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CEMENTING SCIENCE

Understanding Science through Its Development

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Academic Dissertation
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Abstract

In this book, I defend the present-centered approach in historiography of science (i.e. study of the history of science), build an account for causal explanations in historiography of science, and show the fruitfulness of the approach and account in when we attempt to understand science.

The present-centered approach defines historiography of science as a field that studies the developments that led to the present science. I argue that the choice of the targets of studies in historiography of science should be directly connected to our values and preferences in an intersubjective process. The main advantage of this approach is that it gives a clear motivation for historiography of science and avoids or solves stubborn conceptual and practical problems within the field.

The account of causal explanations is built on the notions of counterfactual scenarios and contrastive question-answer pairs. I argue that if and only if we track down patterns of counterfactual dependencies, can we understand history. Moreover, I define the notions of *historical explanation, explanatory competition, explanatory depth, and explanatory resources*.

Finally, I analyze the existing historiography of science with the framework built in the previous chapter, and I show that this framework clarifies many first-order (i.e. concerning the history of science) and meta-level issues (i.e. concerning the nature of science in general) that historians and philosophers tackle. As an illustration of the philosophical power of the framework, I explicate the notion of *local explanation* and analyze the question of whether the developments of science were necessary or contingent.

Tiivistelmä
Tässä työssä puolustan nykyhetkikeskeistä (presentismi) tieteenhistoriografiaa, muotoilen tieteenhistoriografiaan soveltuvan mallin kausaalisesta selittämisestä ja osoitan sekä presentismin että selittämisen mallin hedelmällisyyden yrityksissä ymmärtää tiedettä.

Nykyhetkikeskeinen lähestymistapa määrittelee tieteenhistoriografian alana, joka tutkii nykyiseen tieteeseen johtaneita kehityskulkuja. Argumentoin, että tieteenhistoriografian tutkimuskohteiden valinnan tulisi yhdistyä arvoihimme ja preferensseihimme intersubjektiivisen arviointiprosessin kautta. Tämän lähestymistavan suurin etu on siinä, että se tarjoaa tieteenhistorialle selvän motivaation ja välttää tai ratkoo vaikeita käsitteellisiä ja käytännöllisiä ongelmia, jotka ovat vaivanneet tieteenhistoriografiaa.

Kausaalisen selittämisen malli, jonka esitän, perustuu kontrafaktuaalisille skenaarioille ja kontrastiivisille kysymys-vastaus-pareille. Argumentoin, että jos ja vain jos jäljitämme kontrafaktuaalisia riippuvuussuhteita erilaisten tekijöiden välillä, voimme ymmärtää historiaa. Lisäksi määrittelen sellaiset käsitteet kuin *historiallinen selitys, kilpailevat selitykset, selityksen syvyys ja selittämisen resurssit*.

Lopuksi analysoin olemassa olevaa tieteenhistoriografiaa aiemmin muotoillun viitekehyksen läpi ja osoitan, että kyseinen viitekehys selventää monia ensimmäisen kertaluvun (ts. tieteenhistoriaa koskevia) ja meta-tason (ts. tieteen luonnetta yleisesti koskevia) ongelmia, joiden parissa historioitsijat ja filosofit työskentelevät. Osoittaakseni muotoillun viitekehyksen filosofisen potentiaalin analysoin käsitettä *lokaali selitys* ja kysymystä siitä, onko tieteen kehitys ollut välttämätöntä vai kontingenttia.

Contents

1 PREFACE

This is a book on *the philosophy of historiography of science.* The production of this book depended on two conditions.

First was my interest in the philosophy of science. The idea that the philosophy of science, as well as other disciplines studying science, must take into account the history of science is widely recognized these days. It follows that we need to understand the history of science in order to develop a philosophy of science.[1] But how do we gain such understanding? The trivial answer is that *historiography of science*[2] (more or less professional) provides that understanding. This trivial answer deceives us: *historiography of science* is no longer a transparent intellectual enterprise than science itself. As much as we need a *philosophy of science* in order to understand science, we need a *philosophy of historiography of science* in order to understand historiography of science. Once we recognize the intimate relationship between historiography of science and philosophy of science, we are also able to recognize the intimate relationship between philosophy of science and philosophy of historiography of science: if historiography of science stands on shaky grounds, then our philosophical views on science stand on similarly shaky grounds.

The second condition was that, while discussing on a regular basis with people who practice historiography of science and knowledge (to use actors' categories) for a few years, I came to realize the above-mentioned issue, that there are many philosophical concerns related to historiography of science. That this insight came not from the philosophy of science but from historiography of science convinced me that there is a

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[1] Increasingly, many philosophers have questioned this link (see the following chapters). However, even such skepticism toward the link between history of science and philosophy of science must be based on a detailed philosophical analysis of historiography of science. The irrelevance, as well as relevance, of the history of science to the philosophy of science cannot be decided *a priori*.

[2] By "historiography" I refer to the study of the history, and by "history of science" I refer to the developments (events, processes) of science in the past.

need for philosophical framework where some of the philosophical issues in historiography of science can be solved (or at least avoided) and that a such framework is needed even when it has no direct relevance for philosophy of science. Historiography of science deserves its own philosophical attention.

In this book, I scrutinize philosophical grounds of historiography of science and suggest a conceptually clarified framework where those grounds can be examined. However, not every philosophical question concerning the nature of historiography of science can be studied in one book. Therefore, I will discuss two of the most central issues: **the nature and the role of explanations in historiography of science.**

The title of this book is inspired by John Mackie's book *The Cement of The Universe* (1974). Mackie's book is about causation. He writes:

> The causation that I want to know more about is a very general feature or cluster of features of the way the world works: it is not merely, as Hume says, to us, but also in fact, the cement of the universe (Ibid., 2).

My aim in this book is not to find out what causation *is* but how to *use* causal thinking in a particular field, historiography of science. I want to show how this cement can be used to build insights into the developments of science. I add the cement to the historiographical thinking. *Cementing* also means *establishing something firmly or permanently*. The final chapter of this book discusses the issue of inevitability and contingency in the development of science. The question is, in a sense, how permanently science is established and how firm were the processes that shaped science. The title *Cementing Science* captures these important aspects of this book.

Even though I discuss historiography of science, my hope is that the general framework formulated in this book is also applicable to other fields of historiography.

The Structure of the Book:

In Chapter 2, I discuss recent views on the nature of historiography of science. This chapter shows that historiography of science is a field loaded with idiosyncratic philosophical issues. Due to these issues, there is a genuine worry that writing sound historiography of science is an impossible task, philosophically speaking. At the end of the chapter, I outline my own position in relation to these issues.

In Chapter 3, I formulate a present-centered (or *presentist*) approach to the history of science and I solve a set of philosophical problems of historiography of science with this framework. In the presentist historiography, an event or a process is a part of the history of science if and only if it has a causal connection to the present science. I show that the presentist approach enables to see why historiography of science matters and how the causal definition of the history of science avoids many conceptual and methodological problems.[3]

In Chapter 4, I argue that it is possible to rationally evaluate what is significant about science. I show that if the subjects of study of historiography of science are chosen by using *the method of reflective equilibrium*, historiography of science can remain a highly motivated intellectual field. The method also helps historiography of science to avoid morally or politically biased studies due to its intersubjective essence.

In Chapter 5, I discuss previous views on the nature of explanation in historiography of science. I point out four possible perspectives on the history of science in Kuhn's *Structures* and argue that these perspectives open four important families of issues that need to be considered when we, in the following chapters, build an account for explanation in historiography of science. I discuss different perspectives on *the scale and nature of explananda*, *the role of reasons and rationality,* and - most importantly – *the role and nature of causal explanations* in historiography of science. I show that there is an urgent need for a unifying account for explanations in the field.

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[3] Some ideas in Chapters 2 and 3 are published, in a rather rudimentary and probably unrecognizable form, in my paper "Miten tieteenhistorian pitäisi valita tutkimuskohteensa?" ["How Should the Historiography of Science Choose its Targets of Study?"] *Ajatus* 72 (2015). Due to the serious reformulation of the ideas (and due to the language of the paper) I do not cite that paper. However, I would like to thank the editors of *Ajatus* 72 and the anonymous reviewers for their comments on the paper.

In Chapter 6, I formulate an account of causal explanation in historiography of science. This account is based on contrastive explanations and patterns of counterfactual dependencies. I define such concepts of historiography of science as *historical explanation*, *scenarios and situations in the causal thinking*, *competing explanations*, *explanatory goal*, *explanatory depth*, and *explanatory resources*. These are all useful tools in historiographical thinking, as is shown by analyzing existing historiographical claims and studies in later parts of the book. An important meta-level result of the chapter is that we should not be scared of causal notions in historiography once these are defined in a suitable way.

Here we should note a terminological distinction. When I discuss the ideas in this book, by "framework" I refer to the presentist approach with all of its components (formulated in chapters 3, 4 and 6) and by "account" I refer to the account developed in Chapter 6. The account of explanation is a part of the more general framework. (Notice that not every use of "framework" and "account" refer to the positions in this book. For example, I might write about "explanatory framework of science". I trust that it is obvious enough how to decide what is meant in these cases.)

In Chapter 7, I illustrate the philosophical framework of the previous chapters by explicating the notion of *local explanation*. I point out the need for such explication and show how ideas from Chapter 6 can be used in the explication. Within the chapter, I discuss two existing historiographical studies to illustrate the issue.

In Chapter 8, I show how questions of contingency of science can be answered within the framework developed in earlier chapters. I show how we can evaluate the contingency of some feature of science once we (i) have understood why that feature is significant, (ii) have found causal explanation of that feature, and (iii) use, when possible, existing historiographical studies. Within the chapter, I discuss one existing study from historiography of science to illustrate the issue.

In Chapter 9, I conclude by arguing that the historical understanding that historiography of science provides is not a *sui generis*. In other words, historical explanations do not differ from explanations in other fields and in everyday life. They share the same structure, tracking down patterns of counterfactual dependences, and the same explanatory resources, i.e. knowledge that is used when explanations are built. Moreover, I sketch how the framework in this book can be used to build tools to estimate the future of science.

2 HOW TO UNDERSTAND SCIENCE HISTORICALLY?

Science, as a human practice, needs to be understood. Human practices are always difficult to understand in depth. In the case of science, the number of relevant elements that play a role in the practice makes this task even more difficult. There are cognitive agents and social structures; there are theories, models, concepts, instruments, and methods that are produced and used by these agents and structures; there is the universe that these products seek to capture. Moreover, the feedback-loops between these sets of elements and the ways in which these elements are connected with other aspects of human life militate against building any neat picture of how science works. However, it is precisely this complexity that makes science so important a practice to be understood, as the complexity reflects how deeply our present world and science are intertwined.

One way to tackle the complexity of the scientific practices is to approach them historically. If we focus only on the present, we are lost in the labyrinth of the complexities of science. However, if we extend our horizon, the present state of affairs might begin to look understandable. As we examine the developments of science through history, we are able to put our present situation into a wider perspective. Moreover, historiography of science provides a unique perspective: it is the perspective of the actual world. If we want to understand science as a part of the actual world, there is no other perspective except the historical one. Historiography of science, revealing the history of science in our actual world, provides a key to understand the complex workings of science.

But what, exactly, is historiography of science about? What can it achieve? Different considerations pull us in different directions. On the one hand, modern science is a relatively new invention. The cluster of events traditionally known as 'the scientific revolution' happened somewhere between 1500 and 1800. Only since that revolution, whatever its exact characteristics were, have there been epistemological practices that share recognizable similarities with our science, a point made by Richard S. Westfall:

> [The] question [about the scientific revolution] is whether the enterprise of science as it was carried out after 1687 was radically different from that before 1543. Clearly, I think that it was and that the transformation was a once and for all event that has never been reversed. Scientists of today can read and recognize works done after 1687. It takes a historian to comprehend those written before 1543. (2000, 44).

However, even that period was mainly a revolution in physics. In chemistry, a revolution began in the late 18th century and is often credited to Lavoisier. Other fields, such as biology and psychology, began to take their somewhat mature forms only much later. For example, Darwin published his *The Origin of Species* in 1859 and Wundt his *Grundzüge der physiologischen Psychologie* [*Principles of Physiological Psychology*] in 1874. Moreover, even in physics many fundamental developments, such as the formulations of quantum mechanics and the theory of relativity, have occurred in the 20th century. In general, the science in the early 21st century looks very different from what there was at the beginning of the 20th century. How can the history of something so recently developed be written?

The fact that many features of our sciences have developed so recently is not even the main problem. Historians of science warn us about the dangers of considering, say Newton, as a scientist in our sense. The world has changed since the days of Newton, and we should not impose our own ideas of science on Newton's work and practices. The past should not be seen from the viewpoint of the present. Among other unfortunate things, a presentist approach is said to lead to the projection of present categories on the history of science and thus to the distorted use of the sources. It allows us to see only what is absent in the past and prevents us from finding anything concrete from the past. (Ashplant & Wilson 1988.) Moreover, Cunningham argues that it can be hardly appropriate that the historians in the present set the criteria for what counted as science in the past (1988, 367).

On the other hand, knowledge of the history of science seems necessary in order to understand science. As our science has developed only recently, the historians must remind us that things have not always been the way they are now. In general, the historians of science can show the contingency of our science. This point is nicely put by Rée:

> The contemplation of historicity – of the sheer singularity of places and times, situations and conjunctures, including all those you habitually take for granted – will help you see that there are different ways of looking at the world, and that what is obvious in one perspective may be ridiculous in another. (1991, 961.)

Moreover, historians must remind us that the ancestors of our present science were not the only candidates to pass on their thoughts and practices to the following generations. Complex social and epistemological structures, in a continuous flux themselves, need to be revealed in order to understand the developments of science. This is one of the messages that Schaffer and Shapin wanted to convey in *Leviathan and the Air-Pump*:

> Yet we want to show that there was nothing self-evident or inevitable about the series of historical judgments in that context which yielded a natural philosophical consensus in favour of the experimental programme. Given other circumstances bearing upon that philosophical community, Hobbes's views might well have found a different reception. (1985, 13).

However, the focus on the detailed analysis of our ancestors should not blur the fact that these people were not intentionally developing science for the future generations, a point made, again, by Cunningham (1988). These people should be studied in their own right and on their own terms. We should also avoid big pictures that imply that the history of science was a coherent and progressive set of developments. (Shapin 2005, 242).

If there is one lesson above all the others, it is that we should not celebrate those people who turned out to be winners or who thought in the same way as we do. This lesson was given by Butterfield in *The Whig Interpretation of History* (1931). A related demand is that we should not use our present scientific knowledge to explain the past since this knowledge is a historical product. Schaffer and Shapin write:

> ‘Truth,’ ‘adequacy,’ and ‘objectivity’ will be dealt with as accomplishments, as historical products, as actors’ judgements and categories. They will be topics for our inquiry, not resources unreflectively to be used in that inquiry. (1985, 14)

So the puzzle is this: If there has not been science, as we conceive it, in the past, and if we cannot find a coherent set of developments, what are all the articles and books in the historiography of science about? What do they achieve as historical works about science? The following topics[4], just to pick some examples (many more are easily found[5]), have been discussed in the journals for the history of science:

> "Early Modern Iberian Science, from the Fifteenth to the Seventeenth Centuries". *Early Science and Medicine* Vol. 21.
> "Trading Zones in Early Modern Europe". *Isis* Vol. 106 (4).
> "Restoration commerce and the instruments of trust: Robert Boyle and the science of money". *History of Human Sciences* Vol. 29 (1).
> " A Patient with Word Blindness in the Seventeenth Century". *Journal of the History of Neurosciences* Vol 24 (4).
> "Reputation in a box. Objects, communication and trust in late 18th-century botanical networks". *History of Science* Vol. 53 (2).

Perhaps the historians study the practices that were sciences in the past? Sciences were different in the past, and each of the papers above, for example, discusses a science in a particular era. Whether a practice was a science depends on the historical context of that practice. This has been suggested: "[--] historians of science are as likely, perhaps even more likely, to consider their work part of a conversation about a particular time and place, science in the nineteenth century rather than the nineteenth century's contribution to the history of science" (Findlen 2005, 235). This view can be described as the *science-in-the-past view*.

The science-in-the-past view has been built in the reflexes of the historians at least since Kuhn published his *Structures*. Kuhn's whole project, with the notion of a paradigm shift, is understandable only with the assumption that there have been different sciences in the past. Kuhn also explicitly writes:

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[4] The high quality of the work mentioned here is not in doubt.

[5] "Current Bibliography of the History of Science and Its Cultural Influences, 2016" *Isis* 107, no. S1. i-240.

> The more carefully they study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today.[6]
>
> Gradually, and often without entirely realizing they are doing so, historians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an *older science* to our present vantage, they attempt to display the historical integrity of *that science in its own time*. (1970, 2-3 [emphasis added])

However, there are serious dangers in the science-in-the-past view which suggests abandoning it. First, the view is problematic if we want to understand the past *in its own terms*. As Cunningham writes:

> But did these people in the past perhaps also describe this self-same activity of theirs as 'science'? The answer must be that until at least 1750, and possibly until as late as 1800, no-one at all described their activity like this. [--]
>
> Thus we customarily take people who, by their own accounts, were engaged in intentional activities other than science, and treat them as having been engaged in science. We mistake one activity for another. As a consequence we also give the wrong identity to what these people said and did. For us to ascribe the activity 'science' to people who were not only not engaged in science but who were actively engaged in another activity altogether, is for us to hijack their actions and statements into our context, a modern-day context, and give them a post factum identity. (1988, 380).

It is questionable whether the intentions of the people in the past should be decisive in defining what they were doing.[7] However, most of the scientific fields, theories, methods (especially statistical), and the institutional structures of science have developed only recently. Once we connect this observation with the right sensitivity to the intentions of past people, Cunningham's demand, that we should not think that there where sciences (in any historically reflective sense) before the 19th century, seems

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[6] It must be noted that in the rest of the paragraph Kuhn writes that past practices were more science than myths. Of course, if we need to choose which one of these descriptions is more adequate, the description as "science" is more adequate. This, however, does not change the picture that the *Structures* conveys as a whole.

[7] Surely the intentions do not define science in our time. Astrologists are not doing science no matter what their intentions are. Moreover, the practices of CERN are scientific even if it is difficult to tell whose intentions would be relevant when judging that.

justified. This means that we should avoid describing the past epistemological practices as sciences if we want to describe past in its own terms.

Moreover, the problem with the claim that science has taken different forms in different eras is that this claim presupposes that there is a universal category of science that can be used to describe practices of different eras. But if one takes seriously the idea that different eras should be understood in their own terms, it does not make sense to presuppose such universal categories. We should accept that because different eras were different they require different categories and concepts to describe them. Only in this way can we understand the past in its own terms.

Of course, there can be endless debates about whether some past practices were similar to the present sciences with respect to some criteria of *science*. However, this is not the point behind the science-in-the-past view. Rather, the lesson is that science has taken (sometimes radically) different forms in the past, and we should appreciate this fact in our thinking about science. We should learn from the difference, not from the similarity. The changing nature of science is what interests us *historically*. It is difficult to deny this intuition: historiography of science would be a rather odd practice if its purpose was to produce checklists that tell what practices in the past were science according to some present-day criteria.

Secondly, the problem does not go away by stating that the concept of science has itself developed through history. When we speak about science, we use our own concept of science. If we say that something was science in the past, we use our own concept to deliver the message no matter how much we specify the claim. We could perhaps use concepts such as "science" ($=_{df}$ our concept of science) and "$\text{science}_{\text{past}}$" ($=_{df}$ concept of science of some past era), but this would only add unnecessary opacity to our language and give the mistaken impression that the historical actors used similar conceptual resources as we do.[8] If we wish to avoid the obsession of seeing past

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[8] Of course, this does not mean that our concept of science has not developed historically. That there were no human beings 65 million years ago does not mean that human beings have not developed from the species that lived back then.

actors as similar to us, the introduction of concepts such as "science$_{past}$" is the last thing to do. We should not make the past look a cartoonish place[9] where everything is exactly as it is in our world but built from different materials. It is simply wrong-headed to take a category we happen to use and expect that the studies of a historical period or society then fill that category with the contents of that time or society.

In the third place, the science-in-the-past view magnifies one fundamental problem, the disunity of science. The problem is that if there is no unity in the practices that historiography of science studies, what makes historiography of science a coherent field? Dear aptly writes:

> [History of science] may comprise any sort of knowledge or human activity to do with the world that we regard as serious, formally organized, and respectable. It could range from gnomons to genomics; from satellites to stalactites; from ancient Kenyan iron-ore smelting to Polynesian navigation. Very little would be off-limits, and a broad vision to encompass it would have little real coherence. (2012, 37.)

This a pressing problem for the science-in-the-past view. The assumption that the sciences of different eras were different from one another implies that there is no unity in the practices that are studied in historiography of science. Therefore, the science-in-the-past view builds in historiography of science the unattainability of a coherent picture. The science-in-the-past view, then, not only leaves the problem of disunity unsolved but makes it unsolvable. It is difficult to find any reason not to abandon a field that cannot in principle produce any coherent pictures from its separate results. The science-in-the-past view cannot save historiography of science.

In the fourth place, describing something as a *science in the past* obscures our view and can lead to a construction of misleading images of science. For example, it is not acceptable to argue that religion and science cannot be distinguished one from the other (or that religion can even improve scientific thinking) since in Newton's thinking

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[9] Think of *The Flintstones*.

science and religion were in close contact. [10] We can find this kind of argumentation, for example, in Brooke (2014, 24) and in Dembski & Ruse (2006). Dembski and Ruse write:

> If Boyle, Kepler, and Newton did superb science while believing that the success of the scientific enterprise depended on God's Providence, it does not seem absurd to suggest that science again might flourish in a non-naturalistic framework. (2006, 44).

It should be obvious that, as the past was very different from the present, arguments such as these are simply irrelevant. However, the discussions about "the past sciences" and "the sciences in the past" fuels the industry of arguments like the one above.

In general, we should not evaluate the value of science with reference to the epistemological practices of the past. Neither negative nor positive assessments of science should be made on the basis of what happened in the past. We should not say that since the alchemists had illusionary goals, we should be skeptical toward chemistry today. Neither should we say that because the scientific revolution was a highly progressive era in knowledge, we can trust that the present sciences can solve the major challenges of our time such as climate change. In the similar manner, we should not project the positive or the negative aspects of the present science on the past. The gap between the past and the present is too wide in order for there to be straightforward inferential or evaluative links between the two.[11] The science-in-the-past view blurs this fact.

In sum, there are several problems in science-in-the-past view: (i) it leads to a conceptually crippled historiography by projecting our categories and concepts onto

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[10] My claim is not that there is no connection between the present science and religion. I leave the issue open. I claim only that facts about Newton's practices are not relevant for establishing this connection.

[11] See e.g. Pitt (2001), Schickore (2011), Kinzel (2015B) and *Journal of the Philosophy of History* 12 (2) (2018) discussing a related issue, that of relationship between the history of science and the philosophy of science.

the past; (ii) it does not answer the problem of the disunity of science; and (iii) it easily leads to a confusion about our present situation and our prospects by building unwarranted analogies between the past and the present.

The problems of the science-in-the-past view have not gone unnoticed. Perhaps the idea of historiography of science is a relic from an age when the past was not understood correctly and when science was thought to be a universally applicable category. Perhaps the term "science" in journals and books lives only due to the institutional inertia. Daston writes:

> Historians of premodern science grew increasingly skittish about calling what they studied science at all, and the word *scientist* when applied to Archimedes or Galileo set their teeth on edge. (2009, 806).

Perhaps science is just one way of knowing the world, and historiography of science is a study of the ways in which the world was known in the past. Renn writes:

> For many historians of science, science no longer seems distinguishable from other forms of cultural practices. It has ceased to be a paradigm of universal rationality and presents itself as just one more object of study for cultural history or social anthropology. Even the most fundamental aspects of the classical image of science -- proof, experimentation, data, objectivity or rationality -- have turned out to be deeply historical in nature. This insight has opened up many new perspectives on the study of the history of science, which is turning more and more into a history of knowledge. It thus includes not only academic practices, but also the production and reproduction of knowledge far removed from traditional academic settings, for instance, in artisanal and artistic practices, or even in family and household practices. (2015, 37–38.)

It is becoming more and more acknowledged that the historiography of science is not literally about *science* but about ways of knowing the world, i.e. epistemological practices in general. Moreover, historians have argued that science does not even deserve a distinct treatment. People have always had knowledge of the world and tools to gain and to use such knowledge. There is nothing special about science, and thus there cannot be a distinct field of historiography of science. It can also be pointed out that how science became conceptualized is itself a historical and thus contingent fact. In fact, Dear is desperate enough to suggest that historiography of science should focus mainly on the history of how the current conception (or "ideology", as Dear puts it) of science developed. (2012, 38).

I agree that historiography of science must avoid the many pitfalls suggested above. I have already argued that it is futile to study the sciences *in* the past as this would lead to all sorts of problems. “Science” is our category, and science is a practice that exists in our present world as it is conceptualized by us. [12] At least, this is the only claim that can be made on firm historical and philosophical grounds and I will argue that there is no need to go beyond it. *In this book, I show that there is no need to take the conceptual risks that are involved in the science-in-the-past view.*[13] However, I disagree with the idea that there cannot be historiography of *science*. I think that the history of science deserves serious reflection and that the history of epistemological practices in general does not satisfy our need to understand *science* historically. We know more than ever that this knowledge is organized and available in an unprecedented manner, and the practical implications of this knowledge penetrate every aspect of our lives. However, this does not imply an uncritical stand towards science. On the contrary, only by accepting the special nature of science can we understand the dangers that have been created together with the scientific achievements. To put it bluntly: Aristotle or Paracelsus could not have caused a nuclear apocalypse, our science can. Moreo-

[12] Some might have the reflexes to say that, in the present day, different societies have different conceptions of science. *If this is true*, then everything said in this book about historiography of science can be applied to the practices that are scientific under some conception of science. “Our” does not then refer to the author’s society but to the reader’s. However, the truth and even meaningfulness of the claim raises serious philosophical problems. Here lies a similar trap as in the *science-in-the-past view*. Why should we (or any society, for that matter) think that other societies share the same conceptual framework with us (involving the concept of *science*) and, at the same time, in a paradoxical manner, think that those concepts differ from our concepts? Again, this is only unnecessary conceptual maneuvering. The reason cannot be ethical: in many sections of this book, I argue that the label “science” does not automatically make a practice good or preferable.

[13] I am not in principle against a new concept of science (based on family-resemblance, for example) that would unify historiography of science. However, the surest way to prevent conceptual problems is to build a historiography of science that is not built around any concept of science.

ver, science is the most important epistemological practice for *us*. It would be a historiographical inconsistency to suggest that while we need to understand epistemological practices of the past as different from our practices, we need to understand our own practices as not-so-unique. The asymmetry exists in both ways.

I also disagree with the idea that our present scientific knowledge cannot be used in historiography of science. This point is connected to the previous one: Science is a special epistemic practice and it is *our* epistemological practice. We cannot expect that the historians go beyond our own system of knowledge. Moreover, science describes the universe and, as the past is a part of that same universe, science also describes the past. That the earth rotates around the sun; that electrons are constituents of the universe; and that evolution has shaped the life on earth are facts about the world as it is and as it has been for a long time. These facts are parts of our knowledge and establishing credible alternatives to them would be an enormous achievement that would change our worldview dramatically. However, as there are no credible alternatives, historiography cannot be practiced as if there were.[14]

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[14] We need to be careful here. Perhaps we could have a different science, as discussed in Chapter 8 (or a plurality of sciences, see Chang [2012]). However, we do not actually have a different science and the historiography of science cannot transcend the actual conditions, at least not based on the mere possibility of a different science. Moreover, the possibility of a different science implies that we could perhaps have an alternative explanatory framework. It does not imply that we could write a historiography of science without any such framework. As I argue in this book, the historiography of science cannot take an agnostic position based on the possibility of a different science. Therefore, the historiography of science either (i) uses the current scientific knowledge and, in the case of emergence of a different science, collapses with the current science (if the emergence implies the untenability of the current science), or (ii) is unable to do explanatory work. It must also be noted that the question of philosophical interpretation of our knowledge is irrelevant for the historiography of science (see Section 6.7). This means that even if the possibility of a different science (or any other consideration) makes us cautious in our ontological commitments (see Stanford 2006), this has no implication on the acceptability of our historiographical explanations that cite the results of science.

Of course, many people have argued that science does not achieve genuine knowledge about the universe. We have also seen that some people think that science is not that special. The task of this book is not to argue against the arguments of such people.[15] I set aside skepticism toward science.[16] In this book, I make the following claims:

1. The present-centered historiography of science is a viable option that serves scholarly work and wider science-related social interests.

2. There is something special and significant in our present science.

3. Explanations exhibit patterns of counterfactual dependencies.

4. Historiography of science can use all knowledge available, involving scientific knowledge.

5. All knowledge is always fallible and subject to a philosophical problematizing. We should not let these facts bother us too much.

I suggest that these claims enable us to build philosophically sound historiography of science and avoid paralyzing effects of skepticism and open-endedness. If someone is inclined to accept skepticism towards science, there is not much I can do.

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[15] If the reader wants to read such arguments against arguments, I can recommend Nick Tosh's papers ([2006] and [2007]).

[16] I do not claim that skepticism towards science is impossible to establish. Rather, I do not see skepticism as a sound methodological assumption. In fact, Chapter 6 indicates how skepticism towards science can be established if skepticism is the correct attitude. However, establishing skepticism would require a long series of reflectively sound studies that provide results that support skepticism. The general lesson is that there does not exist shortcuts to meta-views on science.

I cannot defend science in this book. What I can do is to show that it is possible, contrary to the reflexes of many historians, to write present-centered historiography of science that uses the explanatory resources of sciences. Moreover, I show that this approach is the solution, not the ground (as is usually thought), for many problems that the historians of science face. *The theory is a tool for historiographical thinking about science.*

Daston once wrote:

> As of yet, a new vision of what science is and how it works has yet to be synthesized from the rich but scattered and fragmented materials gathered by some twenty years of historicized history of science. The very practices that made that history possible militate against such a synthesis coming from the history of science itself. Science studies seems a still less likely candidate for the task. A new form of interdisciplinarity must be forged. Philosophy, anyone? (2009, 803).

This book shows how this can be done.

3 PRESENT-CENTERED HISTORIOGRAPHY OF SCIENCE

We saw in the previous chapter that the historiography of science has, perhaps ironically, led to a situation where the idea of studying *the sciences in the past* has become ridden by historiographical and conceptual problems. There were epistemological practices in the past but they were very different from the science as we now know it. If one is interested in understanding how nature, including human beings, was studied and understood in the past, one can study these epistemological practices. This is an extremely interesting field of historical studies in itself, and I see no reason to be skeptical toward such a project as long as the equivocation of science of our time and those past practices is avoided. However, I do not think that we need to abandon historiography of science. This means that we need to find an alternative to the science-in-the-past view. In this chapter, I formulate such an alternative. As one might guess, such an alternative must turn upside-down many previous views on the nature of historiography of science.

3.1 Causal-Narrative Presentism

The approach I offer is the *present-centered* (or simply *presentist*[17]) approach to the history of science. **From the presentist point of view, historiography of science is the study of the past practices that have led to the present science.**[18] In other words, historiography of science is the study of practices and episodes that were causally relevant to the formation of what is now known as science. Nick Tosh (2003) is a notable defender of this approach. According to Tosh, historiography of science is a study of

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[17] This a specific form of presentism and there are others. To contrast this kind of approach with other forms of presentism, Laurent Loison (2016) uses the term "causal-narrative presentism".

[18] Of course, there are many different sciences. However, using the term "present science" does not affect the argument here.

past activities ancestral to modern science: "Modern science has a causal history, and [historiography of science] could reasonably be structured around a causal backbone of past activities which helped to bring it into being." (2003, 648.)

It is helpful to use Lorraine Daston's words[19] as a guideline to the presentist approach: "[the historians of science] must explain how [the distinctive] character [of science] crystallized out of practices, both intellectual and manual, designed for other purposes". (Daston 2009, 807). The past practices that the historians of science study do not have to be scientific themselves, and we should not force them under the concept of science. All that is required is that these practice are causally connected to the present science. With the presentist approach, we avoid the dangers of *science-in-the-past* thinking. The question whether or not some past activity was scientific itself simply does not arise.

However, avoiding the dangers of science-in-the-past thinking is not the only motivation behind the presentist approach. Arguably, one of the main goals of science studies is (or at least should be) to understand the present scientific practices.[20] The reason for this is that we can affect the world around us only at the present moment. Scientific practices can be evaluated and changed only at the present. Our present science is a broad collection of achievements that has been built through the history. Together with these achievements, a remarkable range of ethical, political, theoretical, methodological, and conceptual problems have emerged in science and in our science-

[19] Of course, I do not want to suggest that Daston is a presentist.

[20] As the science-in-the-past view is not viable, as argued in the introduction, this claim might look trivial. However, it is possible to argue that once the conceptual landscape is clarified, what was named (naively, without conceptual reflection) "*science* studies" is really "*epistemological practices* studies". From the *name* of a field we cannot draw conclusions about the real nature of the field.

related life. We, in the present, live with the achievements and problems that the history of science has generated.[21] These achievements and problems cannot be ignored as we are surrounded by them. Passages from Richard S. Westfall are worth quoting at length here:

> Recall the world about us. To me it appears that the existence of modern science is the precondition for most of the central features of our society. I think of such things as means of communication, from the mass media that bring the world to our homes each morning to individual devices such as the telephone and e-mail, which together have so expanded our lives in comparison with those of the people I know from the seventeenth century. Ease of transportation enabled scholars from all over the country and beyond to gather in New Mexico to hear Dobbs's lecture; we have incorporated the various dimensions of ready transportation into our lives to the extent that we have forgotten it was not always there. The level of material plenty has lifted the burden of poverty from the great majority. Modern medicine has more than doubled the average life span and driven pain and disease, once familiar members of every circle, to the margins of our existence. These features of our life are not evenly spread around the globe. In general, they prevail where modern science flourishes and are in shorter supply elsewhere.
>
> Most people think of these characteristics as benefits. Almost no one considers other features of our world that are also derivative from science as benefits, though they are no less central. Scientifically based technology has accelerated the consumption of nonrenewable resources until we stand already face to face with their exhaustion. It has produced products that nature cannot degrade, so that we are well on the way to choking on our own refuse. It has conjured up weapons of mass destruction more hideous than earlier ages were able even to imagine. I do not think that I have compiled a partisan list. Every item on it appears incontrovertably true, and I am convinced that I could go on indefinitely listing similar ways in which science impinges, both positively and negatively, on our lives until I had more than satisfied everyone who finds my list wanting in some respect. (2000, 42–43).

That we are so intertwined with science should direct our historical reflections. There is nothing wrong with this. This point about the relationship between the historiography and the present state of the world is formulated eloquently by Naomi Oreskes in her paper "Why I Am a Presentist?" (2013). Oreskes calls herself a "motivational presentist" and writes:

> What matters to us about the past has everything to do with who we are, where we live, and what we think is important – to us, here and now, in the present. Our motivations are inescapably presentist. Thus, to qualify the deliberately provocative title of this paper, I am a motivational presentist, and I believe all historians are. (2013, 603).

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[21] Of course, science alone is not responsible for the achievements or problems. However, those achievements or problems would not have been generated without practices that belong to the history of science.

However, we should not only look the past through our present interests. Understanding the past is necessary for us to understand the present. The importance of understanding the history of the present science has been noted by philosophers of science. For example, Schickore argues that

> [--] a history of the present should remain part and parcel of our present efforts to understand the sciences. Fully to understand the concepts, practices, and methodological and epistemological goals and commitments of present science, we need to trace how they have come into being. (2011, 477.)

Moreover, Psillos concludes that

> [--] what science tells us about the world, as well as the reasons to take what it tells us seriously, are issues that are determined historically, by looking at the patterns of convergence in the scientific image of the world". (2012, 101).

In the presentist approach, the historiographical studies of science are seen as studies that provide understanding about our present situation. However, presentists do not build loose and questionable comparisons between the past and present. Instead, presentists show how the present situation *depends on* the past.

To put some flesh on this idea, consider the study, "Distrust and Discovery: The Case of the Heavy Bosons at CERN" by John Krige (2001). In this study, Krige describes "the microhistorical process whereby different groups of scientific actors [--] came to claim that a new fundamental particle (the W boson) had been discovered at CERN" (2001, 517). The study points out a complex set of factors that were relevant to the announcement of the discovery: the personal trust between the actors; the local technological environment; the methodological and theoretical complexity of the scientific work at hand; the limited possibilities that expensive science leaves open to the scientists especially under political pressures, and so on. Krige's study shows that there is no way we can understand the science around the W boson and the announcement of the results without focusing on these factors. We need the historical perspective. Moreover, Krige's study provides us with a very detailed understanding about the development of a particular piece of science. While more general works[22] about the experiments in the history of science deserve their place, studies that give detailed explanations of our present situation are crucial for our understanding of science. The presentist approach captures these insights.

Krige's study shows how one piece of our present worldview came to be established. In this sense, it is about an epistemological aspect of the present science. We saw above that Schickore and Psillos give a good list about epistemological aspects of science that are worth historical inquiry. However, the presentist approach is not limited to the historical study of these epistemological aspects. Science has other significant features as well. In addition to the epistemological aspects, historiography of science can fruitfully deal (at least) with the following aspects of science (and their interconnections):

> *Social*: How is science organized? What kind of social roles are there in the sciences and how do these roles guide the practices? What and whose values are built in the science?
>
> *Science and society*: What is the relationship between science and politics, science and the economy, science and different social groups? How global/local is science? How accessible is science for different groups? How are the results of science communicated to and understood by society in general? What and whose values are built into the science?
>
> *Science and culture*: What is the relationship between science and other aspects of culture (religion, for example)? What and whose values are built into the science?

[22] Such as the *Leviathan and the Air-Pump* by Simon and Schaffer (1985).

Science and technology: What is the role of science in the making of new technologies? How does technology shape scientific practices?

Psychological: How do individual scientists understand themselves? What kind of thoughts and emotions do scientists have during their work and about their work?[23]

This list is far from exhaustive but it gives an idea of what can be studied in historiography of science according to the presentist approach. Present science is an enormous global practice that has these multiple dimensions in it. Thus, the multitude of the aspects of science that a historian of science can focus on follows naturally from the presentist framework. However, I do not want to give the impression that the epistemological aspects of science stand on the same line with other aspects with respect to their importance. On the contrary, I take it that these other aspects of science are worth studying as a part of the history of *science* due to the connections they have with the epistemological aspects.[24] Science is a human practice, but it is a human practice that is devoted to knowing and understanding the world. We already saw, in Chapter 2, that even when the ability of the category of *science* to capture the practices of the past was questioned, the essential connection between historiography of science and history of knowledge and knowing was not doubted.[25] I see no reason to abandon the tradition on this issue: historiography of epistemological practices is an important field and its importance is directly related to the central role that knowledge plays in human lives. Epistemological practices therefore deserve a distinct historiographical field that studies them. Science is our epistemological practice and it is also a very

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[23] The historiographical illustrations that I use in this book provide examples of these aspects. We see that the existing historiography of science discusses and combines aspects in a fruitful and indispensable manner.

[24] I discuss this further in Section 4.5.

[25] E.g. Renn (2015) and Dear (2012) cited in Chapter 2.

successful epistemological practice.[26] Therefore, the history of science deserves a distinct historiographical field that studies it. In this way, historiography of science derives its justification as a field of inquiry from the central role that knowledge plays in human life. It is therefore difficult to see the *raison d'être* for a historiography of science that marginalizes the epistemological aspects of science and I do not discuss that possibility. However, understanding epistemological practices in isolation from other aspects of science is not possible and the impact of the epistemic aspects of science on our lives is mediated through a network that involves social institutions, technology, cultural representations, etc. Thus, even as we recognize the importance of epistemological aspects of science, the historiography of the other aspects of science is necessary in order to understand how science works and how it penetrates our lives.

However, we should note that even if someone does not view the present science as epistemologically special (for whatever reason), that person would change from a (perhaps reasonable) skeptic to a complete nihilist if she did not acknowledge that the present science has a multitude of interesting connections to many aspects of our lives. Nothing in the presentist approach itself forbids us from asking, for example, "Why are scientific theories produced by power structures rather than by sound methods?" or "Why is the misconception about the epistemological soundness of science so widespread?" In this way, the presentist approach is suitable for anyone who is not a nihilist and acknowledges the significance of science. Presentism allows disagreements about the exact nature of science. In the next section, we begin to see how the presentist approach achieves such neutral ground for historiography of science.

3.2 Significant Features as a Starting Point

Giving a definition of science is notoriously difficult as the age-old discussion on the problem of demarcation has proven (see Laudan [1983], and Boudry & Pigliucci

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[26] In the philosophy of science, the implications of this success are debated, but the success itself is beyond reasonable doubt (see discussions in Psillos 1999 & 2009).

[2013A]). Thus, it would be bold if a presentist historian of science began her study by giving a definition of science and then studied the causal histories of the practices that are scientific according to the definition. However, presentists have an escape route from this problem. One does not need to give necessary and sufficient conditions (or anything like that) for science in the presentist approach. We can simply take the practices of our society that have a scientific status and study the history of these practices: The theoretical, conceptual, ethical, social etc. aspects of science which are significant to us are problems of *those practices*. It would be nonsense to suggest that we should consider some other existing practices as *really* scientific.[27] This would not solve our problems but change their name. In addition, if the historians of science were to challenge the conception of science of their own society, they would act against the principle of "different eras and societies must be understood on their own terms" by making their own society an exception. This certainly would be a paradoxical position. (See also Section 3.4 *Contingency*). Finally, as philosophers working on the demarcation problem have noted, there exists widespread agreement about which practices count as science even if formulating the criteria to separate science from non-science is an extremely challenging task (e.g. Pigliucci & Boudry [2013B, 2]; Hansson [2013, 61]). Thus, telling which practices count as science in our society is not too difficult a task. This ability to bypass the demarcation problem is a clear advantage of presentism.

One thing must be underlined here. In the previous section and in the sections to come I often mention "a conception of science". It should be now obvious that this

[27] Of course we can ask whether science could be improved. This, however, does not affect which of our *existing practices* are the scientific ones. Moreover, we may even have wrong beliefs about the nature of scientific practices but this does not change the fact that *these practices* are the ones we want to understand in the science studies (and thus in the historiography of science). In fact, science studies would become a redundant field if we already knew the exact nature of those practices that we consider as science.

does not refer to any explicit conception that can be philosophically clarified. "A conception of science" is interchangeable with "the set of activities considered as scientific".

Of course, one could argue that there are some borderline practices between science and non-science and thus the presentist approach is vague. We must remember, however, that there are also practices that are considered as clearly scientific. The borderline cases can thus be compared with the clear cases in order to evaluate their scientific status. Furthermore, whether the borderline practices are counted as science or not does not matter much. *The history of science should not study the history of all the features of the present scientific practices but only the significant ones.* Thus, the existence of borderline cases does not make the presentist approach vague. In the next chapter, I develop this idea of significant features in detail and I argue that the borderline cases are usually insignificant. For now, it is enough to note that not everything in science is equally significant to us. For example, the existence of nuclear physics is significant unlike the exact notations used in scientific texts. It is important to analyze in a systematic way what the significant features of our science are and what would count as an interesting alternative to such a feature. In the next chapter, I show how this can be done. The idea is that by collectively negotiating which features of the present science are significant and then studying the history of these features, the historians are able to make their studies more meaningful and understandable for other scholars and for the wider audiences outside the specialist circles. By focusing on the significant features of science, the presentist approach can bring unity to the shattered field of the history of science that is sometimes confusing in its heterogeneity for the non-specialist consumers of the literature and even for the scholars in related fields.[28]

In the presentist approach, historiography of science is defined as the study of the causal history of the significant features of those practices that are considered sciences in our society. There is no need to understand (and thus no need to agree on) the exact nature of science, whatever that means, before we study its history.

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[28] Shapin (2005) and Oreskes (2013, 606) share this worry.

Finally, historiography of science can build causal chains. If we know that Z was the cause of a significant feature, we may then ask what the cause of Z was. This means that Z does not need to be a significant feature in itself in order to be explained in historiography of science. Its causal link to a significant feature is enough. In this way, an explanation can provide interesting research questions (see Section 6.6 for further discussion).

3.3 Causal Explanation in Historiography of Science

Our next task is to sketch a conception of causation that can be used in the history of science. As I formulate this notion fully in Chapter 6, only the general lines are given here.

I take it that the conception of causation as difference-making (see Beebee & al. [2017]; Menzies [2004]; Lewis [1986]) is well suited to historiography of science. As Ben-Menahem puts it, "Historical analysis seeks to separate the factors that made a difference from those that did not" (2016, 374).[29] In the difference-making conception of causation, "cause is something that makes a difference to its effects" (2004, 139). This notion is best understood by using examples.[30]

> *Example 1*: That I hit the cue ball makes a difference to the movement of a red ball. Had I not hit the cue ball, the red ball would not have moved.

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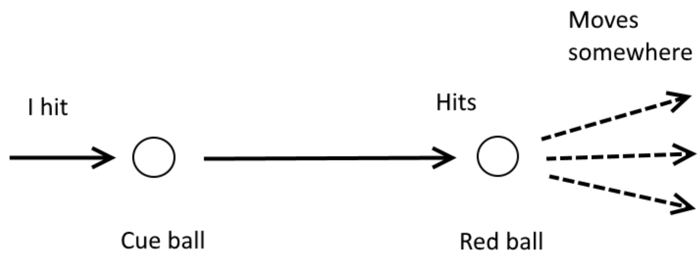
[29] This is not a new insight. Max Weber wrote: "[The] attribution of effects to causes takes place through a process of thought which includes a series of abstractions. The first and decisive one occurs when we conceive of one or a few of the actual causal components as modified in a certain direction and then ask ourselves whether under the conditions which have been thus changed, the same effect or some other effect 'would be expected.'" (1949, 171).

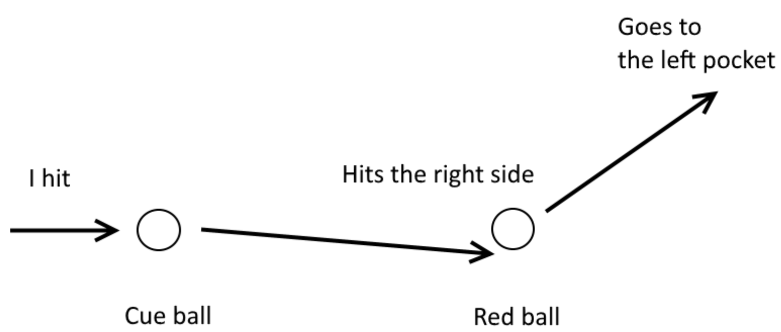
[30] The first examples are not from the historiography of science since the built-in ideas about the nature of that field might obscure the insight one should gain from the examples.

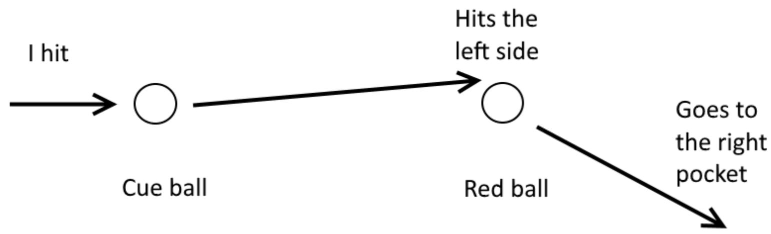
Thus, hitting the cue ball was a cause of the movement of the red ball. (See the next figure.)



Example 2: That I smile when I hit the cue ball does not make a difference to a movement of a red ball. Had I not smiled, the red ball would have moved anyway. Smiling is not a cause of the movement.

Example 3: That the cue ball hits the right side of the red ball makes a difference to the red going in the left pocket. Had the cue ball hit the left side of the red ball, the red ball would have gone in the right pocket. Thus, the hitting of the cue ball on the right side is a cause of the red ball going in the left pocket. (See the next figures).





Example 4: That I hit the cue ball does not make a difference to which (left or right) pocket the red ball goes. The cue ball must be hit in both cases.

Now it is time to shift the focus lightly. The task of the historian of science is not to find the causes of present scientific practices but to provide causal explanations of these practices. The conception of causation as difference-making is closely related to the notion of contrastive explanations[31]. We noticed that the hitting *rather than* not hitting the cue ball makes a difference to the movement of the red ball. It is the *contrast* between hitting and not hitting the cue ball that is explanatorily relevant.

Moreover, hitting the cue ball (rather than not) made the red ball move rather than not move. Hitting the cue ball in contrast to not hitting made the red ball move in contrast to not moving. The contrast between hitting and not hitting explains the contrast between the red ball moving and the red ball not moving.

[31] There has been a lot of discussion about contrastive explanations (see e.g. Hart and Honoré 1959; van Fraassen 1980; Garfinkel 1981 and Lipton 1990). Here, I follow Woodward's (2003) theory. See the beginning of Chapter 6 for the justification of the choice. It is important to notice that Woodward's theory differs from other theories of contrastive explanations and therefore the account of explanation in this book does not exhaust the possible ways in which contrastive analysis of historical explanation can be formulated.

In the similar manner, the cue ball hitting the right side rather than the left side of the red made the red go to the left pocket rather than to the right pocket. The contrast between hitting the left side and hitting the right side explains the contrast between the red ball going to the left pocket and red ball going to the right pocket.

However, smiling rather than not smiling does not make the red ball move rather than not move. The contrast between smiling and not smiling does not explain the contrast between the red ball moving and not moving.

Moreover, hitting the cue ball rather than not does not make the red ball go to the left pocket rather than right pocket. The contrast between hitting and not hitting does not explain the contrast between the red going in the left pocket and the red going in the right pocket.

By following this connection between difference-making and contrasts we can sketch the notion of contrastive explanation and their use in the history of science. The basic idea in contrastive explanations, put in somewhat simplified terms, is that when we are explaining X, we are not explaining X *simpliciter*. What needs to be explained is why X rather than Y took place. In the explanation, we must refer to a factor Z that led to X but not to Y, and we must also be able to tell which factors W would have led to Y, had W occurred. An explanation answers the question "Why X rather than Y?" by stating that "because Z rather than W".[32]

For example, someone might ask why the red ball went to the left pocket. If we answer that it is because I hit the cue ball, this answer might not be satisfactory. Suppose that the questioner wanted to know why the red ball went to the left pocket rather than to the right one. The answer is that this happened because the cue ball hit the right side of the red ball rather than the left side. The explanation has the following structure: The question, why X (the red ball went to the left pocket) rather than Y (the red ball went to the right pocket), gets an answer from the statement that because Z

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[32] There must exist appropriate counterfactual dependencies between X, Y, Z and W. If contrary to facts Z did not take place but W did, then X would not take place but Y would. I discuss the relevant counterfactuals in Chapter 6.

(the cue ball hit the right side of the red ball) rather than W (the cue ball hit the left side of the red ball).

Two remarks are in order here. First, it must be noticed that the fact that I hit the cue ball does not explain why the red ball went to the left rather than right pocket since the cue ball must be hit in both cases. Secondly, the question "Why did the red ball go to the left pocket rather than the right?" is not answered in a satisfactory way if the answer is only "because the cue ball hit the right side of the red". Genuine understanding is achieved only when one knows when the red ball would have gone to the right pocket.

Let's take an example from the history of science[33]: We ask

> Why did scientists come to believe that atoms[34] exist?

This question has (at least) two readings:

> (I) Why did scientists come to *believe* that atoms exist rather than have no beliefs at all?

> (II) Why did scientists come to believe *that atoms exist* rather than believe that atoms do not exist?

The first question can be answered by citing all sorts of factors, such as nutrition and oxygen, that are necessary for there to be cognitive agents. Had there not been nutrition or oxygen, scientists would not have had any beliefs. However, there is something wrong with this question. Even though it is significant that scientists came

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[33] See e.g. Renn (2005) and Psillos (2011) discussing this topic. I have used the same example in Virmajoki (2015). [In Finnish.]

[34] In a somewhat modern sense.

to believe in atoms, the lack of beliefs altogether is not an interesting alternative to our present situation. There would not be an interesting alternative to science; there would be nothing cognitively interesting.[35]

The second question is much more interesting. Here the contrast is between two beliefs with different contents. The factors that are necessary for cognitive agents to exist are irrelevant in the explanation since those factors need to be in place in order for there to be either of these two beliefs. Those factors do not tell us why one of the beliefs came to be hold rather than the other. Thus, the informative answer is:

> (1) Scientists believe in atoms rather than believe that atoms do not exist because Einstein *formulated an explanation* of the Brownian motion and Perrin confirmed this explanation with his *experimental work*. Had there not been such explanation or experimental work, scientists would not believe in atoms.

This answer has the form of explanation that was introduced above. It shows how the belief in the existence of atoms *depends on* the work of Einstein and Perrin.[36]

However, even if we agree on the above reading of the question and on the process, involving Einstein and Perrin, that must be analyzed in order to answer the question, we may still have disagreements about the explanatorily relevant aspects of that process.

Consider the following explanation:

> (2) Scientists believe in atoms rather than believe that atoms do not exist because *Einstein* formulated an explanation of Brownian motion and *Perrin* confirmed this explanation with his experimental work. Had someone else

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[35] As indicated, this analysis is developed in chapter 4. See also Chapter 8.

[36] To be sure, the knowledge gained was transmitted to other scientists and to the future generations through communication and education. In other words, Einstein's and Perrin's works are not the only factors that led to the current beliefs but they are (among) the most important factors.

formulated the explanation or performed the experimental work, scientists would not believe in atoms.

If the first explanation is found (with historiographical studies) to be more plausible, then the epistemological factors are relevant in the history of science, at least in this case. If the second explanation is found to be more plausible, then the personal prestige and social status are relevant factors in the history of science, at least in this case. It is an empirical question to which one of the explanations is more plausible. Thus, the model of contrastive explanations allows different kinds of causes to be considered as relevant in the history of science and the model even provides tools for comparing explanations that are based on different assumptions about the main causes of certain events and processes. With the notion of contrastive explanation, there is no need to make assumptions about what kind of causes are always at work in history. There can be (and there always are, as far as I know) many different kinds of causes that must be cited in a good historiographical explanation. These topics are discussed in detail in Chapter 6.

Moreover, it is important to note that the model of historical explanations sketched here differs substantially from the idea that detailed descriptions of historical developments are necessary for historical understanding. This idea is made famous by Butterfield who writes:

> It is only by undertaking an actual piece of research and looking at some point in history through the microscope that we can really visualize the complicated movements that lie behind any historical change (1931, Section 2).

The historians of science have shown, correctly, how complicated and multidimensional practice science is. However, adding more and more details to our accounts of the history of science does not help us to make sense about this complexity. *Instead of a microscope, we need a welding mask*. We need to blind ourselves to the details and focus on what is relevant, i.e. on significant features of science and on the factors that they causally depend on. The presentist approach with contrastive explanations provides the tools for achieving this. This topic is also discussed in Chapter 6.

The fact that details only matter when they make a difference to an outcome also means that narratives that describe a detailed chain of events between cause and effect are neither necessary nor sufficient for an explanation. They are not sufficient because a description of a chain of events does not tell how the outcome depended on the chain. They are not necessary since sometimes many causal chains lead to the same outcome, given that certain initial conditions hold (see Chapter 8). In these cases, detailed histories do not add explanatory force. Of course, it is still possible that a detailed description of a series of events is often more explanatory than a less detailed one. However, this has nothing to do with the amount of details involved *per se*. The only thing that matters is whether those details are such that replacing them would have led to a different outcome. As Ereshefsky and Turner puts it,

> The more the narrative is filled in, that is, thickened, with information about intermediary events that bring about the outcome, the stronger the historical explanation. [--] The mere introduction of any factor does not make a historical narrative stronger. Only citing factors that are *causally relevant* to the outcome explained make a narrative stronger. (2019 [emphasis added]).

This means that we need to understand what causal relevance is before we can assess whether a particular narrative is explanatory. At the heart of every kind of explanation lies causal dependencies.[37]

Finally, the model of explanation in historiography of science can be summarized as follows:

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[37] Due to these reasons, I will not discuss narrative explanations as a distinct category in any part of this book. That would lead us astray from understanding what causal explanations in general are. However, in Chapter 6, I discuss what makes one explanation better or deeper than another in a given context. If the reader is interested in analyzing what makes one narrative better than another, the ideas in Chapter 6 can be applied in such analysis.

1. By rational discussion we isolate a significant feature (F) of the present science.[38]

2. Next we isolate a feature F* such that: if the present science had the feature F* in contrast to the actual feature F, the present science would be interestingly different from what it actually is.

3. The task of the history of science is to provide explanations of the form: Had there been (in the past) an event W, in contrast to the actual event Z, the present science would have the feature F* in contrast to the feature F. In other words, historiography of science provides explanations for significant features of the present science. (Virmajoki 2015).

As we have now explicated the presentist approach in historiography of science, we need answer a set of common objections to presentist thinking.

3.4 Presentism Is Not the Problem; It Is the Solution

Next I discuss a set of problems associated with presentism. This discussion shows that the problems are not a serious threat to presentism. On the contrary, in many cases presentism turns out to be the most promising way to avoid problems that are recognized in the philosophy of historiography of science. The discussion shows that presentism, with its causal criterion for what counts as a history of science, is able

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[38] We do not need to require that every study in the historiography of science is *directly* related to the present situation. For example, a historian can study why it was the case that Z rather than W happened in the nineteenth century, as long as it is at least prima facie plausible that the difference between Z and W had an effect on the difference between X and Y, X being a feature of present science, and Y an interesting alternative to it. I will discuss this topic in the further chapters.

to clarify many historiographical issues. Moreover, I argue that the wider causal perspective is useful in historiography beyond demarcating what counts as the history of science. In the previous sections, I argued how presentism can be motivated with reference to the central place science has in our world. In this section, I argue that presentism is preferable for reasons internal to historiography. I show that if we look at particular historical events and processes as parts of a wider causal nexus, many problems in historiography, such as questions of contingency of science and what categories can be used to describe past practices, can be answered. The following discussion also further clarifies the presentist approach and points out important aspects of presentist commitments.

Contingency: One could perhaps argue that if historiography of science is only a study of causal histories of the present science, the historiography of science loses its grip on explaining how things were different in the past and how they could be different in the present. It is one of the main tasks of historians – the objection continues – to show that the present state of things is only contingent and that the ways in which societies have organized their epistemological practices have varied greatly. Rée writes:

> The contemplation of historicity – of the sheer singularity of places and times, situations and conjunctures, including all those you habitually take for granted – will help you see that there are different ways of looking at the world, and that what is obvious in one perspective may be ridiculous in another. (1991, 991.)

I agree that there is some force in this objection – understanding how the past has been different, and gives us tools to *imagine* how things could be at present. Yet this kind of imagination does not give us any idea of what should have happened in the past in order for things to be different at present. As we will see in Chapter 8, presentism with its contrastive explanations can answer this question. When one states that the present science has the feature F rather than F* because episode E rather than E* occurred in the past, one is automatically providing information about conditions (E*) that would have led to a different science. Thus presentism with its contrastive explanation is a rather powerful tool to reveal the degree of contingency of our own scientific practices. The presentist approach allows us to pinpoint which conditions our present practices are based on.

In connection with this objection, one could also argue that how science became conceptualized (i.e. what practice fell under the concept "science")[39] is itself historical and thus a contingent fact (e.g. Dear 2005; 2012). It could have been the case that we took as scientific different practices than we actually do. This observation is not a problem for presentism. Presentists can accept that the line between science and non-science could have been drawn differently, but once the line has been drawn, historiography of science studies the history of practices that are conceptualized as scientific as a result of this contingent process. We need to separate the history of scientific practices and the history of our conceptualization of science. Even if it is contingent which practices we conceptualize as scientific and which not, the only practices in which we are interested when we analyze and evaluate science are those that fall under our actual conception of science, a point already argued for in Sections 3.1–3.2.[40]

An analogy: Our conceptualization of different species of animals has developed historically. However, the evolution of *those species* is a completely different matter. These things need to be studied separately– in fact, only because this is separation is possible, have we been able to revise our conceptions of species (taking into account their evolutionary histories). Now, without a doubt, the history of our conceptualization of science is an interesting process that must be studied. However, this is a different project from the study of the history of scientific practices. Moreover, we need to make a further distinction between conceptualization of science (i.e. which practices

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[39] See Section 3.2 in order to avoid confusion.

[40] Of course this could be sometimes a fruitful way to reflect on the practices we understand as science. We could ask, for example, what would follow if we took an Aristotelian conception of the study of nature and applied it to our categorization of different fields of sciences. The scenario could reveal some implicit assumption in our categorization and therefore it could be a tool to understand our own sciences.

fall under the concept of "science", see Section 3.2) and our explicit beliefs (or "ideology", as Dear [2012, 38] puts it) about the nature of science, as there might be a mismatch between our conception and the ideas we associate "science". For example, one may think (after reading Popper) that science must be falsifiable and still think that science exists, even though nothing, strictly speaking, is falsifiable. Such a person would have a false belief concerning science. It would be an interesting project to study the history of beliefs associated with "science". However, this is a project different from the study of the history of how certain fields came to be considered as sciences. To continue the analogy, we can have a false belief about a species of animals and still know which animals belong to a given species. I may have the false belief that a particular snake is poisonous and still classify it as an Emerald tree boa. Why I had the belief and why I included the individual into the species can require different explanations. To assume that the history of explicit beliefs concerning science reveals why certain activities fall under the concept of science is clearly a mistake based on exaggeration of the power of abstract ideas to unambiguously impose order on the social world. Moreover, the distinction between the conception and explicit beliefs grounds the possibility of there being false beliefs concerning science. If our explicit beliefs determined what counts as science then those beliefs could not be wrong. This clearly is not the case.

What is argued above also means that, even if the conception of science changes or is revised[41] and if the historians of science begin to study the history of activities that fall under this new conception, this has no implications whatsoever to the question of which activities are reasonable to study as a part of the history of science in *our society*. Of course, one could worry that if our conception of science changes, then different practices might count as part of the history of science according to presentism. This is true, but there is no mystery here. The past remains the same but different parts

[41] And if we still want to maintain that the new conception is a conception of science, which is debatable. The aim of the argument here is to show what presentism implies if we allow that the conception of science may change.

of that past become explanatory due to the change in the *explanandum*. That different *explananda* require different explanations is nothing to be worried about.[42] One could perhaps also worry that, according to presentism, what once counted as a key contribution to the historiography of science might lose that status due to the reasons that are independent of the developments within the field. I do not see a mystery here. On the one hand, if the future science develops causally from the present one, then the history of the present science will also count as a history of the future science. The key contributions would retain their status in that scenario. On the other hand, if the future science develops independently of the present science, then the key contributions would lose their status. However, in a world that changes so drastically, there is nothing peculiar in the fact that historiography also changes radically. Actually, it is difficult to see what would be a better reason to reconsider the relevance of some historical works than a dramatic change in world history.

Finally, presentism admits that the way in which the boundaries of science are drawn can affect science. We can say, for example, that "had the people P not been excluded from what is seen as science, the discussions concerning theory T would have been different and so would the results". However, here we are not explaining when we would have had a different conception of science but when we would have had an alternative to some feature of present science (e.g. different results). In the example, a different conceptualization would have led to an alternative to the present science (the conceptualization is *an explanans*, not an *explanandum*) and the way in which science has been conceptualized belongs, therefore, to the same historical plane as the rest of the history of science: it is part of the causal history of present science. Moreover, the beliefs that some scientists have about science can be a significant feature of science and therefore require an explanation. Therefore, beliefs about science

[42] Moreover, nothing forbids historians from studying other parts of the past than those which have led to present science. Of course, such a study would not be a historiography of science according to presentism but my claim is not that only historiography of science is valuable.

can sometimes be an *explanandum* in historiography of science according to the presentist approach. However, in such cases the *explanandum* is derived from our interest in the history of present science, not from an interest in the history of beliefs concerning science.

A boring list of causes: Someone might worry that the presentist approach reduces historiography of science to the practice of listing the causally relevant factors in the development of the present science and thus the project of truly understanding the past is abandoned.

The worry can be mitigated: First, nothing in the presentist approach forbids the study of the working environment of Einstein, for example, for its own sake. It does not matter that such study might not have a direct explanatory motivation. Actually, this kind of "basic research" in historiography of science is highly valuable. The better we know and understand the past, the easier it is to find answers to explanation-seeking questions when asked. Secondly, it is not true that explanation-seeking questions can be answered easily, just by taking a quick look at the archives. Finding out relevant factors requires substantial study. What is more, to establish a causal connection between two factors in history requires argumentation that is based on detailed descriptions of the past. The claim, for example, that scientists believe in atoms because Einstein [rather than someone else] explained the Brownian motion, can be established only by detailed argumentation that confirms that Einstein held a unique and authoritative position in the scientific community. It is clear that this kind of argumentation requires enormous amounts of historical knowledge and detailed descriptions.[43]

[43] Take a passage from existing historiography to exemplify the issue presented here. Cohen writes: "Why the Golden Age came to an end when it did, roughly by the mid-2nd century BCE, is a question hard to answer with assurance in view of the *scantiness and unreliability* of the evidence. Still, two major causal factors that we shall meet in our later cases *can confidently be ruled out* for Greece. At the time, about halfway between Alexander and the armed establishment of Roman supremacy, no invasion or military conquest was so destructive as to disrupt the entire culture.

However, it must be noted that not every aspect of the working environment of Einstein, to continue the example, counts as a part of the history of science from the presentist point of view, even if studying them is necessary for historiography of science. Only those parts of the environment that are causally relevant to the present state of science can be considered and presented as a part of the history of science.[44] The point can be put as follows: pinpointing causally relevant factors is the true task of the study of the historiography of the science, but these studies must also display more or less complicated argumentative structure and accurate description of the past to warrant the claims about causal dependencies.[45] These topics, connected with the *boring list* accusation, are discussed in more detail in the chapters to come.

The past in its own term: Presentism has a bad connotation for anyone who is committed to study the past on its own terms. The warnings from Ashplant and Wilson may enter someone's mind. Among other unfortunate things, the presentist approach leads to the projection of present categories on the history of science and thus to the distorted use of the sources. It also makes us see only what is absent in the past and prevents us from finding anything concrete from the past. (1988, 255–266.) Also Cunningham (1988) argues that we should not describe historical actors by using present concepts. In short, we should understand the past on its own terms and the presentist approach cannot do this as it looks at the past from our point of view.

And of sacrilege, in the sense of a widely shared perception of current nature-knowledge trespassing religious boundaries, there was very little question. Nor was any further handling of the various branches of mathematical science bound to be fruitless." (2010, 29-30 [emphasis added].)

[44] If one is puzzled by this, consider: If a car gets into an accident, the causes of the accident can be established only by investigating a variety of factors. Yet not all of the factors are part of the causal history of the accident.

[45] The idea of separating the main thesis of a historical text from the arguments for these thesis is presented in Kuukkanen (2015).

The answer to these worries can be found in Tosh (2003, 656): "The selection of criteria we adopt when defining a discipline need not affect *how* the selected material is then investigated." Furthermore, Loison (2016, 33) points out that these are problems for what he calls "descriptive presentism" not for causal-narrative presentism defended in this book.[46] Loison defines "descriptive presentism" as "the comparison/transcription/translation of the structure of a past explanation in terms that are understandable in the present" (2016, 31). We can generalize descriptive presentism here – as we are not dealing solely with explanations but many different kinds of factors - to be *the comparison/transcription/translation of the structure of some cognitive product or process of the past in terms that are understandable in the present.* Tosh and Loison are right: no need for descriptive presentism is built into the causal-narrative presentism. We can do justice to a historical actor and see the world from her point of view even if we study her as a part of the developments that led to the present science. The claim that an agent was involved in non-scientific (or proto-scientific) activities does not demean these practices or the actor. It is not obvious why one should consider scientific practices to be the most valuable ones, and even if the scientific practices are the most valuable ones, we cannot do justice to a historical actor by changing the conception of science in such a way that it can be used to describe the actor. If science is valuable when understood in the present sense, it may not be valuable when understood in some other sense. Moreover, we have seen, in Chapter 2, that it would be a serious historical distortion to describe the past practices as *the sciences in the past*. We also saw that the presentist approach is tailor-made to avoid such descriptions.

However, it must be noted that not everything studied in close relation to the historiography of science is relevant to the developments of the present science. Only certain parts of the actions of the past actors have turned out to be relevant to those developments and thus count as parts of the history of science. (Notice, however, that there is not a straightforward connection between the causal relevance and correctness,

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[46] The reader might also consult the papers of Hall (1983), Hull (1979), Moro Abadía (2009) and Pickstone (1995).

as we shall see below). Surely, the actors could not have known which aspects of their practices would turn out to be relevant to the development of science – they were not intentionally planting seeds for the future science. However, this is irrelevant to the actual influence they had. Thus, we must adjust Tosh's point: Past practices can be studied on their own terms, but we must distinguish which aspects of these practices count as a part of the history of science and which aspects deserve to be described for some other reason. In other words, we can study every aspect of past actors and practices in their own terms but – from the presentist point of view – not every aspect should be studied and presented as a part of the history of science. To repeat the point made in connection with the objection of *boring list of causes*: pinpointing causally relevant factors is the true task of the study of the historiography of the science, but these studies must also display more or less complicated argumentative structures and accurate descriptions of the past that warrant the claims about causal dependencies.

Moreover, as historians are always products of their own historical context and as they always write to their contemporaries, it is impossible to write about the past completely on its own terms (e.g. Hull 1979). Thus, it is difficult to tell when a description of the past is adequate and when it is distorted by the present viewpoint of a historian. An advantage of the presentist approach is that it can answer this question: *a description is distorted when it does not capture the causal structure of the given historical situation correctly*. If someone claims that Darwin's theory was accepted because its truth must have been obvious to the readers, we have not even begun to capture the complexities of the process leading to the acceptance of that theory. Debate about Darwin's evidence and even his moral status were important factors in the process (see Bellon 2011).[47] The claim, that Darwin's theory was accepted because it was obviously true, would be unacceptable since it would not bring us a correct understanding about the causal structure of the history of science. Thus, presentism is not only compatible with the idea that science of the past must be understood in its own terms but also explains why and when such descriptions are needed.

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[47] This topic is discussed in Chapter 7.

As a final note, descriptive presentism was defined as "the comparison/transcription/translation of the structure of some *cognitive product or process* of the past in terms that are understandable in the present". We now see why. This follows from the criterion that *a description is distorted when it does not capture the causal structure of the given historical situation correctly*. Historiography of science makes history understandable for us in the present and therefore explanations must be based on our explanatory resources. We cannot go beyond our own conditions; neither can we turn back the clock. Historiography of science attempts to build a picture of the causal networks of the past on the basis of current knowledge (I discuss this further in Sections 6.6-6.7).[48] Almost all of the causal structure of the universe must be described in our own terms, not in terms of historical agents, since the only causal picture of universe we currently have is described in our own terms. However, this is not the end of the story. Historiography of science fits the historical actors into a causal nexus of the universe as we conceive it. That the historical actors had different ways of thinking is a component of our conception of the causal nexus of the universe and therefore we would distort our picture if we did not recognize how the historical actors were thinking (unlike us). The cognitive products and process are therefore a special case: they must be described in their own terms if we want to achieve a correct description of the causal structure of the history. Notice, however, that in the process the cognitive products and process of the past are incorporated as parts of our causal worldview. In the end of the process, there is no difference between "their own terms" and "our own terms".

[48] Notice that historical actors did this themselves: "Neptune was not known but we discovered it, and it explains the positions of Uranus"; "They believed that the surface of Moon is smooth, now we know that it is not"; why should we not? Accepting the benign difference between what was believed to be true at different times (or by different people) is nothing new. Moreover, it is necessary to recognize that difference if we want to understand *the past in its own terms* as the historical actors surely recognized the difference. Therefore, I see no reason why we should attempt to avoid accounts that show that difference.

Blind alleys: It might be argued that we can learn many things about science by studying past developments that turned out to be blind alleys. The presentist approach ignores these blind alleys.

In order to answer this, we must separate two versions of this objection. The first one is that there have been research programs in the history that (seemingly) turned out to be on wrong tracks or fruitless and were then replaced by (seemingly) more progressive programs. This case is not a problem for presentism. The process of replacement surely is a causal process that contributes to the development of science. More generally, this also means that Kuhn's worry that

> Scientific development becomes the piecemeal process by which these items have been added, singly and in combination, to the ever growing stockpile that constitutes scientific technique and knowledge. And [historiography] of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation. (1970, 1-2.)

does not arise in the presentist approach.[49] The view that the history of science must be explained causally does not imply anything about progress, cumulativity, or linearity. Which turns have taken place and which kind of causes have been at work in the history of science is an empirical question and must be answered case-by-case (I discuss this issue further in Chapters 5 and 6). Replacements and steps backward surely can be parts of the causal history of science.

The second version is that there have been research programs that ended for some reason and were never replaced but just faded away. There are two possible answers. The first is to say that these blind alleys can be causally relevant to the development of science in the sense that they may have informed people what they should not attempt to do. That a blind alley provides information is surely a causal connection. If this is the case, these blind alleys can be studied as a part of the history of science. The second answer is to bite the bullet: these blind alleys do not count as a

[49] Kuhn's worry also relates to the objections, *The past on its own terms* and *The history of winners and triumphalism* (see below).

part of the history of science. They can be studied, as I have argued, for their own sake – and we can even learn and find inspiration from them (Chang 2009, 256) – but they should not be studied or presented as a part of the history of science (see also Section 3.6). They certainly are part of the *history of epistemological practices*, but we must remember that not every epistemological practice is a scientific one and that claiming some practice as science-in-the-past is seriously problematic historiographical move.

Notice that the distinctions between different types of blind alleys is based on their causal role in the history of science. Such distinctions are important to our historical understanding of those blind alleys and to our general views of science.[50] For example, both phrenology and the phlogiston theory seem like blind alleys. However, their respective roles in the development of science, the factors behind the abandonments of the theories, and our retrospective views on the theories differ.[51] Equating phrenology and the phlogiston theory because they were blind alleys does not seem to serve any real historiographical insight. Therefore, causally structured historiography of science has an advantage in its ability to make those distinctions.

History of winners and triumphalism: It could be argued that presentism only finds the winners from the history and celebrates those who got things right and contributed to the present science. This point was made famous by Herbert Butterfield in his legendary work *The Whig Interpretation of History* (1931).

This is closely related to the two previous objections. In order to answer, we must notice that there are two ways of judging who was a winner at certain point of time. We may consider as a winner a person whose thoughts influenced the following generation the most. It is obvious that from the presentist point of view these persons should capture our attention in the history of science. Yet these persons could have been completely wrong from our point of view and thus their scientific achievements

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[50] For example, the theoretical continuity between successive theories is an important topic in the debates concerning scientific realism. See Psillos (2009).

[51] See Parssinen (1974) and Chang (2009).

are not worth celebration. The motivation for this definition of a winner is clearly debatable. Alternatively, we can consider as winners those who were right from our point of view. These persons could be celebrated if one wishes to do so (personally, I have no need for that) but it is clearly possible that these persons were not very influential and thus do not get our attention as a part of the history of science. (Again, they certainly are part of *the history of epistemological practices*, but we must remember that not every epistemological practice is a scientific one). Therefore the set of people we might celebrate and the set of people who are an important part of the history of science is not coextensive.[52]

To be sure, one of the main advantages of the presentist approach is that it gives objective criteria for which practices count as a part of history of science.[53] Presentism ensures that one cannot pick one's subjects of study just as one wishes and thus presentism restricts the possibility of celebrating the historical actors one happens to favor for some reason. If the choice of the subjects of study were purely a matter of convention, it would be possible to ignore some historical actors who deserve attention. The kind of ignorance that could be based on an ideology or a subjective bias is surely something that every respectable historian of science wants to get rid of. The presentist approach gives concrete tools to avoid these biases. (This issue is also discussed in the next chapter.)

Finally, one could argue there is a deep-level "winnerism" in the presentist approach: our present science is, in as sense, a winner as it became established rather than its alternatives. The history of science is then a history of a winner. If this is the definition of winner, then all historiography is necessarily winnerist. Every actor one

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[52] The formulation of the answer is based on the points made by Hasok Chang (2009).

[53] Of course, if our conception of science changes, then different practices might count as part of history of science. There is no mystery here. The past remains the same but different parts of that past become explanatory due to the change in the *explanandum*. That different *explananda* require different explanation is nothing to be worried about. Again, nothing forbids historians to study other parts of the past.

studies was an actual historical actor unlike her possible alternatives and therefore a winner. Such deep-level winnerism is therefore no argument against presentism.

The problem of the Big Picture: One could argue that presentism is a form of big-picture thinking since it defines the history of science as a comprehensive account of the developments that have led to present science. This kind of big picture should be rejected (Shapin 2005, 242).

It is true that the ideal goal of presentism is a comprehensive account of the causal history of present science. Yet it is not committed to the usual sins that make the big-picture thinking questionable. Shapin (2005, 242) writes: "Big pictures imply coherence [and] in old versions [--] scientific coherence [means] the conceptual unity and universality of science, narratives of rational and linear progress, a specifiable and efficacious scientific method [--]." Presentism is not committed to the claims that the history of science has progressed linearly or that the development of science is driven by rational decision making and by use of clear methods. What turns have taken place and what kind of causes have been at work in the history of science is an empirical question and must be answered case-by-case (I discuss this issue further in Chapters 5 and 6).

We can say even more: Compare the presentist approach to some other framework in which a historian approaches the history of science by some (perhaps implicit or intuitive) definition of science. If we allow this kind of approach to the history of science, it is difficult to say what prohibits a historian to define prior to the research science as a rational practice that is driven by clear methods. Thus, the distortions of the big picture would reappear since it would be possible for the historian to describe the history of science as consisting of activities driven by rationality and clear methodology. This description would follow directly from the definition that the historian has chosen. Again, one of the main advantages of the presentist approach is that it provides objective criteria for which practices count as a part of history of science. Presentism makes sure that one cannot simply pick one's subjects of study as one wishes and thus presentism can get rid of the biases of considering the history of science as a history of rational activities driven by clear methodology (and any other unjustified biases).

3.5 Presentism as a Historiographical Tool

As the previous sections indicate, the presentist approach can be seen as a (conceptual) tool for historiography of science. There are three questions that every historian of science must ask and that can be clarified or bypassed with the presentist approach:

> 1. What is the nature of science? In other words, what are the properties, if any, that make certain activities scientific?
>
> 2. What are the driving forces of the history of science, or, more mundanely: what kind of causes have produced scientific developments?
>
> 3. What are the results that the historians of science are expected to produce? In other words, should the historians of science just describe the past as it happened (and from the point of view of the actors involved) or should there be some more specific results to be communicated?

Presentism is a useful approach to avoid the debates that are concerned with the first question. These debates appear as a distracting kind of philosophical pondering from the perspective of historiography of science, no matter what their philosophical weight. As noted, we can bypass the discussions about the nature of science when selecting the targets of studies in historiography of science. We simply focus on the practices that are considered as scientific by our society. One may or may not have a notion of "science" that she prefers but in the presentist approach this should not affect how the targets of studies are chosen. The discussion about the nature of science is not about which of our practices are considered as sciences. The most critical overviews and the most triumphant celebrations of science discuss the same activities, the sciences of our society, even when they disagree about the nature and status of this activity. Thus, presentism allows us to set the disagreements aside and to focus on the same thing: to understand the present sciences and their history. Historians may disagree after their studies but these disagreements do not need to complicate the beginning of fruitful research projects.

Secondly, the presentist approach and contrastive explanations help to escape the discussions about what are the *real* driving forces in the history of science. One does not need to decide what kind of causes drive the history of science before one begins the search for causal explanations.[54] What kind of causes were behind the fact that some X happened rather than some Y is an empirical matter and depends on what X and Y are. For example, one could say that the belief in atoms was to a large extent produced by explanatory and experimental considerations, whereas the acceptance of eugenics was dominantly due to social forces. Moreover, in the cases where historians offer different explanation for the same episode in the history of science, the model of contrastive explanation helps to explicate the differences between these explanations and leaves as an empirical question which of the explanations is the most plausible one (see Chapter 6). This means that the historians do not need to build their studies on mutually incommensurable models of explanations that have as built-in features certain assumptions about what kind of causes drive the historical developments. We see that, again, presentism allows the historians of science to have a (minimal) shared starting point and leaves the most heated debates to be solved after the relevant studies are carried out.

Finally, presentism allows the past to be described in its own terms and still demands specific results in the form of causal explanations. As argued, to find out and to justify certain conclusions about the causal structure of the past requires detailed understanding and descriptions of the past. This means that the studies produced by

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[54] However, see Section 6.6.

the historians of science must be rich in detail and yet useful for those who want to understand the present situation. Therefore, a reader who simply wants to gain understanding about some era for its own sake will be satisfied by these studies as well as a reader with more present-minded concerns. This benefits everyone.

3.6 Presentist Historiography and the Philosophy of Science

Now that we have outlined the presentist framework, one important philosophical question remains to be discussed. It is the relationship between historiography of science and the philosophy of science. How does the presentist approach affect our picture of philosophy of science?

It has been widely thought that philosophical theories[55] of science must stand tests[56] against the history of science in order to be accepted (see e.g. McAllister [2018, 239] and Donovan et al. [1988, 3–8]). An important consequence of presentism is that the testing of philosophical theories against the history of science is not a straightforward matter. In the presentist approach, a practice is a part of history of science if and only if it has been part of the causal history leading to the present science. Therefore, there are practices that are not scientific themselves but belong to the history of science. This means that studying these practices tell us not about the nature of science but how science came to be. It seems that we should not demand that our philosophical theories of science capture the nature of practices that were not scientific in the strict sense. This means that being a part of the history of science is not sufficient for a practice to serve as evidence for the philosophy of science. This may sound deeply problematic since it is usually being thought that the compatibility with the actual history

[55] By philosophical theories of science I mean theories that philosophers of science usually are interested in, such as theories of confirmation, explanation, scientific realism, scientific progress, and conceptual change.

[56] We need not bother what the testing exactly involves. The basic idea is that there are evidential links between history of science and philosophy of science.

of science is one of the best tests for the philosophical theories of science. If we lose this way of looking at the epistemology of philosophy of science, what is left?

There are five points that mitigate the worries: First, it can be argued that the philosophy of science – at least some branches of it – has not paid enough attention even to present science (Woodward 2014). Moreover, the number of relevant historical cases is not the problem, as it is unclear how any historical study can serve as evidence for the philosophy of science (see e.g. Pitt [2001], Schickore [2011], Kinzel [2015B] and *JPH* 12 (2) [2018]).

Secondly, it is not necessarily a bad thing if certain episodes and practices cannot be used as evidence in the philosophy of science. For example, it is not plausible to make the case that religion and science cannot be separated (or that religion can even improve scientific thinking) since in Newton's thinking science and religion were in close contact.[57] In general it seems to be a good conjecture that the accounts of the older historical practices are less relevant for the philosophy of science than the accounts of the more recent ones. Even if one is willing to describe (in an anachronistic way) these older activities as scientific, this does not make them relevant for the philosophy of science. Falling back to the science-in-the-past view does not solve the problem. The fundamental problem, that the past was different from the present, is built also in this view, although in a deceptive manner.[58]

In the third place, we can test a philosophical theory against certain past practice even if that practice is not a scientific one (in the strict sense). It is sufficient that the

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[57] For example, we can find this kind of argumentation in Brooke (2014, especially on page 24) and in Dembski & Ruse (2006, 44). My claim is not that there is no connection between the present science and religion – that is an empirical matter. I only argue that facts about Newton's practices are not relevant for establishing this connection.

[58] Moreover: assume, for the sake of argument, that science in the past was very *similar* to the present science. If this was the case, there would be no reason to test our theories against history of science, as we would get the same results from studying the present science. We are facing a serious dilemma here.

past practice is similar to the present science with respect to some relevant feature.[59] What these relevant features are depends on the theory we are testing and the questions we want to answer. One chooses a feature of science that is to be analyzed and then studies the historical practices that share the feature. One does not need to consider these historical practices as scientific. It is enough that the practices are similar enough to the present science for the purposes of the research problem at hand. If, for example, we did not consider Newton as a scientist in the present sense (due to his views about the connection between religion and physics, for example) we could still study his practices in order to understand how, for example, theories are formulated. Newton's practices with empirical data and mathematical models that were associated with the data are *prima facie* similar enough to present science with respect to the features relevant to the formulation of theories (i.e. with respect to the use of empirical data and mathematical models). In other words, a practice might not be scientific yet still work as evidence for the philosophy of science.

However, there is a complication: the problem of biased selection of favorable historical cases (see Pitt 2001) is magnified. First, the number of cases available for the philosophy of science becomes much larger once we allow that philosophical theories can be evaluated against "scientific enough" historical episodes. It seems that one could find support for any imaginable philosophical theory from such vast resources. Secondly, the notion of *relevant similarity* seems to guarantee that there cannot be a genuine counterexample for one's theory. If such a counterexample is suggested, one can always claim that the example is not a relevant historical episode. Because there is not a fixed set of historical scientific episodes, there is nothing that a theory of science must be able to capture. In a sense, a philosophical theory can never be incorrect. And if a theory cannot be incorrect, it cannot be correct either: if there were not scientific episodes in the past (in the strict sense), then there is nothing that a philosophical

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[59] Sometimes a difference can be interesting: one could compare the history of science and the history of some other practice in order to find out important things about science.

theory could capture. However, it does not follow that historical considerations cannot play part in evaluating philosophical theories of science. What follows is that there is no distinct set of historical episodes, “sciences in the past”, that have a privileged evidential status in the philosophy of science. Once this is accepted, we need a new picture of how philosophical theories of science can be evaluated against any knowledge we have, not just against knowledge of history of science. In a sense, there is nothing new in this conclusion. It is a plea for full-blown naturalism.

I accept such naturalism. After all, science is a human practice and thus shares features with other human practices. We should be able to use the knowledge about these practices to inform our philosophical theories of science. I discuss this issue in Section 6.6. There I argue that all explanatory knowledge, ignoring disciplinary boundaries, can be relevant to historiography of science. It is not difficult to extend this thought from the historiography to the philosophy of science.

In the fourth place, while tests requiring multiple cases seem necessary in evaluating general philosophical theories of science (McAllister 2018), it is unclear why we should build such general theories with wide applicability rather than answer particular questions. If we do not seek theories but understanding about some particular feature of science, the necessity of testing a philosophy of science against a wide range of historical cases loses its force. If quantitative testing is no longer required, historiographical studies can be used in varied ways to answer any particular question. The amount of evidence is no longer a pressing concern as we no longer attempt to generalize the results. I find this suggestion, that philosophy of science can be fruitful without general theories, appealing. Not only the problem of lack of evidence is bypassed but the suggestion also captures the state where many philosophical debates stand. Take an example: In Chapter 8, I discuss how to approach the question “Could science be different?” I argue that this question must be answered in a piecemeal manner. Surely the exact notations used in science can be highly contingent while development of astronomy is almost inevitable. We do not need – and it might be impossible to get – a general theory of contingency of science. We need – and we can achieve – understanding about the inevitability/contingency of particular features of science. Take another example. Similar piecemeal manner of solving philosophical problems is suggested in the debate concerning scientific realism. It is not possible to give a general theory that tells whether realist attitude toward every theory – and toward everything those theories claim – is the correct one. As suggested, we should only focus on mature and successful theories (Psillos 1999, xvii), and, what is more, we might need to sort out in a piecemeal manner what parts of those theories deserve realist reading. (Psillos (1999, 103-109) discusses this "*divide et impera* move"). What first seemed like a philosophical theory, scientific realism, now seems like a set of answers to particular questions concerning the status of particular theoretical claims.[60] A unified picture might or might not arise, and yet our intellectual needs to understand particular cases under discussion is satisfied.[61]

The idea that philosophy of science gives answers to particular questions recognizes that the very nature of science as evolving human practice goes against general theories.[62] Epistemological practices in the past were different from present science. It

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[60] Of course we need a theory (or rather a definition) of what scientific realism is before we can judge which theoretical components of science deserve realistic reading. My point is that we cannot take scientific realism as a descriptive theory that attempts to capture all the theoretical claims in science.

[61] It even seems confused to ask whether scientific realism is a good general theory after we have decided which theories deserve realist reading. What should this general theory be about besides the cases already decided? There are not too many candidates as theories competing for realist reading anyway – or at least their number is lower than the number required for testing scientific realism once and for all, and therefore we should examine those theories one-by-one anyway.

[62] There is an analogy to *the evolutionary contingency thesis*: "all distinctively biological generalizations describe evolutionarily contingent states of nature [--]. This means that there are no laws in biology". (Beatty 1995, 46). I do not think that it is correct to describe philosophy of science as searching for "genuine laws of science". Yet, the more general philosophical theory one attempts to build, the closer that theory comes to an attempt to establish "the laws of science". The fact that the guiding principles of science have developed through a complex history should make one cautious of their existence in the future.

might very well be that the development continues and with that development the need to reshape our understanding of science persists. Taking the historical nature of science seriously, there probably cannot be any interesting general theory of science, formulated now, that will be useful in the future. If the historical developments did not matter, we could attempt to build a general theory of science by studying only Aristotle's work. This is absurd. Therefore, it is obvious that later developments are relevant. This should also be our attitude toward the future development. For example, artificial intelligence may change scientific practices fundamentally. It is therefore better to have the ability to give admittedly limited but insightful answers than to seek a theory that has great applicability *now*. Presentism, therefore, goes somewhat against the spirit of general philosophical theories of science. However, the way in which the presentist approach orientates us in time enables us to anticipate our future philosophical needs.

Finally, we may question the fruitfulness of descriptive theories of science. Suppose we had a theory of science that captures many episodes in the history of science. It seems that we could still ask whether science should have developed in accordance with that theory. It seems that it could be more fruitful – although much more difficult – to ask *what would have happened in the past, had science developed in accordance with some philosophical theory*. If we take a look at Krige (2001), it seems that different groups of scientists claim that a new fundamental particle (the W boson) had been discovered at CERN after a series of events that was driven by (among other things) political pressures and hurrying. Should we accept the view that political pressures and hurrying are important factors leading to interesting results? This view would be descriptively adequate; yet it seems that it should not be our philosophical take on science. We should ask what would have happened without political pressure and without hurry: if the result of the series of events had been at least equally preferable as the actual result, then we should not accept the descriptively adequate view. This means that: (i) our philosophical theories should have a normative tone, and (ii) therefore we should test those theories against counterfactual scenarios. Counterfactual scenarios are always difficult to work with. However, as we have seen (and as we will develop further in Chapter 6), explanations in historiography of science have the form "(Actual) X rather than (counterfactual) Y, because (actual) Z rather than (counterfactual) W". Testing philosophical theories against counterfactual scenarios is therefore a "mirror image" of explanations and therefore no more challenging: we ask "What would have happened, had W rather than Z been the case?" (the answer is "Y") and decide whether it would have been preferable that Y was the case. On the basis of a philosophical theory of historiography of science, we have here a suggestion of new ways of thinking about philosophical theories of science.

Such a counterfactual account of testing is most naturally suited for theories of scientific development. However, it is not limited to those theories. For example, in the philosophy of biology, there are arguments in favor of explanatory pluralism (e.g. Théry 2015; Mekios 2015). One common theme in such arguments is that some particular account of explanation does not capture some of the important explanatory practices of biology. These arguments seem to have the form (F):

> Had scientists followed the philosophical account A, they would not have achieved some important (actual) results.

More generally, there are many philosophical arguments that claim that some philosophical account does not capture the practices of scientists and they all seem to share the form F (i.e. they are *F-claims*). In fact, it seems that a commitment to an F-claim follows automatically from any argument that proceeds to show that certain philosophical account is not satisfactory. If the claim "Had scientist followed the account A, they would have achieved the same results as they actually did" is true, then it is difficult to see what is wrong with the account A. For sure, the account may not fit the actual case at hand but this only means that there are many equally fruitful ways to proceed in science (which would be an interesting result in its own right). Only if one equates important factors with the actual ones it becomes possible to claim that something important is missing from the account A. However, in this case, one would achieve philosophical excellence by adding more and more details into one's account. It is much more natural to suggest that an important factor is one that makes a difference to the outcome, and therefore an account can be defective only if the process described by the account would have led to a different outcome than the actual course of events.

The presence of F-claims in wide variety of cases indicates that many philosophical debates have counterfactuals in their deep-structure. Once we notice this, an argument of the form F*:

> Had scientists followed the philosophical account B, they would have achieved some important (counterfactual) results.

seems to be on par with "normal" philosophy of science. A counterfactual account of testing does not seem that exotic after all.

Moreover, counterfactuals are also needed in interpreting particular features of science. For example, the *divide et impera* move has a modal aspect: "Theoretical constituents which make essential contributions to successes are those that have an *indispensable role in their generation*." (Psillos 1999, 104 [emphasis added]). Our interpretation of a theoretical constituent depends on what would have happened without that constituent. We will also see in Chapter 8 that answering questions of contingency/inevitability of science involves counterfactual considerations.

Conclusion of the section: In the presentist approach, being a part of the history of science is neither necessary nor sufficient for a practice to count as evidence for a philosophical theory of science. Therefore, presentism is not the best partner for developing (descriptive) philosophical theories of science. However, as the discussion above indicates, it enables us to see clearly the relationship between historiography of science and the philosophy of science against a conceptually clarified background and suggests therefore new directions for the philosophy of science.

4 WHAT IS SIGNIFICANT ABOUT SCIENCE?

In this chapter, I develop further the idea that historiography of science should focus on significant features of science. Historiographical studies should be connected to our values and preferences. I argue that there is a serious need for selecting the subjects of studies in a systematic and intersubjective way. Such selection will lead to highly motivated and bias-correcting historiography of science. Moreover, the problem of demarcating science and non-science in historiography of science can be mitigated once we focus only on the significant features of science.

4.1 Why We Need Judgements of Significance?

Science has many significant features. As a first approximation, the significant features are those that are somehow important or valued – features that stand out when science, as an epistemological practice that has wide connections to various aspects of life, is reflected on. For example, the features of science that have an impact on our practical matters are significant as well as the features that bring meanings and understanding to our lives. In the presentist historiography of science, we study the causal history of the significant features of present science, as we saw in Chapter 2. Focusing on the significant features is important for three reasons:

First, science is such an enormous enterprise that the history of every detail cannot be studied. The study of the history of significant features helps to connect the results of different historians with each other, with the interests of scholars in other fields, and with the wider social concerns. Historiography of science can and should be interesting to a wide range of people.

Secondly, in the absence of clear demarcation criteria between science and non-science, it is important that we have some rationale to ignore the borderline cases of

I thank Helena Siipi and Juha Räikkä for their comments on an earlier version of this chapter.

science in historiography of science. In this chapter, we see that borderline cases can be usually ignored due to their insignificance.

Thirdly, even if there were clear criteria for demarcation, the historians of science do not focus only on those features of the present science that are constitutive of it being science. In order to capture those features that are not constitutive of science but still require explanation, we need a new category: the significant features.[63] For example, we might want to explain why science has been powerless to affect the tobacco industry (see Rego [2009], discussed briefly in Section 5.3) but it would be incorrect to *define* science by citing that feature. In other words, we would like to know why science is so powerless and yet it does not seem right to say that if the researchers had such power, they would no longer be practicing science (which would be the case if the powerlessness was a constitutive feature of science). The powerlessness is therefore a non-constitutive significant feature. Science has much more significant features than it has constitutive ones (or features that are serious candidates for constitutive features, as there might not be a clear-cut set of constitutive features, see Section 4.7).

These reasons suggest that the judgment that a certain feature, F, of science is significant must be based on an intersubjective and transparent decision procedure. Otherwise there is (i) the risk that the study of history of F has no relevance outside the scholarly circles of historians, (ii) and the risk of biased historiography of science. [64] The first would be the case, for example, if some historians focused on the sporting habits of scientists due to their training as historians of sports. This study would

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[63] Constitutive features are a subcategory of this wider category. We will see in Sections 4.3-4.4 that a significant feature is one whose replacement would lead to an interesting different situation. A (counterfactual) situation in which science does not exist would surely be an interestingly different situation, given the widespread impact of science in our lives. Therefore, constitutive features are significant features.

[64] By "biased" I mean historiography that deliberately or without sufficient reflection produces one-sided historical accounts on topics where such one-sidedness has potentially harmful effects.

hardly tell us anything illuminating about science to those outside the history departments (no matter how entertaining such a book could be in itself).[65] The second would be the case, for example, if some historians, sharing a common political agenda, defined as significant only those features that are directly connected to their agenda. This would lead to a biased historiography and our understanding of science would be distorted.

However, it is not pragmatically possible that the historians of science first analyze in depth which features of science are significant and only after that begin their studies. Making warranted judgements about significance is sometimes a difficult and time-consuming process. Moreover, it would have a paralyzing effect on historiography of science if it was required that a historian of science should not make judgements of significance herself. Due to these observations, I suggest that we can be satisfied if the following conditions hold:

First, it must be possible, in principle, to check whether the feature F of science that a historian studies counts as significant or not. Thus, a historian of science must explicate the reasons which led her to judge the feature F as significant. In a further discussion by the society, these reason may turn out to be bad or it may turn out that the judgment that F is significant cannot be justified in any way. However, these should not prevent the historian from studying F if her reasons are at least *prima facie* acceptable. The explication of the reasons, then, serves as kind of a mark that enables other people to recognize and evaluate the possible relevance/irrelevance of a historiographical work

Secondly, in order for there to be a legitimate judgement of the significance of F, the reasons given for the judgement must be judged as good reasons at least by some

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[65] This is just an example. The judgement can be corrected by using the method of RE (see below). For example, physical sports may help the scientist understand anatomy. If this is the case, we can then say that the principle of significance that captures the significance of sporting activities is not "feature F is significant if it is related to sports" but "F is significant if it is serves as a source of insight."

society that shares a reasonable value-system. As we live in a multi-value society, which consists of different groups that share different values, we cannot expect that everyone should agree about the significance of some feature F after a (necessarily, as a fact of life) imperfect discussion. However, explicating the reasons in the historiography makes the process possible. Moreover, even if the historians can never achieve a point of agreement in their judgements of significance, the historians can at least understand why other historians make particular judgements once the reasons for these judgements are made explicit.[66] This enables us to prevent misunderstanding each other. The explication of reasons also enables us to track down the changes in the judgement of significance in situations where the values of a society change. This provides us with a reflective understanding about our own historical situation. These achievements are all that we can hope for – a philosophical theory of historiography, such as the one in this book, cannot be based on the ideal that all the human societies share the same values. However, it is still worthwhile to attempt to give a framework within which the values of a field, such as historiography of science, can be discussed and evaluated. In this chapter, I show how this can be done.

4.2 The Method of Reflective Equilibrium

Due to the considerations above, I suggest that the method of reflective equilibrium (RE) is suitable for deciding which features of science are significant. This method goes back to the writings of Goodman (1954) (who wrote about inductive logic) and Rawls (1951 & 1971) (who wrote about justice). Scanlon describes the method of RE (in the context of philosophy of justice):

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[66] Once we understand the values that a group has, we can also decide whether these values are dangerous or not from our perspective. However, my hope is that, in the historiography, different values do not lead to situations where societies form a threat to one another except for any intellectual sense. What to do in situations where different groups form a serious non-intellectual threat to one another cannot be solved here, but belongs to political and moral philosophy.

> One begins by identifying a set of considered judgments about justice. [--]. The second stage is to try to formulate principles that would "account for" these judgments. [--]. [The] third stage in which one decides how to respond to the divergence between these principles and one's considered judgments. Should one give up the judgments that the principles fail to account for, or modify the principles, in order to achieve a better fit? (2003. 140–141.)

In historiography of science, this method would proceed as follows: Historians[67] begin with the features of science that seem *prima facie* to be significant. Then they try to formulate principles that capture these features as significant. If it happens that these principles do not allow the historians to judge as significant some features that seem to be *prima facie* significant, the historians need to replace the principles or to bite the bullet and say that the significance of the features is illusory. The more the historians study the science, the more features of it we recognize. These newly recognized features can seem to be *prima facie* significant and if the accepted principles do not lead to this judgment, the historians need to reconsider which principles and *prima facie* judgments of significance they accept.

It is important to notice immediately that the use of RE requires that historians and other people already judge some features of science as significant. There is nothing dubious here. Such judgements are always involved when the targets of study are chosen. Moreover, as my purpose is to provide tools for historiographical thinking, not to create a competing historiographical tradition, I see no reason to put the quality of those judgements into systematic doubt (see also Section 4.6). For the purposes of this chapter, we can understand RE as a procedure that enables historians to make their judgements explicit and therefore to analyze what commitments are made by judging that certain features of science are significant and what is the cost of these commitments. By using RE, historians can follow the implications of their judgements of significance and adjust these judgements if the implications are unacceptable. In the

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[67] I will write about how historians in particular can use RE. However, everyone can use RE to analyze which features of science are significant, and the discussion about significance should not be understood as an exclusive task of historians. In fact, it would be preferable if a wide range of people with diverse backgrounds and educations were involved in the discussion about significance.

same way, other people can use RE to analyze the judgements of the historians and find out whether a historical study discusses issues that the people are interested in.

Let's take an illustration. We might say that the fact that scientists believe in atoms is a *prima facie* significant feature of science as well as the use of mathematical tools. Then we could formulate the principle: *a feature of science is significant if it affects a wide range of everyday practices of scientists*. We could also come to believe that gender inequality is a *prima facie* significant feature of some sciences. Our principle would account for this: inequality affects a wide variety of everyday practices of scientists. However, consider then the structure of the lunch and coffee breaks in scientific communities. These affect the everyday practices of scientists but do not seem to be significant. It can be noted that our initial principle is too general. We need to specify our principle: *a feature of science is significant if it affects a wide range of epistemological practices of scientists.* Our initial judgements of significance about belief in atoms, the use of mathematical tools, and the gender inequality would still be correct in the light of this principle.[68] (Note that this is *not* intended as the *only* principle that should be used; another example is given below).

This example gives an idea of how a wide range of features of science might be considered as significant. We can look again at the sketch of aspects of science which are worth historical studies, as this list gives a good idea of the areas where significant features can be found:

> *Social*: How is science organized? What kinds of social roles are there in the sciences and how do these roles guide the practices? Which and whose values are built in the science?

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[68] The case can be made that an unjustified inequality between different groups of people affects negatively almost every goal-directed practice and thus also the epistemological practices of science. I cannot argue for/against this claim here; this is just an example.

Science and society: What is the relationship between science and politics, science and the economy, science and different social groups? How global/local is science? How accessible is science for different groups? How are the results of science communicated to and understood by society in general? Which and whose values are built into the science?

Science and culture: What is the relationship between science and other aspects of culture (religion, for example)? What and whose values are built into science?

Science and technology: What is the role of science in the making of new technologies? How does technology shape scientific practices?

Psychological: How do individual scientists understand themselves? What kind of thoughts and emotions do scientists have during their work and about their work?

Take another example. We may say that the observation of gravitational deflection[69] is a significant feature of science because it plays an important part in the acceptance of relativistic physics (and thus in the overall shape of our physics) and in the understanding and technology that it provides. The principle would be: *F is significant if it gives us understanding and provides technology*. Then a sociologist can point out that, since the contribution to the technological state of our society is significant about science, the origin of the funding in science is also a significant feature of science because it shapes the distribution and use of technology in a way that matters (specified by the sociologists).

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[69] See Chapter 8.

4.3 Contrastive Explanations and the Canonical Form of Significant Features

We saw that it is important that the principles of significance are made explicit so that we can evaluate the judgements of significance made by historians in a transparent way. However, there is also a technical reason to make the principles explicit and to track down the uses of these principles: A historical explanation is always contrastive (see Chapter 6). When we explain "Why F?" we in fact explain "Why F rather than F*?" where F* is an alternative that would make science interestingly different.[70] Thus, the principles on which we base our judgements of significance must be visible in the choice of contrast features. If we claim that gender inequality is a significant feature of science on the basis that it affects the epistemological practices, then we need to choose the contrast in a way that this contrast would mean interestingly different epistemological practices (in this case: better practices). We could, for example, ask why there is gender inequality in science rather than no inequality at all – equal scientific communities would be interestingly different from the actual with respect to epistemological practices (assuming that inequality has a negative impact on such practices). However, it would be pointless to ask in this context why there exists a gender inequality rather than inequality between people from different backgrounds, since there would still be an inequality and probably as problematic epistemological practices as in the other case. There is no relevant difference, with respect to epistemological practices, between two types of inequality; there is such a difference between equality and inequality. If we choose the first contrast, we do not explain why science has an actual feature F rather than an interestingly different feature F*. Such

[70] See Section 4.4 on the notion of an *interesting alternative*. See also Chapter 8.

explanation is irrelevant.[71] To prevent such irrelevant explanations, we need to stay on track with our principles of significance.

To prevent situations where a principle and a contrast do not match, we should proceed as follows: When we use the method of RE, we began by *prima facie* significant features and explicate what alternatives to these are the interesting ones. The principles we then formulate must capture in what sense a significant feature and its interesting alternative differ. Moreover, when we recognize further features of science that seem *prima facie* significant we must, again, explicate the interesting alternatives to these and then see whether our principles are able to capture the importance of this difference. For example, it may seem *prima facie* that gender inequality is significant and that an interesting alternative to it would be completely equal scientific practice. It may also seem *prima facie* that some other form of inequality is not an interesting alternative. If this is the case, the principle that *a feature of science is significant if it affects a wide range of epistemological practices of scientists* captures the sense in which one of these alternatives is interesting and one is not: with respect to the epistemological practices, removing inequality altogether would lead to an interesting alternative unlike changing the form of the inequality.

We are touching an important issue here. To understand the choice of contrast better, we need to note the following:

The significant features can be captured by the *canonical form*:

> Subject S does P in location L at time T.
>
> The subject can be a cognitive agent (e.g. Newton or a computer); a group (e.g. Einstein, Podolsky and Rosen); a community (e.g. contemporary physicists); or an object (a particle accelerator).

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[71] We should not confuse *explanatory irrelevance* with *irrelevant explanation*. The first notion refers to a intrinsic failure of an explanation to find causally relevant factors, the later refers to a situation where we have a true explanation that is irrelevant with respect to an external purpose.

> The "P" can be an action (e.g. looks at the sky); an activity (e.g. observing the movement of heavenly bodies); a having of something (having an air-pump); an instantiation of some property/relation (e.g. being a good calculator/being a part of a group); or simply existence (e.g. the existence of a particle accelerator).
>
> Locations range from a very limited area (e.g. a particular observatory) to a global distribution (e.g. teaching of mathematics) and to the whole observable universe.
>
> Time can also vary from a brief instance (e.g. a particular sound of a Geiger counter) to a period of thousands of years (e.g. the existence of mathematics).

However, not everything in the canonical form must be essential[72] to any given significant feature. An example: Even if it is – in canonical form - significant *that the Laws of Motion have been incorporated in science by Newton in some particular location at particular time*, we may want to abstract away some details, and say for example that *that the Laws of Motion are incorporated into science is a significant feature of science*. Here we have abstracted away the actual subject, the actual location and the actual timing of the creation of those laws. (We can also make the feature more vague; for example we could say that *it is significant that the Laws of Motion have been incorporated into science for more than two hundred years and that they were formulated in Britain*). We could perform such an abstraction if we thought that some part of our present science would

[72] The use of the term "essential" is not meant to be read in a metaphysical sense. Something is not essential if changing it does not give us an interesting alternative.

not be interestingly different even if the subject, location or timing was different.[73] This process of abstraction is important for the subsequent strategies of explanation of the significant features: If we have concluded that some part of the canonical form is not essential to some significant feature, a causal explanation of that feature should not mention that part in its choice of contrast. For example, if we think that the identity of the subject (Newton) is not essential to the significance of *that the Laws of Motion have been incorporated into science by Newton in some particular location at particular time*, then answering the question "Why did Newton rather than someone else formulate the Laws of motion?" does not explain why science has the significant feature.[74] (Causal explanations are discussed in more detail in Chapter 6). Even if someone else had formulated the Laws, the essential parts of the significant feature would still be the same and therefore we would not live in an interestingly different world.

We have now seen how a coherent picture of significant features of science can be approached. We have seen that we must have an account of (i) the significant features of science, (ii) interesting alternative(s) to each of the significant features, and (iii) a set of principles that allow us to judge the significance of features and explain why some alternatives to them are interesting.

4.4 Preferable Scientific Practices

At this point, we need to ask what stands behind the intuitions and principles that determine what counts as an interesting alternative to some feature of present

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[73] I discuss such maneuvers in Chapter 8.

[74] This is similar to a case where we want to explain why a car accident happened but end up getting an answer to the question "Why did the cars collide in place L and not 1 cm to the left of L?" The exact location is not essential to the car accident; if someone died, it would be banal to suggest that an interesting alternative to the accident would have been one where the location of the fatal accident was slightly different.

science. I suggest that we should understand this as reflecting on our ideas of preferable[75] scientific and science-related activities. That we considered mathematical tools as significant is based on the idea that without these tools, our understanding of the world would be undesirably limited in comparison with the present state of science; that science has made nuclear weapons possible is significant because we would prefer to live in a world without the possibility of a nuclear catastrophe. An account of what is and what should be preferred cannot be given here. However, what kind of futures are and should be preferred are the kind of questions that the field of futures studies has been asking (Marien 2002). Many workable methods of finding out and evaluating preferences have been developed (see e.g. Bell [1997] and Hicks [1998]). Even if it is the case, as Niiniluoto (2001) argues, that the question of what is preferable is a question that philosophers, not science, can answer, we can still think that historiography of science should also reflect the actual preferences of people. We may consider this as a democratic duty of the historians. Historiography of science can be seen as a field that answers the questions of the form, "Why does science have the preferable/undesirable feature F rather than the undesirable/preferable feature F*", where the preferences that are built into the question can come from (i) the reflections of the historians themselves, (ii) the reflection of the philosophers, or (iii) the reflections of some group that is interested in the present state of science for some particular reason. However, we should not require that the questions about what is *really* preferable and what is not be answered before there can be fruitful historiographical studies. Instead, we should see historiography of science as a field of expertise that can help us answer preference-based questions when such questions are asked.

4.5 Science as an Epistemological Practice

We saw that historiography of science must somehow reflect our notions of preferable/undesirable scientific practices. Even though science is a multidimensional

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[75] The term "desirable" could be used as well.

practice that has many connections with diverse aspects of life, we should not forget that *science is first and foremost an epistemological practice*. We already saw, at the end of Section 3.1, that historiography of science derives its *raison d'être* from the importance of epistemological practices in our lives. Our preferences and what we judge to be significant about science must take into an account the epistemological nature of science.

I suggest that there are four classes of judgements of significance that can be made if we take seriously the epistemological nature of science:

> 1. We can assert that some feature F of science is directly significant because it is a part of the epistemological framework of science that consists of theories, models, concepts, methods etc.
>
> 2. We can assert that some feature F of science is significant because the epistemological framework depends on that feature. For example, the social structures of science and technological resources are such features.
>
> 3. We can assert that some feature F of science is significant if it is the result of an epistemological framework of science. For example, nuclear weapons and medical treatments are such features.
>
> 4. We can assert that the feature F of science is significant if the existence of F is somehow puzzling, given the epistemological framework of science. For example, scientific frauds belong to this category, as they go against the methodological rules and ethos of science. Another example is the inability of scientific results to affect the tobacco industry (see Rego 2009).

This taxonomy shows how some particular features of science derive their significance from the epistemological nature of science. The significance of science is based on its epistemological nature, and historiography of science should not ignore this. However, not every detail of science connected to the epistemological nature of science is equally significant.[76] Thus, we still need the method of reflective equilibrium to evaluate the significance of particular features of science. Next, I answer a set of problems that is associated with the method of RE.

4.6 The Minimal Method of Reflective Equilibrium in the Historiography of Science

RE has been criticized as a method. Here I discuss some of the main objections and answer them by arguing that, in the philosophy of historiography of science, we need only a very minimal version of RE.

We may begin by noting that the method can be said to be conservative in that it relies on pre-theoretical beliefs that we have now (Cath 2016, 221). What if our intuitions about what is significant about science are based on the wrong beliefs that are influenced by our contingent past? Discussing RE in ethics, Brandt writes:

> Our normative beliefs are strongly affected by the particular cultural tradition which nurtured us, and would be different if we had been in a learning situation with different parents, teachers, or peers. Moreover, the moral convictions of people derive, to use the words of Peter Singer, "from discarded religious systems, from warped views of sex and bodily functions, or from customs necessary to survival of the group in social and economic circumstances that now lie in distant past" [1974, 516]. (1979, 21).

By analogy, the problem in the philosophy of historiography would be that our *prima facie* acceptable judgements of significance are shaped by a tradition and thus cannot be trusted. For example, assume that individual geniuses do not, in fact, matter very much is science. We could still have, based on an erroneous tradition of thinking about individuals and their place in science, the idea that the life of this or that genius

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[76] For example, the experiments in CERN are more significant than the fact that I read a popular-science magazine on the origins of Finns even though neither would be possible without the epistemological aspects of science.

was a significant one. We thus make the wrong judgement because of the tradition in which we live.

To answer this worry, we may first note that it would be ironic if historians attempted to transcend their historical situation. Moreover, the idea that human beings are always shaped by historical traditions is arguably one of the greatest motivations to write history.

Secondly, and more seriously, we can hope that the results of historical studies direct our considerations of significance – the more we know about the history of science, the more enlightened judgements we are able to make about what is significant. For example, even if a historiography of science is written in reference to individual geniuses, nothing prevents these studies from concluding that this or that individual was not that significant after all. Moreover, as Cath (2016, 221) points out, it is built into the RE that we may sometimes need to abandon our initial beliefs.

Of course, one might wonder whether conservativism and the possibly false ideas of significance could lead, in the end, to an erroneous but coherent system of ideas about significance. However, this objection is much more general. Even if we are not conservatives and our initial beliefs are not based on some tradition, we might ask what guarantees that we achieve correct judgements of significance if our *prima facie* judgements are false for any reason. Moreover, we can ask what to do if different *prima facie* judgements lead to a disagreement in the end of RE. (See Cath 2016).My answer is that the principles and judgements of significance we arrive at with RE need not be part of our final[77] (and supposedly correct) position in order to be useful. The more we apply RE to our principles and judgements, the more these principles and judgements allow us to be coherent and understand each other's reasons. This can be explicated as follows:

[77] We, our culture and nature change all the time and thus it might not even be possible or desirable to achieve a final position with respect to principles and judgements of significance that do not change.

By coherence, I mean the possibility of accepting or criticizing historiographical choices that are made on the basis of certain principles. The RE can be seen as a method that reveals the value-commitments of the historians (and the cost of these commitments). Some feature of science might seem *prima facie* significant but a closer look could reveal, for example, that they lead to a historiography of science that is no longer connected in any way to the view of science as an epistemological practice. This would be an unacceptable cost in historiography of science (see the previous section) and the commitments of such historiography should be rejected. Moreover, coherence also means the possibility of taking someone's principles and applying them to study things that this person does not study. For example, if someone writes, due to a political agenda, only about the history of European scientists and justifies this (insincerely) with the principle *feature F is significant if it is connected to the accumulation of knowledge*, we could also study the history of non-European scientists using this principle. This would balance the politically biased history. In this way, the explication of principles of significance can at least prevent biased historiography, whether our principles are right or wrong in the end.

Moreover, mutual understanding is important in order to avoid disagreements that reflect only different (non-malevolent) functions that different historiographies serve. Suppose that somewhere science has led to an environmental catastrophe. In the society living in the middle of the catastrophe, historians might well accept the principle *F is significant only if it has the potential to cause an environmental catastrophe.* This would be reasonable: the environment is the first and only priority which the society has at that moment. However, another society, living in a healthy environment and practicing only, say, mathematical astronomy, would not accept this principle. However, this lucky society could understand the principle of the other society and thus the motivation of their historiographical works. There would be a disagreement but it would not be threatening.

If we allow that principles and judgements of significance can be used in a historiographical work even before the method of RE is followed until the very end, we must also accept the possibility that we sometimes use false principles and make erroneous judgements. However, everything said above about the problem of disagreement also applies to worry about errors that RE might generate. Explication of the principles and judgements used in a historiographical study, even if erroneous, is the only way to understand and then evaluate these principles and judgements. We cannot but accept that there might be disagreements and errors, but the hope is that these can be solved at some point in time, and before that we are able to voice some tentative justification. Moreover, even if there are always disagreements and errors, we can conjecture that the use of RE can diminish their number and severity. There might never be a completely correct (whatever this means) picture of significant features of science, but this does not mean that we cannot at least exclude the most erroneous lines of thought (based on dangerous politics, for example) that can hardly be accepted no matter how our considerations proceeds.

As a fact of life, there will probably always be disagreements about and errors in our principles and judgements of significance. We saw above that disagreements and errors are not automatically problematic. Moreover, it can be argued that, in a philosophy of historiography, disagreements and errors would not be as bad as they are in ethics. We would not do anything wrong (except writing misleading histories) on the basis of our errors and disagreements, we would simply have non-justified judgements of significance. Thus, we can accept more tentative justification in the questions of significance than in the question of ethics. This means that the critique of RE as a method in ethics does not imply that RE cannot be fruitfully used in historiography. Of course, one might now ask what the point of evaluating principles and judgments is, if disagreements and errors are not very serious problems. The answer, as we have seen, is that (i) the principles and judgements can be updated and corrected by RE, and (ii) RE helps us to make the principles and judgements transparent. Even if we can allow errors and disagreements (to avoid paralyzing historiography), it would still be preferable to avoid them as far as we can.

We can conclude that RE required in the philosophy of historiography is minimal: The judgements and principles of significance are required only to: (i) make the evaluative considerations of historians transparent, (ii) be acceptable by some reasonable group and (iii) help us to get rid of the lines of thought that are the least plausible. Perhaps we could connect (ii) and (iii) and say that judgements and principles of significance that are completely erroneous according to many groups can be rejected even if some groups accept these. These three conditions exclude the possibility that a historian can work on solely subjective grounds, the possibility of pushing hidden agendas on some seemingly acceptable basis, and the possibility that matters of significance cannot be discussed rationally. These are the main risks once we accept that historiography is a value-driven activity. Everything that RE accomplishes beyond this is a further achievement that deserves to be sought for. Only the actual practices can show how far we get, but I see no reason to think that these further achievements are only a utopia.

4.7 Significance and the Problem of Demarcation

It has been proven to be difficult to formulate the criteria that separate science from non-science. “[No] one agrees on whether there exists an adequate criterion of demarcation, that is, a reliable decision procedure for deciding whether something is a science (or, more modestly, genuinely scientific), and, if so, what that criterion is” (Nickles 2013, 101). However, this does not mean that the presentist historiography of science is a field built on an untenable conceptual framework even though it is the study of the causal histories of the present sciences. First, there seems to be broad agreement on which practices are science and which are not. The discussions about the demarcation problem is concerned with the criteria that are formulated to distinguish these types of practices from each other. (e.g. Hansson 2013, 61). Secondly, there have been interesting suggestions as to what separates science from non-science and it is plausible to assume that detailed enough criteria for the purposes of historiography of science can be given. These two observations help us to bypass the demarcation problem in historiography of science.

First of all, there are clear cases of science and non-science. In the historiography of the clear cases, there is no problem of demarcation for a historian. The historian can be satisfied with the fact that these cases are judged to be science. It is a philosophical problem what makes them science. Moreover, it does not seem to be a meaningful task for a historian of science to suggest that the practices that are considered as clear cases of science in our society are not *really* a science but something else is (this topic was discussed in Section 3.2).

Secondly, not much depends on the question of whether some borderline activity is considered as science or not. As we have noted, historiography of science should be about significant features of science. This means that the historians of science do not need to make judgements about whether some borderline activity is *really* science or not (and thus spent their time in old-fashioned philosophical reflection). As the historians focus only on the significant features of science, there is no need to write the history of everything in science. Given this, they should not worry too much about the scientific status of some borderline cases. The history of borderline cases can be studied as part of historiography of science only if these cases can be judged as significant. I conjecture that most of the possible borderline cases are such that even if they can be considered – in some sense – as science, they do not satisfy the criterion of significance. This is an intuitively plausible conjecture once we notice that the significance of science is mostly derivative from its nature as an epistemological practice. We can support (although not prove) this conjecture by considering a set of criteria that is suggested to separate science and non-science. It can be shown that these criteria are such that the line from science to non-science is parallel to the line from significance to insignificance. Thus, the features of the borderline cases do not have the same significance as the clear cases of science and historiography of science should not focus on the features of the borderline cases.

We can begin by noting that, even though Popper's falsifiability (see 1962), Kuhn's puzzle-solving (see 1974), or Lakatos's idea of progressive research programs (see 1970) are no longer considered as a final word on the problem of demarcation, they align well with the ideas of significance. The connecting idea behind these criteria is that science gives us new, informative, and useful knowledge. There might be some non-scientific practices that satisfy these criteria to some extent, but the ability to create new, informative, and useful knowledge on a regular and robust basis is clearly a sign of scientific status. Now, if we did not have these robust knowledge-creating practices, we would not know as much as we do nor would we be able to set attainable research agendas. This means that robust knowledge-creating is a significant feature of science. However, it is difficult to see how these epistemological achievements would suffer if we did not have the borderline practices that also produce knowledge but only in a feeble manner in comparison to the clear cases of science. This means that the borderline practices are not significant with respect to our epistemological situation. The line from science to non-science is parallel to the line from significance to insignificance if we accept the classical views on demarcation.

Secondly, and more interestingly, Hansson notes that

> Science (in the broad sense) is the practice that provides us with the most reliable (i.e., epistemically most warranted) statements that can be made, at the time being, on subject matter covered by the community of knowledge disciplines (i.e., on nature, ourselves as human beings, our societies, our physical constructions, and our thought constructions). (2013, 70).

What is interesting to us here is the idea that some practice can be scientific only if it is connected to other "knowledge disciplines". Scientific practices are dependent on each other, and if some field is isolated from other sciences then its scientific status is questionable. If scientific practices are closely connected to each other, then a difference in some of these practices would have a widespread impact on our epistemological situation in general. This suggests that changes in some science would have an interesting outcome. On the other hand, changes in the borderline cases between science and non-science would not have a widespread impact on our epistemological situation in general due to the relative isolation of these borderline practices. A change in the borderline practices would not lead to an interesting alternative but only to a local alternation. Once we remind ourselves that significant features of science are such that they have interesting alternatives, we can conclude that the ability to articulate, with the method of RE suggested in the previous sections, genuinely significant features of some practices indicate that these practices are scientific along the demarcation line suggested by Hansson.

Thirdly, Pigliucci writes:

> Presumably if there is anything we can all agree on about science, it is that science attempts to give an empirically based theoretical understanding of the world, so that a scientific theory has to have both empirical support [--] and internal coherence and logic [--]. (2013, 22)

Taking this suggestion seriously, we can evaluate how scientific some practices are by estimating how much empirical knowledge and theoretical understanding they give. Again, it is difficult to see how there could be any borderline practices that are significant. If a practice is poor in its empirical content and theoretical understanding, this kind of practice probably does not have much significance to us.

We see that only if there is a borderline practice that has a significant feature, the question of whether the history of this practice should be studied as a part of historiography of science would arise. According to the demarcation criteria suggested above, having significant features and clear scientific status go hand in hand. Moreover, it is difficult to see how such a borderline activity *could* have a significant feature. Assume (at least for the sake of argument) that astrology is a borderline practice between science and non-science. We could perhaps say that astrology is significant because it informs people in their decisions. The principle behind this judgement would be *F is significant if F helps people to make decisions.* However, this principle cannot be justified by reflecting on *prima facie* significant features of science in general. It is *prima facie* insignificant that scientists schedule meetings and thus help one another to make decisions when to arrive at a laboratory. We should say that *F is significant if it helps people to make non-arbitrary decisions with detailed information about the factors that affect the decision*. No feature of astrology satisfies this principle. The point here is that the borderline activities must be judged to have significant features on the basis of principles of significance that capture the significant features of science. Principles drawn independently of the reflection on the significant features of *science* are not relevant. This underlines how unlikely it is that the borderline activities have any significant features. We can conclude that *if we can establish that some practice has significant features,* *this indicates that the practice is also scientific.* It is, then, unlikely that borderline cases enter historiography of science from the backdoor once we are careful with our principles of significance.

This, admittedly limited, discussion about the demarcation problem indicates that at least most significant features are part of the practices that are clear cases of science. All the answers to the demarcation problem discussed above gives the sense that the further we move from the clear cases of science, the less significant the features of the practices are. This gives us confidence to hold that as long as only the significant features of science are discussed, there is no need to worry about whether this or that borderline practice should be studied as a part of historiography of *science*.

4.8 Conclusion

In the presentist approach, historiography of science is a practice that tracks down the causal history of the *significant* features of the present science. In this chapter, a process for deciding which features of science are significant was explicated. The process begins by judging that certain features are *prima facie* significant. Then we formulate principles that attempt to capture why those features are significant. As the process continues, we adjust both our judgments of significance of particular features of science and the contents of the set of principles that we accept. The target is to balance the set of principles and our *prima facie* judgments.

We noted that there are many aspects of significance and therefore we need many different principles to capture these aspects. For example, some features are significant because they are related to epistemological practices of science (such as the use of a mathematical model by some group of scientists) in which some are significant because they are related to our everyday life (such as nuclear waste). Yet, I argued that the common core that ties together all the different principles and judgements of significance is that they can be seen as connected to the nature of science as an *epistemological practice* even if they do not directly influence those practice. Science affects many aspects of our lives, but we should not forget the fact that even these seemingly non-epistemological implications, such as the powerlessness to affect the tobacco industry, are a result stemming from the epistemological practices of science.

At a more technical level (connecting to the argumentative structure of this book), this chapter has two implications:

1) We do not need a sharp demarcation between science and non-science. Even if there are borderline cases of activities which are difficult to categorize as science or as non-science, this does not make the presentist approach, with its focus on the history of "what is considered as science in our society" an ambiguous one by allowing historians to focus on whatever someone might consider as science (i.e. on the borderline cases). The ambiguity is solved by focusing on the significant features of science: I argued that there are good reasons to think that not too many borderline-cases would be judged to have significant feature once we have a well-established set of principles of significance. We can remain silent about the question of whether the borderline cases are *really* science and expect that the principles of significance will enable us to ignore those borderline cases.

2) Our principles of significance should be such that they tell us what alternative F* to an actual feature F of science would make science interestingly different. Explications of such contrasts is a necessary pre-condition for causal explanations in historiography of science once we build our theory of explanation on the notions of counterfactual scenarios and contrastive question-answer pairs. Moreover, the contrasts between actual features and interesting alternatives to these features become especially fruitful when we attempt to answer questions of contingency and the inevitability of science. In the chapters to come, I focus on these issues.

5 THE NEED FOR A GENERAL ACCOUNT OF EXPLANATION IN HISTORIOGRAPHY OF SCIENCE

In this chapter, before formulating an account for explanations in historiography of science in the next chapter, I discuss some central topics concerning such explanations. My strategy is to point out that there has been much explanatory talk in the historiography of science without any reference to the theories of explanation, and that this has led to many confusing claims, unnecessary restrictions, and unjustified distinctions. The literature surrounding the topic is enormous and I cannot do justice to its complexity here. Rather, my strategy is to highlight some relevant dimensions from the previous discussions in order to locate my own position in the field. Once these dimensions are made explicit, we are able to understand what kinds of questions a framework for explanations in historiography of science must address. Moreover, the discussion in this chapter points out the need for a completely general framework that does not exclude *a priori* any of the existing perspectives on the explanations in historiography of science but rather enables us to appreciate the richness in the variety and, more important, see the strengths and limitations of those perspectives.

The discussion begins with Thomas Kuhn's account of scientific development. I point out four possible perspectives on the history of science in Kuhn's account. I say "possible" because I do not claim that my reading captures what Kuhn really thought. However, the perspectives stem directly from passages in Kuhn's work and, more important, they touch issues that have been debated heatedly and that are directly related to the nature of explanations in historiography of science. These debates, rather than producing a fresh or detailed reading of Kuhn, are my main concern as they show how loose explanatory-talk has muddied the waters in the philosophy of historiography of science. My aim is to show that the four perspectives in Kuhn's account reveal four associated families of philosophical and historiographical issues: (I) the scale of a historiographical explanation; (II) the aspect of science that the historiography focuses on (i.e. the nature of the *explanandum*); (III) the role of notions of reasons and rationality in historiography of science; and (IV) the role of causal explanations in historiography of science. To widen our understanding of these four families, I discuss (a) some existing trends in the current historiography of science, (b) historical philosophers of science, Lakatos and Laudan, and (c) the sociology of scientific knowledge. That discussion underlines alternative perspectives to those of Kuhn (or perhaps different *readings* of Kuhn and their derivatives). My aim cannot be to tell who was right and who was wrong; rather I will point out (i) valuable insights that should be appreciated and (ii) damaging shortcomings that must be avoided when we build an account of explanations in historiography of science.

I have chosen these four families of issues because they show how many unnecessary restrictions, irrelevant distinctions, and even confusions have been built into the philosophical reflections on historiography of science. The topics are also of the utmost importance when we attempt to understand the nature of explanations in historiography of science. The nature of relevant *explananda*[78] and *explanantia*[79] in the field are the most fundamental questions we might address in this context and the issues (I)–(IV) are directly related to those questions.

To make the connection between this and the next chapter more understandable, we should note that the account of explanations formulated in the next chapter

> (1) is completely indifferent to the nature and scope of the thing to be explained;
>
> (2) can be used independently of assumptions about the rationality in/of science;
>
> (3) is able to accommodate many kinds of factors as explanatory

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[78] Things (features) to be explained.

[79] Things (factors) that explain.

(4) defines two notions of explanatory relevance: one related to the explanatory depth and other related to explanatory goals;

(5) explains how the explanatory resources should be managed.

In this chapter, I explain why such a general account is needed. I began by shortly introducing all four perspectives and then I discuss each perspective and the associated family of philosophical issues in more detail.

5.1 Perspectives from Kuhn on the Nature of *Explananda* and *Explanantia*

Kuhn's *Structure of Scientific Revolutions* (1970 [1st edition 1962]) had an enormous impact on the historiography and philosophy of science. In Kuhn's work, there are many themes that are highly relevant to the philosophical reflections on explanations in historiography of science. It therefore serves as a valuable starting point for our discussion about the topic.

According to Kuhn, there are (mainly) two kinds of periods in the development of science: *normal science* and *revolutionary science*. A normal science period is a one in which a paradigm defines the research in a scientific field. A paradigm is a "universally recognized scientific achievement that for a time provides model problems and solutions to a community of practitioners" (Kuhn 1970, viii). A paradigm, then, is the condition under which science can develop in a steady fashion. Revolutionary science, on the other hand, is a period in which an existing paradigm is challenged due to its inability to solve certain problems and a new paradigm is established. Different paradigms are mutually incommensurable, as there are no shared standards that enable scientists to choose between competing paradigms in the period of revolutionary science. Kuhn makes the point dramatically: "the proponents of competing paradigms practice their trades in different worlds" (1970, 150). It is understandable, then, why a change of paradigm constitutes a scientific revolution.

Perspective 1: Macro-level picture

Kuhn's account attempts to establish a macro-perspective on science (Golinski 1988, 14). The development of science is seen as a cyclical process where two types of periods – normal and revolutionary – alternate. To give just some idea of the temporal scales in Kuhn's account, a normal science may continue over many generations: "many [--] works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners" (Kuhn 1970, 10). It is important is to note that Kuhn seems to think that very general structures (the workings of normal science) and wide-ranging changes (revolutions) deserve the greatest attention in historiography of science – or at least they form the basic lines of research. As we will see later in this chapter, this perspective was soon replaced by its exact opposite in historiography of science.

Perspective 2: Contents of science as explananda

The main target of Kuhn's account is to explain how certain ways of looking at the world become dominant (in the form of paradigms) in science. This means that when it comes to *explananda* the focus is on the contents of science. Kuhn writes:

> [We] shall deal repeatedly with the major turning points in scientific development associated with the names of Copernicus, Newton, Lavoisier, and Einstein. More clearly than most other episodes in the history of at least the physical sciences, these display what all scientific revolutions are about. Each of them necessitated the community's rejection of one time-honored *scientific theory* in favor of another incompatible with it. Each produced a consequent shift in *the problems available* for scientific scrutiny and in *the standards* by which the profession determined what should count as *an admissible problem* or as *a legitimate problem-solution*. (1970, 6 [emphasis added]).

When it is said here that Kuhn's focus is on the contents of science, this is meant in the wide sense as including not only facts (Kuhn 1970, 25) and theoretical claims (or "symbolic generalization") but also metaphysical commitments, values and standards, and exemplars (e.g. Nickles 2003, 3). It is important to add *problems* to this list, as science is dedicated to solving problems (see the citation above). This means that *the contents of science* refers not only to explicit contents that are communicated through more or less official channels, but also to a more general framework of views, "a disciplinary matrix", that governs science.

Of course, the notion of *paradigm* already points toward the social conditions of science. However, it seems that social aspects are discussed in Kuhn's work only because those aspects are closely related to the contents of science. Throughout the book, the main *explananda* in Kuhn's account are contents of science, and the social structures are discussed only in order to understand how the contents of science develop. In other words, when Kuhn sheds light on factors besides the contents of science, these factors are presented as *explanantia*. Moreover, there is hardly any discussion about the connections between science and other human practices. Kuhn did not seem to think that such connections are relevant, as he even mentions what he sees as "the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life" (1970, 164). Later in this chapter we will see that, while some philosophical debates about historiography of science shared this perspective, the actual historiography of science did not.

Perspective 3: Limits of rationality and the need for "external factors"

Kuhn's account of the development of science is based on the idea that only a normal science is a clear rule-driven activity where a community of scientists, sharing a paradigm, solves problems and evaluates the status of those solutions. When science falls into a crisis (when the existing paradigm is challenged), there are no shared standards left to evaluate how to proceed – no shared premises or values (Kuhn 1970, 94). If we take rationality – in the strict sense - to consist of such rule-driven activity[80], then there are developments of science that cannot be understood in terms of its rationality. Something besides rationality-based accounts is needed.

For a moment (when discussing Kuhn), we can use the term "external factor" to refer to any factor that affects the development of science without belonging to the

[80] This has been questioned, even by Kuhn himself (1977).

rational processes governed by a shared paradigm. We can divide these external factors into two categories.[81] The first consists of *reasons* that are not based on a paradigm. Kuhn writes:

> Individual scientists embrace a new paradigm for all sorts of reasons and usually for several at once. Some of these reasons—for example, the sun worship that helped make Kepler a Copernican—lie outside the apparent sphere of science entirely. Others must depend upon idiosyncrasies of autobiography and personality. Even the nationality or the prior reputation of the innovator and his teachers can sometimes play a significant role. (1970, 152-153).

The second group consists of causes. This point deserves its own treatment and we will discuss it next. However, we can conclude the discussion here by pointing out that a dualistic picture of explanation, often cast in terms of external vs. internal factors, is already present in Kuhn's account.[82] That dualism, as we will see later, casts its shadow on many philosophical debates concerning historiography of science.

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[81] Someone might argue that reasons should be identified with internal factors, and causes with external factors. Such an argument does not succeed. First, the above citation from Kuhn (1970, 152-153) shows that Kuhn did not think so. Secondly, in Lakatos's philosophy (1978), rival methodologies make the distinction between internal and external differently and therefore what is internal with respect to one methodology might be external with respect to another methodology. If internal factors are reasons, then some reasons can be external factors, depending on the methodology. In principle, no identification of reasons with internal factors can be made in Lakatos's philosophy. These are only two examples, but the identifications are doomed once we realize that reasons are present in any area of human life. If any such area affects science, then some reasons have an external influence on science. As Shapin writes: "More fundamentally, the division of the e/I explanatory world into the cognitive (science) and the non-cognitive (society) has never been systematically defended. Nor can one imagine a plausible reflective defence against the observation that both "society" and "science" (naturally construed) are systems of cognizing agents, collectively arrayed, doing thing on the basis of what they know." (1992, 349.) I will abandon the distinction between reasons and causes and between internal and external below, but the impossibility of identifying reasons with internal factors must be noted as long as we use the traditional terminology before abandoning it altogether.

[82] The internal vs. external debate has never been unambiguously defined despite the widespread use of the terms (see Shapin 1992.) I use the distinction as a hermeneutic tool that helps the reader

Perspective 4: Causal explanations

There are hints in Kuhn's work that causal explanations are regularly needed when the developments of science are explicated. Even if the periods of normal science can be understood *mainly* in reference to the paradigm-bound rationality, Kuhn suggests that even then there are causal factors that affect science. Kuhn writes: "An apparently arbitrary element, compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time." (1970, 4). However, whether or not normal science needs to be sometimes explained causally does not matter much. The crucial point is that a scientific revolution can be explained causally, at least to some extent. There are explicit statements where Kuhn endorses the possibility of a causal explanation. He writes:

> One need [--] look no further than Copernicus and the calendar to discover that external *conditions* may help to transform a mere anomaly into a source of acute crisis. The same example would illustrate the way in which conditions outside the sciences may *influence* the range of alternatives available to the man who seeks to end a crisis by proposing one or another revolutionary reform. (1970, x [emphasis added]).

Moreover, a search for causal explanations is seen as a valuable way of analyzing science:

> Explicit consideration of effects [of the conditions outside science] would not, I think, modify the main theses developed in this essay, but it would surely add an analytic dimension of first-rate importance for the understanding of scientific advance. (Kuhn 1970, x).

These passages show that the idea that causal explanations are needed in historiography of science is already present in Kuhn's work. However, the exact workings and the role of such explanations were left unclear and, as we will see later in this chapter, this lack of clarity has hampered the philosophy of historiography of science ever since.

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to understand what my account *is not* committed to. The possibility that there never existed a well-defined internalism vs. externalism distinction is therefore not a problem for my account as the account does not build on that distinction but abandons it.

Now that we have extracted the perspectives, we need to widen our understanding about the issues generated by those perspectives.

5.2 Macro and Micro Historiography of Science

We have seen that Kuhn's account provides a macro-perspective on the history of science. It discusses long periods and very general themes: how a normal science proceeds under a paradigm, how a paradigm is challenged and a new one is established. The account also seeks to establish repeating patterns of development in science and "it purports to draw general lessons from the unfolding of our collective cognitive development" (Fuller 1992, 272).

It is remarkable how dramatically the historiography of science has moved away from such macro-perspectives. In 1992, Steve Fuller observed that Kuhn's *Structure* was the last book written from such perspective. Cunningham and Williams, while arguing for the necessity and desirability of such perspectives, also confirm the decline of macro-perspective or "big pictures": "Big pictures are, of course, thoroughly out of fashion at the moment; those committed to specialist research find them simplistic and insufficiently complex and nuanced, while postmodernists regard them as simply impossible" (1993, 407). The trend has continued, as everyone can observe by going through "Current Bibliography of the History of Science and Its Cultural Influences 2016" (*Isis* 107). More recently, explicit discussions about the status of the macro-perspective are found in *Isis* 96 (2) (2005). The discussion was raised by the observation that "[the] ideal of a general history of science seems gradually to have waned" (Kohler 2005, 204). Moreover, a section in *Isis 107* (2) (2016) discusses the booklet *History Manifesto* (2014) from the perspective of historiography of science. This booklet by Jo Guldi and David Armitage argues that short-termism has "killed" the relevance of historiography (2014, 11). The need to discuss such a general thesis about historiography in *Isis* indicates that the move from macro-perspective to micro-perspective has remained an acute concern in the historiography of science.

However, we should not overly dramatize the nature of the move toward micro-histories. There have been many good reasons – for example, the problems in the science-in-the-past view (see Chapter 2) – to be suspicious toward macro-perspectives. Moreover, microhistory has offered extremely valuable insights into the workings of science. Therefore, I do not think that there is any *a priori* reason to be concerned about the marginalization of macro-historiography of science. Yet there is no more *a priori* reason to think that there does not exist any interesting questions that need macro-historical answers. Whether or not we need micro- and/or macro-perspective depends on the questions we need answers to. We have seen (in Chapter 4) that historiography of science should explain significant features of science. We also saw that a significant feature has the canonical form

Subject S does P in location L at time T.

L and T are not limited in any particular way. This means that it is possible that some significant features require explanations from macro-perspective. Whether or not this is the case is not dependent on the value of macro-perspective in itself. It depends solely on what principles and judgements of significance we accept and what questions we ask on those basis. This is in accordance with reactions in the literature concerning the issue of macro-perspectives: Shapin notes that "[the] problem is not the scale of what we write about but our interest in writing about our subjects and the connections we make as we write about them" (2005, 242), and Gaukroger notes that

> [the] difference between what might be termed micro-history/short-termism and *longue durée* history lies not so much in the length of the period studied but, rather, in the kinds of questions asked and the resources needed to answer them. Some questions, when properly formulated, require a very different kind of approach, as well as different resources, from others. (2016, 340).

The choice between micro and macro-perspective cannot be based on the intrinsic merits of these perspectives but on the questions that the historians find interesting to answer.

It must be noted that if it really is the case that "those committed to specialist research find [macro-history] simplistic and insufficiently complex and nuanced"

(Cunningham & Williams 1993, 407), then macro-history might very well be at an epistemological impasse. If there are interesting questions that require a macro-historical answer and if macro-history is impossible, then there are interesting questions that cannot be answered. This would be a sorry state of affairs. However, we should remain aware of the fact that even if some questions cannot be answered, the question might still be important. Otherwise we lose our epistemological modesty. The framework developed in Chapter 4 enables one to discover those questions that deserve an answer. I leave it to the historians to decide which questions can be answered. Yet I would be surprised if it turned out that some questions are unanswerable only because they require answers from macro-perspective, given the heterogeneity of questions that our principles and judgements of significance allow us to ask (see Chapter 4) and the associated heterogeneity in the possible ways to answer such question.[83]

To sum up: According to the framework of this book, there is no need to decide whether historiography of science should use macro-perspective or micro-perspective or both. This is a false dichotomy. The question can be reduced to the questions about significance. Adopting the framework of this book, both perspectives are possible and, what is more, we can explicate an independent rationale for choosing between the perspectives case-by-case.

5.3 Moving Beyond the Contents of Science

Kuhn's *explananda* were the contents of science in a wide sense (see above). We saw that when he discusses the development of science changes in facts, theories, metaphysical commitments, values and standards, exemplars, and problems are to be explained. It is possible to go beyond Kuhn's account and add, for example, causal models and data sets to the list of the contents of science.

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[83] An analogy: from the fact that we cannot know anything about some animals that lived 200 million years ago does not imply that we cannot know anything about any animal. That those things were *animals* does not constitute the main epistemological obstacle.

It might be useful if we did not associate the contents with the conscious cognition of human beings.[84] We could make a (admittedly rough) distinction between the contents that appear in someone's conscious cognition and the contents that do not appear so. It is not difficult to think that a data set on a computer, developed automatically from a given input, would count as a content of science even if no one ever checks the data. Moreover, the deductive consequences of a theory seem to belong to the contents of science even if no one has ever made the deductions – otherwise it would be impossible to reach a situation where an inconsistency was *discovered*. This means that the contents do not need to be explicitly formulated or cognized.

Kuhn took the contents as the *explananda* whereas other dimensions of science were among the *explanantia* in his account of development of science. This is a crucial distinction since explanations provide direct understanding only about *explananda*.[85] It is remarkable to notice that such a content-centered view – even on the *explanandum*-side – is not dominant in the existing historiography of science. Multiple aspects in addition to the contents of science have been viewed as worthy *explananda* in themselves. The attempt is not to understand science *through them* but to understand those dimensions themselves. In this book, I have built on this observation and argue that there is much more to be explained in science than its contents (no matter how broadly *contents* is understood). I cannot give a catalogue of topics that are not directly related to the contents of science here, but I hope that one example is enough: Brianna Rego discusses in the study "The Polonium Brief: A Hidden History of Cancer, Radiation, and the Tobacco Industry" (2009) the relationship between scientific research and the actions of the tobacco industry. Rego argues that:

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[84] Kuhn's rhetoric (1970, 72 & 175) points to this direction.

[85] We might say that we gain a first-order understanding by explaining *explanandum* M by *explanans* E, and second-order understanding by noting that E is explanatory. In Sections 6.4 and 6.6, I discuss how the choice of *explanans* might provide such a second-order understanding in two different ways.

> While external scientists worked to determine whether polonium could be a cause of lung cancer, industry scientists silently pursued similar work with the goal of protecting business interests should the polonium problem ever become public. Despite forty years of research suggesting that polonium is a leading carcinogen in tobacco, the manufacturers have not made a definitive move to reduce the concentration of radioactive isotopes in cigarettes. (2009, 453).

This seems like an explication of a *prima facie* significant feature of science, "the inability of science to affect harmful industry". Yet, this issue has only indirect connection to the contents of science. To exclude this kind of study from the historiography of science would show the incompleteness of a philosophy of historiography of science.

However, there is a complication in this innocent-looking issue. It has been suggested that explanations of beliefs of scientists deserve distinct philosophical attention. Here I refer to The Sociology of Scientific Knowledge (SSK). David Bloor writes:

> The sociologist is concerned with knowledge, including scientific knowledge, purely as a natural phenomenon. [--] [Knowledge] consists of those beliefs which people confidently hold to and live by. In particular the sociologist will be concerned with beliefs which are taken for granted or institutionalised, or invested with authority by groups of people. (1991, 5).

We can for a moment set aside the issue of whether sociology has the explanatory force that SSK argues it has (I return to this topic multiple times in the rest of the book). The remarkable thing here is that it was suggested that scientists' beliefs are important *explananda* if we want to understand science – even if the perspective on science comes from sociology. This focus on beliefs was also adopted by Larry Laudan who strongly opposed SSK. According to Laudan, both SSK and the "intellectual historians" are trying to explain "the belief of some historical agent" (1977, 193). There seems to exist a shared and discipline-independent idea that the beliefs of scientists deserve special explanatory attention.

Of course, advocates of SSK did not claim that the beliefs are the only things that need to be explained. A special focus on beliefs was brought up because no one has ever doubted that other aspects of science, the non-cognitive ones, can be explained by sociology. (Bloor 1991, 3; see also Laudan 1977, Chapter 7). However, the mere existence of a distinct debate concerning the explanations of beliefs indicates that beliefs are taken as a particularly important case within historiography of science. Moreover, we will see in the next section, 5.5, that David Bloor gave four tenets that outlined how beliefs are to be explained. *Here we face another dichotomy, one that exists between cognitive and non-cognitive features of science.*

The problem is that, while everyone seems to agree that many aspects of science deserve explanations, beliefs were taken as the only philosophically problematic *explananda*. For example, while formulating his tenets, Bloor suggested that the tenets are based on "the values which are taken for granted in other disciplines "(1991, 7). The tenets are an attempt to extend certain features of explanations in other disciplines to the explanations of beliefs. It seems that Bloor attempted to downplay the distinction between cognitive and non-cognitive features of science. However, his approach does not achieve this unambiguously. In order to derive an account of explanation of beliefs from other disciplines, one must understand how the explanations in other disciplines work. As we will see in Section 5.5, this makes Bloor's tenets extremely difficult to interpret. There are many possible ways to interpret the tenets, only some of which are compatible with the nature of explanations in other disciplines. If one suggests that explanations of beliefs must follow the same strategies as explanations in other fields, then vague conceptions of the strategies of explanation in other fields lead to a situation where explanations of beliefs *in fact* follow different strategies. The distinction between cognitive and non-cognitive remains, but only unnoticed this time. Moreover, Bloor also implicitly preserves the distinction between beliefs and the rest of science as he attempts to apply principles formulated in the tenets within the category of beliefs, whereas those principles can be applied to beliefs only if beliefs are included in a much wider category where the principles already hold (see Section 5.5).

I suggest that we should be more modest and problematize also the nature of explanation of non-cognitive features of science. As we will see in the rest of the book, the nature of explanation is never a matter that can be trivially explicated. Only by accepting such modesty are we able to seek a general account of explanations that captures how both cognitive and non-cognitive features of science can be explained. I take the fact that the explanations of beliefs have seemed obscure as evidence about the insufficiency of the (sometimes intuitive) accounts of explanations that attempt to capture such explanations. It seems mysterious how we could have at the same time a plausible and fully formulated account of explanation as well as difficulties in understanding how beliefs should be explained (see Section 5.5 for such ambiguous suggestions).[86] Moreover, we should not expect that some principles that hold within the category of explanations in general hold also within the subset of explanations of beliefs. Therefore an account of explanations of beliefs cannot be explicated after a more general account has been explicated. Rather, the general account must incorporate beliefs from the beginning.

To sum up, the discussion in this section indicates that the historiography can and should explain many aspects of science from the beliefs and contents to the wide connections that science has with other aspects of our lives. We have seen that in the presentist approach, historiography of science explains significant features of science. In Chapter 4 we saw that different kinds of features of science can be significant. Therefore, the framework of this book is well suited to capture the variety of topics on which the historians of science can focus on. Again, the issue is not whether some topic is about beliefs, contents, or something else. The only thing that matters is the significance of the topic. Moreover, we should not expect that only belief-explanations are philosophically problematic. If explanations of beliefs seem difficult to understand, we have a much deeper problem in our account of explanation. The traditional distinction between cognitive and non-cognitive should not be allowed to put any weight on us. In order to build a general account of explanations, we must downplay the importance of this traditional distinction.

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[86] If we never achieve an account that can explain both cognitive and non-cognitive features of science, then we should perhaps admit special status for beliefs. This would be an extremely interesting result. However, such a result can be achieved only by formulating different accounts of explanation (as I formulate one in this book) and refuting them. This is the ultimate reason why I think that the distinction between cognitive and non-cognitive is unjustified at the moment and should not be built into the philosophy of historiography of science. (See also Section 5.1 *Perspective 3: Limits of rationality and the need for "external factors"*.)

5.4 Rationality in Historiography of Science

My discussion concerning the perspectives on the role of rationality is mainly negative. In this book, I do not discuss the theories of rationality and the question whether science can or should be seen as rational. One reason for such an omission is that the account of explanation formulated is that this book does not focus exclusively on explaining the contents of science, as we saw in the previous section. It is an account that can be used to explain whatever is taken to be a significant feature of science. Therefore, the account must be much more general than those built around rationality (see below). Moreover, it is a "monistic" account where every explanation has the same counterfactual and contrastive form; explanations track down patterns of counterfactual dependencies. The account does not include categorical distinctions between things that can be explained by rationality or reasons and things that are explained in some other way. The generality and the monistic nature make the separate category of rationality-based (or, more generally, reason-based) explanations redundant. This does not mean that the account implies that science is not rational. Rather, the account makes no assumptions about the issue.

Notice that what was said above goes against some well-known views. For example, Robert K. Merton argues that "Specific discoveries and inventions belong to the internal history of science and are largely independent of factors other than the purely scientific" (Merton 1938, 75). Moreover, Imre Lakatos argues that we should first attempt to "rationally reconstruct" the history of science as far as possible and only after such reconstruction use "external factors" to explain what could not be fitted into the rational reconstruction (1978, 102). Finally, Larry Laudan argues that "application of cognitive sociology to historical cases must await the prior results of application of methods of intellectual history to those cases" (1977, 208 [emphasis removed]). All these positions imply an explanatory dualism between rationality-based and other kinds of explanations.

As already hinted in Chapter 3 (and as is fully explained in the next chapter), the account of historiographical explanations developed in this book is based on counterfactuals of the form "Had W rather than Z, Y rather than X would have happened". I take it that such counterfactual can also cite reasons or beliefs and therefore my account can incorporate explanations that cite reasons and beliefs. Moreover, we can follow rational, as well as non-rational, reasoning (no matter how the notion of rationality is understood) of some historical actor step-by-step and tell how the agent's conclusion would have been different, had a different step been taken. Therefore, my account is completely compatible with the reason-based (rational as well as non-rational) explanations.

Consider the following passage (Krige 2001, 537). It describes the final steps before it was claimed that a new fundamental particle (the W boson) had been discovered at CERN:

> According to Schopper's account, then, two factors [sic] left the CERN directorate little option but to place their faith in Rubbia's results and claim that the W had been discovered. One was intrinsic to the internal politics and institutional logic of a contemporary high-energy physics laboratory like CERN. The Stanford physicist Stanley Wojcicki has described it well: "UA1 decided to go public and publish–that certainly was their decision alone–and once that was done the news was in the public domain. In light of its importance it would have been very unusual for CERN to officially ignore this fact and not have a press conference." The second factor was Schopper's promise to Thatcher and the need to avoid doing anything that might offend one of the major contributors to the CERN budget. When the laboratory was established in the early 1950s the British government had had grave doubts about its participation in the venture; in the late 1960s it had initially refused to participate in the construction of the SPS. Now the Conservative government was hesitating about its engagement in CERN's next big machine, the LEP, plans for which had been adopted in principle by the member states a few months before. The discovery of the W helped dispel any remaining doubts about the quality of the physics being done at CERN and hastened the final agreement to finance the electron-positron collider.

Krige's study, taken as a whole, discusses many types of factor that led to the claim that the W boson was found (see Sections 3.1 and 7.2). Here it is obvious that reasons were an essential part of the unfolding of the process. There are no distinctions between causes and reasons; both are parts of the same narrative. Even the exact term "factor" is used in the passage. Moreover, it is quite natural to interpret the claims here as having implicit counterfactual form: "Had the subject matter been less important, CERN could have ignored the maneuver of UA1 team"; "Had there not been a need to be cautious with major contributors, different decision could have been made".

What needs to be taken seriously is Lakatos's view that we need some way to compare different explanatory frameworks. Lakatos thought that this problem concerns only the choice of a framework of rationality. (Lakatos 1978; see also Kuukkanen 2017). However, as historiography of science explains the features of science that belong to different kinds, not just the contents of science, the discipline also needs to incorporate multiple kinds of factors – not just reasons and rationality – to its explanatory resources (i.e. the kinds of factors that can be cited in the explanations). Therefore, the choice between explanatory frameworks is a much more complex task than a Lakatosian comparison of the theories of scientific rationality. I discuss the issue of explanatory resources in Section 6.6. The outcome of that discussion is that our explanatory resources should be balanced between power and simplicity; whether or not some notion of *rationality* is a part of such resources is a matter that can be decided only after we have studied the history of science.

Conclusion: I do not distinguish reason- or rationality-based explanations from other kinds of explanations nor do I assume that we need to have a theory of rationality before studying the developments in science. An account of rationality might *emerge* from historiographical studies if they are conducted in accordance with the framework of this book.[87] This means that the framework developed in this book is independent of any particular theory of rationality in science and even from the assumption that scientists are rational. In this sense, it is suitable for scholars with different assumptions. However, it carries a promise of convergence of opinions about the rationality of science. Again, the framework shows its ability to unify many different perspectives on science and to avoid unnecessary distinctions.

[87] Note that I am not suggesting that we test philosophical theories against a history of science (see Section 3.6). The emergence of a theory of scientific rationality would be more a fortunate side-effect of my framework rather than an explicit goal.

5.5 Causal Explanations

We have seen that Merton, Kuhn, Lakatos and Laudan all argued that "external factors" – something outside the domain of scientific reasoning – are, at least sometimes, needed to explain scientific developments. However, what counts as an external factor varies between the accounts. For Merton, such things as military, economic, and technical influences counted as external factors (1938, 557). For Kuhn, everything outside paradigm-bound processes can be counted as an external factor – even reasons if they are not embedded in a paradigm. For Lakatos and Laudan, external factors are whatever is needed to fill the explanatory gaps in the most successful rational account (although Lakatos and Laudan seem to think that sociology or "socio-psychology" are the disciplines that will fill the gaps (Laudan 1977, 202; Lakatos 1978, 102)). This indicates that there is a wide variety of explanatory factors that might be needed in historiography of science, even under the assumption that internal and external history can be distinguished. External factors might include anything from reasons to social structures to technology.

This variety of external factors - not to mention their historical changeability (Shapin 1992, 351) - already points toward the artificiality of the distinction between internal and external factors. The artificiality becomes even clearer when we remember that there is much more to be explained in historiography of science than the contents of science. If we were to stick to the category of internal explanations, these explanations would explain only one kind of *explanandum* (the contents of science) and they would do so in an incomplete manner (as we saw, everyone cited above thinks that there would be an explanatory use for external factors). Moreover, we saw in the previous section that the explanations citing reasons and rationality can be subsumed under a wider category of explanations that is based on counterfactuals. This means that internal explanations (as rationality/reason-based explanations) do not have a distinctive explanatory structure and therefore internal explanations do not deserve a distinct category due to their explanatory structure. For these reasons, I will abandon the distinction between external and internal explanations as an analytical tool.[88] We need much more general tools to understand historiographical explanations.

In the next chapter, I will discuss the nature of causal explanations in historiography of science. This account is based on a counterfactual account of explanation, where explanations have the form "Had W rather than Z happened, Y rather than X would have happened". If one feels uneasy about calling reason-citing explanations "causal explanations", one may simply forget the terminology used. The terminology does not matter much. The terminology is mainly based on the origins of the account (Woodward 2003). However, I prefer to speak of causal explanations, as one of the main target of this book is to clarify the structure and role of causal explanations (in the sense of not including reason-based explanations) in historiography of science – and since I discuss causal explanations anyway and since I see all the explanations as having the same form, I use the term "causal explanation" to include all the explanations sharing that form. This is simply a matter of convenience, and it has no antiquated implications that it might be thought to have – that there are strict laws of nature, similar to those in physics, that govern human actions, etc. As we will see in the next chapter, the account of causal explanation formulated in this book carries no such metaphysical commitments – it is a methodological tool.

An account of causal explanations that explicates many important notions related to such explanations has an intrinsic philosophical value. However, it is even more valuable once we notice how often there has been explanatory talk in the historiography of science without any reference to the theories of explanation. This situation is, of course, familiar everywhere in human life. The problem becomes pressing once we notice that loosely formulated explanatory claims in the historiography of science are sometimes seen as having extreme power to change our views on the nature of science. This makes them worthy of special attention and systematic treatment.

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88 Now historians of science do not need to "wish away" the distinction (Shapin 1992, 334), here are the systematic philosophical reasons to abandon it.

Moreover, the lack of a general account of explanations is not only a problem for the philosophical debates about the nature of science. Without such an account, it is difficult to interpret the nature and the plausibility of explanatory claims made in the historiography of science. Consider the following claims:

> If we believe, as most of us believe, that Millikan basically got it right, it will follow that we also believe that electrons, as part of the world Millikan described, did play a causal role in making him believe in, and talk about, electrons. But then we have to remember that (on such a scenario) electrons will *also* have played their part in making sure that Millikan's contemporary and opponent, Felix Ehrenhaft, *didn't* believe in electrons. Once we realize this, then there is a sense in which the electron 'itself' drops out of the story because it is a common factor behind two different responses, and it is the cause of the difference that interests us. (Bloor 1999, 93.)

> [Science's] social, political, and religious respectability depended on the governance of imagination by consistently patient and humble behavior. (Bellon 2011, 396.)

> Distrust demanded independent replication; it also influenced the way in which the CERN Director-general managed the credibility of the results for the world's press, turning a plausible but not yet widely accepted hypothesis into an undisputed fact. (Krige 2001, 517.)

> [The] larger significance of the expedition, both contemporaneously and in the present, was largely the result of Eddington's contextual concerns. (Stanley 2003, 58.)

> British provincial urban cultural renewal and industrialization were important factors in the emergence of a distinctive developmental worldview. (Elliot 2003, 3).

It is all but clear (i) what these claims exactly mean, (ii) what their value and implications are, and (iii) how these claims can be warranted. They are explanatory claims, but as long as the dimensions (i)-(iii) remain unclear, our understanding is hampered, even confused. In later parts of this book, I will show how we can clarify such claims once we have a general account of explanations in historiography of science.

To understand the state of the lack of clarity, we may take a look at David Bloor's four tenets of the sociology of scientific knowledge (SSK). The debates concerning these tenets illustrate the obscurity of causal thinking related to historiography of science: Both the tenets and their criticisms share similar deficiencies in their assumptions on how explanations work and how an account of explanations can be built.

The tenets of SSK are the following:

> 1. It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.
>
> 2. It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.
>
> 3. It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.
>
> 4. It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need to seek for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories. (Bloor 1991, 7)

It is somewhat difficult to interpret these rather brief passages on explanations of beliefs. Different readings open different sets of issues concerning such explanations.

The main problem in **Tenet (1)** is that it leaves open what exactly is meant by "causal explanation". It is easy (even trivial) to accept the tenet once we establish a general account of causal explanation that is able to capture the variety of factors that need to be used to explain heterogeneous features of science (see Section 5.3). The account developed in this book is compatible with the idea that many types of causes are cited in historiography of science.

However, if one has a less general account of explanation that has built-in metaphysical commitments about what kinds of things are explanatory, the tenet (1) makes substantial claims about science. However, it seems difficult to understand how the explanatory force of certain types of factors in the history of scientific beliefs could be established before that history is scrutinized. The idea that beliefs require causal explanations does not mean that they can be explained by social factors. Therefore, if SSK is to succeed, it must be built on a non-committed notion of causal explanations and use that account to show the alleged central role of social factors. In fact, any account of causal explanation can be a successful historiographical tool only if it does not have built-in metaphysical commitments. Which types of causes are explanatory is an empirical matter. (I return to this point soon.)

The problem that the tenet (1) does not clarify what is meant by "causal explanation" is something that the critics of SSK share, albeit in a more sophisticated form.

Laudan criticizes the prospects of SSK by making claims about the nature of causal explanations:

> (I) Explanations can never be complete and thus we should choose the factors that are relevant. Laudan dismisses SSK on that basis without an explicit account of the degrees explanatory relevance (1977, 209-210).
>
> (II) SSK has not provided us with general laws and therefore no explanations (1977, 217-218). Neither has it explicated the general mechanisms connecting social factors and the contents of science (1977, 219).

Laudan seems to have some implicit idea about the nature of causal explanations, but is obvious that his criticism can be established only if we have tools to (i) define the degree of relevance and (ii) analyze the role of laws and mechanisms in explanation. Therefore, not only does SSK require a general account of explanations to lean on but also the critics of SSK. The question is not who is right but how certain claims can be made in the first place.

Tenet (2) can also be accepted. However, we need to distinguish between what can be explained and what in fact is explained. We could be (and probably are) able to explain both true and false beliefs. However, it does not follow that we should be impartial towards those beliefs. Not even natural sciences are completely impartial with respect to the phenomena they explain. They might *be able* to explain many *types* of phenomena, but usually they *actually* explain only those types that we happen to be curious about or that we want to learn to control. What is more, not even natural sciences explain every *singular* occurrence in the universe; nowhere can I find a ready-made explanation why the temperature in my room is 21 Celsius degrees at this moment. In a similar vein, we should seek to be able to explain both true and false beliefs but we should also direct our explanatory efforts since explaining everything is impossible. My framework offers the tools for this task: We have seen that the significance of a feature is the only thing that matters when we consider whether it should be explained or not. The truth or falsity is not directly relevant to such choice. Moreover, as discussed in the previous section, I do not postulate a unique category of "rational explanations", and therefore there is no distinction between the kinds of explanations that are offered for true or false beliefs as both must follow similar patterns. Notice, however, that this follows from an explicit account of explanation and not from a loose derivation from "what other sciences do".

Tenet (3) is the most difficult one to understand. We might begin by distinguishing a substantial and a methodological reading. According to the substantial reading, the history of science has been shaped by factors that belong to a limited set of types[89] and those types have been the causes of both true and false beliefs.[90] Sometimes claims about the content of such explanations have been added to the substantial reading. For example, it has been claimed that "the electron 'itself' drops out of the [causal] story [explaining a belief]" (Bloor 1999, 93). Moreover, we can distinguish strong and weak readings of the substantial claim. The strong reading says that if T is a type of cause that is sometimes explanatory, T is always explanatory (and therefore the symmetry follows.)[91] If we find out, for example, that social interests explain (i.e. are among the explanatory factors) one thing, they must explain (i.e. be among the explanatory factors) everything. The weak reading says that a limited set of types explain everything without every type being explanatory with respect to every particular occurrence. It is enough for the symmetry, the reading continues, that the types

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[89] "Symmetry principles have always [recommended that] certain treatments should not be applied to any beliefs" (Bycroft 2016, 21).

90 The language of "types of causes" might seem cumbersome. Lewens paraphrases this as: "the same family of explanatory concepts should work to explain all kinds of belief formation" (2005, 463).

[91] Bycroft (2016, 19) calls this view the "restrictive approach". He defines it: "[the] historian [assumes] that all beliefs really can be explained in the same way" (2016, 19). Notice that it is unclear whether Bycroft is making a substantial or methodological reading. Use of the terms "approach" and "really" pull in different directions.

occur roughly evenly in the explanations of true and false beliefs (see Bycroft 2016, 21). The problem with the substantial claim is that it is difficult to see how either the strong or weak reading, let alone claims about the contents of the explanations, can be warranted before we have explained relevant parts of the history of science. And before we can conclude that we have explained the relevant parts of the history of science, we must understand how explanations work. Therefore, the substantial reading can be established only after a well-formulated account of explanation is in place.

Notice that both the substantial reading seems extremely dubious once we extend our explanations beyond beliefs: for example, how could the public understanding of Einstein's works and the formalism in those works be explained by citing the same factors? Surely, Bloor did not discuss such explanations as they do not refer to beliefs, but we surely do not want "asymmetrical symmetry-principle" according to which true and false beliefs are explained in one way and the rest of the science in whatever way is suited. We need a more general symmetry-principle than that provided by Bloor according to which all explanations are based on the same explanatory resources. I discuss this topic in the next chapter. We should notice that this problem is derivative from the unnecessary distinction between cognitive and non-cognitive features, discussed in Section 5.3. Bloor's strategy to extend some ideas concerning explanations from other disciplines to explanations of beliefs preserves the distinction between beliefs and the rest of science and mistakenly takes the central principles, such as symmetry, impartiality, and reflexivity (see below) to apply only within the category of beliefs, even though those principles make sense only if we look on our stock of explanations as a whole. We should not attempt to use principles concerning explanations within the category of different kinds of beliefs. Rather we should attempt to put the beliefs within a system where those principles already hold.

The methodological reading says that we should try to explain true and false beliefs in the same way and use only a limited set of types of causes because this approach carries the promise of a satisfactory stock of explanations (whatever is meant by "satisfactory") where no type of explanation belongs (or is excluded) *a priori*. (See discussion in Pinch 2008). This is a sound principle if we want to keep our explanatory resources as simple and powerful as possible. However, one cannot establish the types of causes that are needed in explanations before having a reasonable sample of good historical explanations. And, again, to be able to conclude that one has such a set, one needs an account or explanation that warrants the judgement. Moreover, the methodological reading cannot be as naïve as is sometimes suggested. For example, some have been willing to *ignore* one set of factors in order to establish the relevance of another set (Labinger 2001, 173). This is obviously a mistake (Bricmont & Sokal 2001, 181), and we need to have an account or explanation that enables us to avoid such mistakes.

Notice that also the methodological reading becomes extremely dubious once we extend our explanations beyond beliefs. It does not seem promising to assume that both (i) true and false beliefs and (ii) the rest of science could be explained by citing same factors. If we cannot assume this, we need to extend our explanatory resources anyway to capture *explananda* beyond beliefs. Once this extension is added, why should we assume that explanations of true (or false) beliefs do not cite the factors we just added to our explanatory resource? (Notice that this could lead to asymmetry within the category of an explanation of beliefs). The reason can no longer be that we do not want to complicate the explanatory resources, as the resources are already complicated. We should notice, again, that this problem is derivative from the unnecessary distinction between cognitive and non-cognitive features, discussed in Section 5.3. That distinction should not be used when an account of explanation in historiography of science is built. As both the substantial and methodological reading gets into trouble when we do not focus only on beliefs, the tenet 3 cannot be part of a general account of explanations in the historiography of science in its original form.

Criticism of the symmetry principle has usually been understood as a criticism of SSK alone. However, the criticism above underlines points that are important for everyone. Think, for example, the idea – expressed by Lakatos and Laudan – that historiography of science should be explained as far as possible by rationality-based accounts. On the one hand, if this is a substantial claim about historiography, it can be established only after some relevant parts of historiographical explanations has been formulated. On the other hand, if it is a methodological claim, it is threatened by similar problem of unwarranted ignorance of factors as the SSK. This shows that the lack of well-developed account of explanation is a shared problem for both ends of the continuum of philosophical positions on the nature of historiography of science. It is difficult to establish arguments about the explanatory structures of historiography of science without invoking some systematic account of explanation. My hope is that developing such an account will help to avoid many sweeping disagreements and to find a shared background for discussions.

Tenet (4) is, again, easily acceptable. The problem, however, is its unreasonable limitedness. We should demand that the totality of our stock of explanations is internally consistent. From this general demand it follows that SSK should be internally coherent. Seen from another direction, this means that that SSK does not need to be only internally coherent, i.e. be able to explain itself, but also coherent with every other field that provides explanations. Notice that this requirement does not apply to SSK alone. Every project devoted to explaining the history of science must satisfy the requirement of consistence. In the next chapter, we will see how this requirement follows from a general account of historiographical explanations. In Section 6.8, we will also see that, even though the demand for consistency is not an *a priori* reason to abandon SSK, it will turn out that this demand seriously undermines SSK when one takes seriously the explanatory resources of other fields.[92] Again, a general account of explanation will enable us to interpret and evaluate a claim made by SSK (as well as positions opposing it).

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[92] It would also damage most of the rationality-based explanations; however none of my analyses (Chapter 7) focuses on such explanations and the case remains unproven. The general problem is that no explanation that cites only one type of cause is *prima facie* promising, given the complexity of the world as it is described by the variety of our accepted sciences, social science, and humanities.

5.6 Conclusion

In this chapter, we have seen some perspectives on the history of science and analyzed briefly the kinds of issues that are connected with each perspective. We have seen that there is no way of – and no value in – choosing between different perspectives and, instead of forcing such a choice, we should aim to capture the valuable insights from each perspective. Moreover, we should also avoid the controversial claims that have produced unnecessary rivalry between the perspectives. We need to go beyond the peculiarities of each perspective.

We have already solved some of the problems related to the different perspectives in the earlier chapters. In the next chapter, I will formulate an account of explanations that shows how explanations can be analyzed and therefore their merits and credibility evaluated. The account

(1) is completely indifferent to the nature and scope of the thing to be explained;

(2) can be used independently of assumptions about the rationality in/of science;

(3) is able to accommodate many kinds of factors as explanatory

(4) defines two notions of explanatory relevance: one related to the explanatory depth and other related to explanatory goals;

(5) explains how the explanatory resources should be managed.

These five dimensions make the account suitable for approaching the history of science from different perspectives and enable one to see where the perspectives complete each other and where they genuinely disagree. The account of explanation defines at the most general level possible what explanation in historiography of science is.

One final note. Even though the account is completely general with respect to many issues in historiography of science, this does not mean that the account is *a priori* or unhistorical. The framework receives its justification from the ability to define important notions, connect different notions, and to clarify causal thinking. “Causal cognition is [--] seen as a kind of epistemic technology—as a tool—and, like other technologies, judged in terms of how well it serves our goals and purposes.” (Woodward 2014, 693–694.) As we will see at the beginning of the next chapter, the account has many advantages, and these advantages are the justification for the use of the account. Moreover, it might be the case that patterns of counterfactual dependencies provide understanding for *us* but were irrelevant for some historical actors. This does not undermine the account. Historiography of science makes the history of science understandable for us and therefore must be structured in accordance with our views on explanations and understanding. Notice also that there is no anachronism in the account: it does not imply that the historical actors thought that explanations are based on counterfactual dependencies. From the fact that explanations have the form “X rather than Y because Z rather than W” we are never forced (nor allowed) to infer that “A did b rather than c because A thought ‘X rather than Y because Z rather than W’ rather than something else”. That the account is built for us does not mean that it makes impossible to describe the past in its own terms in matters that are relevant (see Section 3.4 *The past in its own term*).

6 EXPLANATION IN HISTORIOGRAPHY OF SCIENCE: A COUNTERFACTUAL ACCOUNT

We have seen that, in the presentist approach, historiography of science provides causal explanations for the significant features of science. In this chapter, the notion of *causal explanation* in historiography of science is made explicit. I argue that if, and only if, we are able to track down patterns of counterfactual dependencies concerning a historical process or event, we understand that process or event. This approach is based on James Woodward's notion that "the underlying or unifying idea in the notion of causal explanation is the idea that an explanation must answer a what-if-things-had-been-different question, or exhibit information about a pattern of dependency" (2003, 201). In my account, explanations in historiography of science are defined as explications of causal relations between certain factors. Causal relations, in turn, are explicated as counterfactual dependencies between these factors. Therefore, explanations are explications of counterfactual dependencies.

In this chapter, I show that there are many advantages in building the notion of *historical explanation* on this counterfactual theory of causation:

> 1. By using the theory, we can see that historical explanations are structurally similar to the explanations in everyday life and in special sciences. We do not need to posit a *sui generis* historical understanding.[93] Conceptual scarcity (a unifying theory of explanation) is preferable to conceptual complexity (postulating a *sui generis*). Moreover, in a contested field with many debates such as historiography of science, a theory that has been developed independently of the field has the promise to serve as a neutral arbitrator in the debates.[94]

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[93] I return to this topic at the conclusion of the book (Chapter 9).

[94] Someone might argue that this is brings back the old *unity of science* –framework. I do not think that this is obvious. I think there is nothing wrong, in principle, in an attempt to find similarities

2. The theory enables the citing of different kind of factors in historical explanations. Historians do not need to choose *a priori* what kind of factors are explanatory in historiography of science.

3. There can be multiple explanations of the same historical event. The theory of explanation suggested here allows that different explanatory preferences can lead to different explanations of the same event. However, in such a situation, it is always possible to find common ground between alternative explanations in counterfactual dependencies.

4. If there are genuinely competing explanations, the theory shows what kind of evidence and considerations are needed in order to choose between these explanations. In this way, the theory can direct historical research and clarify disagreements.

5. Within the theory, we can explicate a suitable notion of *explanatory depth* in historiography of science.

6. The theory tells us how explanatory resources should be managed.

I begin the introduction of the theory by giving a set of examples. These examples illustrate multiple important features of the theory of causal explanation that are useful in historiographical thinking and in the philosophy of historiography of science.

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between different fields of inquiry. My account unifies the historiography of science and other fields only at one level: it claims that explanations provide information about patterns of dependencies. There still remain fundamental differences between different fields. For example, in physics such patterns might be based on fundamental laws of the universe whereas in the historiography of science such laws play very little part. This chapter exists for the very reason that the historiography of science has its own peculiarities when it comes to explanations.

The examples used in this chapter are mainly imaginary scenarios, and I begin with examples outside historiography to minimize the possible confusion that is brought by our pre-theoretical "historiographical intuitions". In the next chapters, I analyze the existing historiography of science using the account developed here to tighten the connection between the philosophical theory and actual historiography of science.

In what follows, I rely heavily on James Woodward's theory (2003) of causal explanations.[95,96]As my purpose is only to provide tools for explanatory thinking in historiography of science, I do not present Woodward's extremely rich and complex theory in its full detail. Rather, I extract some basic insights from the theory that are useful in reflecting historiography of science. Woodward argues that his theory "recognizes that causal and explanatory claims sometimes are confused, unclear, and ambiguous and suggests how these limitations might be addressed". I agree, and this makes Woodward's theory suitable for historiography of science, as many claims in this field seem to be – if not confused and unclear – at least opaque enough to cause (unnecessary) intellectual conflicts and problematic views about the nature of science. Moreover, Woodward's theory has proven its philosophical worth in many fields.[97] Thus, there is no worry that the account solves historiographical problems in an *ad hoc* manner. My strategy is to focus on those aspects of Woodward's theory that are related to clarification of causal claims and use Woodward's ideas to show how many historiographical claims and notions can be made clearer.[98] Once this clarificatory task is performed, I turn to discuss more methodological issues in historiography of science.

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[95] Mainly in Sections 6.1, 6.2, and 6.5. In other sections, discussions do not follow Woodward's theory in any straightforward manner (although they are based on what is said in 6.1, 6.2, and 6.5).

[96] This is not the only possible account of counterfactual explanation that one can rely on to provide historiographical insights. See Maar 2016 for different approach.

[97] E.g. Woodward (2010), Ross and Woodward (2016), Rescorla (2017), Kuorikoski & Ylikoski (2010).

[98] For example, Woodward's notion of intervention (see Section 6.2.) is most naturally applicable to context where experiments can be performed. However, Woodward's notion's clarificatory

Finally, it should be noted that the account formulated below is independent of the presentist approach (although presentism requires an explicit notion of causal explanation): even if one does not accept the presentist assumptions, one can still use the theory to understand how explanations in the historiography work.

6.1 Illustrative Examples and the Outline of the Theory

In this section, causal relationships are said to hold between variables. I simply follow Woodward on this:

> [The] theory is most naturally formulated in terms of variables—quantities or magnitudes that can take more than one value. Causal relationships, of course, have to do with patterns of dependence that hold in the world, rather than with relationships between numbers or other abstracta, but in the interest of avoiding cumbersome circumlocutions, I will often speak of causal relationships as obtaining between variables or their values, trusting that it is obvious enough how to sort out what is meant. (Woodward 2003, 14.)

However, I will use my own terminology and distinguish between *scenarios* and *situations*. A *scenario* presents the variables V that are under consideration in a given case and their causal relationships.[99] A *situation* describes a state of the world where the variables V have taken particular values. There are multiple possible situation within one scenario.

Moreover, I use the term *explanandum* to refer to a question of the form "Why $Y = y_1$ rather than $Y = y_2$?", and the term *explanans* to refer to an answer to this question.

To get started, consider the following scenario (S1):

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power is not restricted to such cases. The notion can be used to clarify causal claims even in the contexts where an intervention is only conceptually possible (Woodward 2003, 132). How to clarify causal claims with counterfactuals and how to confirm them are two separate issues.

[99] It would be necessary to distinguish *type-level* and *token-level* causal relations if we wanted to understand Woodward's theory in its completeness. However, the philosophical content (that explanations are answers to what-if-things-had-been-different questions) I draw from Woodward's theory to the philosophy of historiography can be understood without this distinction.

$$X \longrightarrow Y$$

$$x_1 \longrightarrow y_1$$
$$x_2 \longrightarrow y_2$$
$$x_3 \longrightarrow y_3$$

X is the cause of Y. By following Woodward's theory (2003), this means that changing[100] X will change Y.[101] x_1, x_2, x_3 are the values that X can take, and y_1, y_2, y_3, are the values Y can take. Changing X from x_1 to x_2 (or x_3) changes Y from y_1 to y_2 (or y_3). The values of Y are counterfactually dependent on the values of X: had X taken value x_2 (or x_3) instead of x_1, Y would have taken the value y_2 (or y_3) instead of y_1.

In my terminology $X = x_1$ and $Y = y_1$ is a *situation* within this scenario (as well as any $X = x_i$ and $Y = y_j$).

Let X be the number of workouts Smith performs per week and let Y be the amount of muscle Smith builds in a year. If Smith performs one workout per week, she gains one kilogram of muscle. If she performs two workouts, she gains two kilograms, and so on. By changing X (the number of workouts per week) we can change Y (the amount of muscle Smith builds in a year). The amount of muscle Smith gains is counterfactually depended on the number of workouts per week. We can say, for example, that had Smith done two (or three) exercises per week instead of one, she would have gained two (or three) kilograms of muscle instead of one kilogram.

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[100] It is necessary that X is changed in the right way, by *intervention*. I explicate this notion in the next section.

[101] "[A] claim such as "X is causally relevant to Y" is a claim to the effect that changing the value of X instantiated in particular, spatiotemporally located individuals will change the value of Y located in particular individuals." (Woodward 2003, 40.)

We can explain the amount of muscle Smith has gained by citing the number of workouts she has performed per week and adding the information about the counterfactual dependencies between the values of X and Y. This information is a crucial part of the explanation as it helps to answer *what-if-things-had-been-different questions*.[102]

For example, the question: "Why did Smith build two kilograms of muscle in a year rather than one or three?" is answered as follows:

> Smith performed two workouts per week. Had she performed 1 workout, she would have gained 1 kilogram of muscle, and had she performed three workouts, she would have gained three kilograms of muscle.

It is important to note right away that we do not need to know how the number of workouts and the amount of muscle mass are connected (i.e. what kind of process or mechanism connects them.[103]) We could try to establish this relation and provide an additional (and a deeper) explanation. However, once we know the counterfactual dependencies between the number of workouts and the amount of muscle mass, we have an explanation. We are able to answer what-if-things-were-different question. That the threshold for explanatory status is not set very high is an advantage in historiography of science. We should not set the bar too high and end up saying that there are very few explanations in historiography of science. Moreover, even though the threshold is not set very high, we can still seek deeper and deeper explanations (this will be discussed below). Therefore, explanations are best seen as a continuum from minimal to deep explanations. (Woodward 2003, 243).

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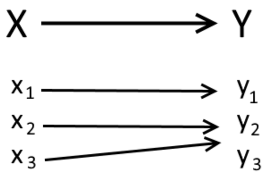
[102] "[Explanations] locate their explananda within a space of alternative possibilities and show us how which of these alternatives is realized systematically depends on the conditions cited in their explanans. They do this by enabling us to see how, if these initial conditions had been different or had changed in various ways, various of these alternative possibilities would have been realized instead". (Woodward 2003, 191.)

[103] E.g. Salmon (1984) and Machamer, Darden & Craver (2000). See also Woodward (2011).

Sometimes the values of Y are dependent on the values of X in a more complicated manner.[104] Consider the following scenario (S2):



Here again the number of workouts per week is the cause of the amount of muscle Smith gains per year. If Smith changes the number of workouts from one to two (or three), the amount of muscle she gains will change. However, changing the number of workouts between two and three does not change the amount of muscle Smith gains. Due to genetics, Smith is not able to gain more than two kilograms of muscle per year. Thus, there are counterfactual dependencies between the values of X and Y but we need to be careful when explicating these dependencies.

In terms of explanation: we can explain the amount of muscle Smith gains in a year (Y) by citing the number of workouts she does per week (X) and adding information about the counterfactual dependencies between the values of X and Y. An important part of this information is that change between x_2 and x_3 *does not change* the value of Y from y_2.

For example, the question "Why did Smith gain two kilograms of muscle in a year rather than one?" is answered as follows:

> Smith performed two workouts per week. Had she performed one workout per week, she would have gained one kilogram. However, if Smith performed three workouts per week, she would still have gained two kilograms of muscle in a year.

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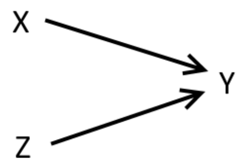
[104] See, for example, Woodward (2003, 67–68).

It is important to note that this kind of scenario is possible in the history of science. We can say that a certain experiment e_1 convinced a scientist. Had she carried out a different experiment e_2 instead of e_1, she would not have had a convincing result. Change from e_1 to e_2 would have made a difference. However, there can still be an experiment e_3 such that: had the scientist made e_3 instead of e_1, she would still have had convincing results. In this case, the e_1 is explanatorily relevant, but it would be important to note that e_3 would have also led to a convincing result while e_2 would not. Take another example: it is almost a truism to say that the social context is an explanatorily relevant factor in historiography of science. What is needed, in addition to this truism, is detailed information about how changes in the context would have changed science.[105]

Next, consider the following scenario (S3):



Here X is not the only cause of Y. Z is also a cause of Y. Assume that X can take values x_1, x_2, x_3, and that these are straightforwardly associated with the values y_1, y_2, and y_3 of Y, as in scenario S1. Assume also that Z can take two values z_1, z_2. By following Woodward's theory, we can say that X is a cause of Y, since changing X changes Y *if we fix the value of* Z. It might be the case that when Z takes the value z_1, no change in Y happens even if X is changed. If we fix Z to z_2, and if changing X then changes Y, X is a cause of Y.[106]

Let X be the number of workouts per week, Y the amount of muscle Smith builds, and Z the number of vitamin B12 pills Smith takes ($z_1 = 0$; $z_2 = 1$). Assume that Z takes

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[105] I discuss this in the next chapter.

[106] See Woodward (2003, 44–45).

the value z_1. No matter how many workouts per week Smith performs, she does not gain more than one kilogram of muscle in a year (due to her deficiency of B12). However, if we fix Z to z_2, changing the number of workouts per week changes the amount of muscle Smith gains.

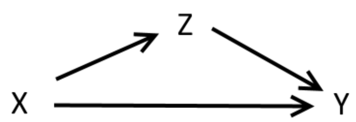
The value of Y is explained by citing the values of X and Z and by providing information about how different values of X and Z would have changed the value of Y. It is important to point out that if Z takes the value z_1, changing X does not change Y.

For example, the question "Why did Smith gain two kilograms of muscle in a year rather than one or three?" is answered as follows:

> Smith performed two workouts per week and took B12 vitamin pills. Had she taken the pills and performed one/three workouts per week, she would have gained one/three kilograms of muscle in a year. Moreover, had she not taken vitamin B12, she would have gained only one kilogram of muscle even if she had performed two or three workouts per week.

Scenarios such as this are possible in the history of science. For example, the existence of fossil records and a society interested in them are both needed to achieve scientific results based on such fossils. The complete understanding of the results requires that we track down the dependencies between the results, the fossil records, and the society.

The importance of the notion of *holding certain variables fixed at some value* is even clearer in the following scenario (S4):



Here X is a cause of Y and Z, and Z is also a cause of Y. In a scenario like this, it is possible that the effect of Z on Y cancels out the effect of X on Y. If we only change X, there will be no change in Y, as the effect of X is cancelled out by the change in Z.[107]

Let X be the number of workouts per week, Z the calories burned by Smith per week, and Y the amount of muscle Smith builds in a year. Increasing the number of workouts per week *increases* the amount of muscle that Smith builds. However, increasing the number of workouts also increases the amount of calories Smith burns which in turn *decreases* the amount of muscle Smith builds. In a scenario like this, changing X alone does not change Y. However, changing X changes Y once we fix the calories (Z) at some level. Y is counterfactually dependent on X on the assumption that Z is fixed.

In the history of science, this example could be about the following scenario:

$X = x_1$ is *the publication of a study S supporting hypothesis H*
$X = x_2$ is *no publication of study S*

$Z = z_1$ is *the publication of a study R supporting not-H*
$Z = z_2$ is *no publication of R*

Y = y1 is *the acceptance of H*
$Y = y_2$ is *no acceptance of H*

Assume that we start from a situation where $Y = y_2$, $X = x_2$ and $Z = z_2$. Then the study S is published (X takes the value x_1). Due to this change, R is published (Z takes the value z_1). Change in Z then cancels out the effect of X on Y, and Y remains in y_2. However, X is a cause of Y. Had $X = x_1$ been the case while Z was fixed at z_2, Y would have taken the value $_{y1}$. Z is also a cause of Y: if we fix $X = x_1$, changing Z would change Y.

---

[107] See Woodward (2003, 49–50).

Assume that actually $X = x_1$, $Z = z_1$, and $Y = y_2$. Assume also that we want to explain why $Y = y_2$ rather than y_1. The answer is:

> Because X took the value x_1 and Z the value z_1. Had X remained in x_1 while Z was fixed in the value z_2, Y would have taken the value y_1.

Let us modify this example. Consider the following scenario (S5): Assume that X and Z can take three values and Y two values:

> $X = x_1$ is *the publication of a study S supporting hypothesis H*
> $X = x_2$ is *no publication of study S*
> $X= x_3$ is *the publication of a slightly different study S* supporting H*
>
> $Z = z_1$ is *the publication of a study R suggesting not-H*
> $Z = z_2$ is *no publication of R*
> $Z = z_3$ is *the publication of a slightly different study R* supporting H*
>
> $Y = y_1$ is *the acceptance of H*
> $Y = y_2$ is *no acceptance of H*

Assume that x_1 leads to z_1, x_2 to z_2, and x_3 to z_3. This is arguably possible: for example, if different instruments were used in S and S*, different instruments could have been used also in R and R*, and as performers of R (or R*) might be more familiar with one set of instruments than the other, the result of the study can be different due to the instruments used.

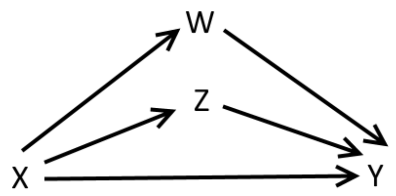
Assume that actually $X = x_1$, $Z = z_1$, and $Y = y_2$. Assume also that we want to explain why Y took the value y_2 rather than y_1. The answer is:

> Because X took the value x_1 and Z the value z_1. Had X remained in x_1 while Z took the value z_2 or z_3, Y would have taken the value y_1.

Moreover, had X taken the value x_3, y_1 would have been the case if Z took the value z_3 or z_2.

Moreover, had X taken the value x_2, y_1 would have been the case if Z took the value z_3.

Next, consider the following scenario (S6):



Let the relationships between X, Z and Y remain the same as in S4. In addition, X is a cause of W, and W is a cause of Y. W and Z have opposite effects on Y. Assume that:

X = x_1 is *the publication of a study S supporting hypothesis H*
X = x_2 is *no publication of study S*

Z = z_1 is *the publication of a study R supporting not-H*
Z = z_2 is *no publication of R*

W = w_1 is *the publication of a study P supporting H*
W = w_2 is *no publication of such result*

Y = y_1 is *the acceptance of H*
Y = y_2 is *no acceptance of H*

Assume that we begin from a situation x_2, z_2, y_2, and w_2. Assume that X then takes the value x_1. Because of this, other variables take values z_1, y_1, and w_1. X is a cause of Y: if we fixed *both* Z and W, changing X changes Y.

The question "Why did Y take the value y_1 rather than y_2?" is answered as follows:

1. Had x_2, w_2 and z_2 been the case, y_2 would have been the case. OR

2. Had x_2, w_2 and z_1, y_2 would have been the case. OR

3. Had x_2, w_1 and z_1 been the case, y_2 would have been the case. OR

4. Had x_1, w_2 and z_1 been the case, y_2 would have been the case.

This example (together with the scenario S5) hints how complex a complete (or ideal) answer to an explanation-seeking question can be.[108] Three binary variables produce four situations where the *explanandum* takes the value under review. This list of conditions answers many what-if-things-had-been-different questions. Each of the 1-4 answers one such question. For example, we can answer the question "What if X took the value x_2 and Z and W took values w_2 and z_2?" by using the first item on the list. Moreover, it must be noted that we have been given only a list of conditions under which Y would have taken the value y_2. We could add also a list of conditions under which Y would still have taken the value y_1. Such a list would be explanatory as it would answer what-if-things-had been-different questions.[109]

The lesson to be learned is that the list of conditions under which an *explanandum* would have changed is usually extremely long.[110] However, this should not frustrate

[108] I leave it to the reader to produce the scenario S6 with three possible values for X, Z and W, as we did in S5.

[109] If the *explanandum* Y can take more than two values, it is important that our explanation tells when y_1, y_2, … y_n, would have been the case.

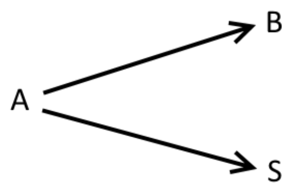
[110] In the natural sciences, the information can be packed in generalizations. However, I do not see how this could be done in historiography.

us. Explanations form a continuum: *A minimal explanation* provides some information about at least one situation that would have changed the value of *explanandum*-variable, while *a complete (or rather: ideal) explanation* provides information about every situation that would have changed the value of *explanandum*-variable. *Historical explanations* lie somewhere between these two extremes. Moreover, even if it is not pragmatically possible to provide complete explanations in the historiography, we will see that this notion enables us to evaluate the quality of explanations (with respect to the interests of questioners) and how further historiographical studies can deepen our understanding. However, before building that account, we need to clarify a few issues about the patterns of counterfactual dependencies that provide us explanatory information.

6.2 Interventions in Historiographical Thinking

We have used notion such as "changing X" and "fixing Z". How should these be understood?

Consider the following scenario (S7):



Here A is the common cause of B and S. There is no causal relation between B and S. However, if we change the value of B by changing A, S also changes. Thus, we need to restrict the relevant notion of "changing X" (or "changing B" in our example).

Let B be the reading of a barometer, S the occurrence of a storm, and A the atmospheric pressure. If we change the barometer reading by changing the atmospheric pressure, we also change whether a storm occurs. However, the barometer reading is not a cause for the storm. The problem is that the way we changed B (by changing A) has a causal effect on S.

According to Woodward's theory, X (here B) is a cause of Y (here S) only if Y (S) changes when X (B) is changed by *intervention*:

> The intuitive idea is that an intervention on X with respect to Y changes the value of X in such a way that if any change occurs in Y, it occurs only as a result of the change in the value of X and not from some other source. (Woodward 2003, 14).[111]

Changing A is not an intervention on B with respect to S, since A changes the value of S independently of B.

Consider that we ask "Why did a storm occur?"

We could answer

> Because the barometer reading fell. Had it not fallen, there would not have been a storm.

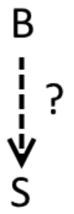
This answer is not a satisfactory explanation even though it answers what-if-things-had-been-different question. The relevant answer would be:

> Because the atmospheric pressure fell. Had it not fallen, there would not have been a storm.

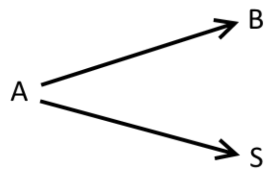
The difference between these answers is that the second one is true if an intervention on the atmospheric pressure was performed, while the first one is false if an intervention on the barometer was performed: if we hold the needle of the barometer fixed, the storm would have occurred anyway (as the atmospheric pressure fell).

Consider the following scenario (S8):

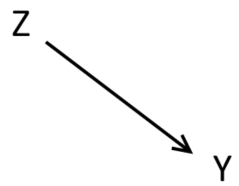
[111] Details in Woodward (2003, 98).



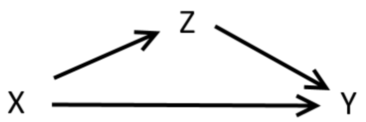
Someone claims that the development of modern medicine (S) is caused by the development of homeopathy (B): if we changed the development of homeopathy by removing people's interest in health (A), there would not have been modern medicine. However, this does not establish the causal relationship between modern medicine and homeopathy, as A is a cause of both B and S:



Next consider the following scenario (S9):



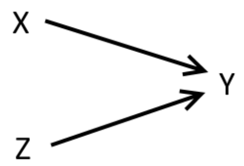
Z is Perrin's empirical work and Y is the belief in atoms. Someone claims that Z is not a cause of Y: had Perrin not made the work because someone else did it first (X), scientists would still have believed that atoms exist. The problem with this claim is that the counterfactual does not describe an intervention because X is directly relevant to Y:



From these examples, we can draw the following tool for historical thinking:

> In order to evaluate the claim *changing X would have changed Y*, one has to provide a (counterfactual) situation where X is changed in a way that does not affect Y independently of the change in X. Otherwise X is not a possible *explanans*.
>
> Similarly, if some other variable(s) Z need to be fixed, a (counterfactual) situation where it is fixed in a way that does not affect other variables beside Z needs to be provided.

Consider the scenario (of the type) S3 (see above):



Let X and Z represent publications of studies, and Y the acceptance of a hypothesis. Assume that X = x_1 and Z = z_1 are both publications of results that support hypothesis H, and Y = y_1 is the acceptance of H. In the actual case, x_1, z_2 and y_1 took place. We can say that:

> X = x_1 caused Y = y_1: Had we (i) imprisoned the scientists behind the publication X and thus changed X from x_1 to x_2, and had we (ii) hold Z to z_2 by imprisoning all the rest of scientists, Y would have taken the value y_2.

This might not be a very far-fetched situation. However, it might be the case that the situation with the imprisonment is not adequate to reveal the causal relations in the scenario. If H was a hypothesis that had wide social implications, it could have had been the case that the scientists in the prison thought that the correctness of H was the reason they were put in the prison. This would have led them to accept H independently of the studies. Thus there remains a doubt about our original situation as a way to establish the causal relations. This means that we need to specify a new situation:

> $X = x_1$ caused $Y = y_1$: Had (i) the scientist behind the publication X suffered a brain damage in a bus accident, and had X thus changed from x_1 to x_2, and (ii) had Z been fixed to z_2 by similar accident and brain damage, Y would have taken the value y_2.

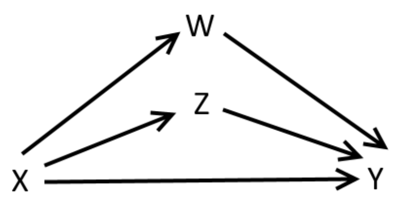
This is admittedly a far-fetched situation, but we only attempted to specify how changing X changes Y. Only if there is a situation, no matter how far-fetched, where X can be changed by an event or act that satisfies the notion of intervention with respect to Y, we have a genuine historiographical causal claim. *Clarifying the content of the claim that X is (or is not) a cause of Y does not require more.* This aim must be separated from the issues of (i) confirming causal and explanatory claims in the historiography, (ii) choosing interesting explanatory information, and (iii) evaluating the inevitability/contingency in the history of science. However, it is not always pointless to ask such clarifying situations, as the problems associated with scenarios S8 and S9 indicate.

To illustrate further the importance on intervention-based thinking, consider the claim (discussed in detail in Section 6.8) "that there are electrons explains Millikan's belief". It might be a counter-possible (and not just counterfactual) to assume that electrons did not exist at all. If this is the case, the electrons cannot explain Millikan's belief, since there is no possible intervention that removes electrons from the universe. We must therefore be more specific: we must specify the situation as *local* (concerning particular electrons in an instrument) rather than a *global* absence of electrons. Such a local absence of (free) electrons is clearly possible: it could have been brought about by the omission of ionizing radiation in Millikan's experiment. The ability to make such a distinction is an advantage of interventionist thinking in historiography of science.

6.3 Counterfactual Situations in the Historiography of Science and the Explication of the Notion of *Historical Explanation*

We have seen that it is important to clarify the claim "X is a cause of Y" by providing a situation that shows that an intervention on X with respect to Y is at least possible. However, scenarios and situations have an even more important role to play in historiography of science.

Let us return to the scenario (S6):



We assumed that:

$X = x_1$ is *the publication of a study S supporting hypothesis H*
$X = x_2$ is *no publication of study S*

$Z = z_1$ is *the publication of a study R supporting not-H*
$Z = z_2$ is *no publication of R*

$W = w_1$ is *the publication of a study P supporting H*
$W = w_2$ is *no publication of such result*

$Y = y_1$ is *the acceptance of H*
$Y = y_2$ is *no acceptance of H*

The question "Why did Y take the value y_1 rather than y_2?" was answered as follows:

1. Had x_2, w_2 and z_2 been the case, y_2 would have been the case. OR

2. Had x_2, w_2 and z_1 been the case, y_2 would have been the case. OR

3. Had x_2, w_1 and z_1 been the case, y_2 would have been the case. OR

4. Had x_1, w_2 and z_1 been the case, y_2 would have been the case.

Once we notice that the change in the variables is explicated by situations where the change satisfies the conditions for intervention, *we see that different items on the list correspond to different situations where the changes in the variables are due to interventions*.

Take, for example, the first item. It could correspond to the following situation:

> Had there been a fatal accident in the laboratory where S was performed, and had the groups that performed R and P lived on as usual, H would not have been accepted.

The second item could correspond the following situation:

> Had there been a fatal accident in the laboratory where S was performed, and had group (P) lived on as usual (i.e. not performing P because S was not performed), and had one group performed R due to (non-suspicious) order from the government, H would not have been accepted.

The fourth item could correspond to the following situation:

> Had the study S been performed, and had the group (R) lived as usual (reacting to S and publishing R), and had the group (P) withdrawn from performing P due to (non-suspicious) orders from the government, H would not have been accepted.

A historiographical account does not have to, of course, specify how the variables are changed or fixed. However, it should tell what combinations of values of variables, had these values been due to interventions, would have led to *explanans* and, *if needed*, clarify what kind of event or act would have counted as an intervention.

The second item above could be framed as follows:

> Had X taken the value x_2 due to an intervention [had there been a fatal accident in the laboratory where S was performed]
>
> and had no intervention on W performed [had the group (P) lived on as usual, W taking the value w_2],
>
> and had Z taken the value z_1 due to an intervention [had the group performed R due to (non-suspicious) orders from the government],
>
> Y would have taken the value y_2 (i.e. H would not have been accepted).

Here the clauses in brackets describe an event or act that counts as an intervention.

I argued above that we distinguish *minimal* from *complete* explanation. We can now say that:

> A *minimal explanation* of "Why y_1 rather than y_2?" provides one combination of values of variables (i.e. one situation) such that (i) had these values been the case, Y would have taken the value y_2, and (ii) all the *explanans*-variables take their counterfactual values due to interventions (or as an effect of change of their causes by interventions).
>
> A *complete explanation* of "Why y_1 rather than y_2?" provides every combination of values of variables (i.e. all the situations) such that (i) had these val-

ues been the case, Y would have taken the value y2, and (ii) all the *explanans*-variables take their counterfactual values due to interventions (or as an effect of change of their causes by interventions).[112]

A *historical explanation* of “Why y_1 rather than y_2?” provides some (but not every) relevant combinations of values of variables (i.e. some situations) such that (i) had these values been the case, Y would have taken the value y_2, and (ii) all the *explanans*-variables take their counterfactual values due to interventions (or as an effect of change of their causes by interventions).

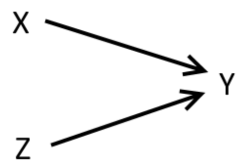
Given the interests of some group searching for an explanation, complete explanations are not always the preferable explanations, as they might include irrelevant parts. Consider the scenario S6. Assume that there are two universities, A and B. The groups behind the studies S and R are in the university A, and the group behind study P is in the B. There is nothing that people in the A can do to affect what studies are performed in B. This makes the explanations of $Y = y_2$ that cite the fixedness of variable W uninteresting for the people in A if those people are only pragmatically orientated with respect to the explanations at hand. For example, it would be uninteresting to say that:

Had X remained in value x_1, y_2 would have been the case, if W was fixed to w_2 and Z was fixed to z_1.

Since there is nothing that the people in A can do to fix W, this explanation would be uninteresting from a purely pragmatic point of view.

To make the point more obvious, consider the following scenario (of the same type as S3):

[112] It can be noted that the idea about historical text explicating many counterfactual situation explains why a historical text as whole, not a single sentence, is the cognitive unit in historiography, as is thought in *narrativist* historiography.



Let X represent the publication of a result R, Y the discussion about R, and Z an asteroid hitting the earth. It is true that there would not have been a discussion about R if an asteroid had hit the earth (i.e. change in Z is associated with change in Y). Moreover, this claim is a part of complete explanation (as would be claims about nuclear apocalypse etc.). However, in normal circumstances, this claim should not be mentioned in the explanation, at least not before the much more "mundane" relationship between X and Y is explicated. Every historian and every historical text has certain limitations, and thus complete explanations cannot be provided. *In practice, a historical study is some collection of minimal explanations.* (We can use the term *historical explanation* to refer to such collection.) This makes it very important that we can say when a historiographical study has provided a genuinely satisfactory collection of minimal explanations, and when one explanation is better than other. I return to this topic below. Before that, we need to consider when two explanations are competitors.

6.4 Explanations: Competitors and Goals

The concept of explanatory competition, with respect to different levels of explanatory scope, can be defined as follows:

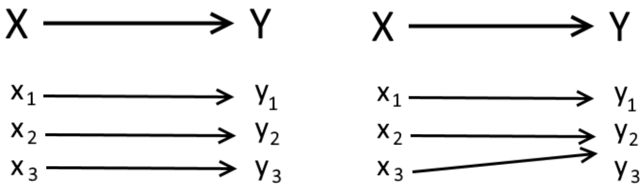
> *Competing minimal explanations* (MC): Two minimal explanations are competitors when they disagree about the counterfactual dependencies between the values of some variables. To be more precise, two minimal explanations E_1 and E_2 are competitors if and only if: As an answer to the question "Why y_1 rather than some $y_{k\neq1}$", E_1 claims that certain combination

C of the values of *explanans*-variables $X_{1 \ldots n}$ would have led to the value y_i, while E_2 claims that combination *C* would have led to $y_{j\,(\neq i)}$.

Competing complete explanations (CC) Two complete explanations CE_1 and CE_2 are competitors if and only if: the minimal explanation E_1 is a part of CE_1, the minimal explanation E_2 is a part of CE_2, and E_1 and E_2 are competitors according to MC.

Competing historical explanations (HC) Two collections of minimal explanations (i.e. *historical explanations*) HE_1 and HE_2 are competitors if and only if: (i) minimal explanation E_1 is a part of HE_1, minimal explanation E_2 is a part of HE2, and E_1 and E_2 are competitors according to MC, or (ii) HE_1 and HE_2 include different minimal explanations (i.e. there is at least one difference between them) but are intended to satisfy the same explanatory goal **E**.[113]

Consider the scenarios S1 (left) and S2 (right):



The claim that Y depends on X as in S1, and the claim that Y depends on X as in S2 generate competing minimal explanations. If we ask "Why y_1 rather than y_2?", we

[113] It should be noted that here the choice of *explanandum* is not what is meant by an "explanatory goal". We saw in Chapter Three how the *explananda* of historiography of science should be chosen. An explanatory goal describes which aspects of *explanans* historians want to shed light on. I return to this below.

get different answers. According to S2, $X = x_3$ would lead to $Y = y_2$, but according to S1, $X = x_3$ would lead to $Y = y_3$.

An important feature of MC is that it shows that there cannot be competition if E_1 answers the question "Why y_1 rather than y_2?" and E_2 answers the question "Why y_1 rather than y_3". For example, answers to the questions:

> Why did scientists come to believe that atoms exist rather than having no beliefs at all?
>
> Why did scientists come to believe that atoms exist rather than believing that atoms do not exist?

are not really competitors because they have different *explananda*. If we ask "Why do scientists believe in atoms rather than not?" there remains the ambiguity between the two questions above. The definition MC is helpful here: it tells when the competition between explanations is real and not produced by elliptic formulations of explanation-seeking questions.

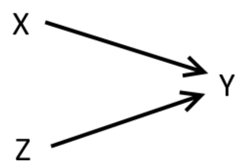
The limiting case of MC is one where the explanatory relevance of some variable Z to *explanandum* Y is denied. If the explanation E_1 says that values of Y depend on Z, and if E_2 says that all the values of Z lead to the same value of Y, E_2 denies that there is an explanatory relationship between Z and Y. However, in this case MC still implies that E_1 and E_2 are competitors. We need this possibility when explicating CC and HC.

The condition CC, in effect, is needed in addition to MC to point out that two complete explanations are competitors when they disagree about the explanatory relevance of some variable Z while possibly agreeing on others. This happens, for example, when the claim is made that "we should not refer to nature at all as an *explanans* in the [historiography] of science" (Bouterse 2014, 298).[114] In most of the cases, explanations in historiography are best not interpreted as competing complete explanations, but merely as different (and perhaps competing) *historical explanations.*

According to HC, historical explanations are competitors when they disagree about the counterfactual dependencies between the values of some variables. This is the straightforward sense of competition, and it is common to MC, CC, and HC. However, there is another, and arguably more interesting, sense in which two historical explanations can be competitors. It is that of providing different information but sharing the same explanatory goal.

Assume that there is a context of historical research where historians attempt to reveal the relevance of knowledge-sharing practices in science. In this context the explanatory goal **E1** of historians is to underline how knowledge-sharing practices work in science.

Consider the following scenario (mentioned above):



Let X represent the publication of a result R, Y the discussion about R, and Z an asteroid hitting the earth. It is true that there would have been no discussion about R if an asteroid had hit the earth. Assume that there are two historical explanations of the $Y = y_1$ (discussion about R).

> 1. Had the result R not been published, there would have been no discussion about R.

[114] I return to this topic below. Notice that Bouterse does not hold this thesis. Only the excellent formulation of the thesis is from Bouterse.

2. Had an asteroid hit the earth, there would have been no discussion about R.

These explanations are competitors if they attempt to satisfy the same explanatory goal **E1**. *Given the explanatory goal* **E1**, it is obvious that (1) is a better explanation than (2). Asteroids are a peripheral phenomenon in the knowledge-sharing practices.

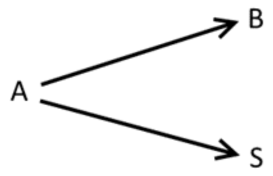
Next, assume that there is a context of historical research where historians attempt to reveal how great a cosmic coincidence science is. In this context, the explanatory goal **E2** is to understand science as a part of the vast universe. *Given the goal* **E2**, (2) is arguably a better explanation than (1).

It must be noted that when two very different explanations are offered (as in our example), the best reading of them is usually that they attempt to satisfy different explanatory goals. It would be most natural to say that explanation (1) is targeted to the goal **E1**, while (2) is targeted to **E2**. *This means that we can accept many different explanations of the same explanandum.* However, this does not mean that anything whatsoever can have an explanatory value. We already saw that only if there are counterfactual dependencies variables (under interventions), the relationship between the variables is explanatory. Moreover*, asserting that something is always (or never) explanatory should make us suspicious.* From the fact that X is explanatory/irrelevant with respect to a goal **E**, it cannot be inferred that X is explanatory/irrelevant with respect to every goal $\mathbf{E}_i$.

It is also important to notice that even if X is not explanatory with respect to some goal **E**, X cannot be ignored during a historical research. Consider the scenario (of the same type as) S8:

$$\begin{tikzcd} B \arrow[d, dashed, "?"] \\ S \end{tikzcd}$$

A politician says that scientists' interests in climate change (B) is the cause of an increase in measured temperatures (S). The politician is naturally interested in political aspects of science and thus he wants to highlight them. However, it is mistake to ignore the way the world is. There has been a global warming (A), and this is the common cause of both B and S.:



If A is ignored due to the explanatory goal, an explanation is found where none exists – B is not a cause of S. Bricmont and Sokal make this point when they write: "The [--] practice corresponds to ignoring some relevant variables [--] not to holding them constant. One can't hold constant a variable that one refuses to measure." (2001, 181.)

Moreover, historians with different explanatory goals can always attempt to find a common ground in an explanation that is *more complete* than the explanations that the historians provide. For example, the discussion about the result R can be explained as follows:

> 3. Had the result not been published OR had an asteroid hit the earth, there would have been no discussion about R.

Two historians with different explanatory goals could both accept this claim about patterns of counterfactual dependencies.[115] However, if they did not, then we would have a genuinely competing explanation. How to proceed in that situation will be discussed soon.

But how should the notion of *explanatory goals* be understood? No neat catalogue of such goals can be given. The notion is based on the idea that historians of science want to highlight certain aspects of a particular process that led to the *explanandum*.

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115 See Woodward (2003, 56–57) on the idea that patterns of counterfactual dependencies are the base on which shared understanding can be built.

The explanatory goal is what the explanations may seek to establish in addition to providing information about conditions on which the *explanandum* depends on. An explanation may seek to pinpoint the role of some (types of) factors (F) in the history of science, and this attempt is centered around the (meta-) explanatory goal of *understanding F in the development of science*. In this sense, the choice of explanatory goal aims at second-order understanding (whereas an answer to a particular *explanandum* gives us first-order understanding).

For example, *giving practically useful information* could be an explanatory goal. In the example above, the reference to asteroids could be omitted due to this explanatory goal. There is nothing that scientists (others than those directly involved with solutions to the threat of asteroids) can do about asteroids and their effects on the research. However, purely pragmatic considerations are not all that is needed to satisfy our intellectual needs. Different historians with different backgrounds might focus on highlighting different aspects of the *explanans*. The list of dimensions of science that are potentially historically interesting can be repeated here:

> *Epistemological:* What concepts, models and theories there has been in science? What kind of practices were developed to establish those? What were the epistemological goals?
>
> *Social*: How is science organized? What kind of social roles are there in the sciences and how these roles guide the practices? What and whose values are built in the science?
>
> *Science and society*: What is the relationship between science and politics, science and the economy, science and different social groups? How global/local is science? How accessible is science for different groups? How are the results of science communicated to and understood by society in general? What and whose values are built into the science?

> *Science and culture*: What is the relationship between science and other aspects of culture (religion, for example)? What and whose values are built into science?
>
> *Science and technology*: What is the role of science in the making of new technologies? How does technology shape scientific practices?
>
> *Psychological*: How do individual scientists understand themselves? What kind of thoughts and emotions do scientists have during their work and about their work?

This list gives an idea of which aspects of *explanans* can be highlighted. For example, one could focus on the values that played a role in a particular episode in science[116] or on the personal qualities of a scientist that shaped a scientific practice[117]. However, it should be noted that the historiography of science has its own history which should affect the way in which the *explanantia* are chosen. Nowhere in the sciences (or humanities) can one seek to build an explanation ignoring the previous research in the field. Neither is this possible in historiography of science. Therefore, it is necessary that the choice of an explanatory goal is based on the previous research that has (i) indicated that some aspects of science are worth focusing on, and (ii) left gaps in the picture that has emerged.

Moreover, we have learned (in Chapter 3) that the significant features of science can (and must) be separated from the insignificant ones. The choice of explanatory goals should follow our understanding of significant features of science so that the choice is made in interesting and non-biased way. In other words, the aspects of *explanans* that are highlighted should be in line with our judgements and principles of significance (that are used when *explananda* are chosen).

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116 See Bellon (2011), discussed in the next chapter.

117 See Krige (2001), discussed in the next chapter.

I suggest that we can accept, within some limits, that historians might have different explanatory goals. This means that a historical event or process can be explained in various ways and by focusing on different aspects of the past that led to the *explanandum*. Even though I argued above that the idea that such explanations are competitors is not the most natural one, it would still be desirable if something could be said about the relative merits of such explanation. I now turn to this issue.

6.5 Explanatory Depth in the Historiography of Science

I develop the criteria for explanatory depth using the ideas introduced in Hitchcock and Woodward (2003) and Woodward (2003).[118] We have seen that *explanandum* of the form "Why y_1 rather than y_2?" is explained by a collection of *explanantia* of the form "Because x_1, z_1 and … and w_1 rather than x_2, z_2 and … and w_2". In science, these collections can often be read off from *generalizations* that describe the relationships between variables. Because of this, the notion of explanatory depth in Hitchcock and Woodward (2003) is defined with respect to such generalization. However, due to the nature of historiography, I do not characterize explanatory depth with reference to generalizations. Rather, I explicate criteria that enables us to compare the explanatory depth of two historical explanations (i.e. collections of counterfactuals). This change from generalizations to collections of counterfactuals requires that the criteria of explanatory depth are somewhat rewritten from Hitchcock and Woodward (2003).[119]

[118] There are other ways to analyze explanatory depth/power, e.g. Ylikoski and Kuorikoski 2010; Weslake 2010; Imbert 2013; Bhogal 2019. I follow Woodward and Hitchcock because their ideas are most closely connected to the ones discussed in the previous sections.

[119] Instead of writing about "more invariant generalization", I write about "deeper explanation". In Hitchcock and Woodward (2003), the relative depth of an explanation depends on the range invariance of the generalization that is used in the explanation. As the notion of *generalization* is rarely applicable in historiography, I do not use the term *invariance*.

However, we can still retain the core from the idea that "[one] generalization can provide a deeper explanation than another if it provides the resources for answering a greater range of what-if-things-had-been-different questions, or equivalently, if it is invariant under a wider range of interventions" (Hitchcock and Woodward 2003, 198) by connecting explanatory depth in historiography of science with the ability to answer what-if-things-were-different questions. In what follows, I explicate when one historical explanation has better ability to answer what-if-things-were-different questions than another.

Assume that E and E* both explain why $Y = y_1$ rather than y_2.[120] We can say that a historical explanation E is deeper than historical explanation E* in the following cases:

1. E and E* both relate the values of variables X and Y. However, E gives more information about what happens to Y with different values of X.[121]

For example, if E says:

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[120] How to compare two explanations with different *explananda* is a question that cannot be answered within this book. Moreover, such criteria could be misleading since there is no guarantee that everything can have an equally deep explanation. For example, purely chancy (singular) events cannot be explained at all. Saying that an explanation is not very deep, in a situation where no deeper explanation is possible, could lead to the false idea that there is something that could be added to the explanation. Thus I restrict the discussion to explanation *depth with respect to the same explanandum*.

[121] This is based on the following: "Let G be a generalization that includes X as one of its *explanans*-variables, and suppose that G is invariant under interventions on the value of X within the range R. Suppose that G′ is a different generalization that purports to explain the same outcome. [--] G′ includes X, but it is invariant under interventions on X within range R′, which strictly contains R." (Hitchcock and Woodward 2003, 184-185).

> $Y = y_1$ because $X = x_1$. Had X taken the value x_2, Y would have taken the value y_2; and had X taken the value x_3, Y would have taken the value y_3.

and E* says:

> $Y = y_1$ because $X = x_1$. Had X taken the value x_2, Y would have taken the value y_2.

then E is a deeper explanation than E*. E answers more what-if-things-had-been-different questions.

2. E and E* both relate the values of variables X and Y. Both give the same amount of information. However, E gives information about what happens to Y with more interesting values of X.[122]

For example, if E says:

> $Y = y_1$ because $X = x_1$. Had X taken the value x_2, Y would have taken the value y_2; and had X taken the value x_3, Y would have taken the value y_3.

and E* says:

> $Y = y_1$ because X = x1. Had X taken the value x_2, Y would have taken the value y_2; and had X taken the value x_4, Y would have taken the value y_4.

then E is a deeper explanation than E*, if x_3 is more interesting than x_4.

Consider that X represents a publication of a study:

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[122] "If the actual values of the variables fall within the range of invariance of both G or G′, it may be reasonable to prefer G′ if it is more accurate within the region of overlap, or if the actual values of the variables fall more squarely within the range of invariance for G′. (Hitchcock and Woodward 2003, 185). I have replaced the "if it is more accurate [--] or if the actual [--]" with the notion of *interesting values*. (See also Chapter 8).

> x_1 = the publication of a study S supporting H
>
> x_2 = no such publication
>
> x_3 = the publication of a slightly different study S* supporting H
>
> x_4 = the publication of study S with the proof that God exists and made the world the way in which H suggests.

As x_4 is not interesting (because it is extremely far-fetched), E* is not as deep an explanation as E. E answers more interesting what-if-things-had-been-different questions *with respect to explanans*.

If E tells what happens when x_1, and E* what happens when x_2, and if both x_1 and x_2 are equally interesting, there might not be a difference in depth. Reflection on whether x_1 or x_2 is more interesting with respect to historiographical research at hand is needed.

Moreover, if E describes what happens when X takes interesting values but E* describes what happens under many different but less interesting values of X, judgement about explanatory depth is complicated. For example, if E says (telling an individual story about every person mentioned):

> Had the person S1 or S2 … or S100 (who are not actually scientists but could have been) explained Brownian motion, scientists would have believed in atoms.

and E* says:

> Had Niels Bohr explained Brownian motion, scientists would have believed in atoms.

then the comparison between explanatory depth is complicated and must be decided case-by-case.

3. E relates X and Y, and E* relates Z and Y. E gives information about the values x_1-x_n of X, and E* gives information about the values z_1-z_k of Z, and (i) n > k (E gives more information), or (ii) the values of X are more interesting than the values of Z (i.e. E gives more interesting information).

For example, if X represents the quality of some research and Z the size of a body that hit the earth, E could be preferable due to an interest in epistemology.

4. E and E* both explain why Y took the value y_1 rather than the value y_2. (E relates X and Y, and E relates Z and Y; it is allowed that Z = X). However, E provides more information about when Y would take some other values besides y_2, *or* E provides information about when Y would take some interesting value y_3. E answers more (or interesting) what-if-things-had-been-different questions *with respect to explanandum*.[123]

For example, if E says:

> Had the result of a study been R, scientists would not have believed that H. Had the result been R*, the study would have been repeated.

and E* says:

> Had an asteroid hit the earth, scientists would not have believed that H. Had a slightly smaller asteroid hit the earth, scientists would have forgotten H.

then E is a deeper explanation, if we assume that it is more interesting to know when a study would have been repeated than to know when it would have been forgotten.

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[123] These criteria are not directly mentioned in Hitchcock and Woodward (2003). However, it is natural to suppose that some values of *explanandum*-variable are more interesting than others, since we assumed this to be the case with the *explanans*-variable.

Again, if E tells when y_2 would have happened, and E* when y_3, and if both y_2 and y_3 are equally interesting, there might not be a difference in depth. Reflection on whether y_2 or y_3 is more interesting with respect to historiographical research at hand is needed. Moreover, if E tells when Y takes interesting values but E* describes when Y takes many different but less interesting values, judgement about explanatory depth is complicated and must be decided case-by-case.

5. E would still be true while E* would no longer be true, if there was a change in background conditions, due to the conceptualization of *explanantia*.[124]

Assume that 1905 was a somewhat boring year, and therefore Einstein's explanation of Brownian motion was the most noteworthy event of the year.

Now, if E says:

> Had Einstein not explained the Brownian motion, scientists would not have believed that atoms existed.

and E* says:

> Had the most noteworthy event of the year not happened, scientists would not have believed that atoms existed.

then E is a deeper explanation. This is due to the fact that E would still be true if the iPhone had been introduced in 1905, while E* would no longer be true. The introduction of the iPhone would have been the most noteworthy event in 1905. However, if the iPhone had been introduced, it would no longer be true that had the most noteworthy event not happened [had the iPhone not been introduced] scientists would not have believed that atoms existed. E* is therefore fragile with respect to changes in the

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[124] This is not from Hitchcock and Woodward (2003). Instead, it is based on considerations in Woodward (2003, 217).

background conditions. We should prefer E as it provides (or, at least, building materials for) better *possible-cause hypothesis*[125] than E* (see Woodward (2003, 217).

6. E provides information about variable Z that is not provided by E*. In other words, E is a more complete explanation than E* as it makes explicit Z that is only a background conditions from the perspective of E*.[126]

For example, if E* says:

> Had there been a study S, hypothesis H was ignored. Had there been a study S*, a further study would have been performed.

and E says:

> (I) Given the actual scientists: Had there been a study S, hypothesis H was ignored. Had there been a study S*, a further study would have been performed.
> (II) Had there been more cautious scientists: Had there been a study S, further studies would have been performed. Had there been a study S*, a further study would have been performed.

E is a deeper explanation. It tells us how the ethos of scientists affects the *explanans* (i.e. it incorporates more variables) and therefore answers more what-if-things-had-been-different questions. It is important to note that Z can be also a variable that mediates the influence of X on Y. Therefore, we do not need to have knowledge about

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[125] Woodward uses the term, *generalization*. However, these generalizations that tell "[X] is among the possible causes of [Y]" (Woodward 2003, 214), should not be confused with explanatory generalizations that give somewhat detailed information about the relationship between the exact values of X and Y. Therefore, I choose to use the term *hypothesis*.

126 "G′ makes explicit the dependence of the explanandum on variables treated as background conditions by G." (Hitchcock and Woodward (2003, 187).

the mechanism that connects X and Y in order to have an explanation, but once we have that knowledge, we have a deeper explanation.

According to Woodward and Hitchcock "[**6.**] is, perhaps, the most fundamental way in which one [collection of counterfactuals] can provide a deeper explanation than another" (2003, 188). The more variables we add, the more complete an explanation we have. Therefore, complete explanations are the deepest explanations.

However, there is a complication in this. In historiography, the historical context (whatever this means) where the *explanandum* is embedded needs to be somehow taken into an account when *explanans* is formulated. We cannot build every feature of a historical situation into an *explanans* and treat them as equally changeable. This would, in some sense, *tear down the historicity of these situation*. For example, it might not seem right to assume that the ethos of scientists in some era could have been different. The fact that scientists had that ethos might seem to be constitutive of that era. If we wanted to understand why something happened *in the context of that era*, it would be a distortion to provide a scenario where the ethos could have been different. We should thus give the following dimension of explanatory depth:

7. E and E* are close in depth with respect to criteria 1-6. If E* incorporates variable Z (while E does not) and if Z = z_1 characterizes the historical context, then E is a deeper explanation than E*.[127]

We have seen how the ability to answer (interesting) what-if-things-had-been-different questions is relevant for the explanatory depth. This enables us to compare and choose explanations, independently of the respective explanatory goals, *when they*

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[127] This is somewhat a mirror-image of Woodward's notion that "for different sorts of generalizations, applicable to different sets of phenomena or subject matters, there often will be specific sorts of changes that are privileged or particularly important or significant from the point of view of the assessment of invariance" (2003, 262). However, the idea remains that not all changes that would affect the *explanandum* are equally relevant for an explanation. *We want to know what would have changed the explanandum, given that certain (important) things remained constant.*

are not competitors. How to compare competing explanations, is a part of a more general question about explanatory knowledge and explanatory resources in historiography of science.

6.6 Explanatory Resources: Balancing Particular and General

We have seen that explanations have the form:

y_1 rather than y_2, because x_1 rather than x_2.

It is obvious that this kind of claim needs to be substantiated with information that makes it plausible that x_2 *would have led to* y_2. That there cannot be information about counterfactual situations is a truism. However, this does not mean that we cannot explicate what kind of considerations are relevant in substantiating counterfactual claims. Many excellent arguments, supporting the possibility of historical counterfactuals, have been given[128] and this encourages to believe that counterfactual claims can be warranted also in historiography of science.

In this section, I provide the general outlines of the types of considerations that must play a part in the explanatory practices of historiography of science. In the next chapter, I analyze the existing historiography of science and show the relevance of the considerations explicated here.

We can begin by noting that *the argument that counterfactuals are epistemologically opaque cannot be used to argue for or against some particular explanation*.[129] Thus, I limit

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[128] On the use of counterfactuals in history, see e.g. Maar (2014), Tetlock et al (2006), Bunzl (2004), De Mey & Weber (2003), Lebow (2000), Bulhof (1999) and *JPH* 10 (3). See also chapter 8.

[129] This is, of course, a judgement from within the counterfactual account and some other account could perhaps proceed without counterfactuals. (It is obvious that I do not think how a powerful account could be explicated without counterfactuals, as I would have attempted to produce such

my discussion to the cases where there are competing explanations.[130] The question is: How can we choose between two competing historical explanations?

First, we saw earlier that the claim "x_2 would have led to y_2" can be argued for (or against) only if it is shown that there is a possible intervention on X with respect to Y. An explanation that cites X as an *explanans*-variable can be accepted only if this minimal condition holds. Presenting a plausible situation where X is changed by intervention is therefore a requirement for X to be a candidate for an *explanans*-variable.

In itself, the establishment of the possibility of an intervention on X (with respect to Y) does not have much evidential value. However, it becomes important once we notice that there exists a methodological rule that would lead to the abandonment of this crucial feature of causal thinking. We saw earlier that variables should not be ignored during the research due to explanatory goals. This point can be generalized – variables should never be (consciously) ignored during the research. We saw that Bricmont and Sokal write: "The [--] practice corresponds to ignoring some relevant variables [--] not to holding them constant. One can't hold constant a variable that one refuses to measure." (2001, 181.) Bricmont and Sokal are answering Labinger, who writes:

> One frequently voiced objection is that the [social] studies [of science] are at best incomplete: by focusing on the social to the exclusion of the natural world they produce severely distorted pictures or even miss the point altogether. I would agree with the part about incompleteness, but how often do we expect a scientific study to be the last word on its subject? In fact, one typical strategy of scientific experimentation is isolation of variables: determining the effect of changing one while holding others constant. We are aware of the limitations of this approach—interactions between variables can well compromise any conclusions we might reach—but we do it anyway, *as a useful strategy*. (2001, 173 [emphasis added]).

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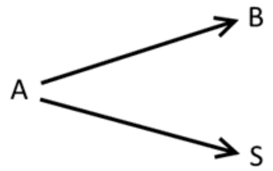
an account if I did.) The point is, however, that one cannot take a counterfactual account (with all its advantages) and then use the opacity as an argument for/against some particular claim.

[130] The question of the credibility of a single explanation reduces to this question once we assume that an explanation is not warranted if there exists equally well a warranted competitor. If it seems that if the evidence for some explanation (that does not yet have a competitor) is extremely weak, one surely can formulate some alternative to it and show that the alternative is equally warranted.

The diagnosis, according to my account, is that Labinger gets things upside down: There is nothing wrong, in itself, to say, as an explanatory conclusion, that some variables are irrelevant (with respect to some explanatory goal). It is, however, a *bad strategy* to ignore those variables in an ongoing the research. For example, this could lead to a misjudgment in a scenario where there exists common-cause relation:



If A was ignored, changing B could seem to change S if the change in B was caused by A. Establishing the possibility of an intervention on X (with respect to Y) is thus important because it establishes X as a candidate *explanans*-variable and because it is necessary for our causal thinking to stay on the right tracks.

There are more central evidential considerations that play a role in historiography of science. Discussion about them needs to be divided into two different questions:

1. How to choose between "y_1 rather than y_2, because x_1 rather than x_2" and "y_1 rather than y_2, because x_1 rather than x_3"?

2. How to choose between "y_1 rather than y_2, because x_1 rather than x_2" and "y_1 rather than y_2, because z_1 rather than z_2"?

The first question concerns the choice between two explanations that map the values of X into Y differently. This question is less interesting philosophically. It can be answered only case-by-case, as is shown in the next chapters. Therefore, we begin with the second question. The second question concerns the choice between explanations that cite different *kinds* of factors in explanation.

Consider the following explanations:

1. Scientists came to believe in atoms (Y) because Einstein explained Brownian motion (X).

2. Scientists came to believe in atoms (Y) because the movie *A Trip to the Moon* was released (Z).

It is clear that X (Einstein's work) should be accepted as an explanation rather than Z. First, X can be said to be preferable in this *particular* case. The reason is that there are indications of the relevance of Einstein's explanation. For example, Perrin's works were closely connected to Einstein's explanation (see Psillos 2011). Moreover, Perrin won the 1926 Nobel Prize for his work which clearly indicated that his work was widely appreciated. However, there are no indications that the movie played a role in this particular case. Secondly, and more importantly, Z *does not seem to be a kind of factor that can play a role in historiography of science.*[131] The details of this particular case are not the only reason to reject the explanation (2). The other reason is that it does not fit our view of historical developments in general.

Within the limits of this book, it is not possible to give useful philosophical insights about which kind of considerations can be used to establish the explanatory relationships between some variables in some *particular* historical situations.[132] As we have seen, historians of science can ask a wide range of questions with different scales about different aspects of science. This means that there needs to be a variety of different ways of searching for the answers. Establishing the influence of some publication to a scientist is very different task from that of establishing the effect of the age

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[131] After writing this example, I heard that *Interstellar* has had an effect on science (see James et al. 2015). It might be the case, then, that the release of movies might play a role in science. Instead of changing the example, I take this to confirm the approach in this section: Some factors might turn out to be explanatory when we consider many historical cases.

[132] In the next chapters, we will see that such considerations are not impossible. Using actual historiographical studies, I show how such considerations can proceed.

structure of a scientific community to the rate of acceptance of a new idea. A diary can solve the first issue; the second could rely on the considerations of the credibility of *Planck's principle*[133] and on the prospects of applying that principle to the case at hand – requiring demographic data and careful study of the views and discussions within the community. We also need to notice that sometimes historical counterfactuals are scientific counterfactuals and therefore equally tractable. For example, a historical explanation can refer to the behavior of electrons in an instrument (see Section 6.8). Moreover, how the values of some variable X should be mapped into the values of Y is similar question concerning some particular historical situations. All these considerations belong to the area of expertise of historians and scientists. However, there is a need for philosophical reflection about what *kinds* of factors can be explanatory in historiography of science. This is the reflection provided here, and the explanatory relevance in particular cases is discussed only when it is connected to the discussion about explanatory resources in general.

I will use the term *explanatory resources* to describe the kinds of factors that can be used in explanations in historiography of science.[134] The starting point for such concept is that historiography of science is not an ahistorical practice. It builds on the results of previous studies and there are (and have been) discernible trends in the field.[135] These developments are not just a mindless river in which the historians swim;

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[133] "[A new] scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it." (Cited from Hull et al. 1978, 718.)

[134] I discuss as if there were only one system of explanatory resources. If there are (or will be) many such systems (scientific pluralism), then the discussion indicates how one such system can be developed internally. How two interrelated distinct systems of explanatory resources cannot be discussed in this book. See Chang (2012, Section 5.2.3) discussing pluralism and interactions between different systems of practice.

[135] One of the most recognized of these trends is that "such ideals as truth, value-neutrality, and objectivity are neither eternally unchanging nor universally accepted. Rather, they are historical constructs, interpreted in a range of different ways, and coming into prominence at particular

they are conscious choices. The developments are based on a search for deeper understanding and unified pictures. Such search cannot proceed if nothing guides the research question and unifies the answers. Explanatory resources have a twofold function. First, and more important, they give guidance to which kind of factors can be explanatory in historiography of science. Secondly, they provide research questions. If we know that Z was the cause of a significant feature of science, we may then ask what the cause of Z was. Z does not need to be a significant feature in itself. In this way, an explanation can provide interesting research question. I focus mainly on the first function, as it is epistemologically interesting.

Our explanatory resources must be restricted. Otherwise they would not serve their role in guiding the kind of questions historians ask and in unifying our worldview. However, the explanatory resources must be rich (or powerful) enough to guarantee that we can answer many explanation-seeking questions with respect to different explanatory goal. This means that the explanatory resources must somehow be managed.

The right way to manage the explanatory resources is to balance the factors that have worked (i.e. that provided explanatory understanding) in particular cases and the general applicability of those factors. Historians have built a stock of explanations. When novel explanations are formulated, the factors that seem to have worked in the historiography previously should be used as heuristic that drive the ongoing research. If it turns out that those kinds of factors are not relevant in the research at hand, new factors must be used. This situation also means that the previous explanations must be re-evaluated in order to stop the inflation of our explanatory resources.[136]

An example (I give a genuine illustration in Section 7.3): If some developments D in the history of science have been credited to crucial experiments (C), then some

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times for particular reasons. Several important works of historical scholarship have mapped this terrain [--].” (Golinski 2012, 20.)

[136] The mapping of X into Y can be evaluated in this way. If it turns out that mapping some x_i to y_i in a particular case does not work, this should also make one re-evaluate the previous mappings.

new research on a development D* should check whether crucial experiments could be used to explain D*. If it turns out that D* cannot be explained by citing C, then a new factor F needs to be given. Moreover, historians should also re-evaluate the explanations that were based on the notion of a crucial experiment, and the choice of F should be such that F can be used to explain a wider set of developments than C.[137] *In this way, the simplicity and power of explanatory resources can be balanced.* We remove one kind of factor from our explanatory resources and replace it with another, presumably more applicable, kind of factor.[138] Management of explanatory resources is one way to gain a second-order understanding in historiography of science: We learn not about any particular *explanandum* but about explanations in historiography of science in general. Management of explanatory resources enable us to find *possible-cause hypotheses*, i.e. hypotheses that tell us what kinds of things are most generally applicable in historiography of science and where to start a research on certain issue.

Moreover, historiography of science should also take note of philosophical considerations. This is best seen when philosophy provides *restrictive results*.[139] If the philosophy of science has shown, for example, that the notion of a crucial experiment is incoherent, then this notion should not be used in historiography of science. This is a natural point, given that any explanatory resources in any given field must be conceptually and logically sound.

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[137] This wider set may involve D and D* but it can also include development D** besides them. In such cases, we need to re-evaluate the factors G that were used earlier to explain D**. Keeping our resources as strong and simple as possible is therefore a global task.

[138] If no other factor provides better understanding than C, we need further studies. Moreover, if C and F have a similar explanatory scope, we may compare their respective explanatory depths with respect to a set of *explananda*.

[139] The complexities of the relationship between historiography of science and philosophy of science requires a more detailed account than can be given here.

More important is to notice that historiography of science should also use explanatory resources of natural sciences.[140] There are three main reasons for this:

First, a historiography of science should show how science works as a part of the universe, including human beings, societies, the physical world and everything there is.[141] Of course, the ways of the natural world cannot be the *only* explanatory resource in historiography of science but this does not mean that it can be ignored. In fact, understanding science as a *human practice* requires us to pay attention to the rest of the universe, as only hubris could lead one to separate humans from the rest of the universe. Refusing to see the impact of the universe on science leads to inadequate understanding of science as a human activity. Moreover, even if one could live with inadequate historiography, it would be irresponsible to refuse to see the effects of human beings and their science on the environment. If we ignore how nature is, we also ignore how humans have affected it. For example, if we want to answer "Why were higher carbon dioxide levels measured in 2017 than in 1967?" ignoring the changes in the atmosphere would be extremely misleading and dangerous.

Of course, there are historical natural sciences, as Kuukkanen points out (2016, 8–10). Nature has changed (even in the short period of time of existence of *Homo sapiens*). However, knowledge of how nature was different in the past surely belongs to the explanatory resources of the current sciences. Moreover, if one claims that science does not adequately capture how nature was in the past since the possibility of fundamental change in the nature cannot be excluded, this amounts to nothing but *Humean* skepticism towards the very basic uniformities of nature. Neither scientists nor historians (or anyone else, for that matter) could work on the assumption of such a chaotic world.

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140 The explanatory resources of human and social science should also be used. I focus on the natural sciences, as these are more controversial in historiography. Everything said below applies also to the use of other sciences in the historiography.

141 I do not suggest non-physicalism. That discussion is irrelevant here.

Secondly, and more academically, historiography of science should be relevant for the scientific practices. At least, the ideal of such relevance should not be abandoned without strong reasons. Otherwise some of the great potential of historiography of science is wasted and, in addition, scientists are left with an ahistorical perspective which is hardly ever a good position. This would be the case if the explanatory resources of historiography of science were completely cut off from the explanatory resources of science. Scientists would have to live in two different explanatory worlds. In their scientific work, they would use the natural world as an explanatory resource, but in their historical reflections they would have to abandon their knowledge of nature and the explanatory trust put on that nature. It is difficult to see how these ways of life could be fruitfully connected. However, if the resources of historiography and science are seen as interconnected, there is no need for such a double life.

In the third place, the explanatory resources are, in fact, connected. For example, a scientist can use historical knowledge about some society to accept or reject a set of measurements. For example, if it is known that the instruments of that society were not detailed enough due to the technological state of the society, then their measurements cannot be trusted. Notice that (i) this is a historical claim about a society, and (ii) this historical claim is based on our understanding of nature (i.e. on our understanding of the instruments and how the world is such that instruments like those do not measure it adequately). If the scientists can use the resources of historiography and successfully theorize using those resources, it seems odd to claim that historians could not do the same with science. The explanatory resources must be understood as forming a unity. Moreover, once we take seriously the *historical* nature of science and understand that science is always build on previous developments, there is no meaningful way to separate the explanatory resources of science and historiography of science. If a scientist knows, as a part of her work, why certain measurements were made in the past, the scientist has knowledge about the history of science. Similarly, if a historian knows why some measurements in the past were politically manipulated, she knows why certain evidence must be rejected in science. One cannot know science and history of science separately if one understands the historical nature of science.

We can formulate *the super-symmetry principle*: everything in the history of science must be explained by using the explanatory resources that are shared between all explanation-seeking fields.[142] There are no disciplinary boundaries when it comes to the explanatory resources.

It must be noted that the use of scientific knowledge in historiography of science does not mean that we attribute our current beliefs to the historical agents. Science is about the universe around those past agents, not about the beliefs of the agents. Actually, this is why we need both science and historiography to understand the history of science: We need to combine what the agents believed, what their social and cultural structures were, and how the world was, in order to build a satisfactory account about the history of science. No single element is enough. Moreover, if we explain the developments of science by attributing current belief to the past agents, we simply fail in our explanatory task – the explanation would be simply false (see Section 3.4). Therefore, the causal approach offered in this book gives a *rationale* for avoiding such anachronist descriptions.

6.7 The Historiography of Science, Scientific Knowledge, and the Scientific Realism Debate.

As the knowledge of the natural world needs to be *part* of the explanatory resources (of course, it is not *the only resource* or a *resource that must be always used*) of historiography of science, the question of how to express that knowledge arises. I argue that the best way to express the facts about the natural world is to think in accordance with the framework of scientific realism, consisting of three theses:

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[142] The original symmetry principle is formulated by Bloor: "[Sociology of scientific knowledge] would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs" (1991, 7).

> *The Metaphysical Thesis*: The world has a definite and mind-independent structure.
>
> *The Semantic Thesis*: Scientific theories should be taken at face value. They are truth-conditioned descriptions of their intended domain, both observable and unobservable. Hence, they are capable of being true or false. The theoretical terms featuring in theories have putative factual reference. So, if scientific theories are true, the unobservable entities they posit populate the world.
>
> *The Epistemic Thesis*: Mature and predictively successful scientific theories are well confirmed and approximately true of the world. So, the entities posited by them, or, at any rate, entities very similar to those posited, inhabit the world. (Psillos 1999, 4).

The advantage of this framework is that it allows the discourses in historiography of science, in science itself, and in the everyday life to have a similar (surface) structure. Moreover, it helps to separate the philosophical interpretation of our explanatory resources from their normal use. For example, consider the following explanations:

> (I) The measurement of high levels of carbon dioxide were made because there was a certain amount of carbon dioxide in the atmosphere.

This explanation appears to be committed to the real existence of carbon dioxide. However, this explanation can be read in different ways:

> (Ib) An instrument I had the reading R because the conditions C [one replaces "C" with the preferred interpretation of "there is an amount A of carbon dioxide in the atmosphere"] obtained.

One can then say "Had C not been the case, the measurement would not have been made" and specifies that the antecedent is the case due to an intervention. (For example, two persons could agree that burning less fossil fuels would count as an intervention even if they disagree on the reality of carbon dioxide.) For every realist-looking claim there must be a corresponding re-interpretation as long as the disagreement is philosophical and not scientific (both sides accept scientific theories and views but disagree on their interpretation, see below). Therefore, the account of explanation that was explicated in the previous sections does not commit one to full-blown scientific realism. All that is needed is a commitment to dependency relations holding between two states of affairs that science captures with more or less ontological depth. However, in providing explanations, it is much easier to speak about carbon dioxide as if it were a kind of entity that exists in a mind-independent world. Even if the talk about carbon-dioxide must be – in a philosophical sense – interpreted in non-realistic way (as anti-realist would claim), our explanatory resources must be packed so that we are able to use simple language in explanations and see the connections between different explanations.

The philosophical point lurking behind these considerations is that the philosophical considerations have no bearing on the first-order questions about which kind of things are explanatory in this or that historical research and how to communicate explanatory knowledge.[143] One cannot claim, for example, that we should not cite carbon dioxide in a historical explanations *because* the realistic interpretation of the theories involving it is not possible (assume this impossibility for the sake of argument). The debate between scientific realism and another position is not whether to accept or reject certain scientific theories – that is a scientific question – but how to interpret them. *Our explanatory resources are what they are – we have a certain very successful network of explanations – and a philosophical interpretation does not change it.* Only skepticism toward science could lead one to abandon such an explanatory framework, but it is difficult to understand how this skepticism could be derived from explanatory considerations in historiography of science. Paradoxically, even the pessimistic induction, the argument that, since there were successful theories in the past that were not true, we cannot infer the truth of the present theories from their success (see Laudan 1981), implying that we can be successful even if the present sciences are not connected to

[143] See Tosh (2007, 192-193) arguing for similar view.

the reality in an adequate way.[144] Therefore, there is no reason to abandon the explanatory resources of the sciences (and, of course, this was never the target of the pessimistic induction). Only if we claim that *genuine* historiographical explanations are those that cite only *really* (in realist sense) existing factors, can we abandon the explanatory resources of the present science due to the possible collapse of realism. However, if this was our definition of genuine historiographical explanation, then there probably would be no such explanation if the realism collapses, as we can conjecture that the collapse of scientific realism would also mean the collapse of historiographical realism. Historiography is hardly *more* oriented towards mind-independent world than science. Now, if our notion of *genuine explanation* implies that there are no explanations, there is something wrong with that *a priori* notion. Therefore, the use of the explanatory resources of science cannot be abandoned. (I return to this topic in Section 6.8.)

It must be noted that a realistic attitude does not imply that the past theories must be judged as similar (in their content) to the present theories.[145] Thus, Arabatziz' argument that

> [Scientific realists] are in a historiographically awkward predicament: they are compelled to maximize the continuity between past and contemporary science. To put it another way, they are hard-pressed to portray past successful scientific theories as imperfect versions of their contemporary descendants. In other words, they are forced to embrace Whiggism, a

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[144] Laudan is criticizing the *No Miracles Argument*: "The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle. That terms in mature scientific theories typically refer [--], that the theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories – these statements are viewed by the scientific realist not as necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate scientific description of science and its relation to its objects." (Putnam 1975, 73.) (See also Psillos [1999] and [2009] discussing this argument.)

[145] An analogy: even if psychology can explain some belief of mine, this surely does not mean that the content of my belief must be similar to the psychological theory. See also the end of the Section 6.6.

> historiographical stance rejected by the overwhelming majority of historians of science. (2018, 36)

cannot be used against connecting the explanatory resources of historiography of science to the resources in other fields. If one defends scientific realism against pessimistic meta-induction or other historical challenges, then a move described by Arabatziz is arguably required. However, we saw that from the perspective of the philosophy of historiography of science, the issue is not whether or not realism is defendable position. Rather, the issue is how to effectively connect historiography of science to other fields of inquiry.

To sum up, our explanatory resources should be seen forming a continuous whole and used as if they were descriptions of mind-independent reality with rich ontology. An eloquent way to communicate explanations in historiography of science is to use realistic mindset. Yet debates concerning the viability of scientific realism are irrelevant for the explanations in historiography of science. Knowledge about nature can (and should) be used in historiography of science even if one does not accept scientific realism. An argument against scientific realism does not imply that scientific knowledge cannot be used in historiography of science. As we will see in the next section, a much stronger scientific case must be made if one is willing to exclude, say electrons, from the explanatory resources of historiography of science.

6.8 Millikan and the Electrons

In this section, I illustrate how one widely discussed claim can be analyzed using the framework developed in the previous sections.

In the history of science, it has been claimed that the way the world is – nature – cannot explain our beliefs about it. In this section, I show that this conclusion cannot be made.[146]

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[146] More detailed discussion of this issue is found in Bouterse (2016).

David Bloor writes:

> If we believe, as most of us believe, that Millikan basically got it right, it will follow that we also believe that electrons, as part of the world Millikan described, did play a causal role in making him believe in, and talk about, electrons. But then we have to remember that (on such a scenario) electrons will *also* have played their part in making sure that Millikan's contemporary and opponent, Felix Ehrenhaft, *didn't* believe in electrons. Once we realize this, then there is a sense in which the electron 'itself' drops out of the story because it is a common factor behind two different responses, and it is the cause of the difference that interests us. (Bloor 1999, 93).

First, notice that, in the explanatory account of previous chapter, the question "Why y_1 rather than y_2?" requires that y_2 is a counterfactual alternative to y_1. Therefore the explanation-seeking question "Why did Millikan believe but Ehrenhaft did not?" is an elliptical one. We need to fulfill the contrast; and that can be (i) that neither of them believed; (ii) both believed; (iii) Ehrenhaft believed but Millikan did not. It seems natural to suggest that (ii) is the implicit contrast. If we are interested in explaining *the actual difference* in Millikan's and Ehrenhaft's beliefs, we probably want to know in what counterfactual situation they would have had the same believes. This, however, does not change much, as explanations are supposed to answer many what-if-things-were-different questions.

Take

P	=	the presence of an electron
M	=	methods that Millikan actually used
F	=	methods that Ehrenhaft actually used
B_m	=	Millikan believed in electrons
B_e	=	Ehrenhaft believed in electrons

Take the following set of counterfactuals:

1. Had P not been the case[147] and had Millikan and Ehrenhaft used M, neither B_e nor B_m would have been the case.

2. Had P not been the case and had Millikan and Ehrenhaft used F, neither B_e nor B_m would have been the case.

3. Had P been the case (as it actually was) and had Millikan and Ehrenhaft used M, both B_e and B_m would have been the case.

4. Had P been the case and had Millikan and Ehrenhaft used F, neither B_e nor B_m would have been the case.

5. Had P been the case and had one of the scientists used F and other one M, either B_e or B_m would have been the case.

This set of counterfactuals shows that the electron is in itself causally relevant in the situation. Change the situation by taking the electron out and fix the methods that the scientists use, and the belief of Millikan will change. This means that electrons were causally relevant to the belief of Millikan and can thus be used in explanations. Nothing in the structure of the explanation forbids historians from using it.[148] More-

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[147] It might be a counter-possible (and not just counterfactual) to assume that electrons did not exist at all. This would be the case if electrons were taken as necessary beings. However, (i) the necessity of electrons is hardly something that advocates of *the strong programme* would accept (i.e. they would probably deny that science is in an intimate connection with necessary structures of the world), and (ii) we can specify the scenario as *local* (concerning particular electrons in an instrument) rather than *global* absence of electrons. The local absence of (free) electrons could have been brought by omission of ionizing radiation. The ability to make such distinction is an advantage of interventionist thinking in historiography of science.

[148] This shows that no matter how the details of particular examples are analyzed and who got it right in the Kochan-Tosh-Lewens debate (see Kochan [2010], Tosh [2006] and Lewens [2005]), the

over, counterfactual (5) tells us when it would have been the case that Ehrenhaft believed in electrons and Millikan did not, and the counterfactual (3) tells when both would have believed in electrons. The electron does not "drop out". Answering what-if-things-were-different questions requires that we do not ignore electrons: if we ignore electrons, we cannot distinguish the antecedents of 1 and 3 even though they have different consequents. Moreover, the factors cited in each counterfactual 1-5 belong to same kinds. This means that the explanatory structure is *super-symmetrical* (see Section 6.6).

Surely, one can give a *minimal explanation* that does not cite the electrons. However, an explanation that cites the electrons *and* the methods is a deeper one according to our principle (see the previous chapter):

> Explanation E is deeper than E*, if E provides information about variable Z that is not provided by E*.

Of course, if we compare two minimal explanations, E_1 citing the methods and E_2 citing the electrons, we can say that E_1 is sometimes the deeper explanation. Here we follow the principle from the previous chapter:

> E is deeper than E*, if E relates X and Y, and E* relates Z and Y, and the values of X are more interesting than the values of Z (i.e. E gives more interesting information).

and assume that information about methods is more interesting than information about electrons. However, it seems that in a case like this, where we evaluate only very few variables, there is no need to choose between E_1 and E_2. It is more natural to connect them in order to formulate a deeper explanation. The case would be

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idea of contrastive explanations is compatible with the citing of facts about nature in our explanations in the historiography of science, if we choose the interventionist account to analyze causal explanations.

different if E and E_2 were not minimal explanations but genuine historical explanations.[149]

Perhaps there is an epistemological concern: Since we believe in electrons because of Millikan's work[150], we cannot use our belief in electrons in the explanation of Millikan's belief without circularity. We have no access to the reality that could confirm – independently of our Millikan-based tradition – whether or not electrons exist. (E.g. Kochan 2010, 136.)

There is no circularity here. It is not *our belief about the electron* that the explanation cites but *the electrons*. If electrons exist, then Millikan was exposed to them (or to conditions where electrons were present according to our theories of physics, see Section 6.7) and our electrons-citing explanation of his belief is right. Whatever our level of confidence toward the existence of electrons is, the true value of the explanation does not change. Most certainly we cannot say that electrons did not affect Millikan because we are not absolutely sure that they did. Ignoring a possible factor is never the right way to proceed, as we saw in the previous chapter.

It is also unclear whether we can justify a theory about some entity without at the same time committing ourselves to the explanatory status of that entity. We ask "What would an entity E cause in experimental setup S if E existed?". If we then infer that it would cause phenomenon P, and if we observe P, then it seems that we have evidence for the explanatory status of that entity. (The exact form of the inference need not bother us here; the point is that it is difficult to see how there could be evidence of the existence of an entity without some (supposed) causal link between the entity and the evidence). If, after many such inferences, we accept the theory about the entity, then we, at the same time, accept that it was a causal factor in our setup. It would not be reasonable to ask "Well, we accepted the theory about the entity. But does it have any causal role to play?" as if we could separate these issues.

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149 These notions are defined in the previous chapter.

150 We can assume this for the sake of simplicity.

Moreover, if we admit our uncertainty – whatever this means - about the existence of electrons, not only are we forced to abandon our explanation of Millikan's belief but also every other explanation that cites electrons and their properties. Because the fabric of scientific theories that use electrons in their explanations is very successful, this is an unreasonable thing to do. Once we acknowledge that electrons are worthy of belief there is no reason why they cannot be used in the case of Millikan.

One does not even need to be a scientific realist, arguing from success to truth, to make the point. The only thing that is required is that the use of our explanatory resources is not arbitrarily divided between historiography of science and the natural sciences. Therefore, the formulation of underdetermination thesis that "the world can sustain diverse, and even contradictory, descriptions" (Kochan 2010, 133) does not do any trick here. Even if we accept the underdetermination thesis (despite the harsh criticism[151] it has faced), this has no bearing on the explanatory status of electrons. As we saw in the previous chapter, the debate between scientific realism and other position is not whether to accept or reject scientific theories – that is a scientific question – but how to interpret them. Our explanatory resources are what they are – we have a certain very successful network of explanations – and a philosophical interpretation does not change it. To see this, assume that under-determination thesis shows that claims about electrons, such as "electrons explain Y", cannot be interpreted as referring to entities and facts about mind-independent reality. If we were to reject explanations citing electrons because of this, then we should accept that natural sciences have not provided explanations when they claim "electrons explain Y". If we accepted this, then our philosophical theory of explanation would have led to the conclusion that the fields (natural sciences) that are taken to be exemplary places where explanations are found do not provide explanations after all. One should wonder what the "explanations" are that the philosophical theory talks about and what the value of such *a priori* theory is.

[151] E.g. Laudan (1990), Okasha (2000).

Moreover, if underdetermination is true and we can choose whichever theory we want, we can equally well choose a theory that talks about electrons. Once this choice is made, we need only to be consistent with the choice in order to use electron-citing explanations. Ironically, nothing can stop us from using electrons in our explanations, if the under-determination thesis is true.

The only argument that is left for not using nature in explanations in the history of science is that this restricts the explanatory space open to historians (Kochan 2010). Since acceptable explanations must refer only to what is true (or accepted, for that matter) and have certain structure, this worry about a restriction must be read as a methodological norm rather than as an attempt to justify every possible explanation as acceptable. As a methodological claim, it says: "Try using different sorts of factors in explanations in order to find those that suit the best". This claim makes sense: the only way to find out whether, for example, economic or political factors are generally more explanatory, is to formulate explanations in terms of these factors and see which set of explanations gives us the most coherent worldview. This was shown in the previous chapter. However, it is difficult to see how electrons could drop out from the explanations once this methodological framework is in place. Electrons are so central to our worldview and explanatory practices that abandoning them would mean a complete change of worldview. Historians of science are probably not the ones who can accomplish a change of this magnitude. Thus, we had better stick with the electrons and the rest of nature.[152]

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[152] Surely there might be cases where we are less confident about a description of nature produced by science than we are in the case of electrons. In such cases our historical explanation, citing features of nature that are a part of this description, would stand on shakier grounds. However, there is nothing problematic in the idea that sometimes our explanations, including historiographical ones, are not completely reliable. In such cases, only time can tell – as we continuously update our explanatory resources – whether the explanation was correct or not. It is not compelling to claim that because we are sometimes on shaky grounds, we are always on shaky grounds. The issue must be solved explanation-by-explanation.

6.9 Conclusion

We have seen that an explanation in historiography of science has the form:

y_1 rather than y_2, because x_1 rather than x_2.

In this chapter, an account of explanation was built around this basic notion. This account was explicated mainly as a tool for causal thinking in historiography of science. In Chapter 5, we saw that there are debates concerning the nature and prospects of historiography of science (and science studies in general) that require clarification and systematization. The account here provides the needed clarifications and systematizations, as the account (I) is completely indifferent to the nature and scope of the thing to be explained; (II) can be used independently of assumptions about the rationality in/of science; (III) is able to accommodate explanatory factors from multiple categories; (IV) defines notions of explanatory relevance and depth; (V) explicates how the explanatory resources should be managed.

Moreover, such a list is only the beginning of a philosophical work. There is probably a great number of problems related to historiography of science, some of which we might not be able even to think of yet, that require analysis. It is, therefore, valuable to have a general framework of explanations in place. Such a framework carries the promise that the philosophical problems can be approached systematically.

In the next two chapters, we will see how the framework can shed light on existing issues in history and philosophy of science. Both chapters illustrate the point of the previous paragraph: philosophical problems related to historiography of science present themselves from different perspectives once we approach them with an explicit philosophical framework.

7 SHEER WONDER WITH SOUND METHODOLOGY. ON LOCAL EXPLANATION

In this chapter, I offer an explication of the notion of *local explanation*. In the literature, local explanations are considered as metaphysically and methodologically satisfactory: local explanations reveal the contingency of science and provide a methodologically sound historiography of science. However, the lack of explication of the notion of *local explanation* makes these claims difficult to assess. The explication provided in this chapter connects the degree of locality of an *explanans* to the degree of contingency of the *explanandum*. Moreover, the explication is shown to be compatible with the methodological need for general consideration in historiography of science. In this way, the explication (i) satisfies the need to explicate an important notion, (ii) connects local explanations and contingency, and (iii) enables us to see how local explanations and general considerations can be connected.

7.1 The Need for an Explication

The notion of *local explanation* is a topic of wide interest in historiography of science. James Secord writes:

> As will be evident to anyone who has looked over publishers' catalogues in recent years, historians of science have developed superb techniques for placing science in local settings of time and place. A standard model for historicizing science is to locate specific pieces of work in as tight a context as possible, binding them ineluctably to the conditions of their production. (2004, 657.)

Moreover, Peter Galison notes that "the turn toward local explanation in the historical, sociological, and philosophical understanding of science may well be the single most important change in the last thirty years" (2008, 119). The idea behind the local focus is "that a science constructed in a locality reflects that locality and possesses different characteristics from a science conducted in another. This thought also has fundamentally changed how one explains the practice of science." (Kuukkanen 2011, 591.)

But what is meant by local explanation? Kuukkanen writes: "In general and as a first approximation, [*local explanation*] refers here to all the positions that regard science and/or scientific knowledge as local in some sense, or explains them by reference to locally existing factors." (2012, 478). This definition allows us to separate two lines of philosophical problems. We need to distinguish between (1) the explication of the notion of *local explanation* and (2) the question of local applicability or validity of science. Even if science is explained by locally existing factors, it does not follow that science is only locally valid or applicable. A further argument is needed to establish the link. Moreover, the line (2) is related to the questions of globality, circulation and movement of scientific knowledge. Often the discussions concerning local explanation focus on how locally produced science became global.[153] I do not discuss the issues related to the line (2). In this chapter, I will focus on explicating the very notion of *local explanation*. The lack of an adequate explication of that central notion is a philosophical concern in its own right and the issues concerning the line (2) cannot be discussed without such explication.

It seems that local explanations are attractive for two reasons. First, local explanations are viewed as "metaphysically" satisfactory as they seem to describe in detail how science works.[154] A very important aspect of this satisfaction seem to follow from the supposition that local explanations seem to provide us with a perspective on the contingency or even fragility of science. Local studies produce sheer wonder in the audience. Secondly, it has been claimed that local explanations are epistemologically preferable to their alternatives. Kuukkanen argues that

[153] E.g. Galison 1997; Secord 2004; Kuukkanen 2011.

[154] I use "metaphysical" in a relaxed way here to refer to the idea that local explanations do not explain only their intended *explananda* but also show something (allegedly) deep about science and human practices in general.

> [On] the methodological level, the essence of localism can be said to be its rejection of *a priorism* in historical studies of science. It is a movement for more intensive empiricism which, as a consequence of this, rules universalism out as an inaccurate and unwarranted description of science (2012, 481).

According to universalism, "science and at least some of its production conditions are universal" (ibid, 487).

Suppose that everything exists *somewhere*, i.e. everything has some location. It follows that every explanation cites only locally existing factors. This proves that everything is explained by local factors. There is no need for a distinct notion of *local explanation*. The end of the story. Not so quick! This "trivial localism", even if true, does not capture the supposed aspects of local explanations: that they show the contingency of science and that they are methodologically preferable explanations. Explaining something does not automatically reveal the contingency of the *explanandum* which means that local explanations differ from other explanations because they are supposed to reveal the contingency of science. Lorraine Daston writes:

> Probably most historians of science these days, if asked about an episode [--] would answer that such scientific practices are both socially constructed and real. [They] capture some aspect of the world; they work. But they are neither historically inevitable nor metaphysically true. Rather, they are *contingent to a certain time and place* yet valid for certain purposes. [2009, 813. emphasis added]

Also Kohler and Olesko argue that:

> This understanding [that science is a product of the society that creates and harbors it] was achieved not by abstract reasoning [--] but empirically, by detailed study of local sites of knowledge making. These showed concretely how scientific findings were the *products of particular local situations and communal practices with all their historical and social contingencies*. (2012, 3 [emphasis added])

This relationship between local explanations and the contingency of science is not just a byproduct of the localist historiography. The wonder that contingency produces is great motivation for localist studies:

> The contemplation of historicity – of the sheer singularity of places and times, situations and conjunctures, including all those you habitually take for granted

> – will help you see that there are different ways of looking at the world, and that what is obvious in one perspective may be ridiculous in another. (Rée 1991, 961.)

Moreover, Kuukkanen argues that "[the *methodological localism* studies] bounded localities in order to acquire knowledge that would otherwise be difficult to formulate or be without proper warrant" (2012, 478). Local explanations are not just a better way to understand what happened in a particular location but they also carry the promise of "well corroborated (general) knowledge, the validity of which extends beyond a particular locality" (Kuukkanen 2012, 484). There must be a distinct category of *local explanation* if such explanations are methodologically better than other explanations.

That local explanations are methodologically preferable and that they reveal the contingency of science indicates that the "trivial localism" is not what grounds the category of *local explanations*. What is striking is that, despite the general historiographical interest in local conditions and despite all the philosophical discussion about the notion of *explanation*, detailed analysis of the notion of *local explanation* cannot be found in the literature. In this chapter, I offer an explication of that notion. My strategy is to take a general account of explanation and use that account to explicate how the *local* in the *local explanation* can be understood. The assumption is that local explanations form a distinct class of explanations. As we saw, this assumption is made in order to avoid a trivialization of the notion on *local explanation*. One aim of this chapter is to satisfy our basic philosophical need for exact notions.

However, we have seen that there are other needs that such an explication must satisfy. To motivate the explication further, I argue that without a detailed explication of the notion of *local explanation* we cannot understand what the connection between local explanations and contingency is. I will provide an explication of the notion of *local explanation* that (i) interprets locality as a matter of degree, and (ii) connects the degree of locality of an *explanans* to the degree of contingency of the *explanandum*. My argument is that *if we assume* that there is a connection between local explanations and contingency, then the notion of *local explanation* must be understood in particular way to guarantee that connection. Notice that what are described as local explanations in the literature may not satisfy the explication given here. This also means that the historiographical studies that claimed to establish the contingency of science may not achieve much on that front. This is in line with the general aim of my book, that is, to provide analytical tools that help to interpret the implications of historiographical studies.

Moreover, I argue that the methodological status of local explanations is not as clear as is supposed. I will point out three fundamental roles that generalizations and general considerations play in historiography of science as a field. Without generalizations and general considerations, the methodology of historiography of science remains inadequate. We need to be careful when explicating the notion of *local explanation* in order to keep the explication compatible with the need for such generalizations and considerations. The caution stems from the fact that, while generalization require that factors fall into general categories, local explanations put so much focus on a particular context that the general categories seem irrelevant and therefore the factors in the context are easily treated as unique. To establish my own explication, I will argue that *strong localism*, "a form of particularism, according to which only particular or individual objects exist, and therefore, there are no universals [--], conditions in which science is produced are unique" (Kuukkanen 2012, 485), is not compatible with generalizations and general considerations. Moreover, I argue that the explication offered in this chapter differs from strong localism which allows us to capture both the uniqueness of locations and the need for general considerations. As a surprising result, we will see that moderate forms of localism and universalism are compatible and even symbiotic. However, it also follows that local explanations are methodologically preferable to only a crude universalism that attempts to capture science once-and-for-all by very limited set of factors.

It must be noted that my explication is going to be somewhat revisionary, as it does not intend to capture how the notion of *local explanation* is in fact used but how it can be connected with the notion of *contingency* and with the methodological issues in historiography of science. However, I will illustrate this with an example from the historiography of science that the explication can be closely connected with the existing historiography.

7.2 Local Explanations and the Contingency of Science

In Chapter 6, we saw that explanations answer questions of the form "Why X rather than Y?" by pointing out factors Z and W such that "Had W rather than Z been the case, Y rather than X would have been the case".[155] Explanations are explications of counterfactual dependencies and contrastive in nature.

Let C be the set of all factors such that: had any of these factors (or any subset of them) been different, Y rather than X would have been the case. Now, let CL denote the subset of all the factors in C such that: had any of these factors (or any subset of them) been *located differently*, Y rather than X would have been the case. CL answers what-if questions and therefore explains. Then we can say that:

> (Initial explication): The more factors there are in CL, the more local the explanation of "Why X rather than Y?" is.

Take an example (1): Why was a wallet picked up on a street in New York? There are many factors, including: because the wallet was in the location L and because there were people in L. The location of the wallet does not belong to CL whereas the crowd in L does belong. Had the wallet been on a different street, it would still have been picked up; had there been no-one on the location L (had the people been somewhere else), the wallet would not have been picked up.

Next, consider second example (2): Why was a rare bird spotted in New York?

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[155] I have modified the terminology for the purposes of this paper. In Woodward's theory, an *explanans* consists of (i) a value(s) of a variable(s) and (ii) a "test-invariant" explanatory generalizations. Explanations relate changes in the *explanans*-variables to changes in the *explanandum*-variable. (Woodward 2003, 403).

There are many factors, among them: because the bird was in location L and because an ornithologist was in L. Both factors belong to CL: had the bird been located differently, it would not have been spotted; had the ornithologist been located differently, the bird would not have been spotted.

We can notice that only if *none* of the factors in CL was located differently, then the bird would have been spotted (X) rather than not (Y). This means that if CL is complex, then there are many possible changes in the locations of particular factors that would have changed X to Y. If many such changes are possible, then the occurrence of X seems to have been fragile: it depended on the right occurrence of all the factors in a particular location. This seems to explain the close connection between local explanations and the contingency of science.

However, we need to be somewhat more precise here. The contingency of X depends on how plausible it was that some W (leading to Y) happened (see Chapter 8 for discussion). It might be that there are many factors in CL and yet X was rather inevitable. This is the case if the situations where the factors in CL are located in relevant alternative locations are not plausible. For example, we can say (contrary to our earlier thoughts) that in the example (1) the location of the wallet could have been such that the wallet was not picked up. If the wallet had been located at the bottom of the ocean, it would not have been picked up. Here, CL contains two factors: the location of the wallet and the location of a crowd of people. Yet, it was not contingent that the wallet was picked up. Both a particular street being empty in New York and the wallet ending up at the bottom of the ocean are somewhat implausible scenarios. On the other hand, in the example (2) there are also two factors in CL: the location of the bird and the location of ornithologist. It seems that both the bird being somewhere else and the ornithologist being somewhere else are plausible scenarios. Therefore, the spotting of the bird was highly contingent. Of course, the difference here is a matter of degree. It is not impossible for a wallet to end up at the bottom of the ocean or that a street be empty. However, these are less plausible situations than that of an ornithologist or a bird being in different places.

Let P_f denote the set of places where a factor f (that belongs to CL) can be located. Let P_f* be the subset of P_f such that: had f occurred in a place that belongs to P_f*, Y would have been the case. We can say:

> (Explication): The more plausible it is that some factors that belong to CL occurred in places that belong to their respective P_f*s, the more local is the explanation of "Why X rather than Y?"

This explication connects the degree of locality with the degree of contingency.[156] But is it *ad hoc*? I do not think so. The explication says that we have a local explanation when a plausible change(s) in the location(s) of a factor(s) would have changed the outcome. Local explanations are supposed to be detailed and concrete. This focus should be extended to the modal sphere and therefore the explanatorily relevant possibilities should be fairly close to the actual course of events. It is disproportional to jump from the actual events to far-fetched scenarios while writing historiography with a detailed local focus. Moreover, if the explication above is *ad hoc*, then local explanations and the contingency of science are not closely connected. This would not be a worrisome result - it would be rather interesting to notice that local explanations produce only an illusion of a contingency. However, one should wonder what explanatory role locations play if one does not restrict the relevant changes in them. The presence of a telephone keeps me alive: had there been a monster rather than a telephone, I would not be alive. What is the explanatory role of a telephone here? Is it not the absence of a monster that is really relevant? Similarly, an explanation citing a far-fetched change in the location of a factor should make one wonder whether the explanatory work is done by something else than the location, perhaps by the fact that explanatory factor existed in the first place.

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[156] The claim is not that only local explanations show the contingency of science. If there is a plausible change in location that changes the *explanandum*, then there is contingency. There follows from the more general claim that there is contingency when some plausible change in *explanans* changes the *explanandum* (see the next chapter).

A further issue is whether the existing historiography of science that claims to produce local explanations satisfies either the initial or the fully-developed explications and whether the local explanations which claim to point toward the contingency of science satisfy the fully-developed explication.[157] However, at least sometimes historical explanations satisfy the explications. Let's take an example from the historiography of science to illustrate the issues in this section. In the paper ″Distrust and Discovery: The Case of the Heavy Bosons at CERN″ (2001), John Krige describes "the microhistorical process whereby different groups of scientific actors [--] came to claim that a new fundamental particle (the W boson) had been discovered at CERN" (2001, 517) The paper illustrates how factors including the theoretical background, the personal qualities of the scientists, the pressure from the funding agencies, and the competition for prestige affected how the W boson was discovered and how the results were announced. The microhistorical focus of Krige's study *prima facie* connects it to the family of local explanations. As Galison's discussion about local explanations points out, a microhistorical explanation is a very typical local explanation (2008, 120).

Krige's paper is complex. However, we can focus on (i) the technology at CERN, (ii) the political pressures, (iii) Carlo Rubbia, and (iv) certain decisions. The decision of CERN to search for the W boson was due to a technological advantage over the competitor, Fermilab, and due to problems with the image of CERN (Krige 2001, 522-523). Once that decision was made, the CERN directorate decided to perform two experiments because (i) the most advanced technology was uncertain, because (ii) political situation required the participation of many scientist and (iii) because the directorate did not trust Rubbia (ibid. 525-528). However, two different experiments did

[157] For example, Paul Elliot has concluded in one of his papers that "Far from being disembodied, placeless, abstract conceptions, the evolutionary theories of Erasmus Darwin and Herbert Spencer, which through the latter exerted a global influence, were rooted, shaped, and developed in the social, landscape, and industrial character of the English Midland provinces and the scientific communities they nurtured" (2003, 29) even though a closer look reveals that, throughout the paper, Elliot describes in detail connections between ideas and worldviews that were moving around Europe.

not matter much in the end: Rubbia suddenly decided to publish results before adequate scientific work had been done to check those results (ibid. 533-535). Once that decision was made, other people were forces to adapt to the situation due to political and institutional situation (ibid. 535-537).

How local is this explanation? Plausible changes in the location of technology or political processes probably would not have changed the process or the outcome. Scientists would have gathered around the technology and political information would have travelled anyway. However, suppose that the decision to search for the W boson had been made in Fermilab. This would have changed the technology used in the experiments and the political and institutional context. Perhaps the W boson would not have been found; perhaps there the results were never published due to lack of clarity in the data. If either of these is a credible outcome, we can say that the location of the decision might be an explanatory factor. Moreover, suppose that Rubbia was located somewhere else than CERN. This would have decreased the need for two experiments and probably there would not have been a sudden turn. Without two experiments or Rubbia's maneuvering, the process of experimentation or the outcome could have been different. If either of these is a credible outcome, the location of Rubbia might be an explanatory factor. Moreover, both (i) the change in the location of the decision and (ii) the change in the location of Rubbia seem plausible. Fermilab considered performing the experiment (ibid. 521-523) and perhaps Rubbia would have worked in that project.[158] Moreover, human life is contingent, and some event could have affected Rubbia's presence in CERN's experiment. That these scenarios are plausible means that the location of the decision and the location of Rubbia are explanatory factors.

I hope that this brief discussion indicates that the explication of the notion of *local explanation* formulated above is not trivial nor too complex. The discussion also shows how the question of explanatory relevance of locations can be approached in historiography of science. Many questions concerning the relevance of the locations of the

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[158] Notice that his idiosyncrasies would not have forced everyone else to adapt to a rushed action, as the political and institutional situation would have been different.

factors in the process leading to the announcement of W boson was left open but we saw where the answers might be found.

7.3 The Need for Generality

The idea that general laws are needed for causal judgements goes back to at least Hume. Hume thought that every causal judgement is based on an observed regularity, and a singular causal judgement is always an instance of such a regularity (T 1.3.14). In the famous covering-law model, general laws must be cited in every explanation (Hempel & Oppenheim 1948; Hempel 1952). A more subtle thesis is formulated by Davidson: A true singular causal claim is entailed by the premises citing the occurrence of cause and a true causal law, once the cause and effect fall under suitable descriptions (1967, 701). We cannot accept these views in their original form in the philosophy of historiography. The views of Hume and Hempel would imply that there are no causal explanations in historiography of science, whereas Davidson's view would imply that the search for reductive descriptions is the main epistemological task in establishing historiographical explanations.[159]

However, even if we do not accept the views above as they stand, this does not mean that we can have a complete methodology of explanations in historiography of science without some place for generalizations or general considerations (i.e. considerations that are essentially about *kinds* of things and relationships between those kinds). In this section, I will not examine the general philosophical questions about such generalizations. Rather, I will point out some essential roles that (admittedly weak) generalizations play in the historiographical thinking and in the structure of historiography of science as a field. Once these roles are explicated, we see that the explication of the notion of *local explanation* formulated above is a methodologically

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[159] Of course, Hempel described historical explanations as "explanation sketches". I do not see any reason why explanations, having the form presented in the previous chapter, would not be genuine explanations. See Woodward (2003, 4.9) discussing this issue.

sustainable explication. In the conclusion (Section 7.4), I discuss whether local explanations are methodologically preferable and, if so, preferable to what approach.

To clarify my position, I contrast it with *strong localism*, "a form of particularism, according to which only particular or individual objects exist, and therefore, there are no universals: [--] conditions in which science is produced are unique" (Kuukkanen 2012, 485). Strong localism implies that there are no categories of (relevant) causal factors and therefore no historiographical (causal) generalization. If there were such categories, the conditions would not be unique to the relevant extent, as there would be universals or resemblance-relations that ground the categorization. My argument is the following: Assume that we can write about events in the past and warrant singular causal judgements even in the absence of categories and generalizations. Even in this case we need generalizations, and therefore categories, to direct and control historiography of science in order to achieve important explanatory ends. Therefore, even if strong localism did not make the world unintelligible (which I doubt), we would still need generalizations, and therefore an alternative explication of localism.

1. As I argued in Section 6.6, historiography of science is not an ahistorical practice. It builds on the results of previous studies and there have been discernible trends in the field. These developments are not just a mindless river where the historians swim; they are conscious choices. The developments are based on a search for deeper understanding and unified pictures. Such a search cannot proceed if nothing guides the research question and unifies the answers.

In Section 6.6, we used the term *explanatory resources* to denote the set of kinds of factors that can be used in an explanation in historiography of science. The explanatory resources can be seen as consisting of possible-cause hypotheses: they describe which kinds of factors are among the possible *explanantia* for some event or process. We also saw that the explanatory resources must somehow be managed. I suggested the following picture: The method of managing the explanatory resources is to balance the number of factors that provide explanatory understanding in particular historiographical cases with the general applicability of those factors. This implies that general considerations play an important role in the process of balancing the resources.

Consider an illustration: In the paper "Inspiration in the Harness of Daily Labor: Darwin, Botany, and the Triumph of Evolution, 1859–1868" (2011) Richard Bellon describes the process that led to the acceptance of Darwin's evolutionary ideas in the British scientific community. Bellon claims that the publication of *On th*e *Origin of the Species* in 1859 was not a decisive event in this process. On the contrary, the book was judged to be speculative in character and against the scientific and moral standards of the community. Only after publishing a study on orchid fertilization in which he applied his evolutionary ideas, did Darwin's framework meet with approval by the community.

Bellon is providing an answer to the question:

> Why were Darwin's ideas accepted by the Victorian scientific community rather than not?

Arguably, answering this question explains significant feature of science, the acceptance of Darwin's ideas. Even if the question is not directly about the present science, it is about a feature of science that, arguably, has had a wide impact on the present science.[160]

Bellon gives the following answer:

> The *Origin* was packed with evidence, but it communicated few entirely original scientific observations, and this allowed its critics to dismiss it as vainglorious speculation untethered from the manly discipline of original discovery. [--] If Darwin had not tied the theory of the *Origin* to productive, technical, and specialized research — in the event, his floral biology, but it could have been any number of things — the ultimate reception and received meaning of the *Origin* would have been significantly different. [--] science's social, political, and religious respectability depended on the governance of imagination by consistently patient and humble behavior and [--] Darwin's adversaries frequently used this ideology to bludgeon the *Origin*. Ultimately, Darwin vanquished his foes by reversing the weapon and claiming the mantle of heroic conduct for himself and his theory. (Ibid., 395–396.)

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[160] See Bowler (2013), a counterfactual history of what would have happened without Darwin.

It may seem that the explanation is a rather straightforward one, at least for a philosopher of science.[161] Darwin first introduced a theory in the *Origins*, but since the evidence for the theory consisted of "old evidence" rather than from novel discoveries that the theory entails, and since novel discoveries are important in the confirmation of a theory, Darwin's theory was not well confirmed and thus not accepted. What is more, Darwin's theory had not yet been shown to produce a progressive research program, and because it seemed like an improbable candidate to produce one, it was not accepted. Once Darwin published the *Orchids*, Darwin's evolutionary framework proved to be a fruitful research program and to produce novel discoveries and was accepted. Moreover, it seems plausible to assume that the way in which Darwin formulated his evolutionary framework is a factor without which it would have been impossible to make evolutionary ideas work in empirical research. "Darwin simultaneously illustrated the conceptual and methodological power of his theory and its prodigious ability to bring order to the study of natural history where older methods and concepts had failed." (Bellon 2011, 409.) With hindsight, we can say that only because Darwin got things right (to a relevant extent) in the *Origins* it was possible to use the evolutionary framework in empirical research.[162] The explanation of the acceptance of the evolutionary framework can be formulated as follows:

> (E1) Had Darwin not formulated an evolutionary framework very similar to that of the *Origins* or had he not shown in *Orchids* that this framework

[161] This simplified form of analysis can be made, for example, by following Lakatos (1978), Laudan (1977) or Kuhn (1977, 322). Needless to say, the discussion about details of confirmation and acceptance of theories and research programs has been enormous.

[162] See the section, "Historiography of Science and Scientific Realism," above. One can interpret the expression "got things right" in whatever way one's philosophical theory of science suggests. The distinction between theories that do work empirically and those that do not can be drawn within every theory. Philosophical issues about realism are issues about what features of theories explain this distinction.

> was successful and progressive (i.e. *fruitful*) in empirical research, the evolutionary framework would not have been accepted.

Yet there is a complication in the issue. Bellon also points out that before the *Orchids* were published, Darwin's work was dismissed on *moral standards*. Darwin did not seem to work on a patient, humble and honest manner and, therefore, was not taken seriously (Ibid. 403–407). Only after the *Orchids*, was Darwin accepted as a morally righteous scientist, and therefore the fulfillment of Victorian moral standards was an important factor in the acceptance of his theory. As Bellon points out, the requirements for novel discoveries and progressive research program (i.e. requirement of fruitfulness) were an embodiment of the Victorian value system and moral standards:

> [The] technical issues of scientific theory and method fed into larger Victorian preoccupations with ideal behavior. Refusal to acknowledge phenomena a favored theory did not fully explain did more than violate philosophical principles. It revealed an arrogant bending of nature to the theorist's notions, rather than a humble submission to the truth. A well-worn and culturally formidable vocabulary of praise existed in nineteenth-century British science: the words "cautious," "laborious," "painstaking," "exact," "humble," "disciplined," "earnest," and, above all, "patient" were pinned like medals on men of science and their work. (Ibid. 395.)

This explanation for the acceptance of Darwin's theory can be framed as follows:

> (E2) Had Darwin not lived in accordance with the values of the Victorian society, his evolutionary framework would not have been accepted.

In the previous chapter, we saw that E1 and E2 are not necessarily competing explanations. For example, if E1 is given with respect to explanatory goal "understanding epistemology of science" and E2 with respect to explanatory goal "understanding values in science", there is no real competition between them. However, we can still evaluate the explanatory depth of the two explanations.

In the previous chapter, we formulated the following dimension of explanatory depth:

> Explanation E is deeper than E*, if E would still be true while E* would no longer be true if there was a change in background conditions, due to the conceptualization of *explanantia*.

We also saw that the difference between E and E* is due to the fact that E is a better candidate for a possible-cause hypothesis than E*.

We can see that there is an asymmetry between E1 and E2. There are important changes in background conditions that would have made E2 false. For example, nothing in Bellon's paper indicates that had Darwin did something morally wrong that was not related to his scientific work, his theory would not have been accepted. In other words, it seems possible[163] that had Darwin done something morally wrong (perhaps some minor misbehavior) and produced his theory, his theory would still have been accepted. Moreover, it seems possible that had Darwin lived in another society, his theory would still have been accepted even if he did not follow the Victorian value-system.

However, it is difficult to figure out any change in background conditions that would have made E1 false. What should we add to "Had Darwin not proved his theory fruitful, it would still have been accepted" to make it possible? Of course, we can say that had Darwin lived in a society that considered fruitfulness as a reason to reject a theory, his theory would not have been accepted. However, it seems difficult to tell what kind of change this would have required. The claim seems to have the form "X would not have been the cause of Y if X occurred in the context where X does not cause Y", and thus lacks informative content. The problem is that we do not have a clear conception of what kind of intervention on a variable would have made E1 false. (See Section 6.2 discussing this topic).

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[163] I say "it is possible that" because the nothing in Bellon's paper does not establish that theory would have been accepted. However, the possibility is not excluded. In the next paragraph, we see that this possibility is enough to show which direction the explanatory resources should be taken to *on the basis of this one study* (see the introduction to this chapter on this methodology used here).

One way to look at the difference between E1 and E2 is that one can abandon the Victorian moral standards and still perform fruitful scientific work. However, it is not possible to abandon the standard of fruitfulness and still live in accordance with the Victorian moral standards as the standard of fruitfulness is an embodiment of the moral standards. The antecedent of E2 is never true when the antecedent of E1 is not true. However, the antecedent of E1 can be true even if the antecedent of E2 is not true.

The difference can also be seen if we consider the following hypotheses: (I) Theory T is accepted if it is fruitful, and (II) Theory T is accepted if it is formulated by a scientist living in accordance with the moral values of the society. There have been theories that have been accepted due to their fruitfulness despite the moral condemnation of the scientist. The case of Galileo Galilei is probably the best known..[164] E1 and the associated possible-cause hypotheses (I) seem to deserve their place in our explanatory resources. Producing fruitful results seems to be much more invariantly connected with the acceptance of a theory than the moral virtues of scientists. In Darwin's case, the close connection between fruitful scientific practice and moral virtues was a rather lucky occurrence.

However, cases such as *Lysenkoism* seem to indicate that the moral or political values of scientists sometimes do, in fact, matter more than the fruitfulness of a theory.[165] Therefore, it remains (to some extent) an open question whether fruitfulness or

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164 See McMullin (1998) discussing the condemnation of Galileo. Notice that someone could argue that in the case of Galilei, the theory was accepted because the moral atmosphere relaxed. This is of course hypothetical but if this were a justified claim, then we should re-evaluate the respective explanatory powers of *fruitfulness and moral righteousness*. This issue cannot be solved here; my account gives only the tools to approach such evaluations.

165 See deJong-Lambert and Krementsov (2017) for discussions about *Lysenkoism*. The *Michurinist biology* associated with *Lysenkoism* "openly contradicted the basic tenets of genetics, including Gregor Mendel's laws, Thomas Morgan's chromosomal theory, and the concept of the gene as a material unit of heredity, and supported the Lamarckian idea of the inheritance of the acquired characteristics" (Ibid., 5). Yet, "with the Cold War reaching a crescendo over the status of divided

values are more important in the development of science. This kind of open-endedness was expected in the previous chapter. However, it seems safe to conjecture that values may play an important role with respect to the short-term acceptance but, in the long-term, it seems that fruitfulness matters more. After all, Lysenko's theories are now rejected but Darwin's ideas live on in science. This implies that when it comes to the acceptance of a theory, it is important to define what temporal interval one has in mind. In Section 4.3 we noticed that different *explananda* may have different temporal extensions. The discussion here suggests that values and fruitfulness may play different roles with respect to *explananda* that appear very similar but differ in their temporal extensions. Ideas from Section 4.3 further clarifies the issue that began with Bellon's paper.

I hope that this brief example illustrates how explanatory resources can be managed and which kind of general considerations play a role in that process.

2. Whereas explanatory resources guide historiographical studies globally by suggesting what questions are asked and how answers are unified, *explanatory depth* is a notion that is related to a particular *explanandum*. We discussed this notion in Section 6.5. The idea behind the notion is that sometimes one *explanandum* can be explained in different ways and we can compare the depths of different *explanantia*. There are many dimension of explanatory depth and they are all related to the ability to answer what-if questions (see Section 6.5). Roughly, the more answers an explanation gives to such questions, and the more interesting and accurate the answers are, the better the explanation. Assume that every explanation cites unique factors. What should the what-if questions be about in such case? If they are about unique factors that never existed (by definition, as what-if questions are about counterfactual alter-

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Germany and its capital, Berlin, Lysenko managed to attract Stalin's personal attention to his struggle with geneticists and to secure the Soviet leader's personal support" (Ibid., 8). Soon "the ´undivided rule of Michurinist biology´ had indeed been established" (Ibid., 9).

natives to actual events), it seems difficult to understand why we want deeper explanations. The more natural view is that the questions are about the kinds of things that we are more generally interested in and whose causes we would like to understand. If we accept this, *explanatory depth* and *explanatory resources* are fruitfully connected: deep explanations are applicable to different cases that we are interested in and therefore provide possible-cause hypotheses. The deeper the explanation, the more it tells about interesting cases which did not actualize in a particular historical situation but which may actualize in other historical situations. This means that our search for deeper explanations depends on the possibility of such hypotheses. One cannot achieve deeper explanations without possible-cause hypotheses. As long as we do not abandon the idea of explanatory depth, we must assume that there are possible-cause hypotheses and therefore general considerations.

3. Not only are our what-if questions but also our explanation-seeking questions themselves are based on general considerations. Historians do not explain everything and they cannot explain everything. *Explananda* must be chosen and such choices are value-driven. One can choose an *explanandum* randomly, but such random choice would leave the historiographical study completely irrelevant for other scholars and wider audiences. It is necessary that historians make judgements concerning the significance of different *explananda*. In Chapter 4, it was argued that we are able to justify the significance of an *explanandum* in many cases. That justification must derive from considerations of general kind: How is this *explanandum* related to other significant *explananda*? How is it related to our wider concerns as human beings? Moreover, when such general considerations are applied to a particular *explanandum*, the *explanandum* must be described in general terms. For example, it is impossible to evaluate the significance of “Galileo did A by B-ing”, where A and B are a unique act and a unique

attribute of such act.[166] However, we are able to evaluate the significance of "Galileo described the motion of falling bodies by using mathematical formulas". Relevant historiography requires the use of general considerations and therefore categorizations of factors.

We have seen that there are three ways in which generalizations and general considerations play a fundamental role in the methodologically sound historiography of science. Perhaps additional ways can be found. However, this already shows that strong localism is incompatible with healthy historiography. Strong localism implies that no such general considerations are possible. The alternative to that position was formulated in the previous section. Following that explication, we can think of local explanations as having the form:

> X rather than Y because (Z and B) rather than [(W and B) or (Z and H)], where Z = factor f_1 is in location L, B = f_2 in L, W= f_1 in L*, and H= f_2 in L**.
>
> In other words: had either f_1 been in L* or f_2 in L**, Y would have been the case.

For example, a bird was spotted in New York because an ornithologist was on the building A and the bird was on A; had the bird or the ornithologist been on another building, the bird would not have been spotted.

Now, it is possible that (Z and B) explain only X and nothing else in the history of science; yet f_1 and f_2 might be factors that fall under general categories and deserve

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[166] One cannot answer "Tell me what A and B are and I tell you if they are significant", since any informative answer would require some categorization of A and B that goes beyond them being A and B.

their place in our explanatory resources since the combinations of factors f_1 and f_2 with other factors f_3 and f_4, such as (f_1 and f_3) and (f_2 and f_4), are explanatory. "That there was a bird" and "that there was an ornithologist" can explain many things: for example that a cat run (f_3 = there was a cat) or that a new species was found (f_4 = there was a bird of the species S). There is no incompatibility between local explanations and causal generalizations, once we accept the explication formulated in the previous section. We need causal generalizations. Therefore, we need to accept that explication rather than strong localism.

7.4 Conclusion

Even if there are no unique factors, there can be local explanations: a local explanation points out a set of factors such that: had any of the factors been located differently, the *explanandum* would have been different. The co-occurrence of such factors can be a unique occurrence in itself. Here universalism, in the form of more-and-more generally applicable factors, and localism are connected: the same factors can exist in many locations where science is produced but the exact co-occurrences of such factors can be unique. Moreover, such a co-occurrence might be fragile: some of the factors could perhaps have easily ended in a different location and therefore the course of history could have changed, making the present situation contingent on the co-occurrence.

Moreover, I have argued that it is methodologically necessary that a general explanatory framework and the explanations of particular occurrences guide each other. The illustration focusing on Bellon's paper did not directly involve a local explanation, but we can note that focusing on particular historical context is necessary in historiography of science in order to build a good stock of explanatory resources that have more general scope. This is where the localist tradition is on the right track, methodologically speaking. It carries the some ingredients for "well corroborated (general) knowledge, the validity of which extends beyond a particular locality" (Kuukkanen

2012, 484). Surely, localism must be supplemented with general considerations to achieve such knowledge but local explanations, *qua* explanations of particular occurrences, can be methodologically highly valuable. Universalism and localism are therefore symbiotic positions.

However, we need to note that local explanations, as explicated here, are not methodologically preferable to every other kind of explanations, only to a crude universalism that attempts to capture science once-and-for-all by a limited set of factors. This might not be too great an achievement, as it is not clear whether such crude universalism has had any supporters (see Chapter 5). It might be that what seems like universalism is just a different way of looking the same thing, that the particular and the general must be connected in healthy historiography. Be that as it may, we now see why someone, taking crude universalism as a serious threat, can point out that localism is preferable to such universalism. Moreover, we see why trivial localism should not be accepted. Even crude universalists can claim that (i) explanatory factors are always instantiated in a particular location but (ii) the factors so located are always the same and therefore universal. If trivial localism is accepted, then local explanations are not methodologically preferable to anything (as crude universalism would also provide local explanations in the trivial sense).

8 COULD SCIENCE BE INTERESTINGLY DIFFERENT?

“Could Science Be Interestingly Different”
by Veli Virmajoki,
Journal of the Philosophy of History (2018)

References only to the original paper.

Foreword to the section

In the paper “Could Science be Interestingly Different” I apply the framework developed in the previous chapters to formulate a framework in which the questions of contingency and inevitability of science can be approached.

I show how we can evaluate the contingency of some feature of science once we

(i) understand why that feature is significant, a theme related to Chapter 4,

(ii) find causal explanation of that feature, a theme discussed in Chapters 3, 5, 6 and 7,

(iii) use, when possible, existing historiographical studies, a theme continuous with Sections 6.8 and 7.3.

(iv) have a new and energy-efficient way of approaching historiographical counterfactuals, a theme related to Chapters 3 and 6.

My hope is that the discussion in the paper gives the reader an impression of the value of an explicit framework on the role and nature of causal explanation in historiography of science.

Abstract

In this paper, I investigate the issue of the contingency and inevitability of science. First, I point out valuable insights from the existing discussion about the issue. I then formulate a general framework, built on the notion of contrastive explanation and counterfactuals, that can be used to approach questions of contingency of science. I argue, with an example from the existing historiography of science, that this framework could be useful to historians of science. Finally, I argue that this framework shows the existing views on historical contingency and counterfactuals in a new light. The framework also shows the value of existing historiography in philosophical debates.

8.1 Introduction

Could science be different? In this paper, I investigate this issue. The debate between inevitabilists and contingentists concerns the question of whether science could be different from what it actually is, or if it is necessary that science has the particular features that it in fact has. Inevitabilists argue that it is inevitable that science has certain features, while contingentists argue that science could have different features from what it in fact has. These are not mutually exclusive positions. Hardly anyone denies that some features of science are contingent (for example, the exact notations used). Yet it is also clear that there are cases where people have differing views on whether or not certain features of science could be different from what they actually are. It is here that the debate is of philosophical and historical interest.

By focusing on the contingency of science, I do not mean to suggest that science is fundamentally different or independent from other human practices. Science may or may not be just like any other human activity. However, the nature of science is a question that can be answered only *after* historical and philosophical study. The issue of the contingency of science is one that can lead us to a better understanding of the relationship between science and other human activities. For this reason, I focus on the contingency of science in this paper. Moreover, one could argue that science is a special kind of activity with respect to contingency. Such views cannot be dismissed without a closer look. For example, scientific realists could argue that scientific activities are limited by the way the world is.[167] Science tracks the truth about mind-independent reality, and this fact sets very strict limits

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I thank anonymous referees for valuable comments on the previous versions of this paper.

[167] Stathis Psillos, *Scientific Realism: How Science Tracks Truth* (Routledge, 1999) is an excellent book on scientific realism.

to the kind of science we can have.[168] Thus, the realists could argue that science is much more restricted than human activities in general. On the other hand, the argument could be made that science is much less restricted than other human activities since science is an elite culture that does not have to care about politics, economy, and the like. Perhaps both of these views are wrong and science is just as contingent as any other human activity. However, before we can answer questions on the contingency of science, we need some tools to help us approach such questions. The development of those tools is the main task of this paper. Therefore, I focus on the contingency of science more as a methodological choice than as a statement about the special nature of science.

In this paper, I make three main arguments:

> 1. Science is a multidimensional global enterprise. Because of this, having a *different science* can mean many things. We must specify what we mean by different science, and in what way this different science would be an interesting alternative to our actual science.
>
> 2. We can approach questions on the contingency of our actual science by using a framework built on the notion of contrastive explanations and counterfactual scenarios in the historiography of science

[168] The connections between scientific realism and the contingency of science is, of course, a much more complicated issue than suggested here. See: Léna Soler, Emiliano Trizio and Andrew Pickering, *Science as It Could Have Been. Discussing the Contingency/Inevitability Problem* (University of Pittsburgh Press 2015); Léna Soler, "Revealing the analytical structure and some intrinsic major difficulties of the contingentist/inevitabilist issue," *Studies in History and Philosophy of Science Part A* 39:2 (2008), 230–241; Howard Sankey, "Scientific realism and the inevitability of science," *Studies in History and Philosophy of Science Part A* 39:2 (2008), 259–264.

3. The framework developed in this paper makes the issue of the contingency of science relevant to a wide range of historians. The framework also helps us see how the existing historiographical studies can be relevant to questions of contingency.

In the next section, I begin my investigation by briefly introducing the existing debate between contingentists and inevitabilists in the philosophy of science (in short: the *C–I debate*).[169] However, it must be made clear that my task in this paper is not to argue for inevitabilism or contingentism, and that the principal aim of my framework is not to solve the C–I debate. The reason I introduce the C–I debate is because it has been a source of insight and inspiration for my investigation. From this debate, I have come to understand that

(i) only some alternatives to the existing science would be relevant, and that

(ii) historiographical studies play a fundamental role in answering questions of contingency. The debate is essential to understanding the framework I formulate.

Once we have gathered the most fruitful insights from the C–I debate, I then formulate a framework of contrastive explanations and define the concepts of contingency and inevitability within this framework. This framework

(i) connects questions of contingency and inevitability directly to issues that are interesting,

(ii) enables us to pinpoint historical events and processes on which the degree of contingency of a given feature of science depends, and

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[169] I use the term "C–I debate" to refer to this particular debate. See next section.

(iii) tells us what kind of (counterfactual) considerations are relevant in assessing the degree of contingency of a given feature of science.

This framework does not make questions of contingency completely empirically soluble, but it nevertheless helps us find common ground between rival views and clarify where they disagree. Thus, the framework is best understood as a philosophical tool.

Once this framework is in place, I demonstrate its application using Matthew Stanley's study, "'An Expedition to Heal the Wounds of War': The 1919 Eclipse and Eddington as Quaker Adventurer."[170] Given the information in Stanley's paper, I ask how the contingency of observations of gravitational deflection can be approached. This example shows how asking contrastive questions enables us to use actual historiographical studies in the discussions on the contingency of science. However, it also shows that many questions remain open—some requiring further historical research, and others philosophical reflection.

In the final sections, I point out how my framework relates to the existing discussions of contingency and counterfactuals in the philosophy of history. I show that there are interesting advantages if these issues are approached in the way suggested in this paper. I also point out why it is important that existing historiographical studies can be used to answer questions of contingencies, and in philosophy in general.

8.2 Insights from the C–I debate

Recently, there has been a rich debate between inevitabilists and contingentists. In 2008, *Studies in History and Philosophy of Science* and *Isis* both devoted a

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[170] Matthew Stanley, "'An Expedition to Heal the Wounds of War': The 1919 Eclipse and Eddington as Quaker Adventurer," *Isis* 94:1 (2003), 57–89.

special issue to this topic. In 2015, a book named *Science as It Could Have Been: Discussing the Contingency/Inevitability Problem* was published and included a variety of articles devoted to the theme. Of course, the debate had its beginnings much earlier, and can be found, for example, in Ian Hacking's frequently cited paper "How inevitable are the results of successful science?"[171]

The debate is nuanced. Katherina Kinzel's analysis can be used as a helpful guide to the complexities of the debate.[172] However, the debate usually centers on the features of science that have traditionally been under discussion in philosophy of science, such as theoretical commitments. As Joseph Rouse puts it:

> Does the emphasis on ontological commitments suggest that, despite all the talk about scientific practice, we philosophers still believe that the really important changes in science concern theoretical beliefs and ontological commitments? Or is the contingency issue itself a new way to reassert the philosophical primacy of theoretical commitments?[173]

Moreover, the question is usually about the possibility of a science that is fundamentally different from but equally successful as the actual science. Ian Hacking writes:

> I asked: How inevitable are the results of successful science? Take any result R, which at present we take to be correct, of any successful science. We ask: If the results of a scientific investigation are correct, would any investigation of roughly the same subject matter, if successful, at least implicitly contain or imply the same results? If so, there is a significant sense in which the results are inevitable.[174]

Along similar lines, Léna Soler defines contingentism and inevitabilism as follows:

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[171] Ian Hacking, "How inevitable are the results of successful science?" *Philosophy of Science* 67:3 (2000), 58–71.

[172] Katherina Kinzel, "State of the field: Are the results of science contingent or inevitable?" *Studies in History and Philosophy of Science Part A* 52 (2015), 55–56.

[173] Joseph Rouse, "Laws, Scientific Practice, and the Contingency/Inevitability Question," *Science as It Could Have Been* (2015), 321.

[174] Hacking, "How inevitable are the results of successful science?" 61.

Contingentism:

(I) more or less the same initial conditions obtain as those which have occurred in the history of our own science;

(II) nevertheless, the possibility, as 'final' (subsequent or later) conditions, at least in the long run, of an alternative physics,

(i) as successful and progressive as ours, and

(ii) which yields results irreducibly different from ours (notably which involves an ontology incompatible with ours).

Inevitabilism:

(I) if more or less the same initial conditions obtain as those which have occurred in the history of our own science;

(II) and a successful and progressive physics has indeed been developed;

(III) then, inevitably, as 'final' (subsequent or later) conditions, at least in the long run:

(i) more or less the same results and the same ontology as our own,

(ii) or different but reconcilable results and ontologies as our own.[175]

Soler's definition helps us set aside the overly trivial and speculative definitions of contingentism and inevitabilism. First, we may notice that it is trivially true that if human beings had never existed, there would be no science. It is also true that if human beings were very different from what they are, there would not be any science. For example, it could have been the case that human beings were interested only in drinking beer, and science as we know it would not exist. This case does not fit Soler's definition.

[175] Soler, "Revealing the analytical structure and some intrinsic major difficulties of the contingentist/inevitabilist issue," 233. (The formulation of the passage has been slightly changed.)

Secondly, it is too easy to be an inevitabilist if one simply claims that in the end only one science will exist, and that no other science is possible. The characterization must specify what is meant by "the end": Is it a situation in which no alternative considerations for the accepted science exist? If so, the debate will begin anew with the question of the possibility of such an end point. Perhaps the end could be seen as a situation where all our possible material needs are satisfied. Then the inevitabilist claim would be that if there came a point where all our possible material needs were satisfied, then there would only be one accepted science, and thus no alternative science could be accepted without diminishing material welfare. This implies that there might be no end point in the development of science, and would thus make the inevitabilist position uninteresting. Thus, as Soler's definition points out, inevitabilists must minimally claim that there actually exist or will exist features of science that cannot be different from what they are, *given that certain antecedent conditions hold*. The claim cannot be that science has or will have certain necessary features if a certain goal is achieved. Since the C–I debate is so closely connected to the idea of antecedent conditions, the history of science has an essential role to play in the debate.[176]

There are three main insights that I draw from the C–I debate. First, the debate must be about whether some antecedent conditions in history could have led to a different science. Secondly, the history of science has an essential role in the debate. Thirdly, a discussion about the contingency of science cannot proceed without an

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[176] See: Andrew Pickering, Constructing Quarks: A sociological history of particle physics (University of Chicago Press, 1984);

James T. Cushing, Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony (University of Chicago Press, 1994);

Peter J. Bowler, Darwin Deleted. Imagining a World without Darwin (The University of Chicago Press, 2013);

Soler et al. Science as It Could Have Been;

and the papers in *Isis* 99:3.

explication of what is meant by a *different science*. In the C–I debate, a different science usually means an equally successful but fundamentally different science. While this focuses on the possibility of an equally successful but fundamentally different science is understandable from the perspective of philosophy of science, this is where I leave the C–I debate behind and widen the scope of discussion on contingency in the history of science.

8.3 The Point of Departure

We already noted that Joseph Rouse ponders why theoretical commitments have been so central to the C–I debate. He also writes:

> Grant that the instruments, experimental systems, and models with which scientific understanding is realized are more obviously contingent. It does not follow from the mere recognition that abstract ontological commitments are the only conceivable locus of scientific inevitability that such commitments are all that important.[177]

This seems correct to me, but we can go even further. There is an enormous variety in the topics in which historians of science are interested. Some of them are interested in the relationship between science and religion in the past; others in the history of medical institutions. The list could go on and on. Historians do not only study what the past was like. They are also professionals in building pictures about alternative ways of life. Historians of science are well positioned to spot significant aspects of our current practices and describe interesting alternatives to those practices—alternatives that would mean perhaps drastic changes in our lifestyle. Equal success is neither necessary nor sufficient for something to be an interesting alternative to science. It is not necessary because contexts of historical inquiry exist where an interesting alternative to our present science lacks some features that make our science so successful. Why is our present biology loaded with naturalistic

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[177] Rouse, "Laws, Scientific Practice, and the Contingency/ Inevitability Question," 321–322.

explanations rather than religious ones? It would certainly be an interesting (although, arguably, not as successful) alternative to our present science if biology looked for religious explanations—an alternative that some quarters try to advance. On the other hand, equal success is not sufficient. For example, in a medical context there could be a hypothesis that is not an interesting alternative to an accepted one, since it would not suggest changes in the treatment of patients. Even if this alternative hypothesis would have been as successful as the accepted one, this does not guarantee that it is an interestingly different alternative[178] to a historian who is interested in the treatments that are used. Moreover, the contingency of features of science is a matter of degree. Even if everything besides theoretical commitments are obviously contingent, we can still ask how contingent these features are. I now proceed to build a framework that can be used to approach the degrees of contingency of the variety of features that historians of science are interested in.

8.4 Counterfactuals, Explanation and Contingency

In this section, I argue that the best way to approach questions of contingency is to build counterfactual scenarios that would have led to an interestingly different science, and then evaluate the plausibility of these scenarios. In other words, we need to know *what should have happened in the past* in order for there to be some alternative to actual science, and then evaluate how plausible or far-fetched that occurrence was. In the following, I develop the details of this approach.

In my earlier work,[179] I defended the present-centered approach, also known as presentism, in the historiography of science. This approach has the following structure:

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[178] See the next section for what is meant by "interesting alternatives."

[179] Veli Virmajoki "Miten tieteenhistorian pitäisi valita tutkimuskohteensa?" ["How Should the Historiography of Science Choose its Targets of Study?"] *Ajatus* 72 (2015).

1. By rational discussion, we isolate the significant features (F) of the present science.

2. Next, we isolate such features F* that: if the present science had the feature F* in contrast to the actual feature F, the present science would be interestingly different from what it actually is.

3. The task of the historiography of science is to provide explanations of the form: had there been (in the past) an event[180] Z, in contrast to the actual event Y, the present science would have the feature F* instead of F.[181] In other words, the historiography of science provides explanations for significant features of the present science.[182]

Using this approach, we can define the contingentist position as follows:

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[180] Usually, we must find a set of events that satisfies this condition. I will discuss the limiting case—only one event—to simplify matters.

[181] There has been much discussion about contrastive explanations. See:

B. van Fraassen, *The Scientific Image* (Oxford University Press 1980);

P. Lipton, "Contrastive Explanations," In Knowles (ed.), *Explanation and its Limits* (Cambridge University Press 1990) 247–266.

In this formulation, I mainly follow James Woodward's theory from *Making Things Happen* (Oxford University Press, 2003).

[182] This is a normative view on the historiography of science. The suggestion is that the historiography of science should, and sometimes does, provide such explanations. Moreover, much of existing historiography of science can be fruitfully read through such a conception of historiography of science. It is a philosophical view that goes beneath the surface of what historians actually say.

> It could have been the case that science has the feature F* rather than the actual feature F, where the difference between F and F* is considered interesting in the given context of discussion.
>
> We can accept that F* could have been the case if and only if (a) someone points out a counterfactual past event Z that would have led to F*, and (b) it is shown that the occurrence of Z in the past is not an impossible (or extremely far-fetched) scenario.[183]

Moreover, the contingency of a certain feature F of science is a matter of degree:

> Feature F can be judged to be (a) inevitable if and only if the occurrence of any Z_i is seen as impossible and (b) a truly chancy feature if Z is a part of the actual history. Moreover, the more far-fetched the occurrence of Z is judged to be, the more inevitable feature F is.

For example, if it turns out that a theory that is actually held was chosen from among many theories by a flip of a coin, then the fact that we hold that particular theory instead of another is a chancy feature of science. On the other hand, the fact that we have theories of celestial motion lies at the other end of the continuum of contingency. Only in the far-fetched scenarios where human beings were not interested in repeating patterns of time (the understanding of which is necessary for agriculture) there would be a complete lack of theories of celestial motion. Finally, if there are many counterfactual scenarios such that each of these scenarios would have led to F*, then the contingency of F depends on the scenario which is judged to be the most plausible one.

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[183] The example in the next section suggests how this can be done.

The claim that a feature F of the present science is contingent can (and must) be substantiated by (a) showing that the existence of the feature F* would have made the present science interestingly different, by (b) pointing out event Z that would have led to F*, and by (c) making sophisticated judgments about the plausibility or the far-fetchedness of the occurrence of Z (i.e. about the scenario where Z is the case). In this way, the framework here helps us understand how the degrees of contingency can be approached by using historical studies, as we will see in the next section.

The definition above requires that, in order to get discussions going, judgments about the plausibility of historical scenarios involving Z can be made and that it can be assessed that Z would have led to F*. Nothing general about this topic can be said within the limits of this paper. However, in the next section, I give an example of how historical study can be used to argue for certain degrees of contingency of certain features of science. This discussion points out that, even in the absence of a theory of how judgements about the plausibility of counterfactual scenarios work, historians always make these kinds of judgments when explaining historical processes. Moreover, these judgments make sense and can be rationally debated. Of course, there might not exist a point where everyone agrees on a given set of counterfactuals.[184] Nevertheless, we will see that the framework developed here can narrow down the topics of disagreement and highlight which disagreements are relevant with respect to a given topic. Moreover, there exists a comforting amount of analysis of the use of counterfactuals in history,[185] and there exist, in the

[184] Moreover, if someone is not skeptical only about particular evaluations of historical counterfactuals but denies the possibility of historical counterfactual altogether, questions on contingency of science are not meaningful to this person. After all, descriptions of science “as it could have been” are descriptions of counterfactual science. Thus, global skepticism towards counterfactuals is not something that affects my account alone.

[185] On the use of counterfactuals in history, see:

literature concerning the C–I debate, many studies that speak to the possibility of finding out points of history that have had an influence on the path that has led to the present situation in science.[186]

Before proceeding to the example, one thing needs to be taken into consideration. The isolation of significant features is typically carried out in the following manner: we take features that are *prima facie* significant and then attempt to establish more general principles that make these features significant. In a continuous process, we then compare the features and the principles in such a way that the judgments about the significance of some feature and the plausibility of our principles can both be adjusted. For example, we may say that the observation of gravitational deflection (see details below) is a significant feature of science since it plays an important part in the acceptance of relativistic physics (and thus in the overall shape of our physics) and in the understanding and technology that it provides. If these were different, we would have an *interestingly different* science. Arguably, the way we see the world and the technology we use impact our lives in a remarkable way. Now, if one accepts this, a sociologist might point out that, since the technological state of our society is one of the features that makes science significant, gender distribution in science is also a significant feature of science since it shapes the distribution and use of technology in a way that matters. If the distribution of technological resources were different, we would live in an interestingly different world. In contrast, if we used different symbols in the periodic table, science would

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Alexander Maar, "Possible uses of counterfactual thought experiments in history," *Principia: An International Journal of Epistemology* 18:1 (2014), 87–113.

Tim De Mey and Erik Weber, "Explanation and Thought Experiments in History," *History and Theory* 42:1 (2003), 28–38.

Johannes Bulhof, "What if? Modality and history," *History and Theory* 38:2 (1999), 145–168.

and *Journal of the Philosophy of History* 10 (3).

[186] See footnote 10.

not be interestingly different. Everything we know about chemistry and the applications of this knowledge would be the same. At least one could, if they disagreed, argue for the significance of the symbols themselves.

In this way, we can rationally evaluate which features of science are significant.[187] Once we know the significant features and why they are significant, we also know which alternatives to these are the interesting ones. What would be an interesting alternative to science depends on what we want from science, and what achievements of science (good or bad) we see as relevant. Ultimately, considerations such as these are based on our values. Deep questions of values cannot be discussed here. However, we have seen that it is possible to distinguish the interesting alternatives to science from the non-interesting ones.

8.5 Eddington and the Gravitational Deflection

In "'An Expedition to Heal the Wounds of War': The 1919 Eclipse and Eddington as Quaker Adventurer," (2003), Matthew Stanley describes the process that led to Eddington's 1919 eclipse expedition and the observation of gravitational deflection. According to Stanley, the execution of this expedition was a pivotal event that had a notable effect on scientists' acceptance of Einstein's general theory of relativity.

I use Stanley's article to demonstrate the framework formulated above. The main goal of this section is to show that the framework can help us use historical

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[187] Jean-Marc Lévy-Leblond nicely illustrates the wide variety of features that might be taken to be significant: "Had the alternate approach been realized [in physics], there would be a number of significant differences: [among other things] pedagogical approaches to the theory would be different and certainly more convincing [and] the large cultural impact of relativity theory would have been quite different, from its many and often problematic philosophical exegeses […] to its vernacular manifestations, for instance in popular iconography (Einstein's tongue!)." ("On the Plurality of (Theoretical) Worlds," *Science as It Could Have Been*, 341–342.)

studies to evaluate the degrees of contingency of particular features of science. I show that while we can have progress in these evaluations, we cannot settle the questions once and for all. I will make it explicit when further historical research can settle an open question and when we are forced to use considerations that are more philosophical—and thus also potentially undecidable—in their nature.

Two things must be noted: first, for the sake of illustration, I assume that Stanley's account is correct. I also make, to the same end, some claims that might seem controversial with the purpose of isolating interesting issues that are connected to contingency and inevitability. This illustration does not have the purpose of arguing for or against inevitabilism. It only aims to show how the issue can be approached by using existing historical research and how such research can fuel the discussion.

Stanley highlights the following aspects of the process leading to the expedition:

> The eclipse's scientific significance had gradually become clear over the course of the war years. The first mention of relativity's prediction of the bending of light in the Observatory was an anonymous 1913 note entitled "Gravitation and Light." [188]
>
> [Eddington published] *Report on the Relativity Theory of Gravitation*, a small volume that was the first complete treatment of general relativity in English. Soon, enough interest in the theory had been generated to begin investigation into the logistics of an expedition to test it.[189][M]any astronomers thought the expedition would be a waste of time. The refugee scientist, Jonckheere, warned that there were several different mechanisms that might duplicate the predicted deflection, making observations useless. [Yet] such a vague objection, Lindemann said, should carry little weight compared to Einstein's detailed and consistent theory.[190]
>
> [Astronomer Royal] Frank Dyson felt that while the theory was speculative, its implications were so important that it needed to be investigated.[191]

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[188] Stanley, "'An Expedition to Heal the Wounds of War': The 1919 Eclipse and Eddington as Quaker Adventurer," 71.

[189] Ibid.

[190] Stanley, "An Expedition," 72.

[191] Ibid., 70–71.

> Einstein's theory predicted that a ray of light traveling near a massive object, such as the sun, would undergo a small but measurable deflection of its path. This was one of the three "classic" relativistic effects predicted by Einstein: the advance of the perihelion of Mercury was already established, and the measurement of the redshift of the solar spectrum was proving difficult. This left observing the gravitational deflection as the only realistic hope of confirming general relativity.[192]

Passages such as these convey the impression that the British scientific community was interested in making the observations purely for scientific reasons—even the objections to the journey were based on scientific considerations. The expedition to an eclipse was thought to be the only way to test the theory, and there were scientific reasons to think that the expedition could bring useful results. Yet Stanley points out that "[d]iscussions such as these were important in the scientific debate but had little impact on the actual planning of the expedition. This was chiefly in the hands of two astronomers who were also interested in ramifications beyond the scientific test: Eddington and Dyson."[193] The execution of the expedition required the individual effort of Eddington and Dyson despite the fact that the *Joint Permanent Eclipse Committee*, a group set up by both the Royal Society and the Royal Astronomical Society to pool the intellectual and logistical resources of the two groups, already existed.

The question we must ask is why Eddington was so keen on the expedition. Stanley points to Eddington's Quaker background and the context of the First World War. Stanley describes how the beginning of the war changed the attitude of British society, including scientists, toward the Germans. Despite an early declaration that science is above politics, the reality of war resulted in hatred toward the Germans, as Stanley points out in detail.[194] In this context, it was Eddington's Quaker background that shaped his attitude toward the situation:

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[192] Ibid., 71.

[193] Ibid., 72.

[194] Stanley, "An Expedition," 59–67.

> Eddington's reaction […] was largely shaped by his membership in British society's traditional bastion of pacifism: the Society of Friends.[195]

> The Friends' goal was to demonstrate "the brotherhood of man overstepping all artificial barriers of race, politics or creed, which we believe to be the only true foundation upon which the family of nations can rest."[196]

> Those Friends who ventured to Europe to relieve this suffering, both during and after the war, worked in difficult and sometimes dangerous conditions. These relief workers came to be known as "adventurers," and they hold a special place in Quaker history as men and women who journeyed into far and foreign lands as a duty of conscience. The strategies used by these adventurers became the models for Eddington's efforts to use the eclipse expedition as a tool in repairing international relationships.[197]

Given Stanley's arguments, we can ask to what degree the observation of gravitational deflection was a contingent matter. To use the framework formulated in the previous section, we must begin by defining the significant feature of science that resulted from Eddington's expedition. Arguably, the answer is: the significant feature F in this case is that *the observation of gravitational deflection has been made*. A significant alternative (F*) to this feature F is that *the observation has not been made*.

Next we must ask the question (C):

> What conditions should have been different so that the observation would not have been made?

We can begin our search for the answer by formulating the following question and answer in order to find these conditions:

> Q1. Why did Eddington execute the expedition rather than not?

> E1. Because Eddington attempted to reunite the international scientific community due to his Quaker background and because the expedition

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[195] Ibid., 61.

[196] Ibid., 68.

[197] Ibid., 67–68.

had the chance to produce scientifically interesting results due to the interest in Einstein's theory.

One might think that Eddington's execution of the expedition of 1919 is the condition that can be given as an answer to the question C, and thus answering the question Q1 provides the details of the answer. If this is the case, then F is a rather contingent feature of science: it depends on Eddington's personal religious worldview, and it is easy to provide plausible scenarios in which Eddington did not have this worldview—he could have lost his faith in humanity because of the war. However, the details given by E1 are not the answer to C. This is due to the fact that Q1 leaves it open as to whether or not someone else would have made the expedition if Eddington had not. Thus, we must continue our search for the answer to C.

Next we can ask:

> Q2. Why did Eddington, rather than someone else, execute the expedition?

> E2. Because Eddington was concerned about the unity of the international scientific community and wanted to unify it due to his Quaker background, and no one else had this concern.[198]

It might be argued that once we know why Eddington, rather than someone else, executed the expedition, we can then evaluate how plausible it is that the expedition would have been executed without Eddington. But the contrast in Q2 is misleading if this was the evaluation we wanted. E2 tells us in which situation someone else would have executed the expedition instead of Eddington, but Q2

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[198] Stanley's description makes one think that Eddington played the pivotal role due to his unique concern in the context.

assumes that the expedition would have been carried out in either case. E2 in itself would become interesting only if an argument was given for the claim that the identity of the executor of the expedition had an impact on the making of the observations. The possibility of this must be settled by investigating whether or not Eddington had a unique set of skills that were necessary in making the observations. This is a step in the right direction, but we must still continue our search for the answer to C.

We can ask:

> Q3. Why was the observation attempted[199] in 1919 rather than later?
>
> E3. Because Eddington was concerned about unifying the international community and had the urge to work at a quick pace in this matter due to his Quaker background (and because of lucky occurrences during the journey).

This is again a step in the right direction, because E3, unlike E1 and E2, specifies the conditions in which the expedition (and thus the observations) would not have been made in 1919. It can be argued that the execution of the expedition in 1919 was a rather contingent event based on, again, the contingency of Eddington's background. But once we notice that we defined the significant feature F to be that *the observation of gravitational deflection has been made,* and once we notice that almost one hundred years have passed since 1919, we see that the contrast in Q3 is not relevant to answering question C. This is due to the fact that the answer to Q3 says nothing about the years since 1919 and thus E3 does not tell us anything about how

[199] This innocent change of terms is due the fact that later an expedition itself may not have been necessary (it could have perhaps been made in a place where an observatory already existed, etc.).

plausible it is that the observation would have been sought later in the twentieth or at the beginning of the twenty-first century.[200] The correct question to ask is:

> Q4. Why was the observation attempted rather than not?
>
> E4. Because there was an increasing scientific interest in Einstein's theory and because the observation of gravitational deflection was viewed as a practical way to test the theory.

We saw earlier how Stanley points to these factors. British scientists were planning the expedition even before Eddington and Dyson took the execution into their hands. Moreover, Stanley does not refer to or give any reason to believe that had Eddington not become involved when he did, the observation of deflection would not have been made at any point in history. This means that what seemed to make the feature F highly contingent, i.e. Eddington's religious worldview, is no longer a condition that can be used to answer question C.

We can now explicate the philosophical lessons that can be drawn from the example. First, it must be noted that we did not analyze the contingency of having the observation of gravitational deflection in detail. The example only pointed out how it is possible to find the conditions on which having the significant feature F

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[200] If we had formulated F in our illustration in another way, for example, saying that it is significant that the observation was made in 1919 and not later, then a highly contingent condition—Eddington's Quaker background—would be the condition on which the significant feature depends. F could be defined in this way if we wanted to know, for example, the degree of contingency of a certain other feature G, that has become part of science only recently and that is a result of developments that began in 1919 when Eddington made the observation of gravitational deflection. That science has the feature G, and not some alternative to it, would be judged to be highly contingent if it was necessary, in order to have G, that the expedition took place in 1919. The more recent the features of science in which we are interested, the more likely it is that they could have been otherwise.

instead of F*depends. We came to the conclusion (for the sake of illustration, of course) in Q4-E4 that only if there had been no scientific interest in Einstein's theory, it could have been possible that the gravitational deflection would never have been observed. To evaluate the contingency of having the observation of gravitational deflection requires an investigation that evaluates the plausibility of scenarios in which there was no interest in Einstein's theory. How plausible is the scenario where Einstein's theory was not formulated? How plausible is the scenario where Einstein's theory was ignored by the scientific community?

These questions cannot be answered based on Stanley's paper. It is possible to get closer to the right answer through further historical studies. These studies could investigate the following questions:

> 1. How widespread was the idea of spacetime curvature among scientific communities?
>
> 2. How was the evidential value of such observations viewed in scientific communities?
>
> 3. How seriously did scientists plan to isolate German scientists, including Einstein, after the war?
>
> 4. What was the status of physics among the socio-political environment? Were the politicians planning to solely focus on the aspects of physics that would have direct impact on military technology?

Of course, no matter how much empirical information we have, questions of counterfactual paths of science cannot be unequivocally answered. Here we come to the point where different philosophical considerations and intuitions about science divide us on the issue of contingency. Can we, for example, assume that the world has a determined structure? Is science an opportunistic field where very small changes in the social environment can direct scientists to pursue different activities? Do observations really matter in theory choice? This means that there is no guarantee that discussions on the contingency of a particular feature of science can be settled.

However, once we have identified crucial historical points on which our actual science presumably depends, we have also limited the number of philosophical considerations that are relevant to settling the questions of contingency. For example, if we are able to show that the existence of Einstein's theory (and the scientific community's interest in the theory) is the best candidate for the factor on which observations of gravitational deflection depend, then the considerations of the connections between observations and theoretical works play a role in the attempts to decide the contingency of those observations. On the other hand, if empirical research leads us to believe that Eddington's religious background is the best candidate for the factor on which the observation of gravitational deflection depends, then the considerations of the connections between personal background and scientific work play a role in the attempts to decide the contingency of those observations. Thus, the framework formulated in the last section does not rid us of all disagreement in questions of contingency, but does help narrow down the relevant considerations in a particular case. Thereby, the framework helps us find common ground between rival views.

There is one more lesson to be learned from the example. It must be noted that the difference between having the observation of gravitational deflection and not having that observation is interesting only in certain contexts of discussion, as the definition of contingency given in the previous section asserts. An example of this kind of context would be a discussion on the building of GPS navigation devices. One could wonder how these devices became so useful, and someone could answer the question by pointing out that this is partly due to the fact that we have begun to understand the effects of gravitational deflection. If the observation of deflection had not been made, we might still have inaccuracies in these devices and they would not be as useful as they are. We would attempt to find the cause of the malfunction of the devices in places it does not exist. Thus, it is significant that our thinking is no longer limited by assumptions based on a Euclidean view of space. In this way, it becomes clear that there exists a hidden structure in our definition of significant feature F that is given by the context of discussion: F is that *the observation of gravitational deflection has been incorporated into our thinking* and F*, the significant alternative, is that *we do not use the concept of gravitational deflection in our thinking in the problem situation which we face in the modern world*.

The last point is important since it counters the following argument:

> Every feature of science is dependent on a previous event, and this event is again based on some previous event, and so on. Moreover, even if we ignore all the trivial factors that present science depends on (such as the existence of humankind), every event in the given chain is a contingent event and these contingencies add up to a highly contingent present situation. For example, the observation of gravitational deflection was dependent on the formulation of Einstein's theory; the formulation of Einstein's theory was dependent on the physics of the nineteenth century; the physics of the nineteenth century was dependent on the work of Newton; and so on. The chain could have broken at any point, and thus it is a great coincidence that we have observed gravitational deflection.

First, it must be noted that this kind of thinking is flawed. Causal chains in the world do not work in this neat manner. There are situations of overdetermination, pre-emption, non-transitive causal chains and overlapping causal chains that make this kind of argumentation simply naïve.[201]

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[201] James Woodward's *Making Things Happen* is a good place to become familiar with these issues and the complexity of causal thinking and philosophy of causation.

Secondly, and more substantially, this argument can be overcome by simply pointing out that if Newtonian mechanics, for example, had never developed, then there would not be a theoretical framework against which the consequences of not having evidence for gravitational deflection could arise. If we had not dealt with the actual questions of physics, then the thinking process that uses knowledge of gravitational deflection would not exist, nor would the thinking processes that take the absence of gravitational deflection as a serious possibility (a process like this would be the futile search for the causes of the malfunctioning of GPS navigation devices). In the absence of physics, lacking one piece of data would be the least of our concerns. To summarize, the counterfactual situations in which an alternative feature F* is assumed to be part of science must be somewhat close to our actual situation. Otherwise the differences between the features of our actual science and the interesting alternatives to these would not be meaningful. As Rouse puts it: "Differing judgments about scientific significance thus matter well beyond whether they lead to differences in accepted truth claims. We need to ask which accepted beliefs matter to science, and how they matter."[202]

To ask whether science could have been different is to ask whether or not it could have been different in an interesting way. Asking whether science could have been different in an interesting way is not the same thing as asking whether science today could have been fundamentally different or missing altogether. Only historical studies that show the degree of contingency of the things that we find significant in science can increase our reflective understanding of this question.

An example (not from Woodward): My soccer team's losing to ManU depends on the goal they scored in the first minute. This depends on my team's inability to stop the attack. However, had we been able to stop the attack earlier, ManU would still have scored a goal in the second minute (by understanding our defense better) and my team would have lost anyway.

[202] Rouse, "Laws, Scientific Practice, and the Contingency/Inevitability Question," 320.

8.6 Contingency, Counterfactuals and the Relevance of Historiography

Historical Contingency: The Nature of Relevant Initial Conditions

The concepts of contingency and necessity are widely discussed in the philosophy of history. One definition closely similar to the one presented in this paper is that of Yemima Ben-Menahem. According to Ben-Menahem, "contingency (necessity) varies in magnitude: the greater (smaller) the sensitivity to initial conditions, the greater the degree of contingency (necessity)."[203] To reiterate:

> Contingency: Similar causes lead to different types of effects. High sensitivity to initial conditions.
>
> Necessity: Different types of causes lead to similar effects. Low sensitivity to initial conditions.[204]

However, there are remarkable differences between the definition of this paper and the definition of Ben-Menahem. First, my definition of contingency does not require that similar causes lead to different outcomes.[205] The factor Z that would have led to F* does not need to be similar to Y in order for there to be contingency. All that is needed is that Z is not far-fetched.[206] Secondly, even if different types of causes lead to similar effects, this does not mean that science is inevitable.

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[203] Yemima Ben-Menahem, "Historical contingency," *Ratio* 10:2 (1997), 102.

[204] Ibid., 101.

[205] Notice that also Soler's definition (section 2) mentions more or less the same initial conditions.

[206] There does not seem to be any reason to equate similarity and non-far-fetchedness. Pulling the trigger of a gun and merely holding a finger on the trigger are very similar events. Yet the pulling of the trigger can be a far-fetched alternative in a situation where an experienced and trustworthy police officer holds their finger on the trigger.

In my framework, this similarity of effects can still mean that there are some interesting differences between them, and thus there can exist contingency. Similarly, even if the effects are very different, this does not automatically mean that one is an interesting alternative to the other. A great advantage of my framework is that it makes explicit what differences and similarities we are interested in. We do not need to find out the general properties of the causal structures in history to gain knowledge of the contingency of features we are interested in. We can focus on the structures and features we are interested in.

Reconsidering Counterfactual History

It is important to make note of how the contrastive explanations based on counterfactuals differ from what is known as *study of counterfactual histories*. In these studies, an event C is assumed to be the cause of an event E and the question one tries to answer is: What would have happened had C not occurred,[207] or more generally, what could have happened had certain things been different in the past? On the other hand, the formulation of a contrastive explanation begins by specifying the relevant alternatives to the event E and proceeds to find out which alternatives (C*) of C would have led to some relevant alternative (E*) to E. Although studies of counterfactual histories can achieve the conclusion that science would be interestingly different had certain things been different, this is not guaranteed. The reasoning could also lead to the conclusion that science would be different, but not interestingly so, or to the conclusion that science would not be different.

This means that my framework offers an energy-efficient way of approaching questions of contingency. When we first specify what differences we are interested in and then proceed backwards in history to the causes of these differences (rather than forwards, as in the study of counterfactual history), we are able to bypass a

[207] Maar, "Possible uses of counterfactual thought experiments in history," 88.

variety of considerations that do not have direct relevance to understanding the contingency of science in particular cases.

The Value of Existing Historiography in a Philosophical Debate

The C–I debate has produced and highlighted many excellent historical studies that are relevant to the issue of contingency.[208] The debate has shown that good historical work is extremely useful in approaching the issue of the contingency of science. Nevertheless, once we are interested in generalizing the questions on the contingency of science to a vast range of issues that historians of science are interested in, a general framework that tells us how we can approach questions of contingency needs to be in place. There are two reasons for this. First, historians of science have produced high quality works. It is advantageous to have a framework that can be used to extract relevant points from these works; there is no need to wait for studies that directly intend to discuss the degree of contingency of a particular feature of science. Secondly, the framework can work as a tool in the historical research that discusses such questions. Due to its complete generality, it can unify the structures of such studies. We also saw above that my framework is energy efficient. Moreover, since historical case studies in the philosophy of science face difficult

[208] See examples in footnote 10.

methodological issues,[209] the possibility of using historical studies that are made independently of the issue of contingency is an advantage.[210]

8.7 Conclusion

In this paper, I formulated a framework that helps us discuss the degree of contingency of any feature of science in which a historian of science might be interested. While this framework does not give a perfect algorithm to solve questions of contingency, it nevertheless tells us which types of questions to ask and what to consider when approaching questions of contingency. The framework also shows the place of our evaluations of significance in questions of contingency. Some alternatives to science are more interesting than others, and we should focus on those interesting alternatives. Furthermore, the framework shows how already existing historical studies can provide insights into questions of contingency. Although the framework is a very general one, it is humble in one important sense: we can approach the contingency of science only in a piecemeal manner, asking whether this or that particular feature of science could be interestingly different.

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[209] See Katherina Kinzel, "Narrative and evidence. How can case studies from the history of science support claims in the philosophy of science?" *Studies in History and Philosophy of Science Part A* 49 (2015), 48–57; and

Joseph C. Pitt, "The Dilemma of Case Studies: Toward a Heraclitian Philosophy of Science," *Perspectives on Science* 9:4, 373–382.

[210] Of course, there does not exist a perfectly objective historical study, and historical research can benefit from the discussions on contingency and inevitability, as Soler argues in "Introduction. The Contingentist/Inevitabilist Debate: Current State of Play, Paradigmatic Forms of Problems and Arguments, Connections to More Familiar Philosophical Themes," *Science as It Could Have Been*, 22–23, and as is argued in this paper. However, it is still useful to be able to use historical studies that are made independently of the debate. They are probably the most objective ground that we may wish to have.

9 CONCLUSION

9.1 Historiography of Science Is Not a *Sui Generis*

In this book, I have argued that there are many advantages if we consider explanations in historiography of science as exhibitions of patterns of counterfactual dependencies. Moreover, I argued that an explanation can be warranted in historiography of science if and only if X is a part of the explanatory resources of the field. Since historiography of science shares the same stock of explanatory resources with sciences and other disciplines, historiography of science shares both the structure of explanations and the explanatory knowledge with other explanation-seeking fields. If we accept the framework formulated in this book, historiography of science does not differ in any fundamental way from other fields of inquiry. From the traditional philosophical perspective, one controversial consequence is that historical explanations do not provide understanding of a unique kind; historical understanding is not *a sui generis*. The *sui generis* view is exemplified in the following claims:

> [The main task of a historian] is to think himself into [the action under investigation], to discern the thought of its agent. [--] To discover that thought is already to understand it. After the historian has ascertained the facts, there is no further process of inquiring into their causes. (Collingwood, 1974, 25).

> Historical understanding (verstehen) has to do with grasping the intentional content attached to human actions. (Bevir 2007, 259).

We have seen that basically everything in the universe, from reasons to values to electrons, can be explanatorily relevant in historiography of science, not only thoughts or intentional contents, no matter how widely these are defined. The question, how did we arrive at such view, arises. Answering the question is useful as it helps to understand the philosophical content of this book.

The crucial move was made in **Chapters 2 and 3** where it was argued that both (i) the developments in the historiography of science and (ii) philosophical considerations show that the idea that there have been *sciences (with era-specific features) in* *the past* is no longer sustainable in historiography of science. In order to avoid the impasse, I suggested that which events count as a part of the history of science does not depend on how those events can be conceptualized but on their relations to the present science: In the presentist approach, the backbone of the history of science is the causal connection to the present science. Further justification for the presentist approach was due to its ability to solve many historiographical problems (Section 3.4) and due to its ability to work as a tool in the historiographical thinking. The presentist approach clashes with the *sui generis* view because historical understanding is provided by patterns of counterfactual dependences between the present and the past. This implies that the target is no longer to understand different ways of thought in the past. If that were our target, we would need to define what makes some thought a scientific thought.

Notice that I am not claiming that there cannot be historiography that is fundamentally about thoughts and intentional content. My argument is that such historiography is not suited for understanding the history of science. Throughout this book I have argued that there are questions that can be answered only if science is located in the causal nexus of the universe. The fundamental reason for this is that science, whatever its exact nature, is connected to the causal nexus of the universe for the very reason that it attempts to gain knowledge of that universe. Neither am I claiming that thoughts or intentional content are irrelevant in historiography of science. They do deserve their place in the picture that emerges. Discerning thoughts and grasping the intentional content are also methodologically indispensable.

In **Chapter 4**, we saw that historiography of science should provide understanding about the *significant features of science*. I argued that since we have a plurality of values and preferences, historiography of science should shed light on many features of science. The significant features also belong to different kinds (such as social, epistemological and cultural). The heterogeneity in the significant features is important because it implies that historiography of science must include a variety of explanatory resources to explain them. The unfeasibility of the *sui generis* view is already present in Chapter 4.

In **Chapter 5**, I discussed why a completely general account of explanations in historiography of science is needed. The main reason was is that such an account can capture important perspectives within the field which are often seen as separated by insurmountable barriers. Philosophical debates on the nature of historiography of science have generated unjustified distinctions, confusions, and unnecessary restrictions that can be removed with such general account. In **Chapter 6**, such an account was formulated. It was built on the notions of counterfactuals and contrastive explanations. The account connects the explanations in historiography of science structurally to explanations in other fields and every-day life: Explanatory strategies in historiography of science are the same as in everyday life and in other fields of inquiry. Within the account, we were able to define important notions such as *competing explanations* and *explanatory depth.* Moreover, I argued that historiography of science, if it is to have a sound and progressive methodology, must share the same explanatory resources with other fields of inquiry. Knowledge in historiography of science and knowledge in other fields of inquiry cannot be separated. In **Chapter 7** the account developed earlier was used to explicate one central notion in historiography of science, that of *local explanation*. Within the chapter, I used cases from existing historiography of science to further illustrate ideas developed in earlier chapters.

In **Chapter 8**, the explicit framework on the role and nature of causal explanation in historiography of science was applied to a question (both philosophical and historiographical) of contingency/ the inevitability of science. This chapter, as well as Chapter 7, indicated that the theory of explanations developed in the earlier chapters has serious value as an analytical tool.

In conclusion, this book established the close connections between historiography of science and other areas of human understanding. *Both the explanatory strategies and the explanatory resources of historiography of science are shared with other fields of inquiry.*

9.2 From the past to the present and beyond

In this book, we have seen how the present-centered historiography of science, equipped with a suitable notion of causal explanation, enables us to see the value and prospects of historiography of science. First, we saw how being openly present-centered revives the coherence and importance of the field known as historiography of science. Secondly, we saw how reflection on significant features of science, based on values and preferences, enables historiography of science to remain a widely interesting discipline and to avoid biased studies. Thirdly, we saw how contrastive and counterfactual notion of causal explanation can guide and clarify historiographical studies. We also saw how to decide what kinds of factors should be incorporated in the explanatory resources of historiography of science. Finally, the last chapter described how the developments of science can be used to evaluate the contingency and necessity of our present science. We can conclude that *this book is about building a bridge between the present and the past.*

It is interesting to notice that the conceptual framework developed in this book is also applicable to the studying the possible futures of science. First, the reflection on the significant feature of science, as a preference-based activity, can be oriented toward the future: we can ask what kind of science could be significant for us in the future. Moreover, the causal explanations and the explanatory resources that are used to connect the past developments to the present science can be also used to estimate what the future will be like or how we could achieve the kind of future we prefer. For example, if we know why X rather than Y is the case now, we know what would have led to Y. If we know this, we can perhaps build a situation that leads to the Y in the future. Therefore, causal knowledge of the past, if structured the way suggested in this book, can be highly valuable in practice. This opportunity of futurizing the science studies should be taken seriously. However, the exact details of that project must wait for later studies.

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