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Cause, Chance, Determinism and Counterfactuals in History

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... postmodern critics of history turn out to be recirculating little more than an ancient but extreme form of scepticism, coupled with an irrational refusal to distinguish more from less reliable forms of history.

Patrick Karl O'Brien

Abstract

My basic aim in this thesis is to undermine scepticism towards a causal approach to historiography. Scepticism about causal explanations in history is the result of inadequate understanding of what causal knowledge involves and how it may be provided. I identify where failures of philosophical understanding generate such scepticism, and then attempt to correct some of those failures.

Post-modern approaches to history hold that common philosophical accounts of causation seem inapplicable to the kinds of explanation appropriate in history. My response is to show that Lewis's counterfactual theory of causation fits well with the way historians make causal claims.

Historical explanations must deal with accidental happenings, chance events, and free human action; post-modern historians claim that causal explanation in history cannot account for the occurrence of these. My reply involves identifying notions of accidentality, chance and freedom, which are consistent with causal explanations.

The previous claim is sometimes supported by holding that causation presupposes determinism. I respond to this in two steps: I first explore the senses in which determinism may be presupposed by causal explanations in the natural sciences, and then consider arguments against historical determinism, showing that they fall short.

Finally, I consider whether the construction of counterfactual thought experiments may be a useful tool in the provision of causal explanations in history. I argue that counterfactual thinking plays a limited role in history, and that it is compatible with both, determinism and indeterminism.

Key-words: causation, explanation, chance, determinism, historical counterfactuals.

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Preface

During my early academic years overseas, in Brazil and in Italy, I observed a rather strange phenomenon, something that always caught my attention: many professional historians in these two countries exhibited a clear tendency to avoid making causal claims of the sort ‘event x is a cause of event y .’ On the very few occasions they did, they either apologised for using causal terminology, or employed some strategies to avoid explicit causal claims, such as giving a description of the background against which an event takes place, or the apparent ‘motivations’ for some occurrence, or even worse, indicating how we may ‘mentally construct’ our view of past happenings in order to make the sequence ‘coherent’ (but not causal?). In all cases I observed, none of the historians I met had the attitude of trying to give a causal explanation which, s/he hoped, resembled reality to some degree, as this would be ‘too presumptuous.’

On a few occasions I asked professional historians why they would avoid causal claims as if they were some sort of sin. The answers may be summarised along these lines: speaking of causes implies the ‘adoption of an archaic model for the human sciences;’ or ‘talking about causes means belief in universal causal determinism;’ causation ‘denies freedom and consequently the role of the agent in history;’ or causal claims ‘entail the existence of a mechanism of cause and effect, or the existence of productive (teleological) forces;’ etc. Once causation, determinism, chance, and freedom are properly defined, none of these fears, I believe, are well-founded.

A non-causal view of history would possibly say that history is just a mental ‘construction,’ or ‘invention,’ or just a ‘story.’ More than once I heard it said that historians just *imagine* the past in a coherent way, that ‘historical knowledge somehow *emerges* from the sources,’ among other even more obscure theories. To my surprise (and personal dislike) even those who were (allegedly) Marxists did not dare to talk explicitly of historical causation in front of their peers. The result was clear: a vast number of theses and dissertations on topics such as ‘deconstructing the past,’ or ‘subjective history,’ or the role of ‘historical discontinuities.’ They all had little to say about history, and a lot to say about how historians mentally ‘project’ history. This is known as the *Michel Foucault effect* on Brazilian historiography, which started in the 1960s and unfortunately has grown in popularity ever since.

Margareth Rago, a well-known local historian, summarises her appreciation of this ‘revolution’ in the Brazilian historical academy: “Foucault defended a historiographical attitude which is no longer concerned in discovering or explaining reality, but in *deconstructing reality as discourse* (Rago, 1995: 71).¹ She explains that, for *Foucauldian* historiography, “the historical objects [events?] are taken to be *effects of discursive constructions*, rather than *starting points* [causes?] for the explanation of social practices” (*Loc. cit.*).

Foucault himself famously claimed that: “We have to rid ourselves of the prejudice that a history without causality is no longer history” (1994a: 607).² Elsewhere Foucault recognises the problem of causation, but does not know what to do with it: “The problem of causality. It is not always easy to determine what has caused a specific change (...) I [leave] the problem of causes to one side; I [choose] instead to confine myself to describing the transformations themselves (...)” (1994b, xi-xii).

All of these pronouncements strike me as nonsense. How do we ‘deconstruct the past as discursive practice’? How could a ‘subjectivist approach,’ not just to history but to any area of knowledge, be something to be appreciated? How do we enquire into changes and not into causes? If history is just an invention, or a very personal interpretation of facts, is it still different from fictional literature? It cannot possibly be. Can a historical explanation avoid the notion of

¹ [My translation].

² Translated by Clare O’Farrell.

causation (or causality, as Foucault uses the term) and still solve any problem? I feel strongly inclined to say no.

So I became a Foucault refugee. Not so much a refugee from Foucault himself, who indulges our patience by talking eloquently about a number of aspects of history,³ but more so from his followers, the so called *Foucauldian* historians who, not rarely, misinterpret the words of their favourite scholar and create what is even more dreadful than *Foucauldian* historiography – ‘bad’ *Foucauldian* historiography.

My flight has brought me to New Zealand, where I undertook to study philosophy of history from an analytical perspective. I sought in authors such as William H. Walsh (1960), Edward H. Carr (1964), William Dray (1964, 1980, 1989), Ernest Nagel (1960, 1961), Ronald F. Atkinson (1978) and Aviezer Tucker (1999, 2011), to mention just a few, the antidote to Foucault (1994a, 1994b) and also to other *post-modern* ‘theorists’ such as Hayden White (1973, 1978, 1987) who, for reasons unbeknown to me, are so highly appreciated in other places. I looked for an approach to historiography which rehabilitated the idea that historians, albeit not being in quite the same situation as natural scientists, should also be committed to (causally) explaining aspects of *reality*.

³ For example, the fact that any aspect of human history might deserve our attention, depending on what our enquiry is about.

Introduction

The present thesis is not meant to be a comprehensive work in the philosophy of history. In analytical philosophy of history there are four main areas of research: (1) how history relates to other fields of knowledge, (2) truth in history, (3) historical objectivity, (4) the nature of historical knowledge. I will occupy myself with some aspects included in (4). I will try to provide some important clarifications of central notions such as what historical *causation* is, how *chance* and *accidentality* are to be understood, what *determinism* and *chaotic determinism* are, and finally, the epistemic function of *historical counterfactuals*. I will define each of these elements, and discuss some of their implications.

My hope is that the way I approach each of these elements will form a coherent whole which is capable of demonstrating that there is no threat to history if historians make causal inferences. On the contrary, if historians embrace causation and get their terminology right, much confusion can be avoided. This is especially true when historians talk of *historical determinism* and the *necessities* and *contingencies* of history. Lack of clarity in the use of terminology is one of the reasons, I believe, historians long ago delegated the philosophy of history to philosophers.⁴ Each of the following chapters aims to address this situation by offering historians a better insight into the realm of philosophical analysis, and providing some useful clarifications.

⁴ Even in the famous *École des Annales*, most authors did not write on theory/philosophy of history. One exception is Marc Bloch (*The Historian's Craft*, 1953). Part of his work was dedicated to rejecting *scientistic* approaches to history. In contrast to Foucault, Bloch does not reject causation.

In **Chapter 1**, I explore the plurality of views about causation employed by historians by means of a few examples of historical explanations. I proceed by showing that a suitable theory of causation needs to be able to accommodate the many senses in which historians make causal claims. For the sake of brevity I have selected three different approaches to causation. First I investigate the *Humean* notion of causation, also known as the regularity theory. I present some common objections to it, and explain why it is particularly difficult to employ this theory in historiography, given the uniqueness of historical events, and the inherent difficulty of classifying events into types.

Next I present two singular theories of causation, which resist the common challenges made against the *Humean* regularity theory, namely Mackie's *INUS* conditions and Lewis's causation as counterfactual dependence (later redefined as causation as patterns of influence). I argue that, although Mackie's theory correctly points out that causation is to be understood in terms of conditionals, it differs from the way historians think of causation because of his notion of a causal field – something difficult to specify in the case of history. Furthermore, not all causal claims are divisible into claims of either sufficiency or necessity.

Finally, I defend Lewis's analysis of causation as the best approach to what a causal relation is and how it is to be expressed. One of the advantages of this view is that it does not require an appeal to laws (and remains agnostic as to whether there may be laws of historical development). Lewis's theory is influential in historiography, especially after the 1990s, with the popularisation of so-called counterfactual history. Historians are usually accused, however, of not having much awareness of important aspects of Lewis's theory, and for this reason I undertake to explain it in detail, from the first proposal to its later refinements concerning the fragility of events, modifications which were necessary to explain away cases of redundancy.

In **Chapter 2**, I analyse the view held by some historians, such as Isaiah Berlin, that history has an 'accidental' or 'chancy' element to it, which makes it inconsistent with certain account of 'historical determinism.' Berlin's position was strongly criticised by Carr, who claimed that historians should not focus on the accidental, as accidental events do not tell us much – instead historians should look for patterns and regularities. This criticism was the beginning of a long and famous controversy. Carr was accused of historicism, and Berlin was

taken as a libertarian who defended freedom on the basis of indeterminism (although his arguments read somewhat differently).

As in Chapter 1, the issue of a plurality of definitions is salient. What do we mean by a chance event in history? Is chance the unexpected or the unpredictable? Is a chancy event an uncaused event? Is it the same as objective chance? Is it the effect of a coincidence or conjunction of independent causal chains?

In the first part of this chapter I explain ten different possible meanings for chance (some ontological, some epistemic) and provide examples whenever possible. In the second part I discuss which of these accounts are consistent with what Berlin and Carr say about historical accidentality. I intend to show that each author uses the term in a very different way, so that there is no real controversy once the terminology is well understood. I also claim that even for theoretical frameworks usually regarded as rejecting the accidental view (Hegelianism and Marxism) chance remains a possible, meaningful notion, and that it is a mistake to always oppose it to historical determinism, another notion in need of clarification.

In addition, I draw attention to the fact that most of the possible definitions of chance are consistent with the deterministic assumption. The ‘accidental’ view of history, and also the idea of agent causation (and freedom), need not necessarily conflict with causal determinism. The conflict between Carr and Berlin, and, more generally, between historicists and libertarians, can be partially resolved if the terminology used is clear and unambiguous. My major concern is to highlight that the *chancy* view of history need not be a non-causal view of history.

In **Chapter 3**, I explore four different ways (senses) in which determinism is defined. Determinism is a doctrine or assumption best defined in the realm of the natural sciences. In this chapter I move away from the philosophy of history and explain in detail the four senses of determinism, from the most fundamental metaphysical sense, to the most complex epistemic (predictive) sense. I refer here to the analysis of determinism by Stephen Kellert (1994). Although I adopt Kellert’s analysis, I do not endorse his conclusion that association of chaos theory with quantum mechanics proves determinism to be false. I rather sympathise with John

Earman's approach (1986, 2004 and 2006), which leaves open the possibility of causal determinism.

The most important of my tasks in this chapter is to differentiate between the metaphysical and epistemic consequences of the deterministic assumption. My objective is to show that determinism as an ontological tenet is capable of withstanding criticism, even though predictive determinism is likely to be false. I say little about history in this chapter, but I provide the theoretical background necessary for my consideration of historical determinism in Chapter 4. It seems only logical that, before inquiring into the possibility of *historical determinism*, we should first have a sufficient grasp of what determinism is, and what each of its senses entails.

In **Chapter 4**, I analyse some arguments raised against historical determinism. I refer mostly to the treatment given to the topic by Ernest Nagel in a very enlightening paper. I will start by defining historical determinism, a notion which is usually combined with the existence of historical laws. Such combination is controversial, as laws of history have never been satisfactorily identified.

Nagel identifies five different arguments which allegedly show that history is not deterministic (or at best only partially deterministic). I will revisit and re-evaluate each of these arguments, and give special attention to one of them: the idea that the emergence of novelties in human affairs would be incompatible with the standard view of determinism. I use the theory of convergent evolution to clarify why novelties cannot be used against the deterministic assumption, especially if by determinism we mean only the ontological sense of the doctrine, and not predictive determinism as defined in Chapter 3.

It will be clear, I hope, that most attacks on the possibility of historical determinism are grounded on incomplete human knowledge of the laws governing the evolution of the world – and therefore on our incapacity to predict the future states of the world. I show that determinism (as an ontological doctrine) may yet hold true of the actual world, and that none of these counterarguments is sound. At best, these objections show only epistemic (predictive) determinism to be *practically* unattainable.

In **Chapter 5**, I discuss the role of historical counterfactuals, i.e. claims originated from *what if?* questions in history. If things had occurred differently in the past, how would the present be? Some historians like to entertain and describe what they consider plausible alternative worlds.

Counterfactual history, which for some is a new branch of historical research, is the effect of the combination of two ideas previously discussed: that causation is to be understood as causal dependence (which in turn is understood in terms of counterfactuals), and that historians explain the weighing of causes by appealing to the idea of ‘historical contingency.’

I start this chapter by illustrating counterfactual thought experiments in history, highlighting what it is that such experiments attempt to explain about actual history. Next I identify three possible defences for the use of counterfactual thought experiments, i.e., answers to the question why actual historians should bother with them. I argue that only one of these defences sufficiently justifies historical counterfactuals (causal weighing), and that it depends on how we define historical necessity and contingency.

I provide a definition of *historical contingency* in line with the way in which historians use such confusing terminology. I argue that Ben-Menachem’s (1997) definition of historical contingency is capable of explaining what historians mean by contingency (in a way very different from philosophers), while avoiding the conflation of necessity and causation. I also show that Lewis’s definition of *sensitivity* is similar to Ben-Menachem’s characterisation of contingency.

Counterfactual history’s importance seems a bit overrated, but it does have some epistemic value, as it helps historians to demonstrate the relative weight they assign to different historical causal antecedents. Furthermore, because contingency differs from causation, there is no reason counterfactual history would demonstrate historical determinism to be false, or an impossible approach to history. Ferguson (1999) has argued that historical counterfactuals are incompatible with deterministic outlooks on history, but this seems ill-founded, as I intend to illustrate.

Throughout these five chapters I will engage with different (but not independent) problems in the philosophy of history: some old and well-known, such as the dispute between Carr and Berlin; others more recent, such as chaostory and counterfactual history. I will offer my criticism of a number of claims made in favour and against the idea that historical events are deterministically caused in order to support my view that historians do not have to decide on the truth of determinism in order to give explanations. And good explanations are causal, either in deterministic or indeterministic worlds.

The overall stance of the present thesis is to argue that a sound historiography accepts that one important aim of the historian is to provide causal explanations. I reject a widespread post-modernist tendency to deny that history has this aim. Scepticism about causal explanations in history, and rejection of the aim of providing it, results from inadequate philosophical understanding of what causal knowledge involves, the conditions under which it may be possessed, and the implications of attempting to provide it.

Furthermore, attempts to provide a ‘non-causal’ historiography are clearly unsatisfactory. Accordingly, we need to do the hard work of reinstating the provision of causal inferences as a proper historiographic aim by: (1) identifying just where failures of philosophical understanding generate the scepticism that leads to the abandonment of this aim; and (2) tackling the task of correcting those failures. But there are limitations on how much of this task the present thesis can accomplish.

Post-modern approaches to history seem to hold that common philosophical accounts of causation seem inapplicable to the kinds of explanation appropriate in history. My response is that there is a contemporary theory of causation which can fit history well – viz Lewis’s counterfactual theory (Chapter 1). Historical explanations must deal with accidental happenings, chance events, and free human action; post-modern historians infer that these cannot feature in historical causal explanations. My response is to show that we can identify notions of accidentality, chance and freedom, which are not inconsistent with causal explanations (Chapter 2).

The previous post-modern claim is sometimes supported by holding that causal explanations presuppose determinism. My response takes the following approach: *Firstly* I

explore the senses in which determinism may be presupposed by causal explanations in science (Chapter 3). *Secondly* I consider arguments against historical determinism and show that they do not rule out the possibility of causal explanations (Chapter 4). Finally, I investigate whether counterfactual history is a viable option for historians aiming to provide causal explanations irrespective of their position regarding the truth of causal determinism (Chapter 5).

The list of topics I have selected is not exhaustive, but it represents some of the most common obstacles to accepting the provision of causal inferences as proper to the historian's tasks. By overcoming such obstacles I hope to dissipate, or at least undermine, scepticism towards a causal approach to historiography.

Finally, a note on terminology: some authors (Tucker, 2004 and 2011) like to draw a very precise distinction between *history*, *historiography*, *philosophy of history*, and *philosophy of historiography*. *History* is the objective past, the actual sequence of past events. *Historiography* is the science/art practiced by historians when they make inferences about the past. Historians are practicing historiography when they explain and theorise about parts of history - an organised body of knowledge about past events and processes is historiography.

Philosophy of history is ambiguous terminology. There are two kinds of philosophy of history: *critical* and *speculative*. *Critical philosophy of history* (or *philosophy of historiography*) is a branch of epistemology that examines the nature of historiographical knowledge (for example, the nature of causal claims in historiography). This thesis is a work in philosophy of historiography. *Speculative philosophy of history* is an attempt to get knowledge of the past by means of intuitions on what the essence of history is. Such an approach relies on mysterious ideas of self-conscious knowledge, or the realisation of a *World Spirit* (Hegel), etc. Theories advanced by speculative philosophies of history are generally not testable, do not depend on empirical evidence, and therefore are completely unscientific.

It is, however, conventional to refer to the discipline of history as simply 'history.' I will not, in general, make use of such specific terminology here.

Chapter 1

Causation in History

1. Causation

What makes historians more than collectors of facts and events? In what sense does history provide useful information about the world and its past? The only viable answer I can see relates to causation. If historical events, in whichever way they are presented (usually narrative), are not shown to stand in causal relations to one another, then no historical understanding is possible and no explanations may be given – narratives become meaningless. Without causation – in a sense which needs to be properly defined – historical events remain unrelated. Causation is a fact of the history of human affairs and the world.

1.1 The causal view of history

The previous paragraph basically explains my view on the purpose of history: to identify causes, and to provide explanations about the past showing how, based on adequate empirical data and rational enquiry, one event is the effect of causal antecedents. This is the only way to understand how events occurred as they did.

This is certainly not an original position. Tapp is one example of an author who claimed that without causation there is no proper history (1952: 68). Before him, Teggart had already insisted that historians are not just chroniclers of the past, that historians must not be content in

saying just ‘what’ happened, but they should also attempt to provide answers to ‘how’ or ‘why’ events happened as they did. Such a task is possible only by finding the ‘connection’ between events (1942: 3). Carr insisted not only on the importance of causation,⁵ but also on the overall importance of a certain *kind* of causal explanation, and highlighted the fact that much of history is based on the selection of events and their subsequent ordering into a hierarchy of causal importance.⁶ Atkinson argued that “even in cases where it would be natural to say that there was agreement on facts but dispute over causal connections, there will still be causes in the facts” (1978: 144). More recently Evans (2000), Froyeman (2009), and Brien (2013) have defended the importance of historical causation (mostly against irrational post-modern attacks). This is just to mention a few authors who, in my view quite rightly, believe that causation must occupy a central role in historiography.

But the acceptance of the importance of causation has faced perennial difficulties. Some of these involve the identification of causal factors and their definition: is a cause to be understood as a necessary and sufficient condition? Do we have a plurality of causes affecting a single event in the past? What about the procedure of selecting causal factors and placing them into a causal explanation. Can this be done sufficiently objectively? Or does historical explanation require a subjective selection which endangers any scientific approach to history? Are there causal laws of historical development? If so, are these of a natural or supernatural nature? Furthermore, there is the discussion surrounding causal determinism and its alleged threat to free will and the role of the individual, and the role of accidents or chance events in history.

It is also easy to get distracted from the discussion of *what* causation is into ‘aspects’ of causation and their ‘implications’ (Cf. Conklin, 1974 and Brien, 2013). For example, does a causal view of history imply the truth of determinism? I believe it does not. Does determinism undermine the causal role of individuals by making free will impossible? I believe it does not. Are historical ‘accidents’ evidence against determinism? I believe that for most senses in which

⁵ “The study of History is a study of causes” (Carr, 1964: 87).

⁶ Hierarchy, although frequently used, is perhaps not the most adequate word. It refers to Carr’s methodology of history in which causes presented as part of trends or regularities are more useful or relevant for historians.

chance is used it does not.⁷ Still, all these questions are not simply about causation. The importance of the role of causation in history is usually not denied, but obfuscated by topics which easily gather more attention, such as the problem of freedom.

There is, of course, worse in store. Because of some difficulties arising from the application of causal theories in history, some have argued against the possibility of historical objective causal knowledge altogether. This is the case of White, one of the first philosophers of history I became acquainted with while an undergraduate student. It was not a good start. White argued that the selection of causal factors by historians is necessarily subjective. In his three key texts on this matter - *Metahistory: The Historical Imagination in Nineteenth-century Europe* (1973), *Tropics of Discourse: Essays in Cultural Criticism* (1978) and *The Content of the Form: Narrative Discourse and Historical Representation* (1987) – White argued that historians perform a ‘constructive’ approach to historiography, and that their explanations are rhetorical and poetic rather than scientific.

White thinks of historical narrative as representations of events *ordered* in sequential time, constructed in a coherent manner.⁸ Representations are descriptions of real facts but always through the subjective lenses of the narrator. A good narrative, according to White, is one that persuades the reader, and not one that is constructed more objectively, based on a relevant selection of historical causal antecedents and how they relate to some event’s occurrence. From this claim of historical subjectivity, White concluded – and I do not endorse the premiss – that investigation of ‘actual’ causal links is not of central importance for historians.

If White is correct, i.e., if there is indeed an inescapable great deal of subjectivity in historical knowledge, then it is certainly difficult to investigate *objective* causal connections. It would be impossible to determine, for example, which of two rival narratives provides a better explanation of reality.⁹ In Collingwood we had the idealistic view that all history is the history

⁷ I investigate the different senses of *chance* in Chapter 2.

⁸ The use of the term *ordered* here raises the question: what orders them? I think it is more than temporal order; causes must be involved.

⁹ We may have fictional narratives which are perfectly coherent.

of thought,¹⁰ a view which renders a cause something that happens in the mind of the historians and is achieved by means of mentally re-enacting the past by placing oneself in the shoes of the historical agent.¹¹ White, however, insists that subjectivism in causal selection and interpretation speaks against the possibility of objective knowledge of the past, so maybe historians could dispense with it.

It is important to distinguish causation *qua* the causal facts from causal knowledge. White is certainly aware, if I interpret him correctly, that even fictional narratives make causal claims. There are historical causal facts, but there is no point in historians attempting to gain knowledge of those facts because of subjectivity, or so believes White. A common consequence of such a view is the abandonment of causal inquiries and the focusing on 'discourse and representation' - which is not (or should not be) the purpose of history.

White's position is mistaken. A historical narrative is not simply an ordered and coherent subjective representation, but very often an attempt to objectively explain reality. Subjectivity concerning the selection of causal facts is not an excuse to dispense with the attempt to gain causal knowledge (or *qua* causal explanations). In this sense I agree with Daniel Little's definition of a historical narrative:

A narrative is intended to provide an account of *how* a complex historical event unfolded and *why*.¹² We want to understand the event in time. What were the contextual features that were relevant to the outcome - the settings at one or more points in time that played a role? What were the actions and choices that agents performed, and why did they take these actions rather than other possible choices? What causal processes may have played a role in bringing the world to the outcome of interest? (2010: 29).

A possible retort to Little is to say that not all narratives are necessarily causal. Maybe so. But the price to pay for doing away with causation is high. If a narrative is not causal, if it is

¹⁰ "All history is the history of thought; and when an historian says that a man is in a certain situation this is the same as saying that he thinks he is in this situation. The hard facts of the situation (...) are the hard facts of how he conceives the situation" (Collingwood, 1946: 317).

¹¹ This raises the epistemological problem of our knowledge of other minds, which Collingwood believes to have resolved.

¹² I would add that *why* in a narrative is usually a *contrastive* question, in the sense that a narrative explains why one chain of events occurred rather than another. Cf. Bromberger (1966).

not a coherent representation of a sequence of events in time, unfolding as they *ought* to in accordance to their causes, is it the kind of narrative historians should be concerned about? I believe it is not.

In the following chapters I will deal with some of the aspects of causation, such as belief in causal determinism and chance. These related issues are all important and relevant, and shall be given proper attention. But for now, they shall not distract us from our basic task in this chapter: to understand what an historical *cause* is and how to characterise causal factors in history.

I start this chapter by illustrating and characterising the multiplicity of ways historians express (or claim to have identified) causal relations (§1.2). Once we have reviewed some of these claims, we may proceed to try to define causation, or say what it *is*. Three theories of causation are discussed: *Humean* regularity (§1.3.1), Mackie's *INUS* conditions (§1.3.3) and finally Lewis's theory of causation (§1.3.4). In the case of Lewis, I analyse some important aspects of his theory, such as his notion of comparative similarity of worlds (§1.3.4.2) and some of the challenges to the theory and the later modifications made by Lewis in order to get around them (§1.3.4.3).¹³ This analysis is important because Lewis's theory, being a singular theory of causation, fits the way historians talk about causation better than other theories do, so it is worth understanding it in detail. Lewis's improved version (§1.3.4.4) is particularly useful for overcoming some challenges to the theory. Finally, I briefly inquire into the relation between understanding causation as counterfactual dependence, and entertaining historical counterfactual alternatives (§1.4). The later part of the chapter offers a more extensive analysis of the counterfactual causal theory. This is important because historians make counterfactual claims but usually do not really have a full grasp of what Lewis's theory entails.¹⁴

¹³ This will be particularly relevant for my analysis of the function of counterfactual thought experiments in history, the subject of Chapter 5.

¹⁴ Tucker makes a similar remark: "Though historians have not demonstrated much footnoted awareness of Lewis's theory, they have been debating whether they use or could use counterfactuals in their work and how they could possibly be confirmed" (2011: 103).

1.2 Examples of causal claims in history

I previously said that historians make causal claims in different ways. It might prove useful to have a look at a few examples.

- (a) War began when the Athenians and the Peloponnesians broke the Thirty Years' Truce which had been made after the capture of Euboea. As to the reasons why they broke the truce, I propose first to give an account of the causes of complaint which they had against each other and the instances where their interests clashed (...). But the real reason for the war is, in my opinion, most likely to be disguised by such an argument: What made war inevitable was the growth of Athenian power and the fear which this caused in Sparta. As for the reasons for breaking the truce and declaring war which were openly expressed by each side, they are as follows (Thucydides 1, 23: 48-g, as quoted in Atkinson, 1978: 141).
- (b) The Dust Bowl brought ecological, economical and human misery to America during a time when it was already suffering under the Great Depression. While the economic decline caused by the Great Depression played a role, it was hardly the only guilty party. What circumstances conspired to cause the Dust Bowl? Economic depression coupled with extended drought, unusually high temperatures, poor agricultural practices and the resulting wind erosion all contributed to making the Dust Bowl (Trimarchi, 2011).
- (c) The fundamental cause of the Great Depression in the United States was a decline in spending (sometimes referred to as aggregate demand), which led to a decline in production as manufacturers and merchandisers noticed an unintended rise in inventories. The sources of the contraction in spending in the United States varied over the course of the Depression, but they cumulated in a monumental decline in aggregate demand. The American decline was transmitted to the rest of the world largely through the gold standard. However, a variety of other factors also influenced the downturn in various countries (Pells, 2014).
- (d) The decline of Rome was the natural and inevitable effect of immoderate greatness. Prosperity ripened the principle of decay; the cause of the destruction multiplied with the extent of conquest; and, as soon as time or accident had removed the artificial supports, the stupendous fabric yielded to the pressure of its own weight. The story of the ruin is simple and obvious: and instead of inquiring why the Roman Empire was destroyed we should rather be surprised that it has subsisted for so long (Gibbon, 1840: 529).

We may draw a few observations from these brief excerpts.

In the case of (a) historical causes are *intentions*, or *reasons* are presented as causes. It was the fear of Athens's growth which prompted the Spartans into war, into action, according to Thucydides. Although this quotation is the oldest of the four, the idea that historical causes are reasons for actions is still very present. In the words of Brien "to truly understand why an event happened historians must seek out the participants' intentions and examine the conditions under which they acted" (2013: 77). Surely intentions, *when* they bring about action, turn out to be *causes*. For Collingwood this idea was combined with the claim that 'free' choice is history's only proper cause. Stanford for example, advocated the view that historical causes are to be found in agent's actions and decisions, and their motivations (Cf. Stanford, 2001).

I once took part in a discussion on the claim that the eruption of Vesuvius was not a historical cause of Pompeii's destruction. How can this be? The criterion my opponent seemed to be using was that the volcanic eruption is a natural event, and should not enter the list of properly *historical* causes. Such an approach is usually motivated by a desire to link historical explanation exclusively with 'free' human action. But this is silly: one would have to conclude that the Roman decision to create a settlement just below the volcano is a historical cause of Pompeii's destruction, while the eruption itself is not – because it would be in a different category of cause (non-historical). Nevertheless, historians' explanatory use of events seems to be very broad; events may be either simple or complex state of affairs or facts (depending on how one defines each of these terms), and certainly include both natural occurrences and actions. Actions are historical causes, but not exclusively so; volcanic eruptions may also be historical causes, as long as they are responsible for 'bringing about' any event of importance for historians. I do not endorse the view that actions belong to a different or *special* category of cause, nor the view that before humankind the world had no history – I cannot see any epistemic advantage in doing so.

Another important remark in relation to (a) is the fact that it is unclear whether Thucydides's causal claim is capable of generalisation. Is fear of a neighbour's growth of wealth and political power *always* a cause of war? Or is it true only in the case of Sparta? If one wants to

endorse a sense of causation according to the regularity view, such claims may prove problematic, as they seem to refer only to the relation between event particulars.

In the case of (b) we have the popular idea that a single event may have a number of different causes. This idea has become common after the repeated failed attempts to identify the cause of historical developments – Kant’s *Divine Providence* (1883) or Hegel’s *World Spirit* (1977 and 1988) are good examples of the monocausal view. The plurality of causes raises questions about how historians attribute different causal weight to causal antecedents – what Carr called a ‘hierarchy’ of causes. Can this be done objectively? Other questions raised include: are all antecedents responsible for the event in question? Are these conditions independently necessary but jointly sufficient for the occurrence of the event in question? As Atkinson has highlighted,¹⁵ the characterisation of a plurality of causes in terms of, for example, necessary and sufficient conditions, has as a consequence the fact that the concept of causation must permit such plurality, as we shall see in §1.3.

In (c) we have the idea that, although an event has multiple causes, one or two of them may be more important, or carry a greater ‘weight,’ than others. The relative importance of causes has long been a controversial issue. How do we assign different weight to historical antecedents? Is it an objective decision or is it strongly influenced by the historian’s conceptual framework? As we shall see later in §1.3.4.3 Lewis on *sensitivity* provides a good explanation of how to make sense of causal weighting, but it requires adoption of a counterfactual view of causation.

Finally, (d) represents the idea that we have long term ‘forces’ at play in history. For Gibbon, it was the Roman Empire’s large territorial extension and its ever-increasing administrative difficulties which causally explain the fall of Rome. Incommensurate ambition is at the root of the Roman decay, it is the most fundamental, long-term cause. This view of causation has sometimes been condemned as ‘deterministic’ because it leads to idea that (some) historical events are inevitable; and inevitability in history has undoubtedly raised many eyebrows (Cf. Berlin, 2002).

¹⁵ Cf. Atkinson (1978: 143-151).

Other types of fundamental causes have been put forward; for example, the Marxist's insistence on the preponderance of economic factors for any historical system, or Fernand Braudel's (1966) insistence on the causal preponderance of geographical factors over cultural ones. Sometimes these long-term causes have been called causal *structures* – but I personally find this terminology unnecessarily complicated. It is clear that some causal chains are very long, and go well into the past, and are highly complex, but there is no reason to refer to them as a *structure*. Little gave some examples of such long-term processes, usually imperceptible for the historical agents involved, as a “slow increase of population density relative to available resources; a gradual shift in the gender ratio or age structure of a population; the gradual silting of a river system and estuary; a slow erosion of a traditional system of values (...)” (Little, 2009). At times, an historical explanation is a combination of such long-term causes with more immediate causal factors, usually an individual's actions (Cf. Atkinson, 1978: 143, and Little, 2011).

1.3 Defining causation

I previously argued against some post-modern approaches to historiography which dispense with the importance of causal inquiries. I believe that reference to causal information is necessary for an explanation to count as such (Cf. Lewis, 1986c). Some explanations do not make explicit causal claims, but the causal inferences can *always* be implicitly found in the order of the description of events in sequential time.

Explanations may be simply defined, in *Lewisian* terms, as the provision of causal information. But to define causation is not an easy task. If we perceive one event following another as part of a causal relation, the nature of this relation is controversial. Is it that one event *necessitates* another? What kind of necessity is it? Is it *logical* or *natural* necessity?

In his introduction to the regularity theory, Atkinson asks himself: “is it not possible to produce radically non-causal expressions, equivalent at least in the sense of having the same truth-values as causal expressions, even if they are not in all respects the same in meaning?”

(1978: 144). Or, as Garrett puts it more simply: “is the causal reducible to the non-causal?” (2011: 77). In the next section I try to answer to this question.

1.3.1 Humean regularity

Probably the first modern attempt of finding a reducible theory of causation is that of David Hume. In Hume’s *Treatise of Human Nature* (1738), the British empiricist was concerned with the origin of the ‘idea’ of a cause. According to Hume, there are three important elements in the *idea* of what a causal relation is.

Firstly, Hume claimed that we perceive causes and effect as *contiguous*, i.e., adjacent in space-time. If not so we would have a chain of causes across a temporal and spatial gap, which would seem impossible. *Contiguity* is not problem-free for Hume, as he believes that mental events stand in causal relations too; but are mental events located in space and time? (Cf. *Treatise*, I, IV, 5).

Secondly, Hume believed that an effect is always temporally *preceded* by its cause, i.e., there can be no backwards or simultaneous causation. He called this temporal precedence *priority*. Again, priority is not unproblematic, and in the past century physicists have put forward various theories allowing, for example, for simultaneous causation.¹⁶ In the past Kant argued that when a heavy object causes an impression on the cushion it is placed on we have simultaneous causation.

The third element of our idea of causation is *necessity*. If *a* is a cause of *b*, then the connection between *a* and *b* is *necessary*. Hume claimed that although we perceive the first and second element in any succession of events, we do not perceive the *necessity* in the relation between *a* and *b*. What happens is that we become aware that in some succession of events, events of a Type-*A*, for example, are always followed by events of a Type-*B*. If our exposure to such regular succession is uniform, we form the idea that there is a necessary connection between *a* and *b*.

¹⁶ For example: the Wheeler-Feynman theory of radiation, and also Beauregard’s ‘quantum handshake’ explanation of the violation of the Bell inequalities (Cf. Schaffer, 2014, and Jones, 2014). See also §3.5.

Hume believed that such projection was an image of the world originated from repeated exposure to *constant conjunction*, and not a true representation of a feature of the world itself. *Necessity* would be a projection of our minds onto the real world – a position which is now known as Hume’s *projectivism*:

For, after a frequent repetition, I find that upon the appearance of one of the objects the mind is determined by custom to consider its usual attendant, and to consider it in a stronger light upon account of its relation to the first object. It is this *impression*, then, or determination, which affords me the idea of necessity (*Treatise*, I, IV, 14).

Hume is often interpreted not as realist about causation, but as a *projectivist*.¹⁷ Collingwood was sympathetic with such approach to causation, but I would like to reject it. The reason is rather simple: if there is no such thing as worldly or natural necessity, and if by reality of causation all we have is the constant conjunction of Type-*A* events with Type-*B* events, then is not clear what it is that makes causal claims true (unless it is the fact of the conjunction itself that makes the casual claim true; but on the regularity analysis this appears to be trivially true). There may be more to the reality of causation than just constant conjunction, for reasons which I will rehearse below.

In the previous paragraph I described Hume’s concern with the *idea* of causation, i.e., how it is formed in our minds. Hume has notoriously defined causation twice, and the two definitions are not equivalent (although he may have thought they were).¹⁸ First Hume says that a cause is “an object, followed by another, and where all objects similar to the first are objects similar to the second.” Secondly, he added a second definition of a cause as “where, if the first object had not been, the second never had existed” (*Enquiry*, Section VII).

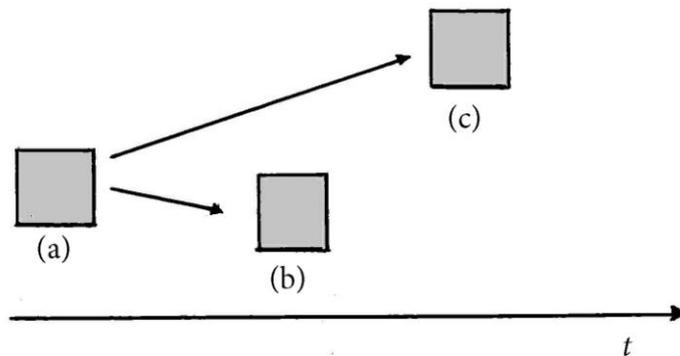
From Hume’s first definition we have the origin of the so-called *regularity theory of causation* (RT). This reductivist account of causation may be summarised along these lines: *a* causes *b* iff it is the case that *a* temporally precedes *b*, and all Type-*A* events are *always* followed by Type-*B* events.

¹⁷ This interpretation is contentious. Hume’s projectivism concerns the origin of the idea of causal necessity. Whether such idea has real reference is a further matter. And if it does have real reference, it is a further question how to give metaphysics of causal necessity.

¹⁸ This is a reference to Hume famous “in other words” (*Enquiry*, Section VII).

There may be reasons to reject the RT. If RT requires temporal *priority*, and if *priority* is false, RT must be rejected.¹⁹ Or we may have cases in which we do have priority, and there is a regular conjunction in which all Type-*A* are followed by Type-*B* events, and yet *A* cannot be the cause of *B*. This occurs in the case of epiphenomenalism, in which an event, say the fall of the barometer (b) always precedes the storm (c). But the fall of the barometer does not cause the storm; (b) and (c) are *joint effects* of a common cause²⁰ (a): the pressure dropping, as the figure below illustrates. The ‘→’ stands for causal relations; note that there is no ‘→’ connecting (b) and (c):

Figure 1 - Epiphenomenalism²¹



Epiphenomenalism alone might be a good reason to reject the RT. Garrett (2011: 80-2) summarised other well-known good reasons for rejection. Some of them are very straightforward, for others the RT might have a reply:

(1) When we say that *all* Type-*A* events are *always* followed by Type-*B* events we are making a *universal* claim which cannot be proven true.²² Garrett says that in order to prove the claim true of our world we would have to describe the *impossibility* for there to be a Type-*A* not

¹⁹ But we could easily modify the account in order to accept simultaneous causation, so that *A* either temporarily precedes, or is at the same time as, *B*. Lack of *priority* would then be no reason to reject RT.

²⁰ Observe that if we appeal to a *common cause*, causation gets back into the picture. This is a problem for RT, since it is meant to be a reductivist account of causation.

²¹ Adapted from Schaffer (2014).

²² It could, however, be proved false.

followed by a Type-*B* event. This requirement is probably too strong. But we need to rely on an inductive inference here,²³ and this raises the question of how justifiable an inductive claim is. Clearly, we can never fully verify unrestricted universal claims.

(2) Causation does not need to be defined only in terms of *generalisations*, i.e., there is no “conceptual infelicity in someone endorsing a singular causal claim, yet refusing to endorse any corresponding general claim” (Garrett, 2011: 81). The reason for the refusal is epistemic. Garrett claims that we usually do not infer from ‘Carol has developed cancer’ that everyone in similar biological circumstances will develop cancer as well because we may not know what the biological circumstances are. Although we may characterise causation in terms of generalisations, we sometimes make causal claims without any reference to general claims simply because at times we are not able to do so.²⁴

There is an important side note here: for a Type-*A* event to cause a Type-*B* event Hume wanted the connection between events of Type-*A* and events of Type-*B* to be 100%, with no exceptions allowed, or we would have to concede the presence of intervening factors. But if the world is indeterministic this would imply that some causal-effect relations are not captured under the exceptionless generalisation that *whenever A then B*. We can, however, modify Hume’s account and accept that a causal-effect relation expresses a causal frequency only, so that causation can be defined in terms of generalisations even for an indeterministic world. Indeterminism alone is no reason to reject a non-strictly *Humean* account of the RT.

(3) Garrett believes that the RT is counterintuitive, especially in the case of history. When we say that Oswald shot Kennedy we refer to a local relation between event particulars: Oswald’s shooting, and Kennedy being killed. According to the RT we would have to look at past shootings (similar types of events) in order to determine *A* caused *B*. Garrett says that other shootings should be irrelevant, and that a singular theory of causation respects such intuition. But maybe the RT defender could reply that re-descriptions of events *A* and *B* may offer a causal

²³ As von Wright explains: “According to Hume the relation between cause and effect is a regular sequence in time of (instantiations of) generic phenomena. That the regularity will continue to hold in the future is an inductive generalisation based on past experience” (1971: 34).

²⁴ Nevertheless, we may be committed to *there being* a subsuming generalisation, even though we do not – and perhaps could not – know it.

relation in the *Humean* sense. A shooting can cause a killing without there being a regularity under those descriptions. It is also clear that a generalisation is logically possible: *As* (the shooting events of the right sort) are *always* or *usually* followed by *Bs* (the death events).

(4) The RT seems to suggest that causation is imperceptible in individual cases; otherwise why bother looking for regularities? But we frequently infer causation as a relation of necessity between two local events only. If we were to use the Oswald example, one would clearly identify Oswald's shooting as the cause of Kennedy's death, even had there never been similar events (other shootings) before. Of course we do not actually 'see' the relation of necessity; but the inference need not be a consequence of repeated exposure to 'constant conjunctions,' as we do not 'see' the necessity there either. Assume someone has never been exposed to, nor is theoretically acquainted with, any kind of shooting (no exposure to constant conjunctions of this kind). Would that person, witnessing Kennedy's assassination, make the causal inference from the shooting to Kennedy's death? Quite possibly so.²⁵

(5) The RT requires that events be perceived as belonging to a certain *type* of event. But what exactly does it mean? What is it for an event to be of a *type*? The typical answer would say that events of the same type have some degree of *similarity*. Since two events cannot be similar in all respects, they must be similar in some large number of *relevant respects*. Sometimes relevant respects can only be identified in terms of causal properties; i.e. an event is of a *type* that causes or produces an event of another certain *type*. But this is problematic because the RT is meant to be reductive; explaining causation only in terms of regularity and temporal precedence between *types* of events. If we have to appeal to causes in order to specify the *types*, then the definition becomes clearly circular.

Before we reject the RT in favour of the alternative, singularity view of causation, it is important to discuss it a bit further.

One difficulty for the RT is clear; historical examples resist generalisation, yet generalisation is required by the RT. In the same way, models of causal explanation under the RT require reference to regularities and/or *laws*. If no regularities or laws are known, no

²⁵ S/he would, perhaps, identify the shooting and Kennedy's death as abnormal events, and presume that these are causally linked.

explanations may be given (Cf. Hempel, 1942). But do we have proper generalisations in history? If we do, it is hard to establish them given the uniqueness of historical events, and the difficulty of finding sufficient *similarity* in order to classify events in *types*.

I mentioned the fact that in example (a) (§1.2) it would be bold to claim that fear *always* leads to war. We have a more recent example in which fear of the enemy's power not only did not cause a war, but possibly prevented one: the Cold War. In the same way, it is almost certain that some wars had causes very different from fear, and fear despite being present had little influence either producing the war or preventing it, i.e., cases in which fear was less of a causal factor.²⁶

The problem of historical generalisations has persisted since Aristotle, who claimed poetry to be more 'scientific' than history, because the former allows for universals, while the latter does not.²⁷ History is often about what is unique: "It is one thing to say that a ball broke the window (...); quite another to say that Mikhail Gorbachev caused the collapse of the Soviet Empire as there has been only one Gorbachev and the Soviet Empire fell only once" (Tucker, 2011: 100).²⁸

Historians find it difficult to deal with the RT for a number of reasons: it is hard to come up with historical regularities (or laws); attempted historical generalisations seem arbitrary or vague; history is about the unique; historians are usually concerned with particular causal relations and, at times, find it unnecessary (or impossible) to make universal claims.

Although causal explanation is to be distinguished from causation, there is a relation between thinking of causation in term of RT, and looking for general-laws. The reason is clear; if causation is to be defined as the RT requires, then the best possible explanation is the one that

²⁶ It shall be clear that the explanatory force of historical explanations does not amount to subsumption of the events explained under suitable regularities.

²⁷ Aristotle: "The true difference is that [history] relates what has happened, [poetry] what may happen. Poetry, therefore, is a more philosophical and a higher thing than history: for poetry tends to express the universal, history the particular. By the universal I mean how a person of given character will on occasion speak or act, according to the law of probability or necessity; and it is this universality at which poetry aims in the names she attaches to the personages. The particular is – for example – what Alcibiades did or suffered (*Poetics* IX, 1451b, 1-5, translated by S. H. Butcher).

²⁸ Tucker's remark is a bit odd, as the ball breaking the window is also a unique event.

identifies causal laws. But historians are known to deny commitment to laws. Dray has illustrated the case with the following example. Suppose that a historian is explaining the causes for Louis's XVI's remarkable unpopularity, and yet she refuses to accept commitment to the law "rulers who pursue policies detrimental to their subjects' interests become unpopular" (1957: 33). The probable reason for the refusal was that she was not intending to claim that any detrimental policy would cause a country's ruler to become unpopular. As Dray explains, it is precisely because of the peculiar form taken by such policies in a particular case that they can be used as part of an explanation. Let us say that in the case of France the peculiar form included "the involvement of the country in foreign wars, the persecution of religious minorities, the maintenance of a parasitic court..." (*Loc. cit.*)

A logician could challenge the historian's objection by simply absorbing the peculiarities of Louis XVI's policies into the form of the law, which now reads: "rulers who involve their countries in foreign wars, who persecute religious minorities, and who maintain parasitic courts, become unpopular" (Dray, 1957: 33-4). The logician could, of course, accommodate any further restrictive particular characterisation of the case made by the historian, and absorb any additional factors.

Still, the historian objects that his/her explanation identifies a general law, because although in the case of 'unpopular policies' s/he seems to be making a general claim, in providing a more detailed explanation of Louis XVI s/he will not only talk of generalities such as warlike policy of foreign affairs, but most definitely describe event particulars such as "attacks on the *Jansenists*" (Dray, 1957: 37).²⁹

The most compelling argument against the RT in the case of history, as illustrated by Dray,³⁰ can be summarised along these lines: the truth of generalisations (or causal 'laws') operates on the level of the description of events, i.e. how events are *typed*. We get the

²⁹ A French theological movement condemned by the Roman Catholic Church because, among other things, of its alleged affinities with Calvinism.

³⁰ After criticising the role of general-laws in history Dray embraced the view that historical explanations are essentially description of agents' actions, showing why the course of action taken was the 'rational' thing to do. See also von Wright (1971: 25-6). This raises the question: are reasons or intentions proper causes? Donald Davidson (1963) argued in favour of the view that reasons (under certain descriptions) are causes of actions, and denied the existence of psycho-physical laws. This has become a dominant view.

following picture: if *a* causes *b*, there must be a description of *a* and *b* under which we can say that these can be subsumed under a *general law*. This is only possible if *a* is a *token* of a Type-*A*, and *b* a token of a Type-*B*, so that there can be a law covering the case of *As* cause *Bs*. But we can have a number of causal statements about the causally related events *a* and *b*, and some of the descriptions of *a* and *b* may not be suited for the formulation of laws. Maybe it is the case that, perhaps because of our insufficient knowledge, *a* and *b* cannot be adequately *typed* into *A* and *B*. The general formulation of the RT requires that there be *types* (and there may be), but historians may not be in a position to know what the *types* are.

But if historians are not in a position to identify *types*, how can they specify universal regularities of the form *whenever A then B*? Historians are, frequently, not in a position to come up with (to find) the generalisations required to establish causation according to the RT; they do not always know when they are dealing with an event *a* causally connected to a *b*, or if *a* and *b* are not causally connected.³¹

This is not to say that (tentative) causal explanations which appeal to regularities can never be found in history; historians sometimes use theories from the social sciences which, *prima facie*, seem to conform to the RT requirements. But causal explanations in history, in general, do not proceed by subsumption under laws or regularities (universal or statistical). Many paradigm cases of historical explanation do not.

1.3.2 Necessity or sufficiency

Let us put the problem of historical generalisations aside for a moment. One way of referring to causes in the sense of the RT is in the shape of *necessary* and/or *sufficient* conditions. If *whenever* a person is bitten by a rabid dog (a certain type of event, say *A*) that person contracts rabies (another type of event, say *B*), then *A* is a *sufficient* condition of *B*. We have a case of *necessity* when there is a certain type of event, say *D*, which, should it be absent, would prevent an event of a type *E* from occurring, but alone would not cause it. For example, let *D* be the

³¹ But it may still be a necessary condition on the truth of '*a* causes *b*' that there are – perhaps unknown, perhaps unknowable – types *A* and *B*, such that whenever an *A* occurs a *B* follows.

presence of oxygen, and *E* an explosion; *D* is a *necessary*, but *non-sufficient*, condition of *E*. Ideally, if a certain type of event is alone a sufficient and necessary condition for another, then we have identified the cause.

But talking of *necessity* and *sufficiency* does not always capture the plurality of senses in which causal claims are made in history. In the case of our example (b) (§1.2), the Dust Bowl, it is instructive to consider whether any of those causes, when taken in isolation, is *sufficient* for the phenomenon. If so, every drought would *always* be followed by a dust storm, and this is not the case. But one may well argue that the drought is only *necessary* for the dust storm. In the case of example (d) (§1.2), the fall of Rome, one may argue that the continuous territorial expansion alone causes the decline and fall of the empire. It is a case of *sufficiency*. But is it a case of necessity? Probably not, as it is easy to imagine many other types of events capable of bringing about the end of the Roman Empire, had the territorial expansion been much less impressive and more manageable.

Another problem for the RT in the case of history is that sometimes historical causes are deemed to be neither sufficient nor necessary, but the causal relation may be *probabilistic*, or perhaps said to be true only for a unique, particular case (not generalisable).³² In our example (c) (§1.2) we saw that the Great Depression started with a decline in spending. So, in 1929 the decline in spending is probably a cause of the crisis. Does the decline alone cause the crisis? It is unlikely. Can we have an event of the type of a crisis without a decline in spending? Quite possibly so.³³ What this illustrates is that historians are prepared to concede the decline in spending as a cause of the 1929 crisis, but not that it is a necessary and/or sufficient condition of that crisis. The singularity theory of causation would do a much better job in this case.

Atkinson rightly claimed that “causes in history need not be necessary or sufficient conditions and (...) necessary and sufficient conditions need not be causes” (1978: 145). Don Crewe has summarised the issue concerning causation in term of sufficient/necessary conditions by claiming that these “distinctions are not qualities that help us to speak about what constitutes

³² Although some historical generalisations have been put forward: “accidental causes cannot be generalised (...) they teach no lessons and lead to no conclusions” (Carr, 1964: 107).

³³ Perhaps some economists would disagree, but for the sake of illustration I shall maintain that not all crises are preceded by a decline in spending.

a cause” (2013: 37) precisely because sometimes causal factors are not rightly captured under any of these conditions (being necessary or sufficient).

Historical causes are (almost) never shaped in terms of sufficiency. Even Gibbon would have resisted such a strong interpretation of his claim in example (d) (§1.2). It would, of course, be great if there were indeed such sufficient causes, as it would provide a complete explanation of a phenomenon, but this does not seem possible, particularly in the case of history.

When several causes are mentioned as causes of a given event, if the causal connection between each cause and its effect involves a sufficient condition, then we have *overdetermination*. In addition, historical events are usually complex, and it is unlikely that any single factor may be pointed out as the unique cause of it. Even if it were, is it not true that there would have been many other factors which count as necessary factors and which, if they had been absent, would have prevented the event from occurring? In the case of (b) (§1.2) Gibbon would not entertain the thought that Rome would have fallen had there been no Germanic tribes willing to invade the empire, or no generals conspiring against each other, or no local governors attempting to decentralise the political power, or no soldier willing to desert. Sometimes we may refer to an event as if it were sufficiently causing the effect, but this is usually done by ignoring the conditions which allowed the cause to produce its effect, i.e. conditions which are part of *the* condition that is *overall*, sufficient.

What if historians treated causal factors only as necessary? The claim of necessity seems to occur much more frequently than that of sufficiency. Consider this example:

(...) one could argue that poverty is a cause of communist revolutions. Indeed, the presence of poverty motivated Marx, Lenin, and Mao in their writings and strategies, and there was great poverty in pre-revolutionary Russia and China. Yet, many impoverished nations have not undergone Marxist revolutions. Why a revolution in Cuba but not in Haiti? Perhaps poverty is necessary but not sufficient for such a revolution (Sirkin, 2006: 15).

Although referring to causes only as necessary conditions seems to be preferable in some cases (instead of referring to a unique cause in terms of sufficiency), this strategy may lead us to false inferences. Is it impossible to have a Marxist revolution in the absence of greater poverty? I

do not know the answer, but the government of the United States certainly entertained the possibility; otherwise the internal repression of any communist sympathy during Joseph McCarthy's senatorial term (known as the 'witch hunting season') would (probably) not have been so strong. Perhaps my intuition is false and poverty is indeed a necessary cause for a communist revolution. Nevertheless, we could say, with a reasonable amount of certainty, that in the particular case of Russia, China and Cuba poverty was an important cause. Again in the words of Atkinson: "[historical] causes are conceived more as factors *tending* to bring about an effect than as factors without which it could not occur" (1978: 146).

In addition, many necessary factors may be deemed to be irrelevant when part of a causal explanation. Electricity is a necessary cause for the writing of this chapter on my computer, but this factor is not going to be part of the explanation as to why or how I wrote it.³⁴ If all historical causes are equally necessary, it seems difficult to justify any selection of causal factors. In conclusion, if necessary conditions were always the only kinds of conditions relevant in historical causal explanations it would seem impossible to make a deterministic causal claim, because necessary factors, by definition, do not bring about anything, they only open possibilities.

One better alternative approach to causation is to operate with *conditionals*. We say that a cause is *relatively sufficient* when its occurrence is always followed by an effect *if* certain other conditions are present. In this sense one event is said to be relatively sufficient for another on the condition of other factors being present, i.e. the sufficiency itself is determined by the conditional factors. For example, we may say that in the case of China, poverty (given the historical conditions), was, perhaps, relatively sufficient for bringing about the revolution. Such a conditional treatment is advantageous because it explains how one factor may be a sufficient cause in one case, and not in another. This new approach, favoured by Dray (1980, 1989), concedes that historians frequently provide causal explanations assuming that a cause is something which is *added* to certain background conditions. So the claim that *A* causes *B* is to be understood as the claim that *A* plus certain other (background) conditions *C* are such that *A* and *C* are sufficient conditions for *B*.

³⁴ It would have to be part of any physical explanation, however.

The distinction between causes and conditions in historiography was also influenced by studies of causation in the law: “Why, for instance, do we refuse to say that the presence of oxygen in the air is the cause of a house catching fire, or the sale of a gun to a particular man the cause of every shot that he fired?” (Foot, 1963: 505). Hart and Honoré in *Causation in the Law* (1959) argued for the distinction between causes and conditions as an essential tool for assessing moral responsibility. They pointed to different ways of differentiating causes from conditions: (1) as relative to the context of occurrence; and (2) as relative to the context of enquiry.

For an example of the first kind of case let us analyse the claim that the presence of oxygen is a cause of a building catching fire. The presence of the oxygen as well as the presence of combustible material and the dryness of the building are conditions allowing the fire to occur. Do we want to say that they are causes? If we want to stick to the distinction between causes and conditions we do not want to say it. The presence of oxygen and combustible material are expected to be there; but a spark igniting the material and causing the fire is not to be expected, it is not part of the background conditions. The spark is what we may call the *variable factor*, or the *difference maker*, and is therefore the relatively sufficient cause of the fire. A cause as a relatively sufficient condition is, for Hart and Honoré, an *abnormal* feature. Citing a cause thus implies a contrast “between what is abnormal and what is normal in relation to any given thing or subject-matter, and between a free deliberate human action and all other conditions” (1959: 31).³⁵ Morton White argued that when historians talk of a decisive cause for the occurrence of an effect, the cause takes precisely the form of an *abnormality* (1965: 107).

In the second kind of case, the context of enquiry seems to dictate what shall be deemed a background condition and what may count as a cause understood as a *difference-maker*. This approach comports with the view that different causes have different degrees of importance, as Tucker illustrates: “if we ask why did the Berlin Wall fall *in 1989*, rather than prior to that year, the slow decline and growing inefficiency of the command economies of the Soviet bloc are less important causes than the particular actions and inactions of the Soviet leadership in 1989” (2011: 101). The attribution of different degrees of importance to different causal antecedents

³⁵ In the sense that the cigarette thrown out the window of the car is a cause of the fire in the forest, but the wind is not; only actions are proper causes for Hart and Honoré.

as part of providing an explanation is a separate issue from that of causation.³⁶ But it is clear that when an event has several causes, those which are deemed to be background conditions may be interpreted as being less important as part of a causal explanation, than those deemed to be *difference makers*, or the *abnormal* factor.³⁷

Although history and law share in common the perspective that actions are causes, the scope of identifying causation in the law is more restricted. Jurists are usually concerned with causation in the context of determining moral responsibility. Historians, on the other hand, admit actions as causes, but also consider other factors to be causal. Hart and Honoré's account would provide historians with an incomplete theory of causation, or a theory which does not capture all the senses in which historians employ causation.

Furthermore, if we look at *causes* as the abnormal factor in the sense Hart and Honoré define it, and regard the background conditions as merely factors which facilitate or allow a cause to produce a certain effect, we may be providing a misleading guide for historians. It would often be the case that a *cause* would be the so-called *occasioning* cause or *precipitating* cause, but:

(...) the sort of factor one may be inclined to light upon as a [cause] will often be (...) a rather minor element in bringing about the event in question (...) the firing on Fort Sumter, the assassination at Sarajevo, the guarantee to Poland in 1939, though they have their importance, are never the conclusions of historical inquiries (Atkinson, 1978: 147).

In many situations historians would be interested more in the 'background conditions' than in the *precipitating* causes. The most popular example is the case of the assassination of Archduke Franz Ferdinand and the beginning of the First World War. The assassination may be accurately described as the 'trigger' or *difference-maker* which started the war given the *conditions* of world diplomacy in 1914. If we are inquiring into the causes of the war, the assassination as the precipitating cause is less important than the more general factors which help explain how

³⁶ See Chapter 5.

³⁷ Once again we have the problem of historical objectivity. Historians frequently distinguish between causes and conditions; but the distinction itself may be affected by the historian's subjective values or motives. See Atkinson (1978: 69-94) and Danto (1968: 88-111).

such a ‘local’ event in Sarajevo could trigger such dramatic consequences. Of course, if the question is only why it started *in July 1914*, the assassination becomes more relevant.

So far it seems that defining causation in terms of sufficient or necessary conditions is inadequate, given the plurality of causes we have in history; a plurality which is not easily captured by talking of necessity or sufficiency. And what I am concerned with is precisely what account to give of causation *as salient in* historical causal explanations. Let us see other theories of causation and decide whether or not they fare any better.

1.3.3 Mackie’s *INUS* conditions

A more elaborate theory of causation without commitment to regularities is Mackie’s *INUS* conditions. This is a theory of singular causation, and therefore an important breakthrough from the RT, and a big step away from Hume.

For Mackie causes are at minimum “Insufficient but Necessary parts of a condition which is itself Unnecessary but Sufficient for their effects” (1965: 245). What this means is that a causal factor, when taken in isolation from the other factors, is said to be insufficient to bring about the effect. But the factor is itself a necessary part of a set of conditions which are *jointly* sufficient (though not necessary) to produce the effect. In other words, each element is a necessary part of a set of elements collectively sufficient for bringing about an effect.

Causation as an *INUS* condition allows us to claim that:

(...) the economic depression of the 1930s caused Nazism without saying that economic depressions *invariably* cause totalitarian regimes as the regularity account of causation would affirm. Instead, it is a part of the set of conditions that sufficed for the rise of Nazism, and a part that could not have been replaced by another condition (Tucker, 2011: 101).

Another important feature of Mackie’s paper is the introduction of the notion of *causal field*: “in all (...) cases, the cause is required to differentiate, within a wider region in which the effect sometimes occurs and sometimes does not, the sub-region in which it occurs: this wider region is the causal field” (1965: 249). So the *INUS* conditions are relativised to a certain causal field.

For what reason does Mackie introduce the causal field? A causal field would allow us to deal with the difficulty that it seems impossible “without including in the cause the whole environment, the whole prior state of the universe (...), to find a *genuinely sufficient* condition, one which is ‘by itself, adequate to secure the effect’” (Russell, 1917: 187). In short, Mackie’s approach is also conditional; a cause is only a cause as relative to a causal field, conditional on some other factors being present. For Mackie our *idea*³⁸ of causation is basically the belief that a cause is a necessary condition for its effect, given certain circumstances (causal field).

Mackie’s *INUS* conditions are quite unlike the RT in that they concern singular causation. Although Mackie’s theory admits generalisation,³⁹ it does not require regularities of any sort, i.e. it does not have to assume the existence of covering laws. In this sense *INUS* conditions suit historians’ needs better. The theory is also pointing in the right direction by defining causation in terms of conditionals. Conditionals are essential if one is to enquire into causes by postulating hypothetical changes to the ‘background conditions’ or causal field, as in Mackie’s terminology. In addition, under ideal circumstances *INUS* conditions provide a working simple abstract model of causal relations, such as in economics.⁴⁰ In conclusion, there is no reason to assume that a cause cannot, at times, be accurately described in terms of Mackie’s *INUS* conditions.

However, Mackie’s *INUS* conditions do not provide a completely satisfactory account of causation in history. One reason is that a cause as an *INUS* conditions may be very difficult to specify, given the vast and comprehensive set of ‘standing conditions’ of a causal field a cause has to be relativised to. As a consequence, any causal claims in history would be weak, as they would only represent a *necessary* part of a set of jointly sufficient conditions which cannot be

³⁸ For Mackie (as earlier for Hume), *necessity* cannot be found in the world, but has a psychological origin, that is why he calls it our *idea* of causation.

³⁹ Mackie’s *INUS* conditions also extend to general causal claims: “All we know is that sweet-eating combined with a set of positive and negative factors which we can specify, if at all, only roughly and incompletely, constitutes a minimal sufficient condition for dental decay - but not a necessary one, for there are other combinations of factors, which do not include sweet-eating, which would also make teeth decay, but which we can specify, if at all, only roughly and incompletely” (Mackie, 1965: 252).

⁴⁰ Consider a set of equations expressing the relation between national income and the level of employment. It should be possible to specify then the necessary and sufficient conditions for an increase or decrease in unemployment (Cf. Atkinson, 1978: 151). But historical systems do not admit of such simple equations.

specified given the complexity of historical systems, and our incomplete knowledge of them.⁴¹ Another reason has to do with how the universe evolves. If a set of conditions is said to be jointly *sufficient* for an effect to occur, does it imply causal determinism? What if the universe is indeterministic? It may be challenging to reconcile Mackie's theory with an indeterministic universe.⁴²

If we are looking for a definition of causation which fits all the senses of causation employed in explanations by historians, then clearly one must conclude that *INUS* conditions cannot be the model for all causation. But it does point our enquiry in the right direction: a working definition of causation has to deal with singular causal claims, and has to be in a conditional form. It is only that the conditional form is best captured under the notion of counterfactual dependence, as we shall see in the next section. In the remaining part of this chapter I will consider the counterfactual theory of causation. Historians usually do not show much awareness of Lewis's theory, so a detailed explanation of what his model entails is probably going to be useful.

1.3.4 Causation as counterfactual dependence

Lewis attempted to deliver a theory of causation general enough to be applicable to all different kinds of causation, "and applicable even to kinds of causation never found in our own world" (Lewis, 2004: 76). Lewis's theory of causation is a singularity theory, and causation is defined in terms of counterfactuals. But what exactly is counterfactual dependence?

Lewis's analysis of causation in terms of counterfactuals (1986b) refers to David Hume's second definition of causation: "if the first object had not been, the second never had existed" (*Enquiry, Section VII*). Causation is hereby understood not as a regular succession, but as a special type of dependence, an extensional relation between two *event particulars* (*c* and *e*) in which the

⁴¹ If we had complete knowledge of all the conditions that are jointly sufficient for an effect *E* (a full cause *C*), then we could, in the appropriate circumstances, warrant a counterfactual inference of the kind had *C* not occurred, *E* would not have occurred.

⁴² For a brief discussion of other problems affecting the definition of a cause as at a minimum an *INUS* conditions see Pearl (2009: 313-16).

occurrence of *e* depends on the previous (or simultaneous) occurrence of *c*. And the non-occurrence of *c* would be followed by the non-occurrence of *e*. The claim is that *e* *causally depends* on *c* iff *c* were not to occur *e* would not occur.⁴³

An important side note before we proceed. Although Lewis's theory uses examples of deterministic causation, his theory can accommodate indeterminism (probabilistic causation) along these lines: *c* causes *e* iff the occurrence of *c* raises the probability of *e*; and the non-occurrence of *c* lowers the probability of *e*.

Let us consider a simple historical example.

On the 15th of March 44 BC Gaius Julius Caesar was stabbed twenty-three times by a group of senators and his adoptive son, Brutus, and died.⁴⁴ There is a simple causal claim here: that the stabbings to the chest (*c*) caused Caesar to die (*e*). Had Caesar not been stabbed, he would not have died. Or, at least he would not have died that very same death.

The antecedent condition 'had Caesar not been stabbed' is *counterfactual* – it is against the facts (Caesar *was* stabbed). The use of a counterfactual claim allegedly 'reveals' the connection between these two event particulars: the stabbing, and Caesar's death. There is no essential reference to any generalisation employed by the counterfactual claim. What is important is to establish that these events are causally connected in the sense that the occurrence of one depends on the occurrence of another – the present depends on the past.

Lewis explains this relation:

The way the future is depends counterfactually on the way the present is. If the present were different, the future would be different; and there are counterfactual conditionals (...) that tell us a good deal about how the future would be different if the present were different in various ways. Likewise the present depends counterfactually on the past, and in general the way things are later depends on the way things were earlier (Lewis, 1986a: 32).

⁴³ Lewis's "Events" (1986d) was a later addition to the theory. Lewis considered *events* things like everyday happenings, changes (like the crash of a car, a man's death, the throw of a stone...) and also occurrences not involving changes (an object continuing to move, the presence of a given substance in a sample...). Clearly, Lewis account of events includes all historical occurrences, processes, or state of affairs.

⁴⁴ Perhaps not all the stabbings were *necessary* for Caesar's death. According to Suetonius, a physician later claimed that only one of the wounds had been lethal, the second stab in the chest (Suetonius, Julius c. 82).

According to this view, a cause is an event that *makes a difference* in the world; it *determines* the way the future is going to be. In Lewis's words: "We think of a cause as something that makes a difference, and the difference it makes must be a difference from what would have happened without it. Had it been absent, its effects (...) would have been absent as well" (Lewis, 1986b: 160-161).

Lewis's motivation for a different approach to the analysis of causation is that the RT carries a few problems, in particular the problem of epiphenomenalism, as we have discussed in (§1.3.1). According to the RT, c causes e under a given *law*. This means that any occurrence of a Type-C event is *always* followed by a Type-E event. But the falling of the barometer is an event that always precedes the rain, and yet it is not a cause of it. We know that the lower air pressure (c) has two different consequences: the falling of the barometer (e_1) and the rain (e_2). Even if e_1 always precedes e_2 , there is no causal connection between them; both are generated by their common cause c . Can the RT account for this? I previously said that it cannot (§1.3.1).

Not all forms of regularities express causation, as epiphenomenalism illustrates,⁴⁵ and to differentiate genuine causal regularities from regularities that are not causal might be one of the advantages of a counterfactual theory of causation (assuming we are in a position to discover the truth conditions of the counterfactuals). Note that we can say, still on the barometer case (epiphenomenalism), that if the air pressure had not dropped (c) the barometer would not have fallen (e_1), and it would not have rained (e_2). But *backtracking*⁴⁶ is banned: we cannot say that had the barometer not fallen it would not have rained – this is a non-causal regularity – e_1 is not a cause of e_2 because there is not counterfactual dependence between these two events. We must concede that it is possible that c occurs, e_1 does not occur, and e_2 occurs.⁴⁷ Causation as counterfactual dependence is capable of expressing causation without commitment to regularities, and it is precisely this intuition that I want to explore, as I think it is in line with the way historians make causal inferences.

⁴⁵ See Figure 1.

⁴⁶ Running from the *effect* to the *cause* or *causes*. Lewis believes that the occurrence of a putative effect is not a reason for us to conclude that had the *effect* not occurred the cause would not have occurred. This is based on the intuition that the present depends on the past, but not vice-versa.

⁴⁷ This could be the case if there is the cutting of the relation c causes e_1 , or if the relation itself is not deterministic.

1.3.4.1 Counterfactual dependence and causal dependence

It is important at this point to call attention to an important difference between *causal dependence* (a real relation between events), and *counterfactual dependence* (which involves families of propositions). So, *if* we take two different families of propositions, say A_1, A_2, \dots and C_1, C_2, \dots and all the counterfactuals (we are to read $A \rightarrow C$ as if A were not to occur then C would not occur) $A_1 \rightarrow C_1, A_2 \rightarrow C_2, \dots$ hold true, *then* we can say that C_1 and C_2 counterfactually depend on A_1 or A_2 .

It is easy to see in the classic case of the barometer how this counterfactual account holds: for each alternative reading (R_1, R_2, \dots) of the instrument, we have a corresponding air pressure (P_1, P_2), and each of the R s counterfactually depends on the P s, or the reading of the barometer depends on the pressure.⁴⁸

If we consider that for any event e , there is a corresponding proposition $O_{(e)}$ (e stands for the event, and $O_{(e)}$ stands for the proposition that e occurs) that is true for every possible world in which e occurs, then we should not have a problem saying that “counterfactual dependence among events is simply counterfactual dependence among the corresponding propositions” (Lewis, 1986b: 166). Or, the family of events e_1, e_2, \dots *causally depends* on c_1, c_2, \dots *iff* the family of propositions $O_{(e_1)}, O_{(e_2)}, \dots$ *counterfactually depends* on $O_{(c_1)}, O_{(c_2)}, \dots$ ⁴⁹ If c occurs then e occurs, and if c and e are actual events⁵⁰ (not just propositions), then e casually depends on c : if c had not been, e would never have occurred.

There are three other theses about causation in Lewis’s theory:

(1) If c causes e the converse is not true, causal dependence is not symmetrical.

⁴⁸ If that is not the case for a world w_1 , where there is no correspondence between R s and P s, we should probably consider a malfunctioning of the barometer. This is because a world in which the barometer works precisely and its measurements seem to violate a natural law is, I assume, less similar to $w_{@}$ than w_1 .

⁴⁹ In this case we have two counterfactuals which must be true: 1. $O(c) \rightarrow O(e)$ and 2. $\sim O(c) \rightarrow \sim O(e)$.

⁵⁰ In this case we have only $O(c) \rightarrow O(e)$.

(2) Causation is necessarily *transitive*: if *a* is a cause of *b*, and *b* is a cause of *c*, then *a* is a cause of *c*. But causal dependence does not have to be. Examples of causation by absences are cases in which we do not have causation but we still have causal dependence.⁵¹

(3) We may have causation without causal dependence: “let *c*, *d*, and *e* be three actual events such that *d* would not have occurred without *c* and *e* would not have occurred without *d*. Then *c* is a cause of *e* even if *e* would still have occurred (be caused otherwise) without *c*” (Lewis, 1986b: 167). This happens, for example, in cases of preemption or overdetermination (as I discuss in §1.3.4.3).

Let us now turn to the notion in terms of which causation is to be defined, i.e. causation relies on *counterfactual dependence* which in turn is cashed out in terms of *possible worlds* and their *comparative similarity*.

1.3.4.2 Comparative similarity of worlds

In the case of history, I believe that *possible worlds* stand for what historians call *alternative histories*; and *comparative similarity* is the method by which historians claim one alternative to be more *plausible* than others. One understanding of this is what philosophers consider under the heading of the degree of similarity possible world bears to the actual world. These concepts are usually not fully developed in historiography.

To understand in some detail how possible worlds offer a semantics for Lewis’s theory will help us explain, later on, how historians entertain alternative possibilities in terms of similarity to actuality, and this is used to decide on the probable truth of counterfactual historical claims. The present section will focus more on the philosophical and technical aspects of similarity of worlds. The objective is to cover those aspects of the theory which are usually ignored when it is used in different contexts, such as the case of counterfactual history (see Chapter 5).

⁵¹ I explain this in §1.3.4.5.

Lewis's definition of comparative overall similarity among possible worlds is based on what he regards as a *primitive* relation. Actually, there are two *primitives* here. One is the idea of a possible world which is not analysed further.⁵² The other is a relation of similarity between worlds; this is not analysed further either.

Comparative similarity of worlds is used to give an account of the truth conditions for counterfactuals (Lewis, 1973). The similarity comparison is usually a three place relation: a possible world w_1 is said to be *closer* to the actual world $w_{@}$ than another possible world w_2 , if w_1 *resembles* $w_{@}$ more than w_2 does.⁵³ By definition, worlds may be given a partial ordering, that is, some might be tied together as equally similar.

Now, let us consider the counterfactual if c had occurred, then e would have occurred. We may state the *truth condition* for this counterfactual in terms of similarity of worlds: $O_{(c)} \rightarrow O_{(e)}$ (where ' \rightarrow ' stands for a relation of counterfactuality between $O_{(c)}$, the proposition that c occurs, and $O_{(e)}$, the proposition that e occurs) is *true* in the actual world $w_{@}$ *iff*: (1) there are no possible c -worlds (in this case the counterfactual is only vacuously true); or (2) *some* c -world which is also an e -world is *closer* to $w_{@}$ than *any* c -world which is not an e -world (Cf. Menzies, 2009).

The basic idea, or intuition, presented by this analysis is to say that if c had occurred then e would have occurred is true "*iff* it takes less of a departure from actuality to make the consequent true along with the antecedent than it does to make the antecedent true without the consequent" (Lewis, 1986: 164). This intuition seems to fit many examples remarkably well, although there may be some counter-examples (Cf. Fine, 1975).

Let us go back to the case of Caesar. We say that the stabbing (c) is a cause of Caesar's death (e). We have a strong intuition that the counterfactual had Caesar not been stabbed he would not have died (the death he dies) is true, i.e., we believe that $\sim O_{(c)} \rightarrow \sim O_{(e)}$ is true in $w_{@}$. We may now cash out this counterfactual claim in terms of similarity of worlds by stating that

⁵² Lewis is a modal realist: possible worlds are concrete entities (just like our actual world), not spatio-temporally connected in any way.

⁵³ We compare possible worlds in the same way as we compare objects, people or cities, in terms of their differences and similarities - ordinary everyday judgements.

$\sim O_{(c)} \rightarrow \sim O_{(e)}$ is true in $w_{@}$ if there is some $\sim c$ -world which is also a $\sim e$ -world *closer* to $w_{@}$ than any $\sim c$ -world which is not a $\sim e$ -world. By similarity we may judge, for example, that a possible world in which Caesar is not stabbed and lives another twenty years is closer to actuality than, say, a possible world in which Caesar is not stabbed but still dies the same death (suppose a lot of knives miraculously fall on him).

In this example it is relatively easy to judge on the similarity relation, but other examples are trickier, and we may wish we had a clear system of priorities to guide our judgements. But can there be one? In “Counterfactual Dependence and Time’s Arrow” (1986a) Lewis says that “overall similarity (...) is (...) resultant of similarities and differences of many different kinds, and I have not said what system of weights or priorities should be used (...)” Thus, Lewis counts the ability of proceeding to such comparative analysis a ‘virtue,’ and admits that “(...) counterfactuals are both vague and various [and] different resolutions of the vagueness of overall similarity are appropriate in different contexts” (Lewis, 1986a: 41). The point here is that counterfactuals are vague, and the idea of similarity of worlds is also vague. But if similarity is used to analyse counterfactuals then they should be vague in the same ways.

The clue to our comparison of worlds in terms of their similarity lies in the implicit assumption that similar worlds share similar laws. But Lewis states that it must not be a rule that “a world that conforms perfectly to our actual laws is *ipso facto* closer to actuality than any world where those laws are violated in any way at all” (Lewis, 1986b: 163-164). It seems that the nature of the violation plays a role in matters of comparative similarity, and that it might be possible for differences other than violating the laws to require an even greater departure.

In “Counterfactuals” (1973) Lewis approach was to place all possible worlds into a system of spheres ordered according to their degree of similarity to the actual world. So, at the centre of the system we have S_x , which contains $w_{@}$ and some other very similar worlds, or the worlds that most closely resemble $w_{@}$. S_2 includes all worlds in S_x (not conversely) and a set of other worlds not so close to $w_{@}$, S_3 includes all S_2 worlds and some more worlds, and so on... Under this model “a counterfactual p is true *iff* there is a sphere S_n such that (a) S_n contains at least one antecedent satisfying world (world in which the antecedent of p is true), and (b) all antecedent satisfying worlds in S_n are also consequent satisfying worlds” (Schlossberger, 1978:

80). The suggested conclusion is that we should look only for worlds that satisfactorily resemble $w_{@}$, and ignore the others.

In his “Causation” paper (1986b), however, Lewis abandoned the centring principle and suggested that we do not need to impose that there must be a unique closest world or a set of closest worlds to the reference-world. Lewis accepted that we could talk of an infinite sequence of closer worlds but no closest.⁵⁴

In contrast to Lewis, Stalnaker (1975, 1986) suggested something different regarding the existence of a closest world. Stalnaker accepted the *Limit and the Uniqueness Assumptions* i.e. for any antecedent proposition $O_{(a)}$ there must be a possible world in which a is true that is *uniquely closest* to the actual world (*Uniqueness*); and for a given antecedent $O_{(a)}$ there must be a unique and closest set of worlds in which a is true (*Limit*). Therefore $O_{(a)} \rightarrow O_{(c)}$ is true *iff* the closest a -world is also a c -world.⁵⁵ Lewis, however, rejected this proposal and suggested that it would be quite acceptable to think of an infinite series of worlds, and in each of them my coffee cup would be slightly to the left of its position, and at the same time none of these worlds would be the *closest*.

The notion of comparative similarity is primitive, and there is no clear method for the comparison of worlds on their similarity. But counterfactual dependence might cast some light on the way we understand causation as a real relation in the world, involving two different events, and this (dependence) relation may be cashed out in terms of possible worlds.

I have already stated it, but it is important to highlight it once more. We are dealing with two separate notions: truth conditions of counterfactuals and comparative degree of overall similarity of possible worlds. Similarity as a relation between worlds is a vague concept. Equally problematic is to discover what the truth conditions for counterfactuals are. What Lewis does here is to reduce these two notions to just one, which I believe to be an advance.

⁵⁴ Worlds can differ in only small details.

⁵⁵ Stalnaker is an actualist, i.e. he believes possible worlds are just *possible* states of the world (properties the world *might have had*). There is only one concrete world, because just one property (among the many properties the world might have had) is instantiated. The many non-instantiated properties are these possible worlds. Cf. Stalnaker (1986: 7, 27–28, 54).

The thesis is that when two alternative counterfactuals compete, we tend to pick the one that is more *similar* to the actual world (or any world we are using as the reference world).⁵⁶

1.3.4.3 Fragility

The most important challenges to Lewis's definition of causation as counterfactual dependence are cases of *redundant causation* (divided into cases of late and early *preemption* and *symmetrical overdetermination*). Because of these, Lewis had to reformulate his original theory of causation. The present reformulation is not just a way around such challenges, but a redefinition of what causation *is*, namely *patterns of causal influence* (see §1.3.4.4).

Let us see in detail what redundancy is, why it is a problem, and how *fragility* may be a solution for it. Lewis defined redundant causation as follows:

Suppose we have two⁵⁷ events c_1 and c_2 , and another event e distinct from both of them; and in actuality all three occur; and if either one of c_1 and c_2 had occurred without the other, then also e would have occurred; but if neither c_1 nor c_2 had occurred, then e would not have occurred. Then I shall say that c_1 and c_2 are *redundant* causes of e (Lewis, 1986: 193).

So cases of redundancy go against the definition of causal dependence of e on c_1 , because the *same* effect e , would have been caused by some redundant cause, let's say c_2 . It seems to be a case in which we have a causal relation, but not *dependency*. This seems to be a challenge for Lewis's original formulation of the theory, as causation *requires* dependency. But it is also not always straightforward to decide whether we have a case of redundant causation or not. Let us try to deal with some simple situations.

Consider the case of an event e having two causes, c_1 and c_2 . Is it redundant causation to shoot a terminally ill patient very close to death? Common-sense intuition would, I assume, require us to say no, it is not redundant causation. Why? Because although the patient would have died one way or the other, the death the patient dies if I shoot him/her is a *different* death

⁵⁶ However, it must be clear that the similarity is just a relation between classes of possible worlds, and not a property of the counterfactual.

⁵⁷ 'Distinct' events is preferable.

from the one s/he will die should I not shoot him/her – different in time, manner and maybe space too.

Consider a different case. Is shooting a man who is being simultaneously shot by seven other gunmen a case of redundant causation? In this case common-sense might argue that it seems so: “The exact number of bullets through the heart matters little” (Lewis, 1986b:194).

These examples suggest that the big question when it comes to redundant causation, c_1 and c_2 causing e , is to answer the question ‘how much difference does it take to call the effect e a different event?’ Take the case of the man being simultaneously shot. Let us say that you kill him shooting a single bullet through the heart on Tuesday morning (c_1 causes e). Without your act he would have been killed on Wednesday, with a similar wound passing through his heart (c_2 acts as a backup cause of e). “Is it that without your act he would have died a different death – numerically different because somewhat different in time and manner?” (Lewis, 1986b: 195).

The answer seems to depend, in some degree, on our considerations about the *relative fragility* of the *identity* of an event. If we take the event e as very fragile in its identity, this would mean that any difference (to its causal history, time, manner, etc.), even if just a slight alteration, would imply a different death, and therefore a different event e . In this case there is no redundancy, because the death the man actually dies (e) depends only on one cause (c_1). This leads us to the intuition that some account of the fragility of an event’s identity seems to be the right answer to cases of redundant causation.

But the idea of *fragility* needs some important refinements. At first glance one might want to say that the event (e) is the ‘product’ of its causes; e could not have occurred had any aspect of its causal history been any different. Had some alteration, even minimal, been introduced, we would not be talking about the occurrence of e , but some alternative outcome e_1 . In other words, e_1 is very like e , but they are distinct.

But is accepting extreme standards of fragility a solution to the problem? Lewis believes this is not the best approach. His answer is in part a reply to Peter van Inwagen’s paper *Ability and Responsibility*, in which the author claims that “ x is the same event as y iff x and y have the same causes” (van Inwagen, 1978: 208). The criterion for individuation of events is what van Inwagen calls causal-genesis: “substances (...) should be individuated by their causal

origins, and since we are talking about events that, like substances, are particulars, the present proposal is plausible” (1978: 209).

The motivation for van Inwagen’s account is clearly the fact that the notion of one event being the ‘same event’ is not clear at all. As van Inwagen says: “if Cleopatra had poisoned Caesar in 48 [BC], then, clearly, there would have happened an event that did not in fact happen, an event that it would have been correct to call Caesar’s death” (*Loc. cit.*) This case shows that ‘Caesar’s death’ would, under different causal circumstances, have been a different event from the assassination that actually occurred.

If we accept that this event is very fragile in its identity, then any minor alterations, say Brutus not taking part of the killing plot, would imply Caesar dying a different death. According to van Inwagen, the actual event e can only be caused by a *certain* set of causes..., and the result of this very same set could not result in anything different from e . Still according to van Inwagen, this approach seems plausible because we individuate substances according to their causal history. Had anything been different in the causal history of e , then e would not be identified as e anymore, but as a different event.

According to van Inwagen’s view, the identity of e depends on the occurrence of c_1 – the existence of a ‘backup’ cause (c_2) is irrelevant – the chain leading from c_2 to e is never completed (say c_2 ’s occurrence is *preempted* by c_1 ’s occurrence), so only c_1 is a cause of e (no redundancy). We cannot say ‘had c_1 not occurred c_2 would still cause e ,’ because that e depends on c_1 occurring and c_2 not occurring; e can only be caused by c_1 and cannot have any other causes.

Clearly, van Inwagen’s thesis about the individuation of events according to their causal history is something Lewis wishes to deny. The motivation for the rejection is that, for Lewis, it is central to accept that the ‘same’ event can be caused by different causes (Cf. Lewis, 1986b, footnote 20: 195). Adopting extreme standards of fragility for the identity of an event may help us to get rid of redundancy, but the price to pay is high: the counterfactual analysis of c causes e is just trivially correct. The usefulness of adopting standards of extreme fragility is to clarify how c_1 is a cause of e and c_2 is not. But this strategy needs refinement, as the following cases will illustrate.

Let us go back to Caesar's death again. Assume that just the first stab to Caesar's chest was lethal, and the success of this first attack preempts⁵⁸ all other assailants' stabs as causes for Caesar's death. But suppose that the attack of the others assailants (the non-lethal stabbers) was especially noisy and frightening, causing Caesar to twitch, allowing the first dagger to perforate his heart. Is it not true that without the noisy, non-lethal assailants, Caesar's first stab to the chest would not have been lethal (or not *exactly* lethal as the stab that actually happened)? So, the lethal assailant owes his success to the existence of the preempted redundant causes? Are the first assailant and the others *jointly* causing Caesar's death? If so, then instead of disregarding c_2 as a cause of e , we are now talking about joint causation, which means we have not got rid of redundancy, despite considering the event itself very fragile. Still, common-sense tells us that it was the first assailant who caused Caesar's death, and not the others.

Let us consider a similar example inspired by one of Lewis's. Imagine that a firing squad of seven soldiers is about to execute a prisoner. But one of the soldiers, a very gentle person, decides not to shoot, and the prisoner dies by the bullets of the other six gunmen. So, under extreme standards of fragility, the man's death depends not only on the firing of the cruel soldiers, but also on the gentle soldier's omission – had he fired, the prisoner's death would have been different (seven bullets pierce the prisoner's heart, instead of only six).

Under extreme standards of fragility of an event's identity we would have to include the nice soldier's omission as a cause of the death that actually occurs, because had the gentle soldier fired, the 'same' death would not have occurred, but a different death instead. But common-sense tells us that the gentle soldier is not responsible for the prisoner's death. How could he be? He did not shoot the prisoner. We want to say that the nice soldier is responsible for its being true of the prisoner's death that he (the soldier) did not causally contribute to it, and this is not true of any of the obedient soldiers of the firing squad. Counting the gentle soldier's *omission* as a cause of the death being the death it was may, for some, seem a strange inclusion.

Other problematic cases for extreme fragility have to do with 'delayers.' For an extremely fragile event the exact time of its occurrence is an important identity element.

⁵⁸ In the sense that it allegedly 'cuts' the other causal chains.

Caesar dying at 3:00:01 pm, and not 3:00:00 would mean Caesar dying a different death altogether. Now imagine that right after the deadly stab in the chest Caesar was rescued by a doctor who diminished the bleeding, and therefore postponed Caesar's death by, say, a couple of minutes. Under extreme fragility this is equivalent to saying that the doctor's aid is a cause of Caesar's death being the death it was – had the doctor not attempted to stop the bleeding the event in question would have been a different one. Do we want to consider delayers as causes? Is it the case that the doctor's aid is a cause of Caesar's death? Common sense tells us this seems strange.⁵⁹ We want to say that the doctor's aid slightly modified the time of death, or the conditions Caesar died (assisted by a physician); but we do not want to say that the doctor caused his death.

Even if the standards of fragility are not extreme, but moderate, the individuation conditions for events remain vague. Imagine someone claims: 'the performance should have been postponed until the singer was over his laryngitis; then it would have been better.' Is the counterfactually postponed performance the same performance? Or is it a different event? How fragile is the identity of the performance? It is not clear. Lewis's initial solution to this vagueness was to say that we do not actually need a definitive answer to cases of fragility,⁶⁰ but a proper way of approaching the problem, or having fragility 'in right doses.'

Lewis's treatment of redundant causation in his "Postscripts to Causation" (1986b: 172-213) was not entirely satisfactory. Although Lewis pointed out a viable solution to some cases (*early preemption* and *symmetrical overdetermination*),⁶¹ he failed to provide a solution to all cases of redundancy (in particular cases of *late preemption*⁶² and *trumping*⁶³). The suggested solution for all

⁵⁹ This raises the question whether Lewis should make any distinction between causes and conditions.

⁶⁰ Lewis (2004) adds that in some cases we might be in the 'mood' for considering far-fetched alterations, and in some cases we might be in the 'mood' for ignoring them. There is no definitive answer, but one must pay attention to sustain the same 'mood' in all parts of his argument, not 'alternating' between severe fragility to robustness.

⁶¹ Lewis used the framework presented by Martin Bunzl (1979: 134-150).

⁶² When the occurrence of the effect (*e*) caused by a cause (*c*₁), cuts the causal chain leading from a secondary cause (*c*₂). For example: two assassins shoot at the same target. The first of the bullets to arrive instantly kills the victim, pre-empting the causal link originating from the second shooter.

⁶³ See the example given in §1.3.4.4.

cases of redundancy, which he called *quasi-dependence*, was bad (as Lewis later admitted)⁶⁴ so there is no reason to spend any time discussing it.

A viable solution for all cases of redundancy seems to be having a ‘double standard’ of fragility: stringent when showing that an effect depends on the alleged redundant causes, but more lenient when trying to say (in accordance to common-sense judgements) that an effect cannot be caused by something that only slightly affects the time and/or manner of the happening. The idea of such a double standard has been abandoned at the time of Lewis’s *Postscripts* (1986b), and would only return much later, modified into the account of *patterns of influence* (2000 and 2004).

For clarity and illustration, let us consider a case of late preemption: we have two individuals, Billy and Suzy, throwing rocks at a bottle. Suzy’s rock arrives first and shatters the bottle, and the shattering itself (the final effect) preempts Billy’s rock; when Billy’s rock arrives the bottle is not there anymore. However, had something prevented Suzy’s rock arriving on the target, then the bottle would still have been shattered by Billy’s rock. Under extreme standards of fragility the solution for this case is to call Billy’s shattering of the bottle a different event, so that what actually happened is counterfactually dependent on Suzy’s rock.

The difference to the shattering, regarding ‘when-where-how’ is minimal; do we really want to call it a different event? Lewis would say no: “the bottle would have shattered at very nearly the same time (...) in very nearly the same way that it actually did. Yet we’re (...) happy to say that an event might have been slightly delayed and that it might have differed somewhat in this or that one of its contingent aspects” (Lewis, 2004: 86). We may now ask: “how much delay or change (or hastening) do we think it takes to replace an event by an altogether different event, and not just by a different version of the same event?” (*Loc. cit.*).

The difficulty in providing a clear-cut answer reveals that *fragility* may be a problem for Lewis’s account of causation. But it is only a problem if we continue to focus on ‘whether-whether’ counterfactual dependence, i.e., whether *e* happened at all depended on whether *c* happened. The solution seems to be to admit that other *kinds* (or *modes*) of counterfactual dependence exist too, like ‘when-on-whether’ dependence. This is the same as saying that

⁶⁴ Cf. Lewis (2000: 185).

“Suzy’s throw caused the shattering of the bottle in virtue of ‘when-on-whether’ dependence. When the shattering occurred depended on Suzy’s throw” (Lewis, 2004: 87). So, if Suzy’s rock had not reached the bottle, the delay between her rock and Billy’s would slightly change ‘when’ it actually happened; therefore it is clear that the exact time it took place is counterfactually dependant on Suzy’s rock. So, had c been different, ‘something’ (time, space, manner) in e would have been different too.

In order to have fragility in ‘right doses’ we do not need to suppose the final event to be extremely fragile, we just need to consider an *alteration* to ‘when,’ or ‘where,’ or ‘how’ an effect occurred. The effect still counterfactually depends on the cause, because had it not occurred (or been slightly different) then the final result would have been slightly different in *at least one* of these modalities, space, time or manner.

To properly refer to fragility we need to consider that the final effect is the ‘same’ event but with different features; this is the only way we can talk of one event being caused by different causes. The most important element of this later paper is the notion of *alteration*, and alterations are always fragile. In Lewis’s words: “Suzy’s throw caused the shattering of the bottle and Billy’s preempted throw did not because, without Suzy’s throw, the alteration of the shattering which actually did occur would not have occurred, and a different alteration would have occurred instead” (Lewis, 2004: 88). Fragility corrected seems to solve the problem of late preemption.

1.3.4.4 *Patterns of causal influence*

For Lewis any event which is part of a causal history is a cause of an event which occurs further down the causal chain (remember that causation is transitive). According to the notion of alterations of an event, if an earlier event c influences the time and/or manner in which an event e occurs, then c is a cause of effect e being the effect it is. Any element of a causal chain is a cause, even if common-sense would only call it, say, a delayer.

Consider an example similar to the case of Caesar’s doctor: if a man eats poisoned chocolates after having dinner, then the full stomach delays the action of the poison and the time

of the man's death. So having dinner is part of the causal chain leading from the poisoning to the death (even if it's just a delayer) and it is important, among other things, to determine, for instance, the time of the death (and possibly the space and manner too). Therefore having dinner has to count as a cause of death, because had the victim not had dinner, his death would have been a different death. It seems that having dinner makes a causal contribution to the actual properties of the death.

Among other causes we intuitively want to disregard are many preempted alternatives, like Billy's rock: "by the law of universal gravitation, a distant planet makes some minute difference to the trajectory of Suzy's rock, thereby making a tiny difference to the shattering of the bottle" (Lewis, 2004: 89). So, is this tiny difference tiny enough for us to disregard it as a cause? In most cases we want to say yes, since alterations on the final effect are virtually unnoticeable.

The previous example illustrates an interesting challenge; if we grant that preempted causes count as a relevant part of the causal chain, then they are *jointly* causing it. So even absences, like not having nerve-gas around Billy and Suzy (which would kill them before the throwing of the rocks) have to count as causes for the shattering of the bottle.

Lewis's solution to the problem of spurious causes is pragmatic.⁶⁵ He just says that it is true that conceptually such spurious things have to count as causes, but are they relevant? Cannot we find a huge asymmetry between Suzy's and Billy's rock as causes of the shattering? In Lewis's words: "even if Billy's rock makes a minute difference to the shattering (...), yet Suzy's throw may make much more of a difference to the effect than Billy's." It is true to say that "the alteration that would have occurred without Suzy's throw (...) may differ (...) in time and manner much more than the alteration that would have occurred without Billy's" and this would cause a break of the "symmetry between Suzy and Billy, and to account for our judgment that Suzy's throw and not Billy's causes the shattering" (Lewis, 2004: 89-90). So if Billy's rock is not relevant, then it is not a joint cause of the effect. The attribution of causal status is made

⁶⁵ See also §1.3.4.6.

by the observer.⁶⁶ If the observer wants to consider spurious causes as joint causes, that is fine. If the observer wants to disregard them (because they are not important for his purposes) that is fine too – a *pragmatic* solution.

Until now we have been discussing alterations of the final effect, and in this case it has not been necessary to draw a distinction between alterations as different versions of the very same event, and alterations as different but very similar events. This distinction might be important however, if we want to talk about *alterations of a cause*. There must be a distinction between saying that *c* does not occur at all, and *c* does occur but differently in time and manner. In this case is *c* replaced by some ‘not-quite but very similar to *c*’-event? Or is it “just barely over the border that divides versions of *C* itself from its nearest alternatives?” (Lewis, 2004: 90).

What if we ask about the closest way to the actual worlds for *c* not to happen? Say that *c* causes *e*, and *c* is fairly fragile. So instead of *c* we have almost-*c* occurring, but as almost-*c* is very similar to *c*, its effects are likely to be pretty much the same as the effects of *c*, i.e. almost-*c* causes *e* or something quite similar to *e*. Lewis does not want to grant that there is a closest to actuality version of *c*, or a set of closest possible alterations. This proposal has to be widened.

Let us grant that we do not have only one alteration, but a range of them. I previously said we need a distinction between an alternative event and a similar version of the same event. That was not quite true, if we take it that such alterations form a pattern of counterfactual dependence, linking alterations of the effect with alterations of the cause. In this case the point at which a version becomes an alternative is not a significant matter.⁶⁷ In Lewis’s words, this pattern represents how alterations of the cause influence the final effect, and this actually leads to a new definition of causation:

Where *C* and *E* are distinct actual events, let us say that *C* influences *E* *iff* there is a substantial range *C*₁, *C*₂, ... of different not-too-distant alterations of *C* (including the actual alteration of *C*) and there is a range *E*₁, *E*₂, ... of alterations of *E*, at least some of which differ, such that if *C*₁ had occurred, *E*₁ would have occurred, and if *C*₂ had occurred, *E*₂ would have occurred, and so on. Thus we have a *pattern of counterfactual*

⁶⁶ Menzies (2004) suggests that we need to rehabilitate the distinction between *condition* of occurrence and *cause* as a *difference-maker*, and depict them as *context sensitive* (modality).

⁶⁷ So there must be a ‘point’ in which a version becomes a different but very similar event, but this is impossible to determine and is not relevant for our purposes.

dependence of whether, when, and how on whether, when, and how (...) C causes E iff there is a chain of stepwise influence from C to E (2000: 190).

Under this amendment we are in good position to say that there are different *degrees of influence*. Going back once more to the case of Suzy and Billy (§1.3.4.3), we can say that Suzy's rock is a cause of the shattering of the bottle (and Billy's rock is not) because any alteration of her act (let's say that Suzy's rock has a different speed, shape, endurance, and so on) would be followed by a sensible alteration of the final effect. In the case of Billy, however, alterations to his throw (holding fixed Suzy's throwing of the rock, and holding fixed that Suzy's rock arrives first) do not represent a huge difference to the shattering of the bottle.

Another good definition of influence, illustrated by Suzy and Billy, is the idea that a causal process entails transmitting a mark. We can think, for instance, of processes in which alterations of later stages will counterfactually depend on alterations of the initial stages. The solution seems to fit well in different challenging cases, and apparently solves most cases of redundancy.

*Trumping preemption*⁶⁸ represented a different challenge to Lewis's theory. Take the example of a major and a sergeant shouting orders to the soldiers. Let's imagine that both shout 'advance.' This should be easy for the soldiers to follow, since both, sergeant and major, are in the position of giving them orders, and both agree on what has to be done. Suppose that the sergeant shouts 'advance' and the major orders 'stop.' In this case, as the soldiers recognise hierarchy, they obey the major. The major's orders perform a trumping of the sergeant's ones. In the case in which both, major and sergeant, shout 'advance', it is the major who causes the soldiers to advance (trumping alternative), and the sergeant acts as a 'backup' commander (trumped alternative). Allegedly, this is not a case of 'cutting' of a causal chain, but it is a case of redundancy. We may solve this problem using our new concept of causation as influence, as in the case of Billy and Suzy, altering the major's commands would influence the final effect at a *much higher degree* (whether, when, how...) than altering the sergeant's orders while holding the major's fixed would do. In other words, there is an important *pattern of causal influence* between

⁶⁸ Although Shaffer (2000) introduces *trumping* as a special case of redundancy, Bernstein (2014) argues that cases of trumping are not a special case of redundancy, but fall into the categories of *asymmetrical overdetermination*, or *early preemption*.

the major's orders with the soldier's behaviour. Not so in the case of the sergeant. The solution seems to fit well in this case too.

It seems, so far, that correcting fragility and talking about causation not as a simple relation, but as a *pattern of influence*, help us dealing with some of the challenges to Lewis's initial theory. At least two problems apparently remain to be discussed: causation by *absences* and *spurious causes*.

1.3.4.5 Absences as causes

In our ordinary talk it is normal to make reference to absences as causes. We may want to say for instance that a train not arriving at the station – the absence of the train – causes a waiting passenger to catch a bus. But does it follow that an absence is an event? That conclusion does not seem right, as absences do not have properties. Absences are non-events.

So, if absences do not have real properties, they do not take part in any theory of events and according to Lewis they are literally 'nothing.' In other words, absences cannot be proper causes, since causation is a relation, or *pattern of influence*, between two distinct events. But it is important to remember that counterfactual dependence happens between *propositions* concerning events, and not events. Absences are non-events, but *propositions* about absences are not meaningless: the proposition that an absence occurs is a meaningful negative existential proposition, and by way of these propositions an absence may have a place in terms of counterfactual dependence.⁶⁹

Absences play an important role in many of our counterfactual claims, and they act sometimes *as if* they were causes, and sometimes *as if* they were effects. According to Lewis, there is no reason to be afraid of saying so, since it falls within our linguistic practices: "we distinguish between the cause itself and the true proposition that describes it" (Lewis, 2004: 100).

⁶⁹ See the distinction made in §1.3.4.1.

If we accept absences as causes and effects in our counterfactual claims, then we shall have cases in which we have counterfactual dependence but not causation (absences are nothing in the ‘real’ world). In many situations the simple event/proposition distinction seems to clarify in which sense absences can be regarded as causes.⁷⁰ But there is a question about what the *truthmakers* are for true propositions about non-events, and for true claims about what causally depends on their not occurring.

1.3.4.6 *No need for a causal field*

In §1.3.3 I presented Mackie’s introduction of a causal field into his conditional theory of causation. The basic function of such an introduction was to differentiate between causes and conditions (and between sufficiency and necessity). It was argued that this concept had the advantage of highlighting the conditional aspect of causation, and at the same time it enables us to perceive a cause as the difference-maker, or the ‘abnormal’ factor within a system.

Peter Menzies (2004) has argued that Lewis’s definition of causation is in the line with Mackie’s in thinking of causes as *difference-makers*, and yet Lewis makes no distinction between causes and conditions. Menzies believes that the idea of a *causal field* should also be introduced into the theory of *causal influence* as a way to do away with cases of *spurious causation*, especially in the case of absences as causes. It may prove useful to say a bit more about this.

For Lewis, any event which is part of a causal history of an effect is a cause of this effect. This also includes absences, hasteners and delayers. For Lewis, we may rightly call any of these causes, because a cause is something that makes *any* difference to the effect. My mother giving birth to me three and a half decades ago is a cause of my writing this chapter. In the same way the absence of poisonous gas in the PhD room (at least during the time I was in it) is a cause of my breathing and typing. Finally, the Big Bang is also a cause of my current studies. These are all examples of spurious causation: although all these events are indeed causes, we usually do not want them to appear in our *causal explanations* because they are deemed obvious and

⁷⁰ Cases of preemptive preemption (preemptive cause of an absence) are still puzzling, as we seem to have causation without direct causal influence. See McDermott (1995: 525) and Lewis (2004: 102).

uninformative. What defines a cause as *spurious* is, I believe, simply our unwillingness to include it as part of a causal explanation.

Menzies argues that to think of a cause as a *difference-maker* implies contrasting it with a set of background conditions (causal field), as was the case in Mackie's theory. This is not something Lewis contemplates. According to Menzies, Lewis is "on the right track (...) in saying that we think of a cause as something that makes a difference and that this thought is best explicated in terms of counterfactual concepts" (Menzies, 2004: 144). But Menzies adds that "the particular way in which Lewis spells out the concept of a cause as difference-maker is unsatisfactory" (*Loc. cit.*). And this, Menzies thinks, is because "Lewis's theory is insensitive to the different context-relative ways in which common sense draws the distinction between causes and conditions. His theory treats mere conditions as causes because they are factors without which the effect would not have taken place" (*Loc. cit.*).

Lewis does not make the causes/conditions distinction because he accepts that causation is "an absolute relation, specifiable independently of any contextual factors" and therefore it is true that his theory "generates countless causes for any given effect" (Menzies, 2004: 139). But why is this problematic?

Think of the causal claim: 'Caesar died because his mother gave him birth.' According to Menzies, common-sense deems such explanation inept. We may draw a distinction between a condition and a cause (difference-maker) on the grounds that being alive is *always* a necessary condition for death.⁷¹ Not that philosophical enquiry should always endorse what common sense has to say, but it is true that an agreement between the theory and common sense is desirable. In our case, common sense advocates that it is absurd to say that Caesar's mom caused his death.

One way of dealing with this problem is indeed in terms of specifying a causal field – identifying the environment or specific circumstances in which a causal sequence occurs. Caesar's mom is then just a 'background condition', and not a difference-maker to his death. So, taking this *background* into account we have to look for something else as a cause, something that really made a difference to *how* Caesar died.

⁷¹ It could be argued that since we are all by nature mortals, it is also a sufficient condition.

But this is not the only solution. I believe the introduction of a causal field unnecessarily complicates Lewis's theory of what causation is.

It is probably a good idea to remember that Lewis's analysis has the sole purpose of saying what causation *is*, and not how to identify, in context, what are causes (difference-makers) and what are (pre-existing) necessary conditions. Lewis's theory is not unsatisfactory: it clearly states that causation is a special relation between events expressed in the form of counterfactual dependence. From an ontological perspective, all events of a causal chain are causes, even hasteners and delayers, because they make a difference (even if minor) to the effect. Either the effect would not have happened had any of the causes (spurious or not) been absent, or the same event would have happened *differently* to some extent (all events are modally fragile to some degree).

Why are cases of spurious causation problematic for the definition of causation? Well, they are not. They are only problematic for a theory of *causal explanation* which is unable to discard spurious causes. This is certainly not the case with Lewis's theory of causal explanation. For Lewis a *causal explanation* is the provision of information on the causal history of an event. We may well focus on the parts of the causal history which are relevant for our inquiries. If we ask why Caesar died *as he did*, do we have to trace back the causes all the way back to the Big Bang in order to give an acceptable explanation? Or is it sufficient to focus, say, on causes which are immediately or more closely related to *when-where-how* the effect occurred?

Our criteria for which part of the causal history to pick may not be entirely clear,⁷² but we may say that some causes influence the effect more highly than others. It may be argued that such causes should be given more attention when providing an explanation. We do not need to introduce the notion of causal field to understand that some events – when part of a causal explanation – are *deemed* to be more causally relevant than others.⁷³ The idea of a 'causal field' may well be replaced by making clear what the context of enquiry is. Causation as *patterns of*

⁷² And once more this raises questions regarding objectivity in the causal selection.

⁷³ In Chapter 5, I deal with the attribution of causal weight to historical antecedents.

causal influence implies that different causes have different causal impacts, and we may pragmatically choose which ones need to be highlighted according to the context of enquiry.⁷⁴

1.4 Final remarks: historians and counterfactuals

As we have seen, Lewis's view of causation only requires a relation of counterfactual dependence between propositions about events: if we say that *c* is a cause of *e*, we claim that *e* would not have occurred had *c* been absent – a singular view of causation. We rejected the *Humean* RT of causation on five grounds (§1.3.1), in addition to the case of epiphenomenalism. We also discussed that the RT fails to be reductivist because it cannot explain how events are to be organised into types without appealing to causation. Does Lewis's theory do better than the RT?

I believe it does. None of the criticisms we have seen in §1.3.1 applies to Lewis's theory. Epiphenomenalism is not a problem as we clearly do not establish causal links only based on the fact that an event *always* (or usually) precedes another. Causation *is* successfully reduced to a relation of counterfactual dependence once we use *fragility* to solve the problem of redundant causation. Causation as counterfactual dependence is not counterintuitive; it does not require generalisations; events need not be organised into types; our idea of causation is not said to be the consequence of exposure to constant conjunction.

Lewis's theory is also perfectly viable either in deterministic or indeterministic (probabilistic) worlds. I highlighted the most common challenges (redundant causation, absences as causes, and spurious causation) and argued that Lewis has satisfactorily replied to them. Lewis's later refinements of the theory – causation as patterns of causal influence – can handle those problems well.

But there are still some challenges to the theory. Even if I do concede that Lewis's account escapes much of the criticism imposed on the RT, it is still unclear how we may

⁷⁴ For an alternative view see John Elster (1978: 175-220).

discover the truth conditions of counterfactuals and how these are satisfied. This is because the *comparative similarity of worlds* (§1.3.4.2) is widely regarded as a vague and imprecise notion.⁷⁵

But even if vague, it is still possible to see the model being ‘put to work’ in the case of historiography; when two or more counterfactuals compete, historians would tend to ‘choose’ the one which requires ‘less of a departure’ from actuality to make the antecedent true. Intuitively, historians believe to be in a position to discern and ‘judge’ in terms of similarity.

Consider the counterfactual claim ‘had Franz Ferdinand not been shot the Great War would not have occurred.’ It is relatively easy to imagine that any detail of the plan to assassinate the Archduke in Sarajevo could have gone wrong (a change of schedule, a jamming gun...). If we accept that the introduction of such an *alteration* (a failed assassination attempt) is enough to produce an entirely different effect; then it seem reasonable to consider a world in which the Great War does not occur in consequence of Ferdinand’s survival as *resembling* actuality to an important degree. What is relevant here is whether there is such a world that resembles the real world more closely than any world in which Ferdinand is not shot and the Great War does occur.⁷⁶

Tucker (2011) highlights two remaining difficulties for the adoption of counterfactual theory into historiography.

Firstly, there is the problem of *objectivity*. Tucker says that sometimes “competing counterfactuals may claim greater similarity to the actual world in some fashion or another; since what we consider similar depends very much on contexts and pragmatic considerations” (2011: 103). This is especially so because the supporting evidence at hand may differ depending on our (subjective) pragmatic choices. So, we may have two competing candidates, and not be

⁷⁵ G. Lee Bowie argued that not only is it vague and imprecise, but that no plausible understanding of comparative similarity will give us as true *only* those ordinary counterfactuals which are in fact true: “while the similarity approach generates a successful logic, it does not generate a successful analysis” (1979: 478). This is an important issue, no doubt. But maybe it is possible to make the theory more stringent when necessary, in order to rule out (most) false counterfactuals. The fact that ‘all’ ordinary true counterfactuals can be adequately represented by means of semantic of possible worlds certainly speaks in favour of the theory.

⁷⁶ For example, if Ferdinand had not been assassinated, some other seemingly minor event would have triggered a great war.

in position to say which one requires less of a departure from actuality. If so, we have to be content in saying that, until we identify further supporting evidence, both alternatives are similarly *plausible* (or similarly *implausible*). I do not see it as an important objection to the applications of the counterfactual theory of causation in history.

Secondly, there is a strong tradition in historiography to consider that history is only about what *actually* occurs. Still, some historians with a focus on military history, for example, appeal to counterfactual claims and support them, whenever possible, with empirical evidence.⁷⁷

Problems in ‘counterfactual history’ include contemplation of how close to actuality some alternative worlds are. Counterfactual historians use counterfactuals to “substitute or change causes associated with significant points in history and hypothesise alternate [sic] outcomes” (Brien, 2013: 76). In addition it is believed that “the actual course of past events can be better appreciated if *alternative realities* are considered” (*Loc. cit.*).⁷⁸ Tucker believes that historians are sometimes capable of justifying counterfactual inferences based on evidence, but “the evidential aspect of counterfactual justification is broader than Lewis’s account [thus we have a] gap between the ability of historians to justify their causal assertions, and their ability to justify counterfactuals, in favour of the first” (2011: 103). Tucker concludes that perhaps historical causal claims are epistemically irreducible to counterfactuals, and that the natural complexity of historical systems, combined with Lewis “disregard for the background conditions of causes”⁷⁹ make the case very difficult for a “subjunctive conditional [counterfactual] reduction of causation” (*Loc. cit.*).

⁷⁷ Sometimes historians use our knowledge of a political leader’s ideals to explain how things would have been had an alternative leader replaced Churchill, Stalin, or Hitler. For example, if instead of Churchill we had a strong pacifist leader, there is a greater possibility that England would have remained neutral during WWII.

⁷⁸ Many mainstream historians would claim thought experiments to be an uninformative waste of time. We will address the issue of historical counterfactuals in Chapter 5.

⁷⁹ I argued that Lewis’s model does not require talk of *background conditions*, and that any antecedent condition may be a relevant cause, depending on the context of enquiry.

What Tucker seems to say is that counterfactual claims in history are harder to justify than ‘ordinary’ (indicative conditional reduction) causal claims.⁸⁰ Tucker’s position is that the epistemology of our knowledge of the past inquiries into what we can know about causes in historiography. Tucker insists that we evaluate historical counterfactuals on the basis of evidence, as we do with the evaluation of factual claims. But there is a significant gap between evaluating counterfactuals and factual claims, in the sense that for the former evidence is usually scarce.⁸¹ This *evidential* aspect of the evaluation of counterfactuals is not an important part of Lewis’s position, in the sense that Lewis’s project is entirely metaphysical.

Lewis has defined causation *in terms of* counterfactual dependence. For this purpose there is no difference between ‘ordinary’ causal claims, and claims of counterfactual dependence. When historians say that the Great Depression caused the rise of Nazi-fascism in Europe, there is an implicit counterfactual claim that without the Great Depression the Nazi-fascism would (probably) not have occurred.

There seems to be a wrong assumption that if historians embrace causation as counterfactual dependence they become counterfactual historians, i.e., scholars who entertain alternative possibilities for the actual course of events, and try to say which of the possible outcomes is the most plausible. So if the Great Depression had not occurred, Nazi-fascism would not have existed, but *X* would have occurred instead. This need not be the case; to embrace causation as counterfactual dependence is not the same as pursuing counterfactual history.

Again, if *c* causes *e*, then had *c* not occurred, *e* would not have occurred is *only* the claim that the occurrence of *e* is dependent on the occurrence of *c*. It is probably true that some of our causal inferences may be false, that some other causal relations remain unknown; these are epistemic problems. Lewis offers an ontological thesis of what causation *is*, and whereby all causal claims may be, in theory, reducible to relations of counterfactual dependence.⁸² This kind

⁸⁰ An indicative conditional is the logical operation given by a statement such as if *A* then *B*. Indicative conditionals face some important challenges as well. Cf. Stalnaker (1975: 269-70).

⁸¹ See Tucker (2004: 208-53).

⁸² Ontologically and possibly also epistemically, considering we are in a position to identify the causal relation.

of relation between particular events captures well the meaning of causation historians usually employ.

In this chapter my task was to identify the most suitable account of causation for the case of history, a discipline which aims at providing causal explanations (usually in a plurality of ways) but that struggles to come to terms with the meaning of it. I presented three different theories (RT, *INUS* conditions, and counterfactual dependence) and compared them in terms of their capacity of withstanding criticism, and adequacy to historiographical language.

Lewis's theory is not problem-free, and I showed some of the common challenges. But it offers historians a model of causation which seems to fit quite well the kind of causal relations we identify in historical explanations: singular causal claims without reference to generalisations or laws. For this reason alone it becomes a key notion for some of the problems in philosophy of history I engage with in the following chapters. It helps illustrate, for example, why the *accidental* (non-deterministic) view of history is not the thesis that some events are *uncaused*, as we shall see next.

Chapter 2

The Notion of Chance in History

2. Historical 'chance'

Chance is one of those notions we are confronted with on a daily basis – we talk about it as having good or bad luck, or the result of the unexpected, or when we assign a probability for an event to occur, etc. However, the meaning of chance is far from clear; the notion has been defined several times over the last centuries, and has become rather complex and even ambiguous. In the case of history, chance is sometimes used with reference to the view that historical events are not determined, are the result of accidents, have unknown causes, or are even the outcome of human free will (the thesis defended by Berlin, 2002).

Given the multiplicity of senses for chance, any study of 'the chance worldview' needs to make clear what is meant by it. In this chapter I will present, illustrate and comment on ten different notions of chance (§2.1.1 to §2.1.10). These different definitions will be a guide for all future references throughout this thesis, to the accidental or 'chancy' approach to history.

As determinism and accidentality have also been subject to a variety of definitions and approaches, the cross-reference between accidentality, chance, and indeterminism has far-reaching implications, and must be the object of philosophical scrutiny. Such scrutiny is an important step towards bringing some clarity to important confusions in the dispute between Berlin and Carr with regards to the role of *chance* as an explanatory element for historical developments (§2.2 and §2.2.1).

Carr defended the view that accidents and chance do occur, but are rather irrelevant, and that historians should look for proper ‘causes’ in the uniformities or regularities or patterns of history. Although Carr is influenced by Marxist historiographical tradition, his position is more radical than, for example, Trotsky. The Russian theorist held a distinctive view of chance, different from Carr, admitting that, at times, chance plays an important role (§2.3).

Berlin, on the other hand, defended the view that history is (rather) *accidental*, and that agents’ actions are chance events in the sense that these are ‘uncaused’ (§2.4). Being antagonists with regards to the role of chance in history, Carr and Berlin were parties to a dispute which influenced and divided historians for a number of years, and is still, to some degree, influential (Cf. Ferguson, 1999).

I argue that in reality there is no (or there should not be any) *real* controversy between Carr and Berlin. Both authors use this crucial term – *chance* – in senses so different that their arguments do not really engage; they are simply talking about different things (§2.5). In order to demonstrate this I refer back to the plurality of senses in which *chance* has been used, and investigate which of these senses conform, respectively, to Carr and Berlin’s views.

In §2.6, and as a side note, I investigate the differences between Berlin’s position and that of Hegel. This is because Berlin’s account of chance was meant to be an attack on Hegelianism. Berlin claims that in Hegel’s view of historical evolution *chance* is completely excluded. I show that this is not the case.

Finally, in §2.7 I attempt to make evident that out of the ten different senses of chance, two are clearly incompatible with the doctrine that every event has necessitating causes (§2.1.6 and §2.1.7). Most other senses, epistemic in nature, are not challenged by problems associated with the doctrine of determinism.

2.1. Ten different definitions of ‘chance’

In his introductory chapter to *Aristotle’s Concept of Chance: Accidents, Cause, Necessity, and Determinism*, John Dudley (2011) briefly identifies the main differing views on chance, including that of Aristotle, which he undertakes to investigate more fully. Also Phil Dowe (2005)

investigates different senses of *chance* and how they conflict with the notion of Divine Providence. An overview of the matter might prove useful to our case. The ten different definitions I present next are based on Dudley and Dowe's lists, but I propose complements and corrections. I include Hegel's view of chance, and I reject one of the definitions Dudley presents (chance as related to the free will problem).

2.1.1 *Chance as an empty notion*

The first view of chance is mentioned by Aristotle:

But chance and also spontaneity are reckoned among causes (...) Some people even question whether they are real or not. They say that nothing happens by chance, but that *everything* (...) has some *definite cause* (...) it is always possible, they maintain, to find something which is the cause; but not chance, for if chance were real, it would seem strange indeed (...) (*Phys.* II, iv, 195^b31-37, 196^a1-8, transl. Hardie and Gaye).

Aristotle is expressing here the view (not his own) that all events are caused, and nothing can be caused by 'chance.' Hence, when we say an event was caused by 'chance' we are using a misleading notion, a term which has no content.

This is also the approach taken by Francis Bacon in his definition of the idols of *idola fori*:⁸³

The idols imposed by words on the understanding are of two kinds. They are either names of things which *do not exist* (... names which result from fantastic suppositions and to which nothing in reality corresponds), or they are names of things which exist, but yet *confused and ill-defined* (...) Of the former kind are *Fortune*⁸⁴ [chance], the Prime Mover, Planetary Orbits, Element of Fire, and like fictions which owe their origin to false and idle theories. And this class of idols is more easily expelled, because to get rid of them it is only necessary that all theories should be steadily rejected and dismissed as obsolete (*Novum Organum*, I, lx).

⁸³ Usually translated as *Idols of the Forum*, or *Idols of the Market place*. Such idols represent a certain category of logical fallacy, an invalid inference which is the result of an imperfect correspondence between the proposed definition of a word and the real thing in the world it represents.

⁸⁴ Best translated as 'luck,' but it could also signify 'chance.'

Bacon advances an ontological thesis, i.e., belief in the causal principle: all events are caused – hence for him there is no such thing in reality as chance. Of course, one might say that as a matter of custom we usually call chance events those of which no cause has been identified (yet) – but this is an epistemic claim, not the one Bacon is making here.

2.1.2 *Chance as a psychological notion referring to surprise at the unexpected*

The second definition of chance is presented by pre-Socratics such as Heraclitus, Empedocles and Anaxagoras. According to these philosophers chance refers to “the subjectively unexpected nature of certain necessary events” (Dudley, 2011: 3). Chance does not refer to the nature of occurrences, but to our subjective response to them: a chance event is one no one ‘expected’ to occur. In this sense, chance simply refers to being surprised by the unexpected.

No ontological or epistemic thesis is advanced here, and, unsurprisingly, there is nothing incoherent or inconsistent with the psychological approach. But it says nothing about chance as part of reality – precisely the question philosophers are concerned about.

2.1.3. *Chance as a notion conveying our ignorance of the causes of an event*

The third view of chance highlights the idea that the causes of chance events have an unpredictable nature. This is the view of Cicero who holds that because nothing happens without a cause, *Fortune* (chance) is the effect of hidden or obscure causes.⁸⁵ A similar remark has been made by Voltaire, who believes that because there is a cause for every occurrence, so chance is simply how we refer to events whose causes we are ignorant of.⁸⁶

Hume, in *An Enquiry Concerning Human Understanding*, expressed a similar view:

Though there be no such thing as *Chance* in the world; our ignorance of the *real cause* of any event has the same influence on the understanding (...) There are some causes,

⁸⁵ Cf. Cicero (*Top.* X vii, 63). For Cicero, chance is not a completely misleading term as it is for Bacon; it is only a term we use to convey our ignorance of the causes of a certain effect.

⁸⁶ Cf. Voltaire (*Le Philosophe Ignorant*, Ch. xiii).

which are entirely *uniform* and *constant* in producing a particular effect; and no instance has ever yet been found of any failure or irregularity in their operation (...) But there are other causes, which have been found more *irregular* and *uncertain* (...) when any cause fails of producing its usual effect, philosophers ascribe not this to any irregularity in nature; but suppose, that some *secret causes*⁸⁷ (...) have prevented the operation (*Enquiry*, XXXVII, II and IV).

A similar epistemic solution (that chance events are caused, but the causes are not known) is also embraced by Bertrand Russell: “What do I mean by a ‘chance’ event? I mean one of which the causation is *unknown*. (...) I should regard the birth of Napoleon as a ‘chance’ event. We do not know why a man of supreme military genius was born in Corsica at that time” (1944: 738).

But Russell had previously employed *chance* in different ways, and some examples he gave certainly do not fall under the present category of chance. Let us see two of his illustrations:

The best example of a ‘chance’ event which had large consequences (...) [was] the German decision in 1917 to allow Lenin to go to Russia. I call this a ‘chance’ event because, obviously, the German government must have thought of strong reasons on each side, and might just as easily, so far as we can see, have come to a contrary decision (1944: 738-39).

Here he seems to employ chance in the sense that there seems to be equally compelling reasons for deciding in favour of, say, *A*, *B* and *C*. There is something that led to *C* rather than *A* or *B*, but we do not know what it is – given the situation.

Elsewhere Russell illustrates chance differently again, by means of a counterfactual that is quite a departure from reality (and not an unknown cause):

That if Henry VIII had not fallen in love with Anne Boleyn, the United States would not exist. For it was owing to this event that England broke with the Papacy, and therefore did not acknowledge the Pope’s gift of the Americas to Spain and Portugal. If England had remained Catholic, it is probable that what is now the United States would have been part of Spanish America (Russell, as quoted in Hook, 1944: 673).

⁸⁷ [Unknown causes].

Sidney Hook interpreted Russell's initial notion of chance, as can be seen in the previous quotes, as "events whose causes lie outside the system in which they have effects independently of whether the causes and effects are trivial or great" (Hook, 1944: 673). Hook explained that because "we can never know whether any system is absolutely isolated from other systems and events, every prediction concerning the future behaviour of a system is conditional upon its freedom from interference" (*Loc. cit.*); and concluded that Russell's examples of chance events "conform to this (...) notion of chance" (*Loc. cit.*). But, clearly, Hook's example adds something different here; there is an unknown cause, but it is due to a system not being sufficiently isolated (the example of the birth of Napoleon is not of the isolated sort).

Russell rejected Hook's interpretation (1944: 734-735) and made it clear that his epistemic approach is rather simpler: all events are caused, but when one or more of causes are unknown, we call this a chance event. Applying this to the previous example we can say that event (a) Henry VIII's fancy for Anne Boleyn is a *cause* of event (b) the English colonisation of the United States, but we do not have the necessary knowledge to infer *b* from *a*, so that *a* causing *b* appears chancy.

Clearly, some examples given by Russell do not fall under the current sense of chance. Russell's later epistemic approach does not breach the ontological postulates of causal determinism⁸⁸ because all events are understood as being caused.⁸⁹ For Russell, nothing really happens *by* chance, chance is just how we name our failure to identify the causes of an event.

2.1.4. *Chance as an event resulting from the coincidence of independent causal chains*

Similar to §2.1.3, but offering a more subtle and particular idea of chance, is the notion of chance offered by J. S. Mill, (1973-4: 525-47). The particularity of this position is to define chance as the result of the coincidence of two independent causal chains.

As expressed by Mill:

⁸⁸ Understood here as the view that every event has a cause, and that *every* cause *necessitates* its effect.

⁸⁹ Russell's example of (a) being a cause of (b) – two event particulars – would clearly benefit from a counterfactual view of causation.

Chance is usually spoken of in direct antithesis to law; whatever (it is supposed) cannot be ascribed to any law, is attributed to chance. It is, however, certain, that whatever happens is the result of some law; is an effect of causes, and could have been predicted from a knowledge of the existence of those causes, and from their laws.... An event occurring by *chance*, may be better described as a *coincidence* from which we have no ground to infer a uniformity (...) It is incorrect, then, to say that any phenomenon is produced by chance; but we may say that two or more phenomena are *conjoined* by chance, that they coexist or succeed one another only by chance (...) (1973-4: 526).

The event that occurs at the conjunction of two independent causal chains is what we call a chance event. Mill's position would seem to endorse the view that such *chance* events are the *necessary effect* of the two coinciding chains. As *coincidences* do not conform to any laws we fail to infer uniformities; therefore chance events are unpredictable (epistemic thesis), but caused (ontological thesis), nevertheless.

I believe a useful historical example of Mill's idea of chance, which highlights the unpredictability of what happens when two independent causal chains coincide, is the case of the Spanish Armada. In 1588, under the orders of Catholic King Philip II, Spain launched a powerful attack against England's Protestant Queen Elizabeth. The Armada consisted of 131 vessels, about 25 thousand men, and another 30 thousand fighting men waiting for a pick-up in Holland.

The Spanish attack constitutes one causal chain (*a*), starting from Philip's intentions of conquering England to the planned landing of the Spaniards near London. Given the Spanish superiority in numbers and firepower, one might suppose that the English fleet commanded by Sir Francis Drake would not be able to resist, alone, the invading force; the causal chain was likely to run its course without major problems.

However, Drake's attack forced the Armada to change its course, navigating in the direction of Scotland, where a decisive blow of *Fortuna* [luck or chance] helped England. The Spanish fleet was caught in one of the most powerful sea storms ever registered along the Scottish coast, and the heavy and less manoeuvrable Spanish galleons were fully hit by the tempest. The fleet quickly became out of control and was scattered. A quarter of the ships were destroyed; sunk at sea or wrecked against the shoreline. Only half of the Armada returned home, after facing further storms and hurricanes on their way back (Cf. Hanson, 2005).

The sea storm was another independent causal chain (*b*), originating from natural causes (tides, sea temperature, winds, barometric pressure, etc.), consisting of a highly complex chain of meteorological events, all causally related. The outcome of the coincidence of (*a*) the invasion of a powerful armada, with (*b*) the passage of a powerful storm, can be said to be *de facto* unpredictable. Since both systems are independent, we fail to infer any uniformity (or laws) capable of predicting the sequence of events once both systems ‘coincide’ – hence the unpredictability of the outcome. But the destruction of the Armada was, from an ontological perspective, certainly caused by the storm.⁹⁰ That we call this a ‘chance’ event is clearly an epistemic matter – we lack predictive knowledge of what follows when two or more independent causal chains intersect.

Francis Drake and Queen Elizabeth, however, attributed the victory not only to chance, but to Divine intervention: the storm became known as ‘The Wind of God,’ or also as the ‘Protestant Wind.’⁹¹ This belief in Divine Intervention is implicated in the following interpretation of chance.

2.1.5 Chance as referring to events resulting from Divine Intervention

The fifth definition of chance is also given by Aristotle: “Others there are who, indeed, believe that chance is a cause, but that it is inscrutable to human intelligence, as being a divine thing and full of mystery” (*Phys.* II, iv, 196^b6-8, transl. Hardie and Gaye). Or, as French writer Anatole France puts it: “Chance is perhaps the pseudonym of God when He did not want to sign His name” (France, as quoted in Freund, 1973: 1).

This idea consists in the belief that reality can be divided into two realms, the realm of God (or Divine) and the realm of men or nature, where the realm of God may causally intervene in the realm of nature. Even if all events in the realm of nature have causes and are determined (part of a causal chain) – no natural system is ‘immune’ to divine (or supernatural)

⁹⁰ King Philip himself acknowledged that: “*I sent you* [Duke of Medina-Sidonia, Commander of the Spanish fleet] *out to war with men, not with the wind and waves*” (<http://www.tudorplace.com.ar>).

⁹¹ At the time, commemorative medallions portraying a wrecked Spanish galleon were issued with the inscription: ‘Jehovah blew with His wind and they were scattered.’

intervention. Ultimately, no natural event can be predicted or expected with certainty, as God's will might be decisive at any moment, and change the course of events. The 'chance' element is that no one in the realm of nature/men is capable of knowing God's intentions or predicting when He will intervene; therefore because of our ignorance of divine matters we call an event allegedly resulting from Divine Intervention a 'chance' event.

Once more, this is an epistemic thesis: if we knew when and where God is to intervene, we would not call the outcome of the intervention a 'chance' event. Ontologically, such events are caused, and the cause is God, or Divine Providence. To some extent, this view is a reply to the notion of chance in §2.1.3; our lack of understanding of what caused a chance event can be explained by the fact that the 'hidden' cause is of a different nature – divine or supernatural. When St Augustine talks of chance, he refers to an event whose cause is unknown, but there is a 'definite' cause of all events that are said to happen by chance: Divine Providence (Cf. Dudley, 2011: 7).

Chance events *appear* to have an exceptional and accidental character because they do not occur in accordance with known regularities. Because of our ignorance of the 'mysterious' power which caused the event, we called it 'chance' (epistemic thesis). But this only appears to be so because Providence acts as a hidden cause of which men must remain ignorant (§2.1.3); ultimately, chance events are caused (ontological thesis) on the relevant definitions.

2.1.6 Chance as the denial of necessity

According to Dudley the definition of a chance event as an event which is not caused by *necessitating causes* can be found in Democritus and Hume. However, I believe these two philosophers say different things here.

Democritus ascribed the causes of all things to *necessity* and also to *chance*. One of the leading interpretations of Democritus, however, suggests that his views on chance should be

understood not as the denial of necessity, but as something that occurs without *order* or *purpose*⁹² (Cf. Barnes, 1982: 423-6).

The thesis advanced by Democritus is that the order and necessity we perceive on the macro-level of reality is a product of *chance*, which occurs at the micro-level. His approach to chance had to do with his concern about how atoms behaving disorderly could produce an orderly cosmos “in which atoms are not just randomly scattered, but cluster to form masses of distinct types” (Berryman, 2010). His view was that order can be “automatically generated as a byproduct of ‘random’ collisions between all bodies at motion” (Furley, 1989: 79). It is unclear whether the events that occur at the micro-level, according to Democritus, are caused by necessitating causes or not.

Dudley correctly believes that the definition of chance as the denial of causal necessity can be found in Hume. In his footnotes, Dudley presents the relevant passages in Hume which he believes would support his interpretation:

- (a) (...) as chance is nothing real in itself, and, properly speaking, is merely the negation of a cause (*Treatise*, 125);
- (b) 'Tis [sc. necessity is] the constant conjunction of objects, along with the determination of the mind, which constitutes a physical necessity: And the removal of these is the same thing with *chance* (*Treatise*, 171);
- (c) (...) since 'tis commonly allow'd by philosophers, that what the vulgar call chance is nothing but a secret and conceal'd cause (*Treatise*, 130);
- (d) (...) liberty, when opposed to necessity, not to constraint, is the same thing with chance; which is universally allowed to have no existence (*Enquiry*, 96);
- (e) (...) necessity makes an essential part of causation; and consequently liberty, by removing necessity, removes also causes, and is the very same thing with chance (*Treatise*, 407);
- (f) (...) As liberty or chance, on the other hand, is nothing but the want of that determination, and a certain looseness, which we feel in passing or not passing from the idea of one to that of the other (*Treatise*, 408).

(Hume, as quoted in Dudley, 2011, Footnotes 28-30: 9)

In Dudley's interpretation, *all* these passages would support the view that chance is the denial of necessity. But this cannot be right; Hume's selected quotes often claim something

⁹² 'Purpose' is teleological. A series of events is said to be purposeful if it is goal-directed.

different. Claim (a) is ontological - ‘chance’ is not something real, similar to Bacon’s view in §2.1.1; (b) is ontological and epistemic – Hume is often interpreted as saying that necessity is not to be found in nature, but is the result of a mental projection of necessity by the mind (causation is a mental notion);⁹³ (c) is just an epistemic claim - that when we lack knowledge of the causes, we call the obtaining event the result of chance; (d), (e) and (f) are claims related to Hume’s reconciliation (compatibilism) between necessity and freedom.⁹⁴

It is important to note that Hume is not defining chance as the denial of a cause, but explaining what we mean by it in different circumstances, and by doing so he develops an ontological and an epistemic view of chance:

Ontological: chance is to be contrasted with necessity. According to the view that Hume has an idealistic account of causal necessity, Hume would say that necessity is unreal; it is rather the product of the mind that identifies the constant ‘conjunction of objects’ because of our natural inclination to assume the existence of a necessary connection between an event and its cause. If necessity is unreal, so is chance.

Epistemic: similarly to view §2.1.3, we sometimes call a chance event one of which we are ignorant of the causes; and it does not entail the belief that the event is uncaused.

Dudley also says that Hume was a determinist who believed in the doctrine that every event has a cause, i.e., believed in the truth of the *causal principle* (2011: 9). But this is contentious. In “Why a Cause is always necessary?” Hume says in the particular case of the coming into existence of some item: “We can never demonstrate the necessity of a cause to every new existence (...) [that every event has a cause] is utterly incapable of demonstrative proof” (*Treatise*, I, 3). And because we cannot prove the *causal principle* true, it must be at least *logically possible* for some events to be uncaused. Consequently, it is at least logically possible for chance to be a real feature of the world.

⁹³ This is a controversial reading of Hume, however. Another possible interpretation would be to say that Hume locates *necessity* at the level of laws/regularities, so it is real. See also §1.3.1.

⁹⁴ Dudley lists the problem of freedom as one that would benefit from a clarification of the ontological and epistemic status of chance. However, it is unlikely that ‘chance’ can be used to solve the controversy between necessity and freedom or free will, so excerpt (f) is misleading.

It is contested among scholars whether causal necessity, for Hume, is to be found only on the level of the mind or if it is located on the level of physical laws. It is also not clear whether Hume believed in the causal principle despite our repeated failures to prove it true. Chance is, however, to be understood in contrast to causal necessity – but the question of the reality of necessity (and of chance) will be here left unanswered.

2.1.7 Chance as objective probability

Dowe maintains that if the development of the world, from moment to moment, is at times a matter of chance, then “the world does not know in full detail where it is going next” (2005: 170). This is a metaphysical claim and not an empirical question – it is not expected that science can provide a justification for such a claim, although it is not excluded that empirical investigation could, in the future, confirm that chance is part of how the world works.

Dudley exemplifies this view of chance with the work of Charles Sanders Peirce.⁹⁵

According to Peirce there is no evidence in favour of the truth of determinism, but there is scientific evidence which suggests the falsity of this doctrine⁹⁶. Robert Burch presents a useful explanation of Peirce’s view:

(...) the universe does not display deterministic *law*. It does not directly show anything like total, exact, non-statistical regularity. Moreover, the habits that nature does display always appear in varying degrees of entrenchment or *congealing*. At one end of the spectrum, we have the nearly law-like behaviour of larger physical objects like boulders and planets; but at the other end of the spectrum, we see in human processes of imagination and thought an almost pure freedom and spontaneity; and in the quantum world of the very small we see the results of almost pure chance (Burch, 2013).

Peirce believed that the raw results of scientific observations demonstrate that not everything is ‘fixed’ by a deterministic law, as no scientific measurement is perfectly uniform or provides exactly ‘the same’ results every time – the lack of perfect exactitude was not

⁹⁵ K. Popper or D. K. Lewis would have been better examples, but Dudley is giving credit here to an early attempt of defining chance as featuring a probabilistic universe.

⁹⁶ See §3.6.

attributed by him to methodological or experimental deficiencies, but as a result of nature's indeterminism.

The later Peirce would say that all reality is lawless (at best the laws are probabilistic) and events are the result of chance, where chance has an objective status: he called this view of the 'fallibility' of enquiry *Tychism* – derived from *tychē* (chance). Fallibility of enquiry for “nature is not a static world of unswerving law but rather a dynamic and dicey world of evolved and continually evolving habits that directly exhibit considerable *spontaneity*” (Burch, 2013). Spontaneity refers here to the *workings* of chance.

There is a good chance that Peirce would have embraced *indeterminism* as proposed by quantum mechanics. Tychism is the doctrine that reality is in fact indeterminate – it is not the idea that chance is lack of knowledge. According to Peirce, in the physical world there are no determining causes whatsoever. Peirce's view on 'spontaneity' is similar to the belief in objective chance. John F. Phillips defines such idea along these lines:

In the literature on probability, two kinds of probability (...) are generally distinguished. There is subjective probability [epistemic account], which involves the beliefs that someone has which are dependent upon the available information, and there is objective probability or *objective chance* [ontological account], which involves consideration of the probability of some proposition independent of the information anyone has (2005: 267-8).

Lewis, in his “A Subjectivist's Guide to Objective Chance” (1980), argues that there is a relationship between *subjective chance* (credence) and *objective chance*. Lewis theorises that our subjective beliefs (credences) ought to 'match' (be equivalent to) objective chance; where the latter is used to describe what he calls irreducible indeterministic processes. Examples of such cases of indeterminism in nature are radioactive decay and photon emission and absorption. Quantum physicists normally believe that for such phenomena only a *probabilistic* account may be given.

Lewis's approach to objective chance was to define it as a physical propensity (or disposition) for a given type of physical situation to yield a certain kind of outcome – the objective 'chance' for an event to occur. In his example, if we toss an unbiased coin many times there is a high probability that the frequency of heads will be close to the probability assigned

for a single toss (50%): that we observe such a phenomenon is not a consequence of our degree of credence, but a fact resulting from an objective ‘disposition’ of the world.

Peirce’s thesis of nature’s indeterminism is an early example of a view that others (such as Lewis) have developed more consistently. According to Lewis’s position, for instance, from the alleged indeterminism of nature it does not follow that there is no causation, only that no event is fully necessitated by its causes, or no effect is absolutely certain to occur.⁹⁷ Lewis’s account is probabilistic; we may say that the occurrence of a certain antecedent *c* raises the probability of a certain consequent *e* to occur: for every occurrence of *c* we experience, say, 80% of the time, the occurrence of *e*, where in the absence of *c*, the frequency of the occurrence of *e* is lower than 80%, (or perhaps *e* never occurs in the absence of *c*). This is in line with Lewis’s understanding of a cause as a *difference-maker* (§1.3.4). Objective chance is clearly an ontological notion, and one that can only exist on the assumption that universal determinism is false.⁹⁸

2.1.8 Chance as a notion rendering free will possible

There is a mistaken view of chance held by Peirce (Cf. Burch 2013), William James (1979) and Berlin (2002); it is also a mistake which Dudley fails to point out. The mistake is the claim that indeterminism suffices to secure free will. This position is normally associated with non-causal libertarianism.

Libertarians believe that if our decisions were deterministically caused, this would contradict our assumption that we are free agents, because it will never be the case that an agent ‘can do otherwise.’ In order to rescue free will from the threat of determinism, libertarians sometimes appeal to the reality of ‘chance’ to falsify ‘necessity’ and to support the thesis that

⁹⁷ Lewis refers to an event as a ‘determining’ cause even in a probabilistic account. This is a particularity of Lewis theory. In this chapter, however, when I say that a cause determines its effect, this is to be understood as an event being fully necessitated by its cause.

⁹⁸ If universal determinism were true, ‘chance’ could only be used as a reference to subjective probability (Sober, 2001: 303).

humans actually get to choose (in a libertarian sense). This view consists in saying that ‘chance’ is the notion we use to make sense of the reality of free willed actions.

Berlin does not deny the ultimate possibility of determinism to be a theory true of our world, but points out that this would be inconsistent with the way we think of our moral categories, which seem to require the truth of free will.⁹⁹ He also believes that certain ‘deterministic’ approaches to human history fail to recognise that human behaviour is unlikely to be caused in the same way as other natural events.

I come back to Berlin’s view in §2.4.1 and also in §4.4.5. For now it will suffice to say that indeterminism would not set us ‘free.’ If determinism is true, the libertarian says – if our actions are caused by our beliefs and desires (as is commonly suggested) – then we are ‘unfree’ because our beliefs and desires, according to deterministic laws, uniquely fix what happens next. Would it help the libertarian case to say instead that our actions are probabilistically (or non-deterministically) caused by beliefs, desires and objective chance?¹⁰⁰

The idea that indeterminism is necessary for freedom seems a case of wishful thinking: because we ‘want’ to be free (or ‘need’ to think of ourselves as free agents in order to make sense of our moral categories), and determinism would ‘enslave’ us, then ‘chance’ (indeterminism) ‘must’ be an objective feature of the world. But even if we concede that quantum mechanics has proven the world to be more ‘dicey’ and not strictly deterministic, this would still not explain how free will is possible.

Elliot Sober provides a witty example as to why indeterminism does not help the libertarian: “suppose (...) your beliefs and desires determine what you will do. I now offer you a brain implant, whereby a tiny roulette wheel is introduced into your deliberation process (...) would the operation make you free? It seems implausible” (2001: 304). It seems that instead of being ‘enslaved’ only by beliefs and desires, now humans are enslaved by “beliefs, desires and a roulette wheel” (*Loc. cit.*). It is true that the roulette wheel provides objective chance, and the

⁹⁹ Free will defined in a non-causal libertarian sense.

¹⁰⁰ If, however, ‘chance’ is defined not in terms of probability, but in terms of the absence of a cause, the thesis worsens even further: it becomes impossible to explain any action, all becomes arbitrary.

possibility that historical events might evolve along different trajectories, but it certainly does not help the libertarian's case.¹⁰¹

2.1.9 *Chance as notion referring to events which are mere actualised possibilities*

The ninth view of chance, omitted in the list provided by Dudley, is that of Hegel. The German philosopher held the view that chance events are those which are merely *possible* – in the sense that a merely possible (*contingent*) event is one that may or may not become *actual*. For Hegel 'chance' is to be contrasted with *necessity* (an event must become actual, or must not become actual). If a possible event does become a past actual event, then we say that the event only got to be *actual* by *chance*. The 'character' of the event is the result of chance.

Now, *chance* is an important part of Hegelian dialectics. In order to portray an accurate description of chance and its role in Hegelian philosophy of history much more time would have to be dedicated to this matter, and this is not what I intend to do here. It will suffice to say that Hegel did not consider chance to be an empty notion. As Raoni Padui points out, Hegel "can be said to believe that there is [objective] chance in nature [therefore] Hegel does not share the dominant philosophical view of many contemporaries that nature is fully determined by causes we do not know" (2010: 251).

This seems to contrast with Hegel's famous remark that "the [World] Spirit does not toss itself about in the external play of chance occurrences; on the contrary, it is that which determines history absolutely, and it stands firm against the chance occurrences which it dominates and exploits for its own purpose" (1988: 58).¹⁰² Although the notion of a *World Spirit* (a sort of impersonal force) may be unpalatable for some (for empiricists, for example), this

¹⁰¹ It may also be argued that whenever historians examine agents' choices the explanation is *ex post factum*, and this makes some aspects of the discussion of free will less relevant for the philosophy of history.

¹⁰² Andy Blunden identifies two different meanings of *World Spirit*: "in Hegel's earliest works, 'spirit' meant the character of a people or a times, which is formed by their experiences in history, producing and reproducing their lives, enjoying victories and suffering defeats in war. Later on, after about 1804/5, it was a *World Spirit* which expressed or manifested itself in the life and times of civilisation as a whole. Rather than an *outcome* of history, it was present *at the outset* only needing to unfold and become conscious of itself" (2007).

claim is not a contradiction in itself. One way of making sense of this is to say that certain *kinds* of events (for instance, a revolution) are ‘due’ at some point of the development of the history of the world. The occurrence of such an event is something fully (deterministically) caused and necessary (*Notwendig*). However, the *actual* revolution (‘what’ occurs, the event that takes place ‘out there’), and certain aspects of it – who leads the revolution, who survives and who dies, precisely where it occurs – are mere ‘actualised possibilities’; events which are said to be merely possible or contingent, and not necessary. This is not to say that such events are uncaused or caused *by* chance: ‘chance’ here refers solely to the absence of necessity (under their specific descriptions as individual particular events) for they are necessary under certain higher level descriptions of Hegel’s view.

We may infer that certain kinds of events are necessary according to (and caused by) the *World Spirit*; but associated with (and contrasted to) this necessity there are a number of possibilities, events which may be actualised, and when they do they acquire their character (meaning) by mere chance. So, there is *necessity* in the real world, that which is caused by the *World Spirit*, but there is also *chance*, a notion we use to refer to the character of actualised possibilities: their character is to be explained as the result of chance. This is an ontological approach to chance: all events which are not necessitated by the *World Spirit* can be said to be ‘chance’ events, because chance is for something to occur in the absence of a determining cause.¹⁰³

2.1.10 Chance as a notion referring to meaningful unusual accidents

The tenth and final view of chance to be considered is that of Aristotle. Dudley (2011) examines exhaustively the meaning of chance and accidentality in Aristotle’s *Physics* (II, iv-vi) and *Ethics*, and how these relate to Aristotle’s rejection of determinism. Aristotle’s treatment of chance in his *Ethics* is similar to the view that chance is necessary for free will (§2.1.8), and will

¹⁰³ This view of chance is essentially different from that associated with the idea of the exercise of providence by a Supreme Divine personal agent, because ‘chance’ here resides in what is not caused (necessitated) by the *World Spirit*. On one traditional understanding of Providence, as discussed in §2.1.5, chance amounts to the ‘unpredictability’, from humankind’s perspective, of events *caused* by Divine Intervention.

not be investigated here. I will focus on Aristotle's treatment of chance in his *Physics*, and try to determine whether it is (or can be made into) a coherent account.

According to Dudley's interpretation of Aristotle, 'chance' is "neither a substantial reality nor a purely subjective notion, but has the real and meaningful status of an accidental cause" and should certainly not be viewed as "an ultimate cause or dismissed as absence of knowledge by the contemporary philosopher" (Dudley, 2011: 13). This claim needs explaining – but first a few words about terminology are needed.

Aristotle has two terms for chance: *tychē* and *tautomaton*. Because Aristotle uses *tychē* in reference to human beings the best English equivalent would be 'luck'; *tautomaton* has a wider scope (it includes *tychē*), hence is best translated into English as 'chance.' However, Aristotle uses *tychē* to refer to chance as a whole in different parts of his *Physics*, and therefore, for the sake of simplicity, I'll refer to *tychē* as chance, but when it refers solely to the domain of human action it should be read as 'luck.' For Aristotle a chance event (or concurrence, or coincidence) is caused by an 'unusual accident,' hence a coincidence (*symptōma* – 'to fall together') occurred due to an 'accident' (*symbebēkos* – 'contingency', 'happening').

A usual occurrence (event) is one that occurs always in the same way (like experiencing hot during summer and cold during winter), and these do not occur, we say, by chance. But we use chance to refer to unusual occurrences, claims Aristotle, hence we must investigate what we *mean* by a chance event. Not any unusual occurrence shall be named a chance event; it needs to have 'purpose' or 'finality'¹⁰⁴. Aristotle's idea of chance refers here to certain 'coincidences' (*symptōma*). Oddly, the idea of an unusual event occurring for a purpose is associated with it having meaning: "some events are *meaningful* to man, whereas others are not (...) human beings are at all times attempting to understand events and discover their *meaning* for their own *purposes*" (Dudley, 2011: 23).

Charlton offers a useful remark here: "we ascribe a thing to chance only if we think it remarkable" (Charlton, 106-7). What makes an occurrence remarkable is the fact that it is interpreted as relevant for our pursuit of goals; if it cannot be interpreted as such it is deemed

¹⁰⁴ For Aristotle 'purpose' is a reference to *intentionality*, or being a relevant occurrence according to one's goals.

meaningless.”¹⁰⁵ It can be said that intelligibility and having a purpose are elements that come together in Aristotle’s *Physics* – when we see how a chance/accidental event serves any purpose, then we say that such event’s occurrence is meaningful, or relevant.

Chance events are unusual and meaningful. Whenever something which is done for a purpose occurs in a rather unusual fashion we have what Aristotle calls a chance event. The idea that *tychē* is itself a special kind of ‘cause’ (incidental) is a difficult aspect of Aristotle’s theory. According to Dudley, Aristotle treatment of chance is based on the belief that chance acts as an agent; therefore it must be a cause – but a cause that ‘not the many nor the wise’ were capable of understanding.¹⁰⁶

For Aristotle causation is to be understood as occurring in to two different modes: what he calls *proper* causes (the doctrine of four causes) and *accidental*, or ‘chance’ causation. The difference in mode is important, as Aristotle rejects the idea that chance could be a cause similar in nature of the other four causes, so it cannot be simply an addition to the list of *proper* causes.

In spite of Aristotle’s effort to distinguish the two modes of causation, that chance is considered a cause seems incoherent: how can ‘chance’ cause anything? Up to this point we said that chance can only be the result of ignorance, or the absence of determining causes, or the unpredictable result of Divine Intervention, or a particular idea in Hegel’s dialectics. For chance to *cause* something (even if not *per se*), it must be an event, or force, or substance (form) – which it is not. Aristotle explicitly says that chance and spontaneity are a certain sort of *cause*:

Both [chance and spontaneity] belong to the mode of causation ‘source of change,’ for either some natural or some intelligent agent is always the cause; but in this *sort of causation* the number of possible [incidental] causes is infinite (...) Spontaneity and chance are *causes* of effects which though they might result from intelligence or nature, have in fact been caused by something incidental (...) (*Phys.* II, 6, 198^a1-8, transl. Hardie and Gaye)

¹⁰⁵ Someone having a bath when a solar eclipse takes place is an unusual but *meaningless* coincidence: hence it is not a chance event – not caused by chance.

¹⁰⁶ That chance is a ‘fifth cause’ is also the view of Jerold C. Frakes, who argues that *casus* and *fortuna* are for Aristotle *incidental* causes, subordinate to the efficient causes *physis* and *nous* (1988: 60).

We sometimes say that an accidental event is the result of chance, or was ‘caused’ by chance, perhaps in a careless use of language, and this is perhaps what Aristotle is acknowledging. Aristotle makes clear that this mode of causation differs from the doctrine of the four causes. If chance is to be understood as a ‘coincidence’ (*symptōma*), as such it cannot causally explain how the separate events ‘come together,’ as each one of these has a separate number of members of the ‘four’ causes. So, if two people find themselves in the same room, each one having independent reasons for being there and unintentionally meeting the other, such a coincidence is explained by chance, where chance is the fortuitous outcome of separate causal chains that coincide.¹⁰⁷

Chance is indeed a cause for Aristotle, but it is subordinate to his doctrine of four causes. For instance, there can be no accidental efficient cause or final cause because an accident (*symbebēkos*) “has no existence on its own” (Dudley, 2011: 30). In a simple example, we might say that there is no accidentality when a house is built by a skilled builder (nothing unusual), but that the builder himself is, say, a musician, is the case of an accident or of having ‘coincidental properties.’ But for the accident to be a ‘chance event,’ the coincidental properties (builder and musician) need to be meaningful, they need to “belong to the area of that which is for a purpose” (*Ib.*:32, in reference to *Phys.* II, v). So, the fact that the builder is a musician is not a chance event.

Chance events are *unusual meaningful accidents*. Aristotle provides an example of such a case:

A man is engaged in collecting subscriptions for a feast. He would have gone to such and such place for the *purpose* of getting money, if he had known. He actually went there for another purpose and it was only *incidentally* that he got his money by going there (...) – it [the chance event] belongs to the *class of things* that are *intentional*¹⁰⁸ and the result of intelligent deliberation. It is when these conditions are satisfied that the man is said to have gone ‘by chance.’ If he had gone of deliberate purpose and for the sake of this – if he always or normally went there when he was collecting payments – he would not be said to have gone ‘by chance’ (...) It is clear than that chance is an *incidental cause* in the

¹⁰⁷ This is close to what J. S. Mill says in §2.1.4, with the addition that for Mill such coincidence need not be meaningful: the coinciding chains need not belong to the same ‘area of purpose.’

¹⁰⁸ Ultimately, the intention was to collect money, but *how* it occurred was unintentional.

sphere of those actions for the sake of something which involves *purpose* (...) (*Phys.* II, 5, 196^b34-197^a8, transl. Hardie and Gaye).

The present example is a case of a chance event.¹⁰⁹ It is meaningful because going to the market place to collect money belongs to the area of the purposeful – but it is also unusual because the man did not know he would collect if he went there, and did not usually go there when he was collecting. The event is accidental, because the man went there for different reasons and ended up collecting money by ‘chance’ – it is unexpected or unusual, but purposeful: the outcome attends the purpose of collecting money for the feast. Therefore, chance relates to the accidental – that which does not have a purpose *per se*, but belongs to the area of purpose, and is meaningful, intelligible or choice-worthy. In another example a man goes to the market to sell olives (make profit) and there accidentally finds a man who owes him money who repays him, this is said to be a ‘chance’ event: unexpected, accidental and meaningful (both events belong to the same sphere of purpose – profit or collecting).

In §2.1.4 the case of the Spanish Armada adequately exemplifies the unpredictable outcome of the coincidence of independent causal chains, but does it attain the extra requirement imposed by Aristotle – to fall under the area of purpose? When Drake attacked the Armada he had the purpose of sinking the Spanish ships; that a natural phenomenon gave him a hand was certainly unexpected and in ordinary sense a case of ‘luck’ (*Fortuna*), but it is unclear whether this example would suit Aristotle’s definition. In Aristotle’s case the man was not expecting to collect where and when he did, and went ‘there’ for different reasons – not quite the same happened in the case of Drake.¹¹⁰

Suitable historical examples of Aristotle’s approach to chance will necessarily involve actions and intentions; a chance event is one that results from the coincidence of two sufficiently independent chains of causes, and among them we have to find motives. One agent has the general intention of achieving some goal, but succeeds when she is not expecting to do so, and in a rather unusual and unforeseeable way.

¹⁰⁹ Commentators disagree on what event is the ‘chance’ event in this example. According to Charlton, “there are two things which might be called lucky [chance], recovering the debt, and going to the place where [the debtor] is” (1970: 107).

¹¹⁰ Although we could say that the sinking of the ships did not occur in accordance with Drake’s action plan.

I believe a suitable example of Aristotle's notion of chance is the conquest of the Aztecs by Hernán Cortéz in 1519. When the Spanish forces arrived in Tenochtitlan,¹¹¹ Cortéz was probably expecting to do battle, with the help of some indigenous alliances,¹¹² and to enter the city, annihilating any resistance, enslaving the people and plundering its gold. We have here one causal chain, starting with Cortez's decision to march to Tenochtitlan until his entering the city.

However, what occurred next was rather unexpected; the superstitious Aztec emperor Montezuma believed Cortéz was an emissary of the feathered serpent god Quetzalcoatl, or the God himself.¹¹³ The interpretation that one of the causes of the Fall of the Aztec Empire was the belief in Cortéz as the returning god Quetzalcoatl can be found in David Carrasco's *Quetzalcoatl and the Irony of Empire: Myths and Prophecies in the Aztec Tradition* (1982); a view also shared by H. B. Nicholson in *The 'Return of Quetzalcoatl': did it play a role in the conquest of Mexico?* (2001). Montezuma's belief in the return of Quetzalcoatl and his will to act as Quetzalcoatl's humble servant constitutes a separate causal chain.

Montezuma did not attack Cortéz, but let him enter the city, presented him with gold, and according to Cortéz himself, surprisingly swore loyalty to the King of Spain. That Cortéz, mistaken for Quetzalcoatl, was peacefully allowed to enter the palace was certainly a case of a chance or 'luck' (*tychē*) event, and the Spanish conqueror seized his opportunity, taking Montezuma as a hostage in his own palace, and for a while the Spaniards tried to govern Tenochtitlan through Montezuma. Cortéz went there with the intention of plundering, and ended up as the local authority – something that certainly pleased his quest for *grandeur*.

Just as in Aristotle's example, Cortéz went to Tenochtitlan with the intention of taking over the city, but expected to do it by force. Instead, he was received as a 'god,' covered in gold, and obeyed as an authority. Cortéz did not know the Aztecs believed in the return of

¹¹¹ Capital of the Aztec empire; today Mexico-City.

¹¹² Republic of Tlaxcala.

¹¹³ This is a contested view however. More recent studies say that Montezuma was just trying to learn the enemy's weaknesses before attacking, and that the myth about Cortéz being mistaken for Quetzalcoatl was a Spanish post-conquest invention: "The legend of the returning lords, originated during the Spanish-Mexican war in Cortés' reworking of Montezuma's welcome speech, had by the 1550s merged with the Cortés-as-Quetzalcoatl legend that the Franciscans had started spreading in the 1530s" (Restall, 2003: 114).

Quetzalcoatl, and that one of the representations of this god is anthropomorphic, which would explain the Aztecs' ingenuous mistake. Cortéz's deification, even if short-lived, was an unusual and unforeseeable event resulting from a meaningful coincidence of causal chains. Hence, the event itself shall be considered an Aristotelian case of a chance event, because it falls under the general area of purpose of the Spanish expansion - to expand its sovereignty over the new found territories – and also under the area of purpose of Cortéz himself.

In general terms, Aristotle's position on chance shares some degree of similarity with the view that chance is the unexpected outcome of two systems which coincide (§2.1.4), but is more complex than that, as chance is only justifiably invoked as part of an explanation when certain specific requirements are fulfilled. Chance, as is defined in Aristotle's *Physics*, has been a very influential account, and one that can be made coherent, I believe, if we put aside Aristotle's insistence that chance is a special (subordinate) mode of causation, and stick to the idea that chance events are those which result from unusual meaningful accidents. By naming such an event a case of 'chance' we need not commit to chance as a cause, but solely as a reference to the unpredictable but 'meaningful' outcome of two causal chains that coincide¹¹⁴ – it becomes an epistemic approach.

2.2 *The controversy between Berlin and Carr*

In a famous book entitled *What is History?* (1964), the semi-historicist Carr dismissed the view that historical 'what ifs' are of any use in the case of history. Carr's position was a reply to Berlin's essay "Historical Inevitability" ([1954], 2002), in which Berlin attacks the idea that historical processes evolve according to the action of "vast impersonal forces" rather than prioritising the importance of the individual and the 'accidental' in history. Berlin believed that Carr's position belonged to an old Hegelian and Marxist tradition: that of believing that historical systems are processes governed by laws of necessity, where 'chance' (in a sense we still have to define) plays a non-important role. This was the reason Carr rejected not only the

¹¹⁴ Charlton says something similar here: "It would be more correct, then, to say that a thing due to chance is a concurrent outcome, than to say that chance is a concurrent cause" (1970: 108).

idea that accidents may be causally relevant, but also any methodology of history in which assumptions about alternative courses history might have taken are present.

What is at the core of the disagreement between Carr and Berlin? I argue that agreement between them could never be reached because the way they use such a crucial term (chance) is so different that their arguments do not really engage – and yet both authors fail to see it.

At the same time, I shall argue that although Carr was indeed influenced by both Hegelians (through Collingwood's influence) and Marxists, his particular view of chance is different from Hegel's and from that of well-known Marxist theorists such as Trotsky and Plekhanov. These believed that 'chance' does play a role in history, and were more open to accepting that accidental events may be an important part of a causal explanation than Carr himself would accept.¹¹⁵

2.2.1 Carr's dismissal of chance's importance.

Carr believed that writing about what 'might have happened but did not' is meaningless and not worth the historian's time. In his own words, counterfactual suppositions are "theoretically conceivable; and one can always play a parlour game with the might-have-beens" (Carr, 1964: 97), but no useful information arises from such speculations apart from restating already known causal links. Carr argued that counterfactuals cannot be used against deterministic positions in history: "for the determinist will only reply that, for these things to have happened, the causes would also have had to be different" (*Loc. cit.*).¹¹⁶

Essentially, Carr's perspective of history was that of an enquiry into *actual* causes, and a sort of enquiry in which the historian tries to explain causes and effects by identifying patterns, trends or regularities. Any different methodology, such as speculating about alternative

¹¹⁵ This is made evident in "Chance and Necessity in History: E. H. Carr and Leon Trotsky Compared" (2009), where Ann Talbot compares the methodologies of Carr and Trotsky.

¹¹⁶ As we shall see in Chapter 5, the mistaken idea that counterfactuals strengthen anti-determinists' positions in history is still commonly found in authors such as Ferguson (1999: 1-90).

outcomes for some historical events, would be to “violate the cardinal principle of the discipline” (Talbot, 2009: 88).

Carr not only emphasized that historians should look for causes, but also insisted that causes should be classified or ‘ranked’ according to their degree of historical importance (weight). Events which are identified as part of a well-established regularity should occupy the most important position as part of an explanation, while ‘accidental’ events should be the least important. Ideally, if a certain *kind* of cause is generally found to be succeeded by a certain *kind* of event, then we have established a causal regularity, which for Carr is the ultimate goal of the historian. ‘Accidental’ events are for Carr those events which are *caused* in some way, but are not known to belong to any established regularity or trend, and therefore should be regarded as epistemically unimportant or trivial, at least until our knowledge improves and we are able to accommodate such ‘accidents’ into a pattern – and therefore the term ‘accident’ would no longer apply.

This is clearly an epistemic understanding of historical accidentality. Carr’s position is in principle compatible with the epistemic accounts of accidentality I discussed before (§2.1.3 and §2.1.4). It is clearly incompatible with the account of chance given in §2.1.6, §2.1.7 and §2.1.8. Carr would probably also reject the Aristotelian account given in §2.1.10 because of the confusing idea that ‘chance’ can be a cause itself. In the following pages I shall say more about the definitions of chance that are consistent with that of Carr.

Independently of the definition of ‘accidental event’ adopted, because of his ‘ranking’ of causes according to their degree of epistemic importance (causal weight), Carr clearly disliked any view which imposes great importance on *historical accidentality* (the least important of causes).¹¹⁷ This is very different from the position taken by Berlin in his approach to historical accidentality.

¹¹⁷ In Carr’s own words: “During the past five or six years, almost everyone [in the UK and in the USA] who has written an article about history (...) has cocked a knowing snook at Hegel and Marx and determinism, and pointed out the absurdity of failing to recognise the role of accident in history. It is perhaps unfair to hold Sir Isaiah responsible for his disciples. Even when he talks nonsense, he earns our indulgence by talking it in an engaging and attractive way. The disciples repeat the nonsense, and fail to make it attractive (1964: 92-3).

Berlin perceived Carr's position as a case of historical determinism or inevitability, and held that it underestimates the causal importance of historical accidentality/chance. Berlin's long essay "Historical Inevitability" (2002) gives a confusing treatment of accidentality; it is rather an attack on different doctrines which he labels as cases of historical inevitability – ranging from Hegelianism and Marxism, to scientific approaches to history. In short, Berlin rejected any theory conflicting with three basic beliefs he held about history: (1) that accidents are causally relevant; (2) that determinism is incompatible with free will, and (3) that free will had to be real for history to be 'moral', i.e. for historians to be capable of blaming or praising the actions of the heroes or tyrants. Although the discussion of free will occupies a central role in Berlin's paper, and at times the notion of free agency is interwoven with that of accidentality, I will focus on (1) and see how and why it conflicts with Carr's position.

Berlin and Carr were mostly concerned with Britain's attitude towards the Soviet Union at the height of the Cold War. Carr was interested in rejecting counterfactual claims attacking the 'inevitability' of the 1917 Revolution in Russia. He opposed, for instance, the claim that the revolution could have taken a different course, had the Provisional Government managed to establish a proper and functional parliamentary system. Since Carr is understood to emphasise historical necessitarianism, Berlin believes that Carr's treatment of the Russian Revolution as a case of historical inevitability.

But Carr clearly denies this charge. He admits that 'inevitability' is used in a rather charitable sense of the word, and that 'probability' should be preferred. Carr also wisely adds that historians do not think that events are 'inevitable' before they have actually occurred, and this becomes obvious when they:

(...) discuss alternative courses available to the actors in the story, on the assumption that the option was open (...). Nothing in history is *inevitable*, except in the formal sense that, for it to have happened otherwise, the antecedent causes would have had to be different. (...) I am perfectly prepared to do without 'inevitable,' 'unavoidable,' 'inescapable,' and even 'ineluctable' (1964: 96).

The previous paragraph makes it clear that Carr was not, strictly speaking, an 'inevitalist' in the sense Berlin (and Talbot) wants us to believe he was. It would be much fairer to his theory of history to say that he argued for a methodology of historical enquiry which

accepted the *possible* truth of the deterministic assumption,¹¹⁸ i.e., the idea that it is possible for the world to evolve deterministically. Carr himself explains the motivation for the ‘charge’ against historical inevitability as being an emotional response against Marxism, and against himself, and the result of a confusion between events being caused, and some doctrine of historical determinism:

[when] the historian writes of the Norman Conquest (...) *as if* what happened was in fact bound to happen (...) nobody accuses him of being a determinist and of failing to discuss the alternative possibility [of the Norman defeat]. When, however, I write about the Russian revolution of 1917 in precisely this way (...) I find myself under attack (...) for having (...) depicted what happened as something that was bound to happen (1964: 97).

Carr’s conclusion is that historians inquiring into aspects of the Norman Conquest are capable of emotional detachment, and therefore are less prone to pursue imaginary alternative outcomes for William’s invasion of England.¹¹⁹ This is certainly not so in the case of the Bolshevik Revolution, and people who have suffered its consequences often wish to protest against it, and in doing so they frequently let “their imagination run riot on all the more agreeable things that might have happened, and [become] indignant with the historian who goes on quietly with his job of explaining what did happen and why their agreeable wish-dreams remain unfulfilled” (1964: 97-8). This example also elucidates Carr’s position that counterfactual history is a game played by the ‘losers’ of history.

Let us go back to Carr’s insistence on the need for a hierarchical classification of causes: “every historical argument revolves around the question of the priority of causes” (1964: 90). Or in a more complete explanation of his position:

(...) from the infinite ocean of facts the historian selects those (...) which are historically significant; and the standard of historical significance is his ability to get them into his *pattern* of rational explanation (...) Other [causal] sequences (...) have to be rejected as *accidental*, not because the relation between cause and effect is different, but

¹¹⁸ I explore the different senses of determinism in Chapter 3.

¹¹⁹ Although it should be fairly easy to do so, as King Harold Godwinson, the last of the Anglo-Saxon rulers, could have won at the Battle of Hasting (1066) had his troops not abandoned their strong hilltop position to pursue the retreating Bretons in open field. Cf. (Lawson, 2003).

because the sequence itself is *irrelevant*. The historian can do nothing with it (...) It is true that Cleopatra's nose (...) had [causal] results. But it makes no sense as a *general proposition*¹²⁰ to say that generals lose battles because they are infatuated with beautiful queens (...) (1964: 105-6).

How are historians supposed to prioritise some causes and neglect others?

Carr believed that a successful causal explanation is one capable of identifying a trend of history, i.e., when a certain *kind* of event always (or usually) is followed by a certain *kind* of effect. Carr is committed, if I interpret him correctly, to some version of the *Humean* regularity, i.e. *a* causes *b* iff it is the case that *a* temporally precedes *b*, and *all* Type-*A* events are *always* followed by Type-*B* events (as seen in §1.3.1).

Economic explanations fit this requirement quite well. A good example would be the claim that famine after prolonged wars or severe droughts is usually followed by mass migrations. Because finding food is the most basic of human needs, famine seems to impose an irresistible demographic pressure. This exemplifies the kind of *pattern* Carr believes to be necessary for the provision of explanations in history: significant generalisations.

Other perceived historical regularities, Carr acknowledges, are less evident, weak or vague. But because they may still be explanatorily significant, he accepts that they should not be ignored. Perhaps a good example of such a weaker regularity can be found in the case of *democratic peace theory* (Cf. Doyle, 1997), which states that all democratic countries are more hesitant to declare war against other democratic countries than non-democratic countries are to declare war against any other country (democratic or not).

However, some historical events are said to produce causal consequences, and yet we fail to subsume them under general claims. These are the so-called accidents of history; the least

¹²⁰ Dray (1957: 103) makes a similar remark here, as he says that Cleopatra's nose is not a crucial cause, in the sense that had Cleopatra not existed things would have evolved in a similar fashion. To say that a cause is crucial is to say that it is a necessary condition for a certain effect. This is compatible with a counterfactual approach to causation.

important causes in Carr's classification. This is because for these there is not any explanation – or only of the minimal kind that simply identifies a cause.¹²¹

Accidents of history are events that are, in general, not describable as belonging to a certain *type* of event. And for Carr typification is essential as we only really 'learn' from history when we are capable of expressing a claim of the form: *whenever x then y*. Therefore causes which can be placed as belonging to a well-established *general trend* of history are more relevant than weaker or vague regularities, which in turn are more relevant than 'accidental' events because these cannot be placed as part of any pattern.¹²² In Carr's own words: "accidental causes cannot be generalised; and, since they are in the fullest sense of the word unique, they teach no lessons and lead to no conclusions" (1964: 107).

It is important to note, however, that accidentality for Carr has nothing to do with determinism: "[accidents] are causally determined *as anything else* that happens, an accidental event is caused as any other event" (1964: 98).

Berlin perceived Carr's dismissal of the 'importance' of historical accidents as an unjustified position. Carr's 'causal weighting methodology' was reason enough for Berlin to perceive him as a determinist, although such an interpretation is clearly not straightforward. Berlin offered a libertarian perspective of human agency whereby agent's actions are not deterministically caused, and the future is a vast and open field of real possibilities – 'chance' is for Berlin a major player in history because it is how we name the result of non-deterministically caused human agency (§2.1.8). It was the beginning of an ideological dispute that Talbot baptised as *Historikerstreit*,¹²³ a dispute "so very un-British that there is not even a satisfactory English word for it" (Talbot, 2009: 89).

The core ingredient of this dispute was the difference of treatment with regards to the role of 'chance' in history. If one were to defend Berlin's position that chance should be given a

¹²¹ Carr is here replying on the regularity theory of causation (§1.3.1). Perhaps a counterfactual theory (§1.3.4) could deal with this. If we define causation in terms of counterfactual dependence, it is hard to see how this idea of 'accidents' would get a grip. Carr, of course, did not know the counterfactual theory.

¹²² Carr is here cautious as not to speak of laws.

¹²³ A 'fight' between historians.

prominent role in causal explanations, then it is natural to think that history could ‘easily’ have followed a different path – a sort of historical *contingency* (to be defined in §5.4.1 and §6.3.2.1) in which historical counterfactual assumptions come easily into play.

Some have concluded that if we endorse Carr’s idea that historians should look for causes which belong to identified trends or regularities, while dismissing other events as mostly unimportant, then “counterfactuals play no role, since events were determined and inevitable” (*Loc. cit.*).¹²⁴ Talbot, in his analysis of ‘chance’ and ‘necessity’, turned to the work of Leon Trotsky, whom he called a ‘third participant’ in the dispute between Carr and Berlin. Talbot’s intention was to show that Carr’s reluctance to accept the role of ‘chance’ is a result of Carr’s own methodological preferences and beliefs, rather than a simple consequence of his Hegelian and Marxist theoretical influences.

The key differences between these three authors are well known, and may be summarised as follows:¹²⁵

- (a) Berlin accepted the validity of counterfactual assumptions and the role of historical accidentality, but rejected determinism;
- (b) Carr endorsed a version of determinism, and rejected the causal importance of historical ‘accidents’ and counterfactuals.
- (c) Trotsky also endorsed a determinist position, but admitted the plausibility of counterfactuals and agreed that ‘chance’ sometimes plays an important role in history.

As we will see next, the reason for this disagreement regarding the role of chance/accidentality in history is that we have three different notions of chance being used.

¹²⁴ I argue in Chapters 5 that counterfactuals have epistemic value, and they can either operate under Carr’s or Berlin’s perspective, and tell us nothing about the truth or falsity of historical determinism.

¹²⁵ Cf. Talbot (2009: 89) and Haslam (1999: 192-217).

2.3 Carr and Trotsky on chance

Carr read some of Trotsky's historical work,¹²⁶ and both generally shared a view of the Soviet Revolution which portrays it as having, as Berlin would say, a certain 'aura' of inevitability. But they differed with respect to the causal importance attributed to chance events. Even though Carr was not strictly speaking a Marxist, he often argued in dialectical terms, and believed that economic forces were more influential than other forces in 'shaping' the course of history. John Hallett has summarised this position quite well: "there is a point up to which we are all Marxists now. We all seek to explain political history in terms of the underlying economic realities" (Hallett, as quoted in Haslam, 1999: 54).

Talbot argues that the differences between Carr and Trotsky can be made evident in terms of their different treatment of 'chance.' Trotsky argued in favour of the importance of accidental events: "the entire historical process is a refraction of historical law through the accidental. In the language of biology, we might say that the historical law is realised through the natural selection of accidents" (1971: 515).

The previous quote is metaphorical, of course. How is it possible for one to have laws refracted through the accidental? A possible way of making a bit of sense of Trotsky's claim would be to rethink it in this way: the world evolves from S_{t_1} to S_{t_2} according to the laws, in a *loosely* defined trajectory; whereby any *accident* pushing the evolution of the system away from its central trajectory, is *necessarily* compensated by another *accident* which puts it back on track.¹²⁷

Another way to make sense of this, as Trotsky's reference to biology indicates, is to consider the process of natural selection as one of those cases in which a *law* is realised through *accidents*. The law of evolution, I presume, operates at a higher level; it determines (or makes probable), for example, the appearance of intelligent animals within a certain timeframe. It does not indicate which species would be the first to evolve into that stage, or *when* and *where* it

¹²⁶ Carr refers to Trotsky several times in his *History of Soviet Russia* (1966).

¹²⁷ I argue in Chapter 5 that the idea of one event being compensated by another, keeping the system's evolution on its tracks despite its sometimes unpredictable behaviour, is a case of chaotic systems with strange attractors.

would occur. From that higher level standpoint many events would be described as ‘accidental;’ for example, which species of animal would be the first to develop reason?

From the standpoint of natural selection it seems accidental that New Zealand be home to a number of flightless birds. But from a lower level description of New Zealand’s physical conditions (including fauna and flora) the absence of mammals and other predators causally explains it. Accidentality and determinism are not at conflict here, as long as we properly distinguish the levels of explanations the laws refer to. This is so either in biology or in history.

Carr remained unconvinced by such a position; although it is obvious that accidents do occur, and at times they could not be completely ignored. For Carr, “they [simply] did not enter into the significant *hierarchy*¹²⁸ of causes” (Talbot, 2009: 90).

Trotsky gave the following illustration of chance. He claimed that his absence at the Central Committee and the Central Control Commission of the Bolshevik Party, which took place from 25 to 27 October 1923, was *caused* by an accident – an accident that should not be ignored as it had important causal consequences.¹²⁹ After spending the night in his tent, during a duck hunting session, Trotsky stepped into icy water, and contracted an illness he described as “a dogged, mysterious infection, the nature of which still remains a mystery to my physicians” (1947: 381). The illness lasted for four months, and motivated Trotsky to write that “one can foresee a revolution or a war, but it is impossible to foresee the consequences of an autumn shooting-trip for wild ducks” (1971: 519). This is because the revolution is believed to be a robust system, one which will probably run its course to completion without any major interference. A hunting trip, however, may exemplify the collision of independent causal chains: (a) nature chilling the water overnight clashing with (b) Trotsky being sleepy, and being careless on waking up – as in §2.1.4. The result of the collisions cannot be predicted, although it may be fully causally determined; this is just a consequence of our inability to foresee.

¹²⁸ *Hierarchy* suggests an ordering of relative importance, for the historian’s explanatory task.

¹²⁹ For a detailed description of the consequences refer to Cliff, T. (2009).

In a different example, Trotsky refers to Kornilov's insurrection¹³⁰ using these words: "an accident occurred, which like all historic accidents opened the sluice-gates of necessity" (1977: 715). Trotsky's remark here illustrates once more his desire to explain how accidentality can be made compatible with the idea that the world evolves according to a 'loosely' defined trajectory, i.e. it is an attempt to compatibilise accidentality with Marxist teleology of history. This attempt contrasts with a common belief that there is no place for accidentality in Marxist theory of history, or, as Talbot puts it, "the accidental, the contingent and the individual are often thought to have no role in historical materialism because it is deterministic" (2009: 91).

The belief that historical materialists do away with *chance* (and also with free will) is false. Neither of the senses of chance loosely employed by Carr and Marxists (§2.1.3 and §2.1.4) conflicts with determinism. The Marxist sense of chance is epistemic; they claim only that although we suppose events to have causes, we often fail to know what those causes are. Yet critics of Marxism, such as Berlin, often appeal to historical 'accidentality' as evidence against the theory, which is a mistake.

The following remark should make clear that there is no dismissal of accidentality within the Marxist tradition.

[history would] be of a very mystical nature, if 'accidents' played no part (...) These accidents naturally form part of the general course of development and are compensated by other accidents.¹³¹ But acceleration and delay are very much dependent on such 'accidents,' including the 'accident' of the character of the people who first head the movement (Marx and Engels 1965: 319-20, as quoted in Talbot, 2009: 91).

We may explain this, I suggest, by saying that a Marxist revolution was determined to occur, given the economic, social and political struggles the Western world was experiencing at the beginning of the 20th Century (antecedent conditions) and the historical law. Yet, exactly

¹³⁰ The Kornilov Affair (or Putsch) was a failed *coup d'état* led by General Lavr Kornilov against the Russian Provisional Government in August 1917. Isaac Deutscher claimed that from the defeat of Kornilov started a new chain of events leading to the Revolution of October (2003: 233).

¹³¹ Carr is unsatisfied with the view that 'accidents', such as the premature death of Lenin, are 'compensated' by some other 'accidents' in order to restore the *balance* of the entire historical process. Carr does not endorse a teleological view of history, which may explain his position.

‘how,’ ‘where,’ and ‘when’ the revolution will occur cannot be foreseen.¹³² From that higher standpoint, that *the* revolution occurred *in Russia*, and *in 1917*, seems accidental. But that a revolution occurred may be said to be the result of a higher-level determination. The key idea is, again to show an event as chancy *relative* to a certain level of descriptions.

Accidents do play a role in Marxist history. Let us now turn to when such accidents are said to occur. We often say that an accidental event is one that occurs when two or more well-known deterministic causal chains collide (§2.1.4). Although we can predict the evolution of each chain independently (according to a theory), we fail to predict the result of their collision. Since causal chains in history are in constant collision one with the other, prediction in history seems a herculean task.¹³³

In *History of the Russian Revolution* (1977), Trotsky identifies four separate causal chains responsible for the onslaught of the Revolution of 1917: (a) the growing consciousness of class by the proletariat; (b) the change of mood in the Russian army, following important defeats against Germany; (c) the peasants’ increasing desire for a *vendetta* against the aristocracy; and (d) the insurgence of oppressed minorities/nationalities.

Each of these chains is believed to evolve deterministically and independently, and we have sufficient knowledge of their (lawful) evolution. For example, Marxist theory provides a good explanation of how the capitalist society, and its exploitation of labour, created a proletarian consciousness of class (a). In the same way, we may provide good causal explanations for (b), (c) and (d).

When two or more of these collide we have as a consequence a ‘chance’ event’ – *chancy* not in the sense of not being deterministically caused, but in the (epistemic) sense that it cannot be *foreseen* by our knowledge of the evolution of (a), (b), (c), and (d). And it is precisely the collision of these chains which constitutes the causal narrative of the Russian revolution, a

¹³² Critics of Trotsky could also argue, more simply, that he is trading here on a vague (not genuine) prediction. This may well be the case, but it does not conflict with the point that the *law* of history, as expressed by Marxists, is meant to be a higher level generalisation.

¹³³ In Chapter 3 we shall see that an attack on predictability (§3.1.4) is not effective rejection of ontological determinism (§3.1.2).

narrative that can be supplied only in hindsight. This is how the historical law is, paraphrasing Trotsky, ‘realised through accidents’: that a revolution occurred is no surprise, but exactly how the *actual* revolution occurs, is accidental *relative* to our knowledge of all the forces at play.

A similar position is endorsed by Marxist theorist Plekhanov, for whom it is clear that ‘accidents’ should be considered as potentially relevant for any historian. His notion of chance is also that of §2.1.4, as the following excerpt makes evident:

Accident is something relative. It appears only at the point of intersection of inevitable processes. For the inhabitants of Mexico and Peru, the appearance of Europeans in America was accidental in the sense that it did not follow from the social development of these countries. But the passion for navigation which possessed West Europeans at the end of the Middle Ages was not accidental; nor was the fact that the European forces easily overcame the resistance of the natives. The consequences of the conquest of Mexico and Peru by Europeans were also not accidental [but] were determined by the resultant of two forces: the economic position of the conquered countries on the one hand, and the economic position of the conquerors on the other (1961: 305).

The Marxist theory of history, as exemplified by Trotsky and Plekhanov, is capable of accommodating accidentality/chance, as defined in §2.1.4., within their theoretical framework.¹³⁴ Carr, on the other hand, adopted an epistemic view of historical accidentality which is a combination of the senses of chance identified above in §2.1.3 and §2.1.4. This can be seen in his claim that “accident [chance] is not *simply* something which we fail to understand” (1964: 102-3); and his acknowledgment¹³⁵ that §2.1.4 sets out a coherent view: “[historical accidents] represent a sequence of cause and effect interrupting (...) the sequence which the historian is primarily concerned to investigate. Bury, *quite rightly*, speaks of a ‘collision of two independent causal chains’” (1964: 99).¹³⁶

¹³⁴ It is also capable of accommodating free will, as we shall see in Chapter 5.

¹³⁵ The earlier Carr (1950s) did not acknowledge *chance* as in §2.1.4, failing to identify the distinctive treatment given to chance/accidentality by Marxist historiography. Cf. (Deutscher, 1981, doc. 58). The later Carr did acknowledge §2.1.4 as coherent.

¹³⁶ Bury drew attention to §2.1.4 in “Darwinism in History” (1930), and also “The Idea of Progress” (2008).

Events which are produced as a result of the collision of independent causal chains cannot be foreseen or predicted, and therefore cannot occupy a privileged position when part of a causal explanation, according to Carr's weighting/classification of causes. Such a classification does not entail either a defence or an attack on the doctrine of determinism. At least it entails the recognition that the epistemic layer of determinism is perhaps untenable (as I discuss in Chapter 3).

Talbot, in his comparison of Carr and Trotsky, claimed that historians should try to find the right *balance* between 'necessity' and 'chance' in history when providing causal explanations. He believes that one way of doing it is by avoiding Carr's fatalist¹³⁷ position and favouring the use of counterfactual thought experiments: "the historian who gives due weight to both *chance* and *necessity* must inevitably consider the counterfactual possibilities" (Talbot, 2009: 93).

However, it is not clear how counterfactuals may help us find such 'right balance'; nor is it clear how 'chance' contrasts with historical 'necessity.' We may call an event *chancy*, in the sense Trotsky and Plekhanov employ this notion, and still leave it open whether the world evolves deterministically or not. As I previously said, an event is characterised as *accidental* relative to a certain framework – in this case a higher-level law of historical evolution for which some occurrences are unforeseeable and seemingly chancy. But those seemingly chancy events may be deterministically explained at their proper level of physical descriptions. Chance and necessity are not in conflict with one another.

Talbot contends that it is "ironic that the politician who considers the counterfactual possibilities inherent in a situation tends to be considered a wise statesman while the historian who dismisses those same counterfactuals after the event tends to be regarded as a sober judge of evidence" (*Loc. cit.*).

But there is nothing ironic about that. As we have previously discussed, the idea of a 'chance' event predominant in Marxism is not that of an event which is uncaused or caused indeterministically. Chance so defined is an epistemic notion consisting in our inability to predict or forecast; and this inability as such, is no evidence against ontological determinism.

¹³⁷ Fatalism is an illegitimate accusation, in my view.

Therefore, the ‘statesman’ is forced by his predictive inability to consider multiple possible outcomes, while historians, because they only retrodict, seem to be in a better epistemic position to explain how one event led to the other based on historical evidence, without necessarily appealing to counterfactual assumptions. Counterfactual thought experiments in history are no replacement for claims based on solid evidence; at best they fulfil a certain interpretative function – as I discuss in Chapter 5.

2.4 Berlin on chance.

It is often said that historical determinists reject the role of accidents, while those like Berlin, who emphasise the importance of the ‘accidental’ in history, reject historical determinism as false.

There are two main theses to be identified in Berlin’s (2002) work:

(a) Berlin’s controversial view that if historical determinism is true,¹³⁸ there can be no free human agency, as determinism entails that things ‘cannot happen differently’, neither natural events, nor historical ones. If humans cannot really choose how to act, they become ‘puppets’ of external, impersonal, irresistible forces (laws of historical development), and the historian’s moral judgments would then be rendered meaningless, since, in order to blame or praise historians must presuppose that an agent is able to freely choose from a range of possibilities. Berlin does distinguish between ‘substantive’ deterministic theories of history and ‘scientific’ determinism, but uses the same argument to criticise both. It is not sound, I believe, to apply the same argument to the two different cases, and this displays insensitivity to important differences between these two kinds of determinism.¹³⁹

¹³⁸ Berlin quotes Bernard Berenson’s interpretation of doctrines of historical determinism as “dangerous dogmas, which tend to make us accept whatever happens as irresistible and foolhardy to oppose” (Berenson, 1952:116). For Berlin it is “a tendency to return to the ancient view that all that is, is best; that to explain is to justify; or that to know all is to forgive all; ringing fallacies which have led to special pleading and, indeed, obfuscation of the issue on a heroic scale” (Berlin, 2002: 94).

¹³⁹ Christopher Dawson in his review of Berlin’s essay asks: “how is it possible to condemn in one breath the idealist view that history is the self-manifestation of the absolute spirit in time and the behaviourist view that

(b) “Historical Inevitability” is still commonly referred to in the field of counterfactual history,¹⁴⁰ presumably because it highlights the importance of the ‘accidental’ in history - as opposed to the view that events are causally necessitated or ‘inevitable.’ Historical evolution is to be viewed as a process that includes *open* possibilities, because for Berlin freedom and moral responsibility cannot exist in a deterministic world.

Let us now offer a criticism of such views.

2.4.1 *A response to Berlin.*

I must add to what I said about freedom and chance in §2.1.8. Consider the claim ‘agent *A* decided to do *x*.’ Berlin would say that *A*’s action was free only if it was the case that *A* ‘could have done otherwise,’ i.e. *A* could have chosen not to do *x*, but to do some other action *y* instead. It is the possibility of doing *y* instead of *x* which renders *A* a free agent. So, for Berlin, it would appear that freedom requires the absence of 100% antecedent causal determination.

Now, if universal determinism is true of our world – in the sense that every event is necessitated by *total* antecedent events (under laws of nature) – how can *p* still be a free agent? Even if we adopt a dualist position and say that *A*’s actions are not *caused* in the same way as physical events are, such actions still take place in the physical realm. If universal determinism is true, *A* ‘cannot do otherwise.’ Berlin concludes in favour of incompatibilism between freedom and determinism: either *A* is free and determinism is false; or determinism is true, and *A* is unfree.

If *A*’s actions are said to be deterministically caused by factors ‘outside’ *A*’s control (genes, environment, laws of historical development, etc.) we must say that *A* cannot help but acting as he/she *must*: *A* is *always* unfree. As a consequence, any moral judgement of *A*’s behaviour is nonsensical. If our world is deterministic Berlin believes that historians cannot

history is meaningless and that human societies should be studied in the same way we study the termites? No doubt both views are open to objection, but not for the same reasons” (1957: 585).

¹⁴⁰ Cf. Ferguson (1999: 83).

blame Pol Pot for being evil, or praise Gandhi for being courageous, for each of these could not help but act as they did.

But what if the world is indeterministic, in the sense that some events are not 100% causally determined by their antecedents; would that render freedom possible? No, it would not. In §2.1.8 I mentioned Elliot Sober's explanation of why indeterminism cannot help the libertarian (the case of an action being caused by beliefs, desires and a 'roulette wheel'). One must conclude from that argument that both theories – determinism and indeterminism – impose a challenge for the notion of freedom. I feel inclined to think that freedom should be characterised in a way compatible with determinism and also with indeterminism.

A typical compatibilist view of freedom would characterise agent *A*'s actions as free if *A*'s actions have been *caused* in a proper way. Let us say that *A* decided in favour of doing *x* rather than *y* because *A* believed *x* to be the best course of action for obtaining something *A* desires. Free action is hereby characterised as an action which is *caused* by *A*'s intentions (beliefs and desires), and the intentions as such are generated by a perfectly operational mind – *A*'s intentions caused *A*'s actions in a *proper* way ('deviant' causal chains have to be excluded). If *A* suffers from a mental illness, an illness which affects the way *A*'s mind works, then I say *A* is unfree because his actions are not being caused in a *proper* way.

I provided an overly simplified and certainly incomplete compatibilist account of free action; but an account which is compatible with the doctrine of determinism as it does not define freedom as the absence of fully determining antecedent conditions. I do not wish to enter any further into the problem of freedom at this point.¹⁴¹ But it is rather remarkable that Berlin neither criticises nor even acknowledges the possibility of a compatibilist account of freedom. Instead, his attack on historical determinism is based on a rather odd argument about the implications for our moral language should determinism hold true of our world.

Berlin believes that although it is possible for the world to be deterministic, if it were so, it would clash with our use of moral language, for such language implies more than the simple logical possibility of realising alternative courses of action, so that individuals can be

¹⁴¹ I engage with Berlin's libertarian views again in §4.4.5.

accountable for their actions.¹⁴² We think of what would have been the best decision or course of action in a given situation, precisely because such acts are believed to be open in past, present and future times. So, if determinism is ever proven true of our world, our moral categories would have to undergo serious revision – or so claims Berlin.

In a concluding remark about historiography Berlin interweaves libertarianism with accidentality: “the placing of what occurred in the context of what could have happened and in the demarcation of this from what could not (...) this is what the sense of history comes to” – and adds that the historian’s ability to judge depends upon his/her capacity to “speak of criticism, or praise and blame [and] this is the sole and obvious reason why ‘accidents’ (...) are necessarily outside the category of responsibility and consequently beyond the bounds of criticism” (2002: 121).

What seems to follow from this is that a question of the type: ‘Could the First World War have been prevented?’ can only be answered, according to Berlin, if the event itself is characterised as the outcome of free choices. If a deterministic theory of human behaviour were found to be true, then, believes Berlin, all notions of moral responsibility would turn out to be inappropriate; and “these categories permeate all that we think and feel so pervasively and universally that to think them away is impracticable” (*Loc. cit.*). But maybe a compatibilist account of free action could deal with the problem of moral responsibility quite well.

2.5 Carr and Berlin

In Berlin’s view, historical determinism is not simply the view that every event is causally determined, but it is also the adoption of a certain ‘ranking’ of causes which neglects the role played by the individual and by *chance*. Furthermore, Berlin claims that such a view fails to recognise that there is one type of event which is not caused in the same way as other physical events; human actions – a typical libertarian position. Therefore, Berlin offers an anti-causal perspective on history, and refers to ‘chance’ as an element required for freedom (§2.1.8) – a view I have argued against.

¹⁴² The alternative possibilities must be real.

The libertarian can avoid some criticism of his/her views by saying that an event that lacks a deterministic cause (and so is to some degree ‘chancy’) is intrinsic to an agent’s action only when it is brought about *by the agent* (theory of agent-causation).¹⁴³ I believe the notion of agent-causation is problematic, but to provide a fair criticism of it falls outside the scope of this chapter and thesis.¹⁴⁴

The historical ‘determinist,’ like Carr, says that causes, those which might explain long-term processes (economic developments, a country’s demographics, rise and fall of political *régimes*, etc.) should occupy a position of priority for any historian because they express regularities. Carr does not deny, however, that there are indeed ‘accidents’ in history – it is only that we are incapable of causally explaining them (according to the regularity account of causation). Carr’s definition of an accident is mostly an epistemic account, a modified version, of §2.1.3 and §2.1.4 combined with view that ‘accidental’ events have little to no epistemic value in history. Clearly, no agreement between Berlin and Carr is possible, because their uses of ‘chance’ are different.

Carr’s position is close to that of Hume (§2.1.3, not §2.1.6). It basically says that although we assume ‘accidental’ events have causes, they are counted as accidental because they cannot be subsumed under any *known* causal regularity. But Carr also believed in the causal principle: “The axiom that everything has a cause is a condition of our capacity to understand what is going on around us” (Carr, 1964: 93-4) – thus his position may be said to be that of §2.1.3 (ignorance of the cause of an event) combined with his personal views on the methodology of history.

With regards to agent causation he says that “everyday life would be impossible unless one assumed that human behaviour was determined by causes which are in principle ascertainable” (Carr, 1964: 94). So Carr thinks that all actions are ontologically

¹⁴³ The idea that an agent can start a new causal chain which is not causally determined by antecedent (physical) events and the relevant laws of nature. For a criticism of agent-causation, see C. D. Broad (1952: 215).

¹⁴⁴ I will say bit more about this in §4.4.5.

deterministically caused,¹⁴⁵ but sometimes we ignore what caused our behaviour, or fail to see it as part of a pattern.

Talbot described Carr's notion of a chance event in history as identical to that expressed in §2.1.3. In his own words: Carr, despite being uncomfortable with historical chance "admitted it rather begrudgingly because he accepted that there were inevitably many facts of which the historian must remain ignorant" (Talbot, 2009: 93).

But Talbot's interpretation is too simplistic. As I explained in §2.4, Carr's notion of chance says that we only label an event 'accidental' because we fail to see its occurrence as part of a *relevant* sequence of cause and effect, but we do not deny that the event was caused,¹⁴⁶ nor do we say that the causes are always *unknown* (§2.1.3). Carr would concede, I believe, that should we eventually succeed in placing the 'accidental' occurrence under a general claim, perhaps even being in a position of explaining its occurrence in the light of antecedent conditions and governing laws, the event would not be labelled 'accidental' any more, and would be granted a higher place in the ranking of causes.

In the case of Cleopatra's nose, Carr wrote that "the connexion between female beauty and male infatuation is one of the most regular sequences of cause and effect observable in everyday life" (Carr, 1964: 98). Thus, we must conclude that should we ever know the laws that govern such relation between female beauty and male infatuation, then also Cleopatra's nose could be given causal priority as it would (partially) explain Mark Anthony's behaviour in such a manner that military leaders could 'learn' from it.

For Berlin 'chance,' as in §2.1.8, is the idea that free action requires the absence of fully determining causal antecedents. If agents are free, then their actions are not deterministically caused in any way. As a consequence, historical events could have happened otherwise because

¹⁴⁵ Carr was clearly a compatibilist: "the logical dilemma about free will and determinism does not arise in real life. It is not that some human actions are free and others determined. The fact is that all human actions are both free and determined, according to the point of view from which one considers them" (1964: 95).

¹⁴⁶ Carr ironically claims: "it is unnecessarily discourteous to Cleopatra's beauty to suggest that Antony's infatuation had no cause" (1964: 98-9).

it is *always* the case that agents could have chosen different courses of action.¹⁴⁷ As Talbot points out, Berlin's conception of chance was "the product of the individual will and was intensely voluntaristic, harking back to the German Romantics he admired so much. Berlin allowed chance a sweeping field of operation" (Talbot, 2009: 93).

When Berlin accused Hegelians and Marxists of being determinists, he meant precisely this: 'such approaches to history reject the role of chance.' But this need not be; Hegelians and Marxists had different views of *chance* – not the same notion employed by Berlin – and admitted not only the existence of chance events, or 'accidents,' but in the case of some who differed from Carr (e.g. Trotsky) highlighted their importance.

2.6 Berlin and Hegelianism.

Despite the popularisation of the controversy between Carr and Berlin, "Historical Inevitability" (2002) was largely written as an attack on Hegelianism. Berlin believed that Hegel's philosophy of history was the perfect example of a view in which chance is denied since all events are held to be necessitated by irresistible *impersonal forces*.¹⁴⁸ But as we saw in the case of Marxism, Hegelianism is also capable of accommodating 'chance' as explained in §2.1.9.

Berlin's worry about necessitarianism in Hegel's philosophy of history may be synthesised along these lines: overrating impersonal forces would lead to a deterministic outlook of history, in the sense that in the presence of necessitating external forces the historical agent could not have done otherwise, and takes no genuinely free decision. According to Berlin, Marxists and Hegelians are examples of thinkers who adopt such a deterministic perspective. But in fact the notion of *necessity* advocated by these thinkers is not one that defends 'irresistible forces' acting on the course of *all* events of history.

¹⁴⁷ Berlin does not simply equate a free action with a chance event. But because actions are free, their outcomes are, in a way, *chancy*.

¹⁴⁸ An impersonal force can be a demographic or economic factor, over which one has no or little control, an aspect which certainly will influence, if not determine, how one is going to live his life, the decisions one makes. But is it possible to write history as free from impersonal factors? Is it possible to defend the thesis that human choices are ultimately free from impersonal forces? It seems unlikely.

The *Hegelian* notion of historical necessity is not one that renders free will an illusion, or endorses the view that all events happen as they *must*, inevitably, according to irresistible forces or obscure entities such as the *World Spirit*. In addition, Hegel never claimed that a philosophical grasp of the necessities of history would allow historians to make predictions about its future course. As Dray points out: “Hegel does not really attempt to show that each [event] follows *inevitably* from what preceded it in the main line of historical development” (1964: 74). Hegel’s description of the Persian Wars (499 - 459 B.C), for example, conveys no aura of determinism: “the interest of the world’s history hung trembling in the balance” (Hegel, 1956: 257) – a quote which suggests that the outcome of the Greco-Persian confrontation was open, undecided at that point, and not fully necessitated by the circumstances.

Hegel’s account of the life of great leaders, the so-called heroes of history (Alexander, Caesar, or Napoleon) is also one that does not undertake an *impersonal* approach to history. In some moments, it seems to be rather the opposite. Dray identified in Hegel’s account of the role of these ‘great men’ an aspect of historical contingency, which Dray defines as “the fortunate timely availability of both men and conditions”, and adds that “[Hegel] envisaged their unpredictable interventions as making a real difference to the course of history” (1964: 74).

But Hegel did believe that some historical events are necessitated, or determined by the circumstances. Caesar’s rise to power was, according to Hegel, an event *postulated* [determined] by the circumstances:

(...) the Republic could no longer exist in Rome. We see, especially from Cicero’s writings, how all public affairs were decided by the private authority of the more eminent citizens – by their power, their wealth; and what tumultuary proceedings marked all political transactions. In the Republic, therefore, there was no longer any security; that could be looked for in a single will (...) Caesar, judged by the great scope of history, did the Right; since he furnished a mediated element, and that kind of political bond which men’s condition required (Hegel, 1956: 312).

According to Dray’s correct interpretation the only event that is necessitated, or postulated by the circumstances is the fall of the republic: “conditions being what they were, the republic *would* necessarily fall” (1964: 75). But we can also ask: “Would the state be saved? Only

if there was a Caesar¹⁴⁹ to do what was required – and *if* he would do it” (*Loc. cit.*). So, the event of which Caesar has become the *irreplaceable* protagonist – the fall of the Republic – was *necessary*, but ultimately it was ‘contingent’ human agency that took responsibility for ‘how’ it occurred. Dray explains that “what was required [of Caesar] was, of course, *necessary*; but only in the sense of being necessary *for* the salvation of the state – and for the continued development of history in the development of increasing freedom” (*Loc. cit.*). Even the latter is not, we may observe, *guaranteed* to occur; Hegel recognises that there are periods of history where all gains in terms of development are entirely lost.

Hegel’s metaphysical speculation – the belief that history has a ‘goal’ (rationality), which is achieved by means of a certain *Will* – is not a thesis I wish to examine here any further. However, if the previous interpretation of Hegel’s philosophy of history with regards to historical necessity is correct, then such necessity is not deterministic in the sense feared by Berlin. Hegel is not placing historical agents as completely powerless victims who causally succumb under the pressure of ‘impersonal forces’ – Caesar’s particular actions may be caused by Caesars’ intentions. The events that followed Caesar’s interventions, or the form in which the Roman Republic falls, is a consequence of Caesar’s freedom, and therefore Hegelianism does not entail the denial of free will, but only that some *kind* of events, such as a government being overthrown, can be ‘postulated’ (necessitated) by the circumstances.

Furthermore, it is clear that one can speak of *impersonal* factors as historical causes without committing to the *inevitability* claim. One could talk of, say, social pressures which count as a cause for an agent’s certain action, but ultimately we may say that it was the agent’s decision to react in a certain way. There is obviously a relation between behaviour and the external world. If we adopt a *Lewisian* view of causation, for instance, we can say that a complex economic event (an *impersonal* force), such as the economic crisis of 1929, *caused* the suicide of a few bankers and speculators who decided to jump from their skyscrapers’ windows. Had the crisis never existed, the suicides would (probably) not have occurred. The crisis as such fits the category of an external factor over which the individual has little power. One can, and we often

¹⁴⁹ A *Caesar-like-figure*. But what if Caesar had died in his infancy and no *Caesar-like-figure* was available? It is unclear what Hegel would say in this case. Possibly, he would ‘postulate’ the *necessity* of a *Caesar-like-figure* coming into existence.

do, say that the ‘pressure’ was just too overwhelming, and at some point suicide seemed the only way of relief for some of the people concerned. It would be silly to say that each window jump was fully *necessitated* by irresistible external forces; that each individual caught under its influence was *unfree*, and *must* have jumped. Hegel would say, if I interpret him correctly, that an event of the *kind* of a world crisis was *necessitated*, but some of its undulations – as who jumps out of the window and who does not, are rather the product of *chance* (as in §2.1.9).

Let us summarise the *Hegelian* treatment of chance and see how it works.

Hegel defines chance/accident (*Zufall*) using the Aristotelian contrast between *energeiai* (what is actual) and *dynamai* (what is only potential). According to Hegel, *actual* events can be either *necessary* or *contingent* (a potential which happened to actualise). If an event is contingent, then it is possible for it to become actual, and that it is actual is only a matter of *chance*. In Hegel’s philosophy the concept of *accidentality/chance* is rather complex and a bit obscure; not only does *chance* contrast with what is said to be *necessary*, but also implies that the contingent depends on (is non-deterministically caused by) something else.¹⁵⁰

In Hegelian philosophy when a *contingent possibility* becomes actual, it is made actual by an *other* which gives the first the conditions to actualise. (*Enc. I* and 145A). Based on this claim I’d like to suggest the following interpretation of Hegel: a contingent event (*b*) is said to be *caused* by this *other* event (*a*) in the sense that without the *other* (*a*) the first (*b*) could not have become actual – a case of causation as counterfactual dependence. The caused event (*b*) is said to be *contingent* – made actual by *chance* – it is not *necessary*, because it could have not become actual and remained only a potentiality.

In the case of history, Hegelianism tells us that *necessary* events are those which are actual and *must* become actual – they must occur – while *contingent* events *may* actualise or not, despite the occurrence of the *other* [event] they are contingent upon. Actual events are actual either because they *must be*,¹⁵¹ or made actual by *chance* (§2.1.9) and grounded on something *other*. We can explain the accidents of history by identifying these *others* – in modern language their causes.

¹⁵⁰ Cf. Inwood (1992: 198).

¹⁵¹ Determined by laws of historical development.

Despite some unclarities about the use of the terminology, it seems that Hegel is advancing the view that although some events are *necessary* while others are *contingent*, all are *caused* in a certain way – or, in other words, there is always a *reason* for any *potentiality* (*dynamei*) to become an *actuality* (*energeiai*).

As it turns out, also in Hegel’s philosophy of history there is a place for *necessity* and a place for *contingency*¹⁵² and *chance*: at least some historical events could have been otherwise, or not have been at all, remaining potentialities. *Prima facie*, there is no reason why Hegelians should reject, for example, counterfactual thought experiments in history. Hegelianism need not be an extreme version of historical determinism as Berlin wants us to believe it is, and it is also capable of accommodating some version of historical accidentality/chance, as §2.1.9 had already indicated.

2.7 Final remarks.

Berlin’s attack on a view he called *historical inevitability* was motivated by an important confusion; namely that the causal view of history entails the adoption of an extreme deterministic position – a kind of determinism which would rule out the possibility of free will and neglect the role of chance in history. This attack was also based on the assumption that historians are not just chroniclers of the past but need to engage in moral judgments; therefore should determinism hold true of our world, our moral language – and consequently much of our historical language – would need to undergo serious revision.

But of all the different definitions of chance I presented it is *objective chance* (§2.1.7) that really conflicts with the idea that the actual world evolves deterministically. Carr, Trotsky, Plekhanov, and Hegel make no reference to objective chance. Berlin, on the other hand, believes that free actions require the absence of fully necessitating causal antecedents – which would be the case if objective chance was real. But Berlin makes no direct reference to objective

¹⁵² The interpretation of Hegel’s nomenclature is nevertheless controversial. Inwood says: “The concept of *contingency* is unclear. The claim that something is contingent may mean that (a) it is a matter of sheer chance, so that there is no reason for it; (b) there is a reason for it (...) but [it is] inaccessible to us; (c) the reason for it is accessible to the natural sciences, but the phenomenon cannot be shown to be necessary *a priori* by philosophy” (1992: 198).

chance either. What was referred as a *chance* event (*accidental*) was either the fact that we sometimes fail to *foresee* events which are consequents of the conjunction of independent causal chain (§2.1.4), or the fact that we sometimes do not know how an ‘accidental’ event is subsumed by a lawful regularity, although we clearly see it as part of a causal sequence (§2.1.3); or the belief that a certain category of events is causally determined by other events (contingent), but not necessitated by the action of *impersonal forces* (§2.1.9). There is no justification for interpreting Carr, Trotsky, Plekhanov, or even Hegel, as defenders of *historical inevitability*.

I said that a *chancy* view of history is preferred by historians who wish to engage in counterfactual claims, because it makes it easier to imagine alternative historical worlds. But I see no reason why the so-called *historical determinists* should necessarily avoid historical counterfactuals. On the contrary; if I say that a set of causal antecedents, together with the laws, deterministically caused a certain effect, then I say that that effect could not have been otherwise *unless* any of the conditions (or the laws) had been different. Marxist and Hegelians, if so desired, could well entertain thought experiment about what would have been the case should such antecedents have not occurred (or occurred differently).¹⁵³

When historians talk about the role of chance in history, they need not adopt the view that events are not caused, or are caused indeterministically. In the next chapter I will examine the different senses in which determinism is used, hoping that a better understanding of each of these senses, which I will call layers, will prove a clarifying exercise towards our understanding of the plurality of views on chance, determinism, and contingency, and how they intermingle.

¹⁵³ A different question would be: is there anything to be learned from such thought experiments? I will attempt to answer this question in Chapter 5.

Chapter 3

Varieties of Determinism

3. Determinism

In the previous chapter we considered Berlin's view that history is not 'deterministic,' that there is no 'historical inevitability,' or otherwise individuals would be unfree, and the historian's understanding of the world, which relies on moral evaluation, would be lost. Ferguson (1999), in a more recent work, attacks vehemently four different doctrines which he pejoratively labels as cases of historical determinism, only to embrace the view that history is chaotic.

In order to bring clarity to accusations of historical determinism it is important to consider what the doctrine, or doctrines, mean and what reasons there are for holding determinism to be possibly true. Are there different senses of determinism? How does determinism relate to chaos theory? Can determinism remain plausible despite findings in quantum mechanics suggesting that it does not hold of our actual world?

This chapter focuses on whether the physical world evolves deterministically, and what we mean by that claim. Here I say little about history in particular; the reason being that determinism is more clearly definable as applied to the natural sciences, especially physics, as two of the senses of determinism we are going to examine require quantification and modelling.

Before advancing any further, I need to forewarn the reader that I feel unable, given my lack of adequate knowledge of the physical sciences, to give a proper and substantial

contribution to issues surrounding determinism in physics. But I feel that a careful characterisation of determinism and its different senses, as the one found in Stephen H. Kellert's *In The Wake of Chaos* (1994), in particular his third chapter, "Unpeeling the layers of determinism," will aid me with my future discussion of *historical determinism* (Chapter 4).¹⁵⁴ I would like to ask for the reader to be patient, as I am afraid the purpose of the present chapter, and the reason for its inclusion in this thesis, will only be clear once the reader advances into my later chapters. I can say now that one of the problems exposed by Kellert, which I believe will serve us well, is the idea that the relation between ontological determinism, and predictability (the idea that there is symmetry to be found between causally explaining and predicting or retrodicting) is weakened by (fairly) recent developments in physical theory, and also better philosophical analyses.

This chapter starts with my exposition and interpretation of Kellert's characterisation of the various *layers*¹⁵⁵ of determinism – namely **A**, **B**, **C** and **D** (§3.1). I will then consider, in the light of Kellert's exposition, whether determinism can operate at any one or combination of these layers (§3.2), and what the consequences are, for the doctrine (or doctrines) of determinism, for ideas originating from chaos theory and quantum mechanics (§3.3 to §3.7).

I will then defend the idea that, if determinism is properly characterised as a metaphysical tenet, it does seem to escape refutation from criticism arising from the association of chaos theory with quantum mechanics (§3.8). This is the conclusion reached by John Earman (1986, 2004, 2006, 2008), a position which, from a philosophical perspective, seems very sensible, and which I wish to endorse and contrast with that of Kellert.

¹⁵⁴ Although historians frequently label some doctrines of historical evolution as historical determinism, their use of this terminology is sometimes obscure, and reflects little understanding of what scientists and philosophers mean when they say that a system (or the entire world) is deterministic. Also the differences between an ontological view of determinism and its epistemic consequences are often unclear.

¹⁵⁵ Throughout this thesis I make reference to these layers as **A**, **B**, **C** and **D**. It shall always be understood as reference to the definition presented in §3.1.

3.1. Identifying the layers of determinism

Determinism is sometimes used as a description of systems of a certain kind, and sometimes, more generally, in a cosmological sense. The latter requires us to consider the entire universe as a unique system. In either case, the characterisation of a system as deterministic goes through four different levels or layers, as distinguished by Kellert: **A** – Differential Dynamics; **B** – Unique Evolution; **C** – Value Determinateness; and **D** – Total Predictability.¹⁵⁶

In the following sections I provide formulations for each of these theses, and comment on their philosophical implications. Although I mostly refer to Kellert's individuation of the layers, I propose a few modifications, in order to highlight the contrast between ontological and predictive versions of determinism. I show that layer **A** (§3.1.1) is a layer applicable to models only; layer **B** (§3.1.2) is the most fundamental, and is essentially an ontological doctrine of determinism, layer **C** (§3.1.3) has a dual ontological / epistemic aspect, and layer **D** (§3.1.4) is essentially epistemic.

3.1.1. Layer (A): Differential Dynamics

A dynamical system is one whose states evolve in accordance to mathematical expressible rules (differential equations). There is a mathematical expression or rule(s) which describes, for instance, how the spatial position of a certain 'object' evolves over time. In simple words, the state of a certain system S at a time t_1 , together with the relevant rules, determine the state of S at t_2 .

The state of the universe at time t is related to the state at time $t_0 < t$ by a mathematical expression with differential equations,' because any two quantitative formulations of the 'state of the universe' are surely related by some function or other. To put teeth into this notion, it may be necessary to specify that the mathematical rule must involve simple, time invariant differential equations (Kellert, 1994: 58).

¹⁵⁶ Kellert attributes this fourfold division to Mark Stone (1989).

A dynamical system is said to be deterministic if its evolution over time can be fully specified by a set of equations where no probabilities (chance) are present. As Keller explains:

(...) a system is deterministic if the dynamical system that *models* it makes no reference to chance. (...) a dynamical system has two parts: a representation of all possible states of the system and a set of equations that describes how the state of the system changes with time. When neither of these two parts involves chance explicitly, we have a deterministic dynamical system (1994: 57).

As the previous quote illustrates, for Kellert, a deterministic dynamical system is not a real physical system, but a *model* of a system, or as he says it, a system that ‘models’ a real system – so I’ll refer to it as M_s . It is a model that describes a real system’s evolution according to deterministic rules. There are two requirements for M_s to be characterised as deterministic:

(1) The rules and the state descriptions must be precise, and not just approximate. Also, the state description of S at a given time t must include all the relevant physical information of M_s at that time t .

(2) If M_s is deterministic, then there are no probabilities in which, say, the state of $S_{(t_1)}$ evolves into $Sa_{(t_2)}$ 30% of the time, and into Sb_{t_2} 70% of the time. There must be no ‘branching’ in the evolution of M_s : “the rules or evolution equations must be deterministic in that they allow no (...) plurality of possibilities; they must not be stochastic equations (Kellert, 1994: 58).¹⁵⁷

Because M_s is a model, and not a *real* system, we must not claim that the rules which apply to it are laws of nature. The key idea of layer **A** is to say that a model M is deterministic if every state description of M_s is fully deducible (in principle) by (non-stochastic) rules and the description of any prior state of M_s . In other words, we are able to deduce from a prior state of the model, a subsequent state.

We must accept the possibility that there may be a discrepancy between the model of S and S itself. It may well be the case that while the model seems to obey a perfectly deterministic evolution, the system being modelled could have some level of indeterminacy which we are

¹⁵⁷ “stochastic” - To be defined as any equation containing reference to *chance* or *probability*; or equations containing one or more *random* variable(s). Interestingly, the word comes from Greek *stokhazesthai* – “to guess at” (The American Heritage Dictionary of the English Language, 2009).

unaware of. Determinism according to **A** is more applicable to models rather than to real systems of the world.

I believe the next layer represents best what is normally intended by determinism.

3.1.2 Layer (**B**): *Unique Evolution*

Layer **B** conveys the idea that determinism is a metaphysical notion applicable to real systems of the world. This notion simply says that a deterministic world, or system, is one that evolves along a unique trajectory, and if the system were, so to speak, be wound back to some initial state then all subsequent states would have the same history.

We can give a more precise definition.¹⁵⁸ Let S be any system which has a history over time, and let $S_{(t)}$ be a state of the system at a given time t . Suppose now that there are two systems, S and S^* which have a history over time and which are governed by the same laws L . We may now say that S is ontologically deterministic (**B**) if S and S^* agree at any time t in their respective states (*viz.*, $S_{(t)} = S^*_{(t)}$), then they agree at any other time t' (*viz.*, $S_{(t')} = S^*_{(t')}$). If S and S^* disagree at any other state at t' (*viz.* $S_{(t')} \neq S^*_{(t')}$), then S is indeterministic.

It may be the case that a system is only *futuristically deterministic*, in the sense that t' is later than t . Or a system may be *historically deterministic*, if t' is earlier than t . If we then take these two kinds of determinism together we can define what it is for S to be overall deterministic. Thus we can define determinism in terms of unique evolution of a system.

Based on the definition of determinism of a system, we now can, derivatively, say what it is for a set of laws L to be deterministic in terms of a system S to which the laws apply being deterministic. We say that L is deterministic just in case the history of S which they govern is (overall) deterministic. If the history of S is not deterministic, but indeterministic, then L is

¹⁵⁸ The following definition is adapted from Earman (1986: 12-4) and Montague (1974: 319-36). I believe this to be a strictly ontological, non-epistemic, view on determinism, different from Kellert's characterisation of **B**-style determinism (1994: 59).

indeterministic. In sum, *L* is deterministic only if *S* has a unique evolutionary history. That *L* is deterministic, and that *S* has a unique evolution are closely tied to one another.

Lewis gives a similar account of determinism of laws deriving from uniqueness of evolution; although he expresses it in terms of possible worlds (we may take a possible world to be an all-encompassing system):

A deterministic system of laws is one such that, whenever two possible worlds both obey the laws perfectly, then either they are exactly alike throughout all the time, or else they are not exactly alike through any stretch of time. They are alike always or never. They do not diverge, matching perfectly in their initial segments but not thereafter; neither do they converge (1986a: 37).

Determinism according to **B** is a theory about how the world evolves independently from anyone *knowing* that it does so, or there being any theory about that world's evolution. There has been, however, in the history of philosophy, a certain tendency to conflate the ontological version of determinism, uniqueness of evolution, with our capacity to predict the future states of deterministic system, i.e. our ability (actual or idealised) to deduce its future states from the laws and a perfect description of the state of the world at any given time. This conflation is misleading.¹⁵⁹ Layer **B** only states that the trajectory of the world (or a system) is unique – deducibility or predictability are epistemic matters which constitute a different approach to the meaning of determinism, as we will see in §3.1.4.

John Earman makes an interesting remark here:

I take determinism to be an ontological doctrine (...) about the temporal evolution of the world. This ontological doctrine must not be confused with predictability, which is an epistemological doctrine, the failure of which need not entail a failure of determinism [as an ontological doctrine] (Earman, 2004: 21).

The tendency to conflate the ontological doctrine with the epistemological one is, I believe, because we think **B** as a real feature of the world (or a system), so that under *ideal* circumstances, deterministic systems have somewhat knowable trajectories. But as Earman points out, these are two different theses about determinism, and must not be confused.

¹⁵⁹ This is based on my interpretation of Montague (1974: 303-5). See my further comments in §3.2.

Determinism as in **B** is a real feature of the world – it says nothing about uniqueness of evolution being knowable (this will be discussed in §3.1.4).

3.1.3. Layer (C): Value Determinateness

The question remains whether a system in which no branching is possible, and there is only a unique path of evolution, would suffice for a definition of determinism. Well before Earman, Clark Glymour (1971) already maintained that a proper definition of determinism also needs *determinateness of quantities*. In simple words, such value determinateness means that all values attributed to quantities have to be *exact*, which is precisely where quantum mechanics and determinism move in different directions.

As Glymour explains, quantum theory:

(...) does not seem to require that all its quantities have precise values at all times. The appropriate determinist attitude toward this feature of quantum mechanics is that it, too, results from incompleteness. Determinists must hold that, if the quantum theory is true, then its observables always do have precise values even though the quantum theory does not tell us what they are, and even, perhaps, though our ignorance of such values is a necessary ignorance (1971: 746-7).

Kellert briefly mentions in one of his footnotes the work Hans Reichenbach's *The Direction of Time* (1956) in which the author seems to imply that an adequate definition of determinism ought to entail **B** and **C**. As Reichenbach explains:

[Determinism] is a theory derived from observations by way of an extrapolation. Since observations lead to physical laws of great predictive power (...) if we could make more precise observations we could make predictions that would come true without exception. A set of ultimate causal connections is supposed to be hidden behind observable relationships. Determinism is thus based on an extension of observed regularities to unobserved ones; and it is assumed that the flaws of attainable predictions would vanish if we could only uncover the *ultimate* causal structures (1956: 82).

From an idealised perspective, we could still say conditionally that *if* we knew all the relevant initial conditions of a system, and the ultimate physical laws involved, our prediction

would reach probability 1; “but it is the unconditional form of causality which expresses *determinism* and which we must analyse” (Reichenbach, 1956: 83).

There is a logical relation between the sharpness of the values, and the prediction of the future state of the system.¹⁶⁰ Let D_1 be the description of the state of the deterministic system S at time t_1 , and a description that includes numerical values of relevant properties, and E_1 be the description of S at a later time t_2 . Let us say that we are mostly concerned with a certain measurable parameter, say the spatial volume of some object O . Now, based on our actual knowledge of the numerical value of O 's volume for $S(t_1)$ – say the volume of the object has x units – and the laws, we *predict* with a probability p_1 ($1 > p_1 > 0$) the volume of the object for $S(t_2)$, say it has a volume of y units.

We may achieve a higher probability p_2 ($1 > p_2 > p_1$) by replacing D_1 with D_2 , which is a more precise description of the values of all properties for the state of $S(t_1)$, including the volume. D_2 is more accurate because it uses, say, better measurements of the parameters involved, and/or uses an improved knowledge of the physical laws. Thanks to the more precise description D_2 , we now predict with a higher probability p_2 the value of the O 's volume for $S(t_2)$, and therefore achieve a better description E_2 of the state of $S(t_2)$.

Ideally, we may refine the process even further by replacing D_2 with D_3 , D_3 with D_4 and so forth (finding higher values for $p_3, p_4, p_5 \dots$). As D becomes more and more precise we are likely to move from macro to microstate descriptions of D_n . Determinism entails that for the probability p_n to be 1, we need to determine *exact* values for the volume at different times, by means of having an *ultimate* (complete) description $D_{n(t_1)}$.

The whole idea behind this is that if any of the values for the volume are indistinct, i.e. one cannot assign sharp values for O 's volume at different times, the system S would not be sufficiently set or fixed, or strictly speaking, deterministic. Then of course, an ultimate description for D would be unattainable. Perhaps for this reason Stone reformulates Reichenbach's definition establishing that for deterministic systems “the accuracy of a state

¹⁶⁰ Cf. Reichenbach (1956: 84).

description is infinitely refinable, even though any given state description will contain some error” (Stone, 1989: 125).

There is an important distinction to keep in mind. Often, when we say that the values need to be sharp, we mean that our measurements need to be accurate enough. This is an epistemic sense of value-determinateness (**C**), and for this reason cannot conflict with ontological determinism (**B**), as defined in the previous section. But **C**-style determinism may also have an ontological meaning: the belief that physical properties *have* determinate values, despite our (epistemic) failure to attribute exact numbers to (or precisely measure) all parameters. If the values are (ontologically) vague, we have value-indeterminism.¹⁶¹ This could, in principle, pose a threat to **B**-style determinism. I will come back to this issue in §3.2 and in §3.6.

3.1.4. Layer (**D**): Total Predictability

Layer **D** may be defined as the idea that if a system *S* is deterministic, and if we had complete and perfect knowledge of the laws, and perfect descriptions of the various states of the system at different times, and the mental apparatus necessary to understand all the complexities involved in the evolution of *S*, and perfectly perform all the necessary calculations, then the *S*'s trajectory would be entirely predictable, or logically deducible.

The probably best known formulation of determinism in terms of total predictability (**D**) is that of Laplace:

We ought to regard the present state of the world as the effect of its antecedent state and as the cause of the state that is to follow. An intelligence knowing all the forces acting in nature at a given instant, as well as the momentary positions of all things in the universe, would be able to comprehend in one formula the motions of the largest bodies as well as the lightest atoms in the world (...) the future and the present would be present to its eyes (Laplace, 1820, as quoted in Nagel, 1961, Footnote 4: 281-282).

¹⁶¹ There is a sense in which only the science of fundamental particles could be called deterministic as in **C**-style determinism, because all other sciences deal with the average of processes occurring at micro-scales.

We can redefine *Laplacian* determinism using contemporary nomenclature. We can say that it is the definition of a certain variety of determinism in which the state of the system S at time t_0 , together with the relevant laws, *uniquely determines* the state of S at any other time t_n - there is a *uniquely possible* (relative to the laws and the initial state) sequence of states of the world, and no *branching* (forward or backward) is allowed. If *Laplacian* determinism holds, then a complete description of a system S at a given instant, and the laws, completely *fix*¹⁶² the past and the future. This equals saying that the system is deterministic: given the way the world is 'now,' the 'future' cannot (logical impossibility) be otherwise than it will be. The definition starts as an ontological thesis, but then evolves into an epistemological account.

A similar formulation of determinism had already been put forward by Leibniz about a century earlier:

(...) everything proceeds mathematically - that is, infallibly - (...) so that if someone could have sufficient insight into the inner parts of things, and in addition has remembrance and intelligence enough to consider all the circumstances and to take them into account, he would be a prophet and would see the future in the present as in a mirror (Leibniz, as quoted in Cassirer, 1956: 12).

Also Dalmatian scientist Roger Joseph Boscovich developed, well before Laplace, a deterministic atomic theory based on a similar characterisation of determinism:

Any point of matter (...) must describe some continuous curved line, the *determination* of which can be reduced to the following general problem. Given a number of points of matter, for each of them, the point of space that it occupies at any given instant of time; also given the direction and velocity of the initial motion (...); and given the law of forces expressed by some continuous curve (...); it is required to find the path of each of the points, that is to say, the line along which each of them moves (...). A mind which had the powers requisite to deal with such a problem (...) could, from a continuous arc described in an interval of time (...) derive the law of forces itself; (...) if the law of forces were known, and the position, velocity and direction of all the points at any given instant, it would be possible for a mind of this type to *foresee* all the necessary subsequent motions and states, and to *predict* all the phenomena that *necessarily* followed from them (Boscovich, [1763] 1966: 141).

¹⁶² This needs to be expressed as a matter of the deducibility of a statement of the total state of the universe at a time $t+n$ (say) from a statement of its total state at t in conjunction with a complete statement of the laws governing the system.

The problem with these three definitions, according to Earman, is that they conflate determinism as an ontological notion (**B**) with prediction (**D**). This is a challenge for determinism, because if prediction fails, then it would appear that determinism fails. We can avoid this by keeping the two theses separate. As Earman explains, the conflation originates “from a failure to distinguish determinism – an ontological doctrine about how the world evolves – from predictability – an epistemic doctrine about what can be inferred (...) about the future (or past) state of the world from a knowledge of its present state” (2006: 1389).

Laplace, Leibniz and Boscovich seem to believe that the truth of **B** entails the truth of **D**. In other words, if a system is perfectly deterministic, they say it is (in principle) deducible by a powerful *ideal* mind. This entailment relation does not seem obvious to me. There is no contradiction into thinking of a possible world which is ontologically deterministic, and yet allows for no deducibility. Furthermore, layer **D** requires a *knower*, and I previously said that **B** is a doctrine about the evolution of the world *independent* of there being *any* knower.

Laplacian determinism is still a useful characterisation of determinism as **D**-style determinism. But remember that **C** added an extra requirement to the formulation of determinism. According to **C** determinism requires that all parameters of a system need to have sharp values. So, we can now redefine the formulation of **D** in order to include **C**. The predictability of a deterministic system involves: (1) all the relevant parameters must have sharp values, (2) knowledge of the applicable deterministic laws, (3) complete information of the state of the system at a given time, and (4) the ability to make the necessary calculations which yield predictions. If 1, 2, 3 and 4 obtain, then all states of the system (future and past) are logically deducible by a powerful (ideal) knower. Observe that layer **D** only holds if layers **B** and **C** hold. If **B** and/or **C** fail, then there can be no predictive determinism of the kind in **D**.¹⁶³

Total predictability is, of course, an idealisation. Practical predictability is, nevertheless, possible in many cases, and quite often is a task given to scientists.¹⁶⁴ We do possess sufficient

¹⁶³ Kellert says that also layer **A** must hold, i.e. complete knowledge of a system requires that it be quantified properly, and that the laws are formalised as differential equations.

¹⁶⁴ *Scientific determinism* is somehow similar to Laplace’s position but does not require infinite precision. It basically says that the deterministic theory the scientist uses to predict should say what degree of precision (value

knowledge of the laws and the initial state of some systems, in order to perform sufficiently accurate predictions.¹⁶⁵ But for some deterministic system (chaotic systems), our attempts to predict their future states consistently fail. This is by no means reason to infer indeterminism. Our failure to make predictions does not undermine determinism as a metaphysical tenet. This is the message I would like to highlight. One reason for our practical failure to predict is obvious; prediction is problematic because of our limited measurement devices and practical impossibility of assigning exact quantitative values to all the relevant physical parameters.

3.2. Additional considerations

All these four layers (**A**, **B**, **C** and **D**) offer distinct, and sometimes complementary, insights on the meaning of determinism. One of the most useful aspects of Kellert's exposition of determinism as dividable into different layers is precisely to show that such layers are separable. The separation is such that it is a mistake to think that if one layer holds, then the next layer holds as well, i.e., that one layer necessarily entails another – although it is reasonable to say that layer **D** presupposes layer **B**.

It may be the case that we have **A**, **B** and **C**, but still do not get **D**. In other words, there may be a system describable by means of deterministic differential equations (**A**), which follows a uniquely possible trajectory (**B**), and has determinate values for all relevant parameters involved (**C**), and of which we have a good deal of knowledge (of the laws and the state of the system at a time), but we consistently fail to predict its future states. Chaos theory offers plenty of examples of such systems.

Dynamic systems (**A**), on the other hand, are not real systems, so they obviously cannot have any implication on determinism as uniqueness of evolution (**B**), which is a real feature of the world. Similarly, by establishing unique evolution we do not imply – and this is an epistemic

determinateness) we need for our knowledge of the initial conditions, even before any calculations have been performed. Cf. Popper (1956: 36).

¹⁶⁵ Earman provides an example: “there is an interesting connection between determinism and practical predictability for laws of motion that admit an initial value problem that is well-posed in the sense that, in some appropriate topology, the solutions depend continuously on the initial data” (2006: 1389).

matter and therefore not part of **B** – that we can assign exact values for all the relevant physical parameters (**C**). I also said that layer **C** may have an ontological sense; parameters do have determinate values, although we may not know what these are. A minimal degree of value determinateness – from an ontological perspective – seems to be presupposed by **B**.

Earman explains this last point well: “Determinism does not presuppose sharpness of values, for we can understand determinism as a doctrine about the evolution of set or interval valued magnitudes as well as about point valued magnitudes” (Earman, 1986: 226). This requires a refinement of the definition given in **B**. The unique evolutionary path need not be linear but within a specified volume of evolutionary space. Earman also says next that “determinism does seem to presuppose some minimal amount of determinateness; if the world were entirely a froth of potentialities (...) one would be at loss to say whether determinism held or failed” (*Loc. cit.*).

Finally, I would like to once more discuss the relation between layers **B** and **D**, as I believe this is a possible point of contention, and it is of utmost importance to make clear the distinction between these two layers in particular. I said that the ontological view is separable from epistemological and semantic considerations. This is based on Montague’s view that a purely ontological characterisation of determinism would not necessarily imply deducibility. Montague explains that determinism would entail that “for any instants t_0 and t , there are sentences $\varphi(t_0)$ and $\varphi(t)$, expressing the state of the universe at t_0 and t respectively, such that φt is *deducible* for φt_0 , combined with the laws.” This, in turn, would require that there be as many sentences φ as there are instants t (real numbers). “But there are only denumerably many sentences, [and] there are more than denumerably many real numbers [in standard languages].” Montague then concluded that “Laplace’s allegation has been shown to be quite trivially false” (1974: 303-4).

In defence of views such as Laplace’s (which I say conflates the ontological thesis of determinism with the epistemological thesis), one may say that Laplace is only using an epistemic notion (predictability) to *make sense* of the ontological thesis (uniqueness of evolution), and it is a highly idealised notion, as previously explained. So, the ontological thesis of determinism would seem to be closely tied together with predictability *in principle*. This would,

for some, give deducibility/predictability an ontological status – I take this to be, for example, Kellert’s view. My attempt is, however, to provide a characterisation of **B**-style determinism that is purely ontological, and that does not make any commitment to there being deterministic theories, and an all-powerful ideal mind with the computability powers to perform the deductions. But I do concede that if the deterministic laws are indeed part of the system they govern (as a real relation of the system’s properties), and if there is such a powerful mind, and if there are *non-standard* languages¹⁶⁶ capable of providing all the necessary descriptions, then one may well argue that under such ideal circumstances, **D** perfectly captures the evolutionary history that is required in **B**. Nevertheless, I am mostly concerned with *practical predictability*,¹⁶⁷ or predictability in its ordinary, non-idealised, sense. The decision on the ontological status of deducibility is, for our purposes, not crucial.

3.3. *Determinism and chaotic systems*

The notion of chaos has changed over the centuries, and this perhaps explains why some people associate chaos with randomness or chance. In Classical Greek (*khaos*) it was defined as “abyss, that which gapes wide open, is vast and empty” (Harper, 2014). In late 14c., Old French *chaos* retained the sense of a ‘gaping void.’ Up to this point, chaos had a strong ontological sense, and was often used to define the nature of the universe before the ‘Creation.’ In around 1600 its meaning shifted and extended to connote ‘utter confusion’ (*Loc. cit.*) i.e. lack of order, a disorganised state of affairs.

Chaos theory employs a modern, mathematical use of the notion (c.1977), which contrasts sharply with its earlier meaning. In this modern sense, a chaotic system is simply one whose trajectory through state space is highly sensitive, or highly dependent on its initial conditions, in the sense that minor unobservable changes to these may yield major (observable) effects. There is no implication that the system is not organised, or that objective chance is present, although predictions of future states of the system are very difficult. There is

¹⁶⁶ Richard Montague claims that it would be preferable not to appeal to non-standard languages (1974, Footnote 1: 304).

¹⁶⁷ This will be an important discussion in Chapter 4.

something in the nature of a chaotic system that prevents finite knowers from improving their epistemic situation. So chaos theory has ontological and epistemic implications; chaotic systems may be entirely deterministic, and yet unpredictable (*deterministic chaos*).

The origin of this concept is to be found in that of linearity. In simple terms, the laws of nature are said to be linear if the so-called ‘effects’ are *proportional* to the so-called ‘causes.’ So, if linearity is the case, a billiard ball hit twice as hard as usual, moves twice as quickly, for instance, a case easily represented by a linear function. It would be correct to say that such a function represents an approximation, since we are not taking into account a number of variables; the friction of the ball with the table, or the slight differences in the angle the ball gets hit, or the resistance of the air, and so on. But if the system is sufficiently stable (not highly sensitive to variations) the linear function yields a sufficiently accurate prediction.

A number of non-linear problems in classical mechanics were solved, at least until the beginning of 20th Century, by approximation using linear functions. A good example is the case of planetary motion. In the case of two planets gravitationally attracting each other, classical physics can explain the attraction using linear functions, and this yields successful predictions. But when a third planet comes into play, classical mechanics mathematics cannot account for it. Astronomers practically dealt with the problem by using the attraction exerted by the sun on the second planet as a basis, while considering the third planet’s force a perturbation they could not account for. Their predictions were not very accurate, but approximate, because of the nature of the laws governing planetary motion.

Newton’s ‘three-body problem’ represents a case where a deterministic system – the planetary motion of planets – is governed by nonlinear laws. Therefore minor changes to the motion of Venus, for instance, could result in major unpredictable alterations to the motion of other planets. The meteorologist Edward Lorenz (1993) popularised the idea of high sensitivity to initial conditions (‘the butterfly effect’) using the example that predicting the weather is very difficult, because meteorologists deal with cases of deterministic nonlinearity, or *deterministic chaos*. The mapping of a chaotic system trajectory is very irregular, and might give the impression of randomness or chance, but in reality it is driven by deterministic forces.¹⁶⁸ There

¹⁶⁸ Cf. Heylighen (2002).

are two conditions for deterministic chaos: we need at least three dynamical variables, and the laws must be non-linear.

The failure to yield an accurate prediction due to the complexity of a non-linear system is epistemic. In the case of a linear system, the accuracy of any prediction depends on how well a scientist knows the laws and the relevant initial conditions: the more complete the information, the more accurate the prediction. Deterministic chaos acts as an amplifier; because any minor 'cause' yields a large effect, the predictions will be less and less accurate as time unfolds.

Because chaotic systems are unstable, any minor intervention may yield large unpredictable consequences, and yet this need not be the result of indeterminacy, but could be that of non-linearity combined with 'multiple variables' or 'interacting components.' The issue at hand here is not whether the instability is generated by genuine indeterminacy or deterministic chaos; the worry is about prediction (**D**). The meaning of chaos is somewhat independent from that of determinism and indeterminism, and only seems to conflict with one of the layers of determinism, as we shall discuss below.

3.4. Chaotic theory and the layers of determinism.

Bearing in mind the fourfold division of the senses in which the doctrine of determinism gets used, we may proceed to some useful comparisons. Chaotic systems are normally linked to layers **A**, **B**, and **C** (to some degree), but not **D**. Chaotic systems are representable by dynamical systems, they are shown to have a unique historical trajectory, or evolution, to have value determinateness, but are practically unpredictable. Therefore, if chaotic theory is true, then **D** does not obtain: "even a system characterised by point values that evolve along a unique trajectory according to straightforward differential equations will be unpredictable if small differences in initial conditions lead to widely separated trajectories later on" (Kellert, 1994: 62).

Chaotic systems are by definition highly sensitive-dependent on differences to the initial conditions, and because no measurements are in effect conducted with total accuracy, from an epistemic perspective total predictability is impossible. It is important to note, however, as Earman rightly says, such high sensitivity, which causes the chaotic evolution of a system, in itself “does not contradict the continuous dependence of solutions on initial data, and, therefore, does not undermine the task of predicting with any desired level of accuracy (...) assuming that error in measuring the initial conditions can be made arbitrarily small” (Earman, 2006: 1390).

The practical impossibility of predicting chaotic behaviour (evolution) is explained by the fact that our measurement are not perfect, and no matter how small the inaccuracy in ascertaining values for the initial conditions, the forecast of the future state of the world for a chaotic system (even assuming deterministic chaos) increasingly degrades as time unfolds. Epistemic determinism (predictability) fails.

Classical physics normally represents deterministic systems well, when it idealises problem cases in which perfectly accurate measurements, combined with layers **A**, **B** and **C** lead to total predictability.¹⁶⁹ This is idealisation in which we assume to have exact measurements, and powers of precise calculation (no average numbers, approximations, cutting corners, etc.), so that even slight alterations to initial condition could be accounted for (see Hunt, 1987: 132). Practical predictability of real system is never total predictability, and this is especially so in the case of chaos.

The dissociation of ontological and epistemic versions of determinism constitutes Earman’s central defence of the doctrine of determinism when compared to chaos theory: “determinism and prediction need not work in tandem; for the evolution of the system may be such that some future states are not predictable (...) although any future complement than the one fixed from eternity is impossible” (Earman 1986: 9).

In a later paper Earman says the following:

¹⁶⁹ There are, however, some problem-cases in classical mechanics which are understood as conflicting with determinism. Cf. Earman (2008: 817-829).

Ontological determinism is compatible with sensitive dependence on initial conditions. When measurement procedures for ascertaining the values of state variables are not error free¹⁷⁰ (...) the link between determinism and prediction is weakened and even broken. Furthermore, a strong form of sensitive dependence on initial conditions (...) plus the compactness of phase space implies ‘chaos’ (...) On a macroscopic scale a deterministic system can behave in a seemingly random and stochastic fashion. This raises the issue of whether and how critters such as us can be justified in believing that the stochastic behaviour we are observing is due to indeterminism in the form of an irreducibly stochastic element or to deterministic chaos (Earman, 2004: 27).

Our failure to predict the future is an epistemic matter, and a separate matter from determinism understood ontologically. It is, therefore, not incoherent to argue that *if* we had complete knowledge of all relevant physical properties of a given system, and complete knowledge of the laws, and had all the requisite powers of computability to make the deductions, *then* any apparently erratic behaviour (evolution) could be explained away as a case of deterministic chaos, and not a result of inherent indeterminacies of nature.

As previously said, the failure to predict with total accuracy need not imply the failure of **B**: it is clear that a system which behaves so as to manifest high dependence on even minor alterations to its initial conditions is practically unpredictable because scientists cannot foretell which unique trajectory the systems will follow. But that there is uniqueness remains a quite plausible theory, but **D**-style determinism is not plausible.

Because of the difficulty in obtaining **D**, for reasons previously mentioned, we might be well justified in trying to define determinism, fundamentally, according to the layers **A**, **B** and **C**. Of these, uniqueness of evolution (**B**) is the most important layer for Earman. Our inability to ascertain which uniquely possible path a chaotic system will follow over time has no effect whatsoever on the (metaphysical) doctrine that the future and past states of a deterministic system (even chaotic ones) are uniquely fixed according to the laws. It should now be easy to see that **B** is more fundamental; if **B** holds of some system, then we may expect to model a deterministic system’s rules in terms of differential equations (**A**), hope to attribute determinate values to relevant parameters (**C**), and perhaps talk of prediction under an idealised

¹⁷⁰ As is the case of any actual measurement.

model such as Laplace's. If **B** holds it would offer support for **A**, **C** and **D**, but not the other way round.

Because Earman's defence of determinism is metaphysical - and not the result of empirical observation – its fulfilment depends solely on “the structure of the world, independently of what we could (...) know of it (...) ontological determinism does have epistemological implications (...) but let us not confuse the implications of the doctrine with the doctrine itself” (Earman, 1986: 7-8).

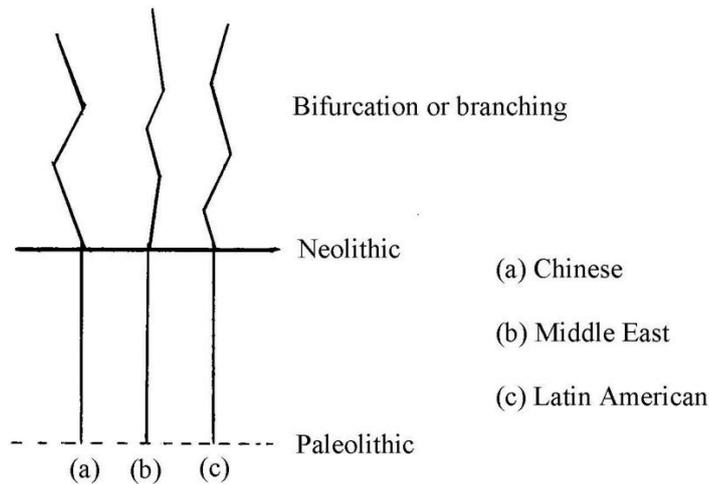
Kellert briefly mentions, in opposition to Earman, the work of Ilya Prigogine. Such a reference is a useful step towards our understanding of chaotic systems as allegedly posing a threat not only to **D**, but also to **B**. During an inaugural lecture titled “The Arrow of Time” (2nd of February 1999) Prigogine explains why he suspects the deterministic worldview, according to layer **B**, not to hold of the actual world. This has to do with so-called bifurcations in history – there are known phenomena represented by highly unstable systems. In such systems the role of probability is evident: “in front of a bifurcation, you have (...) many branches. The system ‘chooses’ one branch; if you repeat the experiment it may choose another branch. The choice of a branch is associated to probability” (Prigogine, 1999). So, if objective chance or probability is part of the equation, the future (and past) is not fixed, as it is governed by probabilistic rules.

As Prigogine then argues, this may give some useful insights domains other than the physical sciences:

(...) in *biology* (...) Jean-Louis Deneubourg has made very nice experiments (...). Imagine an ant nest, a source of food, and two bridges. You see that after some time all ants are on one bridge. Should you repeat the experiments, they may be on the other bridge. The mechanism is again an autocatalytic mechanism because each ant encourages the other ants to be on the same bridge. This is a very simple example of a bifurcation in biology. Also, *human history* is full of bifurcations. When we went from the Palaeolithic Age to the Neolithic Age due to the fact that humans could explore the resources of vegetation and of metallurgy, we may consider this as a bifurcation; even as a bifurcation with many branches, because the Chinese Neolithic is different from the Middle East Neolithic or the Latin American Neolithic (...) In fact, one can probably say that every time we find a new resource [coal, electricity...] (...) the world is reorganised, and we have a bifurcation (Prigogine, 1999).

In this brief excerpt, Prigogine has provided a good example of indeterminism in the history of society. This is not to say that the example is true, but it accurately describes indeterminism in history by rejecting uniqueness of evolution. I suggest the following illustration for Prigogine's claim:

Figure 2 - Bifurcations in the history of society



This diagram is, of course, an idealisation; we must think of (a), (b) and (c) being exactly alike up to the Neolithic, and equally obeying the applicable laws. The example illustrates what is meant by a bifurcation. Until reaching the Neolithic, the evolution or trajectory of Chinese (a), Middle East (b) and Latin America' (c) societies were the same – they followed the same unique trajectory. Then, with the agricultural revolution and the possibility of exploring new resources, (a), (b) and (c) evolved differently; we have a bifurcation or branching, there is not a uniquely possible path of evolution anymore. This fits the definition of indeterminism. As previously seen, if three systems share the same conditions at any given time t (which a , b and c do until reaching the Neolithic), and are governed by the very same laws, for determinism to hold all three systems need to have identical states at any other time t_n ; if they are dissimilar it's a case of indeterminism.

It is hard to assess the value of such insights into the non-physical domains of knowledge. In the case of biology, the ants' behaviour (in a certain respect only) could appear random at

macro-level, but it could be non-random (deterministic) at micro-level.¹⁷¹ In the case of history, the situation is even more complicated, and the ‘branching’ identified by Prigogine as starting from the finding of new resources need not occur at random. In the case of human history, it is also hard to see whether all the relevant variables involved can be properly quantified and ‘packaged’ into equations (probabilistic or not).

For Prigogine the deterministic worldview is false because strictly deterministic trajectories are not observable.¹⁷² This is a weak epistemic objection. **B**-style determinism can hold ontologically though it is not observable. Since our measurements always suffer from some degree of inaccuracy, it is never the case that we can specify with *complete precision* the state of *S* at a future time *t* based on our knowledge of the state of *S* at a previous time *t'* (*t' < t*). Sufficiently unstable systems (systems which are highly sensitive to changes to initial conditions) quickly render approximations obsolete.

Prigogine is in fact challenging the distinction between layers **B** and **D** (in its ordinary sense), claiming that these cannot be separate; so if **D** is false, then this also has to be the case for **B**. This strategy is mistaken if determinism is defined only as an ontological doctrine (**B**). Prigogine is not willing to concede the separation of the problem of determinism from that of prediction (as defended by Earman). In the case of highly unstable systems (chaotic) “everything takes place, physically speaking, *as if* the knowledge of ... [the initial] data would *not* determine the [system’s evolution]” (Hadamard quoted in Kellert, 1994: 65). As Kellert point out here, if systems behave *as if* the present does not determine the future, determinism seems to be challenged.¹⁷³

¹⁷¹ Prigogine’s example raises the question of the levels the laws operate, as previously discussed in §2.3. If we have a higher-level law which is probabilistic in nature, it does not preclude that events occurring at the micro-level of reality are not behaving deterministically. Or, we could have some level of indeterminism in a micro-scale of events, while the macro-scale exhibits determinism. The notion of *partial determinism* (§3.8) brings some clarity to this issue.

¹⁷² See exposition in Kellert (1994: 64). One might well argue that this claim is verificationist.

¹⁷³ Universal determinism is challenged, at least. What seems to be challenged is whether there are any actual systems which fit the definition. Maybe our actual world is not deterministic after all. Still, this does not go against the possibility of defining determinism in ontological terms. On the contrary, we need the ontological definition to make sense of the notion of indeterminism.

Earman's response to this is rather simple. If we look into the equations used in classical mechanics, and we are capable of seeing how real systems would satisfy the condition of uniqueness postulated by layer **(B)**, we have a plausible metaphysical definition of determinism, which is not challenged by the fact that total prediction **(D)** is not practically achievable – prediction and determinism belong to different categories.¹⁷⁴ As a consequence, chaotic systems alone only challenge epistemological accounts of determinism.

3.5. Value determinateness and quantum theory

Layer **C** presupposes that each parameter of a system has determined values, *i.e.* there are in fact precise, sharp, values for all observables, for any state of the system *S*, in any time *t*. Kellert's argument uses quantum mechanics in order to show that either determinism as requiring such sharpness of values is false, or we have to revise our definition for determinism once more, this time eliminating layer **C** as a requirement.

Can determinism still survive in a world where there is (ontological) value-indeterminism? Earman argues that it can be so; determinism is not undermined by the failure of value-determinateness. Even if the world is as quantum mechanics represents it to be: “a universe where systems with somewhat ‘spread out’ properties evolve along uniquely determined paths retains coherence” (Kellert, 1994: 68-9).

This is a point of contention in the sense that the minimal amount of value determinateness necessary for a system to be characterised as deterministic seem to be arbitrary. I can imagine that some philosophers would maintain that even a small amount of value indeterminateness would account for a system not being, strictly speaking, deterministic.

How much value determinism is required by uniqueness of evolution is an important issue for which I can offer no contribution. This, however, does not have a great impact on the definition of determinism in the human sciences, which are qualitative, and not quantitative in nature. The reason I briefly present the debate, is solely to demonstrate that even if value

¹⁷⁴ Again, this only relates to one reading of **D**, *i.e.*, predictability in its ordinary sense. It does not apply to **D** as an idealisation.

determinateness fails, one cannot easily conclude that the ontological version of determinism is false.

One distinction to keep in mind is that value-indeterminism can be simply an epistemic doctrine – we do not know what more precise values can be obtained though they are there to be got. But it could also have an ontological meaning, as some may argue that some properties are themselves vague in their values. The latter may be a difficult doctrine to maintain.

3.6. Unique evolution in the light of chaos theory and quantum mechanics

Chaotic systems may be deterministic, and, for many cases, we still cannot obtain total prediction (**D**) because of our limitations to measuring the observables with infinite accuracy. So a chaotic system is deterministic as long as our definition of determinism is confined to layers **A**, **B** and **C** only. Chaos theory alone only poses a threat to determinism as defined in **D**.

Kellert suggested that quantum theory, if correct, goes against the idea that all observables can be assigned sharp values – value indeterminacy. Quantum mechanics poses an immediate threat to layer **C**. What happens when we combine the notion of systems which are highly sensitive to minor changes to their initial conditions - chaotic systems - with the intrinsic value indeterminism of quantum mechanics?

We may explain the difference between classical physics and quantum mechanics by representing the state (x) evolution of a given physical particle. We say that at time t_0 the particle's state is represented by x_0 . If the system is deterministic (in accordance to layers **A**, **B** and **C**) then the trajectory of the state of the particle, as time unfolds (t_0, t_1, t_2, \dots), is fixed and will exhibit an identical (single-line) evolution.

This is not the case for quantum systems. The trajectory of x cannot be given by a 'point' in a chart, but it has to be represented as a function $f_{(x)}$. As Kellert explains, the function contains all the information there is about the particle's state: "the uncertainty relations tell us that specifying this state-function suffices only to associate the particle with a patch, not a point." (1994: 69). In other words, all the physical information available, according to quantum

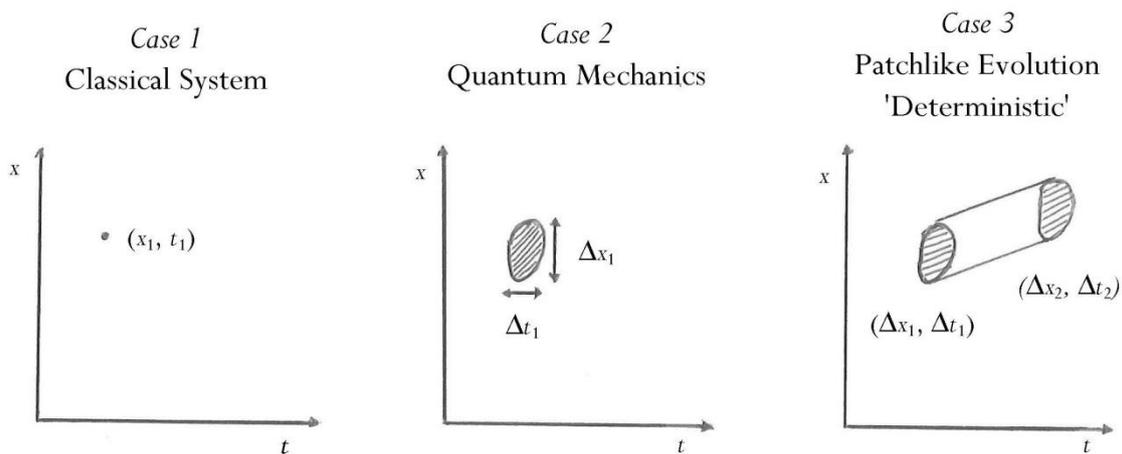
mechanics, is only sufficient to identify the state of the particle (x) within a finite area (not a point) of state-time.

Does this defeat determinism?

Earman has an interesting reply to this view. He reformulates his original defence of determinism in terms of unique evolution and says that it is not linear, but *patchlike*, and the patch is really a small volume in space-time.¹⁷⁵ We have to expand the definition of ontological determinism (B) in the following way; two systems are said to be deterministic if they evolve along the same volume in space and over time (of nonzero thickness) at all times. The objects in the system do not need to have exact positions, but their possible spread out of positions are fixed, i.e., the historical trajectory of a system is fixed within the patch, i.e., volume of space-time. In other words, $f_{(x)}$ uniquely fixes the evolution of the patch for all times t_n . In a deterministic universe, if two worlds w and w' have identical patch regions, and both worlds are ruled by the $f_{(x)}$, w and w' shall exhibit identical patchlike evolution.

The difference between the state-time (x, t) representation of a particle p in classical physics, and in quantum mechanics, is illustrated below:

Figure 3 – State-time representation, Cases 1, 2 and 3



¹⁷⁵ See Case 3 in the following figure.

Cases 1 and 2 are my adaptations from Kellert (1993: 70); they represent respectively, the precise location of a particle at a place and time, and the location of a particle within a small extended area of place and time.

Case 3 is my illustration of Earman's position (1986: 226-33). An informal way of expressing the idea of this diagram would be the following. Consider the initial state of two system S and S' . Suppose they start not at the same precise point but they do fall within the area of the patch $(\Delta x_1, \Delta t_1)$. Suppose further that they evolve over time not to exactly the same point in $(\Delta x_2, \Delta t_2)$ but to a point again within the second patch. That is, the evolutionary histories of each system, while not the same, are allowed to deviate slightly but only within the 'cylinder' of space-time as depicted. Then these two systems can be deemed to be deterministic. This is an expansion of the original definition of ontological determinism given for layer **(B)** that allows for some limited deviation in historical trajectories.

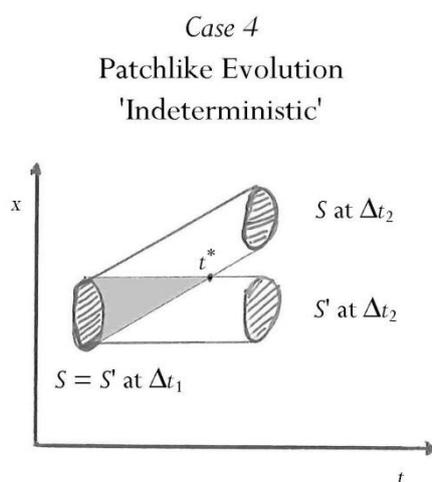
Even in a quantum world, determinism as uniqueness of evolution may still hold, believes Earman, despite the fact that we cannot assign exact values for the state of the particle – just a range of possible values. According to Earman's modified account of **B**-style determinism, and as long as the range of possible values is sufficiently small, and not arbitrarily large, we may still be entitled to characterise the evolution of the patch (*Case 3*) as a case of a deterministic evolution within bounds.

Kellert's conclusion is that chaos theory and quantum mechanics, independently, do not defeat determinism as an ontological doctrine **(B)**, but when they are associated they do. Kellert explains that two identical chaotic systems will eventually diverge greatly at some point, due to the combination of value indeterminateness and high degree of instability. Therefore even the patch's trajectory of x for two identical systems at t_1 , will not follow the same unique path for all times t_n ; despite sharing identical initial conditions and being ruled by the same laws (or function), both systems will (or are likely to) evolve differently.¹⁷⁶

See the diagram below:

¹⁷⁶ Into different patches' trajectories.

Figure 4 – State-time representation, Case 4



In *Case 4* we have indeterminism, according to Earman's modified account. Two systems S and S' , are governed by the same laws, and are identical at Δt_1 . But because the systems are unstable, and the values are somewhat vague, the trajectories will eventually diverge. It is not possible for S and S' to have the same value (or closely related values) for the state x at any time later than t^* , as both trajectories no longer overlap.

Alternatively, imagine that two worlds have identical initial conditions (same relevant properties) at t_1 , and both worlds are ruled by the same invariable laws. Layer **B** of determinism only holds if the evolution of w and w' is identical, but given the instability of such systems, and the value-indeterminacy, the antecedent conditions can never be met. After the unfolding of time, the trajectory of the patches will be widely distinct: in w a tsunami will hit the coast of New Zealand at a time t , and in w' it will not; in one world Caesar will fall in love with Cleopatra's nose, in other he will not... At a strictly theoretical level, the association of quantum mechanics with chaos theory may eliminate layers **B**, **C** and **D** and defeat determinism as such defined, leaving us with unpredictable and open-ended trajectories.¹⁷⁷

Earman has a response to this challenge; he admits the challenge of quantum-chaos but says that no such system is yet *known*. Here, Earman response takes an interesting turn. Instead of retreating into the metaphysics, he now defends the possibility of determinism by appealing

¹⁷⁷ Something that in my view does not entail that chaotic-quantum events are not caused. Lewis's theory of causation can account for an antecedent event a causing e , even if uniqueness of evolution is false.

to the fact that quantum chaos has not been properly observed,¹⁷⁸ and that we know so little about it that the combination of both theories could also prove inconsistent (this is clearly an epistemic approach/response).

I cannot offer a satisfactory contribution to the Kellert-Earman debate about the consistency of quantum-chaos and its implications for the doctrine of determinism in the domain of the physical sciences. My point is solely to say that determinism, even in physics, may be characterised as uniqueness of evolution, but still allow for some degree of bounded variation in evolutionary history when it comes to assigning sharp values to the relevant parameters – but it requires us to accept Earman’s idea of a deterministic patchlike evolution.

3.7. Kellert’s notion of local determinism

Earman’s position is that if unique evolution (**B**) is taken away, then we are left in a world where physicists would need to appeal to strange elements such as blind chance to provide understanding of quantum phenomena. But for Kellert, layer **A** alone, or what he calls differential dynamics as *local determinism*, would suffice to avoid Earman’s worries. As Kellert explains the appeal to “nonstochastic differential equations to understand physical change urges us to seek *local connections* between events without concern for the global property of unique evolution” (Kellert, 1994: 75).

This solution basically entails that at a local level, a subsystem, it would be possible to fix the rules for a differential dynamic system so that no branching is allowed (no chance or probabilities involved). In other words, no branching of a systems’ trajectory is no different to defining determinism as (local) uniqueness of evolution. Research into chaotic systems “takes place wholly within a context of local determinism (...) chaos theorists seek to understand complex behaviour by building models that obey precisely the strictures of differential dynamics” (Idem). This fact alone seems to contrast with an important idea that permeates

¹⁷⁸ The reason for this might be the case that studies in quantum chaos are fairly recent; and most problem cases try to elucidate chaotic behaviour in classical systems, and non-chaotic behaviour in quantum mechanics. Cf. Gutzwiller (1992).

Kellert's and Prigogine's position; that failure to prove determinism would result in a decrease of its methodological importance.

Local determinism is not without its problems. Even if we only consider a sub-system, for which we observe the evolution of, say, the state of a particle over time, any small amount of value indeterminacy can account for huge variations as time unfolds, resulting in an idealisation of deterministic behaviour. On the other hand, it may be that stochastic behaviour is intrinsic to quantum theory; therefore even local determinism is unattainable.¹⁷⁹

3.8. *Partial determinism as a better definition*

I wish to bring attention to the fact that talking about *local determinism* is not an entirely original solution, it only denies that there is something like a global or universal uniqueness of evolution. Earman says wisely that determinism “needn't be an all-or-nothing affair. A world may be partially deterministic, deterministic with respects to some magnitudes (...) but not with respect to others” (1986: 13).

Earman also provides an example of such a world. We may imagine that a world is only *partially deterministic* because:

(...) it is deterministic only with respect to the magnitudes which characterise the ordinary matter of which we and our scientific instruments are composed but not with respect to the magnitudes which characterise the behaviour of a free-spirited species of particle, the freeon, say (...) The world might be *non-deterministic* but still *conditionally deterministic* on a subset of magnitudes: if two worlds agree for all times on the values of the conditioning magnitudes and if they agree at any instant on the values of the other magnitudes, then they agree at any other instant (...) (*Loc. cit.*).

The central idea here is that, at a conceptual level, it is coherent to say that the universe could be only partially deterministic, i.e. deterministic only “with respect to partial histories defined by the values of magnitudes in some proper subset $D \subset O$ of the occurrent physical

¹⁷⁹ This need not mean that in an indeterministic world events cannot be explained; only that predictability has to be replaced by new form of explaining probabilistic phenomena. Chaos theory does provide understanding of indeterministic events. Cf. Kellert (1994: 77-118).

magnitudes but non-deterministic with respect to partial histories defined by the values of magnitudes in some other proper subset $N \subset O$ " (Earman, 2006: 1371). In Earman's example D and N are subsets of magnitudes, or partial (sub) systems, part of a larger set of magnitudes O of the system S . Partial determinism means that S may evolve in a certain way that some of its subsets evolve deterministically, while other subsets are indeterministic.

There are some difficulties to be dealt with here. One has to assume that it is theoretically possible that a system evolve like this, in the sense that the indeterministic subset does not ever interfere with the deterministic subset. It is however difficult to imagine a scenario of partial determinism if D and N are so-called basic magnitudes; the indeterminism of the evolution of the magnitudes in N must not ever interact in any possible way with the deterministic evolution of the magnitudes found in D , otherwise D could not possibly have a unique trajectory. If such isolation of D is implausible, then we could only rescue D 's deterministic evolution by postulating a theory showing that any upsetting effect of N on D are somehow cancelled out.

I wish to contend that there is really not much difference in postulating the possibility of a nonstochastic differential equation (local determinism, as Kellert names it) and the world being only *partially* deterministic, or *conditionally* deterministic on a subset of magnitudes (as Earman names it). Research into quantum mechanics might pose reasonable doubt as to the plausibility of global uniqueness of evolutionary trajectories; but determinism is still to be defined as the non-branching trajectory of a system. It is just not possible to refer to determinism without layer **B**, I believe.

It may be that after all things are considered, we are incapable of identifying any single system, in the actual world, which fits our definition of determinism. If nothing fulfils the definition, then there is indeterminism. So be it. But we cannot say with certainty, at this point, that such failure is not simply the result of our cognitive limitations.

3.9. Final remarks.

The primary aim of this chapter is to spell out in some detail the different characterisations (layers) of determinism in the physical sciences. Such definitions of determinism should be applicable, however, to any systems, from quantum mechanics to systems in the human sciences. Kellert concluded that determinism survives only as an ideal model of reality (**A**), so that layers **B**, **C** and **D** do not hold of the actual world, in the light of contemporary physics. Other authors, such as Karl Popper (1956), believe that determinism is the combination of *all* these layers (**A**, **B**, **C** and **D**). For Clark Glymour (1971) and Mark Stone (1989), determinism means the combinations of **A**, **B** and **C**, doing away with **D** only. John Earman (1986, 2004, 2006, 2008), however, contends that only **B** would suffice for determinism to hold – a position I see as perfectly coherent.

Furthermore, in association with the idea of determinism we have that of causation. As it is usually believed, in a totally deterministic world, if *c* causes *e*, then *e* occurs out of necessity. Not so if we have only partial determinism. If partial determinism holds true, then some events will be caused as a result of necessity, while others will be caused as a result of a combination of antecedents and probabilistic laws (or objective chance). I argued that *partial determinism* is a promising alternative, and is it a similar solution to Kellert's notion of 'local determinism.'

Certainly, this fourfold division is more clearly identifiable in the realm of the physical sciences, than it is in the case of non-quantifiable domains, such as history. Yet, I believe that also historians would benefit from an understanding of how such layers may be separately characterised. Because history it is not a quantitative science it may be said that only layers **B** and **D** really apply: the former is the ontological view that the world evolves according to a uniquely possible trajectory, the later the epistemic idea that knowledge of the initial state of the world, combined with the laws, would allow us to deduce (predict) its future state. Clearly, a scenario where **B** holds while **D** does not, may well be the case.

Now that we have established the different senses of determinism in the physical sciences, we may pursue how determinism is used in the human sciences, and why exactly is there hostility against the idea that historical systems may be determined. As we shall see, many

critics of 'predictability' (epistemic determinism) in history abandon the deterministic assumption (ontological determinism) altogether because our best social theories do not allow for accurate predictions; others attack historical determinism because of the alleged incompatibility with free will. Both positions are ill-founded. The precise characterisation of determinism in the physical sciences will now help us, I believe, bring clarity to some issues regarding historical determinism. Bearing the ontological/predictive distinction of determinism in mind, it shall be easier to show that, in principle, history, like any other system in nature, may be ontologically deterministic.

Chapter 4

Determinism in history

4. Historical determinism

Before advancing any further on the issue of historical determinism, I would like to restate what this thesis is about, and what the function of the present chapter is. My basic aim is to defend the view that historical explanations are best when they are causal explanations. My specific consequent aim is to rebut arguments to the effect that causal explanations in history would imply determinism, or that determinism in history is to be rejected.

Various things can be done in pursuit of this specific consequent aim. (1) Clarify what determinism means and what approaches to historical knowledge are usually considered to be deterministic. (2) Show that historical explanations may be causal (a) without implying physical determinism, but (b) be consistent with it. (3) Show that historical explanations can be causal without implying historical determinism, but be consistent with it.

In the previous chapters I defended the view that causation need not be deterministic (Lewis's account of causation is compatible either with determinism and indeterminism), and that there are different varieties of determinism, and that one of them (ontological determinism) has not been refuted by advances in scientific and philosophical knowledge. In this chapter, I focus on the idea that even if we take causation to be a deterministic relation (though it need not be), there is nothing fundamentally wrong with this idea. Popular arguments raised against causal determinism in history miss the point when determinism is taken to be an ontological doctrine.

4.1. *The problem of historical determinism*

In the previous chapter I concluded that determinism should be fundamentally seen as an ontological doctrine about how the world runs, or evolves. In Kellert's terminology, my view is that it is ontological determinism (layer **B**) that characterises the most basic claim of determinism: a system is deterministic if it has a uniquely possible history or trajectory. In my prior exposition I did not make a proper distinction between the natural and human sciences; 'systems' covered both *human* and *natural* phenomena – e.g., the collision of billiard balls on a pool table, the planetary motions, the global financial markets, soldiers in a trench, and so on. The layer **B** of determinism can apply to any of these cases.

There are, however, important methodological differences between these two realms, and differences in what is required for explanation and prediction. These differences do not, however, preclude the application to both human and natural explanations of the same definition of determinism as *uniqueness of evolution* – that is, for a deterministic system there is but one evolutionary history given the laws and the initial state.

There is plenty of hostility against determinism in the human sciences, and we have already exemplified such hostility in the work of Isaiah Berlin (2002). The origin of this hostility rests on a number of popular assumptions or arguments – for example: that universal determinism must be false; that the laws governing human behaviour, if there are any, cannot be deterministic because of the problem this would pose for free will; that human (historical) events cannot be explained in the same way as natural events; that prediction in human sciences usually fails; to mention just a few. For reasons such as these it is concluded that historians should reject all varieties of determinism.

But if we agree that determinism is an ontological thesis, it follows that failing or succeeding in predicting or explaining does not prove or disprove the doctrine. By properly characterising and separating the ontological and epistemological theses of determinism, we are in better position to handle these issues. This is why I believe the identification of the varieties of determinism, as set out in the previous chapter, will now serve us well. The arguments against the possibility of determinism in history we are going to discuss, have remained popular throughout the last five decades. For this reason, it is still important to provide new and better replies to each of them.

The primary objective of this chapter is to consider whether historical systems may be ontologically deterministic. The answer is yes, because so far there is no reason as to why one should reject it. Criticisms raised against determinism in history are usually a muddle, because of the confusion between causation, causal explanation, and determinism. Critics of ‘historical determinism’ sometimes appeal to the fact that historical ‘explanations’ do not conform to, say, the covering-laws model, that such explanations are always incomplete, that explanations of human behaviour resist capture under laws, among other claims.

These are all relevant issues indeed, but they do not give grounds for rejecting determinism as an ontological thesis (as in **B**-style determinism); these are discussions about the ideal logical form of an explanation in history, an epistemic matter. It may be the case that the world is not entirely deterministic (ontological), and still the best explanations for some classes of phenomena (large-scale social events, such as migrations, for example) are provided according to a certain outlook on history which says that we are to look for regularities or patterns (but not precise deterministic *predictions* as in the case of layer **D**). Maybe this is so because some historical systems evolve deterministically, or approximately so, in sufficient isolation from the world’s indeterminacies. We may have sufficient ground to doubt of the doctrine of universal determinism, but determinism need not be an all or nothing affair, as we previously discussed. Deterministic systems may coexist with indeterministic systems as long as they are sufficiently isolated. Or we may have indeterminism at the level of, say, sub-atomic particles, but the laws operating at a higher level are adequately characterised as deterministic.

As J. R. Lucas pointed out, historians sometimes become ‘determinists’ in the epistemic sense because “they are committed to explaining historical events, and explanation, they feel, involves determination. If a historian can explain why an event took place, he is explaining why it *must* have taken place” (Lucas, 1970: 51). This kind of explanation is at times confused with historical *inevitability*, or fatalism. It is well known, at least among philosophers, that to provide a regularity kind of explanation, in the *Humean* sense, is quite different from fatalism.

Even if we do concede that, in the case of human affairs, law-like explanations are usually not possible, I argued that Lewis’s view of causation and causal explanation would be a suitable alternative as it does not require determinism to hold, and allows us to talk about probabilistic causation. Whether a cause necessitates its effect or not is a question about how the

world runs – whether determinism is ontologically true of the actual world, or not – but a good causal explanation, in history as in general, need not dwell on this. Historians may pragmatically point out an event’s cause or causes, but their explanations succeed, if they do, irrespective of the question whether the laws of nature are deterministic or only probabilistic.

My analysis of historical determinism starts with a description of the failed effort to identify laws of historical development (§4.2). I claim that strictly historical laws do not exist – a fact which does not by itself undermine determinism. I then proceed to a famous analysis of the arguments against determinism in history by Ernest Nagel (1960) in a key paper on the subject. It is rather surprising that an article written more than fifty years ago is still a key paper on the defence of the idea that also history may be, in an ontological sense, deterministic. The reason why Nagel’s paper is not an ‘old hat’ is rather simple: the arguments against historical determinism have not changed much since then. In a fairly recent and very popular book, Ferguson, for example, attacks some versions of historical determinism on the same old grounds that deterministic outlooks, such as the Hegelian and Marxist approach to history, would seem to deny the role of individual agents, and chance (1999: 20-90).

Once we settle for a view of determinism as in **B**-style determinism, it shall be easier to re-examine each of the popular arguments raised against determinism in history (§4.4.1 to §4.4.5). In this paper I will revisit the arguments analysed by Nagel (1960), hoping to add some value to them, based on more recent philosophical discussions. There are some aspects I do not think have been satisfactorily explained, or sufficiently explored. Overall, Nagel’s article is sound and convincing. But I do feel that Nagel’s characterisation and defence of the possibility of determinism shifts throughout the essay, as we shall see.

Some of the issues to be investigated have already been partly discussed in my previous chapters. In order to avoid undesirable repetition I will here presuppose knowledge of the conclusions we drew back then, and focus on yet unexplored aspects of these arguments. Particular attention will be given to the argument about *novelties* in history. In §4.4.3.1 I will explain how the theory of convergent evolution may help illustrate how the emergence of novelties is perfectly compatible with determinism.

4.2. *Laws in/of history*

A question that emerges from the previous chapter is whether determinism as the doctrine of *uniqueness of evolution* (**B**) can also apply in the case of the subject matter of the human sciences, especially that of history. Ontological determinism would seem to imply that laws, whatever they are, are non-stochastic or deterministic. It may seem problematic, however, that history should be subject to such laws, for two correlated reasons: (a) it is hard to come up with empirical generalisations that might have the status of such laws, and (b) the uniqueness of historical events prohibits the corroboration of such laws (see also §1.3.1).

Before proceeding, I need to make an important distinction between (1) laws *in* history and (2) laws *of* history.

In the case of (1) we refer to any law which might be applicable to explain the causes of a historical event. As a matter of fact, many explanations in history appeal to well-known regularities. I previously said that historians would benefit from a general theory of explanation that does not *require* subsumption under general-laws; it is not precluded that laws may be used in historical explanations.

One of the best defences of the idea that historians should also look for general laws or hypotheses is to be found in Hempel's famous paper "The Function of General Laws in History" (1942). This article, which defended the idea that there is a *logical* similarity between explanation in the natural and human sciences, has often been misunderstood by historians and philosophers of history; it attracted back then and still today, criticisms. (Cf. Little, 2012). William Dray (1957), for example, believed that historical explanations are different from explanations in the physical sciences, and more akin to *rational* explanations of human behaviour,¹⁸⁰ and explanation focusing on offering a comprehensible understanding of why agents act as they do, and not law-like explanations. As an alternative, historians should simply attempt to identify or 'trace out' causal chains, and explain the particular causal mechanisms in place, without implying that causal explanations require general-laws.

¹⁸⁰ When we provide an explanation based on rational principles of behaviours, however, we may try to formulate such principles as generalisations, or theories.

We are not going to examine what the *Hempelian* logic of explanation is, and why historians, in general, reacted against it – there is vast literature on this topic already. For our purposes, it suffices to say that the kind of laws Hempel suggested historians should look for, are not laws of history; these are laws that may come from anywhere, such as physics, sociology, economics, and psychology. A similar position can be found in von Wright: “we should think of [laws in history] as instantiations of general laws of sociology, and perhaps of economics, than as ‘laws of history’ proper” (von Wright, 1976: 434).

Our focus in the section will be on the claim (2), the idea that history is governed by laws of historical development; i.e., laws that *only* apply to historical phenomena. The idea that there are all-encompassing laws of history is to be found in the so-called *substantive theories* of history. Daniel Little defines such theses as any approach aiming “to discern large, embracing patterns and directions in the unfolding of human history, persistent notwithstanding the erratic back-and-forth of particular historical developments” (2012). Sometimes it is the view that humankind evolves toward an ultimate end (history has ‘direction’), or it could be the view that there is a fixed order of change which has been repeatedly manifested in all human societies.

The philosophy of history of Hegel is one example of such a theory. In Hegel’s *Phenomenology of Spirit* ([1807],1977), he advocates a particular view of determinism in history: he believed that something like *Divine Providence* was the determining force of all that occurs, and that history’s evolutionary trajectory is predetermined by the ‘purposeful’ movement of what he called the *Spirit*. Nagel identifies Hegel’s position as originating from a theory of civilisation “which finds the causes of human progress or decline in the operations of impersonal factors such as geography, race, or economic organisation” (1960: 291). The facts of history are immutable facts for Hegel. Either way, nowhere is there to be found a neat formulation of the law of history propounded by Hegel. In its place, he offered an explanation that the unfolding of history is caused by this obscure entity – the *Spirit* – who determines everything according to a Divine ‘purpose.’

Another example of a substantive theory of history is Marxism. Marx did not postulate the existence of *supernatural* forces, but instead “a general law to which all social revolution (epochal transition, in his terms) conforms; a general law of the same sort as a natural scientist

might propose to cover some uniformity in nature” but a law that “is not merely a statement of a regularity, but also a law of progressive development” (Enfield, 1976: 267).

Let us see how Marx and Engels ‘formulate’ such law:

At a certain *stage* of their development the material productive forces of society come in conflict with the existing relations of production, or (...) with the property relations within which they have been at work hitherto. From forms of development of the productive forces these relations turn into their fetters. Then begins an *epoch* of social revolution (...) No social order ever perishes before all the productive forces for which there is room in it have developed; and new, higher relations of production never appear before the material conditions of their existence have matured in the womb of the old society itself (Marx and Engels, 1958: 363).

It is easy to see that Marx and Engels’s formulation of such a law is unsatisfactory.¹⁸¹ One of the reasons is that they talk of a certain *stage*, or *epoch*, but do not say what these are. The laws of history, according to the standard interpretation of Marx’s views, is said to govern not all historical events, but the passage from one *stage* to the other. Little is said about the law itself, except that it is to be understood in materialistic terms. In other words, Marx’s law of history is not formulated as a neat, testable statement. The famous prediction that *a* Revolution will occur, once society reaches a certain *stage*, whatever it is, is too vague to be either confirmed or refuted.

Other substantive approaches can be found in the works of Immanuel Kant (1883), Arnold Toynbee (1976) and Reinhold Niebuhr (1949). Substantive theories of history in their various forms, share one important premise: that individual voluntary action is, in general and in the long run, incapable (powerless) of significantly changing the course of events, or the ‘tide’ of history. This is because for such doctrines historical changes are produced by underlying forces, which in turn presumably conform to some larger pattern of historical development. But such theories are not formulated as testable statements; therefore we may reject them on the grounds that they lack any empirical support. The simple identification of some historical trends is insufficient to validate the claim that the function of history is to discover the existence of a greater plan for mankind, or any over-arching law *of* history.

¹⁸¹ It is contentious whether Marx thought of himself as having found the laws of history; some argue that this was a later formulation/interpretation following Engels’s attempt to make Marxism seem more ‘scientific.’

It is hard to offer examples of what a law *of* history would be. The authors we just mentioned talk about the idea that historical *necessity* is a fact of the world, and that there is an all-encompassing law capable of explaining the trajectory of human affairs, and also capable of giving it ‘meaning.’ But the terminology used to refer to such a law is obscure. Maybe this is so because substantive theories patently failed in identifying any law of history.

Ludwig von Mises was an early theorist to acknowledge that historians consistently failed in identifying laws *of* historical development:

The formulation of historical laws, i.e., laws of historical change, has repeatedly been designated as the task of history. Many even set out to formulate such laws. Of course, these laws did not meet the demands one must make of a scientific law. They lacked universal validity ([1933], 1976: 118).

A less famous defender of the existence of a law of history was Edward P. Cheney. After studying the causal importance of the actions of some historical agents during the American Revolution, he concluded that:

These great changes seem to have come about with a certain *inevitableness*; there seems to have been an independent trend of events, some inexorable *necessity* controlling the progress of human affairs (...) the personal, the casual, the individual influences in history sink in significance (...) Events come (...) so consistently and unavoidably as to rule out as causes not only physical phenomena but voluntary human action. So arises the conception of *law* in history. History (...) has not been the result of voluntary [action and] chance; but has been subject to law (Cheney, 1927: 7, as quoted in Nagel, 1960: 291, and also in Walsh, 1962: 25).

In his essay, which was initially written as a presidential address to the *American Historical Association* at Columbus, Ohio (December 27, 1923), Cheney elaborates on his idea that chance and voluntary action do not play a fundamental role in the unfolding of history. History, just like other sciences, relies on finding certain laws, and the way to find these is, according to Bieber “to use the method of other sciences-to consider the phenomena, make a guess at some large principle, test it by a wider comparison with the facts, and then make a generalisation which one can fairly call a law of history” (1928: 164).

Cheney did not explicitly say that there is no *chance* or *free will*, but that actions are rendered powerless by some *necessary* and *independent* trend of events. This raises an obvious

question: How can an *independent* trend of events affect human action, and remain independent? Is a historical law a special kind of law? Cheney's characterisation would be a good example of what Berlin (2002) called a thesis of *historical inevitability*. Berlin has directed his criticism mostly against Hegelianism and Marxism; but Cheney would have been an equally suitable target. Cheney's remarks seem to be of *fatalistic* nature, especially when he talks of an 'inexorable necessity' of human affairs. There is no proper argument for determinism here, only the unjustified claim that history will lawfully evolve *irrespective* of the workings of chance and voluntary action – which do exist, but for some reason are causally ineffective.

In his own attempt to 'discover' historical laws, Cheney talks about six important 'guesses' (generalisations), which he hopes may yield laws of history. The six generalisations are the following. (1) 'Continuity' – all historical events are caused by immediately preceding events. (2) 'Permanence through change' – nations possessing the capability of adapting to changes (environmental, for instance) are more likely to survive. (3) 'Interdependence' – the human species is a unit, and progress is never made when one group injures another. (4) 'Democracy' – all forms of government will eventually evolve into democracies. (5) 'Freedom or consent' – humans are free because they cannot be continuously compelled to act in a certain way, it is consent,¹⁸² and not coercion, what holds societies together. (6) 'Moral progress' – moral influence will become more prominent, in human affairs, than material interest (Cf. Cheney, 1927: 245).

Cheney's generalisations are controversial and romantic, and his guesses are problematic for different reasons:

(1) This is not a law, but the causal principle as applied to historical events (or any event). This is just the thesis that all events are caused. The causal principle as such is not directly testable, as we cannot show all events to have causes. But we may have good grounds to believe that it may be true for most cases we observe, even if quantum mechanics says that some events occurring at sub-atomic level are uncaused.

¹⁸² Cheney seems to adopt some sort of compatibilist position. The law of history operates at a higher, emergent level. Free action operates at a lower level and is causally ineffective when it comes to interfering with the general trajectory of historical development.

(2) This could, in principle, be tested, providing that a nation's capacity of adapting to change could be identified independently of the nations' merely surviving; otherwise if the identity conditions are linked the statement becomes untestable. Is there a good reason to think that individuals, or societies, would adapt any differently should they belong to different nations? It might be argued that political, religious and cultural differences would affect the conditions of survivability of the entire group, or nation. But it is hard to see how this thesis could be practically tested, as the variables involved are just too many.

(3) Seems false. Progress – technological, material, and scientific – is often made at the expense of one group injuring another. Humankind has also a strong tendency of not seeing itself as a 'unit.'

(4) Borderline testable, as 'eventually' is too vague as a timeline. The statement could, in principle, be verified, but not falsified.

(5) Borderline testable; it is hard to see how 'consent' could be sufficiently isolated from other factors in order to determine what it is that 'keeps societies together.'

(6) Same case as (4), evidence seems to be contrary, nevertheless.

In favour of Cheney's generalisations we may say that he truly attempted to provide testable statements, but they either fail, or are not clearly testable.

The conclusion we may draw from our critique of Hegel, Marx, and also other substantive theories of history, is that attempting to explain the past in the light of an a priori general-theory, bypassing the search for empirical evidence, is biased and wrong. I endorse Nagel's remark that *if* we could formulate such doctrines in terms of verifiable statements (and to a large extent we cannot), it would seem that "the available evidence supports *neither* the thesis that all human events illustrate a *unitary* (...) law of historical development, *nor* the thesis that individual (...) effort never operates as a decisive factor in the transformations of society" (1960: 292).

It seems fair to say that all aspects of Hegel's doctrine, for example, are dismissible on the basis that such an interpretative framework does not appeal to contemporary historiography. Historians are not any longer in the business of trying to discover the 'meaning'

of history, or identifying the vast, general ‘trends,’ but in the business of showing how the state of the present causally depends on the past – where causes are not the result of supernatural forces, but chains of events whose causal connections may be studied on the basis of empirical evidence.

So, let us settle for the position that there is no such thing as a law *of* history. Is this a reason for historians to worry? Can historians still provide lawful explanations? Or should they attempt to do so? If historical explanations are not lawful, can they still be causal?

In the previous chapters I have referred to Lewis view of causation and causal explanation. Causation is defined in terms of counterfactuals only; there is no need to appeal to regularities or laws. Events in a causal chain, even hasteners and delayers (as discussed in §1.3.4.6) are causes in the following sense: had they been absent, the effect would not have been the same, or not have been at all. Among the causes we count both physical events and reasons for action. In the causal networks that Lewis identifies there is a role for laws governing the events in the network. But what does the explaining is not necessarily these laws (though aspects of Hempel’s position are not ruled out). What does the explaining of some event are the causes making up the historical chain of events leading up to that event. The Lewis position on both cause and explanation is agnostic about both laws *of* history and what might be laws *in* history. It does not entail that there are laws *of* history; but there must be some laws governing the relations between events in the historical structure, though we need not know what these laws are in order to provide explanations.

Clearly, there *are* laws of nature giving support to counterfactuals. If I say that had Caesar not been stabbed, he would not have died (as he *actually* died), it is certain that there are physiological laws which would explain why it is always the case that a stabbing (of the right sort) to the heart, causes death. But a causal explanation in history need not subsume cause and effect under a general-law in order to be satisfactorily informative (Cf. Lewis, 1986c). For a causal explanation in history it is sufficient to provide *relevant* information about the causal history of an event, by tracing back its causal antecedents. The relevance is decided based on the context of inquiry. It is fairly easy to see that the kind of information about the causal history of an event in which one will be interested when trying to explain, say, the origin of diasporas,

revolutions or wars is remarkably different from the kind of causal information necessary to explain ordinary, everyday, events.

Note that by providing an explanation of this *Lewisian* kind we may still provide deterministic explanations – all we need to do is to claim that a causal antecedent *necessitate* its effect in a uniquely possible way (uniqueness of evolution). There may be natural laws governing the causal relation (and if determinism holds, the laws are non-stochastic), but we do not need to know them in order to make sense of the causal relation. I will say more about this in §4.4.2. The important thing to bear in mind is that the laws that give support to historical counterfactuals are not laws *of* history; there is no such thing. Still, historical explanations are causal, and may be deterministically causal, even in the absence of laws *of* history.

4.3. Nagel's definition of determinism.

One of the reasons Nagel's paper gives clarification is precisely that Nagel defines determinism as a system which, according to the laws, has a *uniquely possible evolution*:

(...) if a deterministic system is in a definite state at a given time, the occurrence of that state at that time is determined - in the sense that the *necessary and sufficient condition*¹⁸³ for the occurrence of that state at that time is that the system was in a certain state at a certain *previous time*" (Nagel, 1960: 294).

Nagel then illustrates the case of determinism with an example of a physiochemical system, taken from Lawrence J. Henderson (1935). The deterministic system is characterised like this:

[It] consists of a mixture of soda-water, whisky, and ice (...) completely isolated (...) The sole characteristics (...) of concern are [concentration, temperature, and pressure]. Under the stipulated conditions, and for a given temperature and pressure, each component will occur (...) in the various phases with definite concentrations; and conversely, if the concentrations are fixed, the temperature and pressure will have a unique set of values. Thus, if the pressure of the mixture were increased (...) the concentration of water in the gaseous phase would be reduced, and its concentration in

¹⁸³ I do not see why the conditions have to be necessary. Only sufficiency seems to be required by determinism. Otherwise we rule out that the same effect could be caused by different causes.

the liquid phase would be increased (...) The value of a variable at any given time is determined by the values of the other variables at *that* time (Nagel, 1960: 293-294).

There are a few comments to be drawn on the example. This is clearly the case of a dynamical system, and one that may be modelled by a set of equations (layer **A**). A temporal reference seems to be implied, and the system's evolution is uniquely fixed (as in by **B**-style determinism). Nagel seems to presuppose sharpness of values (layer **C**) as necessary for determinism to hold. The value of one variable is *constantly* being determined by the values of other variables at the *same time*. Finally, such a system is perfectly predictable by an ideal knower (given the laws and the initial state), and probably sufficiently predictable by finite knowers as well (layer **D**).

In Henderson's example we are not concerned with the complete picture of the state of the system, but only with some specific variables (number of components, phases, etc.) and how their values relate to each other simultaneously. The system is said to be deterministic with respect to certain variables only, and it remains an open question whether for other variables (not the ones we are concerned with here) there can be indeterminism.

Three important remarks are made by Nagel.

(1) A system being [ontologically] deterministic "does *not* entail that the states of the system are [practically] *predictable* (...) a system may be a deterministic one, though we may not *know* that it is such; and it is a mistake to identify (...) the meaning of 'determinism' with the possibility of prediction with unlimited accuracy" (Nagel, 1960: 295).

(2) We should strongly consider the case for *partial determinism*. If we claim that a system operates deterministically with respect to some set of magnitudes or variables, it may be the case that the same system (or a subsystem) operates indeterministically with respect to some other set of variables - provided that the indeterministic variables do not interfere with the deterministic evolution of the first set. If a system *S* does not appear to evolve deterministically, we should consider the case that either *S* is not sufficiently isolated or protected from 'outside' disturbances.

(3) Determinism as (**B**) cannot be (a) conclusively established or proved, or (b) refuted or disproved, by means of empirical investigation. In the case of (a) this may be so because for

some classes of events we simply do not *know* (yet) the determining conditions, whereas for other classes of events, it seems logically possible that no such conditions in fact exist. In the case of (b) this may be so because the failure in discovering determining conditions does not prove that there are not in fact such conditions – maybe the conditions exist but are not discoverable.

I said that Nagel's treatment of determinism shifts in his paper. This is because he later concludes that the doctrine of determinism can only be *partially* supported on the ground that it may be a true and general description of the world as we *know* it. Here his argument takes an objectionable move. Nagel says that determinism has a 'heuristic function.'¹⁸⁴ For example, the doctrine of universal determinism worked as a guiding principle for 19th century physicists and gave them some useful insights. In the case of social science and psychology the gains were more limited, but some versions of determinism brought attention to the determining factors such as "heredity, attitudes acquired by training, repetition of exposure to stimuli, modes of economic production or social stratification and social mobility" (Nagel, 1960: 297).

But a 'guiding principle' is not a thesis about the world's evolution, but only a set of recommendations as to how scientists should proceed. It is an orientation, an advice to look for determining conditions for different classes of phenomena. A guide is not true or false. Nagel correctly says that failing to find determining conditions does not disprove the doctrine of universal determinism, but then it is easy to see that success in finding them does not prove it true either.

So far, there is no good reason to reject the claim that determinism entails (and is best defined as) *uniqueness of evolution* (**B**). This is not to say that 'there is' at least one system which perfectly fits the definition; it may well be the case that no system is deterministic after all, or is only approximately so. But as we mentioned, there is strong hostility to deterministic outlooks in history. Such hostility resulted in some flawed arguments against determinism. In the

¹⁸⁴ Nagel's move here is typical of logical positivists: he adopts the deterministic assumption as a 'regulative principle' for scientists, a 'motto' which seems to say 'always look for determining conditions.' In practical terms, he evades the question about the possible truth or falsity of determinism, and replaces it with a heuristic view.

following sections I will revisit such arguments, highlight their flaws, and demonstrate that determinism as an ontological tenet is not incompatible with the ‘facts of history.’

4.4. An analysis of the arguments against the doctrine of determinism in history

In Nagel’s words, “critics of historical [determinism] who have argued for either a radical or a qualified indeterminism in human affairs, have rejected one extreme position only to adopt another one no less extreme and dubious” (Nagel, 1960: 293). This is not to say that human affairs cannot be inherently indeterministic (governed by probabilistic laws, for example), but to say that the ontological doctrine of determinism has not been proved false by any of the arguments we are going to review next.

By revisiting such theses against historical determinism, I will try to clarify some obscurities, but most importantly, I will attempt to answer whether historical explanations (a) presuppose determinism to be false, or (b) require it to be true. The answer is neither.

There are mainly five different arguments against determinism in history worthy of our philosophical scrutiny. In recent years, Ferguson (1999) has raised what for some would count as a sixth argument against historical determinism – the idea that thinking counterfactually about history, or postulating historical ‘might have beens,’ would be inconsistent with universal determinism. We will discuss the latter more extensively in Chapter 5.

4.4.1. Argument 1 – ‘there are no universal laws of historical development’

In §4.2 we have discussed the failure of the identification of laws of historical development. Some have tried to postulate testable claims about the laws of history, as was the case of Cheney, but the propounded generalisations turned out to be false, or weakly testable. In the case of Marxism, some claims seem to be testable, but only partly so, because the predictions are too vague. Although the Marxist outlook of history does offer an appealing framework for historians aiming to provide explanations of, say, economic changes, the division of labour, or the gradual implementation of labour laws, one would not feel at ease in saying

that proper laws *of* history have been identified. The generalisations are too imprecise to be characterised in terms of law-like statements.

Substantive theses of history are, to a large extent, *untestable*, thus we may dismiss them as false or at least as speculations lacking empirical support. Hegelians and Marxists, for example, did not make predictions in terms of a timeframe, or a specific place, and a finite number of players. In order to ‘test’ their theories the historian would have to deal with: (a) an indefinitely large class of human phenomena to contrast the theory with; (b) deal with an indefinitely large area of space to investigate human affairs; (c) deal with an indefinitely long period of time.

So, the quest for laws of history was not a fruitful enterprise. What implications does it have for causal determinism in history?

Explanations in any domain may depend on accepting ‘framing’ interpretative principles that cannot be independently testable. The causal principle, for example, is one of these framing principles. Determinism is also, in a certain sense, framing. The failure in identifying laws *of* history is no reason to abandon causation and determinism as framing principles. We may provide causal explanation without reference to laws, and determinism as uniqueness of evolution may still hold, also in the case of history.

In general, proponents of substantive theories of history would appreciate it if we could indeed show that historical events also exhibit the same stability and periodicity we observe in the case of, say, planetary motions. But that is not the case, so they have to assume that such periodicity and stability is of a hidden order, that there is an underlying deterministic pattern that, if discovered, would offer the ultimate framing for a causal explanation in history.

Critics of historical determinism, however, claim that because the general evolutionary trajectory of history does not exhibit a discernible pattern it can only be so because there is none – there are no laws *of* history. But from the lack of a general discernible pattern of evolution they infer that historical determinism is always false, that events in history are not causally necessitated by their antecedents. This seems a mistake for two reasons.

(1) Let us accept that the whole of history does not exhibit a discernible pattern, and consider particular historical phenomena. Does it follow that from the absence of a universal pattern of historical evolution, historical ‘local’ systems will not exhibit discernible patterns of evolution? The inference seems wrong. We may have a subset of systems which are independently deterministic; but when we consider them all together we fail to see any pattern.

(2) Clearly we could have a case where a system does not exhibit *apparent* regular behaviour, but it conforms to a more complex pattern of changes. The case of deterministic chaos is a good representative of this view. A chaotic system evolves along a uniquely possible trajectory. But it is so sensitive to changes to the initial conditions, that predictability, in its ordinary (practical) sense, is not a possible task. We have determinism but not predictability. The swinging of the pendulum may be fully determined by the laws and the initial conditions, but we fail to ‘see’ the pattern, or to predict its future states.

In Chapter 3 we defined determinism not in terms of patterns, or laws, but in terms of having a unique evolutionary trajectory – better yet, we defined deterministic laws *on the basis* that they apply to a system which has a unique evolutionary history. There are known *deterministic* systems in physics that exhibit no apparent pattern, the Lorenz model, for example (Cf. Smith, 1998: 147-63). In other words, they are deterministic simply because the laws that govern them are non-stochastic and do not allow for any evolutionary ‘branching.’ And yet, when we try to identify their pattern of evolution we fail. It seems a bad move to characterise systems as deterministic or not on the basis of the pattern they *apparently* present.

No argument has been presented against the possibility that some systems (also in history), especially when taken in isolation, may exhibit deterministic (i.e. uniquely possible) evolutionary histories, or approximately so. The lack of laws of historical evolution, as discussed at length in §4.2, offers no support against the possibility of historical causal determinism.

4.4.2. *Argument 2 – ‘human events are inexplicable and unpredictable’*

This claim against determinism is rather odd. How is it possible to defeat the ontological view of determinism, on the basis that we cannot predict or explain the occurrence of certain historical events? The universe could be deterministic and yet we could be in no position of knowing it as such. This issue has been extensively examined in the previous chapter, and there is little value to be added.

Let us see, however, in what the charge of inexplicability consists. This is an argument that drifts into the problem of explanation, and appeals to the fact that, in order to ultimately explain an historical event, historians would have to identify all its causes, and the causes of these causes, *ad infinitum*, falling into an *endless causal regress*. In the words of Charles Beard: “a search for the causes of [US’s entry into WWI] leads into the causes of the war, into all the history that lies beyond 1914, and into the very nature of the universe of which history is a part; that is, unless we *arbitrarily* decide to cut the web and begin at some point that pleases us” (Beard, 1936: 79, as quoted in Nagel, 1960: 299).

Beard’s claim of arbitrariness is not well founded, as can be shown in several different ways. By providing an acceptable causal explanation we clearly need not fall into such regress, because in order to explain the occurrence of an event in a causal chain all we need to do is to focus on the two adjacent segments (*c* causes *e*). Of course *c* has also one or more causes, but in order to explain the occurrence of *e*, it suffices to give some information about its causal history, and to do so we need not enquire into ‘the very nature of the universe.’ The selection of causes – or the selection of the area of the causal web relevant for the success of the explanation – is determined by the context of inquiry, it is a pragmatic matter related to our interests.

Another possible way out of the problem of the endless regress of causes was presented by James Sadowsky (1980), who argued that a solution would be to abandon the causal principle and accept that the first member of a causal series is not in *need* of a cause itself.¹⁸⁵ It is arguably a solution for the endless regress, but abandoning the causal principle and talking of uncaused events is a remedy many would find too bitter to swallow. Maybe we need not do so.

¹⁸⁵ This is similar to Aristotle’s and Hume’s view as in §2.1.6.

The argument of the endless regress of causes fails, since it implies that we could never really satisfactorily explain any event's occurrence; and patently we can, as Nagel explains:

Although *C* may be the cause or a determining condition for *B*, where *B* is a condition for the occurrence of *A*, *B* is nonetheless a determining factor for *A*; and in stating the determinants for *B*, we are answering a different question from the one we are seeking to resolve when we ask for the determinants of *A* (...) an explanation can be completely satisfactory, even though in offering it we are assuming *something*¹⁸⁶ which has not in turn been also explained (1960: 299).

Historians aim to answer 'why questions' by identifying the most relevant causes of the events they explain. When they say that *A* determined the occurrence of *B*, it does not mean that *A* alone sufficiently does so (see §1.3.2). By cutting the web of causes and focusing on a single stretch of the causal chain (or web), historians do not do so 'arbitrarily', as Beard says. There are pragmatic interest-based constraints which apply to the selection.¹⁸⁷ Explanations may be perfectly adequate, as long as the number of traits whose occurrence is to be explained is limited – and this is often the case.

I believe it is legitimate to conclude that the length of the stretch of causal relations to be analysed is set by the 'why question' itself. Indeed a good and plausible 'why question' in history would not require historians to go back all the way to the 'Big Bang.' Despite the fact that in some cases the stretch of the causal chain can be quite long, 'explicability' in history does not require total explicability.

Let us now focus on the 'unpredictability' argument. It basically says that failure to predict is a sign of the falsity of the thesis of universal determinism. This one is easily dismissible on the basis of our exposition of the different layers of determinism and how they relate one with the other. I have given sufficient reasons, I believe, to justify the claim that the failure of

¹⁸⁶ That 'thing' which has not been asked or is not the focus of our attention.

¹⁸⁷ Van Fraassen formulates a pragmatic view of explanation that seems compatible with Lewis's account. He believes an explanation is an answer to a particular *contrastive why-question* - the contrast class would then determine what causal information is relevant when giving an answer. For Lewis, an explanation is simply the provision of information about the causal history leading up to some event. The use of contrastive why-question may be a helpful way to make clear what aspects of the question should be addressed. Consider the example: why did the First World War start *in 1914?* – rather than *in some other year?* Why *did the First World War start in 1914?* – rather than *not starting at all?* See Van Fraassen (1980: 134-52).

practical predictability is not informative on matters to do with ontological determinism. No need to repeat ourselves.

Surely, however, prediction in humanities is not always a hopeless task. Every now and then, also in recent years, some social or political scientist will put his/her neck out and risk a prediction when this would seem to be sufficiently supported by empirical evidence, and sometimes they get it right.¹⁸⁸ But in the particular case of history, prediction does not seem a possible task.

Such predictions in social science do not foretell the future in a way that we eliminate open possibilities until only one plausible outcome remains. But such forecasts “do exclude an enormous number of logical possibilities (...); and they do point up the fact that though the [agents] may have a considerable range of free choice in their actions, their actual choices and actions will fall within certain limits” (Nagel, 1960: 301). What is logically possible might not be actually possible; which leads us to the “obvious interpretation (...) that there are *determining conditions* for both what has happened as well as what will happen (...)” (*Loc. cit.*).

The conclusion to be reached is that the incompleteness of causal explanations and difficulties involving prediction do not offer any support to the argument against the idea that the world evolves according to a uniquely possible path (as in **B**-style determinism).

4.4.3. *Argument 3 – ‘novelties in human affairs are incompatible with determinism’*

There is another odd argument against the doctrine of historical determinism; it revolves around the idea that the *emergence*, in the world of human affairs, of new inventions, ideas, works of creativity, novel modes of behaviour, and so on, are incompatible with determinism. This is a view originated from those who believe that artistic creation, for instance, is the result of a *free spirit*, something which cannot be accounted for in terms of laws, causes and effects known prior to the emergence of the novelty. It also fuels the argument of the *unpredictability* of the course of human affairs, for obvious reasons.

¹⁸⁸ Recent examples can be found in Chenoweth (2013) and Regan (2002).

The essence of *emergentism* is to argue for the unpredictability of novelties as being inconsistent with determinism. We have already contended that failure to ‘predict’ tells us nothing about whether the ontological doctrine of determinism (**B**) holds of this world, for any given system, or not; thus we could easily dismiss the present argument straight away. But because of its popularity, let us spend some time spelling it out.

This doctrine of ‘novelties’ in human affairs presents itself in two different formats:

- (a) *The doctrine of emergent levels* – this is an atemporal doctrine which stipulates that some traits and modes of actions cannot be explained in terms of the properties exhibited by the component parts of complex systems to which these traits or actions belong.
- (b) *The doctrine of emergent evolution* – this is a temporal thesis which asserts that novelties (new traits, novel form of organisation or new activities) appear in time, and because they did not previously exist, they cannot be explained in terms of their historical antecedents.

But both forms of the doctrine are perfectly compatible with the doctrine of determinism, as we shall see next.

A common illustration of the case of (a) asks us to consider the case of a molecule of water. Many of the traits of the water molecule cannot be *predicted* from the properties of hydrogen and oxygen when they exist in isolation from other atoms, or even when combined, i.e. some of the traits of the particular combination represented as H_2O are novelties.

For Nagel such an illustration is misleading, because the (un)predictability of any trait is always *relative* to a certain theory. Some of the traits of the water molecule are emergents as relative to the atomic theory defended by Dalton, but the same traits are predictable according to quantum atomic theory. We have here an example where our lack of *knowledge* accounts for our ‘surprise’ at the emergence of a novelty. It is essentially an epistemic matter, and as such it may be only temporary, and not absolute. Once a better theory is at hand, what *seemed* a novelty at the light of a previous theory ceases to be one (becomes predictable) in the light of the new theory.

Is it possible that the new theory can account for the emergence of the novelty and at the same time conforming to deterministic evolution? No reason has been presented as to why

this should not be possible. Moreover, empirical evidence may suggest that even if a scientist is not in a position to deduce the properties of the novelty from its components (because of the lack of an adequate theory), it seems reasonable to assume the possibility that the novelty itself only come into existence under *determinate conditions*.

Not only can determinism, in principle, accommodate the emergence of novelties, but the way the novelties ‘emerge’ could even presuppose determinism, rather than reject it. Unless we consider the emergent traits to come to existence without a cause – which would seem a very daunting thing to say – clearly the causes of all novelties are to be found in the prior state of the world, but from an epistemic perspective, they are not expected to occur. If determinism holds, then the prior state of the world, together with the laws, causally necessitates the emergent new trait.

The situation is not very different for (b); it is true that agents are indeed the source of many temporal novelties, i.e., new traits which have no historical precedents and seem like a ‘rupture’ of current knowledge of the world. The question is, what constitutes a novelty and why would that be evidence against determinism?

A novelty is essentially characterised as an event that is said or believed to be temporally unprecedented (i.e., never arose before in history) and unpredictable *in the light of* theoretical knowledge current at the time it ‘emerges.’ In order to characterise any novelty as unprecedented, the historian would need to possess an incredibly enlarged knowledge of the past to show that an event is of a kind that has not occurred before. In addition, one has to show that such novelties could not have been predicted. But even if so, what is the relevance of this? If by novelty we refer to the unpredictable, then it shall be clear that ontological determinism can handle the emergence of novelties.

We need a proper definition of what is to count as a ‘prediction.’ Some would certainly regard magnificent achievements such as the moon landing, or the invention of the internet, or the development of nuclear power as ‘novelties’ – because they have no precedents in history. And yet, such achievements were in some way ‘predicted,’ or deemed quite possible, by

science fiction writers who thought that the internet, for example, was possible.¹⁸⁹ So *unprecedented* in history seems to work well as a definition of a novelty, while *unpredictable* seems to be more challenging, as it is relative to theories, and it is not always easy to see identify the scope of scientific theories.

Even if sufficient evidence is shown in order to support the claims that a certain novelty was unpredictable, such unpredictability is solely a problem of formal logic: “for to predict an event, the traits of that event must be formulated in a statement; and unless the predicates describing those traits occur in the premises of the predictive argument, that statement can follow from the premises neither deductively nor [probabilistically]” (Nagel, 1960: 307). As a matter of fact, if such an emergent novelty is ‘radically’ different from our knowledge of the past, then no regularity or law would be known in order to make a proper prediction possible.

It is only once the novelty becomes known and well established that we are in position of making adequate inquiries concerning regularities or laws. We might, after further scrutiny, identify the initial conditions a novelty’s occurrence is contingent upon. Furthermore, scientists usually start their inquiries by assuming as a heuristic principle that a novel system is deterministic.

Nagel’s argument in (a) is centred on the idea that unpredictability need not be absolute – an unnecessary move to dismiss the argument, but sound nevertheless. In (b) his move is to show that the very idea of a law or regularity is formally incompatible with the idea of predicting a ‘novelty,’ because any applicable laws would not had been known prior to the emergence of the novel trait. Again, this move is unnecessary, as unpredictability – regardless of how we define it – is only a problem for the epistemic layer of determinism: total predictability (**D**). Although emergentism seems flawed since the very beginning, it is rather

¹⁸⁹ Jules Verne’s *From the Earth to the Moon* ‘predicted’ not only the moon landing but also the effect the much lower levels of gravity would have on the astronaut’s bodies. Mark Twain wrote a short science fiction story entitled “From the ‘London Times’” (1898), in which he predicted the creation of the *internet* – called by him the *teleelectroscope* – a device hooked up to the existing phone system. H. G. Wells’ novel *The World Set Free* (1914) predicted the detonation of an atomic bomb. At the time, Wells only knew a little bit about radioactive decay, which was enough for him to foresee a future where scientists would figure out a way to generate a remarkable explosion, killing and contaminating everything within a radius from the epicentres.

entertaining that Nagel makes certain concessions to ‘emergentists’ only to defeat their arguments in their own terms. Also, in the case of (a), we could also suggest that ultimately *emergentism* does not even defeat entirely layer **D** of determinism as, from an idealised perspective, the infinite, ideal knower should see such novelties as no surprise, entirely deducible from the prior state of the world, and the laws.

Little has been said, however, about how novelties are consistent with ontological determinism. Nagel has heavily focused on logical and practical matters to do with predictability. I undertake to set out the argument against *emergentism* in simpler terms, and here I will make good use of the distinction of the different varieties of determinism, as discussed in Chapter 3.

Let us say that a world (w) evolves from $S_{(t_1)}$ to $S_{(t_2)}$ where it produces a novelty (N). Now, N is a novelty because it is unprecedented in the evolution of w , i.e., N is absent at any moment prior to t_2 . We then ‘run’ world w again, starting from $S_{(t_1)}$ to $S_{(t_2)}$, and once again, it produces N . If determinism holds, i.e. if the evolution of S is uniquely fixed, then whenever we run w again, N is produced at t_2 . We have a case of determinism *and* the emergence of N .

One can argue that, despite the world w being deterministic, all the inhabitants of the world were unable to predict or foresee N . It may well be so. But this is clearly not an argument against the ontological layer of determinism, it only says that the inhabitants of w had inadequate knowledge of how w runs or evolves. It suffices for determinism to hold that that every time we run w , we get N at time t_2 , whereby we conclude that the emergence of N is determined to occur. Determinism as **B** fits perfectly together with the emergence of N . Of course, should we run w again and observe that N does not occur at t_2 , then w is said to be indeterministic.

We may also think of w being only partially deterministic and still account for N . Let us say that some magnitudes associated with N are determined, while others are not. So when we run w from $S_{(t_1)}$ to $S_{(t_2)}$ we get N , and when we run w again we get N' which is different but still similar in kind to N – N and N' have identical values for the deterministic subset of magnitudes, but differ with respect to the indeterministic subset. Also in this case, N seems to fit with some

version of ontological determinism.¹⁹⁰ Perhaps an example may help our treatment of the occurrence of novelties.

4.4.3.1. Exemplifying deterministic novelties: the case of convergent evolution

Stephen Jay Gould (1989) is a scientist who believes the world to be indeterministic, at least with respect to evolutionary processes. Let us say that *N* stands for ‘life’, which first occurs in the actual world *w* at time t_2 . Now, let us assume that we could re-wind the evolution of *w* to a previous time t_1 ; once *w* is played back, Gould believes ‘life’ (*N*) would have then taken a very different course.

Simon Conway Morris (2005) has a different view on this; he endorses the theory of convergent evolution,¹⁹¹ a theory which I believe fits best with the notion of partial determinism. Morris believes that ‘convergence’ is the dominant force of evolution; by rewinding the world and assuming the same environmental and physical constraints would still operate, the same *kind* of novelties will occur, at the same (or approximately so) times they did the first time the world evolved from t_1 to t_2 .

Morris is sceptical, however, when asked if the novelty of life would *inevitably* occur elsewhere – it may be the case that there is only one way of generating life, and also the case that other Earth-like (with the same physical constraints) planets are improbable.¹⁹² But his thesis is that once ‘life’ is generated, the novelty of ‘intelligent life’, for instance, would inevitably occur (the case of primates, corvids and cetaceans). It is a case of ‘wait and see’:

¹⁹⁰ In the case of Earman’s patchlike evolution we have an example of a system which is considered deterministic, and still allows for some bounded variation of its evolutionary trajectory (see §3.6).

¹⁹¹ ‘Convergent evolution’ is the name given to the acquisition of the same trait in unrelated lineages. A good example is the wing. Birds and bats are unrelated but their wings are similar because of the physical constraints imposed upon wing shapes.

¹⁹² Dawkins (1986) has an interesting view on this, as he says that what appears to be unlikely in the lab in, say, 2 or even 20 years may become probable or even likely to occur in dimensions of 200 million years. Or still, if the structure of the universe is infinite, then any low-probability novelty will be repeated many times. Gould (1989), however, has a different view on the mechanisms of evolution. He believes that chance may be a very important factor, while changing fluctuation of genes and natural selection are less important factors. For a summary of the points of agreement and disagreement between Dawkins and Gould, see Sterelny (2007: 3-14).

“once life started (...) nothing could have stopped it from evolving to produce us, or something very similar” (Szathmáry, 2005: 850).

But let us assume that there can be other Earth-like planets, with the same physical and environmental constraints.¹⁹³ If there are thousand Earth-like planets, how many of them will produce: (N_1) life; (N_2) life with the genetic code; (N_3) life based on complex cells (eukaryotic); (N_4) intelligence capable of natural language?

We may reply by saying that:

- (a) If the planets are deterministic systems, governed by the same laws, and the initial conditions are the same, or very similar, then each kind of novelty (N_1, \dots, N_4) will be produced at the same times t_1, \dots, t_4 for all the Earth-like planets.
- (b) If the worlds are *partially deterministic*, then it is hard to say what minor variations to some of the (indeterministic) magnitudes may signify for the occurrence of each of the novelties. Perhaps all worlds would probably start with life, quite possibly life with something similar to the genetic code, but then some would produce complex cells and generate, at some point, intelligence; while others would not, or would take much longer to do so. But we might expect that some of the Earth-like planets would very much resemble each other with respects to the occurrence of (N_1, \dots, N_4).
- (c) If the worlds are *indeterministic*, then the occurrence of the novelties (N_1, \dots, N_4) is not inevitable, convergence of evolution is false, and Stephen Jay Gould could be right in saying that nature would have taken a very different course should we ‘rewind’ any planet back and ‘play’ it again.

One must conclude that if convergent evolution holds true of these worlds, i.e., convergence is indeed a dominant force of evolution, then this also testifies to the idea that the ‘algorithm of evolution’ is to some extent ‘robust.’ Some of the products (novelties) of evolution are sensitive, others are not. Therefore the truth of the theory of convergent evolution seems to require that at least partial determinism holds true of the relevant system or world. We have here a clear example taken from biology of a case in which determinism – or partial determinism – can account for novelties.

¹⁹³ Simplified from Szathmáry (2005: 850-1).

4.4.4. **Argument 4** – ‘chance events are incompatible with determinism’

In Chapter 2, I explained in detail that chance is often a misleading term because there is little agreement on what it means; I identified ten different meanings for a ‘chance’ event. I showed that only one of them is clearly inconsistent with the doctrine of determinism: *objective chance* (§2.1.7). Still, even if objective chance is a fact of the world, it still does not rule out that some systems, when sufficiently isolated from other systems where chance operates, could evolve deterministically, nor does it entail that *partial determinism* is unattainable. But if ever proven true, objective chance does rule out *universal determinism* in all its layers (**A**, **B**, **C**, and **D**).

Nagel only identifies four meanings for chance.

(a) Chance is only how we name the absence of a ‘unifying plan’ for historical evolution, where every single historical event is shown to be according to some ‘timeless Reason’ – such as we find in Hegel.

(b) Chance is sometimes equated with an event being *unforeseen* or *unexpected*. Because we sometimes fail in predicting the outcomes of some event, and even our own actions, we sometimes *surprise* ourselves with such occurrences, and label such events as ‘accidents.’ For instance: “The disappearance of slave economy in the United States, which seemed to many southern landowners to be part of the permanent social order, was not foreseen by most of them even as late as 1859” (Nagel, 1960: 309). Our failure to predict is an epistemic matter, a sign of our *ignorance* of how the world evolves. Here Nagel combines the two definitions of *chance* I gave in §2.1.2 and §2.1.3 (chance as surprise with the unexpected, as chance as our ignorance of the causes of an event). Because the definition is epistemic, determinism as an ontological doctrine remains unscathed.

(c) Sometimes an event is described as a chance event because it involves the intersection or collision of two independent causal chains or systems. In §2.1.4 I exemplified such case with the destruction of the Spanish Armada. Because two altogether independent causal chains of causes and effects collide, and we fail to predict the consequence of such a collision, we say that the product of the collision is chancy – although the effect is clearly caused

by the determining conditions specified at the point of the intersection of the independent causal chains.

Clearly, if there is only one possible path of evolution after the collision, the system is deterministic. Chance so defined is not incompatible with determinism. With regards to the predictability of such an event, even if in some cases it does seem difficult to foresee the kind of event that is about to occur, in other cases prediction could perhaps be possible – perhaps we know the independent systems sufficiently well in order to predict when they shall collide, and what will be the consequences of the collision.

(d) The final sense of chance identified by Nagel is that of an event which occurs in the absolute absence of any determining conditions, or causes. This is a trickier case, and Nagel's treatment of the case is not entirely satisfactory.

He says that “*if* there are such events (...), they are not merely unexpected and unforeseen, but are *inherently unforeseeable*; and their occurrence could not be explained, even *after* they had happened, no matter how extensive our knowledge may become” (Nagel, 1960: 310). The conditional form of the claim is important, because Nagel want to leave as undecided whether some events may occur without determining conditions:

(...) despite the well-known rumour that it has been affirmatively settled by modern physics [I argue that] such a question cannot (...) be answered definitively, since even repeated failure to find any causal conditions for some type of event can always be construed as evidence for human *stupidity*¹⁹⁴ (1960: 311).

An event occurring without any determining condition could mean two different things: the occurrence of an *uncaused* event, or more plausibly, an event which may be subsumed under *probabilistic laws*. In the former case there are no, strictly speaking, ‘determining conditions’ for the occurrence of a ‘chancy’ event. In the latter, we say that an event, in the presence of certain conditions and according to probabilistic laws has, say, 80% of chance of occurring, and 20% of chance of not occurring – there is more than one uniquely possible path for the evolution of such a system, i.e. no *uniqueness of evolution* (**B**). There can be determining conditions which

¹⁹⁴ I would rather say ‘limitations.’

probabilify rather than *necessitate*; and there will be always some degree of probability that a certain kind of event will occur under any (sufficiently determinate) initial conditions.

It seems more plausible, in the case of history at least, to attribute the lack of determining conditions not to the nature of the applicable laws, but to the lack of knowledge on the part of the historian. Nagel asks: “would we not ordinarily interpret a competent historian’s readiness to label as *chance event* an occurrence which he is unable to explain (...) an expression of his weariness or despair?” Nagel also concludes that “if there are (...) chance events (...), there certainly is a definite limit to what can be explained. But since we cannot be sure for which specific events this limit is in force [we do not] have an impregnable excuse for stopping our inquiries into their determinants” (1960: 311).

If some events seem to occur indeterministically, there is no reason to claim that no systems can behave deterministically, or that we should definitely abandon the deterministic assumption. If some event *e* occurs indeterministically, we still want to identify the conditions under which the occurrence of *e* becomes more likely as *e*’s causes – all we need is to identify a suitable definition of *causation*. Indeterminism in this sense does not entail that there are *uncaused* events, and even the ‘causal principle’ could still hold.

In the presence of genuine objective chance, we must conclude that some systems do not evolve according to a uniquely possible trajectory. We must also consider that if an indeterministic system collides with other otherwise deterministic systems, this could certainly spread some level of indeterminacy: the thesis of universal determinism would crumble if just one non-isolated system exhibits objective chance. But I argued in §3.6 that it is still unclear whether there are indeed any genuinely indeterministic systems (Cf. Earman, 1986), so this argument takes a conditional form: *if* objective chance is real, *then* universal determinism is false. But I shall add that even in the presence of objective chance, *partial determinism* could survive.

4.4.5. *Argument 5 – ‘determinism is incompatible with free will’*

This is another issue we have already discussed, in part, in Chapter 2. This issue emerges from a certain tendency among historians (and laymen in general), to perceive determinism as a potential threat to free will. This comes as no surprise, given the fact that the notion of determinism (and also that of freedom) is usually not well understood, and different definitions are available. In the case of history, it is claimed that if determinism holds, then humankind is unfree, and judgements of moral responsibility – an essential part of the historian’s work, according to Berlin (2002) – become meaningless. I argued that Berlin should have given reasons for rejecting compatibilism between determinism and freedom. I expressed my view that the definition of freedom given by libertarians was not satisfactory, and provided a brief sketch of how a typical *Humean* compatibilist approach works (in §2.4.1).

I have also expressed my desire not to dwell on the problem of free will. Genuinely new ideas in relation to the problem of free will are rare, and unfortunately I can offer none. As Earman wisely puts it, the free will-determinism controversy “has all the earmarks of a dead problem. The positions are well staked out and the opponents manning them stare at one another in mutual incomprehension” (1986: 235).

Yet, I must concede that to consistently avoid making references to this controversy is just not possible. And again I paraphrase Earman: “why can’t we walk away from [the problem of free will]? Because the issues it joins are essential to an understanding of human action and man’s place in nature. Here we have a major clue as to why the problem has proved so divisive and why it resists any neat solution” (*Loc. cit.*). Once more, I feel compelled to, unwillingly, discuss some aspects of the free will problem.

When I investigated Berlin’s notion of chance, and why he believed that objective chance (indeterminism) was required by freedom, I criticised Berlin for not considering the case for compatibilism (§2.4.1). But I am equally guilty of not having considered the case for libertarianism. This is what I will do now. My aim in doing this is to show that unless the libertarian can show that free will requires indeterminism, and that we are *in fact* free in the libertarian sense, we have no grounds to refute ontological determinism.

How are the actions of human agents different from those of a sunflower turning its ‘face’ to the sun? (Cf. Earman, 1986: 239). If the thesis of universal determinism is true, is it not the case that all events (including actions) are produced by forces or circumstances beyond human control? There is an obvious relation between the free will problem with the mind-body problem – which in turn has also divided philosophers into positions of mutual incomprehension. We usually believe that actions are mediated by mental states such as beliefs and desires. A mental state is either ‘parasite’ on physical states or not. Earman’s notion of parasitism is defined as: “for any possible worlds w_1 and w_2 , if w_1 and w_2 agree on all physical attributes, then they agree on all mental attributes as well” (1986: 240). Parasitism (P) will be the case if we can identify mental states with physical states – whether we can do this is an important point of contention.

Let us assume, for the sake of the argument, that physical determinism holds. We then ‘rewind’ the world from our time t_1 , back to a remote past time t_0 , a time before evolution accounted for the emergence of the first, so to say, ‘mind.’ In other words, the state of the world at t_0 , and even a libertarian will agree, is completely describable in physical terms only. If P fails then the (physical) state of the world at t_0 does not uniquely determine the state of the world at t_1 , because mental state such as beliefs and desires would not be subsumed under the laws governing the evolution of the physical world. In other words, it would seem that universal determinism fails.

If physical determinism holds, however, and P fails, then we must conclude that an autonomous mental life must be completely inefficacious when it comes to bringing about any event in the physical world – mental states cannot cause anything but other mental states. Alternatively, if both physical determinism and P hold, then we say that the state of the world at t_2 (including the mental states reduced to physical states) are determined by the state of the world at t_0 , together with the laws. In other words, actions would seem to be determined by circumstances that existed before the agent was born, and therefore conditions *beyond* the agent’s control.¹⁹⁵ The idea that the causes of action are not under the agent’s control is not palatable for the libertarian.

¹⁹⁵ Adapted from Earman (1986: 239-49)

We may offer a different characterisation of such conditions. Again, Earman offers a useful remark: “all human actions, in so far as they are physically characterisable, are deterministically explained by *exactly* the same factors that explain (...) everything else in the physical realm” (1986: 241). If *P* holds then mental states are causes, if *P* fails, mental states are causally inefficacious.

Now we may properly characterise what the libertarian’s position is. The libertarian wants to add to the previous exposition that if determinism holds and *P* holds, i.e. mental states are determined by the same factors that explain all physical events, then human agents are not ‘free’ to decide as they ‘want.’ Deliberating becomes a deceptive activity, as agents do not really have any choices. And indeed, if by freedom we mean being capable of acting differently from what the physical conditions and laws determine (‘doing otherwise’) then the libertarian’s point is perfectly sound – if physical determinism and parasitism hold, we are ‘unfree.’

Where things go terribly wrong is when (and this is not the case of Berlin) libertarians “want to do a *modus tollens* on physical determinism, arguing that since determinism implies unfreedom [premise 1] and since we are free [premise 2], determinism *must be* false [conclusion]” (*Loc. cit.*). The argument is valid, but we have no way of knowing whether ‘premise 2’ is true or false – and this fact alone offers sufficient justification, I think, to dismiss the argument that free will requires indeterminism. But I said I would consider the case for libertarianism, so let us go a little bit further.

How is it that the libertarian, granting that mental states are in fact not reducible to physical states, provides explanations of actions? Are actions uncaused? Most libertarians (including Berlin) would say that actions are caused, but not in the same way as physical events; and that it is wrong to explain the current state of the world by means of the state of the world at t_0 together with the laws. Actions are partly explained by the state of the world at t_0 , but also by later states – those states in which we have the ‘emergence’ of autonomous mental states such as beliefs and desires (note that there is an overlap of the libertarian’s position with some theses on emergentism). The libertarian also needs to add another premise, that those mental states that occur at times later than t_0 but earlier than t_1 have the capacity of pre-empting some causal chains originating from the state of the world at t_0 – otherwise agents would be ‘free’ but causally inefficacious.

Where exactly do these mental states emerge from? What causes them? Berlin, among other libertarians, defended a version of the theory of *agent-causation*. This theory may be defined as the idea that agents may start new causal chains which in turn are not determined by the prior (physical) state of the world together with the laws. In the case of Berlin, mental states are caused by a special property which is only possessed by intelligent beings (beings capable of deliberation), the *self*. In other words, free choices are made (caused) by the *self*.

We may now turn to the criticism made by Nagel against Berlin. Nagel's point is not to dismiss 'premise 2' ('we are free') as just explained, but to say that Berlin did not offer an adequate characterisation of the *self*. In Berlin's view, the *self* is to be distinguished not only from the body, but also from the 'free' choices an agent makes. And this may seem problematic because:

(...) when I (...) choose between alternatives, I am usually not aware that the choice may be the expression of a set of more or less stable dispositions (...) should I become aware of these things (...) does my choice or my heart cease to be mine? (...) Berlin (...) appears to have an irresolvable puzzle (...) that arises from his [notion of *self* where] any trait or action which stands in relations of causal dependence (...) is automatically cut off from being a genuine phase of the *self*. It is as if a physicist in analysing the performance of a baseball, and noting that [its] shape, the surface quality, and the elastic properties (...) are partly determinative of its behaviour when it is struck by a bat, were to declare that these traits do not properly belong to the ball, but are as much external to it as the impulse imparted by the bat (Nagel, 1960: 312-3)

The result of Berlin's characterisation of the *self* is that, ultimately, 'nothing' can be identified as being the (or of the) *self*: "we conceive ourselves to be acting freely, even though we may recognise that some of our choices are the products of our dispositions, our past actions, and our present impulses" (Nagel, 1960: 313). Berlin not only makes it difficult to define the *self* in the context of our action, but makes it very difficult for any historian to provide a causal explanation of human behaviour, as it would require knowledge of what the *self* is, and how it brings about events in the physical world.

I have here presented two approaches to the problem of free will. Earman (1986) showed that one of the premises libertarianism is based on cannot be shown to be true. Nagel (1960) offered a limited retort, as it focused more specifically on the case of Berlin. Either way,

I believe we have sufficient grounds to conclude that libertarians have not satisfactorily demonstrated that (1) freedom is to be characterised as ‘being capable of doing otherwise,’ (2) physical determinism does not hold, (3) parasitism does not hold.

Berlin is aware that ontological determinism has not been proved false. So he articulates his argument differently from the usual *modus tollens* argument. He claims that if determinism is true the ‘language’ we use to communicate moral distinctions would have to be revised, for such language presupposes *free* agency, in the libertarian sense, i.e., being *free* to choose and act differently from how one *actually* acts. Thus, if the doctrine of determinism holds, all our moral experience would be unintelligible and incoherent. Berlin is therefore committed to the view that, insofar as we are committed to retaining the language of moral responsibility, we are committed to rejecting determinism.

And Berlin now adds another premise, one focused specifically on the case of historical determinism; that the work of historians requires moral judgements, and consequently requires that they *must* retain the language of moral responsibility (premise 3). Historians are usually divided in their positions on the importance of making moral judgments. But if we state, as previously done, that explanation in history is all about providing information about the causal history of events, and that our choices of what causes to prioritise when providing an explanation is pragmatic, it is easy to see that historians could ‘choose’ to explain any event without ever making moral judgments. We may explain Hitler’s or Stalin’s actions based on our knowledge of their beliefs and desires, and other relevant historical circumstances, without ever characterising the actions themselves as ‘evil’ or ‘blameworthy.’ I do not imply that this is what historians *should* do, but they could, and patently *can* provide explanation free from value-judgements. And this seems to render premise 3, and consequently Berlin’s argument, false.

4.5. *Final remarks.*

In the previous section I readdressed some claims which are believed to be incompatible with the doctrine of determinism, in particular historical determinism. Setting out determinism in its different senses or layers (see Chapter 3) was a useful exercise, I believe, as it helped us to show that, among other things, arguments based on the alleged unforeseeability or

unpredictability of events constitute, at best, a case against the epistemic layer (**D**) of determinism, in its ordinary sense.

The five arguments we considered are not an exhaustive list. More recently Ferguson (1999) argued against some particular theories he confusingly placed under the same label of 'historical determinism,' such as Marxism, Hegelianism, scientific determinism and also some narratives styles in which the end is somehow 'predictable.' Ferguson believes that *counterfactual history* (1) provides epistemic gains and (2) is incompatible with historical determinism. This takes us to the topic of the next chapter, in which I will argue that (1) is acceptable, but (2) is not.

Chapter 5

Counterfactual Thought Experiments in History¹⁹⁶

5. Historical counterfactuals

Counterfactual thought experiments (CTEs) in history have become more popular in recent years, and a new and controversial branch of history has originated from their use: ‘counterfactual history,’ also known as ‘virtual history.’ Despite its popularity among the general public, mainstream academic historians tend to consider historical counterfactuals as having no, or little, epistemic value. E. H Carr considered entertaining CTEs playing a parlour game of might-have-beens without any real meaning (1964: 97). E. P. Thompson referred to CTEs as *Geschichtswissenschaft* – ‘unhistorical garbage’ (1978: 300). M. Oakshott thought it to be an “extravagance of imagination” (1933, as quoted in B. Warf, 2002: 26). Benedetto Croce considered CTEs to be uninformative or absurd (1966: 557ff.). More recently, Richard Evans published an extensive analysis (*Altered Pasts*, 2014) of the problems counterfactual historians incur in order to make sense of their claims. He concluded that historical counterfactuals are a ‘waste of time.’

The main objective of this chapter is to investigate three claims concerning the use of counterfactual thinking in historical explanations: (1) that counterfactual thinking may give

¹⁹⁶ An early version of this chapter was written as an article, and published by peer-reviewed journal *Principia: A Latin American Journal of Epistemology* (Maar, 2014: 87-113). In this chapter, I have substantially revised and modified my analysis of the usefulness of counterfactual history.

historians useful insights; (2) that it may be a useful tool for the ‘weighing’ of causal antecedent’s significance; (3) that it shows much of history to be analogous to ‘chaos.’ I argue that only one of these claims sufficiently justifies the idea that CTEs may be important in providing a causal explanation in history.

A secondary objective of this chapter is to consider the relation between counterfactual thought experiments and the rejection of historical determinism. There is a popular belief among practicing counterfactual historians that counterfactual thought experiments are incompatible with doctrines of historical evolution deemed to be deterministic (such as Hegelianism, Marxism, and some scientific approaches to history). This is a mistaken view, as we shall see – CTEs in history are compatible both with determinism and with indeterminism.

5.1 Considering alternative histories

Considerations about counterfactual/alternative histories in history, or attempts to answer the question ‘What might have happened had something in the past been different?’ are not popular among mainstream academic historians. However, such *alternative histories*¹⁹⁷ and their hypothetical differences from actuality have fascinated and inspired fiction writers for decades. More recently virtual history has emerged as a more serious approach, somewhat between academic history and literary fiction. Defenders of virtual history normally claim that considering alternative outcomes is part of any historian’s work, and in some cases helps illustrate how historians weigh the causal importance of historical antecedents. Whether counterfactual considerations in history give us useful information or not is the question I will now try to answer.

It is hard to deny that, while thinking about historical facts and how they were actually caused, we do in fact consider alternative histories. Such considerations are not always explicit in historical explanations, but quite often they are presumed. When we say that the bursting of

¹⁹⁷ Some authors like to distinguish between counterfactual history and alternative history. They are essentially the same, except that alternative history usually involves a larger departure from actuality than counterfactual history does.

the housing bubble in the United States (*C*) caused the 2008 recession (*E*), we are in fact implying that without this bursting of the housing bubble, the crisis would not have happened, at least not the way it did.

The previous paragraph calls attention to a certain account of causation that seems to be common and intuitive in contemporary history, and adopted by counterfactual historians: causation as a relation of counterfactual dependence. In Chapter 1, following Lewis's theory, I defined causation in terms of counterfactual dependence, and also discussed some later modification of the theory, such as causation as causal influence. I argued that historians would be interested in Lewis's treatment of causation as it does not require subsumption under general-laws, does not require that events are classified into types, and is compatible both with determinism and with indeterminism.

But 'counterfactual' historians go beyond the usual practice of historians. They not only refer to causation in terms of counterfactual dependence (had *C* not happened, *E* would not have occurred), but they want to go a step further and say: had *C* not occurred then *E* would not have occurred, but a different effect *F* would have occurred. In other words, counterfactual historians try to give a plausible answer to the question what would have happened had something in the past been different. Typical examples include: What would have occurred had Germany won WWI or WWII? Or the Confederates won at Gettysburg?

In some cases counterfactual historians try to point out from a list of possible outcomes (or worlds) they consider as plausible, the one that seems to be most the likely, or closest to the actual world. Sometimes they move directly from considering altered causal antecedents to the envisaged possible world without further considerations, while at some other times, being more cautious, counterfactual historians list relevant plausible outcomes that might have emerged, and then compare them and select two or more as worthy of attention. Frequently, however, the criteria used for the comparative analysis are not very clear, making one wonder whether counterfactual historians have 'special intuitions' about the weighing of alternatives.

In this chapter I will not focus on the difficulties in providing plausible CTEs in history, but on their hypothetical usefulness. It is important to mention, however, one important challenge to historical counterfactuals. When counterfactual historians entertain alternative

worlds, they usually consider what would have happened had one antecedent been different, or absent, while all other conditions remain fixed. This raises an obvious question: is it *always* legitimate to hypothetically consider an alteration to just one causal antecedent, isolating it from its context of occurrence? Jon Elster argued that some historical counterfactuals will be incoherent with the *ceteris paribus* condition that ‘everything else remains the same’ (1978: 181-92). Think of the counterfactual claim ‘had the Nazis considered Jews to be a superior ‘race,’ the Holocaust would still have occurred.’ A world in which Nazi Germany considers Jews a superior race (while everything else remains fixed) conflicts with the political, cultural and social conditions present in Germany during the 1930s. It seems that we cannot have such a counterfactual and everything else to remain the same. If I interpret Elster correctly, he seems to say internally incoherent counterfactuals do not obtain in any world. So a world in which Nazi Germany loves the Jews, but *Kristallnacht*¹⁹⁸ still occurs, and all the anti-Semitic persecutions remain the same, is not a possible world.

We may find in Lewis’s theory a pragmatic response to that – a world in which Nazi Germany considers Jews a superior race – while all other conditions remain unaltered – would indeed require a large departure from actuality. This is still a possible world (perhaps its Nazi inhabitants behave irrationally and many weird things occur), but we lose our grip on it. In other words, far-fetched possible worlds are, in general, not informative or meaningful for counterfactual historians’ considerations. Lewis’s solution is simpler, as it does not require the postulation of the conditions of coherence for considering alterations to causal antecedents.

5.2 *The rise of counterfactual history*

It may be useful to survey some of the most influential books in counterfactual history.

An early approach to counterfactual history can be found in Robert W. Fogel’s *Railroads and American economic growth: essays in econometric history* (1964). Fogel did not believe that railways were a necessary condition for American’s GDP growth in the second half of the 19th

¹⁹⁸ ‘Night of Broken Glass’ refers to a series of violent attacks against Jews in Germany and in Austria (then part of Germany) on 9-10 November, 1938.

century. He hypothesised that in the absence of railways other modes of transport would have done the work *as effectively* as the trains actually did. Fogel was innovative in the sense that he used a number of econometric tools, and attempted to calculate the difference between the amount of goods actually transported by railways, and the amount of goods which would have been transported in the absence of these. He was criticised on the grounds that not all relevant factors related to the railway industry can be quantified.

It was only in the 1980s and 90s that counterfactual history really started to become more prominent. One of the reasons for the rise of counterfactual (or virtual) history was the increasing popularity of some philosophical ideas concerning counterfactuals and possible worlds (due to Stalnaker and Lewis). A better understanding of how *possible worlds* are characterised encouraged more historians to engage with counterfactual history. Some of the most remarkable recent examples of CTEs in history can be found in the following works.

In *Plausible Worlds: Possibility and Understanding in History and the Social Sciences* (1991) Geoffrey Hawthorn undertakes to study the role of counterfactual judgements in historical causal explanations. He differs from most counterfactual historians in the sense that he admits that it is unclear (in the sense that there is no theoretical answer to) which counterfactuals we should admit as true or plausible. He believes that in our *practical judgements* we presuppose some alternatives to be more plausible than others.

In *History That Never Happened: A Treatise on the Question, What Would Have Happened If...?* (1993) Alexander Demandt examines controversial claims such as ‘What if Pontius Pilate had pardoned Jesus?’ He argues that thinking counterfactually is an instructive tool for the understanding of the actual events, and how exactly they are causally related.

In *Counterfactual Thought Experiments in World Politics* (1996), edited by Philip Tetlock and Aaron Belkin, political scientists deal with a series of *What if?* questions in political history such as ‘What if Stalin had not been selected as the Party General Secretary?’ The contributors to this collection claim that thinking counterfactually is an essential tool for drawing conclusions about causal relations from historical data.

In *Virtual History* (1999) Ferguson dedicates a long and well-known foreword to the meaning of counterfactual history. He undertakes to explain how a chaotic approach to history favours CTEs and how it shows some ‘deterministic’ doctrines of history to be false. Most of Ferguson’s efforts are concentrated on attacking different approaches to historical explanation on the basis that such doctrines seem to endorse predictive views of history – of which he disapproves.

Robert Cowley edited two important counterfactual history collections: *What If?* (2000) and *What if? 2* (2001). In the papers in these collections it is argued that sometimes the ‘path’ of history seems to depend, fundamentally, on a single, crucial event. Cowley argues, for example, that if the British had won the Battle of Saratoga, the French would not have entered the American Revolution, and the United States would probably not exist as it is today.

In *Unmaking the West: ‘What-If?’ Scenarios That Rewrite World History* (2006), edited by Philip E. Tetlock, Richard Ned Lebow, and Geoffrey Parker, a collection of papers inquiries into the ‘inevitability’ of the rise of the west. The authors focus their efforts on making the somewhat obvious point that whenever we make causal claims, we accept that if some important historical causal antecedents had been any different, history would have unfolded differently than it did. They also argue, much more controversially, that without counterfactual reasoning historians easily fall into a biased ‘deterministic’ view of history.

Gavriel Rosenfeld, himself an author of studies in counterfactual history,¹⁹⁹ runs a very popular blog titled “The Counterfactual History Review”²⁰⁰ which collects and comments on recent examples of counterfactual reasoning in history. In a recent posting (10 August 2014), for example, Rosenfeld observes that US president Barack Obama uses counterfactual thinking to justify US international military interventions.²⁰¹

¹⁹⁹ *The World Hitler Never Made: Alternate History and the Memory of Nazism* (2005).

²⁰⁰ <<http://www.thecounterfactualhistoryreview.blogspot.co.nz/>>

²⁰¹ Asked whether it would have been a good idea to keep US troops in Iraq he replied: “in Iraq a residual U.S. troop presence would never have been needed had the Shiite majority there not squandered an opportunity to share power with Sunnis and Kurds. Had the Shia majority seized the opportunity to reach out to the Sunnis and the Kurds in a more effective way (...) no outside troops would have been necessary. Absent their will to do

Finally, New Zealanders too have engaged in counterfactual history in recent years. In two collections entitled: *New Zealand as it Might Have Been*, vols. 1 and 2 (2007 and 2010), a number of papers hypothesise on a variety of historical *what ifs?* ranging from questions such as ‘What if Nazi Germany had invaded New Zealand during WWII?’ to more irrelevant preoccupations such as ‘What if Edmund Hillary had not reached the top of Everest first?’ or ‘What if the All Blacks had lost the final test against the Springboks in 1981?’

5.3 Exemplifying the structure of a CTE in history

In “What if there had been no Treaty?” (2007),²⁰² Giselle Byrnes attempts to answer what would have happened to New Zealand had the *Treaty of Waitangi*²⁰³ not been signed. Byrnes identifies four possible outcomes from Hobson’s hypothetical failure to get the Maori chiefs to sign the Treaty:²⁰⁴ (a) Maori tribes would have continued to be autonomous and politically independent; (b) in the absence of a legal document assigning to the Crown the right to colonise the land, British forces would have taken control by force; (c) some time later another foreign nation (possibly France or the United States) would have claimed control over New Zealand; and (d) British settlers would have continued to colonise restricted areas that were already under the control of the Crown, and Maori tribes would have continued to rule independently in other areas.

She argues that scenario (a) is improbable: “It is totally against the run of evidence and logic to assume that New Zealand, with its rich natural resources, would have been exempt

that, our troops sooner or later would have been caught in the crossfire.” (Obama, as quoted in Rosenfeld, 2014).

²⁰² Byrnes gave a more extensive analysis in “What if the Treaty of Waitangi had not been signed on 6 February 1840?” (2006).

²⁰³ The Treaty of Waitangi was signed by British representatives and Maori chiefs on 6 February 1840. The interpretation of the Treaty is still a point of contention. According to one possible reading the Treaty established British sovereignty over New Zealand, while offering Maori protection, and guaranteeing the rights to land ownership.

²⁰⁴ Captain William Hobson was the British representative who had the task to get the Treaty signed. He later became the first Governor of New Zealand.

from some form of external colonisation.” Furthermore, “there is evidence that many tribes had, well before 1840, expressed a desire for British law and order and at the same time engaged in land transactions” (Byrnes, 2005).

Scenario (b), according to Byrnes, is also unlikely. There is evidence from economic reports of the time revealing that the British could not financially afford a military invasion of New Zealand. Furthermore, the Maori ‘warlike’ stereotype would have discouraged the British from prolonged military engagements.

Scenario (c) is more appealing given American and French presence in New Zealand; James Clendon was appointed American consul in 1838, in the Bay of Islands, and the French organised a settlement at Akaroa in 1840.²⁰⁵ Nevertheless, this is still an improbable scenario. There is no concrete evidence for the American or French ability and/or intention to colonise New Zealand.

Finally, scenario (d) seems the most likely. Byrnes argues that “as early as 1837 Hobson had proposed the crown assume power over certain sites in New Zealand, in much the same way that the British had established trading factories in India” (Byrnes, 2007). In these areas some kind of British-Maori official agreement would have been needed, but the rest of the land would have remained under Maori control.

The important question here is whether the counterfactual scenarios are of any value when compared to traditional historiography. Do CTEs provide historians useful insights? Do they help to causally explain events? Do they give evidence that history is chaotic? Works in counterfactual history usually share in common a desire to explain how useful counterfactual thinking may be for our understanding of history. I will now consider whether there is indeed any important epistemic gain from the use of CTEs.

²⁰⁵ Cf. Tremewan (1990).

5.4 *Why historians should entertain CTEs*

I have selected three possible defences for the usefulness of CTEs experiments in history.²⁰⁶ The first defence claims that some useful information about actual history may be obtained by appealing to counterfactual histories. The second defence claims that CTEs may be a useful tool for the weighing of the degree of importance of different causal antecedents when providing an explanation. The third defence claims that CTEs reveal history's 'chaotic' nature. I will defend the view that only the second claim is acceptable, and the acceptance itself is dependent on a careful distinction between causation and contingency – as we shall see.

5.4.1 *CTEs may provide historical 'insights'*

Counterfactual historians sometimes claim that to be able to engage in CTEs historians need first to achieve a deep and vast understanding of 'actual history.' Historians need to identify the causal relations that are important in providing explanations, showing that without these causes the effect would not have occurred, or not as it did. It is only after meticulous examination of evidence, and selection of data, and identification of causal antecedents, that an explanation acquires legitimacy. In other words, the counterfactual historian must be a skilled historian, in first place, and a compelling CTE may reveal such skills.

This defence of CTEs can be found in the work of Italian historian Franco Cardini:

With the [historical] 'ifs' one can bring out the infinite possibilities of the past, be it in terms of choices made by individuals and groups, or in terms of accidental events, one can capture the infinite variability of the processes that determine human, institutional and other structures' activities, and that also determine their outcomes (Cardini, 2005).²⁰⁷

Cardini believes that there is an obvious connection between actual and counterfactual history; by identifying causes for a historical event, we automatically consider why things have

²⁰⁶ An extensive list of alleged possible uses of CTEs in history can be found in D. Nolan (2013). I will focus only on the three most popular claims.

²⁰⁷ [My translation].

occurred this way rather than another. This is based on the belief that a satisfactory explanation needs not only to explain the causal chain that actually happened, but also to explain what has impeded alternative scenarios from occurring.

It is clear that adequate historical knowledge provides the elements for imagining alternative scenarios. But what does 'actual' history have to gain from the construction of counterfactual scenarios? We cannot say that imagining alternative outcomes and judging their plausibility in terms of our knowledge of 'actual' history, improves our historical knowledge. That would be blatantly circular.

One could possibly argue that 'what if' questions sometimes give historians useful insights. Considering alternative scenarios could alert the historian to the fact that s/he has to look for evidence to confirm or deny the plausibility of the alternative scenarios. And the evidence found would in turn give us useful information about what *actually* happened. In other words, the consideration of the alternative scenario and its plausibility may give historians suggestions as to where to look for evidence, and depending on their findings they may come across causal relations they had not previously considered.

As Jervis explains:

A great deal of thinking about causation (...) is based on comparing two situations that are the same in all ways except one (...) Counterfactuals show the limits of our standard comparative method and make us look for hidden connections that tie the system together (Jervis, 1996: 313).

In one of her 'alternative outcomes' Byrnes considers that the possibility that a French occupation of New Zealand could have motivated England to sign the Treaty with the Maori tribes. An alternative scenario in which New Zealand is occupied by French forces is only plausible if we can find some actual evidence supporting it. Let us say that the historian finds evidence of French plans for the occupation of New Zealand's soil, and then goes on to investigate whether the British were aware of such plans, and whether they truly feared that such an invasion could eventuate. Let us say that the historian finds such evidence, and then uses it to explain that one of the reasons for the signing of the Treaty was to give the British Crown a legal prerogative against the real possibility of a foreign invasion.

Hobson's mission would be characterised as an active intent to inhibit foreign invasion if we could find evidence for his desire to avoid this possible outcome. The evidence found would give us clues about Hobson's intentions and beliefs, and we would be in better position to explain his actions. Considering an agent's intentions is an important part of what historians do, and it is believed that CTEs may help cast light upon what the agent thought were his/her possibilities for acting.

There is here an implicit element of *re-enactment*²⁰⁸ associated with the construction of CTEs. It is claimed that counterfactual history is not just some sort of entertainment, but "a valid tool of the historian (because) by considering plausible alternative events historians are forced to analyse historical situations without the benefit of hindsight and a knowledge of what would follow (...) they must consider events *as did* the historical players" (Gini-Newman, 2003, pp 6 -7).

The empathetic exercise of trying to gain access to historical players' minds may indeed be informative. By doing so historians explore possible courses for action players might have considered, and according to what they know about their intentions they try make sense of their actions. Based on what we know about the agents and their circumstances we understand more easily why they decided to act as they did. For example, insofar as we hold that it was because Hobson feared a French invasion that he attempted to get a treaty signed with the Maori chiefs, we thereby accept that had he not feared the invasion he would (probably) have acted otherwise.

It is probably true that for some historians thinking counterfactually helps, or stimulates *insights*. But it is not an indispensable tool: CTEs do not provide historians any information they could not have discovered by other means. There is value in claiming that after imagining alternative scenarios we may direct our attention to some causal antecedents in particular. It is perhaps the case that some causal chains become more prominent. But this alone is not a reason to justify counterfactual history as a new variety, or even school, of historiography.

²⁰⁸ Explaining actions requires, in some situations, to understand the thought of the historical players. Historians must 'envisage' the situation in which that player stood and think out for himself what his thoughts were in that situation – this process is to be called *reenactment*.

It may be argued that in order to classify possible scenarios (or worlds) as more or less likely relative to actuality, counterfactual historians appeal to what we already know about a certain historical event, and its causal antecedents. Nothing is really ‘added’ to our knowledge of the past. Solid explanations of how some antecedents determine (or influence) the occurrence of a certain effect may satisfactorily explain why things happened just as they did and not otherwise.

It is also questionable whether CTEs as usually employed do indeed succeed in offering new insights, especially given that most publications in counterfactual history take well known and studied scenarios as their subject matter: the American Civil War, the assassination of Archduke Franz Ferdinand and the start of WWI, Hitler and the start of WWII, Gorbachev and the fall of the Soviet Union, etc. I conclude that CTEs are not an indispensable tool for the identification of actual causes, and that historians need no such experiments to make sense of their explanations.

5.4.2. *CTEs help with the ‘weighing’ of causes*

The best defence of the use of CTEs in history is related to what may be called the ‘weighing’ of causes. The issue here concerns how we select causal information in providing a satisfactory explanation. According to the kind of question historians are dealing with, different causal antecedents will be considered. Again, this raises the question whether historians can *objectively* identify causes. I will not attempt to answer this question. Instead, I will assume that if a number of relevant causes is selected, it is invariably the case that their attributed causal importance varies according to historians’ interpretation of data.

The present claim says that CTEs may help settle which causes are interpreted as being more explanatorily predominant or fundamental. Historical counterfactuals may be constructed so as to “isolate the effects of each cause separately, and then compare them with the actual historical result, the greater the difference, the more important the cause” (Tucker, 1999: 266).

That most historians care about the weighting of causes is easily observable. As in Chapter 2, Carr (1964) is an example of a historian who believed that only causes which were perceived to be instances of regularities or trends have any real importance. We can only learn lessons from *regularities*, not from *accidents*, claims Carr. In the case of Berlin (2002) we have an interpretation of history in which free action is understood to be the most fundamental aspect of history. Although agents are influenced by the circumstances, or even ‘impersonal forces,’ it is always the case that agents *can act otherwise*. It would only be natural from this interpretation to highlight the causal importance of action over other factors, or the fact that many historical events are the result of accidents.²⁰⁹ In both cases the weighing of causes seems to be an important element in providing explanations.

How exactly do CTEs assist in the attribution of different causal weight to causal antecedents? The answer has to do with two notions which will now be carefully defined: historical *contingency* and *necessity*. It is also important to explain how *necessity*, in the sense to be defined, differs from *causation*.

5.4.2.1. Historical contingency/necessity

In “Historical Contingency” (1997) Yemima Ben-Menachem provides an explanation of the concepts of historical ‘necessity’ and ‘contingency.’ She explicates and defends such a distinction while retaining commitment to the claim that all historical events – while contingent or necessary – are caused.

The distinction between historical contingency and necessity is not to be understood in the same ways philosophers use these terms. Philosophers recognise several distinctions for contingency and necessity. For example, one way philosophers express these notions is by means of possible worlds terminology: a proposition *A* (the proposition that a certain historical event *a* occurs) is *necessarily* true *iff* it is true in all possible worlds (event *a* occurs in all possible

²⁰⁹ Good examples of such accidentality are: Gibbon’s claim that had the Turkish sultan Bayezid I not suffered from gout, he could have successfully conquered most of Central Europe; or Winston Churchill’s belief that had King Alexander of Greece not died of a monkey bite, the Greco-Turkish War could have been avoided. Cf. Carr (1964: 98).

worlds). Or, *A* is *contingently* true *iff* it is false in at least one possible world and true in at least one other possible world (event *a* does occur in one world, and does not occur in one other). According to this distinction, it is obvious that all historical events are contingently true, as they are true in the actual world, and will be false in some possible world. But this is not what is meant when historians qualify an event as *contingent* or *necessary*, and the difference in the use of terminology is a frequent cause of confusion.

Ben-Menachem's suggestion is that instead of criticising historians for promoting a careless use of language, philosophers would provide more constructive criticism by proposing definitions for historical necessity/contingency, explaining what this contrast ought to mean in historiographical terminology. According to Ben-Menachem, historical *necessity* and *contingency* are not 'modal' notions – not concerned with different ways in which propositions can be true, but rather with different degrees of 'sensitivity' to initial conditions that historical events may have.

Ben-Menachem says that “we tend to speak of *necessity* when we think that what happened had to happen, and of *contingency* when we think things could have happened differently” (1997: 100).

Let us see how, based on this contrast *necessity* may be defined:

(...) what we mean by *necessity* is that the same type of final outcome results from a variety of different causal chains (...) It is acceptable to speak of degrees of necessity [when] the final outcome is relatively insensitive both to initial conditions and to potentially disruptive intervening events. In this type of case, the result seems necessary to some degree, since even were the earlier conditions different, and even were additional factors to have intervened in the causal process, the process would probably still have resulted in the same kind of outcome (*Loc. cit.*).

In contrast to necessity Ben-Menachem defines *contingency*: “a type of outcome is contingent if there is only one course, or at most, a few courses, that could possibly lead to it, and there is sensitivity to initial conditions and intervening factors” (*Loc. cit.*).

Based on Ben-Menachem's definition, we may say that a historical event is necessary *iff* an event of that type would have occurred across a wide range of alternative antecedent conditions (think of cases of overdetermination). Otherwise it is contingent.

Contingency and necessity are terms commonly found in historical explanations. It is not unusual to come across claims of the type: 'our defeat in the battle, given the numerical superiority of the enemy, his superior tactical position and favourable weather conditions, was the *necessary* outcome.' Such an explanation may have practical consequences, for example, in justifying why some military commander should not be blamed for a defeat. It is said that an event of the type of a military defeat would probably have occurred irrespective of the commander's tactical abilities, given the wide range of causal chains leading to it and the low degree of sensitivity to initial conditions.

In other type of cases, however, historians may be of the opinion that a certain outcome is contingent. A typical example would involve the alleged importance of some individual's achievements or failures. Historians may be of the opinion that someone's actions are the effect of a very sensitive chain of causal relations. In such case the outcome is not said to be necessary, but contingent on that person's actions.²¹⁰

An extreme case of necessity would amount to a sort of *inevitability*: a certain type of event is the necessary (inevitable) effect of some initial conditions. A good example would be 'all possible courses for life will inevitably lead to death.' In contrast we may have a case of extreme contingency when a certain type of effect is the result of a highly sensitive causal sequence. Maybe it is so because there is only one (or very few) causal chain that could lead to it; "speculations about how history might have differed had Cleopatra's nose been longer vividly illustrate this sense of contingency" (Ben-Menachem, 1997: 101).

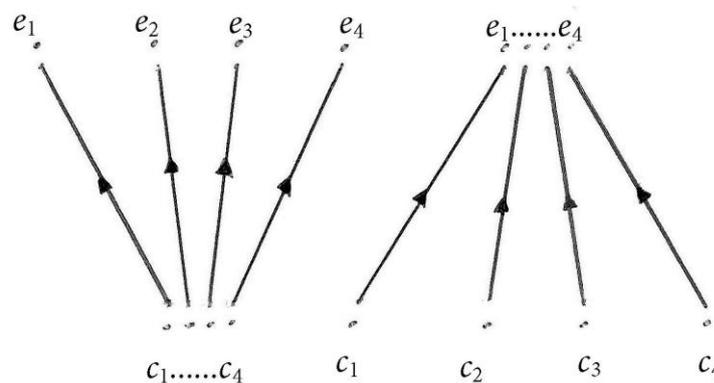
Necessity and contingency occupy different ends of a spectrum, and are not absolute notions; they come in *degrees of stability*, or *sensitivity* to initial conditions. A stable system is thus

²¹⁰ Counterfactual historians tend to think that when a certain kind of outcome is the result of agency, it is usually a case of contingency. This is presumably so because individuals tend to be more sensitive to changes to the initial conditions. But this need not always be the case. It is important to investigate the existence of other causal chains ('backup' causes, if you will) that would have led to the same kind of outcome had the individual decided to act differently than he did.

insensitive, an unstable one is sensitive. In a stable system of causes and effects, small changes to the initial conditions make little difference for the occurrence of a, say, type-*E* effect. In unstable systems small changes to the initial conditions do make a difference; instead of a type-*E* effect, we have an effect that is of a different (not-*E*) type. Scenarios of necessity are relatively stable, while more contingent scenarios are relatively unstable.

We may illustrate this distinction with the following diagram:

Figure 5 – Contingency/necessity²¹¹



On the left side of the diagram we have closely similar causes ($c_1 \dots c_4$) leading to different types of effects ($e_1 \dots e_4$), which illustrates *high sensitivity* to initial conditions. On the right side we have different *types* of causes ($c_1 \dots c_4$) leading to closely similar effects ($e_1 \dots e_4$), which illustrates a case of *low sensitivity* to initial conditions. In the diagram, the closeness of the events on the page models a high degree of similarity – the separation of events models a high degree of dissimilarity.

Extreme situations of historical *necessity* or *contingency* are not common, and historians are entitled to speak of degrees up to the least contingent viz. the necessary. Simplifying the definition just given, we may say that as *contingency* and *necessity* are opposite to each other: *contingency* increases in direct proportion to sensitivity to initial conditions, and *necessity* increases in reverse proportion to them. This approach, as propounded by Ben-Menachem, avoids the conflation of causation and necessity. Events have causal histories, but the effect that

²¹¹ Adapted from Ben-Menachem (1997: 101).

occurs is said to be more or less necessary, and correspondingly, more or less contingent. As a result “we can get a high degree of contingency in a perfectly deterministic process, so causality does not entail necessity” (Ben-Menachem, 1997: 103). Causation and necessity are both relevant to historical explanations, but we cannot reduce one to the other, as Ben-Menachem explains.

The present definition of historical contingency may help bring some clarity to the controversy we discussed in Chapter 2, between Carr and Berlin. Both authors would certainly agree that historical explanations are causal, but they would probably take different sides on the importance of the *necessary* or of the *contingent* in history. When Carr says that historians should look for regularities, or trends, he seems to be saying that the historian is in the business of showing the ‘necessity’ of historical events – but of course, he need not hold that most historical events *are* necessary. Since historians should focus more on necessity, it is usually the case that had any causal antecedent (to the event they want to explain) been different, a similar outcome would still occur. For this reason Carr dismissed CTEs for having no epistemic value (Cf. Carr, 1964: 97).

Berlin, on the other hand, drawing attention to the role of chance, accidentality and unpredictability in history, would probably say that historians are in the business of showing the contingency of historical events. Ferguson says something similar, and summarises the view of counterfactual historians: “we should never underestimate the role of contingency, of chance, of what mathematicians call stochastic behaviour” (1999: 416). Since historians should focus on contingent events, it is usually the case that had any causal antecedent (to the event they want to explain) been different, a dissimilar type of outcome would occur. For this reason Ferguson (and Berlin would probably agree) believes CTEs to be a useful explanatory tool (Cf. Ferguson, 1999: 1-20).

Both contingency and necessity *require* causation. Pascal’s remark about Mark Anthony’s fascination with Cleopatra’s nose is an event with a causal history. The effect, the fall of the roman Republic, is understood to be very contingent: had the nose been less attractive he would not have fallen in love with Cleopatra, and things would have occurred very differently. This is, of course, an anecdote; no historian would seriously entertain to explain the occurrence

of a type of effect such as the fall of the Roman Republic as ‘contingent’ on the beauty of Cleopatra’s nose. However, Mark Anthony’s choices as a political leader could be characterised as relatively sensitive to changes in the initial conditions. Emphasising accidental detail may, or may not be important – how much attention should be given to accidentality can only be decided on a case by case basis. *Contingency* offers no support for the claim that the relation between causes and effects, in history, are not causal, or not causally deterministic. Ben-Menachem’s distinction offers a solution for those who believe that an important part of history is very contingent, and at the same time maintain that explanations are causal.

Ben-Menachem also brings attention to the fact that the separation of necessity and causation has practical consequences for the free will problem. According to her, hard determinists, compatibilists and libertarians will be interested in assessing the overall importance of some action in terms of its relative *contingency/necessity*. In the light of historians’ use of such terminology it does not really matter if an action was truly ‘free’ or determined by factors outside the agent’s control. An action (taken in isolation) is perceived as making more of a difference when it is taken to be a causal factor in a sensitive (contingent) chain belonging to an unstable system, and less of a difference if it is part of an insensitive (necessary) chain belonging to a stable system.

Taking into consideration the previous examination of the meaning of historical contingency, we reach an important conclusion. By carefully distinguishing between causation and necessity historians need not adopt opposite positions regarding what *is* the most fundamental historical category: *necessity* (as exemplified by Hegel’s historicism) or *contingency* (as exemplified by Berlin or Ferguson). Intermediate positions on the spectrum necessity/contingency may be taken and defended, and explanations remain causal even if the effect to be explained appears to be very contingent.

5.4.2.2. *Exemplifying historical necessity and contingency*

The contrast between *contingency* and *necessity*, and how these terms are used in history, shows that Lewis’s theory of causation captures quite well how historians talk of causal relations.

The degree of necessity/contingency of an effect is determined by elements such as the number of causal chains leading up to the effect, and the level of sensitivity of the causal chains to external interferences. So, it is only natural that historians sympathetic to the use of CTEs in history would think of causation as a relation of counterfactuality between event-tokens.

Consider that an event of a type-*E* *does* occur and is caused by certain antecedents (c_1, c_2, c_3). There are, in general, two kinds of CTE:

(1) Historians may be of the opinion that an event of a type-*E* would have occurred had any of the causal antecedents not occurred or occurred differently. In this case the altered scenario has an outcome which is also a type-*E* event, i.e. it is an effect sufficiently similar to the effect that actually occurs. This is a case in which the effect is considered more necessary.

(2) Historians might be of the opinion that an event of a type-*E* would not have occurred, had any of the causal antecedents not occurred or occurred differently. In this case the altered scenario has an outcome which is not a type-*E* event, i.e. it is an effect sufficiently dissimilar to the effect that actually occurs. This is a case in which the effect is considered more contingent.

Observe that Lewis's theory of causation holds in (1) and in (2); the occurrence of an effect *e* is counterfactually dependent on the occurrence of the set (c_1, c_2, c_3), and this is so independently of the understanding of *e* as relatively more necessary or more contingent.

Let us go back to our initial question: Is counterfactual history of any value? The answer depends on our acceptance of the view that such speculations help us settle the 'weight' or importance attributed to different causal antecedents. If this seems acceptable, then the answer is yes. The point is not so much about writing 'history that never happened', but to use a CTE to convey historians' idea that a certain type of event was either (1) expected to occur, perhaps as a result of a number of long term social and economic forces – in which case they regard it as necessary –, or (2) as the outcome of sensitive causal chains, perhaps the result of accidentality or free agency – in which case they regard it as more contingent.

Let us illustrate how comparing relative levels of contingency works. The first example fits into category (1), and the second into category (2).

In 'Camelot Continued' (1999) Diane Kunz considers whether the United States would have fully entered the Vietnam War if John Kennedy had not been killed in Dallas and had then gone on to win the 1964 election that Lyndon Johnson won. She concludes that if that were the case there is no evidence supporting the view that things would have evolved substantially differently from what actually happened. Kunz claims that just before his death in 1963, Kennedy was thinking about withdrawing some of the troops from Vietnam, based on advisors' reports that the current level of aid to the South Vietnamese was sufficient for a victory. A few months later the same advisors realised they calculated it wrongly, and Johnson was forced to act decisively. Kunz reasons that Johnson was more concerned with domestic affairs than external policy; therefore his ordering a withdrawal was more likely than Kennedy's. She concluded that had Kennedy survived and won the election, an American active engagement in the war would still have occurred. In other words, after the introduction of an alteration (Kennedy surviving and winning the election) a similar type of effect was (likely) to occur.

Kunz's analysis is a CTE in which the effect is placed at the 'necessity' end of the spectrum. The CTE was used to argue that the Vietnam War is a more necessary event, since an alteration to its causal history (Kennedy is the US president, and not Johnson) would not have made much of a difference. We may argue that Johnson's level of historical responsibility (as attributed by historians) for the American participation in the war decreases accordingly; the causal importance of Johnson's leadership is understood to be less important than the wider general context of international relations during the Cold War. This is an important consideration, as it affects the way we understand and explain the event in question.

An example of a different kind of CTE is offered by Mark Almond in '1989 without Gorbachev' (1999). In this paper Almond considers the causal importance of the General Secretary Mikhail Gorbachev's reform acts to the fall of the Soviet Union. Almond reviewed some reports from the 1980s in which political analysts were trying to predict the longevity of the Soviet Union. The reports claimed that the Soviet Union was not coming to an end any time soon, and Almond claims that the analysts' prediction would have been right if it was not for the

fact that the new Soviet leader, Mikhail Gorbachev, was of a good character. According to Almond, Gorbachev failed to recognise how irresilient the Soviet system was until he tried to conduct some reforms which ended up with the fall of communism. Almond believes that a more ‘cynical’ leader would have found a way to prolong the life of the Soviet Union, by dealing with the economic and political challenges (such as high oil prices and outdated technology) using other ‘methods.’

If we are prepared to accept Almond’s CTE as plausible, then it follows that we see the causal importance of Gorbachev’s actions to be high. The fall of the Soviet Union is placed at the ‘contingency’ end of the spectrum. The type of effect that occurs is causally dependent on Gorbachev’s actions – had Gorbachev acted differently, a different type of effect would have occurred, such as the Soviet Union surviving for much longer than it actually did.

The two examples of CTEs in history I have presented were simplifications. I have not reported the richness of detail we find in Kunz and Almond’s works. Against the plausibility of these CTEs someone could argue that there are many variables involved in both cases, and for many of these we have no adequate evidence. It can be argued, more generally, that historical events are complex, and that it is illusory to think that we can actually assess one outcome as more likely than others. Most attempts of prediction in history failed, one can say, exactly because of history’s complex nature; because all historical events are connected in an intricate web of causal relations.

We may say in response that a CTE is not decisive with respects to objectively assessing the *actual* importance (degree of causal influence) of different causal antecedents. A CTE is merely an expressive or teaching tool;²¹² it conveys the historian’s interpretation of the causal origins of a certain event, and how s/he hierarchises different antecedents in terms of importance, always relative to a certain question. The attribution of causal importance when providing an explanation is the result of the historian’s intuitions about where, in the necessity/contingency spectrum, an effect is to be placed. We may hope that the historian’s interpretation, and his/her weighing of causes, somehow resembles the antecedents’ actual degree of causal influence. As long as we do not overrate the usefulness of a CTE, we may see

²¹² A recent defence of the use of CTEs in history education can be found in Huijgen, T. and Holthuis, P. (2014).

that the criticism of the previous paragraph is more general, it applies to historians' overall ability to identify relevant causes, more so than it applies to the usefulness of entertaining CTEs.

5.4.3. CTEs show that history is chaotic

In Chapter 3, I explained what a chaotic system is and the consequences it has for the doctrine of determinism. I concluded that chaos, even in a quantum world, would not rule out the possibility of ontological determinism (**B**), only predictability in its ordinary sense.

Counterfactual historians such as Reisch (1991) and Shermer (1995) believe that historical systems deemed to be very sensitive to variation to the initial conditions, or highly 'contingent,' in the sense just explained, can be compared to chaotic systems. For Reisch, 'chaos' was just a metaphor. Shermer took the comparison more seriously and even tried to incorporate many ideas originating from the analysis of chaotic systems into historiography – but with little success. Roth and Ryckman (1995) criticised Shermer's approach on the basis that history resists quantification, and quantification is essential in understanding the patterns of chaotic behaviour.

A bit more needs to be said about the chaotic approach to history, nicknamed by Ferguson as 'chaostory' (1999). This section will attempt to answer the question: 'can CTEs show (much of) history to be chaotic?'

In the light of the previous explanation of necessity and contingency, we may simply say that the view that history is chaotic amounts to the claim that it exhibits an extreme degree of contingency. In other words, if history were quantifiable, and if we could identify the laws that govern the evolution of historical processes (or systems) then perhaps it could be shown to be chaotic – although it could be argued in response that at least some historical processes do not appear chaotic at all.

Ferguson defends historians' appealing to counterfactual or alternative scenarios as some sort of validation of his view that history is chaotic: "the events historians try to infer from [various material] sources were originally 'stochastic' – in other words, apparently chaotic –

because the behaviour of the material world is governed by non-linear as well as non-linear equations” (1999: 89). Ferguson’s perspective of history is anti-historicist and anti-teleological; he attacks all sorts of approaches to historical explanation which somehow entail predictability, calling them, in his idiosyncratic sense, cases of ‘historical determinism.’ He claims that it is always the case that alterations to historical antecedents would cause important differences in the types of consequent events.

By considering alterations to historical antecedents, and postulating plausible narratives of alternative histories, Ferguson intends to defend the application of chaos theory to history:

(...) all that chaos theory says is that certain realms of the world are governed by equations so complex, non-linear equations, that it’s extremely difficult (...) to predict patterns (...) there are causal sequences, but they are so complex that the outcomes appear to be random and therefore are extremely hard to predict (...) chaos theory works very well for history because it says the world is actually governed by causal relationships, causation is real, but is highly complex. And therefore to imagine that one can predict it or reduce it to a few linear laws is hopeless and fanciful (Ferguson, as quoted in Gini-Newman, 2003: 6).

The motivation behind Ferguson’s remark, and behind the overall chaotic approach to history, seem to be that chaos does away with prediction, but the sort of explanation chaos theory provides is still causal. This idea was also put forwards by Ben-Menachem: “chaotic phenomena (deterministic chaos) illustrate a failure of predictability without failure of causality. Identical causes have identical effects, but similar causes do not, in general, have similar effects” (1997: 102).

Extreme sensitivity of the system to its precise initial conditions is chaos’ most important feature. In historical contexts this translates into high levels of contingency. But the sensitivity of historical causal chains, although at times clearly significant, may not be as radical as that of chaotic phenomena. In an example given by Ben-Menachem: “if only [the commander] managed to get two hours of sleep (...) the defeat could have been prevented” (*Loc. cit.*) It seems easy to understand counterfactual historians’ intuition that the actual outcomes of many historical processes did not *have to happen* as they did ‘if only’ some little thing in the causal history of the effect had been different.

A similar example is given by Reisch, for whom chaotic behaviour is deterministic, and law-governed, and all explanations causal. Still, he speculates: “what the world might look like today if a person only slightly more combative than Khrushchev had been at the Soviet helm during the Cuban missile crisis? – certainly very different if nuclear war had ensued” (1991: 6).

It seems correct to say that human actions, considered quite independently from a larger social, cultural environment, may metaphorically be compared to the behaviour of a chaotic system: even slight differences to an agent’s emotional state caused by, for example, not having had an enjoyable breakfast that day, could potentially be followed by different actions and different types of outcomes. But history is not just biographies,²¹³ and when historians refer to the evolutionary trajectories of nations, economic systems, culture and religion, they seem to consider larger processes that are not chaotic.

Reisch grants that in the case of Khrushchev’s actions during the missile crisis it is difficult to relate the Russian’s temperament to the fact that World War III did not occur – and consequently justify the effect as more contingent. We must count on the possibility that Khrushchev anticipated for years what he would do, should he receive a US final warning to remove the missiles from Cuba. Perhaps it is the case that his actions were “expressions of larger and more stable social and economic forces whose particular strength and direction were insensitive to his idiosyncrasies” (Reisch, 1991:8).

Ferguson has been criticised, rightly in my view, for his exaggerated emphasis on the alleged chaotic trajectory of historical causal processes. Clearly, many contemporary historians also interpret historical causal processes as non-chaotic (although perhaps less so today than in the past). John Reilly, for example, asserts that “(...) changes from generation to generation in modern times do not appear to me to be altogether chaotic. There are conspicuous *linearities*²¹⁴ in the history of the past few centuries, whether we are talking about population growth, economic output or even longevity” (2000).

²¹³ Thomas Carlyle believed that history was basically the biographies of ‘great men’ (1888).

²¹⁴ Non-chaotic processes. Chaos theory presupposes laws, which are said to be *non-linear*. As a consequence, proponents of chaostory (Reisch, Shermer and Ferguson) talk of *non-linear* history.

Reilly believes that some of these (apparently) non-chaotic processes seem “to be impervious to minor accidents. It is probably also true that certain cultural trends over the same period have a trajectory with a similar, intrinsic durability. If these things are true, then counterfactuals must respect a ‘main sequence’ that runs through modernity.” Reilly finally concludes his critique by claiming that “(...) there have been periods like modernity in other times and places, and even [periods in which] history as a whole has a loosely defined trajectory” (*Loc. cit.*).

I do not see how CTEs could possibly be used as *evidence* for the alleged chaotic aspect of history. I said that *if* history is chaotic, *then* we would probably interpret most historical processes as highly contingent. But is the other way round true? Can we say that because we interpret the occurrence of some effects as the result of sensitive chains, history must fundamentally (ontologically) be chaotic? What if our failure to predict future states of the world is only the result of our epistemic ignorance? Would it not be the case that the high degree of contingency attributed by historians to many effects is false, and that some unknown underlying non-chaotic (insensitive) process explains the effect? The whole perception of chaos may be false.

The context of enquiry may also change the degree of sensitivity we perceive: “where large ensembles of systems are concerned, we might find a large degree of overall necessity due to the law of large numbers, but a low degree of necessity at the level of individual events” (Ben-Menachem, 1997: 102). Can we still say that much of history appears chaotic, if our context of enquiry deals mostly with long-term structural changes? Chaos would seem less obvious in that case.

Furthermore, I believe that the kind of CTEs historians like to entertain, such as alternative outcomes in military history, are rather the product of precisely a desire to interpret history, in general, as highly contingent. It seems that the comparison of alternative outcomes in history is justified by our intuitions on the sensitivity of causal chains. And CTEs illustrate these intuitions well. But the intuition itself cannot be justified by simply assuming that most historical events are the outcome of real chaotic processes. In short, CTEs do not *show* history to

be chaotic, they simply *illustrate* historians' views on history as potentially resembling the sort of behaviour physicists perceive in highly sensitive (chaotic) systems.

5.5 Lewis on the 'sensitivity' of a causal chain

In Chapter 1, I explained Lewis's definitions of causation; *counterfactual dependence* and *patterns of causal influence*. In the postscripts to "Causation" (1986b: 172-213) Lewis added an important discussion to his thesis of causation, by including a characterisation of the degree of sensitivity/insensitivity of a causal chain. This notion is important, and I take it to be very similar to Ben-Menachem's characterisation of historical contingency/necessity.²¹⁵ Although similar, Lewis's explanation avoids the use of potentially confusing contingency-necessity terminology, which would certainly be an advantage. Let us see how Lewis defines sensitivity of a causal chain, and how it could be a useful notion for historians.

Causal chains may well be long and complex, and historical causal chains are no exception. It may be practically impossible to do, but any event taking place in 2014 can, in theory, be traced back to ancient times. Any action of mine has consequences for a number of different causal chains. In the same way, the Big Bang, if it truly occurred, is a cause of everything else that occurred afterwards.

As Lewis exemplifies:

(...) suppose I write a strong recommendation that lands someone a job; so someone else misses out on that job and takes another; which displaces a third job-seeker; this third job-seeker goes elsewhere, and there meets and marries someone; their offspring and all their descendants forevermore would never have lived at all, and *a fortiori* would never have died, and so presumably their *deaths* would not have occurred, but for my act" (1986b: 184).

²¹⁵ One of the differences is that for Lewis *sensitivity* relates only to the conditions *surrounding* a causal relation, and the potentiality of these to interfere with it. Ben-Menachem talks of *sensitivity* to alterations to both the initial conditions *and* susceptibility to interfering events, without really making a distinction.

The reason Lewis talks of sensitivity is to distinguish between ‘killing’ and ‘causing to die.’ In the previous quote by Lewis, the writing of a recommendation letter is a cause of someone’s death. So, is the referee killing someone? That would seem a mistaken conclusion. But the referee *is* part of a causal chain that leads to someone’s death, so s/he is ‘causing to die,’ in a certain way.

We need a proper explanation of how killing and causing to die may differ, and this is where (*in*)*sensitivity* comes in. The idea is that when a given effect *e* depends on a cause *c* (*c* causes *e*), it also depends on much else. The ‘much else’ may be called the *surrounding conditions*. Lewis says that: “If the cause had occurred but *other circumstances* had been different, the effect would not have occurred” (1986b: 186). If this is the case, then the causal chain is said to be sensitive, and sensitivity itself “is a matter of degree (...) it may be that the causation depends on an exceptionally large and miscellaneous bundle of circumstances all being just right” (1986b: 187). In the case of the referee causing someone’s death, it is easy to see that the causal chain is very sensitive, as any little thing in the surrounding circumstances being different would have a great impact on the final effect that occurs. Many little differences could have deflected the chain of events leading to that person’s death.

In contrast, since sensitivity is a matter of degree, we may have cases where the causal chain is very insensitive indeed. Here we may say that ‘if the cause had occurred and *other circumstances* had been different, the effect would still have occurred.’ Lewis’s example is that of someone shooting a person at point-blank. Is it the case that differences to the *surrounding conditions* where the shooting occurs may interfere with the killing? It seems unlikely. It would have to be an interfering event intercepting the bullet, preventing it from hitting the target; or an interfering event dislocating the target from his/her position.²¹⁶

We now may define *killing* as a causal relation which occurs through a relatively *insensitive* chain. The point-blank shooter *kills*, the referee *causes to die*. In fact, most cases of causing to die are not cases of killing. “And if this is so for killing, perhaps it is so likewise for

²¹⁶ We cannot say that the gun misfiring would be an interfering event; the misfire is an alteration to the *initial conditions*, and not the *surrounding conditions*.

other causatives. Consider the ways in which you can and can't make, break, wake, or bake things" (1986b: 186).

Consider a classical historical example, the case of Archduke Franz Ferdinand's death. We know that Ferdinand was killed as a result of a plot. On the fateful day, there were six killers on the streets of Sarajevo, waiting for the right moment to attack and kill the Austrian-Hungarian heir. They were placed on different sites of the city, where Ferdinand was expected to pass by, so that if the first assassin failed, the second had a chance to accomplish the task, and the third, and the fourth, and so on.

We may, I believe, describe the case of Ferdinand's death as a case of relative *insensitivity*. In part, the insensitivity arises from the fact that the event seems over-determined. Is it the case that minor differences to the surrounding circumstances would prevent the killing? As a matter of fact, the first two assassins failed to act. The third threw a bomb at Ferdinand's car, but it took longer than expected to explode (or maybe the car was too fast), and Ferdinand escaped unharmed.

Later on that day, after thinking the assassination plot to have failed, one of the assassins, Gravelo Princip, found an opportunity to shoot and kill Ferdinand when the Archduke's *chauffer*, Leopold Lojka, took a wrong turn and the car failed to reverse.²¹⁷

Someone may object to the example on the basis that Princip killing Ferdinand was indeed the result of a series of surrounding circumstances being 'just right.' If we focus solely on Princip bumping accidentally into Ferdinand, it may well be so. But I want to say that the presence of six different killers, armed with bombs and pistols, increase the probabilities of Ferdinand being shot dead, and decrease the probabilities of there being interfering events preventing his death. The killers were trained, Ferdinand's car did not have a roof, the distance of the crowd from Ferdinand' car was small, and so on. These considerations would certainly be

²¹⁷ The car, a 1911 Gräf & Stift Double Phaeton, was not equipped with reverse gear (Cf. Tonkin, 2014). We may say that the absence of a reverse gear is *a* cause of Princip finding the opportunity to shoot Ferdinand. The chain (c) Princip shoots, and (e) Ferdinand dies, may be said to be relatively *sensitive* in this respect; had the *surrounding conditions* been different (for example, assume the car had a reverse gear) an event of a type *E* would (possibly) not have occurred.

relevant to defending the view that the plotters *killed* Ferdinand, and did not simply *cause him to die*. This is a case of relative insensitivity.

Also relatively insensitive is the causal link between the assassination in Sarajevo (*c*) and (*e*) the outbreak of the Great War at the end of August. The assassination set in motion a number of treaty obligations, and the mobilisation of different nations into war. In this case we have the little chain of events leading up to the assassination being part of a much larger ‘European’ causal chain. It may be said that the causal relation $c \rightarrow e$ is such that the chain is unlikely to be cut by alterations to the *surrounding conditions*. Had one of these conditions been different, the effect would have been very similar in kind.

The referee in Lewis’s example, and also Princip, are part of the causal history of someone’s death. But the former is part of a relatively sensitive chain; the latter is part of a relatively insensitive chain. In the case of sensitivity, though we have causal determinism, we find unpredictability: “The more the chain depends on a lot of circumstances being just right, the harder it is for a would-be predictor to know all he needs to know about the circumstances. The sensitivity of the chain is an obstacle to prediction” (Lewis, 1986b: 187).

In *chaostory* terminology sensitivity is said to be similar to the evolution of a chaotic system, and insensitivity similar to the evolution of a non-chaotic system. The latter does not allow prediction, while the former does. But chaostory forces historians to commit to the existence of non-linear laws; to say otherwise is to misinterpret chaotic theory. But historians are, in general, unwilling to make such commitment. The case of sensitivity/insensitivity explains how some causal chains are easily disrupted by interfering events, coincidences, etc.. It is part of the theory of causation that it concerns single events only, and their causal relationship (or lack of it). No reference to laws is needed. It also explains the case of predictability: “If a chain is insensitive enough that you can predict it, then it is insensitive enough that you can kill by it” (*Loc. cit.*).

Lewis theory of causation, associated with the idea of sensitivity, explains one aspect of chaos theory; sensitivity of the causal chains associated with an overall difficulty in predicting. It may also explain another feature of chaos: complexity. Chaotic systems are usually complex.

But an *insensitive* chain may be complex as well. Lewis illustrates insensitive complexity with a lethal Rube Goldberg machine which kills in many steps:

(...) it may be full of thousand-year fuses and randomizers (...), its working may require the responses of unsuspecting agents, there may be no way to discover how it is built (...); and yet (...) the causal chain running through it may be far more independent of ‘intervening coincidental events’ than are most of the causal chains in the wider world (Lewis, 1986b: 186).

The key to understand insensitivity is to think of a chain which leads to completion independently (to some degree) of fortuitous circumstances; whether it is a simple or complex chain, it does not matter much. Complex chains may be insensitive. But it is true that simplicity (the point-blank killer) usually involves insensitivity. And overdetermination, in general, involves insensitivity as well (as in the case of the Archduke’s assassination).

There is one more element I would like to explore. A central aspect of Lewis’s theory of causation is *fragility* (see §1.3.4.3). Now, what is the difference between *sensitivity* and *fragility*?

Sensitivity refers only to the causal chain, while fragility refers to the identity conditions of an event. In the case of Ferdinand, imagine that the bomb thrown by the third assassin succeeded in killing Ferdinand. Would we want to say that its effect (Ferdinand’s death) is a different event from the death he died by the hands of Princip? I think we would consider it a different event; the bomb would make a difference to the *how*, *when* and *where* Ferdinand dies. We have relative insensitivity of the causal chain between the combined plotting of the various would-be assassins and Ferdinand’s death: if only one of the killers succeeds, Ferdinand dies. For the sake of the insensitivity of the causal chain, it does not matter much if Ferdinand dies by an explosion or by being shot: the final effect is similar in kind. But the effect is said to be *fragile* in its identity; in terms of the individuation conditions of the event it does matter whether he dies by being shot or as a result of an explosion.

An event is said to be very fragile if any alteration to its antecedent would be followed by alterations to the effect, and any minor alteration would make us want to consider the effect a different event. How we individuate an event is an independent matter from *sensitive* or

insensitive causation. But it seems reasonable to suppose that cases of sensitivity are likely to produce relatively fragile effects; if an event interferes with the causal chain, it would be remarkable if the final effect were similar or ‘the same’ event which would have occurred had the interference not occurred. *Fragility* and *sensitivity* combined are capable of expressing a similar idea to historical necessity/contingency.

Lewis’s theory may offer some guidance with the motivation behind counterfactual history; the different attribution of causal weight. Imagine that a historian is providing an explanation of how Ferdinand was killed in Sarajevo on July 28, 1914. Because I said that the chain between the combined plotting of the various would-be assassins and Ferdinand’s death is relatively *insensitive*, it seems that a complete explanation would have to reveal the existence of the plot, the six assassins, and the fact that Ferdinand escaping unharmed was unlikely. The insensitive status of the causal chain seems to be part of an adequate explanation. The explanation is *narrative* in form and reveals important aspects of the causal history of the event. Because of the relative insensitivity one may feel that the description of the surrounding conditions is less important than the causal chain linking the six assassins’ plot with Ferdinand’s death.

However, if a causal chain is said to be sensitive, as in Lewis example of the referee causing someone to die, a causal explanation would need to describe all the *relevant surrounding conditions* (all conditions being ‘just right’) in order to make sense of the causal claim that the referee is part of the causal history of one’s death. It may be the case that an event is caused by a series of unexpected coincidences, yet this need not be the result of chance (indeterminism). The accidental view of history (as seen in Chapter 2) may be rightly captured by the idea that many historical events are the result of sensitive causation. The possibility of causal determinism, of course, still holds – which leads us to the next section.

5.6 CTEs in history and determinism

In the previous sections I draw special attention to the intuition that believing history to be highly contingent (or chaotic, or sensitive to changes in initial and/or surrounding

conditions) favours engagement with CTEs in history. This is so because we see overdetermination as stable, durable and relatively insensitive causal structures, and less so in the case of events with a single and presumably more sensitive causal chain. Does the use of CTEs in which much of history is taken to be very contingent undermine the possibility of historical determinism? Ferguson seems to believe it does: “Virtual History is a *necessary* antidote to determinism” (1999: 89). But I do not see how the inference can possibly be justified.

In Chapter 3, I explained that determinism is to be understood and analysed in terms of its different layers. I argued that while universal epistemic (predictive) determinism (**D**) seems unlikely to hold true of our world, the same cannot be said of ontological determinism (**B**), or even of partial determinism (§3.8). In Chapter 4 I analysed the arguments against the possibility of historical determinism, and highlighted the fact that at best such criticisms would weaken the defence of predictive determinism. The present section is an additional claim against historical determinism I did not explore in Chapter 4: that the use of CTEs as an explanatory tool undermines historical determinism.

I fail to understand why considering counterfactual scenarios in history would be incompatible with accepting determinism. The simple idea that the present counterfactually depends on the past, that ‘had anything different happened in the past, the present would have been different’ may well be a deterministic claim. Ferguson’s analysis of CTEs in history, for example, favours the view that changes to historical antecedents are likely to provoke major transformations to the present. He thinks of history as a highly sensitive system, and suggests that the contingency of historical events may be the product not just of chaos, but also of indeterminism of nature.

Let us entertain a different mental exercise. Let us concede for a moment that CTEs may be illuminating when it comes to identifying, or proving, how contingent/necessary history really is. Can we entertain a CTE which illustrates much of history to be highly *necessary*, i.e. can we describe an alternative historical scenario in which alterations to the antecedents are always followed by the same type of final event? We could probably do it by attributing – according to our interpretive framework and pragmatic choices – greater weight for any evidence supporting the occurrence of a similar type of effect, and lessening the causal

importance of other evidence (minimising the importance of agent causation, for example). Does this prove history to be in reality deterministic? Or does it solely demonstrate that my *explanandum* has been constructed in a way to portray a high level of historical necessity?

We have previously defined one of the possible varieties of determinism as the view that the state of a system at time (t_1), according to a law, or theory (T), uniquely fixes the state of the system at any subsequent time t_2 for some empirical magnitude (m). If the laws governing the world are deterministic, then the final state of the world could not have been different with respect to m , *unless* something in the initial conditions, or in T , had also been different. If the system is indeterministic, then the set of initial conditions at t_1 under T does not uniquely fix the state of the system at t_2 , and we have the hypothetical possibility of ‘bifurcations’ or the ‘branching’ of the system’s trajectory.

And this is precisely what counterfactual historians do; they consider how the world would have evolved *had one of the initial condition been different*. There is nothing incompatible between CTEs and determinism. On the contrary, at times such counterfactual suppositions seem to presuppose some level of determinism, rather than reject it. In other words, the aptness of using CTEs for understanding a system’s evolution does not imply determinism or indeterminism; they belong to different categories.

Sládek has pointed out well the oddities of appealing to the use of CTEs to attack doctrines of historical determinism:

On one hand, counterfactual thinking provides arguments against historical determinism, reinstating the role of accident and non-obligatory causation, while many alternative histories assume that a single changed circumstance may affect the whole subsequent course of history. This is, naturally, a determinism not unlike the one criticised by counterfactual historians themselves (Sládek, 2011).

When historians give causal explanations they do not have to assume the truth or falsity of determinism. To provide a causal explanation in history is solely to provide information about the causal history of an event, and does not imply demonstrating that an effect e is the uniquely possible state of the world at the time of its occurrence (with respect to some magnitude m), given its antecedents in conjunction with T – as a perfectly deterministic system

would entail. According to Lewis (1986c), explanations are: (1) chunks of information about the causal history of an event, or (2) the act of providing such information. The satisfactoriness of a causal explanation will then depend on multiple elements, such as (a) the truth/falsity or relative verisimilitude of the propositions, (b) the amount and the quality of the information, (c) how justified it is from standpoint of explainer, (d) how well it deals with our previous knowledge, (e) how clearly and unambiguously the information is presented.

If the world is indeed indeterministic or only partially deterministic (§3.8) then we identify the causes of an event in terms of *probabilities*: the occurrence of *c* increases the probability of *e*'s occurrence in the sense that had *c* not happened, *e* would *probably* not have happened either. If *e* occurs with close to 1 probability given *c*, it may be acceptable to say that *c* determines *e*, in a broader sense of the term that does not imply universal determinism.

Why is historical determinism still a problem for counterfactual historians?

An important point for defenders of counterfactual history is to say that it makes perfect sense to inquire what would have happened had an agent decided to act differently than s/he did, because agents *can* act in different ways. If the world is indeterministic, they say, then it is a fact that the agent *could have done otherwise*. Historians who are sympathetic with the use of CTEs are usually wary of what they consider to be 'deterministic doctrines' of history because they believe that such doctrine deny that the 'can do otherwise' condition can ever be satisfied. They claim that the agent's causal contribution stands whether or not, in the very same circumstances of his/her action, there could have been an alternative to what s/he actually does. As a consequence, it is believed that historical explanations that are deterministic tend to minimise the importance of agency and of accidentality.

And again we are thrown back to the problem of free will and determinism, which I have previously discussed in §2.1.8 and also in §4.4.5. No need to restate my view that a compatibilist approach to freedom is to be preferred. We need, however, to restate that causation and necessity belong to different categories. If an event is the sole result of agency, then we may see the causal chain as highly sensitive and the effect as highly contingent; any difference to the initial or surrounding conditions could interfere with the causal chain, and alter the agent's decision. Historians would, in turn, attribute great importance to the agent's

actions as part of an explanatory framework. If determinism holds, the degree of importance attributed to the agent's action is not minimised. The same logic may be applied to cases of accidentality. It makes no difference, to the aptness of the CTEs, if the world is deterministic or indeterministic.

I can certainly understand that someone who has an account of determinism in terms of universal predictive determinism would feel less inclined to entertain CTEs in history. The reason is simple: if all states of the world (the entire world is a single system) are determined by a previous state of the world, and some Grand Theory (T_G), then the postulation of any alteration would seem a contradiction, as the alteration itself would be inconsistent with the prior states of the world – remember that the state of the world is determined by T_G at all times.²¹⁸ But even so, the believer in universal determinism could say that historical explanations are incomplete, historical processes are not quantifiable, and magnitudes not well known; so from an *epistemic* perspective historians have to consider multiple explanatory alternatives.²¹⁹ Would this be in accordance with the apt use of CTEs as an illustrational tool for historians' attribution of causal weight? I see no reason why it would not.

The counterfactual historian need not take any position on determinism. Lewis's notion of causation as counterfactual dependence and its correlated features – *sensitivity* and *fragility* – offers historians an adequate theoretical background when it comes to making causal claims. A causal claim does not require subsumption to laws or regularities – at the same time it is not denied that relevant laws could indeed exist. According to Lewis's theory, it is not relevant, for the sake of providing causal explanations, whether an agent's behaviour is the result of indeterminate non-reducible mental processes, or subsumed by strict regularities of nature, or probabilistic laws. If the agents' actions, free or unfree, are part of a causal chain which lead up to some effect, then the actions are causal antecedents. We are entitled to claim that had the actions not occurred (as they did), the consequents never had existed.

²¹⁸ If I only say that some magnitudes m at t_2 is causally determined by T and a previous state of the world at t_1 , there is no reason to assume the truth of universal determinism.

²¹⁹ The Laplacian determinist could still define *chance* in terms of §2.1.3 or §2.1.4.

Determinism is an ontological doctrine that can only be proven false *empirically*, and never by means of metaphysical speculation, or by entertaining CTEs. And historians are not in the business of finding out the truth or falsity of such doctrine. But historians are in the business of making causal claims, and of providing causal explanations. Lewis's *counterfactual dependence* provides an adequate theory for causation in history (and in other domains). Historical necessity/contingency (as defined by Ben-Menachem), or sensitivity and fragility (as defined by Lewis) provide historians with the necessary tools to attribute different degrees of importance to causal antecedents (as explained in §5.4.2).

5.7 Final remarks

After our previous considerations we may conclude, as did Tucker (2011) and Sládek (2011), that counterfactual history's greatest virtue is the provision of a tool for the 'weighing' of causes. We may also conclude that historians do not have to decide on the truth of determinism to offer satisfactory causal explanations; determinism as an ontological tenet is yet to be debunked. Historians may be of the opinion that an effect was *determined* by its antecedent conditions. Or, alternatively, they may think that antecedent conditions *probabilify* the occurrence of a certain effect. Their positions on determinism have little effect on history's major task: to explain how the state of the present counterfactually depends on the state of the past.

The central point of this chapter was to bring attention to the relation between counterfactual history and the contrastive distinction between historical *necessity* and *contingency*, as defined by Ben-Menachem. The conclusion I draw is that the aptness of undertaking CTEs depends entirely on our willingness to accept such contrastive notions, thereby avoiding the conflation of causation and necessity. If the distinction is objectionable – perhaps on the grounds that historians' attribution of causal weight may be arbitrary – then there is no defence left for the usefulness of counterfactual history, and we must concede victory to Carr (1964: 97), as there would be no epistemic value into inquiring 'what if?'

But the distinction does seem to be sufficiently satisfactory, as we frequently consider causal chains as having different degrees of sensitivity to changes to initial and surrounding conditions. Therefore, CTEs may be a useful tool for historians wishing to explain – in the light of their own philosophical and methodological preferences, and pragmatic choices of causal selection – why they interpret a certain event as a relatively necessary or relatively contingent effect, given its antecedents. Insofar as historians (1) look for explanations and (2) do it by uncovering causes, and (3) casual relations are given a counterfactual analysis (see Lewis), then historians' attempts at explanation are, perhaps, unavoidably caught up in counterfactuals. And this will lead to some degree to a role for CTEs.

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