification of historiography. Where is historiography as a whole located on the axis of subjectivity and objectivity? In what sense can one talk about rational practice? What does 'rationality' mean? Are the phenomena asserted in historiography real with respect to the past? Can objects be real if they are constructed?

10
Historiography between Objectivism and Subjectivism

The essence of my view on narrativism and on justification in historiography has now been outlined. There are still a number of topics with wider philosophical significance that must be discussed. The first concerns the question of the objectivity and subjectivity of historiography. I will approach this issue by considering the senses in which historical knowledge can be said to be 'object-sided' on the one hand and 'subject-sided' on the other. In relation to this, I will analyze what is meant by claims that something is 'real' and by assertions that something is 'constructed'. Given that I have defined historiography as a rational practice that tries to construct arguments with rationally warranted conclusions, it will also be necessary to consider the question of what 'rationality' is. How strongly rationally compelling can historiographical theses be and how widely can their rational force extend?

These all are very broad topics. I deal with them from the angle of this book and only in so far as they are relevant to its specified context. Nevertheless, I hope that this discussion will provide a framework for the claims of this book and can offer fresh insights for further debate in the philosophy of historiography and related subjects.

The concept of objectivity

'Objectivity' and its opposite, 'subjectivity', are elusive concepts, which are widely used in philosophy and beyond, but are rarely clearly defined.[1] I suggest nevertheless that this old conceptual pair is very useful for expressing what is at stake in historiography. In brief, historiography practically always falls between objectivity and subjectivity and all works of history are combinations of 'objectifying' and 'subjectifying' elements. To put it bluntly, historiography is not categorically either

subjective or objective but always lies somewhere in between these extremes. Before explaining this view further, it is worth considering what these concepts, and particularly that of 'objectivity', mean.

One can distinguish between a number of meanings of 'objectivity'. My intention is not to give an exhaustive account of the various meanings but to outline some main ways for understanding the concept. A natural starting point is in noting that 'objective' is the opposite of 'subjective'. Thus, if something is objective, then it is not subjective, and vice versa. These two are opposites. Tastes, for example, are said to be subjective, which means that they cannot be objective at the same. By contrast, measuring is thought to produce objective results. For example, the height of a building is the same for all. A more specific study reveals a number of different senses of 'objectivity'.

(1) Let us imagine a subject, such as a historian, who constructs a view of an object, such as the past or perhaps some part of the past. The resulting view is objective if it is entirely *of the object* and thus devoid of any subjective elements such as personal or cultural tastes, bias and other prejudice. The object is *given*, and an objective view reflects what is given. I call this first sense of the 'objective' *ontological objectivity* because the objectivity of the view is determined by an independently existing reality as given. This sense is typically applied to nature and to naturally existing entities, but can be seen derivatively to cover also artifacts. Although they were once created by humans, they can be seen to exist objectively after the initial act of creation.[2] (2) The second sense of 'objectivity' is related to the first, but whereas ontological objectivity is an attribute of a view, this second meaning is about a certain attitude to one's research object. In order to be objective the researcher is required to approach the research object without any prejudice: neutrally. A famous expression of this view is the one by Ranke that the (scientific) historian must 'extinguish' oneself, that is, empty one's mind of all prejudices and interests and distance oneself from society and from societal interests. Discussion regarding the method(s) of science relates to this issue. Suppose that some individual is not, in fact, neutral, but his or her view is biased by political, religious, etc. preferences and viewpoints. It might still be possible to arrive at a neutral view if there is a method that guarantees it. To put it another way, a communist or a conservative can, according to 'methodological objectivism', in principle produce an objective view as long as one follows the right method. A good example of an attempt to guarantee objectivity in this sense is Popper's falsificationism. Popper does not place any restrictions on how knowledge can be produced, but every hypothesis has to stand the test of falsification,

which eventually produces an unbiased view answerable to experimental testing.[3] (3) The third sense of 'objectivity' is a derivative of the second. Some have concluded that neutrality is an impossible requirement and have also lost faith in methods such as source criticism as an objectivity-generating means for historiography. The suggestion is that one should nevertheless try to be as neutral or as fair and honest in one's judgments as possible (e.g. Appleby et al. 1994). Further, it has been proposed that if one cannot 'extinguish' one's subjectivity, one can at least consciously explicate one's hidden interests and biases in order not to project them onto the research object. It is clear that this kind of *aspiration* to be neutral is a weak conception of objectivity and has already granted much to the critic of objectivity. There is no guarantee that even one's best effort produces anything like bias-free and fair descriptions. (4) The fourth and final sense of 'objectivity' involves justification. Its starting point is perhaps the most realistic of the four. It assumes that all views are subjective but that this is not the end of the matter. Subjective idiosyncrasies may be removed by subjecting the views to intersubjective evaluation and criticism, possibly governed by some specific standards of evaluation. A view is thus objective if it is intersubjectively justified. The concept of 'objectivity' as intersubjectivity is common nowadays (Longino 1990; Martin 1993, 38), but it also has prestigious predecessors in the philosophy of science in logical positivism (e.g. Uebel 1992, 133–134). It must be pointed out that justification is quite a different matter from the ontological objectivity of the 'given'. Even the most stringently intersubjectively justified account may fail to reflect the world as it is. At the very least, in order to pair these two concepts, one needs to include assumptions on ideal communities or on the ideal limits of agreement in the manner of C. S. Peirce.

The object-side and the subject-side

Some statements by Thomas S. Kuhn have posed particular interpretative challenges for philosophers. One such is his view that 'though the world does not change with a change of paradigm, the scientist afterward works in a different world' (Kuhn 1970, 121). With regard to this, Paul Hoyningen-Huene reconstructs Kuhn's philosophy of science very interestingly in his *Reconstructing Scientific Revolutions* (1992). A particularly valuable distinction that Hoyningen-Huene makes is between two different kinds of worlds: the world-in-itself and the phenomenal world. The former is unknowable and purely 'object-sided', while the latter is constituted by the object-sided world-in-itself and by 'subject-sided'

moments originating with an epistemic subject (Hoyningen-Huene 1993, 31–42; see also Devitt 1997, 72, 156–157). Epistemic *subjects* are thus co-constitutive of the 'phenomenal world', which is 'the scientist's world' and which changes in a revolutionary scientific change. The world-in-itself in turn contain 'no moments on the side of epistemic subject' (Hoyningen-Huene 1993, 33) and is thus independent of these subject-sided moments.

While I do not want to commit to the existence of the Kantian unknowable world *an sich,* I find the notions of 'object-sidedness' and 'subject-sidedness' extremely useful as parameters for indicating the origin of our views of the world. The distinction connects well to the discussion concerning the notions of objectivity above. If one managed to construct a purely object-sided view it would be independent of any subjectivity of the constructor; it would be entirely *of the object*. This would correspond to the ideal of ontological objectivity. It is worth citing Kuhn here on the Cartesian epistemological tradition: it assumed that 'observations…themselves are fixed by the nature of the environment and of the perceptual apparatus', which is seen as universally shared. No cultural, psychological or other subjective factors affect the process (Hoyningen-Huene 1992, 37; Kuhn 1970, 120). On the other hand, a view that is a total fantasy has nothing to do with the subject-independent object world. In this case it would be purely subject-sided or, in brief, a subjective view.

In the early stages of modern historiography toward the end of nineteenth-century and the beginning of the twentieth-century, attaining objectivity was seen as an achievable prospect. Ranke's influential request to write history *wie es eigentlich gewesen* may be taken as exemplifying the desire for ontological objectivity (cf. Iggers 1962, 1973). In recent years, objectivity as an aim in historiography has been a subject for criticism and has even been seen as harmful by some accounts. Often the explicit target of criticism has been objectivity as neutrality (e.g. Novick 1988; Newall 2011). Typically the question of whether an objective account is possible has been put very categorically. One tries either to prove that historiography is/can be an objective science or otherwise one succumbs to a position that historiography is inexpugnably subjective for some reason. This kind of dichotomical thinking can, perhaps surprisingly, be found even among postmodernists. For example, for Keith Jenkins and Elizabeth Ermarth there is only subjectivity, that is, the validity of any view is only the validity of its creator for its creator. Pihlainen argues that 'all stories and ways of structuring material' are 'equally imaginary', which necessitates abandoning 'the last illusions of

history as somehow "objective"': 'Objectivity as it has traditionally been understood within history is simply not an option' (Pihlainen 2013b, 516). While the critique of the traditional take on objectivity in historiography may well be justified, it is nevertheless ironical that although postmodernists are the most vocal in talk against dichotomies and dichotomical thinking, they are the ones who seem to commit to these most strongly (that is, postmodernist–modernist historians, subjective–objective, etc.). In the case of objectivity, they seem to understand it to mean some very strong notion of ontological objectivity, and when that is judged not to be universally applicable, the argument ends with the conclusion that we are confined within our subjectivity.[4]

More specifically, the narrativist and postmodernist argumentation takes the following form. 'Narrative' is recognized as the main knowledge contribution of historiography and is identified as a holistic entity that possesses qualities not found in the research object, that is, in the past. It follows that ontological objectivity is impossible and that narrative contains irreducibly subject-sided properties. And because narrativity defines historiography as a scholarly practice, historiography is indispensably subjective. Reference to facts to provide the bedrock for narratives does not help. First, the talk of 'facts', including the border between 'fact' and 'non-facts' is problematic. More importantly, the relation of 'facts' to the higher-order form 'narratives' is unclear, to say the least. As we have seen in Chapter 3, as a consequence of being identified as literary entities, narratives are analytical, unfalsifiable and immune to other narrative challenges.

An alternative view of the relationship between objectivity and subjectivity is based on the rejection of 'narrative essentialism', as it was called in Chapter 5: narrativity does not define historiography, that is, it is not an essential feature of a historical presentation; instead, reasoning and argumentation characterize historiography. As a result of this change of perspective, the *form* of presentation does not deem historiography subjective (even when we focus on texts and complete works of historiography), holistic and automatically immune to empirical challenges. Further, the form does not make it impossible that some features are grounded in the objective-sided world. With regard to the presentations of historiography and their components, one needs to be more circumspect than to assume that they are all of one and the same kind.

I would like now to suggest that one learn to live with the 'sliding scale of objectivity and subjectivity'. In others words, one would talk about the degrees of objectivity and subjectivity that a particular historical presentation possesses. And the degree of objectivity and subjectivity

must be investigated case by case. Moreover, my view is that good historiography may possess objectifying features in both ontological and justificatory senses of 'objectivity.' First I show how the notions of 'objectivity' and 'subjectivity' apply to historiography in the ontological sense. The application of 'objectivity' in its justificatory meaning will be explicated below, when the topic of rationality is discussed.

I go back to the distinction between subject-sidedness and object-sidedness. The distinction differentiates between those entities that derive from an epistemic subject and those whose ontological standing is independent of human cognition. If all knowledge were derived from the object-side, one would have a purely objective account. Note however that this may not necessarily be the most desirable outcome, even if it is possible, as the result might not be very interesting historiographically. I will elaborate on this below. Note also that the distinction between object-sidedness and subject-sidedness has the advantage that one does not need to commit to the metaphysically problematic notion of 'facts' and their separation from 'non-facts', as the narrativists, postmodernist and also historical realists typically seek to do.

It is best to first mention an example of the object-sidedness of knowledge that one can find in books of history. The simplest case would be a claim that refers to a person and a state of affairs that hold with regard to that person. For example, 'Stalin owned a gun' or 'President Urho Kekkonen was born in 1900'. I do not know whether Stalin owned a gun, but if he did then this sentence is true due to object-sided factors, and if he did not then the sentence is false. We may of course never ascertain this, but that is a different matter.

The reader may already be thinking that books of history only very rarely make *cases* for these kinds of simplistic objectifying statements, although such statements are, of course, part of a historiographical text. The thought that historians form synthesizing interpretations or other integrative views and concepts immediately implies that historical knowledge contains subject-sided elements. Indeed, insofar as the central historiographical craft is the production of texts, as the narrativist philosophy of historiography has emphasized, I take it as a (new) default position that historical knowledge is at least in part subject-sided.

What kinds of elements may then be said to contribute to the subject-sidedness of a historical presentation? There are four main categories that I can think of: (1) the absence of reference; (2) the postulation of a nominal categorizing principle; (3) the postulation of narratively construed causal relations; and (4) the postulation of meaning or sense to the past. In what follows, I will introduce an example of each.

1. The absence of reference is common with colligatory and metaphorical concepts. Any entity to which a linguistic term can refer is 'reference'. Most typically it is an individual, a person, to whom a name refers, but it can also be any other object insofar as that object exists independently of linguistic postulation, as some specific gun of Stalin's. Normally, reference is to something materially existing but some philosophers also accept abstract entities as objects that can be referred to. For example, a colligatory expression is an arrangement of historical data into larger wholes and does not itself refer. Arguably there is no such entity or even process as the 'Renaissance' that is given and exists prior to historians' practice. This was discussed extensively in Chapter 6. And if there is no reference, then the 'Renaissance' is a constructed and subject-sided historiographical entity.

2. The postulation of a nominal categorizing principle means a situation in which a historical category has been formed and a number of lower-order entities are subsumed under the category concept but the organizing principle is not given in the object-sided world. It is not 'natural' in any sense. When would such a principle be 'given'? The best example would be the case of natural kind categories. Elements are typical examples of such. Why are all samples of gold of the same kind, that is, why do they belong to the same natural category? This is because there is an essential property, perhaps definable by the possession of the atomic number 79, that they all share and that is allegedly objectively given in the sense that it would have been there even if no human had ever existed. Now let us compare this case once again to an example in historiography. The 'Renaissance' can be seen to organize a very diverse set of objects, paintings, furniture, dressing styles, literature, thinkers, etc. with the consequence that there is no shared property across all the objects that are organized under the concept. This would be a straightforward case of subject-sided organization. But even if one postulated a narrower definition of the 'Renaissance' so that all instances of the 'Renaissance' need to share some kind of artistic style, for example, it is still the case that this feature is not natural in the sense that it would give us a self-evident category devoid of the subjective choices of the historian.

3. The postulation of narratively construed causal relations means that a historian postulates a complex narrative of historical events that together form a historiographical object, such as a process. E. P. Thompson's (1991) thesis of the emergence of the English working class could be seen as an example of this. It unites a very large set of historical events, including methodical religious ceremonies at the

end of the eighteenth-century, the meetings of London-based learned societies at the beginning of the nineteenth-century, illegal activities after 1815 in Britain, changes in the material and economic situation of skilled workers, etc. One can hardly argue for the existence of explicit object-sided causal connections between all these events, even if these events are central to Thompson's thesis on the birth of the English working class. It appears that causal relations are absent in the object world and the links, causal and other, between the events are postulated by the epistemic subject, that is, by the historian.

4. The final case deals with something that especially postmodernists are keen on emphasizing. That is, that historians add some kind of meaning or sense to the past, which cannot be said to be a property of the past itself, but derives, for example, from literary and cultural tropes and emplotments. A good example is Clark's (2012) portrayal of the main players in the development towards the Great War as 'sleepwalkers'. 'Sleepwalking' does not seem to be ingrained in the historical world itself, but is indeed a particular interpretation by a particular historian. In other words, the sense of sleepwalking is a subject-sided addition and a historiographical construction.

My suggestion is that practically all historiography needs to be considered as both subjective and objective at the same time; we should thus talk about the degrees of objectivity and subjectivity of a particular work depending on the specific combination of objectifying and subjectifying elements. It is clear that all books impose meanings on the past and contain straightforward referential statements, metaphors, colligatory notions, narratively construed causal claims, etc., the elements of which all relate to the research object in different ways.

In order to illustrate how subjectifying and objectifying elements can combine in a work of history, let me return to the idea that a rationally warranted colligatory notion must exemplify the historical data it applies to (as discussed in Chapter 6). The implication is that the descriptive content of a colligatory expression must appear to be appropriate with regard to historical evidence, which in turn entails that what is colligated, lower-order statements, must refer to the historical reality. If the release of the prisoners from the Gulag is a colligated feature of Khrushchev's 'Thaw' then, on some fundamental level, the expression must refer to actual 'given' prisoners and the locations at which they were held. They constitute in this sense the *objective grounding* of the notion of 'the Thaw', which is nevertheless a colligatory and thus subjective construction to a high degree. And it is worth emphasizing this point once more. One

should not see the notions of 'subjective' and 'objective' as exclusive; not even in the case of ontological objectivity. The subjectivity that the historian imposes often means the integration of several events, which gives them some specific significance. If they were not object-sidedly grounded at all, they would be entirely fictional entities and could not possibly be informative concerning the research object.

The scale between subjectivity and objectivity is sliding. At one end, we might perhaps imagine a book whose only point is to prove, say, that President Urho Kekkonen was born in 1900. Perhaps the year of his birth had been a mystery for a long time, or there had been lots of conflicting sources and other information on it to provide the incentive for a historian to straighten it out (in actuality, there is not). Given that there really was an Urho Kekkonen and his birth occurred in that year, the truth-functional standing depends on the object. (Now, of course the Julian calendar imposes a culturally loaded standard, which means that not even this statement is 'purely' object-sided). The problem, if one wishes to call it such, and the narrativist insight, as it was called in the first chapter of the book, is that historiography typically goes beyond and above these kinds of simple referential statements and puts forward synthesizing theses concerning the past, the consequence of which is that one loses the possibility of attaining 'pure objectivity' from the very start. The one who sticks to grassroots-level expressions will end up impoverishing historiography and removing its most interesting expressions. Logical positivists tried to purify scientific language in the 1920s, but that attempt ended in failure. I would like to suggest that we would need very strong reasons for trying something like this again because the rationale would be external to historiography. That is, the motif for the simplification of well-functioning historiographical language would stem from the concerns of philosophy and philosophy of science.

It is time to end this section on ontological objectivity with an illustration of the sliding scale of objectivity. This is meant as a hermeneutical tool to illustrate the main idea. All the examples located on the axis are texts. *Alice's Adventures in Wonderland* by Lewis Carroll has hardly any referring statements to actual persons, their properties and the states of affairs. It is a work emanating mostly from the subject-side, although not necessarily entirely.[5] Stephen Clark's *The Sleepwalkers* contains many referential statements, but ends with a synthesizing thesis on the process towards the Great War as 'sleepwalking'. It has other narrative and metaphorical parts that could possibly not literally 're-present' the past. The third example is an imaginary study, whose central thesis is that 'Urho Kekkonen was born in 1900'. Whether or not he was born in this year

Alice in Wonderland	*The Sleepwalkers*	'Urho Kekkonen was born in 1900'

Subject-side   Object-side
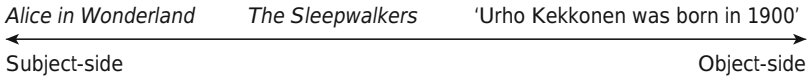
Figure 10.1   The axis of subject-sidedness and object-sidedness

depends on the external states of affairs in the past. That is the closest it is possible to get to an objective work of historiography.

The reader has probably noticed the inverse relationship between object-sidedness and the originality of these works. Any book of historiography that intends to prove that someone owned some object or was born in a specific year is not very exciting; nor is it very interesting in normal circumstances. It can be very secure knowledge of the past, but very trivial and uninteresting at the same time. But if one argues that the main players in the process towards the Great War were 'sleepwalkers', then the claim is much more interesting. Or if a historian argues that the Finns – or at least a significant part of Finland's elite – self-censored themselves and profoundly changed the Finnish political culture of the 1970s under pressure from the Soviet Union (the thesis of 'Finlandization'), the historian makes an interesting statement. Historians are thus faced with the following dilemma with regard to objectivity and subjectivity: The more a historian is willing to state about the past, the less objective but

Figure 10.2   The inverse relationship between originality and objectivity in historiography

more original the historian's account will be; and the more the historian desires objectivity, the less he or she is able to state and the less original the account will be.

It is necessary to emphasize that I do not see any problem with the inverse relationship between originality and objectivity. It just reflects the nature of historiography. Once again, one would not wish to see firmly objective but minimally interesting and insignificant theses, but neither would one want totally outlandish claims with near-absent objective-side grounding. Historiography, denoted by the black arrow, is located somewhere between these two extremes, as the figure shows. In different ways this view connects to those of some predecessors. It is well known that Popper argued, in his demolition of inductivism in the history and philosophy of science, that scientists should attempt to falsify rather than to prove scientific hypotheses as correct. The bolder a scientific hypothesis is, the higher degree of falsifiability it has. It is thus easy to falsify, but if it is stands the tests, it amounts to an extremely interesting and explanatory hypothesis: 'every interesting and powerful statement must have a low probability; and *vice versa*: a statement with a high probability will be scientifically uninteresting, because it says little and has no explanatory power…*as scientists we…seek explanations; that is, powerful…theories*' (Popper 1989, 58). The issue of falsification is not as simple as Popper thought but his view nevertheless expresses something essential about scientific practice. What Einstein did was not to put forward secure observational statements on the environment in his near proximity but risky conjectures about the universe. Obviously, this analogy should not be overstretched in the context of this book since I do not think that there is a role for clear-cut falsifiability and certainly not for verisimilitude in historiography, as Popper assumed there is in science. Other predecessors can be found from nearer the field of this thesis. Dray pointed out that the idea of originality in historiography leads directly back to the discussions on the possibility of objectivity in historiography (Dray 1991, 173). Martin argues that our best and most fully developed interpretation will always occupy a halfway house between literature and science. That is not necessary, writes Martin, but 'that is the way we like our historical interpretations' (Martin 1993, 49). Martin concludes that 'perfect objectivity' may not be desirable, because we simply want something more in historiography. Finally, Ankersmit suggested at the end of his *Narrative Logic* that the 'the essential duty of the historian is to be original and to refrain as much as possible from repeating what his predecessors in the investigation of a particular topic have said' (Ankersmit 1983, 220). But again there are differences too. Ankersmit's comparison of 'narratios'

to propaganda fails because historiographical theses are answerable to rational evaluation. More importantly, Ankersmit links objectivity in historiography to scope so that 'The most objective narratio, the narratio having the widest scope, is the least conventionalist, the most *original* narratio' (218).[6] I think it is exactly the other way round, terminologically at least: an account that owes most to the subject-side is bound to be the boldest and most original.

Semantics of the 'real'

Someone might accuse me now of backing away from my own proposal in Chapter 4 to reject representationalism in favor of non-representation-alism. The section above employed the conceptual pair subject–object. Let me explain how this accusation is harmless or misdirected. I said that I do not have a problem with representationalism as such, as Rorty for example has. I am not a global anti-representationalist. It is not a problem for me to accept that some expression may have a clearly definable object to which it refers. The target of my criticism was the assumption that synthesizing historical theses must be seen as 're-presentations', that texts on the whole must be seen as referring to some (abstract) objects. In my view, this mystifies the central insight, which is that historical theses are *performative argumentative acts*. But, of course, if no parts of argumentative speech acts refer or deal with object-sided entities at all, they would not have much informational value with regard to the past.

Now there is another question that may concern the reader. Are the historical phenomena argued for 'real'? Is there 'really' something like 'sleepwalking' that preceded the Great War or a 'Thaw' after Khrushchev? I have already made clear that these should not be taken as referring expressions and thus not as entities in the past to be referred to by the historian. In this sense, they are not real but colligated synthesizing expressions. There is, however, more to be said about this. The talk of 'reality' and the claims about what is 'real' imply something more. For this reason, it is worth considering the semantics of the 'real'; to consider what is meant by reality-talk. Further, I suggested that a central tenet of the narrativist philosophy of historiography is constructivism, and subsequently agreed about the constructivist nature of historiography (but rejected two other tenets, representationalism and holism). Constructivism relates to the theme of the 'real'. The thought of a constructed entity may suggest unreality. If an entity is constructed, can be it 'real'? In what follows, I will examine briefly the meaning of constructivism as well.

It is often assumed that if something is constructed, it is not real. And in some cases, to claim that a certain X is constructed is bound to prompt accusations of moral irresponsibility and nihilism. Kukla writes that our moral sensibilities are outraged by the suggestion that the destruction of Hiroshima was constructed and not 'real' in the sense that it would exist in an independent realm. This conveys the message that constructor-independent events are 'morally weightier' than constructed ones, although it is not clear why this should be so (Kukla 2001, 49). Indeed, the argument regarding the 'unreality' of some object is often an attempt to discredit the opponent's position on moral grounds; it is to claim that the constructivist position is somehow irresponsible.

Overall, it is much more difficult to understand what the meaning of the 'real' is than may initially seem. Those who insist on the 'reality' of something typically attach some extra layer of significance or weight to an event or object that is 'real'. The danger, and too often also the outcome of reality talk, is a situation in which the meaning of the 'real' is left entirely vacuous. This is the situation that Arthur Fine aptly parodied. The default position of both those who argue for the 'really real' and those who are skeptical of the value of this locution is the acceptance of the results of science or historiography, provided that the results have emerged out of normal disciplinary deliberation and justification. What can the reality talk of the realist add to this? Fine writes:

> What the realist adds on is a desk-thumping, foot-stamping shout of 'Really!' So when the realist and antirealist agree, say, that there 'really' are electrons and that they really carry a unit negative charge and they really have a small mass (of about 9.1×10^{28} grams), what the realist wants to add is the emphasis that all this is really so. 'There really are electrons, really!'(Fine 1996, 129)

Of course, much more may be implied by reality-talk than desk-thumping and a shout. Fine thinks that the realist wishes to link the 'real' to truth or existence. The 'full-blown version' includes truth as a correspondence with the world and the 'surrogate' version implies truth as approximate truth, that is, as near-correspondence (Fine 1996, 129). The idea seems to be that 'real' objects have some kind of robust and tangible existence in an *independent realm* about which one can make true and false assertions.

However, I don't think that the existence of objects in an independent realm is an issue of contestation, although the label 'unreal' is often applied to constructed objects such as fiction. Many artifacts in our daily

lives are indisputably constructed, like smart phones, for example, but they are real nevertheless. No one would want to say that his or her most recent iPhone is not real just because someone has designed and manufactured it. Indeed, Paul Boghossian's (2001) 'metaphysical social constructivism' is a claim according to which something is real but of our own construction. Further, Latour has often been rightly or wrongly seen as a pernicious (social) constructivist, but his point is not to cast doubt on the reality of objects: 'When we [actor network scholars] say that a fact is constructed, we simply mean that we account for the solid objective reality' (Latour 2005, 91). Constructed material objects would seem to be able to function as truth-makers once they are constructed. One can make, in principle, objectively true and false statements of a constructed iPhone. Finally, talk of the 'real' may also connect to various philosophical doctrines of what fundamentally exists. For the materialist, only material objects would be 'really real' and for the idealist only mental or spiritual entities; whereas the physicalist would accept all physical phenomena as a truly 'real' substratum of the world, etc.

Provided that there is no unanimously accepted or even generally approved definition of the 'real', there are a number of options from which we have to choose. The first possibility is to endorse Fine's treatment of the 'realist' and assume that appeals to the 'real' or the 'really real' are rhetorical tricks designed to discredit the opponent's argument without any further substance. The second option would be to understand the 'real' as something the existence of which is not doubted (but could be doubted) in the fashion of Latour. This typically applies to all our everyday material objects but also, for example, to the corroborated results of science. The third possibility is to think that the 'real' is simply synonymous with 'unconstructed'. The 'real' would be something like independence from human construction (but more on constructivism below).

Unfortunately, all these options have their disadvantages. To always dismiss talk of the 'real' as a rhetorical trick would probably be unfair in many cases since something potentially more substantial may be implied. Further, to tie the definition of the 'real' to a collective endorsement would have the unintuitive consequence that something that was once real may not be so any more just because people changed some of their beliefs. In this view, witches were real in the Middle Ages but not anymore. Finally, as already remarked, it does not seem correct to say that something that is dependent on our construction is not real because of that dependence. The context of everyday objects was already mentioned but one can also find examples from the domain of

natural sciences. The periodic table contains over 20 elements that are not found in nature and require synthesizing by humans. The reality of artificially derived elements would dawn on any human being if he or she came into contact with them as they tend to be radioactive. It is reasonable to assume that the unconstructed is real, but, to repeat, the same applies to the constructed too.

Provided that it is not possible to find unanimous agreement regarding what 'real' claims mean, and that they are often ways of claiming epistemic authority and credibility for the objects deemed 'real', the most appropriate course of action is to pay attention to a historiography-specific problem, that is, to the standing of colligatory and synthesizing objects in this regard. Colligatory and other synthesizing expressions organize data into historically meaningful patterns, but it was argued earlier that there is no privileged organization or colligation available. James McAllister has analyzed the relation between data and patterns interestingly. Although his analysis applies to data in the physical sciences above all, it also provides a way for understanding 'real'-locutions in historiography.

McAllister understands a 'data set' as the outcome of individual observations and experiments. A 'data set' is composed of two components: a pattern and a certain level of noise. 'Noise' equals those data points that go beyond the pattern exhibited in any specific case. McAllister argues that 'any given data set can be described as the sum of any one of *infinitely* many distinct patterns and a corresponding incidence of noise' (McAllister 1997, 219; my emphasis). For example, data on economic activity can be seen to exhibit patterns corresponding to economic cycles of various durations. If one wishes to have a zero 'noise' level, one has to reproduce exactly the entire set of data, which is something that no theory in science (and no interpretation in historiography) does. McAllister's (1997) main point is that the notion of investigator-independent phenomena as the objective features of the world cannot be thought to provide us a pattern in data. Instead, 'the term "phenomenon" is…a label that investigators apply to whichever patterns in data sets they wish so to designate. Thus…which patterns count as those corresponding to phenomena is entirely a matter of stipulation by investigators' (224). McAllister writes that while no scientists try to explain all the patterns that a data set exhibits, the pattern chosen is 'not dictated by the world: it is open to investigators to decide' (1997, 227).

McAllister (2010) expands his considerations of the relationship between data and patterns to the question of what kinds of structures it is possible to find in empirical data. He notes that it is common to assume

that one can divide empirical data into a relatively simple component that corresponds to structures in the world and a more complex one that does not. The former he calls 'physically significant patterns'. The division into two types of patterns is used to identify the patterns that exist in the world. McAllister, however, argues that it is impossible to physically distinguish significant patterns from insignificant ones and suggests that because of this we ought to consider the possibility that no such separation exists (2010, 809). These considerations lead him to his radical thesis that the world is 'radically polymorphous' – that is, it contains all possible structures:

> Distinct empirical data constitute evidence of, and in this sense, corresponds to, distinct structures in the world. Coupled with the finding that, mathematically, empirical data show all possible patterns, this suggests that empirical data constitute evidence for the proposition that the world contains all possible structures. (McAllister 2010, 810)

Finally, McAllister notes that the fact that one pattern is picked up does not show that alternative patterns do not exist in data. He concludes by disputing any allegations of relativism: 'all structures are objectively real for all observers' (McAllister 2010, 811).

One might say that McAllister's considerations amount to a form of the radical underdetermination thesis when theories are seen to individuate a pattern in data. That is, all theories are radically underdetermined by any set of data. In other words, no amount of or quality of data alone is sufficient for inferring the uniquely correct pattern. I suggest that this is essentially the case also in historiography even if the data in historiography are typically not the outcome of observations and experiments, but source material of various kinds, including masses of text, pictures and other artifacts. This data may make many interpretations appear unreasonable, but there are no grounds to claim that historical sources contain naturally given patterns. Although I would maintain that infinite avenues for possible interpretations exist within any set of data, I would not claim that data in historiography contain all possible structures. Phenomenological narrativists might note, correctly, at this point that much of historiography is not lifted out of source materials, but that narratives and stories exist prior to the 'configuration of the field' by the historian. However, *this* does not make some 'narratives' more real or natural than others with regard to historical phenomena or historical reality, but only pushes the problem one step further. Similarly, McAllister notes that reliance on prior stipulations

regarding a data set assumes that investigators knew the patterns at an earlier point in time (2007, 222; 2010, 814). The phenomenological narrativist could of course argue that only the existing narratives are the target of historiographical investigations, but this would arguably limit the scope of historical investigation unacceptably and yet remain unmotivated philosophically.

Pihlainen asks, 'What may legitimately be inferred from these individual facts? And the answer, again: Nothing, really' (Pihlainen 2013b, 512). Pihlainen has a point there. But, following McAllister, we can turn the answer the other way round: 'Quite a lot. Actually, too much'. McAllister's stipulation that all patterns inferred from empirical data are real is a very interesting and novel suggestion in the dispute between realists and anti-realists. The narrativist philosophers of historiography, for example, are anti-realists in that they think that none of the configurations of historical data and narratives show us the real structure of the historical world. White famously claimed that evidence does not at all constrain the choice of tropes, which in effect means that all tropes are *epistemically equally unjustified.* By contrast, McAllister does not thus dispute the reality of patterns but only the claim that 'the world has a unique structure and [that] there is a single true theory of the world' (McAllister 2010, 2013). McAllister's idea can be used to turn the 'problem of unreality' of all historical interpretation into the claim of their reality as long as it is remembered that there is no uniquely correct interpretation or colligation of the historical world.[7] Even better, there are two available options that at first seemed to be contradictory, but can now be seen to represent two sides of the same coin: given that there is no inherent or privileged organization, a colligated or otherwise synthesized entity may be seen as either real or unreal, from the standpoint of one's favored philosophical framework, *as long as the colligation is epistemically (and more generally cognitively) warranted.* The possibility of finding other kinds of colligations does not make it or its alternatives unreal since they can all be seen as being warranted. Beyond the cognitive warrant the 'real' talk is little more than an attempt to attribute epistemic authority and credibility for an entity. There may of course be many other criteria for choosing between cognitively warranted colligations of historical phenomena.

This is my solution to the problem of the claims about what is 'real' and it is up to the reader to choose realistic or anti-realistic language in this matter. My suggestion is to opt for the side of 'real' talk. A consequence is that although the Holocaust, for example, as a colligated historiographical phenomenon is not carved as a given in the 'joints of

the past', it has a strong cognitive warrant and epistemic authority as a consequence of which its reality should not be doubted. The reality claim, insofar as it goes beyond the cognitive warrant, says that all the atrocities of the Nazis should be considered as one, colligated under one comprehensive concept, and seen as one phenomenon from the perspective of this colligation.

Constructivism re-considered

The aim in this section is to consider what 'constructivism' means. It is instructive to begin this consideration with Leon J. Goldstein, who wrote prior to the narrativists and understood historiography as a specific form of constructivism:

> The function of historical research is to *constitute the historical past.* As much as we want to say that a true account of some past event is true in virtue of the fact that it accords with what actually took place when the past was present, we have no way to *make* that belief *operative* in historical research. 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. (1976, xix; my emphases)

First it seems that Goldstein makes a bold thesis of metaphysical constructivism ('constitution'), but then it becomes evident that Goldstein is actually, and somewhat confusingly, talking about an epistemic problem. Indeed, later he refers to the role that historical assertions play in the 'verification' (1976, 3). His point is not to deny that there was a past, but that 'the past' as a metaphysical notion does not have a role in the historian's practice. Goldstein's position, and in his words 'the worst fears of the empiricist and the realist', is that 'we have no access to the historical past except through its constitution in historical research' (Goldstein 1976, xxi).

   Goldstein's 'constitution of the past' ultimately boils down to the view that a historian has only inconclusive and incomplete evidence available out which to infer an account of history, or that the historian '*constructs* a course of events which is supposed to make sense of what he has and knows' (1976, 15; original emphasis). This is achieved by 'an intellectual process of hypothetical reconstruction, or constitution, which in no way resembles perception' (Goldstein 1976, 15; similarly 58). This kind of constructivism appears to be a satisfactory description of how

the historian arrives at his or her view. But it is ultimately unsatisfactory as a philosophical account because it does not address the question of whether the view so constructed could be true, in principle, of the 'metaphysical' past, assuming that we had no epistemic problems, as an omnipotent creature would not. Goldstein would probably accuse me of imposing a notion upon history which is brought 'from outside history, which history can never satisfy' (Goldstein 1976, 27). While I agree that the philosophy of historiography should be discipline-specific, I don't think that the application of philosophical notions in the context of historiography should be outlawed. It is true that one should not force historiography into any specific philosophical framework just because of the commonly used philosophical concepts and presuppositions (as in the case of relying on 'experience' as the foundation of knowledge). However, provided that this does not happen, any philosophical concept can be used to illuminate the discipline-specific questions and the nature of historiography. What Goldstein does not explicate is the fundamental problem with truth-claims in historiography: could the past in a metaphysical sense be structured like the output of the historian's inference, if we do not worry about the actual verification of the account?

Some have defined constructivism as a position according to which the objects of knowledge or facts are not independent of the knowing subjects and human activity (e.g. Kukla 2000, 19). This definition may initially look attractive with respect to some non-constructed object, such as a stone, for example, which appears independent of any knowing subject, in contrast to a painting, for instance. However, this definition is problematic even in the sciences as implied above. Some objects in science, such as certain rare elements and probably most synthesized objects in laboratories, simply are not independent of human activity. One might even say that their 'reality' requires construction. This might naturally be taken to mean that the application of constructivism is much wider than philosophers of science have assumed, which is arguably something that scholars in science studies would indeed claim. In the case of historiography, the situation is still different with regard to colligatory notions. Namely, the crux of the matter is not that their object of research is dependent on human intentional activity but that, strictly speaking, they do not have an individually specifiable research object (reference). That is the sense in which colligations are constructions.

The definition of non-constructed objects as being independent of the knowing subject above is problematic also for another reason. Even if an *object* of knowledge could be assumed to be independent of the knowing subject (e.g. gravity) this would not necessarily be decisive since it can

hardly be maintained that *knowledge* of this object is independent of the knowing subject. It is my suggestion that constructivist talk applies best to the *cognitive products* of science and other fields. To say that a specific cognitive product, 'knowledge', is constructed is to say that it owes much to the subject-side and to claim that it is not constructed is to claim that it is in large part determined by the object-side. To put it in other words, the non-constructivist, and literally objectivist, position entails that knowledge merely reflects what there is. This kind of epistemological non-constructivism naturally entails that the object of knowledge exists independently of the knowing subject (although not necessarily independent of any knowing subject, since a previously ontologically constructed object can become an object of knowledge at a later stage). The constructivist position in turn says that the knowing subject determines knowledge, at least in part. We might then say that constructivism in historiography means that historiographical objects are dependent on the historian's activity – and thus on the subject-side. In other words, historiographical objects such as the 'Thaw' or Thompson's thesis concerning the birth of the English working class would not come into being without a historian's construction. It is necessary, again, to remark that constructivism in historiography does not need to include all kinds of historical objects. Finally, it remains to be said that constructed objects in this sense can be real if they are cognitively warranted. One should also remember that cognitive warrant implies adequate exemplification of data and minimal truth-functionality among the lower-level entities.

Rationality and transcendence

The narrativist philosophy of historiography in its early period, and most vocally in Ankersmit (1986; 1989a), argued for the rejection of the epistemological evaluation of historiography. This does not mean that the narrativist philosophy of historiography wanted to abandon the evaluation of narratives altogether or to accept some kind of 'anything goes' principle with regard to interpretation. It should also be noted that this does not automatically imply relativism, because the suggestion is not that epistemic evaluation is relative to some S, but that we ought not to use epistemological evaluation at all. Instead it might be said that the early narrativist philosophy of historiography recommended that we substitute one form of rational evaluation, the epistemic one, with another form of rational evaluation, namely that of assessment by aesthetic and moral criteria. Aesthetic and moral judgment naturally do

not provide an algorithm on the basis of which to decide what historical interpretations or narratives are preferable, but the discussion of aesthetic values and moral goodness can still (arguably) be practiced on rational grounds. If these standards cannot provide unequivocal determination of the best interpretation, they do not make the process entirely arbitrary either.

In general, one can examine the narrativist analysis of historiography and reactions to it in terms of three different issues: (1) epistemic evaluation; (2) rationality; and (3) transcendence. Assessed with these parameters, the narrativist rejects the first, endorses the second and most likely rejects the third, that is, the option that the standards of evaluation could transcend our culture or community.

Postmodernism recommends, just like the narrativism, that we reject epistemic evaluation. Munslow writes that epistemology itself is a problem for a historian (Munslow 2010, 15; Jenkins 2003; cf. Jenkins 2008a, 69). Yet postmodernism can be seen to go further than this. As we saw earlier, the most radical form of postmodernism would commit to individualistic subjectivism, which evaluates historiographical interpretations only by assessing how they serve the interests of an individual and specifically his or her political emancipation (e.g. Jenkins 2003, 14, 18; Munslow 2007, 116). This would come very close to abandoning any kind of rational evaluation. In other words, 'anything goes' as long as it is beneficial for an individual in his or her attempts to make sense of and cope with the world. However, if interpretations are nevertheless seen as aesthetic and ethical artifacts, evaluation does need to go beyond the concerns of individuals. That is, aesthetic and moral values cannot arguably be entirely individualistic but must be socially and communally assessable. Their aesthetic value or moral worth needs thus to be evaluated on a non-arbitrary, principled, basis. There must be some standards to form a judgment that the Mona Lisa is aesthetically valuable or that a racist historiography is morally questionable. The existence of such standards does not imply any point-by-point evaluation, but entails the possibility of rational discourse nevertheless. This would mean a commitment to inter-subjective communal (non-epistemic) rules of evaluation. Naturally, no postmodernist would go further than this and suggest that aesthetic and moral standards could transcend the level of communal construction and communal validity.

Here it is worth making a distinction between those 'communitarians' who wish to employ epistemological standards and those who do not. Postmodernists belong to the latter group, as does Rorty. Rorty attempted to develop 'epistemological behaviorism' as an alternative

to 'philosophy-as-epistemology', as Michael Williams called it (2009, xiv). For Rorty, communal conversation is the ultimate context that defines knowledge and within which it is to be evaluated. He specifically opposed what he called the 'objectivist tradition', which 'centers around the assumption that we must step outside our community. This tradition dreams of an ultimate community which will have transcended the distinction between the natural and the social, which will exhibit a solidarity which is not parochial' (Rorty 2011, 22). The former group includes those who would stick to epistemic standards, but only on a communal basis or within a community. For example, Tucker makes a principled and formal commitment to a community-based epistemic evaluation of historiography. According to Tucker, if a *sufficiently large* and *heterogeneous* group reaches an *uncoerced* consensus, the *group consensus* is a fallible but nevertheless reliable indicator of historical knowledge (Tucker 2009, 29–36). This communitarian suggestion is commented on positively by John Zammito (2013, 414).

The problem with the communitarian solution is that any agreement or knowledge becomes community-relative. Is there any guarantee that consensus has any cash value in another community? And is there any way of knowing whether this community-based consensus or 'knowledge' is of a good quality itself; that it is not merely communally endorsed. Everyone is familiar with consensus in the past regarding the fact that the Earth is flat or that certain women are witches (and therefore, deserve to be burnt as a punishment). Although communities may have reached a consensus on these matters, we would certainly not consider them to constitute knowledge nowadays. Tucker tries to deal with relativism by means of probabilistic thinking and by recourse to the knowledge-conducive values that have led to a particular consensus. It is obvious that this will not automatically give us anything like community transcendence. One would need to say why values are valuable as such or why consensually produced knowledge is valid also externally. As Tucker himself recognizes, if knowledge is defined in terms of consensus regarding beliefs, then reference to knowledge as an explanatory factor for consensus is both circular and vacuous. Tucker makes an attempt to avoid the issue by claiming that several notions of knowledge are compatible with his account. But this won't do. Provided that 'knowledge' is the pivotal (explanatory) notion in the scheme, the question of whether it implies truth (and in what sense) must also be tackled and specifically spelled out. Tucker indicates that knowledge entails truth according to the classical definition of truth (2004, 25). But this is problematic. It is not the case that uncoerced consensus by a large heterogeneous

community indicates the truth of what is believed. Ancient astronomers believed that the earth was located at the centre of the universe and a high proportion of Americans in 2003 believed that Iraq had weapons of mass destruction without any perceptible coercion. If these are knowledge, then 'knowledge' seems to imply false beliefs. If 'knowledge' is understood in some other sense, the issue will be different. The definitional matter cannot be taken so light-heartedly. Furthermore, the three features (uncoerced, uniquely heterogeneous, sufficiently large) do not seem sufficient since communities that fill these criteria may still be possessed by a collective hysteria, for example, or otherwise just end up believing false beliefs. In brief, I don't know how one could assess likelihood or probability without adding something about the *process* itself that justifies the views that the communities have adopted.

Indeed, Tucker eventually comes around and confesses to believing that the 'hypothetical descriptions' of past events, that is, explanations of evidence in historiography, are probably true, but 'not true in an absolute sense' (Tucker 2004, 258). However, this is not really helpful. Does Tucker mean merely to pronounce *his opinion* of their truth-value ('probably true') or rather to suggest that the hypotheses are 'truth-like' or 'approximately true'? The latter could be the opposite of 'absolute truth', although a better phrasing would be 'strictly true', since 'relative' pairs even better with absolute. And if this is not a correct reading, then it is not entirely clear how truth itself (not an assessment of truth-value) could be probable (cf. Rescher 1977, 191). When Tucker comments soon after this that 'most of history is and always will be unknown and unknowable', he introduces yet another, epistemic, interpretation. On the other hand, Tucker correctly observes that historiography may contain various parts, which he calls 'traditional' and 'scientific,' and whose epistemic standing differs (2004, e.g. 183–184; 254). I agree with Tucker that some parts, such as the claim that George Washington was the first president of the United States, are on very solid ground; they may be described as objective (in the 'ontological' sense) in my vocabulary, although it is a different matter how important a role they play in actual historiographical knowledge production, as discussed above. Tucker's reference to common causes would seem to work only in some cases. For example, the use of colligatory notions, such as the 'Thaw', cannot be traced back to any common cause in the external world other than someone's initial colligation and others' subsequent participation in the discourse.

Rorty comments on the threat of relativism emanating from communitarianism as follows:

> To say that unforced agreement is enough raises the spectre of relativism. For those who say that a pragmatic view of rationality is unwholesomely relativistic ask: 'Unforced agreement among whom? Us? The Nazis? Any arbitrary culture or group?' The answer, of course, is 'us'. This necessarily ethnocentric answer simply says that we must work by our own lights. (Rorty 2011, 38)

But if our only principle is to evaluate others' suggestions 'by our own lights', are we not locked into some kind of dogmatic imperialism of our own culture? Again, what guarantee is there that our values are any good? Why should members of any other group, the Nazis for example (arguably not 'our group'), listen to what we have concluded (on, say, the worth and epistemic standing of racist historiography)? The bottom line is that the fact that it is *our* conversation or judgment does not give it sufficient epistemic authority. We need something more. We need to answer the question of what it is 'about good instances of historical thinking [that] make[s] them good historical thinking', as Goldstein put it (1976, 215–216).

I find Rorty flimsy in his attitude towards objectivity. On the one hand, he is against objectivity as a 'desire to escape the limitations of one's community'. Instead he as a pragmatist desires as much 'intersubjective agreement as possible' and hopes to 'extend the reference of "us" as far as we can' (Rorty 2011, 22–23; similarly 38). But if we widen our community so that evaluative standards are applicable both in our and in what was previously a foreign community, do we not transcend our community by going beyond the borders of the community and by creating a new enlarged community? And if one keeps working in this manner, ideally, in the end, one would transcend all community borders and evaluative standards would be applicable in all communities. Or perhaps not; but the idea of community transcendence raises the prospect that some values and standards gain intersubjective and inter-communal validity. If our habits are exposed to the habits of another community, and vice versa, the most striking idiosyncrasies are arguably eliminated. If we repeat this kind of round several times, then more and more communally/culturally specific idiosyncrasies are removed. It is worth considering directly, philosophically, which practices might be community-transcending and gain inter-communal validity? And why?

Realists would have an answer to the problem of community transcendence: all true propositions, narratives and historiographical theses have universal epistemic authority, in all communities, because of the (absolute) truth (as correspondence). Rorty seems to think that truth as

correspondence is the only way to provide rationality. Truth as correspondence does not in any case work, provided the status of synthesizing expression in historiography, for the reasons discussed at length in this book. To repeat, the higher-order constructs in historiography typically lack truth-makers in the object world that would make them true and false. The central suggestion of the book is that historiography is about making rational, argumentative, speech acts. This is based on the idea that well-performed argumentative speech acts are rationally persuasive and that historiography is ultimately a rational practice. This takes us to the final theme of the book, which is of fundamental importance: it is rationality itself that provides the prospect for community-transcendence and the inter-communal validity of historiographical arguments. Alternatively expressed, a good historiographical argument has epistemic authority due to the rational properties it contains. In the previous chapter, historiography-specific rationality was already considered. It is worth saying here, at the very end of the final substantial chapter of this book, a few words on the notion of rationality in general and its capacity for community transcendence in historiography.

Situated universal rationality

It may be said that there are two main forms of rationality: instrumental and categorical. The former says that rationality is relative to ends; an act is rational if it is efficacious in the achievement of a specific end. The latter implies that there is rationality in a categorical sense, which instrumental rationality must also presuppose. There is much debate on the virtues and vices of these options (e.g. Giere 1989; Laudan 1990; Siegel 1989, 1990, 1996). This is not the occasion to go into the details of that debate. It may well be the case that most of the rationality talk is about the instrumental optimization of the best means for achieving desired ends. However, it seems to me that there is also some more fundamental form of rationality, which cannot be easily reduced to the means–ends form.

Rescher has developed an interesting account of rationality in which the principle of rationality is *universal*, but its application nevertheless always *circumstantial* or *situational*: 'Although the rational resolution of an issue depends on the contextual circumstances, nevertheless, rationality is universal in the sense that anyone in just the same circumstances would be rationally well advised to adopt the same resolution' (1988, 1). Or as Rescher also expressed it: 'what is rational for one person is also rational for anyone else *in his shoes'* (1988, 158; similarly 1997,

7). The universality of rationality can be seen as the general force or impersonal compeller for accepting a certain account on the basis of its rational features. This is the sense in which I also differ from Rescher, who reduces rationality ultimately to instrumental rationality. My view is that a *rationally persuasive* argument has force due to certain inherent rational features that are best studied by and exemplified in logic and argumentation theory. There are features like the fundamental cognitive values of consistency, coherence, simplicity, etc. and forms of reasoning, of which deduction is but the most famous one. It is just normally compelling to accept an argument (or come up with a worthy counter-argument) that appears devoid of contradictions and progresses step by step to a reasoned conclusion. One must find a fault in the reasoning, such as implied contradictions or an unwarranted jump in the link of reasoning, in order to cause its rational appeal to wither.

This kind of rational force based on the most fundamental principles of rationality is something very fundamental psychologically and cannot be easily reduced to an instrumental account. Of course, the goal of the historian is ultimately to persuade peers and readers to accept his or her account. One might therefore think that the historian could use any means whatsoever to achieve this end, including irrational and non-rational techniques. Yet this is a very odd sense of 'persuasion'. First, the goal of persuading one's peers and readers is too abstract and vague to provide tools and instructions regarding how to do so. What exactly should one do, if the only aim is to persuade others without any further information about the audience or the substance? My point is that the means–end approach is not sufficient to capture the nature of historiographical rationality. More important, if the rational features are universal, then the audience shares them with the historian on some fundamental level. And even if we ignore the question of universality, I believe that the readers of scholarly historiography expect and appreciate rational argumentation in any case, and are not content with mere propaganda, tricks or threats.

The rationality of a belief, action or evaluation requires that the agent must be in a position to 'give an account' in order to show others why it is appropriate to resolve the matter in a particular way. This is another way to express the idea, introduced in Chapter 9, that a historian's argumentative speech act is a move in the 'space of reasons', which also demands readiness to defend one's move. The circumstantiality or situationality of rationality means that one may appreciate the rationality of a judgment or action by someone else whose conditions, and therefore judgment, are different: 'While I myself do not believe or value these things, I can

see that it is appropriate that some in the agent's circumstances should do so, and in consequence it was altogether sensible for the agent to have proceeded as he did' (Rescher 1988, 158). This notion of rationality is thus compatible with and sensitive to the variation of times, places and the 'thousands of details of each individual and situation' (Rescher 1988, 159). It may have been rational for Galen in his day to believe in a two-leveled circulatory system for blood, given his training and background beliefs, but it is not so for us anymore because circumstances have so fundamentally changed. More provocatively, 'The Siamese king who refused to believe that rivers solidify in northern countries at a certain season of the year was perfectly rational, the freezing of water into ice lying wholly outside of his experience' (Rescher 1988, 7).

Rationality is thus context-sensitive historically. Entirely rational people have justifiably arrived at radically different accounts because they have acted under different circumstances. The universality of reason does not therefore lead to insensitivity regarding the motives and reasons of past agents and to a presentist imperialism of rationality. And although specific historiographical arguments *cannot* be assumed to be universally, or perhaps globally, rationally forceful, this is only to be expected because the background beliefs and the state of the historiographical discourse are so different in different times and places. The fundamental *principles* of rationality, and their persuasiveness, may be seen as universal and shared over different times and locations, however. In brief, while it may be impossible to find an ideally rationally persuasive historical argument, the operative principles in the background are universal and shared at least among scholarly historians who, almost without exception, wish to produce consistent, coherent and well-exemplifying accounts. Their rates of success naturally vary significantly.

I should clarify that I do not mean that there is a God-given rationality or one with some other kind of supernatural origin with a capital 'R'.[8] My point is that the concept of rationality cannot be exhaustively stipulated but it has boundaries, that is, that not just any kind of practice can be understood as and called 'rational'. In this specific sense, fundamentally, the same concept of rationality applies generally although rationality in its purest form may possibly never be applied in historiography. Indeed, it is very important to be open to historiography-specific applications of rationality. It may be that this conception of rationality ultimately reflects Western ethnocentrism that Rorty welcomed with open arms. I am concerned with historiography and its normative requirements as an academic discipline and historiography is deeply rooted in Western tradition, after all. And it is not clear that 'non-Western historiographies'

differ fundamentally from the Western one with regard to rationality. Finally, this conception may not be satisfactory in the end, and it is certainly not meant to be a 'final' one. Nevertheless, I think it is best we have for now and therefore it is worth committing to it.

It admittedly is possible to imagine someone or some community that does not commit to the standards of rationality as we know them, either because of a refusal to do so or because 'rationality' is entirely absent. For example, there has been discussion concerning the Azande and Wazonga tribes (e.g. Bloor 1991, 139–141; Rescher 1988, 161–162) who do not have a concept of rationality or have a profoundly different conception of rationality than Westerners. I have no doubt that there are people and communities who desire to not play by the rules of rationality characteristic to Western academia, that is, critical discussion and a consequent an incorporation of received critique. More specifically, there may be entirely irrational or non-rational means for presenting history, perhaps in the form of a performance, for example. An adequate response here is that everyone is free to choose their community, but everyone is not free to choose their favored concept of rationality. First, I do not think that the notion of 'natural rationality' (Barnes 1976) necessarily makes sense. It would allow one to view radically deviant practices as displaying alternative conceptions of rationality with the consequence that any (seemingly irrational) behavior could qualify as 'rational' as long as it is somehow rule-bound and found to exist in some community. My view is thus that rationality is something more categorical, and the concept of rationality consequently has its limits, beyond which one cannot speak of rationality any longer. Second, scholarly historiography is ultimately guided, although not determined, by the rules of rationality, respect for evidence and the spirit of argumentation, but no-one is forced to belong to this kind of community and take part in the discourse of scholarly historiography. In that case, one chooses to stay out or step out of the academic community and elects to be part of another form of life. In the end, we are talking about alternative forms of life.

There may be nothing wrong with irrational or non-rational practices, but the principle of rationality forms the 'transcendental limit' of historiographical communities. This does not mean that rationality is necessarily some human universal, as humans can clearly be irrational, only that the concept of rationality is universal in the sense of setting the boundaries beyond which one cannot talk about rationality any more. Further, giving up the most fundamental principles of rationality means rejecting scholarly historiography and excommunicating oneself from its respective communities. Rationality is thus a kind of 'normative

transcendental limit' of scholarly historiography. It is a choice and commitment that one must make in order to be a historian in the scholarly sense. Popper formulated his idea of scientific communities aptly and I think that it applies to historical communities too: 'The game of science is, in principle, without end. He who decides one day that scientific statements do not call for any further test, and that they can be regarded as finally verified, retires from the game' (Popper 1997, 53).

Now I am coming to the very end of this final substantial chapter of the book. The last task is to outline the link between rationality and objectivity in a justificatory sense, as promised. I used Rescher's analysis in *Objectivity: The Obligations of Impersonal* Reason (1997) to make this connection. Objectivity in a justificatory sense has to do with the cogency of our claims: 'the question of whether a claim is impersonally and generically cogent rather than personal and idiosyncratic – whether it holds not just for me (egocentric objectivity) or for some of us (parochial subjectivity) but for all of us (impersonal or interpersonal objectivity)' (1997, 4). This kind of epistemic objectivity is tantamount to rational appropriateness, as discussed above. Naturally, it is hard, perhaps even impossible, to reach a universally impersonally compelling account of anything, due to variable conditions, situationality and the influence of non-rational factors. Still epistemic objectivity represents the ideal that scholarly communities must try to reach even when the presentations of history retain their subjectivist flavor in the ontological sense: that as many people as possible and in as many locations as possible see the sense and persuasiveness of what is claimed.

In historiography, there may be several rationally constructed interpretations of the same subject matter. The point is that others should be able to appreciate the inner rationality of the views, provided that they know and understand the context. In this sense, the call for 'impersonal reason' does not lead to 'dehumanization' but instead requires that we situate and see ourselves in the others' circumstances. Criticism of the lack of rationality from our perspective should also proceed from these premises. It is easy to see that objectivity in the epistemic sense is a matter of degree. The more rationally forceful and acceptable an account is, the more objective the account in this sense is. Helen Longino has suggested that we commit to objectivity as inter-subjective justification as the aim of science and has stipulated conditions that enhance the possibilities for reaching as highly justified account as possible (venue, uptake, public standards and tempered equality) (Longino 2002, 129–131). Further, Longino's point is that, if the communal process of knowledge production is successful, it produces something that transcends the

contributions of any individual or sub-community (Longino 1990, 69). However, as noted above, due to the nature of historiography, that is, because it produces higher-order interpretations and colligations, subject-sided elements cannot (and should not) be removed. But it is a different matter to remove bias and the lack of rationality due to subjects' defects, omission and other errors. An account that owes to the subject-side can ideally be generally persuasive and compelling due to its rational features. In this sense, the presentations of historiography should aim at producing something that transcends, in its rational persuasiveness, the contributions of any individual or any community.

Conclusion

This chapter has focused on several significant concepts in philosophy: objectivity and subjectivity, the real, constructivism and rationality. I concluded that historiography can be located between subjectivity and objectivity, between the subject-side and the object-side, in both the ontological and epistemic sense. Another central suggestion is that although historiography clearly is constructivist in the sense that the historian creates cognitive products with subject-sided elements, the outcomes in historiography can be considered real if they are justified, that is, rationally warranted. In the end, it became clear why the governing concept of the *postnarrativist philosophy of historiography*, rationality, must be community-transcending. The reason is that the historian should aim at producing an argument that becomes as widely rationally persuasive as possible.

11
Coda: Postnarrativist Philosophy of Historiography

The perception that it is necessary to choose between a nihilistic 'anything goes' postmodernism and an absolutist objectivism has bewitched much of the contemporary philosophical discussion on historiography and beyond. This book has tried to show the way in which historiography, and specifically its main cognitive products, can be evaluated and ranked rationally, but without a commitment to the correspondence theory of truth. *Postnarrativist philosophy of historiography* endorses the initial insight of narrativism that the texts and entire books of history are the main knowledge contributions of historiography and must be the subjects of philosophical analysis, but it understands them as exemplifying historiographical *reasoning* for *theses of history*.

I have introduced a number of new concepts or novel applications of old concepts in this book. This coda provides a brief code for *postnarrativist* discourse on historiography. The transition from narrativism means regarding historiography as a type of *rational* practice and not as a kind of narrative storytelling. Historiography attempts to produce s*ynthesizing* or *colligatory* views on the past. These are the most original and expressive contributions of the discipline. Historians attempt to *persuade* others to accept the views put forward in their books. Persuasion may be manifested in many forms, from explicit reasoning from premises to conclusions to narrative storytelling, to rebuttals of rival positions, to exemplifications, etc. Ultimately, all such forms are subservient to the thesis or theses defended and historians try to credit their theses with as high a level of *epistemic authority* as possible.

The identification of the historical text as an informal argumentative entity means rejecting holism regarding historiographical theses. The content of a text of history can be seen as divided into the

meaning-constituting elements of a historiographical thesis and the *evidence* for it, although the distinction is not clear-cut. In other words, the meaning-constituting elements may enable one to understand the evidence better, while more extensive evidence makes the meaning of what is claimed clearer. Nevertheless, this 'molecularist' position entails that there are elements in any historiographical text that are inessential for the understanding of the main historiographical thesis and that serve the *evidentiary function*. On the other hand, there is always room for an evidential deepening and a multitude of choices as to how to make the case for any thesis.

Historiography identified as a discursive and argumentative practice also explains the abandonment of representationalism in favor of *non-representationalism*. It is not reasonable to look for abstract entities that are re-presented once the correspondence relation between the historical reality and historiographical thesis is rejected. Historiography is *presentational* and *constructivist*, not re-presentational and re-constructivist. The availability of *rational standards of evaluation* explains why any fears that 'anything goes' are baseless. A historian's construct can be seen as epistemically authoritative if it is seen to be fit with respect to all *dimensions of cognitive justification: the rhetorical, the epistemic and the discursive*. That is, the text is a persuasive manifestation of reasoning for a thesis; it is an exemplary employment of *epistemic values*, including references to actual historical objects with regard to non-colligatory expressions; and it is a successful *argumentative intervention* in the relevant *argumentative context*. In this kind of case, a historiographical text has a *rational warrant* that gives it the epistemic authority for what is stated. Further, any text is an *argumentative speech act* and, in the ideal case, readers feel *rationally compelled* to accept the reasoning of the historian and the historian's conclusion.

Although higher-order historiographical knowledge, synthesizing and colligatory theses are constructions, they can be seen as *real* if they have an appropriate justification and rational warrant. Skepticism and doubt regarding their reality emerges not on the level of historical research, but on the meta-level of the philosophy of historiography. Rationally warranted historiographical theses concerning historical phenomena are real with respect to the historical world, although their status as *object-sided* entities may be questioned in philosophical analysis. 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.

The *subject-sidedness* of historiography means that its constructions are original and expressive. *Objectivity* in the *ontological sense* is possible but not necessarily desirable in historiography. On the other hand, *objectivity as intersubjective* and *inter-communal justification* is desirable. The historian wishes that as many people as possible find his or her historiographical thesis to be maximally justified, that is, rationally compelling to accept. On the most fundamental level, this is due to shared human rational features. It is naturally possible to practice historiography irrationally or non-rationally, or perhaps to find a community that does not commit to Western forms of rationality. And yet rationality forms a normative kind of *transcendental limit* for *scholarly historiography*. Everyone is free to choose their community and discourse, but scholarly historiography, as any academic practice, requires a commitment to rationality at least on a foundational level.

This is the conceptual code suggested in this book in a nutshell. The postnarrativist approach displayed naturally still leaves many questions unanswered and provides ingredients for further investigations in the philosophy of historiography. A *postnarrativist philosophy of historiography* provides only a framework, after all. For example, if narrativists studied and analyzed historiography as a kind of literary form, my suggestion is that we need research on historiography as an argumentative practice. What kinds of structures of reasoning or argumentation are displayed in books of history? Is there a field-specific historiographical reasoning? Or, to what degree can one talk about the field-specificity of historiographical rationality? I suggested that most of historiography produces synthesizing and colligatory theses that owe much to subject-sidedness but that relatively object-sided historiography is also possible. It would be interesting to learn more about what kinds of theses historians actually put forward and whether the interpretative subject-sided element is as strong as it seems. How about originality then? Are there any measures for specifying how original a historian's contribution is?

These are only some potential openings for further research. The postnarrativism of this book suggests that we take seriously the narrativist idea of books containing central theses, but it proposes going beyond narrativism with regard to how historiography is characterized and with regard to its evaluative standards. Historiography is a form of societal and academic critical discourse about us and about our past. Potential

views and arguments in this discourse are infinite, but fortunately there are ways to differentiate between poorer and better argumentative accounts. I hope that this book in its specific philosophical genre is of the latter kind and that it has given motivation for some readers to develop and improve the discourse on historiography in the framework of *postnarrativist philosophy of historiography.*

Notes

1   Introduction: The Narrativist Insight

1. An attentive reader might note that the opposite of objectivism is subjectivism and the opposite of relativism is absolutism. This is correct, and I will specify the meanings of the terms, and specifically that of 'objectivism', later in the book. However, often in philosophical discourse objectivism is paired with relativism, and for this reason, this conceptual pair provides a good entry point to situating the book within the field.
2. To get a taste of the character assassination by analytic philosophers, consider the following excerpt from Gardiner's (1982) introduction to *The Philosophy of History.* Speculative philosophers 'often seemed prepared to override or disregard facts that conflicted with the tenets of cherished doctrine; they were also accused of conceptual imprecision and of formulating hypotheses which turned out on inspection to be either hopelessly vague or else to be no more than the tautological consequences of definitions arbitrarily determined in advance. More generally, it was objected that they tended to rely upon unexamined *a priori* assumptions, regarding both the method they employed and the nature of the material with which they were dealing. Even where (as was frequently the case) they claimed to conform to canons of reasoning accepted in the natural sciences, it was far from obvious that they possessed an accurate conception' (Gardiner 1982, 2).
3. As becomes clear later, I regard 'cognitive' as a more comprehensive term, which subsumes under it also other than epistemic criteria to be used in the evaluations of historical knowledge.

2   From Analytic Philosophy of History to Narrativism

1. Patrick Gardiner's (1974) *The Philosophy of History* is an excellent collection of articles, including those by Collingwood, Mandelbaum, Hempel and Dray, which shows how the debate progressed.
2. Texts are practically the form in which historiography is presented. That is, the form that reaches readers and conveys the historian's message and conclusions about the past. The nature of these entities, which express cognitive claims, will be further discussed in Chapter 5. Note also that I decided to stick to the common expression 'historical knowledge', which can be understood as 'knowledge of history' or 'knowledge about history' although this knowledge is naturally produced and formulated by historians and therefore historiographical by nature.
3. Jonathan Gorman (1997) has criticized the belief that historical research is necessarily expressed in individual (atomistic) statements. Gorman thinks that it is a mistake to assume that scouring the Public Office yields particular 'facts,' which take the form of individual statements and that it would be a

further error to presuppose that historical writing is just a matter of sorting these individual facts with an aim of placing them in a narrative order. Instead, says Gorman, historical records are approached with some interpretative theory and, more importantly, both historical research and historical writing are part of a 'theoretically single activity of thought' (409). The upshot of his argument is that the semantic, epistemological, and metaphysical issues with regard to the relationship between individual statements and whole texts has to be studied independently (of any questions to do with the research and writing of history).

4. But see Vann's (1992) *Turning Linguistic* on how the 'linguistic turn' manifested in the pages of *History and Theory* in the period from 1960 to 1975.

5. The following convention has been adopted in this book: if emphasis is mine, it will be mentioned. If emphasis is in the original, there will no mention.

6. It is true that also Danto assimilated narration to a story-like structure, but he held story as an explanation of how 'the change from beginning to end took place' (Danto 1968, 234). The historian creates 'temporal wholes' that connect two or more temporally separate objects together (Danto 1968, 248, 255), which is a notion that comes close to Walsh's colligatory concept, discussed in Chapter 6. And it is notable that Danto also sees that despite the story form narration contains elements of deductive arguments. He is the only one of the early narrativists who took steps towards a closer analysis of narrative structures, also from the non-story-like perspective. Perhaps his most original ideas are that the changes described in a work of history are nested and may create complex layered structures (Danto 1968, 241). He also introduced the concept of molecular narratives, which differ from atomic narratives in that there is *a sequence* of causes instead of one (Danto 1968, 251–252).

7. He published an essay 'The Logic of Historical Narration' already in 1963 in *Philosophy and History*, edited by Sidney Hook. That volume (Hook 1963) also contains replies by Lee Benson and Maurice Mandelbaum as invited responses and by Glenn Morrow and George H. Nadel as 'other contributions' on M. White's text, which led M. White to revise the paper, which then appeared as Chapter VI 'Historical Narration' in his *Foundations of Historical Knowledge* (1965). The references are to this latter version.

8. Since there are two scholars with the same surname, Morton White and Hayden White, to avoid confusion I will refer to them either by their full names or by the initials of their given names, that is, M. White and H. White, as appropriate in each context. However, since the latter is a more central subject in this book, the initial will be dropped off in subsequent chapters when there is no risk of confusion.

9. See also Kalela's most recent book *Making History: The Historian and the Uses of the Past* (2011).

10. Paul thus finds a unifying rationale in White's career. His book *Hayden White* argues that White was inspired by an existentialist understanding of human 'flourishing.' Freedom and responsibility became the core elements of his thinking on history (Paul 2011, 11, 26). Paul writes that this applies also to Hayden White's writings of the 1970s and 1980s, in which one may detect anti-realist philosophizing (Paul 2011, 56). However, Paul does not explicitly specify, whether the existentialist interpretation fundamentally explains why he wrote the kinds of texts he did in those decades or whether it also,

more consequentially, changes the seeming (anti-realist) meaning of those texts. Indications are that the meaning does not change.

11. Ankersmit similarly characterizes the purpose of *Sublime Historical Experience* (2005) as an attempt to 'do away with all the (quasi-)transcendentalist conceptions' (10).

12. See Icke (2010) for an attempt to form a synthesis of Ankersmit's whole career. The hero of Icke's book is Hayden White against whom much of 'goodness' of Ankersmit is measured: 'White's new language informed style of historical theory was largely completed and comprehensively "wrapped up" to the extent that there was little space for any improvements on it' (Icke 2010, 134). Especially the later career of Ankersmit receives very critical treatment and is seen as a 'lost cause.' Icke even tries to provide a psychological explanation for why the later Ankersmit turned on the questions of experience and direct access to the past, claiming that Ankersmit 'has always harboured a deep seated *need* to retrieve the past in some real/authentic form' (Icke 2010, 135; my emphasis) and that White is 'the spectral embodiment of everything that Ankersmit *found* himself *compelled* (psychologically) to leave behind' (Icke 2010, 134; my emphasis). Unfortunately, Icke's explanation appears more like an insinuation or at most a suggestion rather than a proper explanation (cf. Zeleňák, 2014). Further, due to its divisive language the book is less persuasive than it could have been otherwise. However, the actual substantial problem is that its analysis embeds a number of strong metaphysical commitments (such as, language cannot put one in touch with reality; language cannot lead to a knowledge of the world in itself; knowledge of the world in itself is unattainable; language is 'essentially' metaphorical 'all the way down'; there is no truth 'out there,' e.g. Icke 2010, 52, 56–57) and the success of Ankersmit is judged on whether his thinking in various stages of his career accords with these tenets. Icke sees that the early Ankersmit agrees more with these kinds of commitments than the later. This is undoubtedly a brave strategy, but does not pay off in the end, as it takes Icke to so many metaphysical and semantical disputes, making his analysis unconvincing despite some good insights. Occasional references to Rorty, Derrida, Ricoeur, Barthes and Lyotard are not going to establish the tenability of these theses.

3 Three Tenets of Narrativist Philosophy of Historiography

1. For those interested in the difference between Rankeanism, that is, a form of historical realism to which Ranke's writings gave a birth, and Ranke as a historical thinker and theorist, see Iggers (1962) and (1973). Rankeanism overlaps with Ranke, but is not the same, because Ranke was closer to the German idealistic tradition than the stereotypic image of him suggests.

2. It should be remarked in this context that my analysis differs significantly from that of Icke, who thinks that in his most original contribution of 'narrative substance' Ankersmit was 'simply restating the work of others, and that this and a few other notions of the early Ankersmit can be found in White'. While Ankersmit surely has predecessors, and absolute originality may be impossible, his philosophy is much more detailed than that of White. Occasionally, Icke's philosophical interpretation of White appears simply too charitable (e.g. Icke 2010, 125–126).

3. It should be noted that Ankersmit uses 'representation' in a different sense than is commonly applied in analytic philosophy. For Ankersmit, representation cannot be divided into smaller components, which would refer separately to entities and properties. In analytic philosophy, 'representation' means reflection of the state of affairs in the external world and allows determination of referential relations of composite expressions. For example, in analytic philosophy 'truth-bearers' are prime examples of such inherently representational entities (MacBride, 2014).

4. Ankersmit uses 'presented' and 'aspect' interchangeably.

5. 'We should avoid identifying the person depicted by a photo or a painting with that representation's represented, which is merely an *aspect* of the person in question … All these books then present us with aspects of Napoleon – even though the sloppiness of our use of language will lead us to say that they are on Napoleon himself' (Ankersmit 2012, 70–71).

6. As some artists have pointed out, even the idea of a camera presenting the 'given' without elements of subjectivity itself is naïve. Nevertheless, this kind of photographic realism as a way to 'objective' and faithful representations of the world has been very influential in the history of the sciences. For more, see Lorraine Daston and Peter Galison's *Objectivity* (2010) and specifically Chapter 3 'Mechanical Objectivity'.

7. It is worth noting that the idea to let the archives, and historical 'facts' that are found in them, speak for themselves has been preserved and received a new application in the sociologically inspired historiography of science. Many see historiography as a kind of temporally conditioned sociology, in which one should pretend ignorance with respect to the research object and imagine oneself peeking over the shoulders of past scientists, as Martin Rudwick has expressed it (Rudwick 1985, 8). The resemblance to Rankean thinking is striking. See Latour and Woolgar's *Laboratory Life* (1979) for the (very Rankean) idea of emptying one's mind of any preconceptions and pretending ignorance with respect to the research object, that is, science. See my 'The Missing Narrativist Turn in Historiography of Science' (Kuukkanen 2012) for an elaboration of how this kind of neo-Rankeanism has taken hold in the historiography of science. I will discuss 'sociological descriptivism' and its relation to the 'narrativist insight' also in Chapter 5.

8. I find the notion of the 'translation rule' useful. It can be understood as expressing the relationship either between the past or historical evidence (which are of course quite different things) and the interpretation that the historian draws on the past. A 'translation rule' thus states a logical relationship between these two levels and raises the question, what kind of interpreting is possible on the basis of lower-level data. The narrower understanding, not adopted here, would require that the 'translation' of the past or historical data into an interpretation must proceed in temporal direction (cf. Icke 2010, 66). This is of course theoretically possible, but my interest is in the determination of a historical view, that is, whether a view can be justified in the first place or at all. It may be added that the 'rules,' if they exist, are not in the data themselves (cf. Icke 2010, 70), but they function like methods in science, that is, guide scientists to correct inferences and justified conclusions.

9. Observe how White makes and accepts a similar distinction between an individual factual level and a general interpretative level (White 1987, 45; 1978, 82–84; 1975, 59).

10. Although one does not find mention of 'nominalism' in *Narrative Logic* Ankersmit refers to nominalism in many of his other texts. In 'Six Theses on Narrativist Philosophy of History,' that is, a numbered summary of narrativism à la Wittgenstein, Ankersmit says that 'Narrativist logic is strictly nominalist' (1994, 39). In the essay 'Historiography and Postmodernism', Ankersmit writes about the new 'nominalistic view of postmodernism' as opposed to the old 'essentialist' view (1989, 149; similarly 147). Further, in his 'Reply to Professor Zagorin' we find a slightly puzzling statement that 'Postmodernism is the nominalist version of historism' (Ankersmit 1990, 277).

11. In comparison, the classical nominalist commits to a view that the world is the world of individuals or particulars only, and therefore, the classical nominalist denies the existence of universals (and in some versions, of abstract objects) that the universalist thinks is needed to explain our talk of *kinds* of objects and properties. The universalist claims that red things are red in virtue of their being instantiations of the universal 'redness' and gold things gold due to their being instantiations of the natural kind 'gold.' According to the nominalist, classification and the use of (natural) kind terms do not require invoking any other entities beyond the individuals that fall in the classes and kind categories. It is not clear that narrativism is nominalist in any of these senses, as it does not seem to reject universals or abstract objects. For further reading on nominalism, see Rodriguez-Pereyra (2001).

12. David Carr (1986) and Ricoeur (1990) question that narrativity is only a feature of history writing. In their view, human experiencing takes the narrative form. I will delay my discussion of this until Chapter 5.

13. On White's Kantianism, see Hans Kellner (1992).

14. Ankersmit argued that Peter Strawson's idea of descriptive metaphysics offers an even better although similar analogy to what is attempted in *Narrative Logic*, that is, the primary concern in *Narrative Logic* is how the linguistic instruments that we use to express historical knowledge determine the nature of this knowledge (Ankersmit 1983, 11, 55, 88).

15. The narrative substance (Ns) the Cold War.

16. Ankersmit also analyses the nature of narrative substances by appealing to Leibniz's 'predicate in notion principle', according to which all the properties are included in the subject-term and can therefore be derived from it analytically (Ankersmit 1983, 130–131).

17. I do not take any stand on the tenability of analyticity, but assume that there are analytically true sentences, as Ankersmit implies, for the sake of argument. W. V. O. Quine's influential paper 'Two Dogmas of Empiricism' (1951) argues that a principled definition of analyticity cannot be found and that this notion should therefore be rejected.

18. Ankersmit comments on the potentially corrosive claim that in this analysis historiography becomes unempirical and epistemically immune: 'It does not follow in the least that historical representations have now severed all their ties with historical reality. They are tied to it by countless individual ties, each consisting of some individual true statement contained in the representation' (2012, 95). In other words, the ties have been severed on the level of representations but kept on the level of singular statements.

4 Representationalism and Non-representationalism

1. For more on the difference between the historical Ranke and Rankeanism, see Iggers (1962, 1973).
2. More on scientific historiography, see Lorenz (2011).
3. For the classic critique of resemblance or copy theories, see Goodman (1976, 3–10). Suárez (2003) defines the similarity conception of representation [sim] as 'A represents B if and only if A is similar to B' and the isomorphism conception of representation [iso] as 'A represents B if and only if the structure exemplified by A is isomorphic to the structure exemplified by B' (227). He goes on to present a five-fold argument against these conceptions. The key points are: they do not (1) apply to all representational devices, (2) do not possess the logical properties of representation, (3) do not make room for the ubiquitous phenomena of mistargeting and/ or inaccuracy, (4) are not necessary for representation – the relation of representation may obtain even if [sim] and [iso] fail and (5) are not sufficient for representation – the relation of representation may fail to obtain even if [sim] and [iso] hold.
4. Giere (2004) suggests that we shift focus to scientific practices and that we begin our analysis with the *activity* of representing. His proposal is meant to provide room for purposes in the account of representational practices. Giere's general representational scheme takes the following form: *S* uses *X* to represent *W* for purposes *P*. Yet, the traditional representational function seems to retain its role, as Giere is still interested in the 'fit' between the model and aspects of the world. Grüne-Yanoff (2013) writes that the implicit assumption underlying most suggestions for scientific models is that they represent real targets. The representational criteria proposed include isomorphism, similarity and partial resemblance. He remarks that there is 'convincing evidence' that many scientific modeling practices do not satisfy the central representational requirement. This has led many to say that modeling practices play merely a heuristic role. Grüne-Yanoff writes amusingly that 'it places the use of such models in the same category as taking a walk, reading the newspaper, or whatever else scientists do in order to inspire themselves to further theory development' (851).
5. Suárez (2004) develops an inferential conception of scientific representations, which denies isomorphism and similarity as necessary conditions for representation. The main idea is that representations allow 'surrogate reasoning'. More specifically, there are two conditions for representations: A represents B only if (1) the 'representational force' of A points towards B and (2) A allows making specific inferences regarding B (773). He quite rightly calls this account a minimalist conception of representation, as in practice it widens the applicability of the representational function and the concept of representations significantly. The target of 'representational force' may be almost anything as long as one can draw informational inferences from the object or objects. In Suárez's view, cubist and surrealist paintings are representational but one wonders what their representational objects are.
6. The exception would be the homomorphism conception of representation. Yet it falls prey to most arguments shielded against isomorphism and similarity accounts (see Suárez 2003, 239–240).

7. We can go even further in the history of the philosophy of language and metaphysics. In fact, 'presenteds' as 'real objects' resemble Meinong's theory of objects. Russell famously objected to the latter's idea that any meaningful expression requires that there is some object that it means and that the grammatical form of language necessarily corresponds to its logical form. Meinong's theory was prone to problems because it implies, for example, that although the 'existent present King of France' does not exist, he nevertheless does, because all 'non-beings', including round squares exist somehow or somewhere. In the case of historiography, this would mean that a historical narrative about, say, Napoleon as an arrogant ruler implies that there is a corresponding entity in the world of historiographical objects. Russell's point was that not all expressions that seem to be (grammatically) referring (or perhaps 'being about' in our case) should be assumed to be such (for more on Meinong, see Marek 2013). Although my aim is not to defend Russell's theory of descriptions nor its applicability in the context of historiography, this is the lesson that can safely be taken on board from this debate. Needless to add, Frege never analysed the nature of texts.

8. See John Searle's *The Construction of Social Reality* (1997) and 'Social Ontology. Some Basic Principles' (2006). Searle thinks that we can have epistemically objective knowledge about ontologically subjective entities, such as money. The existence of such objects is not independent of human practice but knowledge, including truth-values, of them is not dependent on any one's cognizing.

9. Note that Ankersmit (2012) occasionally also uses the term 'represented' to mean the same things as 'presented' and 'aspect'. In this kind of context 'represented' denotes not historical reality independent of a historian ('represented reality', 81), but the object that the historian constitutes through writing and that the resulting representation is about.

10. See Ankersmit's 'Danto on Representation' (1998).

11. Some commentators (e.g. Icke 2010, 45, 26), including Ankersmit himself in *Narrative Logic* (e.g. 1983, 8, 16, 28) have attempted to explicate the representationalist nature of narratives and narrative substances by reverting to the 'point-of-view' language. But, if it is denied that there is no true narrative or the fact of the matter about them that would make narratives truth-functionally true or false, it should be obvious why this kind of language is problematic. As such, the point of view implies that there is one shared substance, and the way it is, on which one takes a point of view. This entails that there is, in principle, a correct view of this substance, perhaps all potential views combined, or that getting it right is at most an epistemological problem, and, therefore, there are no conceptual schemes in any interesting metaphysical sense. See Davidson's 'On the Very Idea of a Conceptual Scheme' (1974) for a critique of 'point-of-view' and the concept of conceptual scheme.

12. It is worth explaining in more detail why the term 'realism in idealism' is not an oxymoron as one might think. The opposite of realism is not idealism, but anti-realism. And the opposite of idealism is materialism. Provided this, the ontological interpretation implies that the 'imaginary' world is non-material, 'ideal', in this sense, but that it is real all the same, that is, that it exists. An idealist can thus be a realist but not a materialist, although the world of

the idealist is dependent on human thinking in some way. If the world we inhabit were the world of God's mind, then all would be mind-dependent but nevertheless real. In passing it may be noted that similarly an idealist can be a correspondence theorist (of truth). The facts or truth-makers would just be mind-dependent in this case (see Kirkham 2001, 73–76, 133–134).

13. As suggested by Ankersmit in private correspondence; see also Ankersmit (2012, 69–67).

14. In private communication.

15. For example Icke, in his critical analysis of Ankersmit states in agreement with Ankersmit's 'no representation, no past' (Ankersmit 2006, 328) that, 'arguably, history to be a history has to be a representation' (Icke 2010, 205). On the other hand, Icke makes an interesting remark that it is possible to find a 'non-representational/textual' Ankersmit or an advocate of non-representational/textual history in the later Ankersmit (Icke 2010, 204). This is interesting, considering Ankersmit's intellectual orientation in his career and especially in the most recent books (2001, 2012), the titles of which include the term 'representation' and which deal extensively with the problem of representation. It should be made clear that my non-representationalist option differs significantly from that of Ankersmit and from his reliance on direct historical experience.

16. It is worth adding that my point is not to suggest that the idea of there being such a logic is a problem in and of itself; indeed, I have myself used this kind of argumentation. In 'Making Sense of Conceptual Change' (2008), I suggested that the logic of writing conceptual history requires that all instantiations (sub-concepts) in a history of a concept must share a certain minimal conceptual core. Nevertheless, I think that there must additionally be a link from 'logic' to 'pragmatics': the notions that are derived from the 'logic' must help one to analyse and understand historiography better. If this is not the case, it is questionable whether one has managed to outline the correct 'logic.'

5 Reasoning in Historiography

1. In what follows, I use these two terms interchangeably for these kinds of higher-order entities in historiography. Ankersmit (2005) writes that, because talk of 'narratives' may incorrectly be seen to imply that the historical text is essentially a story, a 'variant of the novel', and thus hides from view that the historical text should do justice to the past, it is better to use the term 'historical representation' (xiiv–xiv). It follows that both 'narrative' and 'representation' are acceptable terms, provided that incorrect connotations are avoided.

2. Interestingly, also Ankersmit occasionally hints that a 'narratio' could be argumentative. He writes that the discourse of the writer of a narratio is 'expositive and argumentative' and 'particular … historical situations.' 'In the narratio the first level leads up to the second: it furnishes the evidence and the illustrations for a comprehensive interpretation of (an aspect of) a historical period' (Ankersmit 1983, 27). Further, 'the historian argues for "points of views" on the past' (1983, 28).

3. Martin Rudwick's *The Great Devonian Controversy* (1985) attempted this kind of 'grass root' level anthropological or sociological description, whether or not he was directly influenced by Latour.

4. See my *The Missing Narrativist Turn in the Historiography of Science* (Kuukkanen 2010) for a critique of this kind of historiography.

5. Also White attributes a transcendental cultural function to narratives. Although he thinks that 'real events do not offer themselves as storied' (White 1980, 8; cf. White 1978, 1–25), which makes their narrativization so difficult, 'narrativity is a metacode, a human universal on the basis of which transcultural message about the nature of a shared reality can be transmitted' (White 1980, 6). He also states that we can never *experience* the 'completeness and fullness' of the historical narrative (White 1980, 24).

6. It is true that also Ankersmit sometimes speaks of narratives as stories (e.g. Ankersmit 2009), but this view contradicts the message of *Narrative Logic*, in which Ankersmit says that he is interested in the logical structure of linguistic entities and that comparisons to the 'story-telling kind of historiography should be avoided' (Ankersmit 1983, 16; similarly 1986, 2). He also explicitly denies that narratives are stories (1983, 16; 2005, viix); instead they are 'logical entities' (1983, 94).

7. White (1980) is amusingly attracted to a rhetorical strategy that includes such locutions as 'professional opinion has it' (9), 'by common consent' (9), 'modern commentators have remarked' (13), 'by common consensus' (19), 'according to the opinion of later commentators' (20), 'common opinion has it' (23), 'the commentators tell us' (25), 'it is the modern historio-graphical community which has distinguished' (27), etc., implying that something is generally accepted without however giving specific references.

8. Despite this, chronicles are not, according to White, proper narratives because they do not terminate, but remain 'unfinished' and, because of this, cannot endow a proper meaning to events (White 1980, 24).

9. Icke thinks that this is the essence of Ankersmit's *Narrative Logic* and 'to fail to grasp this…is to miss the point of his book' (2010, 17). I agree that this is one of the central tenets of the book. Gorman (1997) questions the sensibility to assume that the whole is more than the sum of its parts. However, I do not think that the latter misses the point of Ankersmit's book as Icke claims. He is, rather, not convinced of the argument for the view.

10. Also Hayden White has analyzed Thompson's *The Making of the English Working Class* to illustrate his tropological approach. I have no reason to dispute that we could see Thompson structuring his data through some kinds of tropes or that 'he imposed *a* pattern on his subject matter' (White 1978, 15–16). By contrast, it is far less clear that White's theory of tropes, which Thompson's book thus allegedly illustrates, presents 'signs of stages in the evolution of consciousness' (White 1978, 15) itself. White claims to have found the same kind of theory in Piaget and Freud.

11. We may understand semantic holism as a doctrine according to which the meaning of semantic entity depends on the meanings of many other semantic entities. In other words, no semantic entity could have meaning alone, for example, due to its external reference or internal meaning constituents. Ned Block defines mental and semantic holism as follows: 'Mental (or semantic) holism is the doctrine that the identity of a belief content (or the meaning

of a sentence that expresses it) is determined by its place in the web of beliefs or sentences comprising a whole theory or group of theories' (Block 1998, 488). One should notice that the narrativists adopt a strong holistic position, according to which *all* the meaning constituents of a narrative define the narrative.

12. Dummett (1973) argued that a credible account of how we use language as an instrument of communication requires the rejection of holism. His point appears to be that it makes sense to require only a partial consensus of beliefs and meaning, not an all-encompassing or perfect consensus. See Fodor and Lepore (1992, 8–10).

13. Despite holism, Ankersmit also commits himself to the analytic–synthetic distinction when he states that all the sentences of a narrative/representation are analytically true of it, but that there are other, singular, sentences that are true or false on a factual basis. This is somewhat ironic since, while holism might be seen as a way beyond the problematic distinction, Ankersmit ends up accepting two philosophically contested doctrines.

14. Block (1998) defines 'molecularism' as follows: 'Molecularism characterizes meaning and content in terms of relatively *small parts* of the web in a way that allows many different theories to share those parts' (489).

15. William Mulligan emphasizes the revisionist nature of Clark's argument, the central feature of which is to shift the blame away from Austria-Hungary and Germany and to the shoulders of Serbian, Russian and French decision-makers. Despite the fact that Clark tries to distance himself from the literature that seeks to identify the main culpable players, this in undoubtedly a hidden plot in Clark's book. However, on a more general level it appears that Mulligan too agrees with the 'sleepwalking' interpretation: 'The interaction of these different decisions [preceding the Great War], each of which was made to serve an apparently rational national interest, culminated in "Armageddon" to use Asquith's phrase. Each state, Clark argues, exploited this spectre of general catastrophe, hoping their opponents would back down. While leaders recognised that a general European war would entail catastrophic consequences, they could not "feel" it, something that separated the statesmen of 1914 from their successors in the Cold War' (Mulligan 2014, 663–664).

16. It is interesting to note that the same kind of attitude can rather surprisingly, be found among otherwise anti-realistically oriented philosophers. For example, despite his sympathies towards some very non-realistic research programmes, such as the Sociology of Scientific Knowledge, the historian of science Stephen Shapin has concluded that every scholarly tradition has to frame its talk within 'the realist mode of speech' (Shapin 1995, 311; 315).

17. Goldstein suggests that we make a distinction between *superstructure* and *infrastructure* in historiography, reflecting the 'visible' 'literary product' of historians and the 'intellectual activities', such as the treatment of and thinking about evidence' that precedes final drafting and remains invisible (Goldstein 1976, 141).

18. Remember also that White (1980) mentions Tocqueville, Burckhardt, Huizinga and Braudel as historians who have tried to write non-narrative history (although White went on to argue for 'hidden narrativity' in them).

19. Thompson mentions in the preface (1980, 11) that there is a fourth part that addresses political theory and the consciousness of class in the 1820s and 1830s, which White takes more or less at face value (White 1978, 16). However, this fourth part is not structurally comparable to three others but more likely an after-thought or a reflection on the overall significance of the book.
20. See Pihlainen (2002) who questions that narrativity necessarily implies chronological ordering, specifically when we apply the notion to more complex literary presentations
21. See Marwick's two examples of structure in the works of history. One deals with Ruth Richardson's *Death, Dissection and the Destitute* (1988) and the other with Eric Hobsbawm's *The Age of Extremes: The Short Twentieth Century 1914–1991* (Marwick 2001, 208–213).

6   Colligation

1. This example was suggested to me by Maijastina Kahlos.
2. 'Social ontologists' might see this differently and think that colligatory concepts, once created, are out there as real referrable social entities. See Searle (1997, 2006) for classic expressions of what 'social ontology' is. It must be due to my minimalism and attraction to the Occam's razor that I find it very odd to think that colligatory entities could possess an 'epistemically objective existence and [be] part of the natural world', as Searle 2006 (12) put it about social ontologies.
   Ankersmit's most recent paper (2013) suggests that 'strong individuals' are the sort of entities that don't instantiate any objects in historical reality, as opposed to 'weak individuals' that do. It is therefore possible that my non-representationalist solution applies to what Ankersmit calls 'strong individuals' in his most recent vocabulary.
3. For more on these notions, see Chapter 10.
4. For bibliographical details, see footnote 10 in Chapter 3.
5. There is no need to further discuss how different nominalist theories explain our talk of properties and kinds. There are many suggestions. For further reading on nominalism, see Rodriguez-Pereyra (2001) or Bird and Tobin (2010).
6. This was pointed out also by Cebik, who argued that that colligatory concepts 'do not easily fit the Aristotelian genus-species...mold' (1969, 45), but are better viewed in the part–whole mold.
7. It is easy to lose sight of this because narrativists have a tendency to talk about singular statements as opposed to general ones, which may mislead some into assuming that they make an analogical distinction between singular claims and general claims as in logical positivism. For a number of reasons, this is not so. The first one is the fact that historiographical theses and colligatory concepts are specific because they refer to specific times and places. The second is that 'general' refers to the synthesizing claims that works of history contain, which does not imply that the 'general' theses put forward, as in 'Khrushchev initiated the period of the Thaw', are themselves general in the sense that scientific laws and regularities are. They are the

central claims that historians make in their books and that are supported by argumentation and evidence in those books.

8. One might suggest that we could see colligatory concepts as types and their instantiations as tokens, following the type–token distinction (see Wetzel 2006; Tucker 2004, 100–102), but this does not work for the same reason that colligatory notions are not kinds: the colligated entities are not tokens of type colligations in the way that coins may be tokens of a specific coin type.

9. McCullagh's (1978) basic distinction is between 'formal colligatory concepts', which indicate the form of a historical change that is attributed, and 'dispositional colligatory concepts', which indicate a common set of dispositions, such as ideas or attitudes.

10. Shaw (2013) has recently suggested that we can distinguish three different kinds of colligatory concepts, reflecting the roles they play in reasoning. According to Shaw, colligatory concepts can play the role of the *character,* which is some kind of continuing subject ('early Wittgenstein', 'Christianity'), the role of the *ideal type*, which is a choice to highlight certain features of some objects and a formation of them into an idealized concept ('capitalism') and the role of the *period*, which thus periodizes history into larger units (The 'French Revolution') (Shaw 2013, 1093–1097). I think Shaw's specification of these distinct roles is very useful although I am not sure if it is correct to say that there are different *kinds* of colligatory concepts on this basis since it suggests that there is some fundamental principled distinction between them. It might be better to see these 'roles' simply for what they seem to be: different *functions* of (one kind of) colligatory concept. Cebik in turn suggests that one can distinguish different classes of colligatory concepts on the basis of the 'sorts and structures of criteria relevant to their justified use' (1969, 49). For example, 'plans' and 'plots' would require an orderly occurring of events, which would not be necessary in the case of the 'Renaissance'.

7 Underdetermination and Epistemic Values

1. Or, at most, it could happen only with a very limited set of sources, as in the case of Ilya Ehrenburg's novel – and even then in symbolic form.

2. Confirmation holism or holistic underdetermination is the idea that no statement is tested observationally in isolation but that 'statements about the external world *face the tribunal* of sense *experience* not individually but only as a *corporate body*' (Quine 1953, 41). This means that negative observational evidence does not indicate what exactly is wrong and what needs revising in the conjoined theoretical body of scientific statements and other assumptions.

3. James W. McAllister (1997) argues that any set of data allows infinitely many patterns, and furthermore, in another paper (2010), that the world contains all possible structures. His theses amount to the radicalization of the underdetermination thesis and have interesting consequences for the discussion on the objectivity and reality of historiographical constructs. For this, see Chapter 10.

4. This classical distinction has naturally been also criticized, for example, by many so called historical philosophers of science, such as Thomas Kuhn and Imre Lakatos. However, in this book, it is not necessary to delve deeper into the debate whether these two contexts are in fact logically independent of each other. The general distinction between justification and practice or creation is enough.

5. Due to metaphysical connotations, it is advisable to avoid using the term 'fact' to denote historiographical entities that are in an evidentiary role. I have chosen to use 'data' because it is a relatively neutral term. Some have suggested that 'evidence' would be used instead of 'data' or 'source' to describe the first-order historiographical entities out of which inferences are drawn or which are used to justify cognitive constructions (e.g. Tucker 2004). However, it is necessary to be careful here. Although 'evidence' is certainly one of most foundational concepts in any epistemological considerations, the straightforward substitution remains unmotivated. There is no 'mere evidence' in the absolute sense; 'evidence' is a relative notion. Something is always evidence for something, which shows that the concept of 'evidence' plays a justificatory role in epistemic reasoning. A random set of data is not automatically evidence for anything, although everything is potentially evidence for something. Data becomes evidence only when it is reasoned to play an evidentiary role. This shows that 'data' is a more primitive and more neutral notion than 'evidence'. It does not entail any normative connotations and can safely be used to cover any information, trace, artefacts, sources, etc.

6. The mistake he makes is that he confuses the 'observation' and 'reality' of particular events with their colligation as the 'French revolution'. Beside the fact that the 'observing' of those events is not possible for the contemporary historian, there is not automatic inference from specific events to the colligation of 'French revolution'.

7. For more on the notion of 'scientific revolution', see Nickles (2011).

8. Also Cebik suggest that 'the criteria he uses in his evaluation may be how many facts the supreme colligation can encompass, that is, how many events the notion "renaissance" encompasses as opposed to, say, a mere "period of artistic creativity"' (1969, 54). Cebik also accepts 'poetic' as a criterion. Further, scope is the central epistemic criteria for the 'objectivity' of a narration in Ankersmit (1983, 218).

9. The criteria suggested in this chapter are similar to those that have become known as 'epistemic values' in the philosophy of science. Kuhn was arguably one of most important initiators of the discussion (e.g. Kuhn 1970, 152–155, 'Postscript', p. 199; Kuhn 1977, 322–324; Kuhn 2000, 114). See also Laudan (1978, 1984) and McMullin (1982) and, on the trade-offs between epistemic values, Douglass (2014). There are also occasional references to epistemic values and sometimes to epistemic 'virtues' in historiography although no one has before tried to specify their function in historiography precisely (Ankersmit 1983; Bevir 1994; Lorenz 2002; Paul 2012; Tucker 2004). The exception is Paul, but he discusses virtues as the personal character traits of the historian.

10. One should remember that Ankersmit closely links the notions of 'view', 'colligatory concept' and his own proposal 'narrative substance', which all

express the synthesizing message of a text of history (Ankersmit 1983, 93). The later Ankersmit would speak of 'representation'.

8 From Truth to Warranted Assertion

1. For more on Russell and 'complexes', see Russell's *Principia Mathematica* (1963, 44); Klement (2014).
2. Or he talks about 'consensus on belief' (Tucker 2004, 28).
3. See also Longino (1990; 2002, 129–131) on the conditions of epistemically authoritative belief formation.
4. Steven Shapin (1994) has claimed that, at the birth of experimental science in England in the seventeenth century, gentlemanly identity was seen as a guarantee of the truthfulness of testimony. No-one would propose gentlemanly identity as an epistemically authoritative property in contemporary science. It is noteworthy that Shapin talks about trust and investigates the ways in which assertions are seen as credible and trust-worthy.
5. Dewey continues: 'A more complete (and more suggestive) statement is the following: "Truth is that concordance of an abstract statement with the ideal limit towards which endless investigation would tend to bring scientific belief, which concordance the abstract statement may possess by virtue of the confession of its inaccuracy and one-sidedness, and this confession is an essential ingredient of truth"' (Dewey 1938, 345, footnote 6).
6. Also some 'radical historical theorists' attempt to replace the correspondence notion of truth with something else. For example, Munslow makes the very conventional assumption that historical evidence has to link with truth, although the former can only function as 'evidence for the probability of truth'. Munslow believes that 'a different kind of truth is created in the narrative', which is more precisely 'historical truth': 'a form of truth in which empirically attested statements do not exist independently but that their meaning derives from their functioning *within* the narrative' (Munslow 2007 117; 119).
7. I recommend Richard L. Kirkham's book *Theories of Truth* (2001) for those interested in the concept of truth as such.
8. Some exceptions are possible. See the next chapter and the section on objectivity and subjectivity in historiography.

9 The Tri-partite Theory of Justification of Historiography

1. E.g. Chris Lorenz (1998) argues that narrativism contains positivism in an inverted form.
2. Jenkins quotes approvingly a whole set of White's assertions, which state for example that 'one must face the fact that when it comes to apprehending the historical record, there are no grounds found in it for preferring one way of constructing over another'; that 'the only grounds for choosing one perspective on history rather than another are ultimately aesthetic and ethical/political'; and that 'we are free to conceive history as we please just as we are free to make of it what we will' (Jenkins 2008a, 70).

3. Munslow even states that 'narrative' is a kind of transcendental cognitive mode of representation for 'narrativists': 'Narrativist thinkers...hold that narrative making is wired into the human brain as the key mechanism for representing reality...narrative is the *a priori* concept through which we apprehend reality. This suggests narrative is *the* mode of cognition' (Munslow 2007, 16). This resembles the position of phenomenological narrativists, such as David Carr and Paul Ricoeur.

4. Cf. also the following statement: 'There is no method of establishing incorrigible meanings; all facts to be meaningful need embedding in interpretative readings that obviously contain them but which do not simply somehow arise from them; to the chagrin of empiricists the fact-value dichotomy allows/demands this' (Jenkins 2008b, 41).

5. One could obviously qualify these expressions, for example, by localizing them to certain parts of the historical world: perhaps in some areas agriculture became more common and effective, while in general the world industrialized. This would make the interpretations compatible, but these kinds of localizations are ignored here. The point is that some interpretations simply seem better than others and it is impossible to accept contradictory interpretations applied to exactly the same part of historical reality.

6. It should be perhaps emphasized that I see Skinner's suggestion as insightful and more appropriate than most other perspectives on historiography, but I am not interested in his more restrictive methodological and other normative recommendations on how to practice intellectual history. There are certainly points of disagreement (see Kuukkanen 2008). Further, Lamb (2009), for example, appears correct on the perennial philosophical questions – that is, that some writer could have meant his or her argument to apply on an abstract philosophical level that reaches beyond the immediate context. Further, it seems entirely *possible* that there are certain philosophical problems and questions that have kept the minds of philosophers preoccupied over generations. Expressed by Collingwood's question-answer model, which Skinner endorses, this would mean that philosophers of many generations have provided different answers to the same questions. Skinner has indeed relaxed his view on these perennial questions in his later writings (Skinner 1988, 283). On other hand, the choice to accept or deny that there are perennial questions is not necessarily an empirical one, as Lamb suggests, at least according to Skinner's early formulations. His denial of perennial questions seems to stem from his contextualist commitments, which lead to holism about content or to the 'thesis of the necessary differentness of all the components of any two different thought-complexes, as Lovejoy called it (Lovejoy 1944, 209).

7. See Syrjämäki (2011), who has traced the changes in Skinner's terminology.

8. (1) Was the assassination the act of a group of independent fanatics (for Princip had been one of a group of seven potential assailants), or was the Serbian government involved, either directly or indirectly? (2) What were the actions and intentions of the German government when it gave the Dual Monarchy its support? Did Germany anticipate a local war, a continental war, or a European war? Did the Kaiser and his ministers envision the severity of the Austrian ultimatum to Serbia that followed, and were they informed of the contents of that document in time to have altered its nature had they wished to do so? (This is the most important 'key' and has dominated the

debate since 1961.) (3) (The third 'key' is based on a distinction between two phases: the period between June 28 and July 23, when the crisis involved only Serbia, Austria-Hungary and Germany, and the period from July 24 to August 4, when all of Europe became affected.) What did the French and Russians discuss between July 20 and 23, and what role did the French government (and more importantly the French alliance) play in the determination of Russian policy between July 24 and the proclamation of general mobilization one week later? (4) Why was the British Foreign Secretary Grey so reluctant to warn Germany of Britain's probable attitude in the event of a European war? (5) Why did the Chancellor of Germany Bethmann-Hollweg change his mind (from supporting Austro-Hungary's firm stance to recommending talks with Russia and British mediation)? (6) Was Russian mobilization the point after which the European war was inevitable?

10   Historiography between Objectivism and Subjectivism

1. It is surprising how few books there are specifically on the concepts of objectivity and subjectivity. Many books that contain 'objectivity' in their titles advance some specific thesis in relation to some sub-field of philosophy. For example, despite the promising title of Tyler Burge's *Origins of Objectivity* (2010), the book is not a conceptual investigation of 'objectivity' or of the origins of the concept of 'objectivity' but a contribution to the theory of perception. A notable book that appears to deal directly with the notion of 'objectivity' is Lorraine Daston and Peter Galison's *Objectivity* (2010). However, in actuality, the book investigates the ways in which different scientific practices over centuries have produced epistemically authoritative views on nature and what these views have been like (e.g. artistic drawings, photographs, mechanically produced experimental results, scientists' judgments on experimental results). With respect to historiography, it is necessary to mention Peter Novick's *The Noble Dream: The 'Objectivity Question' and the American Historical Profession* (1988; see also Haskell 1998). But again the 'question of objectivity' is briefly settled at the beginning and the book focuses on the views of American historians on the nature of their field. See also John Passmore for a 'negative' approach to objectivity in which he rejects one conception after another and ends with the view that historiography attempts to 'find out what really happens' and is therefore a science (Passmore 1966, 93). Charles Gillispie's classic in the historiography of science, *The Edge of Objectivity*, portrays the history of modern science as the development of objectivity but even the author subsequently recognized the lack of a clear definition of 'objectivity' (Gillispie 1966, xxi). The only book that directly grapples with the intellectual history and conceptual accounts of 'objectivity' is Stephen Gaukroger's *Objectivity: A Very Short Introduction* (2012). Due to the nature of the book series, Gaukroger's volume manages to carry out this task only to a very limited extent. One is still waiting for an extensive 'intellectual biography' of 'objectivity' and 'subjectivity'.
2. See Searle (1997; 2006) and his idea of social ontologies.
3. Max Kölbel writes in his *Truth without Objectivity* about one sense of 'objectivity' as that of 'ontological independence', that is, the idea that something

is independent (or exists independently) of human thought. The other sense is that something is 'publicly accessible'. He also introduces a sense of 'objectivity' as 'independence from individual viewpoints or personal preferences'. This appears to be a variation of objectivity as neutrality although objectivity also depends on the nature of the objects under consideration. The question of who is handsome is dependent on personal preferences; the height of Canary Wharf is not (Kölbel 2002, 21–22).

4. Jenkins (2003, 139) also confusingly says that there are nevertheless criteria for choosing and judging in any given social formation and we are able to give argumentative support for a position. But if this is so, those criteria are arguably valid intersubjectively, not only subjectively. This is because if all criteria were an individual's own, the notion of criteria that enabled choice would not make much sense. The position judged to be valuable according to these criteria must stand also for others.

5. Perhaps such a book could contain occasional references to actual persons or properties.

6. Ankersmit says problematically that most original theses 'suggest' much, but 'state' very little (1983, 224). As we saw, Popper argues that the boldest theses are the most explanatory and powerful while trivial statements say very little. It appears that Ankersmit is concerned with the specificity of claims.

7. What I find strange in McAllister, however, is that he takes all the patterns to exist *objectively*. Provided that no inference to a pattern from data is suggested by the research object itself, the world or phenomena, it is odd to regard all inferences as objective. It may be that McAllister's notion of 'objective' refers to the equality of access by all observers to the patterns and structures. Nevertheless, when 'objective' is understood as signifying the investigator-independence of the patterns themselves, pure object-sidedness in this sense, none of the patterns are objective.

8. I have benefited from Paul Roth's critical take on my view of rationality at the meeting of the American Philosophical Association, Eastern Division, in December 2014 in Philadelphia. This is Roth's vocabulary.

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