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EVIDENTIAL REASONING IN HISTORY AND PHILOSOPHY OF SCIENCE: CASE STUDIES AND ASSESSMENT

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que para optar por el grado de

Doctor en Filosofía de la Ciencia (Historia de la Ciencia)

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Evidential Reasoning in History and Philosophy of Science

Case Studies and Assessment

by Dubian Cañas

A thesis submitted in partial fulfillment of the requirements for the degree of

Doctor of Philosophy of Science (History of Science) in

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DUBIAN ANØRÉS CAÑAS MORA 518492146 [C]umque me convertissem ad universa opera quae fecerant manus meae et ad labores in quibus frustra sudaveram vidi in omnibus vanitatem et adflictionem animi et nihil permanere sub sole. Ecclesiastes 2:11.

Abstract

SPANISH:

Esta tesis tiene como objetivo reivindicar el estatus del razonamiento evidencial en historia y filosofía de la ciencia (HFC), argumentando que la evidencia histórica sí apoya afirmaciones filosóficas sobre la ciencia. Discuto dos argumentos escépticos que sostienen que el razonamiento evidencial en HFC debe abandonarse porque es objetable epistémicamente: uno es el argumento de circularidad y el otro es el argumento de impropiedad. Para este fin, exploro tres trabajos en HFC como casos de estudio: (1) los programas de investigación historiográfica de Lakatos, (2) la estrategia de la ostensión histórica de Stanford, y (3) HFC integradas de Chang. Después de examinar cómo estos trabajos emplean el material histórico para apoyar conclusiones filosóficas, propongo una solución al problema que cada argumento escéptico plantea.

Los capítulos 1 y 2 presentan la posición escéptica. El argumento de circularidad se basa en la premisa de que las reconstrucciones históricas no son independientes de las teorías filosóficas en contrastación. Dos razones motivan esta premisa. El cargo de manipulación de la historia asegura que los filósofos manipulan el material histórico arbitrariamente de modo que los estudios de caso se ajusten a la teoría filosófica favorecida. La objeción de pluralismo histórico afirma que, incluso si el material histórico no ha sido manipulado, las reconstrucciones históricas están esencialmente cargadas de teorías filosóficas en competencia, por lo que la evidencia histórica no puede adjudicar desacuerdos filosóficos. Por su parte, el argumento de impropiedad establece que la historia de la ciencia es inapropiada para la filosofía de la ciencia ya que ambas disciplinas son mutuamente incompatibles. La impropiedad metafísica arguye que hay una tensión inherente en HFC integradas en la medida en que historiadores y filósofos adoptan compromisos metafísicos sobre la ciencia que están en conflicto. La impropiedad epistémica, en cambio, mantiene que los datos históricos son dispensables o inadecuados para apoyar conclusiones filosóficas que son generales y normativas.

Los capítulos 3, 4 y 5 corresponden a mis tres casos de estudio, donde defiendo el razonamiento evidencial en HFC de ambos argumentos escépticos. Uso los trabajos de

Lakatos y Stanford para resolver el problema de circularidad, mientras que apelo al trabajo de Chang para solucionar el problema de la impropiedad. Mi caso de estudio (1) ofrece una tipología de evidencia histórica independiente que captura diferentes formas en las cuales las reconstrucciones racionales de los episodios históricos son independientes de las teorías filosóficas de la racionalidad científica que están siendo apoyadas. Esta tipología está construida atendiendo a los tipos de teoría usados para hacer reconstrucciones históricas y a la interacción relevante entre estos tipos. Mi caso de estudio (2) muestra el papel de los estándares historiográficos al evaluar narrativas históricas y posiciones filosóficas. Primero, afirmo categóricamente que los estándares de adecuación histórica son cruciales para resolver el debate sobre realismo científico selectivo respecto de las reconstruccciones rivales de Psillos y Stanford del episodio de la teoría del calórico. Segundo, afirmo condicionalmente que si las formas en que Stanford manipula el material histórico generan datos fiables, su estudio histórico de las teorías de herencia biológica del siglo XIX apoya el problemas de las alternativas inconcebidas. Mi caso de estudio (3) bloquea el problema de impropiedad a partir de la epistemología pragmatista de Chang. Aquí propongo que la tensión inherente de HFC integradas puede verse como una forma de incoherencia operacional, cuya resolución está en adoptar el realismo activista como filosofía de la ciencia. Esta teoría filosófica involucra el no-absolutismo, de modo que conduce a integrar satisfactoriamente historia v filosofía.

El último capítulo, que contiene las conclusiones generales de la tesis, esboza la epistemología de HFC que deriva de mi estrategia argumentativa basada en casos. En particular, formulo tres tesis acerca del razonamiento evidencial en HFC: (T1) las reconstrucciones históricas satisfacen tipos de evidencia independiente cuando apoyan teorías filosóficas; (T2) estándares de adecuación histórica son cruciales para evaluar reconstrucciones de episodios históricos y así permiten resolver desacuerdos filosóficos; y (T3) filosofías de la ciencia no-absolutistas concuerdan con la historiografía de la ciencia y, por lo tanto, facilitan integrar historia y filosofía. Por último, explico los criterios de muestreo que usé para seleccionar mis casos de estudio y aclaro el papel metodológico de la así llamada "meta-alternación" en mi caracterización y defensa del razonamiento evidencial en HFC.

ENGLISH:

This dissertation aims at vindicating evidential reasoning in the history and philosophy of science (HPS), arguing that historical evidence supports philosophical claims about science. I tackle two sceptical arguments contending that evidential reasoning in HPS is epistemically objectionable and must therefore be abandoned. These are the circularity argument and the unsuitability argument —which conclude that historical evidence does not support philosophical theses. For this aim, I draw upon three works in HPS as case studies: (1) Lakatos' historiographical research programmes, (2) Stanford's strategy of historical ostension, and (3) Chang's integrated HPS. In examining how these works employ historical material to support philosophical conclusions, I propose a solution to the problem each sceptical argument posits.

Chapters 1 and 2 outline the sceptical position. The circularity argument rests on the premise that historical reconstructions are not independent of the philosophical theories being tested, which is motivated in two ways. The charge of manipulation of history maintains that philosophers arbitrarily manipulate historical material for case studies to fit the favoured philosophical theory. The objection from historical pluralism argues that, even if historical material has not been manipulated, historical reconstructions are essentially laden with competing philosophical theories and hence historical evidence is unable to adjudicate philosophical disagreements. Meanwhile, the unsuitability argument claims that the history of science is inappropriate for the philosophy of science as both disciplines are mutually incompatible. Metaphysical unsuitability contends that there is an inherent tension of integrated HPS as historians and philosophers embrace conflicting metaphysical commitments about science. Furthermore, epistemic unsuitability complains that historical data are ill-suited (if not dispensable) to sustain general and normative philosophical conclusions.

Chapters 3, 4, and 5 contain my three case studies, in which I defend evidential reasoning in HPS against both sceptical arguments. I handle the circularity problem with the case studies of Lakatos' and Stanford's works, and use the case study of Chang's work to address the unsuitability problem. Case study (1) provides a typology of independent historical evidence to characterise different forms in which rational reconstructions of historical episodes are arguably independent of the philosophical theories of scientific rationality being tested. I draw this typology according to the kinds of theory used to produce historical reconstructions and the relevant interaction between them. Case study (2) shows the role of historiographical standards in evaluating historical adequacy are crucial to resolving the selective scientific realism debate concerning Psillos' and Stanford's competing reconstructions of the caloric episode. Second, I claim conditionally that if Stanford's forms of manipulating historical material do not yield unreliable data, then his historical study of 19th-century theories of biological inheritance supports the problem of unconceived alternatives. Case study

(3) tackles the philosophical unsuitability of history on the basis of Chang's pragmatist epistemology. I propose that the inherent tension of integrated HPS can justifiably be seen as a form of operational incoherence, whose resolution can be to embrace activist realism on the philosophy side. As this philosophical theory involves non-absolutism, it ultimately leads to the integration of history and philosophy —which is a less-clear aim to achieve from other representative approaches to HPS.

The concluding chapter sketches an epistemology of HPS as derived from my casebased argumentative strategy. I formulate three epistemological theses concerning evidential reasoning in HPS: (T1) historical reconstructions satisfy types of independent evidence when they support philosophical theories; (T2) standards relating to historical adequacy are crucial to assessing reconstructions of historical episodes in order to settle philosophical disagreements; and (T3) non-absolutist philosophies of science are concordant with the historiography of science and therefore facilitate to bring together history and philosophy. Finally, I explain my sampling criteria for selecting my three case studies and spell out the methodological role of the so-called "(meta-)alternation" when I characterised and defended evidential reasoning in HPS.

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Introduction

This doctoral thesis is a work on the methodology or (as I prefer) the epistemology of the history and philosophy of science (HPS). My general purpose is to vindicate the epistemic status of evidential reasoning in HPS. I argue that historical evidence supports philosophical claims about science.

Whilst there are multiple ways of conceiving and practising HPS, I restrict my attention to the evidential character of this intellectual endeavour, in which historical reconstructions are employed as evidence for and against philosophical claims. For current purposes, I take it for granted this general notion of evidence: history of science is evidence that confers support upon philosophical claims in that reconstructions of historical cases justify or make reasonable to accept/reject such claims. On this view, evidential reasoning in HPS consists in giving epistemic justification to philosophical conclusions on the basis of historical reconstructions —which can also involve relying upon philosophical ideas to reconstruct historical episodes.

So construed, evidential reasoning in HPS offers an interface between history and philosophy in pursuing the aim of understanding science. It also gives primarily a method of philosophical assessment, assuming that "the *justification* of philosophical claims about how science works rests in part on the adequacy of those claims to actual science" (Laudan 1990, p. 49). Interestingly, though, evidential reasoning in HPS has been rejected over and over. Several authors contend that we have no good reasons for using historical evidence to support philosophical claims. This rejection presumably involves two key theses.

As a *descriptive thesis*, the view is that evidential reasoning in HPS has been rendered obsolete. For one thing, HPS-critics have emphasised the failure of history and philosophy of science. For instance, Zammito (2004) pointed out that "by the beginning of the 1990s, the link between history of science and mainstream philosophy of science became significantly attenuated" (p. 111). Shapin and Schaffer (2011) stated that it has been "a largely unsuccessful experiment, the 'history and philosophy of science'" (p. xxi). And Kuukkanen (2016) contended that "there is currently nothing like contemporary historical philosophy of science despite the promising start in the 1960s and 1970s" (p. 6). Furthermore, HPS-critics have indicated that evidential reasoning in HPS was abandoned by both historical and philosophical scholarship. Regarding historiography, Fuller (2019) declared that "no *historian* after Kuhn has tried her hand at divining philosophical lessons from the history of science" (p. 2). Within the philosophical sphere, Laudan's work stands out as an example. Zammito (2004) further noted that his work "failed to win over most philosophers of science" (p. 111). Schickore (2018) also claimed that his ambitious VIP project *Scrutinizing Science*, which aimed to test general theories of scientific change against historical evidence, "was never brought to a firm conclusion" and "was soon abandoned" (p. 202 n. 21).

The *descriptive thesis*, however, caricatures how things have been going in HPS. Recent research in the field contains representative works that are clear counterexamples of this thesis. Let me give a few examples. Whilst the following family of works are different to one another in several important respects, they all ostensively portray what evidential reasoning in HPS consists in.

The strategy of historical ostension employs historical case studies not only to articulate philosophical positions but also to justify philosophical theses related to the scientific realism debate. Stanford (2017), for instance, argues that "the historical evidence supporting both the pessimistic induction and the problem of unconceived alternatives should lead us to embrace [...] 'Uniformitarianism'" (p. 214). Furthermore, both realists and anti-realists have employed historical case studies to bolster their arguments. They have even discussed whether historical evidence is sufficient, necessary, or irrelevant in settling the scientific realism debate (e.g., Lyons and Vickers 2021; Magnus and Callender 2004; Vicedo 1992; Vickers 2013, 2017).

Another example is *scientonomy*. In its original formulation, this project aims at creating the empirical science of scientific dynamics. "Theoretical scientonomy" investigates general patterns of scientific change and describes them in terms of laws, thus providing a "theory of scientific change" (TSC). "Observational scientonomy" accounts for historical cases which the laws of scientific change apply to, thereby producing a "history of scientific change" (HSC). Barseghyan (2015) notes that scientonomy involves evidential reasoning in that "HSC explains individual episodes, provides TSC with historical data and tests TSC's general hypotheses" (p. 75). Correspondingly, Rupik (2019) proposes that *scientonomy* is a clear example of evidential reasoning in HPS.

The historical sociology of knowledge (SSK) is my third example. The "main method" of SSK "is to present historical case studies" (Barnes, Bloor, and Henry 1996,

p. viii). On this basis, sociologists have formulated two substantive philosophical theses: meaning finitism and epistemic relativism. Regarding finitism, Bloor (2004) makes it explicit that "the problematic character of concept application and the need to treat the move from case to case as a social phenomenon is clearly brought out by the historical study of scientific practice" (p. 927). As far as relativism is concerned, Kusch (2020) insists that "the first and 'flagship' SSK argument for non-absolutism is an induction on the history of science" (p. 197). Here "the detailed historical and sociological work on fundamental scientific disagreements" is "taken to support the 'historicist' generalization that justification is invariably local, contingent, and relative" (Kusch 2021, p. 53).

To give a fourth example, consider the *integrated HPS* approach. Chang has criticised the evidential, inductive use of historical cases. He instead suggests conceiving of HPS as a critical-hermeneutical relationship between history and philosophy, which facilitates both the articulation of philosophical concepts and the framing of historical reconstructions. However, this view remains aligned with evidential reasoning (e.g., Bolinska and Martin 2020). At the very least, the critical role of history is to provide philosophers with "counterexamples" (Chang 2011, p. 122) that lead to revise philosophical claims. For instance, Chang uses his own historical study of the Chemical Revolution to challenge monism about science, which ultimately serves to justify normative scientific pluralism vis-à-vis his complementary science project: historical studies "call into question the common intuition that there could only be one right answer to a scientific question, and that once science has answered a question definitively its verdict is final" (Chang 2012, p. 254). Similarly, Chang (2022) notes concerning entity realism that "the history of science is full of very successful practical interventions by experimenters who thought they were using entities that we now regard as unreal" (p. 151).

Lastly, it is worth mentioning the *epistemology of experiment*. Franklin has produced several historical case studies from which he draws epistemological conclusions about experimental practice. For instance, Franklin and Laymon (2021) delve into six episodes in the history of the physical and the biological sciences, examining the rational acceptability of experimental results that were not replicated. With this historical evidence, they make "the point that replicating an experimental result is not a necessary condition for its acceptability", since "sometimes once is enough for acceptability" (Franklin and Laymon 2021, p. 9).

Whilst this list of examples is not exhaustive, it serves to highlight that evidential reasoning in HPS as a line of research has stayed alive. Despite this, HPS-critics would not be impressed by these examples. They go on to insist that the problem is not whether evidential reasoning in HPS still exists in professional scholarship, but rather whether this practice holds a *positive epistemic standing* —*i.e.*, whether historical evidence supports philosophical claims about science. The fact that there are currently representative works is irrelevant for assessing the evidential use of history for philosophical purposes as "legitimate" or "correct". Giere (1973) formulated the problem as follows:

Although there are substantial differences among them, those philosophers of science who make serious use of the history of science form a loosely connected school within the philosophy of science. It is natural that the members of such a school should see their discipline in a different light from others. One would hope, however, the members of the school will not be content merely to practice their art but will make repeated efforts to explain and argue the rationale of their approach [...]. The general problem is to show that philosophical conclusions may be supported by historical facts and just how this comes about. Until this is done, the historical approach to philosophy of science is without a conceptually coherent programme. (pp. 291-2; my emphasis)

Thus, HPS-critics' view is first and foremost a *sceptical position*, according to which evidential reasoning in HPS is epistemically objectionable and must therefore be abandoned. If correct, this is a *normative thesis* that judges evidential reasoning as an illegitimate way of doing HPS. Accordingly, my examples of representative works would be nothing but instances of epistemically fraudulent reasoning.

Notice that Giere's scepticism hinges on a strong assumption: evidential reasoning is epistemically objectionable unless proven otherwise. This implies that HPS-practitioners must justify the positive epistemic status of evidential reasoning before engaging in this practice. However, HPS-critics do not necessarily need to rely upon Giere's assumption to argue for their scepticism. Rather, they can reasonably adopt a weaker claim: evidential reasoning holds a positive epistemic status unless proven otherwise. Here HPS-critics are tasked with offering reasons to demonstrate why evidential reasoning is epistemically objectionable. Crucially enough, HPS-critics do believe there are compelling reasons to "prove otherwise"; they have underpinned some worth considering arguments that bear out the normative thesis.

In this context, I aim to examine and criticise such sceptical arguments. I vindicate the positive epistemic status of evidential reasoning in HPS by rejecting the reasons which the *normative thesis* is based upon. I argue that *historical evidence does support* philosophical claims about science, so evidential reasoning is a legitimate practice. On these grounds, I claim that evidential reasoning has been deemed epistemically innocent, and it remains to be so until proven guilty once again.

In the first two chapters, I argue that the *normative thesis* is grounded in two pivotal sceptical arguments. Firstly, the *circularity argument* (Chapter 1) states that historical cases cannot support philosophical claims as historical reconstructions are not independent of the philosophical theories being tested. There are two ways of posing this problem of vicious circularity. The charge of manipulation of history maintains that philosophers arbitrarily manipulate historical material for case studies to fit the favoured philosophical theory. In producing unreliable historical data, manipulation prevents historical evidence from confirming philosophical theories. I show that manipulation stems from objectionable forms of abstraction and idealisation in historical representation that ultimately curtails historical adequacy. Meanwhile, the objection from *historical pluralism* argues that, even if historical material has not been manipulated, historical reconstructions are essentially laden with competing philosophical theories, thereby historical evidence is unable to adjudicate philosophical disagreements. Pluralism permits historical evidence to confirm philosophical theories but curtails theory-choice. In brief, historical pluralism is rooted in the idea that historywriting depends upon philosophical theory, that there is more than one historical reconstruction of the same historical episodes, and that there is no neutral way of adjudicating between competing philosophical positions and their corresponding historical accounts.

Secondly, the unsuitability argument (Chapter 2) claims that the history of science is inappropriate for the philosophy of science as both disciplines are intrinsically opposed to one another, thus concluding that historical cases are unable to make philosophical points. This problem of the philosophical unsuitability of history is primarily based upon a conflict argument, contending that history and philosophy cannot be brought together because historians and philosophers embrace conflicting metaphysical commitments about science. Whilst philosophical analysis embraces scientific absolutism in being essentialist, universalist, and theoreticist, historical analysis involves scientific nonabsolutism in being contingentist, localist, and practicalist. Besides the conflict argument, I also show that the unsuitability problem stems from a further line of objection, which argues not only that historical data is philosophically dispensable, but also that using such data leads philosophers to commit hasty generalisation and the naturalistic fallacy as inferential pitfalls. In chapters 3, 4 and 5, I defend evidential reasoning in HPS against these two sceptical arguments. To do so, I draw upon three works in HPS as case studies. In examining how these works employ the history of science (i.e., both primary and secondary sources) to establish philosophical conclusions, I propose a solution to the problem each sceptical argument posits. The three works I explore establish impressive philosophical conclusions about scientific rationality and realism. Specifically, I provide a case study of the following case studies:

- 1. Lakatos' *historiographical research programmes*, drawing a pluralist, diachronic view on scientific rationality upon the "rational reconstructions" of past science.
- 2. Stanford's *strategy of historical ostension*, formulating the problem of unconceived alternatives as a serious version of the underdetermination thesis based upon a "new induction" from the history of science.
- 3. Chang's *integrated HPS*, which proposes a new pragmatist philosophy of science by doing "history as philosophy".

I handle the circularity problem with the first two case studies, whilst employing the case study of Chang's work to deal with the unsuitability problem.

My case study (1) (Chapter 3) aims at solving the circularity problem as it emerges from the theory-dependence of historical reconstructions. The objection claims that no historical case study confers support upon the philosophical theory used to reconstruct such a case. I thus provide a typology of independent historical evidence to characterise different forms in which historical case studies are arguably independent of the philosophical theories being tested. I couch these forms in terms of type-independent evidence, token-independent evidence, and higher-order independent evidence, according to how rational reconstructions of historical episodes relate to the actual history of science in different ways. When rational reconstructions are used to critically compare methodologies of science, these types of independent historical evidence prevent the problems of manipulation and historical pluralism from arising. I develop this typology by examining how Lakatos used the history of the Copernican revolution to assess scientific rationality theories. Remarkably, this proposal aligns with Lakatos' practice despite his own methodological pronouncements.

My case study (2) (Chapter 4) shows how the circularity problem is tractable by rebutting the contentious claim that historiographical criteria are too weak in assessing rival historical reconstructions and their corresponding philosophical positions. I focus here on the role those standards play in evaluating the quality of two historical case studies used in the historical challenge to scientific realism. On the one hand, I discuss whether the problem of manipulation jeopardises Stanford's historical study of 19thcentury theories of biological inheritance. I maintain that there are feasible reasons for the antecedent of this conditional claim: if Stanford's forms of manipulating historical material do not yield unreliable data, then he is entitled to confirm the problem of unconceived alternatives. On the other hand, I argue that Stanford avoids the problem of historical pluralism regarding the selective realism debate concerning the caloric theory of heat. I claim that historiographical standards relating to historical adequacy are strong enough to resolve the dispute between Stanford and Psillos vis-à-vis their competing reconstructions of the caloric episode. I thus contend that Stanford's case study is more historically adequate than Psillos' reconstruction, thereby concluding that the caloric episode does not bear out the realist strategy of selective confirmation. This case study suggests that the canons of historical criticism matter to HPS.

Finally, my case study (3) (Chapter 5) manages to vindicate the suitability of the history of science for philosophical theorising by resolving the alleged disciplinary conflict precluding integrated HPS. The objection argues that history and philosophy are mutually incompatible, since philosophers are committed to scientific absolutism whilst historians instead embrace scientific non-absolutism. I apply Chang's pragmatist epistemology to resolve this problem. I claim that the inherent tension of integrated HPS can justifiably be seen as a form of operational incoherence, whose resolution requires adjusting the philosophy of science being adopted. Accordingly, I pragmatically (in)validate standard realism and activist realism as competing philosophical theories. Whilst standard realism adopts scientific absolutism and therefore creates the conflict between history and philosophy, activist realism involves non-absolutism, thus making integrated HPS an operationally coherent practice. As an instance of non-absolutist philosophy of science, activist realism is quite compatible with the metaphysical commitments about science assumed by historians, thereby facilitating an understanding of science in both historical and philosophical terms.

Having done so, I turn to cover three points in the last chapter (Concluding remarks). Firstly, I show how the three works in HPS I have examined can successfully counter the problem of *epistemic unsuitability*, given that I provide no additional case study to address this particular sceptical concern. Secondly, I unpack the criteria I employed to sample my three case studies. I picked out these cases rather than others for at least four main reasons. These three works take it for granted that history is philosophically indispensable, they put themselves in continuity with science, they are diverse in important respects, and they are hard cases or paradigm cases vis-à-vis the

two sceptical arguments. These reasons ultimately block the objection that I would be committing cherry-picking in sampling my cases and making hasty generalisations when I draw epistemological conclusions on their basis.

Thirdly and most importantly, I outline the epistemology of HPS that derives from my case-based argumentative strategy. I formulate some epistemological lessons about the works in HPS I have examined, which also concern evidential reasoning in HPS more generally. Then I explain how these lessons are justified by my argumentative strategy. In brief, the works I have examined are hard and paradigm cases, from which I provide both a confutation of the two sceptical arguments and present a plausibility proof for an ampliative conclusion about the epistemic status of evidential reasoning in HPS. First of all, I draw a case-bound conclusion, namely that the circularity argument fails to undermine the positive epistemic status of both Lakatos' and Stanford's works, whilst the unsuitability argument is unable to defeat the epistemic status of Chang's work. As such, each of these case studies offers a counterexample to the general conclusion of both sceptical arguments, which is that historical evidence does not support philosophical claims.

This case-based conclusion allows me to put forward an existential generalisation claim, namely that *there is historical evidence that supports philosophical claims*. This generalisation is plausibly justified by the positive theses I have abstracted from my three case studies, which can be formulated as follows:

- (T1) Historical reconstructions satisfy types of independent evidence when they support philosophical theories (as elicited from historiographical research programmes).
- (T2) Standards relating to historical adequacy are crucial to assessing reconstructions of historical episodes in order to settle philosophical disagreements (as concluded from the strategy of historical ostension).
- **(T3)** Non-absolutist philosophies of science are concordant with the historiography of science and therefore facilitate bringing together history and philosophy (as established from integrated HPS).

Whilst each thesis holds very properly as instantiated by the specific works I have scrutinised, it arguably seems that they encapsulate some general aspects of evidential reasoning in HPS. My case-based argument showcases some *epistemic properties* of this methodology, namely that there are *types of independent historical evidence*, *historiographical standards for evaluating reconstructions and adjudicating philosophical disagreements*, and philosophical theories quite compatible with historical research.

Accordingly, as these properties are *epistemic qualities*, they can justifiably be couched in terms of the following *epistemic desiderata* for evidential reasoning in HPS: a *desideratum of independent historical evidence*, a *desideratum of historical criticism*, and a *desideratum of concordance between disciplinary principles*. This means that instances of *epistemically valuable* evidential reasoning in HPS are expected to meet these three desiderata.

So I put the ampliative conclusion as follows: there are further instances of evidential reasoning in HPS that meet at least one of the three epistemic desiderata, in which historical evidence therefore supports philosophical claims. This thesis is prima facie acceptable insofar as there are good reasons for its plausibility. I provide the following three reasons: the proposed epistemic desiderata can justifiably be extrapolated (i) by considering my sample of case studies, (ii) given that my sample involves hard-cases and paradigm-cases alike, and (iii) to the extent that HPS-critics have not yet proved that such extrapolation fails.

As such, the ampliative conclusion takes the form of a hypothesis to be tested. Producing more case studies of historical case studies is thus required for judging whether other works in HPS do meet the epistemic desiderata. This is a task for future research, where HPS-critics now have the burden of proof: until they manage to demonstrate that a host of works in HPS are epistemically objectionable and must therefore be abandoned, those works can justifiably be seen as having a positive epistemic status. In the meantime, the HPS-theorist can confidently return to her desk as an HPS-practitioner. It is worth recalling that evidential reasoning in HPS is innocent until proven guilty.

I find a grain of truth in asserting that intellectual paralysis "follows from too much self-reflection on method" (Collins 2008, p. 103). However, a total absence of critical self-reflection is pernicious. It results in dogmatism, which dismisses past shortcomings and masks the potential avenues for enhancing the historical and philosophical study of science. Fortunately, most HPS-critics have been HPS-practitioners at the same time, and dogmatism is not a default position. I am not alone in defending evidential reasoning in HPS. Recent contributions include Bolinska and Martin (2020), Currie (2015), Knuuttila and Loettgers (2016), Scholl (2018), and Rupik (2019). The main value of reflecting upon the epistemology of HPS arguably lies in taking responsibility when necessary, blending humility and tenacity in engaging with (and theorising about) HPS as an epistemic practice. The following pages are intended to contribute towards this aim.

Chapter 1

The argument from circularity

Abstract

This chapter formulates the first sceptical argument against evidential reasoning in HPS. This argument states that historical case studies cannot support philosophical claims about science as historical reconstructions are not independent of the philosophical claims being tested. I examine two lines of thought that justify this claim. The objection from pluralism argues that historical reconstructions are essentially laden with competing philosophical theories, thereby making historical evidence unable to adjudicate philosophical disagreements. Pluralism permits historical evidence to confirm philosophical theories but curtails theory-choice. The objection from manipulation contends that philosophers of science arbitrarily manipulate historical material for the case study to fit the favoured philosophical theory. In producing unreliable historical data, manipulation even prevents historical evidence from confirming philosophical reconstructions and philosophical conclusions is viciously circular, whereby the evidential reasoning turns out to be either dialectically ineffective or self-serving.

1.1 Introduction

The worry about vicious circularity concerning evidential reasoning in HPS derives from the theory-dependence of historical evidence. According to this, the reconstruction of historical episodes presupposes philosophical theories. Therefore, "philosophical claims cannot really be tested against the historical record because the historical record is not independent from the theory" (Schickore 2011, p. 467). More precisely, the sceptic poses a problem of vicious circularity based upon the following argument:

The circularity argument:

1. For historical evidence to support philosophical claims, historical evidence must be independent of the philosophical claims being supported.

- 2. Due to *historical pluralism* and the *manipulation of history*, historical evidence is not independent of the philosophical claims being supported.
- 3. Therefore, historical evidence does not support philosophical claims.

To begin with, premise (1) takes it for granted that the *independence of evidence* is a necessary condition for historical data to confer justification on philosophical claims. This is so because the relation of evidential support will ultimately be viciously circular provided the historical evidence is not independent of the theory it purports to test. Kosso (1992) puts the idea in such intuitive terms:

Imposing the standard of independence blocks circularity in the use of evidence to prove theory and thereby prevents a theory's self-help of providing its own proof. It is the same standard of justification that requires that a court of law listen to more testimony than just that of the defendant. The defendant cannot be trusted to proclaim his own guilt or innocence. Nor he can be trusted to authenticate the material evidence. "That's not mygun!" (p. 163)

Suppose a philosopher employs a philosophical theory to reconstruct a particular historical episode, and then she appeals to the historical reconstruction as evidence of that same theory. This reasoning is viciously circular, so the sceptic argues. The circularity consists in that the philosophical theory is based upon the historical evidence, and the historical evidence is based upon such a theory. As such, this basing relation turns out to be epistemically vicious because historical evidence transmits no justification to the philosophical theory; the theory becomes the only and ultimate source of its own justification. Of course, the philosopher can break the circular reasoning as long as the philosophical theory is based upon the historical evidence, the latter not being based upon the former notwithstanding. The fact that historical evidence is not based upon the philosophical theory being tested makes historical evidence independent of it. In this case, the theory is receiving a justification from a source that does not come from the theory itself.

The theory-dependence of historical evidence yields at least two dangerous effects of vicious circularity. A first consequence is that evidential reasoning is *self-serving*. Vicedo (1992) asks rhetorically: "if we have to adopt a specific position to interpret historical data, how can we use these same data to support our philosophical position?" (p. 491). Permitting such circular reasoning would make philosophers "able to prove absolutely anything, however intuitively unjustifiably" (Boghossian 2001, p. 11). Even the most absurd philosophical claims about science could be supported by historical case studies which were written in terms of those claims. A further consequence is that evidential reasoning is *dialectically ineffective*. Permitting circular reasoning implies that I cannot persuade you that my historical case study supports my proposed philosophical theory when you already disagree with me, and vice versa. It plausibly seems that "this maneuver offends against the very idea of proving something or arguing for it" (Boghossian 2001, p. 11.).

Having conceded the desideratum of independence for historical evidence, the cogency of the *circularity argument* depends upon establishing premise (2), which states that historical case studies constitute no independent evidence. The sceptic underpins this premise in two ways. Firstly, the objection from *historical pluralism* relies upon the general point that historical reconstructions are essentially theory-laden, and upon the specific idea that the theoretical resources to select and interpret the historical material come from competing philosophical theories. Thus, it is *dialectically ineffective* to support *rival philosophical theories* upon their corresponding historical disagreements as this procedure is question-begging. Secondly, the objection from *manipulation of history* claims that philosophers arbitrarily select and interpret the historical record for the historical case study to fit the favoured philosophical theory. As no discrepancy can be found between the proposed philosophy and the historical case study, it is *self-serving* to support a *single philosophical theory* by cooking up historical data.

Whilst historical pluralism curtails theory-choice because historical data constitute *no shared evidential base* for rival philosophical theories, the problem with manipulating history is that it precludes the *confirmation* of the philosophical theory as historical data is *unreliable*. I want to spell out these two objections in turn.

1.2 Historical pluralism

Pluralism in historiography is sufficient for the circularity problem to arise considering rival philosophical theories that are used to reconstruct the same historical episode. It is not clear here "how to relate the history to the philosophical point without begging the question" (Pitt 2001, p. 374). The central idea is that historical data are theory-laden because the past is always reconstructed from philosophical frameworks that can be in conflict. This constitutive aspect of historical representation ultimately explains why "competing historical reconstructions are possible" (Kinzel 2016, p. 125). Specifically,

historical pluralism takes place so long as these two conditions obtain: "(a) when there exist conflicting accounts of the same historical episodes, and (b) when it is not obvious which of the different reconstructions is the correct, adequate or most plausible one" (Kinzel 2016, p. 130).

The argument that underpins both conditions goes in three steps. Firstly, this couches the theory-dependence of historical reconstructions in terms of three methodological resources that are constitutive of historical analysis. Secondly, this argues for condition (a) by showing how those essential resources lead to plurality and conflict vis-à-vis historical accounts. And thirdly, this makes the case for condition (b) by explaining why actual instances of disagreements between rival philosophical theories and the corresponding reconstructions of the same historical episode cannot be adjudicated as a result.

1.2.1 Theory-dependence

Kinzel (2016) maintains that theory-dependence is an aspect of historical representation that is "necessary" rather than "contingent" (p. 126). Appealing to a pragmatist theory of scientific representation, she focuses on the idea that representation presupposes a target system being represented and a user of representation. The target system is always represented from a particular user's perspective, which typically encompasses theoretical frameworks, methodological strategies, and professional goals and interests.

This pragmatist account also applies to historical reconstructions. Historical facts are the target of historical representation, and historians adopt a perspective from which they represent those facts. For one thing, a "fact of the past" becomes a "historical fact" by having "historical significance" for the historian (Kragh 1987, Chs. 4-5). The historian aims at understanding such facts by means of historical reconstructions, which are accounts that provide descriptions, interpretations, explanations, and even evaluations of past science. Furthermore, historical reconstructions are always relative to a point of view. Contrary to the assumption of positivist historiography (Ranke 2021), no direct access to historical facts is possible whatsoever, since "the historical fact is not a simple given, but rather the outcome of a complex and partly constructive methodological process" (Kinzel 2015, p. 51). Historians can only offer historical (re)constructions by which facts of the past come to be intelligible to us.

This constructive process of reconstructing past science involves three methodological resources that are constitutive of historical analysis. The first resource is *selectivity*, which is related to theory-guidance. No historical reconstruction is a complete representation of the corresponding episode. Historians must choose some relevant elements of the historical situation at stake for certain purposes. There are at least three ways of selecting elements from historical material, to wit: delimitating the time-span of the episode, identifying the subject of the episode, and choosing the most important set of explanatory variables. In constructing case studies, historians pick out the topic that is *historically significative* (i.e., the "what" question), *periodise and localise* the episode (i.e., the "when" and "where" questions), and account for that episode in terms of what they consider as *relevant explanatory factors* (i.e., the "how" and "why" questions).

The second resource is *narrativization*. Narrative text is a distinctive form by which historical facts are represented. Historical reconstructions narrate temporal processes, meaning that representing what happened in the past is telling a story about it. A salient aspect of historical narratives is thus the *emplotment*, which is how events are depicted by establishing an assembled chronicle according to a specific plot and using different story genres. According to Kinzel, the story-telling style confers significance and meaning to historical episodes, thereby conveying information and knowledge about them (Kinzel 2016, p. 128). In providing understanding of episodes, narratives not only describe the stages of the succession of events, but also answer the reader's why questions at the endpoint of the narrative, when the story reaches a resolution.

The third and final methodological resource is *theory-ladenness*. Whilst theoryguidance enables historians to delimitate and approach the investigation by making particular selective choices, theory-ladenness concerns the role theoretical assumptions play in structuring historical narratives. Historical reconstructions are theory-laden in that historiographical perspectives partially *constitute* historical facts. Since historical facts are not given whatsoever, historians must interpret and infer such facts from historical sources, whereby a theoretical framework always informs interpretation and inference (Kinzel 2015, p. 52). Theory-ladenness is therefore related to the *content* of historical narratives and the *procedure* for history-writing.

1.2.2 Plurality

The methodological resources described above give room for conflicting accounts of the same historical episodes —so condition (a) for historical pluralism obtains. Regarding selectivity, Kinzel (2016) indicates that "selective choices in historiography are not arbitrary" (p. 126). Whilst selection is arguably subject to historiographical standards, historians are entitled to pick out aspects of the historical episode in different ways. She further stresses that selection is "aim-dependent". As long as goals and interests vary among different historians, there exists a variety of historical reconstructions that conflict with one another. Not surprisingly, historians do give rival accounts

of the same historical case. As an example, Kinzel (2015) mentions Cantor's and Shapin's competing reconstructions of 19th-century Edinburgh Phrenology, indicating that "the two authors make different theory-guided selections of historical events, they include different types of information, and emphasize different aspects of the scientific controversy" (p. 52). Therefore, multiple historical reconstructions conflict with one another as historical analysis is based upon different selective choices.

Kinzel (2016) also argues that the narratological character of historical representation leads to multiple forms by which the same episode can be told: "the repertoire of culturally preexisting genres and story types is vast, and one and the same historical episode can be rendered intelligible in manifold ways by drawing on different story types and modes of emplotment" (p. 129). The disagreement between Collins and Franklin over the gravity waves episode is a case in point. Kinzel (2016) maintains that these two competing historical accounts of why Joseph Weber's experimental results were ultimately rejected "rest on different narrative emplotments of the episode" (p. 134). Collins's reconstruction counts as an "ironic tragedy", in which "Weber is excluded by the society to which he tries to belong" and his "downfall is not a result of him being in error" (p. 134). Meanwhile, Franklin's reconstruction is an "adventure story", which depicts Weber as an "anti-hero of the story" who was progressively defeated by the arguments of the scientific community (p. 135). Thus, historical reconstructions conflict with one another as historical analysis employs different types of narrativization.

Finally, theory-ladenness also leads to competing historical narratives. Kinzel (2016) points out that "since historical facts are theory-laden, disagreement is likely to emerge between historical accounts that reconstruct the past on the basis of different theoretical assumptions" (p. 128). For instance, she contrasts Musgrave's and Chang's renderings of the so-called Chemical Revolution. On the one hand, Musgrave uses the methodology of research programmes and thus reconstructs the episode in terms of successive sets of theories, where Lavoisier's Oxygen programme and Priestley's Phlogistonist programme were progressive and degenerating problemshifs, respectively. On the other hand, Chang characterises both chemical theories in terms of "systems of practice" that were methodologically incommensurable. So Musgrave draws "the conclusion that the Chemical revolution was a rational process", whilst Chang "interprets the situation in terms of the theoretical concept of methodological incommensurability and finds that the decision was not rational" (Kinzel 2016, p. 137). As more than one theoretical framework can justifiably be used to interpret the same historical episode, there are historical reconstructions that conflict with one another.

1.2.3 Precluding adjudication

Condition (a) for historical pluralism has salient implications for the evidential use of historical reconstructions. These implications lead to understanding more clearly the extent to which condition (b) is also established —i.e., historical evidence cannot adjudicate between rival philosophical theories and their corresponding historical reconstructions of the same episode.

Kinzel (2015) identifies these four ways of using history as evidence of philosophical claims:

(i) Novelty: History "provides us with new, previously unknown and perhaps surprising information" (p. 56) about the nature of science.

(*ii*) *Recalcitrance:* "It is the recalcitrant character of the historical material that enables us to learn from history in the sense of having to revise our beliefs" (p. 56).

(iii) Confirmation: "The available evidence makes the belief in question more justified, better warranted, more plausible, more acceptable, or more likely to be true, than it would be if the corresponding evidence were not available" (p. 56).

(i) Theory choice: "A case study may provide the philosophy of science with evidence that adjudicates between conflicting philosophical views" (p. 56).

The last two functions are directly related to the issue of whether historical case studies confer evidential support on philosophical conclusions. Notice that condition (a) for historical pluralism does not prevent historical evidence from *confirming* philosophical theories. Here confirmation is understood as *incremental confirmation*: historical evidence confirms a philosophical theory so long as historical evidence raises the probability of such a theory. More precisely, the probability of the philosophical theory given the historical evidence is greater than the probability of the philosophical theory alone. On this basis, the epistemic standing that is attributed to the philosophical theory also increases, meaning that the historical evidence makes the theory "more justified, better warranted," and so forth.

Despite this, condition (a) for historical pluralism does prevent historical evidence from *adjudicating* between rival philosophical positions; the theory-dependence of historical reconstructions precludes the possibility of *theory-choice*. The reason is that historical evidence cannot be a neutral arbiter for settling philosophical disagreements, since each philosophical theory makes its own historical reconstruction of the same episode. To show this, Kinzel draws an analogy between the theory-dependence of scientific data and the theory-dependence of historical reconstructions as characterised above. Evidence in science depends upon theory in that perceptual inputs are to be interpreted from a particular theory in order to make "observation reports" that count as empirical data. Such data give a base upon which to generate and test scientific hypotheses in a process of scientific inference. Due to the theory-dependence of scientific evidence, theory-choice is difficult because "two rival theories may each produce a corresponding body of theory-laden evidence" and hence "the evidence does not constitute a neutral ground on which to adjudicate between the rivals" (Kinzel 2015, p. 55). This means that competing scientific theories share no evidence base. Scientific evidence by itself is therefore unable to decide between those theories provided that adjudication implies an independent, neutral criterion of assessment.

The situation with historical evidence is similar. Historical facts are *inferred* from historical sources and *interpreted* from a particular historiographical perspective. This process brings about rival historical narratives —which will count as empirical data against which philosophical claims are to be tested. However, since this process leads to plurality and conflict regarding historical reconstructions, it follows that rival philosophical theories share no common evidence base. So historical evidence alone cannot adjudicate philosophical theories without an independent, neutral criterion to evaluate them. Kinzel (2015) thus concludes that "in situations in which one and the same case is reconstructed from competing philosophical viewpoints, the historical evidence alone cannot settle the philosophical conflict in question" (p. 55).

Kinzel goes on to reject alternative criteria for adjudication besides historical evidence. These extra-empirical criteria will permit to settle philosophical disagreements as long as they meet these two jointly sufficient conditions, to wit: *neutrality* (i.e., there are generally agreed-upon criteria for assessing the competing philosophical theories and their corresponding historical reconstructions), and *difference-making* (i.e., the criteria are strong enough to decide between the competing philosophical theories and their corresponding historical reconstructions). Philosophical standards and historiographical standards are two types of candidates, which unfortunately are insufficient to resolve disagreements. On the one hand, philosophical standards are assumptions that arguably seem to meet difference-making. However, those assumptions do not meet neutrality as they make up the auxiliary theory to interpret historical material; "these criteria themselves are theory-laden" and are therefore "highly contested issues" (Kinzel 2015, p. 55). On the other hand, historiographical standards relating to the quality of historical reconstructions arguably seem to meet neutrality but not difference-making; they underdetermine the disagreement provided that "these neutral criteria are weak in that they are easy to meet" (Kinzel 2015, p. 55).

The unpalatable lesson, then, is that historical evidence by itself and extra-empirical criteria are all unable to adjudicate philosophical theories and their corresponding historical reconstructions. So construed, condition (b) for historical pluralism takes place because neither rival historical reconstructions nor competing philosophical theories can be shown to be superior to one another. Pluralism regarding historical narratives stems from the fact that historical reconstructions cannot be ranked. Accordingly, pluralism regarding philosophical theories results from realising that there is no principled way of ranking such theories in light of historical case studies.

Thus, the sceptic is now in a position to summarise systematically the argument for historical pluralism as this:

The pluralism argument:

- 1. *Theory-dependence*. Historical analysis encompasses theoretical assumptions that facilitate selecting and interpreting historical episodes from historical sources. This theory-dependence is couched in terms of selective choices, narrative-writing, and theory-ladenness as methodological resources.
- 2. *Plurality.* There is more than one reconstruction of the same historical episode whenever a variation in such methodological resources is found. As this variation is a commonplace of historical discourse, a conflict between different historical reconstructions is likely to emerge.
- 3. Precluding empirical adjudication. In disagreements between two philosophical theories that were used to reconstruct the same historical episode, the historical evidence is not neutral provided the rival philosophical theories lack a shared evidence base. This makes historical evidence unable to adjudicate such disagreements. (From 1, 2.)
- 4. *Extra-empirical criteria*. Besides historical evidence, further criteria for adjudication can play a role as long as they are both neutral and make a difference. These can be philosophical standards that make up the auxiliary theory to connect historical reconstructions to philosophical claims, and historiographical standards to assessing the quality of historical reconstructions.
- 5. *Dilemma*. Philosophical standards are not neutral as they are also disputed by the two sides in the disagreement. Historiographical standards make no difference whatsoever as they "are too weak to settle all historiographical conflicts" (Kinzel 2015, p. 55).

- 6. *Precluding non-empirical adjudication*. Neither philosophical standards nor historiographical standards can resolve philosophical disputes. (From 4, 5.)
- 7. *Historical pluralism.* Therefore, there exist conflicting philosophical theories that produce rival reconstructions of the same episodes. In this case, it cannot be decided which philosophy and historical reconstruction is the correct, adequate or most plausible one. (From 3, 6.)

Naturally, the conclusion of this argument is sufficient to establish premise (2) of the *circularity argument*. This premise can be rephrased this way: *Due to historical pluralism, historical evidence is not independent of the philosophical claims being supported*—so the evidential reasoning is viciously circular in that it begs the question.

In establishing this premise, the sceptic will conclude with the *circularity argument* that historical evidence does not support philosophical claims as long as *multiple* philosophical theories conflict with one another.

All in all, the sceptic can motivate premise (2) in a different manner, contending that the problem of manipulating historical material seriously threatens the evidential use of history. This problem is stronger than the problem of historical pluralism because it makes historical case studies even unable to confirm philosophical theories. Manipulating historical material produces *unreliable historical data*, which prevents philosophical theories from being individually supported by historical evidence.

1.3 Manipulation of history

The manipulation pitfall is also sufficient for the circularity problem to arise even considering a single philosophical theory that is to be supported. The worry here is that "it could be argued that the historical data was manipulated to fit the [philosophical] point" (Pitt 2001, p. 373). Roughly, philosophers of science select and interpret historical material in a self-serving way, which precludes the confirmation of philosophical theories upon historical evidence as historical data turn out to be unreliable. So premise (2) of the *circularity argument* can be reformulated as follows: *Because of manipulating historical material, historical evidence is not independent of the philosophical claims being supported*—so the evidential reasoning is viciously *circular in that it is self-serving.*

I unpack the idea of manipulating history by examining how the sceptic would justify the antecedent of this conditional premise. I pay attention to how the philosophical approach to history proceeds, why it seems that the manipulation pitfall is likely to happen in historical analysis, and the extent to which philosophers fall prey to committing this pitfall very easily.

The procedure by which philosophers are presumably engaged with the history of science exhibits three silent aspects. First, *history of science is not the source of philosophical theorising*. Philosophers typically formulate the philosophical theory to be tested before looking at the historical record. I. B. Cohen (1974) indicates that "philosophers use history to provide an empirical base for their statements, or at least to find examples in the real world of science [...] which may illustrate a thesis of their own or confute a thesis of their opponents" (p. 349). Rossi (1986) also notes that Lakatosian historiographical methodologies "precede and are absolutely independent of historical analysis" (p. 48). On this view, history of science is not used to generate new philosophical ideas.

Second, *philosophical claims are the criteria for selection and interpretation*. Philosophers consult historical sources and employ the proposed philosophical thesis to identify and reconstruct the relevant historical episode. For Burian (2001), "the philosopher's interpretation of the material covered in the case study must be shaped by the very methodological or epistemological claim that is to be tested" (p. 385). For Brooke, "philosophical case-studies" rely upon philosophical claims to provide historical explanations. He discusses Causey's (1971) case study of Avogadro's volume hypothesis as an example, in which "Duhem's philosophical point is made the basis of a historical explanation: the existence of the 'pitfall' is presented as an important reason for the slow acceptance of the hypothesis" (Brooke 1981, p. 237). The historical case is therefore selected and interpreted in terms of the philosophical thesis at stake.

Third and finally, the historical case study fits the philosophical claim that was used to reconstruct that same case. Philosophers confront the favoured philosophical thesis against the historical case so reconstructed, thus concluding that the thesis is supported by historical evidence. Put differently, they claim that the historical reconstruction speaks in favour of the philosophical claim upon which such a reconstruction is based.

The problem with this procedure is not that philosophers reconstruct historical cases with philosophical theories, but rather that this form of historical reconstruction arguably seems *arbitrary*. This is arbitrary to the extent that reconstructions stemming from manipulating historical record are ultimately *fabricated cases*. Fabricating historical data is an easy way by which a philosophical theory can validate itself: the historical case study will always be a particular instance of the general philosophical claim as the historical case was selected and designed to fit that same philosophical conclusion. Burian (2001) thus argues that "because the cases are chosen and manipulated in these
ways, this procedure is guilty (perhaps inadvertently) of systematically cooking data to fit the investigation at hand" (p. 386). Accordingly, there are no principled criteria for determining the conditions under which a conflict between philosophical claims and historical evidence would take place. Therefore, one can justifiably suspect that any philosophical claim is supported by any historical case, provided no case study will be in discord with the philosophical claim in question. Brooke (1981) gives an example of this concern, noting that "for every hypothesis the historian propounds to explain the apparently delayed appreciation of Avogadro's hypothesis there will be a corresponding philosophical point to make" (p. 236). In short, the arbitrary selection and interpretation of historical material amounts to bad history (i.e., this produces unreliable historical data by cooking up historical facts), thereby preventing case studies from confirming philosophical claims (i.e., this allows philosophical conclusions to be their own proof).

It is important to note that the manipulation pitfall is a general concern for historians. There are reasons to think that manipulating historical material in the practice of reconstructing past science is hard to avoid. As this practice inherently involves selective and interpretative strategies, it is not clear how even historians can escape from the charge of cooking up the historical record in terms of the theoretical assumptions they employ in history-writing.

This risk of generating unreliable historical data emerges from two aspects of working with historical sources. The first aspect is the theory-dependence of historical representation, according to which the study of history always relies upon theoretical commitments. As explained in the previous section, this aspect is at the core of historical pluralism. Interestingly, though, a slightly different aspect of the historiographical practice is the *theoretical permeability of historical material*, which makes historical sources quite amenable to be analysed, organised, and interpreted from many different theoretical perspectives. Whilst theory-dependence implies that theoretical assumptions are indispensable for historical analysis, the theory-penetrability of historical sources gives room for historical material to underdetermine several forms by which historical episodes can justifiably be told and retold. Kuhn (1980) puts that concern this way: "The historian's problem is not simply that the facts do not speak for themselves but that, unlike the scientist's data, they speak exceedingly softly. Quiet is required if they are to be heard at all" (p. 183). Considering the role that beliefs about science play in historical analysis, Hull (1992) echoes Kuhn's remark by indicating that "their influence on the 'data' that are generated are likely to be even more pervasive and elusive than the parallel situation in science" (p. 471). Although theoretical assumptions seem to

operate in a subtle way, as tacit or partially formulated in historical accounts, "the influence of the general beliefs held by historians on the stories that they tell are obvious" (Hull 1992, p. 472). Historical material can therefore be easily penetrated by theoretical assumptions, provided that "historical 'facts' are usually pliable enough to accommodate a preconceived schema" (Brooke 1981, p. 249).

Because historical data is much more affected by theory than scientific data, the risk of using the historical material in a self-serving way is quite high. The historian will likely find in history the very hypothesis that has been postulated to deal with the historical problem in the first place. So how is one to show that the historical material has not been manipulated to fit either the "working hypothesis" (in the case of the historian) or the "philosophical point" (in the case of the philosopher)? In other words, how does one justify that the historical reconstruction providing an answer to a certain research problem is not self-serving?

Answering this question involves distinguishing between good and bad forms of historical analysis. This difference becomes clear when one realises that historical material is arbitrarily manipulated to the extent that standards of historical adequacy are not fulfilled. Manipulating history is therefore a characteristic of bad history of science. On these grounds, some authors have rejected the philosophical approach to history as not being good history. On this view, historiographical standards allow to draw a dividing line between the philosophical approach to history and the mainstream professional historiography of science.

I. B. Cohen (1974) asks philosophers: "is philosophy valid when derived from, or based upon, history that does not come up to the highest critical standards?" (p. 338). He points out that philosophers are "entering the domain of history" (Cohen 1974, p. 310) whenever they claim that a certain historical case illustrates or supports a philosophical point, since those claims amount to *historical statements*. For this reason, using historical material must be subject to norms governing professional historical research, and the philosophical use of history must therefore be evaluated as (in)correct on this basis.

The diagnosis has often been that the philosophical approach is ultimately wrong. For instance, Pitt (2001) argues that the philosopher engaged in producing case studies "is doing bad history" (p. 379), Brooke (1981) characterises "philosophical case-studies" as a "somewhat precarious enterprise" (p. 237), I. B. Cohen (1974) considers that "the philosophical use of history" yields "false" or "imagined" history (p. 349), and Vicedo (1992) insists that "the problem with this approach, then, is that it does not take history seriously" (p. 492). I turn now to reconstruct the reasons for contending that philosophers manipulate history in violating standards of historical adequacy. I draw upon some examples of how the philosophical approach writes bad history. In characterising historiographical standards vis-à-vis good history of science in some detail, I provide a feasible classification of those standards. In the end, this will serve to figure out why the manipulation pitfall comes to be a special conundrum for philosophers of science.

I suggest that historiographical standards relate to a couple of aspects of historical practice encompassed by the production and evaluation of narratives. These aspects are *abstraction* and *idealisation*. For present purposes, I am following Godfrey-Smith (2009) in understanding abstraction as omitting factual elements of historical situations —i.e., as *leaving things aside*. Historical accounts are abstract as they simplify historical situations by ignoring detail intentionally. Meanwhile, I characterise idealisation as misrepresenting historical situations intentionally —i.e., as *conceiving of things differently*. Idealisation is basically "representation as-if" (Potochnick 2017, p. 52). Historical accounts are idealised in that they represent historical situations "as having features they clearly do not have" (Godfrey-Smith 2009, p. 2). On this view, idealisations convey two ways by which they misrepresent history. That is, historians make either distortions of factual elements or fictionalisations that introduce non-factual elements in historical reconstructions.

1.3.1 Abstraction

Historical reconstructions are constitutively abstract; they always ignore certain elements compounding the historical situation under study. As discussed in the previous section, abstraction results from the selective strategies that historical analysis involves. Historians need to abstract from detail to answer questions. They select those elements that are relevant to understand the historical situation under study, while excluding others they consider irrelevant to do so.

The problem with the philosophical approach to history, however, is that this simplifies historical situations too much. The degree of abstraction of historical case studies is excessively high, thereby curtailing historical adequacy. Also, this *oversimplification* of historical events makes it difficult to understand past science in their own integrity, since it leads to overlooking the temporal dimension of science. In doing so, historical case studies are unable to talk about *history*.

For instance, Vicedo rejects historical *cases* because they leave aside *temporal processes*. She characterises case studies as providing information about isolated episodes from the *past* of science, but not about the *history* of science properly. Since

case studies do not account for the patterns that explain how science developed itself over time, they merely work with information about discrete aspects of the past. The role of historical investigation, however, is to figure out how the elements and aspects of science come to exist and grow temporally. As case studies cannot account for the dynamics of science, they deserve not to be called "historical" whatsoever.

Vicedo goes on to emphasise that the isolated and specific character of case studies results from manipulating historical material, particularly in selecting a "case" for making a philosophical "point". She notes that "by focusing on case studies we also run the risk of selecting only those episodes which support our views" (Vicedo 1992, p. 492). And if we can conveniently select a set of cases, why could not others do so to support many philosophical theses different from ours? This procedure is ultimately arbitrary. Vicedo (1992) thus concludes that "the evidential role that can be attributed to an isolated episode from the history of science is usually very low" (p. 492).

In a similar fashion, Pitt argues that historical case studies are not *historical accounts* as they ignore the *contextual character* of past science. He claims that history of science has "problematics" as its subject matter, which are reconstructed in terms of "contextualisation". Regarding problematics, historians formulate the historical problem at stake by identifying and selecting certain historical events: "problematics have their own history, they have starting points and end points, and in between they change, mutate, sometimes they evaporate, sometimes they metamorphise into something new" (Pitt 2001, p. 375). As for contextualisation, historians delve into historical contexts to identify the explanatory variables that account for problematics. This task consists in studying those factors which are relevant to understanding the processes by which problematics originate, develop, and come to an end: "a historical context is a set of factors that provide an explanatory framework for an event, a person's actions or work, or a social trend, etc." (Pitt 2001, p. 379).

Consider the mathematisation of natural philosophy during the 17th-century Scientific Revolution as an example. This problematic can arguably be contextualised in terms of how the metaphor of the world as a machine gave cause to reject the distinction between the "natural" and the "artificial" that "compromised the legitimacy of using in natural philosophy the sorts of procedures used by mathematicians" (Dear 1995, p. 153). Furthermore, the social process leading mathematicians to vindicate the status of their discipline is also explanatory: "the mathematical approach to the understanding of nature grew more persuasive as the mathematician became more authoritative. The mathematician began to acquire the cognitive authority previously reserved for the natural philosopher" (Henry 2002, p. 30). In contextualising problematics, it is possible for historians to leave out some aspects of the past arbitrarily, selecting both the problem and the context in a self-serving way. Pitt indicates that criteria for picking "what is a case study" and "what is the context" are required, since "without credible criteria for selecting or identifying a case as a case the charge can be legitimate" (Pitt 2001, p. 374). Because scientific past is complex and historical sources are always problematical, it is very hard for historians to have a principled way to construct an adequate contextualisation of problematics. As Pitt (2001) puts it:

In the course of working within the problematic, what emerges may not be what was expected. Finally, although this may seem obvious, to identify a problematic one must position it historically. This is to put the problematic in context, which is difficult, for in any historical setting there are many contexts, and we must avoid begging the question by selecting a context which conveniently supports our concerns. In short, if we start with case studies, we are assaulted on all sides by issues of question begging. (p. 375)

The fact that contextualisation is always problematical does not support scepticism about history, however. Demanding explicit and warranted criteria for selection and interpretation should not lead to adopting *monism* about historical explanation —i.e., a uniquely correct way of contextualising problematics. Against monism, Pitt (2001) contends that "the mistake to be avoided is assuming there is necessarily only one explanatory framework" (p. 378). Much on the contrary, the historiographical research shows that there is no such a thing as *the correct* historical explanation; there are rather multiple ways of contextualising problematics for different purposes:

A relevant set of contexts can be identified in terms of their explanatory value, i.e., the coherence they contribute to the story accounting for why what happened happened. One is justified in expanding the set of contexts to the extent that the failure to include certain factors can be shown to be relevant to understanding what happened after the events in question. The adequacy of the context is a function of its ability not only to account for the event in question, but also for its prior and subsequent history. (Pitt 2001, p. 379)

It arguably seems that historians of science can avoid the accusation of manipulation as long as they draw historical reconstructions by contextualising problematics and embrace *pluralism* about historical explanation. Presumably, this is what happens in mainstream professional historiography. Philosophers of science are not in the same position unfortunately. The philosophical approach is objectionable because it abstracts from historical situations what it is historically important, namely, that historical problems are contextual and interlace with other problematics. In doing so, philosopher's case studies both obliterate that any historical episode takes place over time and leave out the "prior and subsequent history". And as I. B. Cohen (1974) noted critically, philosophers are not "any less immune from historical criticism because they are concerned with a restricted rather than an extended subject" (p. 311).

To remedy this pitfall, Vicedo encourages philosophers to move from studies of cases to studies of scientific *processes*, which will be quite fruitful for drawing philosophical theorising upon historical evidence. Her example concerns the scientific realism debate. Regarding the question of whether we should believe in theories according to some criteria for theory-choice, Vicedo indicates that the development of a theory is crucial to figure out its rational acceptability. Thus, "the past record of a theory is important for both realists and antirealists, and this could be an area of common interest in the history of science" (Vicedo 1992, p. 494).

As long as historical representation cannot avoid abstraction, the problem is only how much abstraction is permissible. This draws a key difference between abstraction and idealisation. Mainstream historiography does not seem to tolerate idealisations in historical reconstructions. In misconstruing the past, idealisation *invariably* prevents narratives from meeting historical adequacy, which arguably is the inherent goal of historical analysis. As such, both distortion and fictionalisation are ways of manipulating the historical material which create nothing but unreliable historical data.

1.3.2 Distortion

Historical case studies distort historical facts in the sense that they caricaturise complex historical situations. Distorted historical reconstructions exhibit two salient aspects. First, a philosophical point is introduced to explain the historical episode, and its explanatory role is emphasised as if the point were the only relevant factor. Second, such a philosophical point makes other factors explanatory irrelevant, which will ultimately be excluded from the historical narrative. This characterisation plausibly squares with what Weisberg (2007) denominates "minimalist idealisation" in science. On his view, minimalist idealisation creates a "minimal model of the phenomenon" that "contains only those factors that *make a difference* to the occurrence and essential character of the phenomenon in question" (Weisberg 2007, p. 542). Perhaps Brooke (1981) provides the best illustration of how "philosophical case-studies" are "minimal models" in such a sense. Interestingly, though, he takes distortion to be a vice rather than a virtue as far as historical explanation is concerned.

Brooke (1981) criticises how historical situations are "seized as a case-study to illustrate one philosophical point or another" (p. 235). Specifically, he examines two case studies that use a philosophical thesis to explain chemists' neglect of Avogadro's volume hypothesis between 1811 and 1861. According to Causey (1971), the fate of Avogadro's hypothesis is explained as an instance of Duhem's underdetermination problem. According to Frické (1976), the fate is explained as an instance of "degenerating problemshifs" of research programmes. Both explanations are normative in character. Whilst the former claims that the resistance to accept the equal volume hypothesis was a *mistake* according to confirmation holism, the latter states that such a rejection was *rational* in terms of Lakatos's normative methodology. Brooke (1981) characterises these accounts as caricatures that are not historically adequate when he asks: "Does this imposition of a philosophical point on the chemical literature illuminate or caricature the historical situations? One is reluctantly led to the conclusion that it is the latter" (p. 237). He shows in a great deal of detail how both case studies exhibit this key contrast between "illuminate" and "caricature" the same episode.

Regarding Causey's account, Brooke maintains that the Duhemian pitfall distorts the episode as it leads Causey to make these two "simplistic assumptions": (i) Avogadro's hypothesis was of maximum usefulness, and (ii) Avogadro's research tradition is the commensurable extension of other previous theoretical contributions. Assumption (i) fails to characterise the aim and function of Avogadro's hypothesis and cannot accurately describe the actual practice of chemists in the 19th century. The assumption contains a counterfactual evaluative claim: if chemists had appreciated Avogadro's hypothesis from the start, then chemists would have encountered a principled criterion for atomic weight determination. Causey thus proposes that the actual neglect of the hypothesis left chemists into "confusion" about the atomic weight problem for several years. Brooke argues that this point is wrong, because Avogadro's hypothesis provided a criterion for weight determination of molecules instead of atoms, the hypothesis' application was very limited concerning the weights of solid materials, and the atomic weight problem remains an open puzzle as no chemist of that period (Avogadro included) had an independent method for determining atomic weight (Brooke 1981, pp. 240-22). Moreover, assumption (ii) is not historically defensible because Avogadro's work was closer to Berthollet's physical chemistry and the theory of caloric, so it cannot be linked to the atomist programme. Likewise, Avogadro's aim was to

determine affinity forces rather than solve the weight determination problem, where his technical terminology "was increasingly foreign to the chemical atomists" (Brooke 1981, p. 246). So Avogadro's programme "became both geographically and conceptually isolated" (Brooke 1981, p. 246). This suggests the idea that Avogadro's work was "incommensurable" concerning atomist chemistry, thereby contradicting that Avogadro extended the atomist programme.

The methodology of research programmes also generates a distortion of the same episode. For instance, Frické misplaces Avogadro's research programme within the atomist chemical one, thus characterising the equal volume hypothesis as a "degenerating problemshift", provided that "his method of atomic weight determination was as ad hoc as the divisibility of his molecules" (Brooke 1981, p. 247). In parallel, Frické reconstructs Cannizzaro's programme as a "progressive problemshift". In resolving the weight determination problem, the postulation of polyatomic molecules was a novel prediction. For Brooke (1981), though, "it is this claim for a 'startling and unexpected consequence' which, for the historian, lacks the ring of authenticity" (p. 248). Cannizzaro's work constituted no research programme whatsoever and "it is almost impossible to believe that Cannizzaro was startled by an unexpected inference to polyatomic elementary molecules" (Brooke 1981, p. 248).

From these two examples, Brooke (1981) contends that "if one's ultimate goal is an understanding of what actually happened there must surely be a limit beyond which the degree of caricature becomes unacceptable" (p. 247). Unacceptable caricatures of history stem from the following two general faults.

Firstly, there is a "distortion of emphasis" in *exaggerating the explanatory role of philosophical points*. No single philosophical hypothesis is sufficient to capture the historical episode in all its complexity: "When the circumstances and the problems were so complex, the isolation of a single philosophical or methodological point as the key to an adequate explanation must lead to a distortion of emphasis" (Brooke 1981, p. 257). Arguably, no simplistic hypothesis of why Avogadro's work was dismissed for almost 50 years provides an adequate explanation of the historical process. Philosophers caricature past science because they reduce the causes of what happened to a unique philosophical reason for why things occurred in the way they did. Contrary to this approach, I. B. Cohen (1974) insists that "a careful analysis of the historical record discloses no simple or universally applicable rules for making discoveries, no automatic path from experience to concepts, no unambiguous formation of theories or devising of experiments" (p. 335). Hence Rossi (1986) suggests that "an antecedent philosophical

position cannot constitute the only and inviolable principle for any historiography intending to be meaningful and relevant" (p. 50).

Secondly, there is a "failure of differentiation" in *leaving other factors out*. In imposing a "monolithic rational structure on a complex historical situation" (Brooke 1981, p. 252), "philosophical case-studies" elide important differences among multiple elements of the historical episode at stake. Specifically, they create the image that all historical actors' thoughts and actions fit the philosophical point entirely. For instance, the imposition of Lakatos's methodology on Avogadro's episode implies that "Avogadro's work was perfectly well known and comprehended by every chemist, that it was seen by them all to be degenerate because the postulation of divisible molecules was so indisputably ad hoc" (Brooke 1981, p. 252). This statement is historically inaccurate in putting together all the authors who were not aware of Avogadro's proposal for multiple reasons. So "the richness of scientific enterprise is altogether lost when the dead hand of reconstruction fails to discriminate" (Brooke 1981, p. 253). In short, this depicts historical episodes as if there was no *diversity of factors*.

This failure takes a rather slightly different form regarding rational reconstructions. In assuming a "priority of internal history", those reconstructions draw a distinction between "internal/rational" and "external/non-rational" factors upon the philosophical point. Internal history explains the rationality of science and external history explains the deviations from rationality. Internal history portrays what science is and hence defines those issues requiring an external explanation. The problem here is that it is quite artificial to think that "internal" and "external" explanations are mutually exclusive rather than complementary. For Brooke (1981), "to suppose that only 'dissenters' are affected by externalist aims or constraints is arbitrary" and ultimately creates "an impoverishment of historical understanding" (p. 254). Rossi (1986) reinforces this idea when he notes that "the so-called 'rational reconstructions' have been carefully deleted" (p. 55). Internal history of science also includes a history of "error". In short, rational reconstructions depict historical episodes as if "external" factors were not relevant.

If distorting history is a bankrupted enterprise, then preserving as much detail as needed will enhance the adequacy of historical accounts. Rossi (1986) maintains that historians "must vindicate the need for a non-schematic and non-simplified examination of temporary processes" (p. 195), because they "must not be afraid of displaying as complicated rather than linear those articulated processes otherwise presented as simple and unidirectional" (p. 175). Not surprisingly, Henry (2002) makes it explicit that "a striving for an ever richer contextualization" has been "the driving force in current historiography of science" and "the main ambition of the majority of its practitioners for a number of decades" (p. 7).

Notice that idealisation as distortion does not mean that philosophy-based explanations lack historical adequacy in that they are *fictions*. After all, "there would seem to be a grain of truth even in the oldest and most cursory explanations" (Brooke 1981, p. 255). Rather, the problem with caricatures is that they turn out to be quite *inaccurate*. This marks an important difference between distortion and fictionalisation as varieties of idealisation. The second form of idealisation yields *fictional history* in not being concerned with "historical truth" whatsoever.

1.3.3 Fictionalisation

Historical case studies amount to fictional history to the extent that they introduce fictitious elements and aspects in the historical episodes to be explained. The salient feature of fictional history, then, is that it is not about "what really existed" in the past. I. B. Cohen (1974) calls it "false history", in which historical narratives make false statements due to a falsification of the historical record. In this case, the fictional character of historical accounts depends upon how the analysis leads to falsifying historical material.

I suggest that there are some aspects of the historiographical practice that shed light on the fictional character of historical accounts. Specifically, a historical reconstruction would be fictitious to the extent that historical analysis (i) commits anachronistic errors, (ii) decontextualises historical sources in selecting and interpreting the relics of the past, and (iii) relies upon unreliable sources. Whilst the first two aspects are characteristic of present-centred historiography, the third aspect seems to result from lacking historical method. The philosophical approach to history arguably exhibits this triad of aspects.

Anachronism

A first way of falsifying history is by projecting present categories onto the past. Anachronism conflates the context of the past and the historian's context vis-à-vis ideas and concepts. The problem with this approach is that "historical misunderstanding" cannot be avoided, which is possible given the "probable disjunction between the category-systems of the past and the present" (Ashplant and Wilson 1988, p. 269). Projecting present categories onto the past would be legitimate if "the sources were constituted with the same category system" (Ashplant and Wilson 1988, p. 267). However, the context of the past and that of the present seldom coincide with one another given the temporal distance between the two. Thus, attempting to understand the conceptual context of the past in terms of present categories will lead the historian to state what it is true of the present, thereby stating something that is not true of the past. In short, anachronism introduces true elements of the present which become fictitious when those elements are imposed on the past.

I. B. Cohen gives a good example of the anachronistic error as reflected in some accounts of "early modern science". He criticises the view that Newton's theory was the "synthesis" of Galileo's laws of falling bodies ("terrestrial mechanics") and Kepler's laws of planetary motion ("celestial mechanics"). Since Galileo's and Kepler's laws can be logically deduced from Newton's three laws of motion, Newton's theory contains Galileo's and Kepler's as a "limit case".

This claim about the history of "early modern physics" instantiates a general statement about theoretical change and reduction that can be found in the philosophical literature (e.g., Kemeny and Oppenheim 1956; Nagel 1961). In this case, there is a *historical statement* about the formation of 17th-century science of motion that allegedly supports a *philosophical claim* about scientific progress. In accepting this view, however, some philosophers have made specific claims that are historically false. As an example, Galileo has been portrayed both as creating a radical discontinuity with his medieval predecessors and as the precursor of Newton's fundamental concepts. Ernst Mach (1919) is a case in point, who stated that Galileo founded the science of "Dynamics", discovered the "law of inertia", created the notion of "force", and conceived of the idea of "acceleration" for the first time. Mach applied these terms anachronistically, since they carry meanings that do not correspond to what Galileo did talk about. In doing so, Mach created the false image that Galileo "could have been so completely the *fons et origo* of modern science" (Cohen 1974, pp. 316-7).

To figure out why Galileo's theory is not merely the logical consequence of Newton's, I. B. Cohen elucidates actor's categories in their own context of use. He shows, for instance, how the concept of "inertia" carried different meanings among the big figures of the Scientific Revolution. Galileo's definition of inertial motion presupposes the existence of a "plane" upon which bodies are relying. Kepler's expression "natural inertia" refers to "inclinatio ad quietem" as an intrinsic property of matter, which implies that bodies' motion requires an acting motive force. The Cartesian notion of inertia introduces the idea of uniform and rectilinear "state" of motion according to the non-mathematical laws of nature formulated in Descartes' *Principia Philosophiæ*. And the Newtonian concept of a "force" of inertia conceives of "motion" as a vector quantity and "mass" as a quantity of matter, in accordance with the laws of motion postulated in Newton's *Principia Mathematica*. Therefore, Newton's "dynamics" is not the mere synthesis of these antecedents. To claim such a thing is making "a travesty of both science and history" (Cohen 1974, p. 327).

Sources (de)contextualisation

History can also be falsified by decontextualising historical sources. Besides anachronism, there is a further aspect of present-oriented historiography concerning how textual material as a relic of the past is detached from its own context. For Ashplant and Wilson (1988), the present-centred approach takes historical sources as not being problematical, that is, as if textual material were having a clear content that speaks in favour of historians' working hypotheses. Using textual material this way would be valid if the sources "had been created for the same purposes, as those of the historian" (p. 267). However, there exists an "inevitable discrepancy between the historian's use for a given relic and the use or uses which that relic originally sustained" (p. 269), which ultimately gives room for "historical misunderstanding" as well.

Thus, present-centred historians "build their history from fragments of the sources taken out of context" (Ashplant and Wilson 1988, p. 267). They decontextualise by wrongly conflating the context in which the source was produced and the context in which they employ that same source. As a result, the historical reconstruction gives the false impression that textual material is supporting how historians have depicted the episode, when the material "does so only by its insertion into a new context, namely that of the historian's argument" (Ashplant and Wilson 1988, p. 266).

Contrary to presentism, Anshplant and Wilson (1988) propose to see sources as a historical problem on their own. This means that sources must not be taken for granted as a non-problematical *resource*, but rather as a *topic* that is to be accounted for. Historian's task therefore "consists of explicit *investigation of the process by which the historical source was generated*" (p. 268). This presumably allows historians to avoid the two pitfalls of historiographical presentism at once. Anachronism is avoided provided the historian lavishes the attention on "the category-system underlying the relic in question" (p. 269). Likewise, the decontextualization of sources does not occur as long as "the historian ceases to assume what activities generated a given relic, and begins to ask what those activities actually were" (p. 270). Westman (2004) provides an illustrative example of how Copernicus's preface to *De Revolutionibus* has been decontextualised. He criticises three accounts of Copernicus's argument for his heliocentric arrangement in such a text. These are "Triumphalism", which supports a positivist conception of science; "Demarcationism", which is drawn upon Lakatos's methodological theory; and "Conceptualism", which squares with Kuhn's view on paradigm crisis. Instead of investigating the sourcegenerating process of Copernicus' preface in terms of actor's categories, intentions, audiences, and circumstances, each of these accounts quotes a key passage about the central proof of *De Revolutionibus* as if Copernicus were talking about a corresponding view on scientific rationality and change. On the contrary, Westman (2004) delves into the proper context of *De Revolutionibus* and hence "situates the text within the local circumstances of its production and, at the same time, regards it as a force in shaping the terms of its own interpretation" (p. 168).

More precisely, Westman (2004) inserts the preface in the context of a tradition of mathematical humanism *circa* the end of the 15th century and the first half of the 16th century that was facilitated by "Copernicus' involvement in a non-academic culture of humanist poets, painters, and sculptors at Padua" (pp. 183-4). Westman (2004) thus argues that "the preface is cast in the idiom of church patronage and reform" (p. 175), and "directed explicitly to humanist clerics in the court of Paul III who value mathematical disciplines" (p. 169). Accordingly, Copernicus defended heliocentrism as providing a reformation of astronomy by appealing to a criterion of "mathematical coherence (symmetria, armoniæ nexum)" that is based upon a "Horatian ideal of good poetry" (Westman 2004, pp. 182-3), while also employing Erasmian rhetorical strategies that "appeal to a range of ancient, pagan sources" (Westman 2004, p. 192) as authoritative. In this way, Westman's approach sheds light on how the preface to De Revolutionibus was produced in its original context, thereby making sense of Copernicus's pronouncements in terms of both the author and his contemporaries. Not surprisingly, Westman (2004) concludes ironically that "in recovering Copernicus's idiom of reform, Triumphalist, Conceptualist, and Demarcationist accounts of a Copernican revolution seem, curiously, to come from another era —as indeed they do" (p. 194).

Presentism in historiography remains an open issue, and some forms of it have been defended recently (e.g., Chang 2021b; Hull 1979; Jardine 2003; Loison 2016; Pulkkinen 2023). Despite this, mainstream historians admittedly share the view that "what we do when we do history is to try to tell it as it really was in the past. That is our institutionalized intention, and we're pretty good at recognizing when someone is trying to tell it like it was" (Shapin 2010, p. 13). On this view, both anachronism and sources decontextualisation prevent historical analysis from achieving this aim. Presentism "only projects into the past of the limits of our understanding of what 'we already know'. It thus denies its own existence, the reason for studying history in the first place" (Cohen 1974, p. 349). And regarding the philosophical approach to history, Pitt (2001) notes that "the job of explaining why the past was the past is the historian's job", accusing philosophers of adopting presentism notwithstanding: "the philosopher who looks to the past as revelatory of the present is doing bad history" (p. 379).

Sources (un)reliability

A third and final way of falsifying history is by relying historical analysis upon falsified historical material. Historical research must work with reliable sources; this is required for historical reconstructions to fulfil historical adequacy. To meet this requirement, historians have to consult primary sources. As far as textual material is concerned, they must study the original texts being available. Primary sources are important because secondary ones are much more problematical, especially *translations* of original documents. In entering the domain of history, philosophers are also expected to work with primary sources when they produce historical case studies. Otherwise, they can likely write narratives using falsified textual evidence, which turn out to be fictions.

In this regard, I. B. Cohen accuses philosophers of employing translations rather than original texts, thus writing case studies that are ultimately grounded in unreliable textual evidence. The first example he gives is the attempt to "experimentalise" Galileo in order to support an inductivist view of the scientific method. Here Galileo is presented as the founder of experimental science. Imposing this philosophical point on Galileo is objectionable as it is based upon unreliable translations. There is a falsification of textual evidence in two English versions of Galileo's texts. Crew and De Savio's 20th-century translation of *Discorsi* added the expression "by experiment" to a passage that does not appear in the original published version of *Discorsi*. This is "an attempt to make of our Galileo an empiricist, in contradiction of fact" (Cohen 1974, p. 338). Furthermore, Salusbury's 17th-century translation of the *Dialogo* omits the Italian expression "senza sperienza" — "without experiment", according to Drake's translation (Galileo 1953),— in a passage in which Salviati recognises to Simplicio that performing the experiment of the trajectory of a falling stone in a moving ship is not needed to demonstrate that this phenomenon is consistent with Earth's motion. Such falsification of textual evidence is nothing but Salusbury's "temptation to make Galileo

an empiricist, in the great tradition of the 'new science' in England" (Cohen 1974, p. 340).

I. B. Cohen's second example concerns the well-known notion of "mathematical way" as characterising Newton's method for natural philosophy. On this basis, philosophical scholarship has written extensively about Newton's conception of mathematical-experimental science. For instance, this conception allegedly states the procedure by experimental measurement and demonstration from principles in physical sciences (e.g., Strong 1951).

I. B. Cohen (1974) shows that this "important statement of a philosophical position" (p. 342) is drawn upon an unreliable source. The expression "mathematical way" (more mathematico) was introduced by Andrew Motte's English translation of an unpublished (and not found yet) Latin manuscript of Book III of Newton's Principia (Newton 1934). In his explanatory appendix to this translation, Florian Cajori uses the expression "mathematical way" in a paragraph in which Newton is presenting *De Mundi* Systemate. Interestingly, Cohen finds a discrepancy between the English translation and Newton's Latin manuscript of Book III that is available, proving that Cajori's version introduces some lines in that passage that do not appear in the original text. The English translation contains expressions such as "from the phenomena", "apply what we discover in some cases as principles", and "avoid all questions about the nature or quality of this force". In this way, the translator is using his own understanding of Newton's complete work to make sense of Newton's unpublished pronouncements in such a particular paragraph. Alas, the authenticity of this translation cannot be determined as it is based upon another version of the same Latin manuscript of Book III that is not yet available. It therefore leaves open the possibility (among other potential dangers) to attribute to Newton positions about his methodology that the actor did not subscribe to at the time the manuscript was written —ones which Newton did not even maintain whatsoever.

I. B. Cohen's point is that this philosophical view on Newton's method is only supported by the English translation rather than by the Latin manuscript that is available. As far as the reliability of the translation is concerned, he thus asserts that "we have no warrant whatever for assume —as of now— that this interesting expression of Newton's point of view, however Newtonian it may sound, is an authentic statement by the author of the *Principia*" (Cohen 1974, p. 343). No historian should put her own words in historical actors' mouths without relying upon reliable textual sources.

These two examples illustrate how using unreliable textual materials arguably leads to falsifying history. To avoid this problem, historians must always consult primary sources, being very careful not to alter those sources when translating original texts and writing monographs. This is a lesson for philosophers, too. I. B. Cohen (1974) insists that "it is usually not very difficult for a philosopher or a scientist to base his historical statements on primary rather than on secondary sources and to take cognizance of recent and current research in the history of science" (p. 312). Philosophers can trust in historians' monographs and translations, but first and foremost in their own judgment concerning primary sources. They are also encouraged to both cultivate and apply historical method.

The foregoing discussion enables the sceptic to pose the argument for the manipulation of history in a more systematic way:

The manipulation argument:

- 1. Abstraction and idealisation are aspects of historical analysis. Abstraction is objectionable only when it leaves historical context and process out. Distortion is invariably objectionable as it exaggerates the explanatory role of philosophical points and hence excludes other relevant factors. Fictionalisation is invariably objectionable whenever the historical analysis falsifies the historical record; this encompasses committing anachronism, decontextualising sources, and relying upon unreliable sources.
- 2. Those objectionable forms of abstraction and idealisation prevent historical analysis from meeting historiographical standards of historical adequacy. The philosophical approach to history arguably involves such objectionable forms.
- 3. The philosophical approach arguably violates historiographical standards. (From 1, 2.)
- 4. The historical material is manipulated in violating historiographical standards.
- 5. The philosophical approach manipulates historical material. (From 3, 4.)
- 6. Historical case studies constitute reliable data only if the historical material has not been manipulated. Unreliable data is unable to confirm philosophical theories.
- 7. The philosophical approach produces historical case studies that constitute unreliable data. (From 5, 6.)
- 8. Therefore, the philosophical approach produces unreliable historical data that is unable to confirm philosophical theories. (From 6, 7.)

This conclusion encapsulates the problem underlying the manipulation of history, thereby being sufficient for establishing premise (2) of the *circularity argument*. On this basis, the sceptic will conclude that historical evidence does not support philosophical claims, provided *historical data is unreliable as historical material has been manipulated in a self-serving way*.

1.4 Conclusion

Hitherto I have shown how the sceptic underpins the *circularity argument* by historical pluralism and the manipulation of history. Both objections are two different ways of seeing why historical case studies cannot constitute independent evidence of philosophical claims. And they are both ultimately directed at making the same point, namely, that *evidential reasoning in HPS is epistemically objectionable and must therefore be abandoned*. This sceptical verdict, however, can equally be justified by a different argument. In brief, the sceptic also argues that historical data is unsuitable to support philosophical conclusions as history of science and philosophy of science are intrinsically opposed to one another. In the next chapter, I will examine this second sceptical worry.

Chapter 2

The argument from unsuitability

Abstract

This chapter presents the second sceptical argument against evidential reasoning in HPS. This establishes that historical case studies cannot be used as evidence of philosophical claims as the history of science is unsuitable for philosophical theorising. The philosophical unsuitability of history is couched in both metaphysical and epistemic terms. According to metaphysical unsuitability, history and philosophy cannot be brought together because both disciplines adopt conflicting metaphysical commitments about science. Whilst philosophical analysis assumes scientific absolutism, historical analysis instead relies upon scientific non-absolutism. According to epistemic unsuitability, historical data are dispensable and inappropriate for establishing philosophical conclusions. Handling philosophical issues does not require looking at history, so historical data are dispensable. Using historical data leads philosophers to commit the naturalistic fallacy and hasty generalisation, so historical data are ill-suited for philosophy. In short, the argument contends that historical evidence does not support philosophical claims as history of science cannot do any philosophical work.

2.1 Introduction

The worry about the philosophical unsuitability of history is perhaps more fundamental than the pitfall of vicious circularity (Chapter 1). This worry casts doubt on what is supposed to be the principled value and utility of historical studies for the philosophy of science. According to this objection, the history of science cannot provide philosophical conclusions with evidential support simply because historical information is not appropriate for the theoretical interests of philosophy. Thus, the philosophy of science cannot learn anything useful from historical studies of science, and vice versa. The abstract argument framing this concern can be characterised in these terms:

The unsuitability argument:

- 1. For historical evidence to support philosophical claims, history of science must be suitable for philosophical theorising.
- 2. Due to the *incompatibility between history and philosophy*, history of science is not suitable for philosophical theorising.
- 3. Therefore, historical evidence does not support philosophical claims.

The cogency of this argument depends primarily upon premise (2). To justify this premise, the sceptic proposes to characterise the philosophical unsuitability of history in two ways. According to *metaphysical unsuitability*, there exists a conflict between fundamental metaphysical commitments about science that are embraced by historians of science and philosophers of science. On the other hand, *epistemic unsuitability* maintains that historical case studies are either dispensable or inappropriate for establishing philosophical conclusions on their basis. Therefore, the sceptic states that "it is not clear what philosophical work is being done" (Pitt 2001, p. 373) with historical case studies. I want to address each account of the philosophical unsuitability of history in turn.

2.2 Metaphysical unsuitability

A first factor precluding the suitability of historical case studies to support philosophical claims is that "history of science and philosophy of science are intrinsically opposed to one another" (Dresow 2020), which creates an inherent tension of integrated HPS. This tension is couched in terms of a conflict between two sets of metaphysical commitments about science that historians and philosophers have adopted. These commitments are "metaphysical" as they are primarily concerned with the nature of science and frame the methodology of history and philosophy. Here is a feasible classification of both sets of commitments:

	Modality	Quantity	Quality
Philosophy	Essentialism	Universalism	Theoreticism
History	Contingentism	Localism	Practicalism

On this view, philosophy of science is portrayed as committed to scientific absolutism and history of science instead as committed to scientific non-absolutism. That is to say, whilst philosophical analysis is essentialist, universalist, and theoreticist, historical analysis is contingentist, localist, and praticalist. It is worth emphasising two aspects of absolutism and non-absolutism. First, each metaphysical commitment is sufficient for specific positions to qualify as either absolutist or non-absolutist. If at least one of these commitments obtains, then a specific position will amount to either absolutism or non-absolutism. Each set comprises a triad of metaphysical commitments, so positions involving the three commitments will be either perfectly absolutist or perfectly non-absolutist. Second, these two sets of metaphysical commitments are in conflict with one another in three ways. Put roughly, the modality conflict between history and philosophy concerns the question of whether science has properties that are necessary and hence not subject to change. The quantity conflict refers to whether these properties are universally distributed regardless of time and place, thereby not subject to specific variation among different contexts. Finally, the quality conflict underlies the issue of whether the representational products of science (i.e., concepts, beliefs, theories, etc.) are the only relevant unit of analysis for explaining science. Let me spell out each of these three oppositions in turn.

2.2.1 Modality: Essentialism vs Contingentism

The modality conflict afflicting integrated HPS bears out the thesis that *it cannot bring* together philosophical claims about the essence of science and historical reconstructions of science as a contingent phenomenon (e.g., Burian 2001; Dear 2011; Kuhn 1977; Kuukkanen 2016; Pitt 2001; Rossi 1986). This conflict is couched in terms of temporal necessity —although it could also be conceptualised in terms of metaphysical necessity.

In metaphysical necessity, a certain property of an object is metaphysically essential *iff* the object holds such a property in all possible worlds. Conversely, a certain property of an object is metaphysically contingent *iff* the object holds such a property in some but not all possible worlds. Regarding metaphysically necessary facts, it is *impossible* for an object not to have an essential property. As for metaphysically contingent facts, it is *possible* for an object not to have a certain property that it exhibits in the *actual world*. Whereas necessary facts cannot be otherwise, contingent ones could be otherwise vis-à-vis possible worlds.

In temporal necessity, however, a certain property of an object is temporally essential *iff* the object holds such a property in all relevant periods of time (i.e., past, present, and future). Conversely, a certain property of an object is temporally contingent *iff* the object holds such a property in some but not all relevant periods of time. In respect of temporally necessary facts, it is *impossible* for an object not to have an essential property *across times*. Regarding temporally contingent facts, it is *possible* for an object not to have a certain property that *it exhibited at a certain time*. Again, necessary

facts cannot be otherwise, whereas contingent ones could be otherwise vis-à-vis some temporal parameter.

For current purposes, the issue is whether science have any temporal essence —i.e., characteristics that are not subject to temporal variation. On the one side, philosophers have defended that science holds temporally necessary properties. On the other side, historians have been inclined to conclude that all the relevant features of science that we know are contingent.

Kuukkanen presents the modality conflict as a disagreement between "historicism" and "essentialism" as the cornerstone principles of history and philosophy. According to historicism, "there are no permanent invariant properties. All objects and properties are temporal and subject to variation" (Kuukkanen 2016, p. 4). Applied to science, historicism establishes that all elements of science (i.e., theories, methodologies, epistemic standards, values, practices, etc.) change over time. By contrast, essentialism is the idea that "at least some objects have invariant and permanent, i.e. essential, properties" (Kuukkanen 2016, p. 4). Applied to science, essentialism maintains that science has elements that are constitutive, thereby not subject to historical contingency. Of course, Kuukkanen (2016, p. 6) thinks that there is a "high correlation" rather than a necessary connection between the members of "history-historicism" and "philosophy-essentialism" pairs. This qualification, however, does not undermine the claim that historicism and essentialism are central to both disciplines respectively.

Historicism and essentialism have methodological consequences. Historicism underlies the practice of historians of science, whilst essentialism frames the practice of philosophers of science. Historical scholarship features science as a changing phenomenon —thereby susceptible to historiographical scrutiny. Philosophical enterprise portrays science as having essential properties —which are justifiably cognisable by philosophical analysis. In other words, historical research employs temporal parameters to explain historical events, thus historical knowledge is not about transhistorical matters. Meanwhile, philosophical research aims "to discover and state what is true at all times and places", hence the philosopher "is no teller of stories" (Kuhn 1977, p. 5).

Finally, the modality conflict is reflected on the use of (meta-)scientific concepts. Philosophers believe that some concepts refer to transhistorical aspects, but historians believe that all concepts are subject to change. The pivotal idea is that all the central concepts that come from and are used to account for science have had many multiple uses and meanings in different historical situations. As Burian (2001) notes, scientific "standards for the adequacy of argument, evidence, experimental technique, and theory change —and should change— with time, discipline, subject matter, and setting" (p. 399). Correspondingly, Rossi (1986) emphasises the historicity of key philosophical concepts (or "philosophy *in* past science") as follows:

From historians' perspective, the so-called criteria for demarcation and "rationality" tend to be presented as not historically immutable, but instead as relative to the specific rules of a certain tradition or discipline, and to convictions, beliefs, expectations, and evaluations that are closely relate to (or that depend on) culture. For historians, the concept of "science" (like concepts of truth, evidence, or experience) is a historically variable category that has been "constructed" in any case [...]. Historians always have shown affinity for the "opacity" of historical time rather than for the "delightful velocity of logical time" [...]. They are most interested in (and this is a decisive point) temporal processes rather than "logical substitutes." (pp. 194-5)

Pitt agrees with this diagnosis. He denominates "Heraclitean flux" to the permanent diachronic variability of science, which for him is a "defining feature of science" (Pitt 2001, p. 374). He suggests that philosophers simply decontextualises the epistemological and methodological vocabulary of scientific practice by treating philosophical concepts as referring to permanent entities and properties. In using the vocabulary of their own time, philosophers approach case studies as if historical actors' vocabulary were carrying the same meaning. As an example, Pitt mentions the naïve yet serious mistake of using the contemporary concept of "scientific observation" to account for Galileo's astronomical discoveries at the beginning of the 17th century.

Besides anachronism, philosophers also seem to ignore independent evidence of the historical variability of scientific terminology. Pitt argues that scientific change crucially depends upon technological developments and other cultural factors. For instance, what amounts to "observation" in science is contingent upon inventing new instruments and practical techniques, so it is expected that even the current concept of observation will change in the future as well. From this example, Pitt (2001) draws the following conclusion about the historicity of scientific concepts:

I propose that not just observation, but all of the concepts we use to discuss science are in constant flux. Peter Galison makes that case with respect to the meaning of "experiment" in the 20th century. What constitutes an explanation, evidence, data, observation, etc., all change over time and usually in response to some technological innovation. That being the case —i.e., that the meanings of these concepts are in constant flux— it would seem impossible that we could learn anything about our present concerns from the past. And so once again, the question remains as to what we can gather from case studies. (p. 381)

It is not clear here, however, why philosophical work could not be concerned with the study of the developmental dimension of science. After all, one of the reasons that led philosophers to create the historical philosophy of science was recognising that science is a historical phenomenon, the dynamics of which is central to understand science as an epistemological endeavour. For Laudan (1990), philosophy attempted to become historical partly because philosophers were convinced "that processes of theory change and temporal progress are among the central epistemic determinants of science" (p. 49). Therefore, if the actual development of science is epistemically relevant, and if science is supposed to be a rational epistemic enterprise notwithstanding, then any epistemological analysis of science should include a historical component necessarily. If this is correct, then the problem is not so much about the epistemological character of the contingency of scientific properties as to what degree of diachronical variability is tolerated by philosophical theorising. In any case, some science scholars motivate the modality conflict by insisting that science arguably appears to be too temporally variable in light of historical research that one is compelled to conclude that science does not have any essence. Or better, they assert that if science has an essence at all, then it is change itself.

2.2.2 Quantity: Universalism vs Localism

The quantity conflict afflicting integrated HPS supports the claim that *it cannot bring* together universal and abstract philosophical claims and reconstructions of past science as a contextual, complex endeavour (e.g., Burian 2001; Caneva 2011; Cohen 1974; Kuhn 1977; Laudan 1990; Pitt 2001; Schickore 2018). This conflict is a disagreement concerning the proper quantification of our claims about science, that is, concerning the proper distribution of the scientific properties under investigation. Presumably, essential properties are universal insofar as their distribution is perfectly general —i.e., they obtain always (at any time) and/or everywhere (in all possible worlds). However, it might be the case that some properties are universally distributed yet contingent (Gellner 1982). In fact, contingent properties can also be called universal provided they are generally distributed in the actual world —i.e., they obtain in every actual circumstance at a given time.

The quantity conflict creates a tension between philosophical and historical representations of science. For one thing, history aims at particularisation and philosophy at generalisation. Historians make existential and detailed statements about properties that are taken to be features that science exhibits in specific contexts —ones that could be otherwise in different contexts. Kuhn (1977) pointed out that "the final product of most historical research is a narrative, a story, about particulars of the past" (p. 5). Meanwhile, philosophers of science make universal and abstract statements referring to properties that are taken to be constitutive of science regardless of spatial and temporal parameters. For him, "the philosopher, on the other hand, aims principally at explicit generalizations and at those with universal scope" (Kuhn 1977, p. 5). Kuhn (1977) notes that "above all, in philosophy of science, there is no role for the multitude of particulars, the idiosyncratic details, which seem to be the stuff of history" (p. 14). For instance, it is argued that philosophy is concerned with *the* scientific method, *the structure* of theories, *the principles* of scientific change, *the concept* of explanation and confirmation, *the conditions* under which something is scientific, and so forth.

Other historians share this view. Caneva takes the quantity problem as fundamental to understand the gap between historical and philosophical approaches. He maintains that "we historians deal with time-bound particulars, and our truths lie in those particulars. In contrast, philosophers seek timeless truths from which the historical particulars have been distilled off" (Caneva 2011, p. 51). Similarly, I. B. Cohen (1974) expressed that same view in the terminology of conceptual history as follows:

Philosophers are concerned very properly with the analysis of scientific thought, whereas historians deal with particular instances of the scientific thinking of individuals. The contrast is thus between science as abstraction, or system, and science as a living process of discovery and growth —it is a contrast, to use the phrase so happily introduced by Alfred North Whitehead, between the "logic of the discovered" and the "logic of discovery." (p. 347)

Cohen's point is that, regarding one and the same research problem (say, understanding scientific reasoning), the philosopher and the historian formulate their answers to it in different levels of generality and abstraction. The former typically makes a statement with a general scope that intends to be a definition of science —e.g., the claim that scientific reasoning consists in the valid deduction of theoretical hypotheses from empirical facts. Meanwhile, the latter will make a statement about a particular situation so as to solve a historical problem —e.g., how Newton actually used the terms "hypothesis", "phenomena", and "deduction" from the Book I to the General

Scholium according to different editions of *Principia*. Not surprisingly, Caneva (2011) summarises the quantity conflict as one in which:

What divides us is where we seek answers: in the particulars of the history of science, or in a sanitized abstraction from all particulars. Historians deal with real people in real situations. For us, there is no generic knower; there are only particular concrete people who would see and judge things otherwise if their experiences had been otherwise. Philosophers typically imagine scientists as decision-making algorithms. (pp. 53-4)

This contempt for the universalism of philosophy meets a corresponding contempt for the localism of historical research. Some philosophers maintain that the lack of generality precludes historical case studies from having any philosophical value. For instance, Laudan argues radically that we cannot learn anything from contextual historiography. He considers that micro-history is not only sterile in relation to philosophical results, but also in relation to other academic fields and even to society itself: "history can teach lessons, and transform cultural images, only if it explicitly addresses general issues. A lesson, to be a lesson, must have general applications" (Laudan 1990, p. 55). For Laudan, history of science can recover the status of a *teacher* only if it addresses philosophical topics again.

Burian is more moderate than Laudan. He thinks that particularism does not undermine the place of history as a source of information that would become plenty useful for philosophical agendas. Rather, he claims that contextual history does provide insights and data about science, albeit not the kind of lessons that universalist philosophy wishes to hear. In responding to Pitt's dilemma, Burian (2001) concludes that under a "bottom-up" use of case studies, philosophers "cannot and should not be expected to yield universal methodologies or epistemologies. Rather, they yield local or, better, regional standards —and fallible ones at that" (p. 400). Burian encourages philosophers to reformulate the scope of their conclusions, also calling into doubt the universalist picture of science in their discipline.

Although suggestive, Burian's promotion of the "bottom-up" approach in philosophy is problematic and leaves room for several concerns. For instance, it is not clear how a philosophical account of sciences as local and fallible can suit philosophy's desiderata of clarity and precision —ones that are inherently fulfilled by means of abstraction. In this respect, Schickore argues that these requirements cannot be accomplished if philosophical theorising attempts to fit the desiderata of contextual history. After all, it seems that "writing good history of science means paying attention to particulars: local contexts, unique features of concrete situations, and actors' terms" (Schickore 2018, p. 7). This delivers the view that "science is notoriously messy" (2018, p. 7), which is at odds with the philosophical desiderata of clarity and precision. Schickore (2018) points out that "if the analysis adheres too closely to the historical record, the stories thus produced are unlikely to be of interest to the analyst who is interested in making the philosophical point as clear as possible" (p. 8). As a result, Schickore (2011) observes that there is a tension between history and philosophy in which:

history may no longer produce relevant data for philosophy. Since the 1980s, historians have been increasingly preoccupied with in-depth descriptions of the concrete and unique, while general accounts continue to be the philosopher's goal —or so many commentators assume. This is another reason why scholars have argued that it was very difficult for philosophers to connect with historical studies. (p. 466)

Under this account, historical representation is concrete, thus giving an atomised image of science. Philosophical representation is abstract, thereby providing a simplified account of science. It is not therefore clear how to relate historical accounts that are so rich, detailed, and complex with universal and too abstract statements in philosophy that are instead very clear and precise. As philosophical representation is a "sanitized abstraction", historical representation perhaps amounts to "dirty concreteness".

2.2.3 Quality: Theoreticism vs Practicalism

Third and finally, the *quality conflict* afflicting integrated HPS leads to the belief that *it cannot bring together philosophical claims about scientific representational products and historical reconstructions that portray science as a (social) practice* (e.g., Chang 2012; Dear 1995; Koyré 1963; Laudan 1990; Laudan and Laudan 2016; Miller 2011; Schickore 2018; Shapin 1996).

The quality conflict arguably stems from the "internalism vs externalism" discussion in empirical studies of science (Shapin 1992). The central issue at stake in such debate was what are the internal elements of knowledge. Typically, the internal is associated with "epistemic" properties and the external with "non-epistemic" ones, whereby the former is constitutive whilst the latter is instead accidental. On this view, internal properties are predicated of scientific methodology and theories. By contrast, the practical dimension of science, which includes its material culture and social organisation, are considered external to and hence not constitutive of knowledge. Theoreticism takes it for granted that the "practical" and the "epistemic" correspond to different kinds of properties. Given that epistemic properties are constitutive of knowledge, the study of knowledge must be concerned with them exclusively. Thus, philosophy of science aims at understanding science as a production of knowledge, and the representational products of science (i.e., scientific concepts, ideas, beliefs, theories, etc.) are therefore the only relevant unit of analysis.

The assumption that the practical dimension is not constitutive of knowledge is commonplace in the philosophical tradition. Let me just give three examples. In analytic epistemology, Alston (2005) claimed that the object of epistemology is the normative study of the *epistemic properties of beliefs*: "the various epistemic desiderata of belief are of central importance in philosophical reflection on human cognition. The other matters that are of interest to epistemologists have that interest, in large part, because of their relations to epistemically positively valued beliefs" (p. 6). Accordingly, he added that social factors are quite different from such properties, since the "social aspects of cognition are of special interest because of the ways in which they influence the acquisition of epistemically desirable beliefs" (p. 6).

My two other examples come from philosophers of science directly. Haack made a similar diagnosis to Alston's with respect to the epistemic evaluation of scientific theories. Criticising SSK, she considered it as important to distinguish between "validity" and "credibility", thereby connecting the former to the epistemic and the latter to the social. Regarding scientific reasoning, Haack (1995) argued that:

One misunderstanding [of sociologists] is that the warrant status of a scientific claim is "just a matter of social practice." Warrant is social in the sense that talk of how warranted a scientific claim is, is elliptical for talk of how justified a scientific community is in accepting it; but how justified they are in accepting it does not depend on how justified they think they are, but on how good their evidence is. (p. 262)

In a similar vein, Laudan denies that the social dimension can be explanatory of the epistemic authority of science. He charges sociology-oriented historians with not doing history of *science* at all, since they are concerned with the social and it is "subordinated and secondary" vis-à-vis the epistemic. Laudan (1990) states:

To tell the history of science without explaining why scientists come to hold the beliefs about the world that they do is to confuse trappings with substance, effects with their causes. In sum, if it is true that science matters (both intellectually and institutionally) because of the manipulative and predictive skills which its *ideas* confer on their possessors, then a concern with science as a *cognitive process* must be primary, for until we have understood how science works cognitively, the largest question about science will remain unanswered [...]. Such institutional historians of science, by failing even to confront —let alone to answer— the question of *why scientists believe what they do*, have opted out of the cognitivist game altogether. (pp. 51-2; my emphasis)

Of course, theoreticism is not exclusive of philosophical scholarship. Intellectualist historians also share this view. Koyré is a case in point of this historiographical perspective. He came to affirm that science is an autonomous, intellectual activity of human minds which is ultimately defined by theoretical products. Accordingly, history of science is nothing but the study of the transcendent "search and struggle for truth, *itinerarium mentis in veritatem*" (Koyré 1963, p. 859). Koyré (1963) makes it clear his theoreticism in the following passage that is worth quoting *in extenso*:

It also seems to me vain to attempt to deduce the existence of Greek science from the social structure of the city state, or even of the agora. Athens does not explain Eudoxus, or Plato, any more than Syracuse explains Archimedes; or Florence, Galileo. I even believe, indeed, that the same is true also of modern times, and even of the present century despite the so much closer cooperation between pure and applied science to which I have already referred. The social structure of England in the seventeenth century cannot explain Newton, any more than the Russia of Nicholas I can throw light on the work of Lobachevsky, or the Germany of Willhelm II enables us to understand Einstein. To look for explanations along these lines is an entirely futile enterprise, as futile as trying to predict the future evolution of science or of the sciences as a function of the structure of their social contexts [...]. It seems to me —and if it is idealism, *tant pis*— that science, the science of our epoch, like that of the Greeks, is essentially *theoria*, a search for the truth, and that as a result of this fact it has, and has always had, value as an end in itself, and an inherent and autonomous —though not always regular and logical—development, such that it is only by the study of its own problems, its own history, that it can be understood by historians. (pp. 855-6)

As the practical dimensions of science encompass nothing but accidental aspects, conceptual explanation is enough explanation for intellectualist historiography. It therefore seems that conceptual historical reconstructions turn out to be appropriate to sustain philosophical theses about epistemological issues, so it does not matter too much if some or most historians are engaged with social history of science. After all, philosophers can simply ignore this type of historical studies and instead lavish attention on conceptual and general narratives.

The problem, however, is that the recent state of the historiography of science has become less theoreticist, thus making conceptual historical works increasingly scarce. Although intellectualist historians assume that material and social dimensions of scientific practice are not explanatory relevant, it nonetheless seems that current historiography takes it for granted that these dimensions are indispensable for explanation. At the beginning of the 1990s, Laudan (1990) lamented that "the study of scientific ideas has been rendered obsolete by the emergence of the study of the social life of science" (p. 51). More recently, Schickore (2018) has also insisted on the concern that "historians had moved on to other issues —social contexts, politics, institutions and philosophers of science could thus no longer expect to find ready-made historical studies that spoke directly to their interests" (p. 5).

In sum, historical work in the most recent historiography consists of contextual narratives that portray scientific practice as a complex form of culture. However, the kind of history that is philosophically relevant seems to be one that accounts for the concepts, methodologies, and epistemic standards of science. Therefore, it is currently even more difficult to see how history and philosophy can be brought together.

Hitherto I have outlined *metaphysical unsuitability* as the idea that there is a conflict between two sets of metaphysical commitments about science. Scientific absolutism is couched in terms of essentialism, universalism, and theoreticism about science, and philosophical theorising seems to conduct itself based upon these commitments. On the contrary, scientific non-absolutism involves contingentism, localism, and practicalism about science, and history-writing seems to work under these commitments. In this situation, history and philosophy are intrinsically opposed to one another, thus precluding integrated HPS. The sceptic thus underpins the argument for *metaphysical unsuitability* as follows:

The conflict argument:

- 1. *Absolutism.* Philosophy of science adopts essentialism, universalism, and theoreticism about science.
- 2. *Non-absolutism*. History of science adopts contingentism, localism, and practicalism about science.
- 3. Inherent tension. These two sets of commitments conflict with one another.

- 4. *Conditional claim.* If these two sets of commitments conflict with one another, then history of science and philosophy of science are incompatible —so they cannot be integrated.
- 5. *Conclusion*. History of science and philosophy of science are incompatible —so they cannot be integrated. (Modus ponens 3, 4.)

This conclusion is sufficient to justify premise (2) of the *circularity argument* —i.e., due to the incompatibility between history and philosophy, history of science is not suitable for philosophical theorising. On this basis, the sceptic will conclude that historical evidence does not support philosophical claims, provided *there is an inherent* tension of integrated HPS to the effect that history and philosophy rely upon conflicting metaphysical commitments about science.

It is worth noticing that *metaphysical unsuitability* does not exhaust the space of relevant reasons why the history of science becomes inappropriate for the philosophy of science. Besides the metaphysical issues I have examined above, there are epistemic reasons to reach a similar judgment. *Epistemic unsuitability* concerns the process by which philosophical claims are inferred and justified by using historical data. I now turn to examine this further line of objection.

2.3 Epistemic unsuitability

There are at least three ways of concluding that history of science is philosophically unsuitable in epistemological terms. First of all, the objection from the *(non-)privilege* of history calls into doubt the privilege that some philosophers with historical sensibility attribute to historical information. This privilege assumption relies upon the idea that history of science is an indispensable source of problems and data about the nature of science. But this assumption must be justified. The sceptic asks this question: why use the data from the history of science rather than data from other studies of science —e.g., cognitive science? Or better, why study the scientific past in the first place instead of merely focusing on the study of contemporary science?

The other two ways concern the violation of some established form of inference. The sceptic accuses evidential reasoning in HPS of involving a type of reasoning that is fallacious. In the first place, the objection from *hasty generalisation* contends that philosophers typically proceed by doing enumerative induction from a non-representative sample of historical cases. In this case, the number of cases being employed is not statistically relevant to inductively support philosophical conclusions having a high degree of generality. In the second place, the objection from *the naturalistic fallacy* argues that evidential reasoning in HPS conflates two domains of judgments that must be separated. There is the domain of normative judgments, on the one side, and the domain of descriptive judgments, on the other side. Whilst historical judgments are descriptive and explanatory in character, philosophical judgments are by principle normative and evaluative. Therefore, any attempt to support philosophical claims on the basis of historical statements will exhibit an inferential form of the naturalistic fallacy. Let me turn to characterise these three ways of bearing out *epistemic unsuitability* in more detail.

2.3.1 The (non-)privilege of history

It is common to hear that the history of science became valuable for the philosophy of science with the so-called historical turn since the 1960s mainly because historical studies exposed the inadequacy of "positivist" and "formalist" accounts of science (e.g., Kitcher 1993; Kuhn 1962; Laudan 1977). As the logical analysis of science was too disconnected from the actual science, taking a serious look at history was "seen as the means, or at least a means, of remedying the situation" (Giere 1973, p. 290). In assuming that historical research has a "privileged access" to science, historicist philosophers proposed that assessing philosophical theorising must incorporate to employ historical case studies in order to provide philosophical theses with evidential support.

The privilege assumption is either exclusivist or inclusivist. *Exclusivism* is radical in sustaining that historical discipline is the only empirical study of science that gives the relevant data to a naturalised philosophy of science. *Inclusivism* is moderate in arguing that the history of science merely provides crucial information vis-à-vis certain philosophical problems. Both perspectives adopt the idea that history is indispensable; exclusivism seems to suggest that a naturalised philosophy of science *is* historical philosophy of science, whilst inclusivism considers that historical philosophy of science is just a branch of the naturalist project.

The objection from the non-privilege of history demands that both absolute and relative indispensability must be justified. For one thing, if it is assumed that the philosophy of science is primarily a normative discipline, then it is far from obvious why *epistemological* analyses of science must take into account the non-normative dimensions of science —i.e., the psychology, the sociology, and of course the history of scientific practice. As Reichenbach maintained, philosophy is concerned with the context of justification, not with the context of discovery. To figure out, say, the nature

of confirmation or the structure of explanation, philosophers need not look at actual scientific practice to see how scientists actually evaluate their theories and explain phenomena. For instance, the epistemic status of the nomological-deductive model does not depend upon whether scientists explain phenomena in terms of deductive derivations from laws. Hull (1992) notes that "deduction is deduction, and nothing about the conduct of science can touch that" (p. 468).

Therefore, a first point against exclusivism is this: epistemic normativity, which is presumably the subject matter of philosophical analysis, is not grounded in nonepistemic facts, which are the subject matter of empirical enquiry. Therefore, empirical data are irrelevant to theorising about epistemic facts. If this is correct, then philosophy as an epistemology of science need not be concerned with considerations relating to *real science in general*.

Of course, this point is only compelling for those philosophers who think that "real science" was intentionally dismissed by logical empiricism. However, this arguably seems a caricature that postpositivist philosophers like Laudan fabricated in their own favour. Giere (1973), for instance, convincingly argued that the logical approach to science does have something to do with actual science. Unfortunately, he insisted that the connection between philosophical reflection and real science is not successfully closed by appealing to historical work. Even if philosophical analysis is concerned with real science, it has nothing to learn from scientific past. Its proper subject matter is contemporary science, on which historical analysis is completely useless.

To support this view, Giere criticised at least two main reasons for the exclusiveness of history. Firstly, he calls for the alleged importance of doing historical case studies when philosophers are addressing philosophical problems concerning current science —either science in general or a particular scientific domain. In taking it as obvious that contemporary science is worth studying, and that it has no significant historical record, Giere (1973) goes on to assert that historical discipline cannot account for the present: "surely the study of recent developments in science requires no peculiarly historical techniques —or at least not the techniques now taught by most historians of science" (p. 290). Secondly, Giere attacks the thesis that the process of scientific development demands historical explanation. Whilst Laudan was right in stressing the epistemological character of scientific change, this by no means makes scientific dynamics a historiographical problem:

This would not require the special talents of a historian of science. To argue that any consideration of temporal development brings in history would commit one to arguing that dynamics is a historical science. Moreover, to argue, as McMullin appears to, that temporal development is not subject to logical and mathematical analysis would remove dynamics from physics. Surely this is giving the historian of science more than he seeks. (Giere 1973, p. 289)

A further point against exclusivism, then, is that an engagement with real science —even with scientific dynamics— entails no historical approach to philosophy of science whatsoever. Therefore, it is far from clear what is the value of using historical case studies as empirical data for a naturalised philosophy of science. The naturalist project can remedy the problems confronting the inadequacy of logical empiricism vis-à-vis the real world of science, but this does not require any help from the history of science. If this is the case, then philosophy as an epistemology of science need not be concerned with considerations relating to *past science in particular*.

Hitherto I have characterised the reasons for rejecting exclusivism about the privilege assumption. What about inclusivism? It seems that exclusivism is very weak, but perhaps a more moderate defence of the privilege assumption is plausible and acceptable. According to inclusivism, history of science is just one source of empirical data about science among the multiple recourses that are found in science studies (broadly conceived). This is not meant to deny the existence of some philosophical issues for which the history of science is particularly illuminating. The main reason leading to inclusivism is the realisation that science is much more than its history. Schickore (2011) commented that many people had embraced the idea that "the complexity of the scientific enterprise (and the ensuing difficulty of analyzing science properly) required that we draw on a multitude of science studies, including cognitive science, sociology, and cultural studies" (p. 470).

Despite this, it is still unclear how historical data can do philosophical work. Since scientific dynamics and contemporary science can justifiably be beyond the scope of historical curiosity, what aspects of science compel philosophers to pay attention to historical case studies? Until this question is answered, history-oriented philosophers' professional commitment remains unjustified.

2.3.2 Naturalistic fallacy

Suppose one admits that philosophers are not obligated to justify the assumption of privilege. Someone might nonetheless insist that such a concession does not suffice for philosophers to be entitled to use historical case studies to make philosophical points. In particular, it can be argued that philosophers are confronted with an inferential form of the naturalistic fallacy —even though the history of science is indispensable relative to some philosophical concerns.

Let me illustrate this accusation with a toy example. Imagine a philosopher who discovers novel aspects of scientific change by carefully interpreting certain episodes from the history of physics. These aspects lead the philosopher to arrive at the conclusion that what she has called the "pre-paradigm period" of a discipline is not scientific, because the scientific community has no "paradigm" to base and guide enquiry. Suppose now that, confronted with this general description of the difference between "pre-paradigm" and "normal" science, the philosopher suggests adopting an evaluative claim according to which social science is not "mature science", since social scientists (say, sociologists of scientific knowledge) share no paradigm whatsoever. On these grounds, the philosopher comes to formulate a prescriptive statement according to which social scientists is a paradigm to make cognitive progress, thus holding a positive epistemic status.

My toy example was construed from the debate over the applicability of Kuhn's (1962) model of scientific change to social sciences. The descriptive claim comes from realising that "the entire professional community can therefore ordinarily agree about the fundamental concepts, tools, and problems of its science. Without that professional consensus, there would be no basis for the sort of puzzle-solving activity in which, as I have already urged, most physical scientists are normally engaged [...]. It is, however, by no means equally clear that a consensus of anything like similar strength and scope ordinarily characterizes the social sciences" (Kuhn 1961, pp. 221-2). Accordingly, the normative claim would insist that "the reason that fields such as psychology and sociology are not mature sciences is that the practitioners of these disciplines disagree among themselves about the nature of legitimate scientific problems and method" (Percival 1979, p. 29). So the prescriptive claim would be that Kuhn's model "can be used as an ideological tool of anti-sociology, in so far as sociology appears to be lacking in the diagnostic criteria of scientific maturity —-paradigmaticness and revolutions" (Martins 1972, p. 37). Needless to say, Kuhn (2000) himself indicated that "I am among those who have found the claims of the strong program absurd: an example of deconstruction gone bad" (p. 110). (For a recent account of this debate, see Outhwaite (2018).)

For people with a certain philosophical training, the reasoning depicted by this example is suspect. Their reaction will be that the philosopher in question has committed a certain version of the naturalistic fallacy, whereby a normative judgment about how science should be conducted has been *inferred from* a descriptive judgment about how science is done in different stages of its historical development.

Regarding the use of historical case studies as evidence, the objection from the naturalistic fallacy contends primarily that philosophical claims about science are normative judgments, whilst historical statements are descriptive ones. As Caneva (2011) puts it, "the product of historical work is a narrative of events, either descriptive or explanatory; the product of philosophical analysis is a schema; either descriptive or normative" (p. 52). In consequence, philosophers must face a dilemma: either they carry out the reasoning of deriving philosophical lessons from historical reconstructions, thus falling prey to the naturalistic fallacy; or they stop reasoning this way, thereby recognising that historical data is unsuitable for sustaining philosophical claims. Put otherwise, the problem is that claims about *what is correct* cannot be grounded in and justified by claims about *what is accepted as correct* unless facing an inferential form of the naturalistic fallacy.

Some people take the vision that norms cannot be epistemically based upon facts as vindicating an aprioristic philosophy of science. For instance, Pitt argues that it is correct to say that the history of science can do nothing with the normative goal of the philosophical enterprise —that this goal can be better accomplished by a logical approach to account for epistemic normativity. Pitt (2001) asks:

Have we not been able to see clearly through the lens of logic to important structural characteristics of, for example, explanation and confirmation? If the claim is that what we have come up with doesn't match what scientists actually do, then it is not clear that that is a valid criticism since we have a normative, not merely a descriptive role to play. Determining the logic of key concepts and working that out is a perfectly legitimate activity. What is it that history is supposed to supply? (p. 375)

Other philosophers —who have rejected aprioristic philosophy for other reasons— have attempted to make compatible philosophical evaluations with historical explanations. Lakatos' meta-philosophy is a case in point (§3.2). His pivotal idea is that philosophical theories of rationality can be evaluated in terms of the rational reconstructions of historical episodes. As the history of science is interpreted in a normative way, historical case studies are able to justify claims of methodologies of science. Philosophers can therefore learn lessons about good science from historical data because case studies also involve normative judgments. Likewise, Pinnick and Gale (2000) have put forward more recently that philosophy of science should produce its own case studies, in such a way that they fit the normative goals of philosophy.
This would be an interesting manoeuvre to avoid the naturalistic fallacy. However, the idea of constructing a "philosophical" history of science that is a counterpart of a "naturalistic" history of science is widely suspect for mainstream historians —let alone for some historicist philosophers of science as well (e.g., Kuhn 1970a; Laudan 1990). Alas, this approach that encourages philosophers to write a normative history of science has been "largely programmatic" in character (Schickore 2018, p. 8). Until this method is systematically articulated, the issue of deriving norms from facts without being fallacious or creating a tension between philosophers of science" (Giere 1973, p. 290).

Some historians and philosophers consider this problem to be very difficult to solve, which could even partially explain the alleged failure of integrated HPS. Shapin and Schaffer (2011), for instance, report that "the marriage between naturalistically and empirically inclined history and normatively disposed philosophy of science was not going well" (p. xxiii n. 25). The philosopher Dresow (2020) agrees with this diagnosis:

Philosophy of science, at least in the Anglophone world, is taken to be a normative discipline concerned above all with understanding how science succeeds in producing justified knowledge about the natural world. History of science is a descriptive enterprise concerned to understand scientific activities in context: in relation to the range of factors that influence research agendas and affect the content and reception of scientific ideas. This difference in methodological orientation generates a tension between the two disciplines. (p. 58)

In a nutshell, the lesson according to this line of objection is that normative and evaluative philosophical claims about science cannot be justified by data from descriptive and explanatory historical reconstructions. History of science cannot have any normative function for an epistemology of science.

2.3.3 Hasty generalisation

A third and final line of objection lavishes attention on another epistemic fault that evidential reasoning in HPS involves. Pitt (2001) formulates it as follows: "if one starts with a case study, it is not clear where to go from there —for it is unreasonable to generalize from one case or even two or three" (p. 373). This is the objection from hasty generalisation.

As mentioned previously (§ 2.2.2), philosophers pretend to draw conclusions with the character of generality (if not of universality) about scientific knowledge and practice. This is because the goal (at least that of general philosophy of science) is to produce a global theory of science, which answers conceptual, epistemological, and methodological problems that are general issues. On the contrary, historical case studies are focused on contextual problems. Historical analysis aims to interpret and explain historical events in terms of historical actors themselves, according to the local and specific factors relative to the historical situation in question.

This difference in disciplinary aims aside, history-oriented philosophers typically make inductive generalisations from case studies. In doing so, they "proceed to grand conclusions by induction from absurdly small samples" (Burian 2001, p. 388). Interpreted as a form of radical scepticism, the objection is that evidential reasoning in HPS cannot be rationally defended as a form of inductive reasoning. Schickore (2011) points out that "even if generalizations can be supported by several instances rather than one isolated one, the critic of the inductive method will not be impressed" (p. 469) by enumerative inductions from historical cases. On this view, the problem arises for any kind of induction, including that of historical philosophy of science in particular.

As an instance of Hume's problem, however, the objection from hasty generalisation is very weak. Since this problem affects not only philosophers but also scientists and laypeople in general, advocates of evidential reasoning in HPS will not be impressed by this objection. They may adopt a "parrying" position about it, arguing that the problem is not a challenge for which they should be especially responsible; this conundrum is simply found in empirical reasoning everywhere. Further, they may go on to reply that it is entirely possible to make intellectual life by disregarding such theoretical sceptical worries about induction —as it happens with other forms of radical scepticism such as Agrippa's trilemma, the other-minds problem, or cartesian scepticism.

There is another interpretation of the problem that the objection posits. This shows the specificity of the problem vis-à-vis evidential reasoning in HPS. Those who think that inductive reasoning is rationally warranted would nonetheless object that there are reasons for differentiating between the inductive base evidential reasoning in HPS involves and the samples that are typically used in other domains of empirical enquiry. These reasons show that only the sampling of historical cases is seriously problematical.

Historical case studies constitute no apposite inductive base by virtue of the type of empirical data they provide. The complexity of history makes historical case studies very diverse and even incompatible with one another. This heterogeneity precludes the selection of a sound enough sample from which to establish inductive generalisations. It is hard to group a reasonable number of historical cases as if they were sharing a single relevant property that the philosophical conclusions are intended to refer to. In this respect, Burian (2001) argues:

If we start a case study without any philosophical issue in mind, it is unclear what sort of moral we can or should draw. The risk of hasty generalization is enormous. A series of case studies —even a few hundred of them for that matter— does not provide a sufficient basis for generalizing about science, which is as richly diverse as any human enterprise. (p. 386)

On similar grounds, Chang rejects to conceive of the relation between historical studies and philosophical claims in terms of the general and the particular. For him, enumerative inductive reasoning leads to facing "the dilemma between making unwarranted generalizations from historical cases and making entirely 'local' histories with no bearing on an overall understanding of the scientific process" (Chang 2011, p. 110). Given the diversity of historical cases, philosophers have no good reason to think that the general claim they have inferred from a given set of cases can justifiably be projected to further cases. That is to say, they are not entitled to think that such a claim does have a large scope attributed to it. But the opposite situation is not less bad. If philosophers make no inductive projection whatsoever, then they cannot escape from an atomistic picture of science. As this picture is not informative about general aspects, it turns out to be philosophically infertile.

Unfortunately, the problem with historical cases does not end up here. Apart from being heterogeneous and rich in detail, historical cases feature some aspects that are often in conflict with one another. As a result, disagreements about what is an adequate sample of cases are likely to emerge. Vicedo (1992) puts this methodological concern as follows:

[I]f we can make claims only about specific episodes, it seems difficult, if not impossible, to reach closure or consensus about more global issues in philosophy. By focusing on case studies we also run the risk of selecting only those episodes which support our views. Some philosophers focus on scientific change, others scholars on scientific controversies, others still on particular methodological issues. They often select specific episodes to support generalized views about science. Some of the philosophical interpretations that certain sociologists of science have defended by analyzing a few cases of scientific controversies are clear examples of this methodologically suspect strategy. (p. 492)

If this interpretation of the objection from hasty generalisation succeeds, there is no principled way of going beyond case studies in order to reach philosophical understanding. First, when philosophers claim something like "all science is X", being X an aspect found in only one case study, this general conclusion is not epistemically warranted. Second, if philosophers formulate that same conclusion from a considerable number of cases in which X is arguably featured, they have to show how the selected inductive base does not conflict with the set of historical cases that were excluded. And finally, if philosophers stop doing enumerative induction, they face the unpleasant consequence that no philosophical lessons can be drawn from the history of science. In the final analysis, the problem is not so much with (enumerative) induction as with historical cases *per se*. Put things so, here is the argument that bears out *epistemic unsuitability*:

The non-aptness-of-data argument:

- 1. Unless history is proven to give privileged information about science, historical data are dispensable for addressing philosophical problems.
- 2. History has not been proven to give privileged information about science.
- 3. Historical data are dispensable for addressing philosophical problems. (Modus ponens 1, 2.)
- 4. If using historical case studies involves hasty generalisation and the naturalistic fallacy, then historical data are ill-suited to establish general, normative philosophical conclusions.
- 5. Using historical case studies involves hasty generalisation and the naturalistic fallacy.
- 6. Historical data are ill-suited to establish general, normative philosophical conclusions. (Modus ponens 1, 2.)
- 7. Therefore, historical data are dispensable for addressing philosophical problems and ill-suited to establish general, normative philosophical conclusions. (Conjunction 3, 6.)

By the same token, this conclusion serves to establish premise (2) of the *unsuitability* argument. This argument will therefore conclude that historical evidence does not

support philosophical claims in the sense that historical data is epistemically ill-suited to make philosophical points if not dispensable to address philosophical questions.

2.4 Conclusion

Hitherto I have outlined the central reasons supporting premise (2) of the unsuitability argument. These reasons fall into two categories. Metaphysical unsuitability conveys the idea that historians and philosophers embrace metaphysical commitments about science that are opposed to one another. Such commitments frame the methodology of both disciplines: philosophy is essentialist, universalist, and theoreticis; history is contingentist, localist, and practicalist. This conflict between scientific absolutism and non-absolutism creates a tension between the two disciplines precluding disciplinary integration.

Meanwhile, *epistemic unsuitability* refers to the idea that the procedure from historical case studies to general philosophical claims is fraudulent reasoning. Even if it is reasonable to accept that historical data is relatively indispensable for some philosophical purposes, philosophers who move from specific historical case studies to draw general philosophical conclusions about science fall prey to an inferential form of the naturalistic fallacy and commit hasty generalisation. As a result, they wrongly support unwarranted (normative) conclusions from historical cases.

In the following chapters, I will draw upon three works in HPS as my case studies to close the door to the sceptical arguments as they were discussed in Chapters 1 and 2. I move on to examine the extent to which these works in HPS can avoid vicious circularity and the philosophical unsuitability of history. In the next two chapters, I examine both Lakatos' historiographical research programmes (Chapter 3) and Stanford's strategy of historical ostension (Chapter 4) vis-à-vis the *circularity argument*, leaving my third case study of Chang's integrated HPS (Chapter 5) to handle *metaphysical unsuitability* as characterised in this second chapter. Let me consider Lakatos' work in the first place.

Chapter 3

Historiographical Research Programmes

Abstract

This chapter offers a typology of independent historical evidence as a first strategy to respond to the circularity argument. I sketch this typology by elaborating on Lakatos' conception of HPS as a case study. I argue that Lakatos' proposal suffers from vicious circularity insofar as it faces the problems of historical pluralism and the manipulation of history. However, I show how my typology of independent historical evidence breaks the vicious circularity provided the historical evidence is independent of the philosophical theories being supported. This typology fits Lakatos' actual practice despite his central methodological pronouncements. I conclude that my typology of independent historical evidence can prevent the accusation of vicious circularity from arising against Lakatos' commitment to HPS.

3.1 Introduction

My purpose in this chapter is to offer a typology of independent historical evidence to resolve the problem of vicious circularity. This typology establishes different forms in which historical case studies constitute independent evidence of philosophical claims. In elaborating on Lakatos' conception of HPS¹ as a case study, I shall establish two central points. First, I examine the extent to which the *circularity argument* undermines Lakatos' conception of HPS, arguing that Lakatos' proposal suffers from vicious circularity as it faces both historical pluralism and manipulation of history. Second, I show how the use of historical evidence to support philosophical theories can be defended with my typology of independent historical evidence. This typology, which highlights some aspects of Lakatos' philosophical practice, breaks the vicious circularity provided the historical evidence is independent of the philosophical theories being supported.

¹I will use the following acronyms for citing Lakatos' works: FMSRP (Lakatos 1978a); HSRR (Lakatos 1976); L&Z (Lakatos and Zahar 1973); P-HSRR (Lakatos 1978b); REPM? (Lakatos 1978c); RTC (Lakatos 1970).

My argumentative strategy requires making a constructive criticism of Lakatos' conception of HPS. To do this, I lavish special attention on the historiography of the Copernican Revolution. Specifically, I draw upon Lakatos and Zahar's co-authored case study of this historical episode, which "might in particular serve as an important test case between some contemporary philosophies of science" (L&Z, p. 335).

The chapter is structured as follows. Section 3.2 characterises Lakatos' metaphilosophy of science as an instance of the so-called confrontation model of HPS. Then I reintroduce the *circularity argument* and explain how it bears out the diagnosis that Lakatos' meta-philosophy involves inconsistent tenets. Sections 3.3 and 3.4 show how the problems of manipulating history (\$1.3) and historical pluralism (\$1.2) serve to justify the relevant premise of that argument. This premise states that historical case studies cannot constitute independent evidence for philosophical theories of scientific rationality. Finally, section 3.5 develops a typology of independent historical evidence to defeat the *circularity argument*. This typology squares with what Lakatos does despite what he says in his writings, which I flesh out with some examples from Copernican historiography.

3.2 Lakatos' meta-philosophy

Lakatos conceived the history-philosophy relation in terms of the confrontation model of HPS. Put roughly, this model affirms that philosophical claims about science can be tested against historical case studies. The philosophy of science produces theories about science that can be evaluated considering empirical evidence from the history of science. For Schickore (2011), "the confrontation model portrays philosophical analysis as akin to the practice of natural science, as a practice of constructing a general theory, producing data, and confronting the theory with the data" (p. 471). Arguably, Lakatos' meta-philosophy is an instance of this model. Lakatos maintained that the integration between historical and philosophical approaches to science consists in an *evidential relationship*: history provides philosophy with empirical evidence, and philosophy provides history with a normative historiographical framework. More specifically, Lakatos' meta-philosophy relies on these three tenets:

1. EMPIRICAL ADEQUACY: The evaluation of methodologies of science includes the empirical (in)adequacy of those methodologies to episodes from the history of science (HSRR, pp. 1, 21).

- RATIONAL RECONSTRUCTION: Historical episodes are reconstructed on the basis of methodologies of science, and the resulting reconstructions are supplemented (in footnotes) by an "external history of science" (HSRR, pp. 1, 18).
- 3. **EPISTEMIC EVALUATION:** Methodologies of science are evaluated on the rational reconstructions they generate. The best methodology can save the largest number of historical episodes as rational (HSRR, pp. 1, 31).

EMPIRICAL ADEQUACY is a criterion that philosophers can appeal to in evaluating methodologies of science. These methodologies are theories of scientific rationality and change, which in turn provide a demarcation criterion. Although they are critically evaluated by appealing to logical and epistemological standards, methodologies of science are also subject to historiographical criticism. First, methodologies aim to give a rational explanation of the success of actual science, and their relative explanatory success is ultimately determined by how many historical episodes can be explained as rational. Second, two competing methodologies of science might be equally ranked in terms of logical and epistemological criticisms. In such cases, the historical (in)adequacy of methodologies to the history of science helps to adjudicate between them.

RATIONAL RECONSTRUCTION and EPISTEMIC EVALUATION define the role methodologies of science play both in *producing* historical evidence and in *testing* competing methodologies against such evidence. According to RATIONAL RECON-STRUCTION, a methodology of science has a *first-order function*, whereby it is employed as a historiographical theory to produce rational reconstructions of scientific episodes. Here, the first-order methodology plays at least two roles. One is theory guidance, since the historian resorts to the methodology as a criterion for selecting the relevant, internal aspects of historical events. Lakatos indicated that "in constructing internal history the historian will be highly selective: he will omit everything that is irrational in the light of his rationality theory" (HSRR, p. 18). The second role is theory-ladenness, since the methodology provides the historian with theoretical categories and assumptions for a normative interpretation of historical episodes. For instance, the falsificationist historian will look at history for "falsifiable theories and for great negative crucial experiments" like the relativity theory and Eddington's experiment, thus explaining episodes of theory choice in terms of "conjectures and refutations" (HSRR, p. 8). So construed, the first-order methodology functions as "a code of scientific honesty" (HSRR, p. 2), in which past scientists' epistemic judgments about scientific theories are subject to philosophical appraisal. Past scientific research is subject to epistemic evaluation in terms of some methodology of science.

EPISTEMIC EVALUATION tells us that a methodology of science has a secondorder function, whereby it is employed as a "meta-criterion" to evaluate methodologies of science. Second-order methodologies define what type of epistemic support historical reconstructions confer upon theories of rationality. Notice that rational reconstructions by themselves indicate nothing about how methodologies of science must be confronted with them nor what is the result of such an empirical testing. Second-order methodologies stipulate the evidential status of historical reconstructions and the nature of the evidential relation between reconstructions and methodologies. For instance, the falsificationist historian can rationally reconstruct a specific set of historical cases, but this historical evidence tells us nothing about what epistemic support is being conferred. Depending on which methodology of science the philosopher adopts, falsificationist historical cases could be "confirming instances", "crucial experiments", or "dramatic signs of empirical progress". Correspondingly, second-order methodologies stipulate whether empirical testing yields confirmation, falsification, corroborated novel predictions, and so forth. On these grounds, the second-order methodology functions as a "(normative) historiographical research programme" (HSRR, p. 2), in which philosophers' epis*temic judgments about scientific rationality* are subject to meta-philosophical appraisal. Philosophical analysis is subject to epistemic evaluation in terms of some methodology of science.

These three tenets intend to give a principled way of evaluating theories of rationality according to their success as first-order and second-order methodologies. On the first level, rational reconstructions are critically compared to one another. The historian may ask: does my reconstruction explain the historical episode as rational whereas rival reconstructions do not? On the second level, competing methodologies of science as historiographical theories are compared to one another. The philosopher may ask: is my methodology of science superior to rival methodologies in light of the corresponding rational reconstructions? L&Z is an example of the first level of evaluation, whilst HSRR contains a good example of the second one.

In L&Z, Lakatos argues that his methodology of scientific research programmes (henceforth, MSRP) is the only theory of rationality that can account for the Copernican episode as a case of rational scientific change: "as it happens, the Copernican Revolution can be explained as rational on the basis of the methodology of scientific research programs" (L&Z, p. 368). Presumably, the success of MSRP as a first-order methodology concerning this case study speaks in favour of MSRP as a theory of scientific rationality. It is far from obvious which second-order methodology is behind L&Z. In HSRR, however, Lakatos explicitly appeals to two second-order methodologies to evaluate historiographical theories. Using falsificationism, he concludes that historical evidence falsifies falsificationism itself and any other methodology, including his own (HSRR, pp. 27-9). Using MSRP, Lakatos points out that historical evidence permits historiographical theories to be ranked in terms of degenerating and progressive problemshifts. He concludes MSRP is more progressive than any other historiographical theory, even though Popper's historiographical theory is more progressive than inductivism and conventionalism:

Since we have abandoned naive falsificationism in *method*, why should we stick to it in *meta-method*? We can easily replace it with [...] a methodology of historiographical research programmes. [...] We then reject a rationality theory only for a better one, for one which, in this "quasi-empirical" sense, represents a *progressive shift* in the sequence of research programmes of rational reconstructions. [...] Popper's methodology enabled the historian to interpret more of the actual basic value judgments in the history of science as rational: in this normative historiographical sense Popper's theory constituted progress. (HSRR, pp. 30-1)

The epistemic evaluation would either be more restrictive or permissive depending upon the second-order methodology under use. Falsificationism is restrictive because it requires the rejection of any theory of rationality that is falsified, but MSRP permits the acceptance of at least one methodology according to degrees of theoretical and empirical progress. Presumably, this comparative success of MSRP as a second-order methodology also favours MSRP as a theory of scientific rationality.

Thus, Lakatos' meta-philosophy provides a principled way of integrating historical and philosophical approaches. History and philosophy hold a theory-data relationship wherein the two sides are mutually dependent. The history of science needs theories of rationality as normative historiographical frameworks to produce historical case studies, and the philosophy of science needs the resulting historical reconstructions to empirically evaluate theories of rationality when they cannot be conclusively ranked according to logical and epistemological standards. History and philosophy of science, then, constitute a single meta-scientific enterprise with normative and empirical dimensions.

Arguably, the *circularity argument* would challenge Lakatos' meta-philosophy. Recall this argument states that "philosophical claims cannot really be tested against the historical record because the historical record is not independent from the theory" (Schickore 2011, p. 467). As applied to Lakatos' meta-philosophy, the argument can be characterised as follows:

- 1. Historical case studies test theories of scientific rationality only if historical case studies are independent of the theories of scientific rationality being tested.
- 2. Historical case studies are not independent of the theories of scientific rationality being tested because of the rational reconstruction of historical episodes.
- 3. Therefore, historical case studies do not test theories of scientific rationality. (Modus tollens 1, 2.)

This conclusion implies that the two levels of epistemic evaluation of theories of rationality cannot be accomplished. The first premise propounds that the relation of evidential support cannot be objectionably circular to the extent that historical case studies bearing upon the theory of rationality must be independent evidence. Otherwise, evidential reasoning ultimately becomes a self-serving use of historical evidence to favour the methodology of science under test. Alas, in attempting to decide between competing theories of rationality, appealing to evidential reasoning will ultimately beg the question. In cases of self-serving reasoning, the evidence historical case studies provide to at least one single philosophical theory is not objective. The worry is that "it could be argued that the historical data was manipulated to fit the [philosophical] point" (Pitt 2001, p. 373). In cases of question-begging reasoning, a critical comparison between rival philosophical theories based upon historical case studies is dialectically ineffective. It is not clear here "how to relate the history to the philosophical point without begging the question" (Pitt 2001, p. 374).

Regarding the second premise, it posits that if historical cases have been reconstructed in terms of some methodology of science, then they do not constitute independent evidence. This is because RATIONAL RECONSTRUCTION involves some aspects that crucially make evidential reasoning both self-serving and question-begging. These aspects concern how historical reconstructions depend upon methodologies of science. Lakatos' procedure would involve self-serving reasoning insofar as the first-order methodology is the relevant criterion for producing internal history; this permits the historian to arbitrarily manipulate historical material in terms of the methodology of science. Further, Lakatos' procedure would become dialectically ineffective reasoning as long as the methodology of science is the theory used to reconstruct historical episodes and the theory being tested; this makes historical evidence unable to decide between rival methodologies of science.

By fleshing out these two aspects vis-à-vis RATIONAL RECONSTRUCTION, I now turn to argue that they jeopardise EMPIRICAL ADEQUACY and EPISTEMIC EVALUATION. Therefore, Lakatos' meta-philosophy arguably relies upon inconsistent tenets. Let me begin by examining the issue of manipulating history.

3.3 Manipulation and rational reconstructions

A way of seeing why historical case studies do not constitute independent evidence is to find that historical material has been manipulated. This might be understood as the deliberate falsification of historical material to fit the favoured philosophical theory —thereby fabricating a fictional history in terms of the methodology of science. Lakatos was well aware of this pitfall. Regarding falsificationism as a first-order methodology, he pointed out that "Popper cooks up his history to fit his naïve falsificationism" (L&Z, p. 359), "distorts history to fit his rationality theory" (HSRR, p. 25), and "succumbs to the temptation to simplify the situation into which one to which his methodology is applicable" (HSRR, p. 27). Indeed, Lakatos also accused other first-order methodologies of manipulating history, indicating that "all these rational reconstructions of history force history of science into the Procrustean bed of their hypocritical morality, thus creating fancy histories" (HSRR, p. 28).

Lakatos also manipulated historical material, however. This pitfall arguably results from certain assumptions underwriting RATIONAL RECONSTRUCTION. Lakatos sets forth at least four claims concerning this: (i) historical data are theory-laden with methodologies of science; (ii) methodologies of science account for scientific rationality, in such a way that internal history is primary; (iii) the content of the actual history of science exceeds the content of its rational reconstruction; and (iv) given (i)-(iii), a discrepancy between actual history and rational reconstructions is always to be found, in which case external history explains the residual irrationality that internal history cannot (HSRR, p. 17). Unfortunately, this characterisation leaves room for the problem of manipulating history due to the creation of fictional historical cases and the dismissal of history as a source of epistemic normativity.

3.3.1 Fictional history

Lakatos proposed that rational reconstructions can include historically false statements. First-order methodologies are the relevant criterion for reconstruction: "History of *science* is a history of events which are selected and interpreted in a normative way" (HSRR, p. 19). Further, the historian can make up historical facts deliberatively: "Internal history is not just a selection of methodologically interpreted facts: it may be, on occasions, their *radically improved version*" (HSRR, p. 18). As an example, Lakatos mentions that "the historian, describing with hindsight the Borhian programme, should include electron spin in it, since electron spin fits naturally in the original outline of the programme" (HSRR, p. 18). The statement "Bohr's programme posited electron spin since 1913" is historically false. This amounts to manipulating the historical record in terms of MSRP.

Lakatos believed that the historian will be led to write fictional history when her proposed methodology of science is in discord with the actual history of science. Otherwise, her conclusion from such discord should be that her proposed methodology cannot account for the historical episode as rational. In a critical tone against Popper, Lakatos expressed this idea as follows: "If a historian's methodology provides a poor rational reconstruction, he may either misread history in such a way that it coincides with his rational reconstruction, or he will find that the history of science is highly irrational" (HSRR, p. 25). But if so, then the question arises as to whether a Popperian historian is not in a position to judge that Lakatos' reconstruction has manipulated the historical record.

Perhaps fictional history is justified if the attention is focused on the epistemic evaluation of first-order methodologies. Lakatos indicated that this has the function "to criticise both one's rational reconstruction for lack of historicity and the actual history for lack of rationality" (FMSRP, p. 53). Here, footnotes of external history report the "lack of historicity" whereas historically false statements indicate the "lack of rationality" (HSRR, p. 18). In this way, first-order methodologies will be good as long as the rational reconstructions they produce contain fewer false statements and hence fewer socio-psychological premises. Put differently, the best methodology will be one capable "to interpret more of the actual basic value judgments in the history of science as rational" (HSRR, p. 31). If this justifies fictional history, then Lakatos faces a dilemma: the manipulation of history underlying RATIONAL RECONSTRUCTION is either objectionable or not. If manipulation is not permitted, then Lakatos' reconstruction of the Bohrian historical episode also can be critically dismissed as an instance of self-serving reasoning. But if manipulation is permitted, then Lakatos' accusation against Popper (and against other methodologists) loses critical force. The problem with the methodologies competing against MSRP is not that they produce fictional history per se, but rather that they cannot maximise the rational explanation of relevant historical episodes.

3.3.2 Dismissing history as an evidential source

Lakatos' conception of internal history provides no clear answer to the question of whether there is a principled criterion for the maximisation of rational explanations of cases —in such a way that competing rational reconstructions can be critically compared. Lakatos took for granted a contentious Popperian distinction between the third world of epistemic facts and the second world of psychology and sociology. In historical explanation, the methodology of science is a set of "third-world premises" used for the rational reconstruction of science. This confers priority to internal history to the effect that the methodology of science fully accounts for scientific rationality, stipulates the domain of external history, and can even overrule scientists' epistemic judgments (P-HSRR, pp. 190-1; RTC, pp. 179-80).

Suppose now the historian found an alleged discrepancy between the rational reconstruction and actual history. When and under which conditions should the historian conclude that the methodology is "ahistorical" rather than judging that the historical case is "irrational" in terms of the methodology? If methodologies include "third-world premises", and if the "objective appraisal" of actual science is entirely independent of "second-world premises" (P-HSRR, p. 190), then the philosophical theory always will be imposed on actual science. A falsificationist historian, for instance, is thus permitted to conclude that falsificationism overrules past scientists' actual epistemic judgment about the Copernican system. Falsificationism dictates that "the Copernican Revolution took place (at best) in 1838" (L&Z, p. 360) with the observation of stellar parallax as a "crucial experiment". This worry applies to any historically false statement in rational reconstructions, but also to any methodologically historical interpretation more generally. The fundamental problem is that one cannot know to what extent a "third-world premise" coincides with actual history —i.e., when it "explains more of the actual history of science" (P-HSRR, p. 192). The adequacy of methodologies of science to actual history cannot be determined in terms of the maximisation of rational explanation.

The maximisation of rational explanation arguably demands reconsidering the assumption of the "priority" of internal history. If methodologies of science must be historically adequate, then the actual history of science ought to be a source of epistemic normativity. To determine whether historical episodes have been rational or not, one can appeal to philosophical analysis (i.e., using the epistemic judgments from methodologies of science) and to historiographical analysis (i.e., examining past scientists' epistemic judgments). To do this, one should presuppose that at least some actual past epistemic judgments reflect the epistemic facts.

Lakatos seems to suggest, however, that only philosophical analysis can access the third world: "the rational aspect of scientific growth is fully accounted for by one's logic of scientific discovery" (HSRR, p. 17), thus providing the ultimate criterion for selection and interpretation: "all historians of science who distinguish between progress and degeneration, science and pseudoscience, are bound to use a 'third-world' premise of appraisal in explaining scientific change" (P-HSRR, p. 191). The problem with his view is that history cannot be a source of epistemic normativity with evidential value for theories of rationality as long as actual past scientists' epistemic judgments are unable to reflect the epistemic facts. Accordingly, even the historical thought experiments that Lakatos appealed to (e.g., L&Z, pp. 379-80; HSRR, pp. 16-7) can substitute historical reconstructions, thereby playing the role that EMPIRICAL ADEQUACY attributes to historical cases. Of course, when thought experiments are not to be used, fictional history could play such a role.

In the final analysis, RATIONAL RECONSTRUCTION involves some aspects that leave room for the problem of manipulating history. These aspects concern both fabricating fictional history and reducing scientific rationality to the third world. In the former case, it might happen that the rational reconstruction including historically false statements will never be in discord with the methodology of science. In the latter case, it is unclear how the maximisation of a rational explanation of the actual history of science can work out. As long as manipulating history in terms of particular methodologies prevents historical case studies from constituting independent evidence, the epistemic evaluation of theories of rationality as first-order methodologies will be thwarted.

3.4 Pluralism and critical comparison of methodologies

Another reason why historical case studies do not constitute independent evidence is that such studies are reconstructed from competing methodologies of science. As a result, rational reconstructions cannot decide between these methodologies. Historical pluralism therefore takes the form of a sceptical paradox against Lakatos' meta-philosophy. First, methodologies of science are constitutive of historical reconstructions of science. Second, a plurality of rival methodologies produces a plurality of competing historical reconstructions of the same historical case. Therefore, historical reconstructions are unable to compare theories of rationality when the same historical case is reconstructed from competing methodologies.

The argument draws an unacceptable conclusion from acceptable premises. This establishes primarily that theory-ladenness is a necessary element of historical representation. Lakatos maintained that "history without some theoretical 'bias' is impossible" (HSRR, p. 19), adding that methodologies of science are the constitutive aspect of historical reconstructions of science: "Historiographical 'factual' propositions are also theory-laden: the theories involved are methodological theories" (HSRR, n. 60). Second, the objection goes on to argue that there can be a plurality of historical reconstructions in conflict because they are theory-laden with rival philosophical theories: "since historical facts are theory-laden, disagreement is likely to emerge between historical accounts that reconstruct the past on the basis of different theoretical assumptions" (Kinzel 2016, p. 128). Lakatos recognised the existence of competing methodologies of science with corresponding rival rational reconstructions of historical cases (HSRR, p. 2; P-HSRR, pp. 191-2). Third and finally, the objection concludes that "in situations in which one and the same case is reconstructed from competing philosophical viewpoints, the historical evidence cannot settle the philosophical conflict in question" (Kinzel 2015, p. 55). Lakatos would have disagreed. He considered that historical case studies are able to decide between rival methodologies of science.

As it is inherent to RATIONAL RECONSTRUCTION, the theory-dependence of historical reconstructions upon methodologies of science features two characteristics. First, the methodology used to produce the historical reconstruction coincides with the theory of rationality being tested. Second, the methodology used as a metacriterion for connecting the historical reconstruction with the theory of rationality being tested coincides with that same theory. This theory-dependence makes the relation of evidential support involving two varieties of circularity that lead to dialectically ineffective reasoning.

3.4.1 Narrow circularity

The problem here is this: when two rival theories of rationality are used as first-order methodologies, each one produces a rational reconstruction of the same historical case which is not independent of the theory of rationality being tested. In other words, "the history we write will presuppose the very philosophy which the written history will allegedly test" (Laudan 1977, p. 157). Therefore, a critical comparison of theories of scientific rationality by appealing to historical evidence is precluded, since any attempt to do it will beg the question.

L&Z exhibits narrow circularity. This case study provides the following rendering of Copernicus' work:

[A] new solution was proposed to the question why Copernicus's programme (objectively) superseded Ptolemy's. It was superior to it on all three standard criteria for appraising research programmes: on the criteria of theoretical, empirical and heuristic progress. It predicted a wider range of phenomena, it was corroborated by novel facts and, in spite of the degenerative elements of *De Revolutionibus* it had more heuristic unity than the *Almagest.* (P-HSRR, p. 189)

More specifically, Lakatos considered that the Copernican research programme was theoretically progressive as formulated in the *Commentariolus* and at least empirically progressive in 1616 due to Galileo's telescopic observations of Venus' phases (L&Z, pp. 374-5, 380-1).

In L&Z, however, MSRP is the theory of rationality being vindicated and the first-order methodology used to reconstruct the Copernican episode. The problem here is that any rational reconstruction telling that Copernicus envisaged corroborated novel predictions would always fit the theory according to which scientific programmes are accepted based upon corroborated novel predictions. Therefore, to claim that MSRP as a theory of rationality is supported by a rational reconstruction of the Copernican Revolution in terms of Lakatos' methodology of science will beg the question.

This diagnosis of narrow circularity might be generalised to any rational reconstruction of historical episodes which depends exclusively upon MSRP, since the structure of the evidential relation is the same in all such cases. The lesson is that the historical evidence will never be in discord with the theory of rationality being tested. Alas, the evidential reasoning will appear to be dialectically ineffective for a rival historian who has reconstructed the same episode in terms of a different methodology of science.

3.4.2 Broad circularity

Lakatos faces a further problem: when two rival theories of rationality are used as second-order methodologies, and when the same historical episode is reconstructed from both theories, they cannot be applied as a meta-criterion to critically compare historiographical research programmes. Therefore, any attempt to decide between competing definitions of philosophical rationality by appealing to historical evidence will beg the question.

To see this, consider Lakatos' use of MSRP as a meta-criterion. The historian following the methodology of *scientific* research programmes makes this *prediction about scientific research*: "It will be found that victory was due to empirical degeneration

in the old and empirical progress in the new programme" (HSRR, p. 31). For instance, the prediction that "Copernicus' (and indeed, Aristarchus') *rough model* had excess predictive power over its Ptolemaic rival" (L&Z, p. 380). But how is a reconstruction of Copernicus' astronomy as being superior to Ptolemy's in terms of progressive problemshift able to provide independent corroboration for a novel historical prediction like this? One is inclined to reject that anticipated novel predictions can provide empirical support for themselves. As the case study of Copernicus is laden with MSRP, it cannot be argued that this case study has corroborated the novel prediction anticipated by the Lakatosian historiographical programme without begging the question.

Lakatos also asserts that the methodology of *historiographical* research programmes can be shown to be a progressive problemshift. He made this prediction about philosophical research: "the methodology of research programmes thus predicts [...] novel historical facts, unexpected in the light of extant (internal and external) historiographies and these predictions will, I hope, be corroborated by historical research" (HSRR, p. 32). But if narrow circularity makes rational reconstructions of episodes unable to corroborate the novel predictions of historiographical theories, then no historiographical research programme can be found to be empirically progressive. Besides theoretical progress, empirical progress is a necessary condition for progressive problemshift, which is the standard for the epistemic evaluation of research programmes: "let us call a problemshift *progressive* if it is both theoretically and empirically progressive, and degenerating if it is not" (FMSRP, p. 34). As long as empirical progress is not fulfilled, no historiographical research programme can constitute a progressive problemshift. At best, a successful corroboration of anticipated predictions of novel historical facts cannot be accomplished. At worst, historiographical research programmes cannot be found progressive whatsoever. But this is just what MSRP as a meta-criterion demands.

To sum up: Lakatos' meta-philosophy contains inconsistent tenets. RATIONAL RECONSTRUCTION implies that historical case studies are not independent of the theory of rationality being supported. Since the empirical testing of philosophical theories requires historical data to constitute independent evidence, historical case studies do not test philosophical theories. This means that EMPIRICAL ADEQUACY is not fulfilled. Moreover, EPISTEMIC EVALUATION is also precluded insofar as both first-order and second-order methodologies cannot be subject to critical comparison.

It is worth recalling that manipulation and pluralism are each sufficient for the dependence of historical reconstructions upon philosophical theories to be objectionably circular. Absent pluralism, theory-dependence exhibits vicious circularity vis-à-vis one single philosophical theory if the historical reconstruction has been manipulated. Absent manipulation, theory-dependence exhibits vicious circularity vis-à-vis competing philosophical theories if the historical evidence is unable to decide between them. Admittedly, some instances of theory dependence could not be objectionably circular. Regarding one single philosophical theory, a non-manipulated historical reconstruction can test such a theory. As for rival philosophical theories, non-manipulated historical reconstructions can confirm corresponding theories but not adjudicate between them. However, these two alternatives are unavailable to Lakatos for a couple of reasons. First, RATIONAL RECONSTRUCTION implies manipulating history. Second, EMPIRICAL ADEQUACY and EPISTEMIC EVALUATION imply historical pluralism. According to MSRP, adjudication between theories is necessary for empirical testing: "tests are —at least— three cornered fights between rival theories and experiment" (FMSRP, p. 31).

These considerations are sufficient for the circularity argument to run against the Lakatosian commitment to HPS. Despite this, I now want to sketch a plausible way of defusing such an argument —one that does justice (at least partially) to Lakatos' actual philosophical practice.

3.5 A typology of independent historical evidence

Hitherto I have shown how the *circularity argument* jeopardises Lakatos' metaphilosophy. However, I think it is useful to separate his meta-philosophy from philosophical practice, since his methodological pronouncements and practice do not always coincide with one another. Having done so, I want to argue that the Lakatosian commitment to HPS can be saved from vicious circularity. My aim is to defend the claim that historical case studies, including Lakatosian rational reconstructions, can constitute "independent evidence" (Kosso 1989, 1992). To do this, I propose a typology of independent historical evidence. There are at least three forms of independence vis-à-vis historical case studies. These forms depend upon both the *kinds of theory* used to produce historical reconstructions and the *relevant interaction* between them. My typology therefore allows to reject the second premise of the *circularity argument* as based upon the problems of historical pluralism and manipulation of history.

The first form of independent evidence can be characterised as follows:

TYPE-INDEPENDENT EVIDENCE: Historical reconstructions are theory-laden,

but not laden with methodologies of science.

Historical reconstructions are theory-laden to the effect that historical analysis entails theoretical commitments for selecting and interpreting historical events. However, it happens that much history of science is not laden with philosophies of science. In Lakatos' terminology, not all historical cases are reconstructed with theories of rationality; their reconstruction does not entail any methodology. Historical case studies that are not methodology-laden typically come from professional historiography of science. Such studies are relevant to the epistemic evaluation of first-order methodologies insofar as they permit the evaluator to show that methodologies of science are historically inadequate. Indeed, Lakatos used type-independent evidence to show that the history of science does not bear out rival theories of rationality. He appealed to a few primary sources from historical actors, and some studies from professional historiography whose dependence upon the methodologies of science under test is far from obvious.

Limitations of space prevent me from conclusively proving that historical reconstructions from professional historiography constitute type-independent evidence, let alone those historical studies Lakatos used. However, consider the fact that some professional historians have explicitly argued that they do not (and should not) employ philosophical theories to write the history of science. For instance, Shapin seems to agree with Hacking on the idea that experimental practice lives a life of its own —which can also be applied to understanding the history-philosophy relation. Shapin (1982) states:

I will not deal with programmatic statements and will make only brief references to some admirable, and often historically sensitive, theoretical literature in the sociology of knowledge. It would be quite incorrect to regard empirical literature as if it were merely a "testing" of some theoretical programme. Even though empirical work has an important bearing on the validity of theoretical positions, its significance may only be properly appreciated if it is understood on its own terms. (p. 326)

From a distant historiographical perspective, I. B. Cohen criticised the assumption that the work of philosophers of science is actually useful for historians. According to him, it would be much easier for the philosopher to endow philosophical statements with historical content according to historiographical standards of adequacy and based on the most recent results of professional historical investigation. However, it would be extremely difficult for the historian to employ any philosophy of science as a tool of historical analysis: Let me now contrast so simple an assignment with the situation of a historian of science studying conceptual history, say the formation or "transformation" of some set of scientific ideas, and who may wish to ascertain whether there is any aspect of philosophy of science which may be relevant to his inquiries. I personally find great difficulty in ascertaining which books or articles dealing with the philosophy of science may be useful for historical inquiries. It often seems as if a historian would have to become aware of all the major developments in philosophy of science before he could make a valid argument as to the possible ways in which any of all parts of the philosophy of science might be useful for his problems. This task seems well beyond the philosophical capacities of almost all historians. (Cohen 1974, pp. 312-3)

Finally, Rossi explicitly attacked RATIONAL RECONSTRUCTION. He vehemently rejected to take the philosophy of science as the source of methodological theories to write the (rational) history of science:

Historians (even those of science) have never had strong sympathies for methodologies too rigid, and the image of historiography tends to escape from all sides to the classifications and systematisations [...] that epistemologists have proposed [...]. Neither they are interested in the "pre-constituted examples" nor in the "crisp, clear, and semi-false" answers in the manuals, as Grmek has written. What interests them most —and this is a decisive point— are temporal processes rather than "logical substitutes." (Rossi 1986, pp. 208-9)

Similar pronouncements are hard to find in textual evidence, but they conceivably reflect a tacit, agreed-upon position among mainstream historians of science. Philosophers have even been worried about this fact of professional historiography (e.g., Fuller 1991; Laudan 1989, 1990; Miller 2011; Pinnick and Gale 2000; Steinle and Burian 2002). If this is correct, then there are good reasons to think that historical case studies from professional historiography constitute type-independent evidence. Of course, the sceptic can prove with examples that historians do something too much different from what they typically preach. She thus requires providing clear instances of systematic use of philosophical thesis of science in historical reconstructions from professional historiography. Until this is done, one can accept that historical case studies written by professional historians are laden with theory, but not with theory from the philosophy of science.

Besides this general aspect of historiography of science, notice that the works of professional historians that Lakatos appealed to are not rational reconstructions. Rather, Lakatos used them as if they were providing information about actual history of science, against which rational reconstructions are criticised for lack of historical adequacy. Here are some examples from L&Z.

Lakatos claims that *verificationism* cannot explain the Copernican Revolution in terms of valid deduction from empirical data because "now it is acknowledged that both Ptomemy's and Copernicus' theories were inconsistent with known observational results" (L&Z, p. 357). He appeals to Gingerich's (1973, as cited in L&Z, p. 357 n. 7) paper to support this critical judgment. Regarding *conventionalism*, Lakatos objects to reconstructing the Copernican Revolution in terms of simplicity as an epistemic value, since "the superior simplicity of Copernican theory" was a myth "dispelled by the careful and professional work of modern historians" (L&Z, p. 362). The historical studies he uses include Price's and Ravetz's articles (Price 1959, as cited in L&Z, p. 362, and HSRR, p. 27 n. 102; Ravetz 1965, as cited in L&Z, p. 363 n. 30), as well as Cohen's, Koestler's, and Dreyer's big narratives (Cohen 1960, as cited in HSRR, p. 28 n. 103; Koestler 1959; Dreyer 1953, as cited in FMSRP, p. 33 n. 2). As for falsificationism, Lakatos argues that it is false that Ptolemy's astronomy was either "unscientific" or "conclusively falsified" due to inherently ad hoc heuristics used to save astronomical data of the Alfonsine tables. But Gingerich (1975) demythologises that "Ptolemaic theory included an indefinite number of epicycles which could be manipulated to fit any planetary observations" (L&Z, p. 358), thus suggesting that "the alleged superiority of Reinhold's Prutenic tables over the Alfonsine ones could not provide the crucial test" (L&Z, p. 359). Likewise, Lakatos points out that *elitism* fails to explain the Heliocentric achievement because Kuhn's reconstruction made the false statement that the community of Ptolemaic astronomers was facing a paradigm-crisis. In fact, "Gingerich [1973] showed that Kuhn conjures up a scandal where there was none" (L&Z, p. 366 n. 43).

To favour his MSRP, Lakatos focuses the attention on the equant's elimination as one indicator of progressive problemshift in Copernicus' theory. Ptolemy introduced equants as an *ad hoc* hypothesis that was inconsistent with the principle of uniform, circular motion of celestial orbs. Placing the Sun at the centre of the universe, "Copernicus not only dispensed with equants, but also, through replacing equants by epicycles, he happened to improve on the fit between theory and observation" (L&Z, p. 374). Lakatos supports his verdict in some passages from Copernicus' *Commentariolus* and Neugebauer's (1968, as cited in L&Z, pp. 374, 378) historical account. Indeed, this view was the canonical interpretation of the Copernican Revolution in professional historiography at that time (Swerdlow 1973). In type-independent evidence, the historical reconstruction does not presuppose the kind of theory being tested. That is to say, the theory reconstructing the historical case and the methodology under test are quite distinct because the historical reconstruction is not a rational one —i.e., it does not involve third-world premises. Embracing this conception has at least two advantages.

The first advantage is that it permits avoiding the problem of manipulating history. Using type-independent evidence requires the historian to abandon the assumption that actual history is reducible to facts of the second world, whereby methodologies of science are the only relevant criterion for selection and interpretation. Internal history need not be exclusively laden with methodologies of science, let alone become fictional history. Competing rational reconstructions are (and should be) ranked using historical reconstructions that are not methodology-laden, thus making it possible to maximise rational explanations of historical episodes. The second advantage of using type-independent evidence is to block the problem of historical pluralism. Since the historical reconstruction does not presuppose the *type theory* being tested, narrow circularity and broad circularity are broken. Therefore, definitions of both scientific and philosophical rationality can be critically compared in principle by appealing to historical case studies.

Suppose now a situation in which no type-independent evidence is available to support theories of rationality —i.e., when one intends to test methodologies of science in light of rational reconstructions exclusively. In such cases, one can use rational reconstructions that do not depend upon the methodology being tested, but upon a different methodology. These historical case studies fit this second form of independent evidence:

TOKEN-INDEPENDENT EVIDENCE: Historical reconstructions are laden with methodologies of science, but the methodology that reconstructs historical episodes is not the methodology being tested.

As an example, consider Lakatos' recourse to Kuhn's (1957) study of Copernicus to bear out the claim that the Copernican Revolution was not the result of theory choice in terms of simplicity. Lakatos pointed out that "the modern study of primary sources, particularly by Kuhn, has dispelled this myth and presented a clear-cut historiographical refutation of the conventionalist account. It is now agreed that the Copernican system was 'as least as complex as the Ptolemaic'" (HSRR, p. 28). Notice that this test case against *conventionalism* involves a historical reconstruction laden with *elitism* (i.e., the Kuhnian theory of scientific rationality), but *elitism* is distinct from *conventionalism* (i.e., the theory being tested). This instance presupposes some methodology of science, but the two methodologies in question are not the same.

Using token-independent evidence closes the door to the problem of pluralism. Since the rational reconstruction does not presuppose the *token theory* being tested, the circularity is broken and thereby a critical comparison of theories of both scientific and philosophical rationality is possible. Of course, instances of token-independent evidence cannot easily avoid the accusation of manipulation as long as they involve rational reconstructions exclusively. However, there is a way of preventing this potential charge from arising. What is needed is to see how token-independent evidence and typeindependent evidence can interact with one another when they are used in evidential reasoning. The point is that one can reinforce the epistemic support that rational reconstructions confer upon theories of rationality by appealing to historical case studies that are not laden with methodologies of science. This idea can be formulated as follows:

HIGHER-ORDER INDEPENDENT EVIDENCE: Rational reconstructions that support methodologies of science are confronted with historical reconstructions that constitute type-independent evidence.

This confrontation between kinds of reconstructions involves two distinct yet related evaluative strategies in evidential reasoning, to wit: "calibration" (Franklin 1997; Franklin et al. 1989) and "robustness" (Stegenga and Menon 2017). First, a rational reconstruction has been found to be *calibrated* by using another historical reconstruction that is not methodology-laden. Second, there is a concordance between these two reconstructions in such a way that the methodology of science being tested is found to be *robust*. Let me explain each strategy in turn.

Historical calibration

This strategy consists in taking a *historical* reconstruction which does not depend upon any methodology of science as a surrogate for a *rational* reconstruction supporting the theory of rationality under test. To illustrate this, I want to calibrate L&Z by appealing to the current historical scholarship about Copernicus.

L&Z receives a negative assessment due to calibration —both Lakatos' original reconstruction and Zahar's refined one. Historians (Goldstein 2002; Westman 2011) have recently argued that Copernicus aimed to solve the problem of planetary order rather than eliminate the heuristics of equant points. After all, geocentric models dispensing with equants were commonly designed and used in the early 13th century, and Copernicus did equip his heliocentric modelling with some equants in the technical books of De Revolutionibus. In Goldstein's (2002) words:

We now know that Muslim astronomers, beginning in the thirteenth century, were able to produce many models that resolved the problem while maintaining a geocentric framework. In other words, the equant was an astronomical problem whose solution did not impinge on cosmological issues. [...] To construct a heliocentric system in any detail, Copernicus needed to transform Ptolemy's geocentric models (modified to resolve the equant problem) to heliocentric models. But in my view, this was done only after he made an initial commitment to a heliocentric system. (pp. 220-1)

This new historical account contradicts Lakatos' rational reconstruction, according to which the Ptolemaic programme was degenerating to the effect that "Copernicus recognised the heuristic degeneration of the Platonic program at the hands of Ptolemy and his successors" (L&Z, p. 372). This is because equant elimination was not the reason for introducing the Copernican system in the first place. If the current historical scholarship about Copernicus counts as a standardised surrogate for rational reconstructions of this episode, then Lakatos' reconstruction fails the test from historical calibration.

Consider now what happens with Zahar's reconstruction. This is based upon a modified version of MSRP, which involves an interesting reformulation of the concept of "novel fact" (F) as satisfying these three conditions: (i) F must be a prediction of the programme; (ii) the rival programme cannot explain F but only in an ad hoc manner; and (iii) when F is well-known, F is a by-product of the programme. For Zahar (1976), "a fact will be considered novel with respect to a given hypothesis if it did not belong to the problem situation which governed the construction of the hypothesis" (p. 218). On this basis, Zahar's reconstruction claims that Copernicus' astronomy predicts planetary order by determining the distance-period relationship among planets, arguing that this prediction meets conditions (i)-(iii). Lakatos pointed out that "Zahar's account thus explains Copernicus' achievement as constituting genuine progress compared with Ptolemy" (L&Z, p. 380).

Certainly, this account agrees with current historiography in that Copernicus solved the problem of planetary order. However, Goldstein (2002) points out that "Copernicus argued explicitly against the Ptolemaic system as violating the distanceperiod relationship" (p. 222), which suggests that planetary order was the problem the heliocentric arrangement was designed to solve. If Goldstein is correct, then planetary order is not a novel fact in Zahar's sense. The solution to this problem does not count as a by-product of the Copernican theory because it belongs to the problem situation that governed the construction of the heliocentric hypothesis. Therefore, Zahar's reconstruction also fails the test from historical calibration.

Historical robustness

This strategy aims to support one theory of rationality using multiple reconstructions laden with theories that are independent of one another. These reconstructions in turn are *concordant* —i.e., they coincide in their interpretation of the relevant historical facts. On these grounds, a theory of rationality fulfils robustness as long as concordant historical case studies support such a theory.

Concordance can be understood in two ways. First, a theory of rationality is robust vis-à-vis concordant historical reconstructions that include at least one that is methodology-laden. For instance, the negative assessment of *conventionalism* becomes robust with regard to two different historical reconstructions relative to independent theories, only one of which is a methodology of science. These are Kuhn's (1957) and Gingerich's (1975) historical reconstructions of the Copernican Revolution. Whilst Kuhn's *elitism* presumably qualifies as a theory of scientific rationality, Gingerich's theoretical commitments do not. Gingerich's reconstruction is instead a case in point of conceptual history, which rests on a conception of scientific change in terms of aesthetic and metaphysical factors:

What has struck Copernicus is a new cosmological vision, a grand aesthetic view of the structure of the Universe. If this is a response to a crisis, the crisis had existed since A.D. 150. Kuhn has written that the astronomical tradition Copernicus inherited "had finally created a monster", but the cosmological monster had been created by Ptolemy himself. (Gingerich 1975, p. 90)

Gingerich's conceptual account contradicts Kuhn's elitist reconstruction in that the Copernican Revolution was not brought about by a paradigm crisis. Despite this, Gingerich's rendering agrees with Kuhn's concerning another particular conclusion relevant to *conventionalism*, to wit: that Copernican astronomy was not simpler (Gingerich 1975, pp. 87-8; Kuhn 1957, p. 169). The inadequacy of *conventionalism*, then, is grounded on an argument of historical robustness —which involves one historical reconstruction that is methodology-laden and one that is not.

Another way of understanding concordance is this: a theory of rationality is robust vis-à-vis concordant historical reconstructions that are not methodology-laden whatsoever. To illustrate this, consider the negative assessment of L&Z in light of the current historical scholarship again. This verdict becomes robust relative to both Goldstein's (2002) and Westman's (2011) reconstructions of the Copernican Revolution. Although the two are contextual histories that do not depend upon any methodology of science, the former counts as intellectual history whereas the latter is a social history of science. Unlike Goldstein, Westman provides an explanation of the Copernican Revolution as an answer to the problem of planetary order that appeals to social factors. For Westman (2011), "uncertainty about astral powers and planetary order would become one of the problems —perhaps even the crucial one— to which Copernicus's reordering of the planets was a proposed, if unannounced, solution" (p. 3). In turn, Copernicus' theory allowed him to restore the epistemic status of astrology by reforming astronomy —in a social context wherein astrological prognostications were decisive: "Copernicus's initial turn to the heliocentric planetary arrangement occurred in the context of a late-fifteenth century political controversy about the credibility of astrology triggered in 1496 by Giovanni Pico della Mirandola's attack on the science of the stars" (Westman 2013, p. 101).

The point is that Goldstein and Westman agree on the claim that the solution of planetary order was present in the very origin of the heliocentric theory, thus preceding the problem of planetary modelling related to equants' elimination. Of course, this is a robustness argument *against* MSRP. However, it could be a robustness argument *for* a different theory of scientific rationality. For instance, if Goldstein (2002, p. 222) is correct in arguing that the heliocentric arrangement was for Copernicus the only system known to him that did satisfy the distance period relationship, then this historical evidence will provide a positive assessment for a methodology of science defending that eliminative inference is a cornerstone of scientific reasoning (e.g., McCain and Kampourakis 2020).

To sum up: my foregoing typology of independent historical evidence is intended to establish that historical case studies constitute independent evidence for philosophical theories in different forms. As far as Lakatos is concerned, this shows how rational reconstructions can be independent of the theory of rationality being tested. With this typology, the evaluation of rival rational reconstructions and the critical comparison of competing methodologies of science is ultimately vindicated.

3.6 Conclusion

In this chapter, I have examined Lakatos' conception of HPS in light of the *circularity* argument. I have attacked and defended his view of using historical case studies as a test for philosophical theories of scientific rationality. There is no inconsistency in doing both things at the same time. My line of argument has been Lakatosian in character. Lakatos argued that (philosophical) knowledge progresses via negative criticism, according to his "quasi-empirical" view of epistemic justification (REPM?, pp. 28-30). But he equally affirmed that "important criticism is always constructive: there is no refutation without a better theory" (FMSRP, p. 6). So my constructive criticism of Lakatos' conception of HPS brings about both negative and positive results.

On the negative side, I have shown how the *circularity argument* seriously threatens Lakatos' meta-philosophy. As central to his proposal, the rational reconstruction of historical episodes and the critical comparison of rival methodologies of science open the door to manipulation and pluralism. On these grounds, the sceptic can contend that rational reconstructions are not independent of the theory of rationality being tested, thus concluding that these normative interpretations of history cannot test philosophical theories.

On the positive side, I have outlined a typology of independent historical evidence that prevents vicious circularity from arising in the practice of testing methodologies of science against rational reconstructions. My case study of Lakatos considered this particular type of historical reconstruction and this specific kind of philosophical theory as the two elements of the evidential relationship, showing how rational reconstructions interact with historical reconstructions that do not depend on any methodology of science. In this way, one can reply to the sceptic that even rational reconstructions can test philosophical theories of rationality, since historical case studies do constitute independent evidence in different forms.

It is worth noticing that my line of argument suffices to rebut the claim that the confrontation model is absolutely "misleading and should be abandoned" (Schickore 2011, p. 477). Consider the following analogy with research programmes. Lakatos insisted that problemshifts can be progressive in the presence of anomalies and when resting on inconsistent foundations. The situation of Lakatos' conception of HPS is similar. A return to the confrontation model in the current practice of HPS seems promising as long as each epistemological problem is progressively countered. Closing the door to vicious circularity is just one step in such a problemshift. My typology of independent historical evidence can justifiably be seen as a progressive heuristic device of evidential reasoning in HPS.

As such, my typology is primarily useful to rebut the *pluralism argument* (§1.2.3), specifically the assumption of "precluding empirical adjudication", which states that historical evidence is not neutral provided the rival philosophical theories lack of shared evidence base. In the next chapter, I will present a way of defusing the assumption of "precluding non-empirical adjudication" in that same argument, which contends that historiographical standards cannot adjudicate philosophical disagreements as those standards are too weak. Let me now turn to examine Stanford's strategy of historical ostension.

Chapter 4

The Strategy of Historical Ostension

Abstract

This chapter explores the role of historiographical standards in the assessment of historical evidence as a second strategy to respond to the circularity argument. My case study is Stanford's strategy of historical ostension that poses a two-fold historical challenge to scientific realism. This strategy intends to disconfirm selective realism and confirm the problem of unconceived alternatives in light of some historical case studies. My analysis underpins two claims. The categorical thesis is that Stanford's disconfirmation of selective realism is dialectically effective because historiographical standards can adjudicate the selective realism debate concerning the caloric episode; this closes the door to historical pluralism. My conditional thesis is that if Stanford's forms of manipulating historical material do not yield unreliable data, then his historical case study is entitled to confirm the problem of unconceived alternatives. This will close the door to manipulation of history just in case the antecedent claim obtains. My analysis thus concludes that the canons of historical criticism arguably block or at least weaken the circularity argument.

4.1 Introduction

My purpose in this chapter is to show how historical criticism make the circularity problem tractable. I discuss the extent to which historiographical standards to assessing the quality of historical accounts prevent the problems of both historical pluralism and manipulation of history from arising. To do so, I draw upon Stanford's strategy of historical ostension as a case study.

The strategy of historical ostension uses historical case studies to both articulate philosophical ideas and provide evidence of philosophical claims. Specifically, Stanford employs this strategy to pose a "historical challenge to scientific realism." This challenge is two-fold. First, Stanford draws upon episodes like the caloric theory of heat to reject the selective confirmation strategy for selective realism, since this strategy fails to block the classic pessimistic induction: *If many successful past theories were false, then*

4.1 Introduction

there are good reasons for believing that our successful current theories are false, too. Second, and most importantly, Stanford offers a historical study of 19-century theories of biological inheritance to establish the problem of unconceived alternatives, which threatens scientific realism as based upon the inference to the best explanation: If for many successful past theories there were alternatives which past theorists were unable to conceive of, then there are good reasons for believing that successful current theories have alternatives which today's theorists are unable to conceive of.

Examining this two-fold challenge to scientific realism, I argue for two claims. I affirm *categorically* that Stanford's use of the caloric episode as evidence against selective realism can escape from the problem of historical pluralism. That is, Stanford's evidential reasoning is dialectically effective in disconfirming selective realism. However, I claim *conditionally* that if it is true that Stanford's reconstruction of theories of biological inheritance does not yield unreliable historical data, then his historical case study confirms the problem of unconceived alternatives and his argument is not therefore self-serving. My discussion suggests that there are feasible reasons for the antecedent of this conditional claim.

The chapter is structured as follows. Section 4.2 characterises Stanford's historical challenge to scientific realism as involving these two lines of proof: a *defensive proof* that relies upon the classic inductive base, and a *direct proof* that is grounded in the new inductive base. Section 4.3 examines whether historical pluralism could threaten Stanford's *defensive proof.* I criticise Vickers and Chakravarty's view that the selective realism debate concerning the caloric episode cannot be adjudicated in historical terms, arguing instead that historiographical standards of historical adequacy are strong enough to resolve this disagreement. My argument plausibly rebuts their characterisation of this particular debate, so it also casts doubt on the cogency of historical pluralism itself. Section 4.4 explores the extent to which manipulation of history jeopardises Stanford's *direct proof*. I show that Stanford's philosophical approach to history involves objectionable forms of abstraction and idealisations only in some respects. However, I suggest possible ways in which such objectionable forms do not curtail the historical adequacy of Stanford's account. Although these answers could not be entirely convincing for the sceptics, they make it plausible that Stanford did not manipulate historical material in a self-serving way. On these grounds, I conclude that the canons of historical criticism are useful to block the *circularity argument* or at least to mitigate its dangerous effects.

4.2 Historical challenge to scientific realism

Stanford (2006) characterises his challenge to scientific realism as based upon an "enumerative induction" from the history of science (p. 47). The argument begins by picking out a relevant set of episodes as *historical evidence*. This evidence exhibits a *historical pattern* of scientific enquiry that reveals the problem of unconceived alternatives. This pattern also provides a ground to make an *inductive projection* of the problem of unconceived alternatives from past to contemporary scientific practice. In this way, Stanford concludes that the problem of unconceived alternatives challenges explanationist scientific realism (ESR): the thesis that the empirical success of the best contemporary scientific theories is a reliable indicator that these theories are probably, approximately true —when the (approximate) truth of these theories is the (probable) best explanation of their empirical success (e.g., Boyd 1983; Leplin 1997; Psillos 1999; Putnam 2012; Smart 1963).

The historical case studies Stanford employs to support the historical challenge can be classified into two groups. I call the "classic inductive base" the set of relevant cases related to the pessimistic induction (Laudan 1981; Stanford 2006, Chs. 6-7, 2017):

(CIB)

- The caloric theory of heat
- The phlogiston theory of chemistry
- The wave theory of luminiferous ether
- Dalton's theory of atom
- Newtonian mechanics

Stanford offers a *defensive proof* for the historical challenge based upon CIB. He notes that if correct, selective realism undermines Laudan's original contention and hence dismisses "the significance of the problem of unconceived alternatives as well" (Stanford 2006, p. 141). Selective realists argue that it is justified to believe that current fundamental scientific theories are approximately true "even if we allow that there probably are serious alternatives to them that remain presently unconceived" (Stanford 2006, p. 141). They maintain that ESR is quite immune to the problem of unconceived alternatives provided there are good reasons for accepting selective realism: as some parts of theories are approximately true as they are differentially

confirmed by the evidence, the problem of unconceived alternatives has no bearing on the cogency of ESR (e.g., Kitcher 1993, 2001; Psillos 1999; Votsis 2011). Despite this, Stanford shows that selective realists cannot escape from the historical challenge, since CIB leads to rejecting the selective confirmation strategy as an answer to the pessimistic induction.

Now, I call "the new inductive base" Stanford's (2006, Chs. 3-5) own historical study of 19th-century theories of generation and inheritance. His study focuses on the following cases:

(NIB)

- Darwin's theory of pangenesis
- Galton's theory of the stirp
- Weismann's theory of the germ-plasm

According to Stanford's reconstruction, historical actors were reasoning according to eliminative inference; they accepted one theoretical explanation of inheritance as the only theory that was able to account for the available evidence at the time. Stanford (2006) goes on to argue that historical actors were thus facing a recurrent transient underdetermination, that is, the repeated failure of past scientists and scientific communities "to conceive of all the empirically inequivalent but scientifically serious alternative theoretical possibilities well confirmed by the evidence available" (p. 19). Seeing this historical pattern leads Stanford (2017) to draw the following inductive conclusion about today's science: "the pervasiveness of this pattern [...] generally would seem to give us every reason to believe that there are well-confirmed, fundamentally distinct alternatives to our own foundational scientific theories that nonetheless remain unconceived by contemporary scientists, even if we cannot specify or describe them further" (p. 214). On these grounds, Stanford finally puts forward that the problem of unconceived alternatives rebuts ESR.

As based upon NIB, this "new induction" is a *direct proof* for the historical challenge. Here is Stanford's argument against ESR:

- **P1.** If ESR is likely to be true regarding fundamental theories, then eliminative inference is reliable. (Meta-epistemic condition.)
- **P2.** For eliminative inference to be reliable, it must apply to an exhaustive set of fundamental theories that are relevant candidates. (Epistemic condition.)

- P3. If scientists face the predicament of recurrent transient underdetermination, then eliminative inference does not apply to an exhaustive set of relevant candidates. (Epistemic defeater.)
- **P4.** Past scientists were facing the predicament of recurrent transient underdetermination of fundamental theories. (Historical evidence from NIB.)
- **P5.** Today's scientists face that same predicament vis-à-vis fundamental theories. (Inductive projection, from P4.)
- P6. Eliminative inference does not apply to an exhaustive set of relevant candidates. (Modus ponens P3, P5.)
- **P7.** Eliminative inference is not reliable. (Modus tollens P2, P6.)
- **Therefore,** ESR is not likely to be true regarding fundamental theories. (Modus tollens P1, P7.)

This argument can be contested. Wright (2018) claims that ESR is justified without the explanationist strategy underlying P1. Regarding P2 and P3, Bird (2010) believes that eliminative inference reliably works on an exhaustive space of conceived alternatives that meet a modal condition —i.e., "all the potential explanations that are true in nearby possible worlds" (p. 351). Other authors instead focus on P4 and P5. Some realists who take the historical challenge seriously accept the inductive base (P4) while rejecting the inductive projection (P5). Godfrey-Smith (2008), for instance, maintains that the problem of unconceived alternatives is mitigated as current scientific communities are not (or less) vulnerable to this problem. Whilst the historical evidence shows that individual historical actors failed to conceive of alternative theoretical explanations, contemporary scientists are in a better position to exhaust the space of alternatives. (For a detailed discussion, see also Alai (2017); Devitt (2011); Fahrbach (2011); Müller (2015); Ruhmkorff (2011); Stanford (2019); Wray (2013).)

Besides these critiques of Stanford's argument, I want to explore a rather new line of objection, namely whether the *circularity argument* afflicts Stanford's historical challenge. For it might be argued that Stanford's reasoning is to be rejected as long as both historical pluralism and manipulation of history defeat his use of historical evidence. In what follows, I discuss the idea that historical pluralism could obtain regarding the *defensive proof* that involves CIB, whilst the manipulation of history could obtain vis-à-vis the *direct proof* that relies upon NIB.
4.3 Pluralism and the classic inductive base

Here I examine whether historical pluralism threatens the *defensive proof* for the historical challenge. However, I do not consider all the historical cases of CIB but restrict my analysis to Stanford's account of the caloric theory of heat. The reason is that this historical episode has been especially important for the scientific realism debate and reconstructed from two competing philosophical theories. Whilst the realist position (Psillos 1994, 1999) argues that this case favours the selective confirmation strategy, the non-realist position denies such a thesis (Chang 2003; Stanford 2003, 2006). As there are two rival historical accounts of the same historical episode, the relevant issue is therefore whether the *defensive proof* begs the question —i.e., if the disagreement between the competing reconstructions of the caloric in the selective realism debate cannot be adjudicated. If the answer to this question is affirmative, then this will force Stanford to admit that the *defensive proof* is *dialectically ineffective* in disconfirming the selective confirmation strategy.

Some scientific realists have characterised and evaluated the selective realism debate by appealing to historical pluralism. Specifically, they have contended that the dispute about the caloric theory of heat illustrates how historical evidence makes philosophers unable to adjudicate that debate. This is so because the disagreement is beset with steps 3 and 6 of the *pluralism argument* (§1.2.3). Recall both steps:

- (3) Precluding empirical adjudication. In disagreements between two philosophical theories that were used to reconstruct the same historical episode, the historical evidence is not neutral provided the rival philosophical theories lack of shared evidence base. This makes historical evidence unable to adjudicate such disagreements.
- (6) *Precluding non-empirical adjudication*. Neither philosophical standards nor historiographical standards can resolve philosophical disputes.

Embracing step 3, Vickers (2017) points out:

It is said that all history requires a perspective, all 'historical data' is theoryladen, and there is no such thing as 'pure history'. For example, consider the contrast between Psillos (1994) and Chang (2003): both apparently provide a careful historical analysis of the caloric theory of heat, but Psillos finds here support for realism, whereas Chang finds instead a threat to the realist position. Psillos (p. 162) even writes, "I do not deny that my use of historical evidence is not neutral –what is?– but rather seen in a realist perspective." Is Chang's use of history also not neutral, and biased by an anti-realist perspective? (pp. 49-50)

As for step 6, Chakravartty concedes that historiographical standards cannot decide between realist and non-realist accounts of the caloric episode, thereby insisting that historical case studies are unable to settle debates about scientific ontology more generally. Regarding the role of historiographical standards, Chakravartty (2017) states:

Herein lies the difficulty, however, for the mere existence of shared standards does not yield determinate answers regarding which side in the dispute about caloric is correct. Historiographical standards must be interpreted and applied, and their relative importance weighed when they sometimes pull in different directions, and it is here that even agreement on standards can easily dissolve into disagreement regarding which conclusions are best supported. (p. 27)

This characterisation of the selective realism debate concerning the caloric episode is misleading. I argue that this dispute can justifiably be understood as a resolvable conflict in historical terms. I show how historiographical standards do play a crucial role in adjudicating the selective realism debate in terms of the quality of the historical reconstructions of the caloric episode.

I thus argue for the following points. First, there is a standard of "diachronical historical adequacy" in the historiography of science that is independent of the philosophy of science. Second, this standard is relevant for the selective realism debate, and accepted by and neutral regarding the competing philosophical positions. This standard is implied by the fact that selective realism must be tested against past scientists' judgments of selective confirmation. Third, when this standard is applied, the textual material about caloric suggests that scientists' judgments of selective confirmation are unreliable. On these grounds, there are good reasons to believe that conflicting historical renderings of the caloric episode can be ranked in terms of their historical adequacy, thereby the historical evidence supports the non-realist position rather than selective realism itself. To do so, I draw upon Psillos' historical case study of the caloric as representative of the realist position, whilst employing Stanford's (2003, 2006) critiques of Psillos (1994, 1999) overlap in some important respects.

4.3.1 Independence

Historical evidence must be good evidence in the sense that historical reconstructions aim to provide an adequate representation of historical events. Only in this way, the study of the history of science provides factual knowledge about past science, thus allowing to understand how philosophical conclusions could be ultimately based upon data about historical facts rather than on "imagined history", as I. B. Cohen (1974) called it. Additionally, if historical research intends to tell narratives of "what really happened in the past", then good historical evidence must meet some historiographical criteria underwriting historical adequacy. These criteria are encapsulated by the following standard:

ADEQUACY: If analysts aim at getting historical evidence from good historical reconstructions, then they must reconstruct the historical situation on their own terms by carefully attending to the available textual material.

ADEQUACY presupposes three salient elements. The first element concerns the *diachronical character* of historical analysis, according to which historical explanation involves relevance and asymmetry considerations. Relevance is that explanatory variables must only include those factors that play a causal role in the occurrence of historical events. Asymmetry is that the explanatory relation between historical events is constrained by a chronological succession parameter, whereby only proximate earlier events are factors that can explain later events. According to Kragh (1987):

The diachronical ideal is to study the science of the past in the light of the situation and the views that actually existed in the past; in other words to disregard all later occurrences that could not have had any influence on the period in question. Occurrences that took place before, but which were actually unknown at the time, have to be regarded as non-existent too. (p. 90)

The second element implies a rejection of historiographical presentism, which assumes that "the science of the past ought to be studied in the light of the knowledge that we have today, and with a view to understanding this later development, especially how it leads up to the present" (Kragh 1987, p. 89). For one thing, presentism gives an *anachronistic account* that interprets and explains historical events by projecting contemporary ideas and values onto the past. For another thing, presentism provides a *teleological account* that takes the past as ultimately conducting to the present, thus judging "the past in terms of the present" (Henry 2002, p. 3). Historiographical presentism is objectionable because it is at odds with the central goal of the historical discipline, which boils down to "the study of the past for the sake of the past".

Mainstream historians of science arguably adopt the diachronical perspective at least in principle. Shapin (2010), for instance, claims that "the task of the historian was not to celebrate its contribution to the future but to describe and interpret its historical situatedness", adding that "the standards by which historians should assess past scientific work were not those of the present but those of the pertinent past" (p. 6). On this view, historical investigations become unworthy of pursuing if diachronical historical analysis cannot be accomplished whatsoever. As I. B. Cohen (1974) argued, presentism ultimately denies "the reason for studying history in the first place" (p. 349).

The third element focuses on the role of *textual evidence* in a diachronical historical analysis. If analysts want to understand historical actors' thoughts and actions, then the most direct source of information is to be found in what historical actors have registered in a textual form. Obviously, this does not exclude other types of historical sources. For Kragh, the use of available textual evidence permits picking out the relevant explanatory variables to understand the historical situation in its original context and integrity. Discussing the vices of presentism, Kragh (1987) indicates that "much history of science commits anachronistic sins by streamlining and clarifying past thoughts far beyond what is justified by textual evidence" (p. 90). On this view, historical analysis involves studying actors' original texts, alongside other types of sources providing circumstantial information about the historical situation in question, especially about the generation-process of textual sources.

ADEQUACY thus encompasses some generally agreed-upon historiographical criteria in mainstream historiography of science. This standard calls for the need for a diachronical historical analysis to obtain factual knowledge about past science. To achieve this aim, relevant textual material must be critically employed in approaching the historical situation on "its own terms" and according to the original context.

4.3.2 Relevance

ADEQUACY is also relevant to the philosophy of science. Put generally, the philosophical approach to history makes historical statements and hence is subject to historical criticism. Hence philosophers participating in the selective realism debate are (and should be) sensibly worried about the extent to which their statements are historically adequate. More specifically, ADEQUACY is central to the selective realism debate in considering what is supposed to make a historical case for the selective confirmation strategy. ADEQUACY allows to determine the extent to which competing renderings of historical cases can support selective realism. Applying this standard will therefore be useful in judging which philosophical position is better supported by the historical evidence, since ADEQUACY would ultimately indicate which historical reconstruction of the same episode is more (in)adequate in historical terms.

To see this, notice that the selective confirmation strategy seeks to establish a criterion for identifying the "central core" of theories. This "core" involves the elements of past theories that were responsible for empirical success provided those elements were differentially confirmed by the available empirical evidence. In this way, selective realists argue that the "central core" is preserved through theory change. However, realists must have a principled way of individuating such a "central core" that is to be independent of what has been preserved until now, otherwise the selective confirmation strategy would be flawed.

The difficulty stems from appealing to the trivial fact that some aspects of theories are preserved in order to identify the central core in past theories, thereby making a retrospective judgment based upon what realists know today. In doing so, realists are adopting a historiographical perspective that is anachronistic and teleological in interpreting past theories. They are taking as the "central core" of past theories those elements that apparently resemble the aspects of current theories that realists now accept as differentially confirmed by the evidence. As a result, the selective confirmation strategy will face two problems. First, selective realism would be trivially true, because it is very easy for realists to find retrospective convergence between past and present theoretical elements. By current lights, it is always possible to conclude that these or those elements of past theories are the "central core", but realists' retrospective judgment can nonetheless turn out to fail. Secondly, selective realism begs the question, since retrospective judgments take it for granted that current scientific theories are successful and hence approximately true.

Both Psillos and Stanford recognise that the evidential support historical evidence provides to philosophical claims must avoid these two problems. Consider what Psillos says about whether realists can provide a principled criterion for selective confirmation that is independent of both the trivial historical retention and the realist commitment to current theories:

It is not that realists come, as it were, from the future to identify the theoretical constituents of past theories that were responsible for their success. Scientists themselves tend to identify the constituents which they think were responsible for the success of their theories, and this is reflected in their attitude towards their own theories [...]. My claim is that it is precisely those theoretical constituents which scientists themselves believed to contribute to the successes of their theories (and hence to be supported by the evidence) that tend to get retained in theory change. (Psillos 1999, p. 107)

Psillos is suggesting that realists must find a criterion for selective confirmation in past scientific judgments, which requires scrutinising the available textual evidence. Thus, Psillos would perfectly agree with the claim that the success of the strategy of selective confirmation implies a diachronical historical analysis, examining scientists' pronouncements about the epistemic status of their theories carefully. This methodological manoeuvre boils down to understanding judgments of selective confirmation in terms of historical actors themselves.

Stanford (2006) also thinks that retrospective judgments are not helpful to the selective strategy, indicating that the "realist reply to the historical challenge is either question-begging (if it assumes the truth of present theories) or unconvincing (if it simply fastens on one explanation among several plausible alternatives for the substantial correspondence that it finds)" (p. 174). For this reason, Stanford (2006) demands the realist to provide a criterion for selective confirmation that is historically reliable, in the sense that it "could have been in the past", and prospectively applicable, meaning to say that "it can apply in advance" by future scientists provided the criterion be reliable (p. 168). Otherwise, the realist would face the problems of trivial convergence and question-begging.

Therefore, Psillos and Stanford are committed to the same central idea, namely, that the criterion for selective confirmation must be historically adequate. This means that the criterion by which philosophers assess what counts as the central core of past theories is not a criterion of the present but that of the pertinent past.

4.3.3 Application

Stanford criticised Psillos' historical account of caloric by arguing that it is not historically defensible. Yet, he did so for reasons that Psillos would himself concede. Although Stanford celebrates that Psillos' historical method to approach selective confirmation is diachronical in character, he objects that Psillos does not consider all the relevant textual evidence in the appropriate historical context. Stanford's line of historical criticism uses further textual evidence in order to show that the material Psillos offers is either incomplete or does not support his thesis that "scientists' own judgments of selective confirmation have been historically reliable" (Stanford 2006, p. 174). Stanford's (2006) argument may be read as a dilemma: the textual evidence from historical actors Psillos appeals to either support his historical account or not. If it does, then Psillos cherry-picks textual evidence by doing a "highly selective reading of the historical record" (p. 174). If it does not, then the criterion for selective confirmation is not based upon past scientific judgments whatsoever.

First of all, Stanford recognises the authenticity of the passages from Black and Lavoisier that Psillos cited as supporting the claim that caloric was not taken to be epistemically central for the empirical success of the caloric theory. With these passages, Psillos tries to convince us that the epistemic attitude of both actors towards the cause of heat as a material substance was not of certainty but rather of agnosticism. That is, they did not take the belief in caloric as well-confirmed by the evidence and hence as true. Regarding Black, Psillos shows that Black realised that the caloric theory was more probable empirically than the rival, dynamical theory. However, this does not suffice to believe in the caloric theory because it was facing important problems, to wit: it cannot explain some experimental results, it provides no complete explanation of heat phenomena, and it introduces ad hoc assumptions (Psillos 1999, pp. 112-3). Black thus remained agnostic because the available empirical evidence was insufficient to decide conclusively between the caloric theory and the dynamical theory. Finally, Psillos (1999) pointed out that such an epistemic reservation was shared by the community of calorists, not just Black: "this attitude towards the hypothesis that the cause of heat is a material substance, which amounted to a suspension of judgement until better evidence came in, was not just Black's idiosyncratic behaviour" (p. 113).

Stanford disagrees with this interpretation of Black's pronouncements. Attending to the context of intellectual circumstances in which Black made these statements, Stanford argues that the textual evidence Psillos appeals to does not establish the thesis that Black took the caloric theory as not well-confirmed by the evidence. Stanford shows that Black's attitude was indeed idiosyncratic. For one thing, "Black advocates an official hostility toward all theories and theorizing in general" (Stanford 2006, p. 175). To bear out this contention, Stanford (2006) contextualises Black's "epistemology", indicating that "what Psillos misses, however, is that this restraint simply reflects Black's unusually strict but characteristically eighteenth-century Scottish commitment to Newtonian inductivism" (p. 175). If Black was committed to a Newtonian methodology widely accepted in Scotland, then it is understandable why Black thought that the available evidence made more probable the caloric theory, even though experimental method for Newtonians says nothing about the real nature of the cause of phenomena in this case, whether it is a material substance that causes heat. This Newtonian commitment is not meant to claim that the caloric theory was not well-supported by empirical evidence whatsoever compared to its rival. Indeed, Stanford (2006) argues that Black himself "rejected the dynamical theory in light of the evidence" (p. 176). Just as Psillos cites Rumford for illustrating the explanatory incapability of the caloric theory to account for the weight of caloric and the production of heat by friction between solid bodies, Stanford (2006) cites Black himself (and McKie and Heathcote's historical interpretation) to illustrate the explanatory deficits of the dynamical theory to accommodate his "discoveries concerning latent caloric" (p. 176). On this basis, Stanford concludes that Black's judgment of selective confirmation regarding the dynamical theory features his rejection of the belief in the cause of thermic phenomena in terms of molecular motion and his acceptance that "Cleghorn's material fluid account of heat [is] 'the most probable of any that I know'" (Black in Stanford 2006, p. 175).

Similarly, Stanford criticises Psillos' use of Lavoisier's pronouncements. According to Psillos (1999), Lavoisier and Laplace's consideration of calorimetry features their agnosticism about the caloric theory, since "Laplace and Lavoisier also suggested that the theory of experimental calorimetry was independent of the considerations concerning the cause of heat" (p. 113). That is, the techniques for measuring temperature and its results —especially the empirical generalisation that "the quantity of free heat always remains the same in simple mixtures of bodies" (Lavoisier and Laplace in Psillos 1999, p. 113)— were compatible with both caloric and dynamical theories.

Against this account, Stanford lavishes attention on the appropriate contextualization of the passage Psillos cited. Attending to the context of production and transmission of *Mémoire sur la Chaleur* according to its contents and potential audience, Stanford stresses actors' strategies of persuasion to understand why Lavoisier and Laplace opened *Mémoire* by talking about the consistency of calorimetry with any theoretical explanation of heat: "Since these new calorimetric methods really were compatible with both the material and dynamical theories of heat, it is unsurprising that Lavoisier and Laplace address their new techniques to the widest possible audience of their interested contemporaries" (Stanford 2006, p. 176; cf. Chang 2003, p. 910). That is, the passage Psillos uses is about the virtues of calorimetry but not about the virtues of the caloric theory, which they nonetheless embrace systematically in other parts of the book: "the explanations of specific phenomena offered later in the joint *Mémoire* itself are indeed committed to the view that heat is a material substance" (Stanford 2006, p. 176).

Moreover, Stanford (2006, pp. 176-179) provides further textual evidence from Lavoisier's texts, in which his epistemic attitude towards the caloric theory is one of confidence as long as such a theory was capable of explaining several phenomena and that it was differentially confirmed. Complementing with historical scholarship, Stanford shows that for Lavoisier the caloric explains chemical combination (*Mémoire* sur la Chaleur), it is well-supported by the evidence (Mémoires de Chimie and Donovan 1993), it confirms the oxygen theory rather than the phlogiston theory (Guerlac 1976) and Morris 1972), and it is the cause of most of thermic phenomena (Traité de Chimie). On these grounds, Stanford's (2006) general conclusion from his historical criticism of Psillos' narrative is that "I do not see how the textual evidence can be reconciled with Psillos' claim that 'scientists of this period were not committed to the truth of the hypothesis that the cause of heat was a material substance' (1999 119)" (p. 179). Accordingly, Psillos cannot appeal to this historical case to support his selective realism, because the caloric episode shows that past scientists' judgments were not reliable and are not therefore prospectively applicable. The historical evidence concerning this episode does not support Psillos' realist approach.

Stanford has done just what ADEQUACY demands in the context of historical criticism, namely, taking into account and critically interpreting all the relevant textual material in the appropriate historical context. Of course, Psillos could have replied to Stanford's historical criticism, showing that Stanford's contextualization of historical actors' public pronouncements about the epistemic status of the caloric theory does not work. For instance, Psillos could point to the existence of crucial pieces of (not yet considered) textual evidence in which Lavoisier explicitly asserts to believe in the caloric as a mere dispensable, hypothetical entity. Such pieces of evidence could be unpublished manuscripts or letters of his private correspondence, and so forth. Whilst Psillos has indeed criticised Stanford's line of argument against explanatory scientific realism more generally, a "neutral reader" of this disagreement has no evidence that Psillos has included a response to Stanford's historical criticism against his account of the caloric case (e.g., Psillos 2009). Until this is done, Stanford's rendering of this historical case arguably remains superior. Therefore, I cannot see why we would not be entitled to lean in the direction of Stanford's position at this time.

Hitherto I have attempted to argue that there are good reasons for thinking that the historical evidence of the caloric episode supports one philosophical position rather than the rival. These reasons have to do with the role of historiographical criteria in assessing the historical adequacy of the competing reconstructions of this episode. Stanford's historical rendering is more historically adequate than Psillos' in light of these criteria. This permits to claim that the strategy of selective confirmation as a philosophical position is not better supported by the historical evidence of this particular episode. In consequence, the selective realism debate is ultimately resolvable in historical terms; its resolution is facilitated by historiographical criteria working to reconstruct the caloric episode that are not only independent of the philosophy of science, but also commonly accepted and applied by the two competing sides of the disagreement.

Put things so, I have provided good reasons to counter step 6 of the *pluralism* argument, in which Chakravarty's characterisation of the selective realism debate is grounded. It follows that historical pluralism poses no threat to Stanford's *defensive* proof for the historical challenge. Stanford's reasoning is dialectically effective.

4.4 Manipulation and the new inductive base

I now turn to examine the extent to which manipulation of history jeopardises Stanford's *direct proof* for his historical challenge. The direct proof is based upon NIB, drawing the problem of unconceived alternatives as a conclusion.

As no rival, realist reconstructions to Stanford's historical study of 19th-century theories of generation and inheritance are available yet, the relevant issue is whether Stanford's direct proof is ultimately self-serving —i.e., whether Stanford has manipulated the historical material for his historical reconstruction to fit the problem of unconceived alternatives. If the answer to this question is affirmative, then this will force Stanford to admit that NIB fails to confirm the very problem of unconceived alternatives.

My plan is simple. After clarifying how Stanford's work squares with the philosophical approach to history, I move on to evaluate whether NIB involves objectionable forms of abstraction and idealisation, thereby violating historiographical standards of historical adequacy.

4.4.1 Stanford's historical analysis

The strategy of historical ostension arguably exhibits the three salient aspects of the philosophical approach to history (\$1.3), to wit: the historical case study is not the source of the philosophical claim, the philosophical claim is supported by and explains the historical case itself, and the philosophical claim is the criterion for selection and interpretation of the historical material.

Notice that Stanford uses NIB as evidence that *supports* the problem of unconceived alternatives, but he comes not to *formulate* this philosophical claim from NIB. In the first two chapters of his book, Stanford (2006) fleshes out the problem of unconceived alternatives in the appropriate philosophical context, characterising it as a new version of the underdetermination predicament that rebuts ESR. Once Stanford formulates the problem this way, in the next chapters he draws upon the historical cases involved in NIB to show that historical actors were facing that same problem.

Most importantly, NIB is theory-dependent vis-à-vis the central philosophical claims of Stanford's argument. This aspect can be seen by using tools of both quantitative and qualitative textual analysis. I have analysed the three chapters in which Stanford (2006) presents his historical study of inheritance theories. These pages contain Stanford's historical statements where he interprets historical actors' textual material, and the philosophical conclusions he draws upon his historical interpretation.

My quantitative analysis measures the extent to which Stanford's historical study is laden with philosophical claims. I have employed *Cratilo*, a computational lexicography tool that outputs statistical values from the linguistic occurrence of key philosophical terms in Stanford's historical reconstruction. These philosophical concepts have to do with philosophical commitments about eliminative inference (as the corresponding form of historical actors' reasoning) and the problem of unconceived alternatives (as the underdetermination predicament that historical actors are confronting). I have obtained data that show that Stanford's use of philosophical concepts is not just explicit, but also frequent to some important degree. This strongly suggests that philosophical commitments are systematically employed in writing the historical cases involved in NIB (see Table 1 in Appendix A).

Besides quantitative data, my analysis of the content of Stanford's historical study provides qualitative information about how he employs his philosophical claims as methodological resources in terms of selectivity, narrative-writing, and theory-ladenness ($\S1.2.1$). Apart from revealing a systematic use of philosophical terminology, my qualitative analysis shows that the problem of unconceived alternatives is a theoretical assumption that is entirely central to Stanford's historical reconstruction.

His selective choices concern the philosophical significance of the subject matter, the periodisation and localisation of the episode, and the explanatory variables according to the historical problem at stake. In brief, Stanford's study identifies a long-term tradition of biological thought from the end of the 18th century to the later 19th century in Europe. This biological tradition was concerned with theorising about the broad issue of "generation", which covered specific phenomena such as inheritance, reproduc-

tion, development, growth, and repair. Stanford picks out two scientific programmes within this tradition. The first one is the teleo-mechanist programme, which was a scientifically serious vitalist approach in German biology. This explained embryological and physiological phenomena in teleological terms by postulating "formative vital forces" that were responsible for directing organic processes (Stanford 2006, pp. 53-59). The second programme is represented by the later contribution of Darwin, Galton, and Weismann, who as the "seminal thinkers" of this episode were engaged "to develop a material, particulate account of generation and inheritance" (Stanford 2006, p. 60). Stanford focuses his analysis on the materialistic and mechanistic theories developed by these authors, whose work covered most of the 19th century.

Presumably, the centrality of these three actors relies upon the innovative character of their respective accounts. Darwin proposed the pangenesis theory partly because he attempted to explain the source of variation on which natural selection operates. According to pangenesis, parents are similar to offspring because children are made of gemmules —i.e., material pieces of their parents being causally transmitted through generation. Meanwhile, Galton conceived the stirp theory partly motivated by the results obtained from the transfusion experiments. Unlike pangenesis, the stirp theory presents a common-cause mechanism, according to which parents and children are made of the same germinal material being passed through generation. Finally, Weismann developed his germ-plasm theory to account for ontogenetic differentiation and cellular control based on the hypothesis of germinal specificity.

Both the topic and the corresponding periodisation were selected in terms of their philosophical significance. Stanford (2006, p. 52) believes that this historical episode in biological sciences reinforces the strength of the historical challenge. Notice that scientific realists also want to be so with respect to biology, but CIB is nonetheless concerned with cases from the history of physical sciences exclusively. Some defences of scientific realism against the pessimistic induction have therefore focused on "novel prediction" as a criterion for empirical success, but it is far from obvious how biological theories can fulfil such a criterion. Thus, NIB will provide additional evidential support to the historical challenge provided this historical evidence favours the problem of unconceived alternatives. Furthermore, NIB is relevant to bear Stanford's *direct proof* out because the historical cases belong to the same "paradigm" of contemporary biology. Stanford notes that earlier mechanical accounts of generation and inheritance are continuous to our current theorising in terms of metaphysical, methodological, and evidential commitments (Stanford 2006, p. 60). As such, Darwin, Galton, and Weismann shared the same "disciplinary matrix" as us. The selection of the explanatory variables also relies upon Stanford's philosophical interests. A couple of questions are guiding his historical study:

- (a) Did past scientists reason according to a form of eliminative inference, thus embracing the theory they proposed as the only serious explanation of the inheritance phenomena?
- (b) Did past scientists fail to conceive of serious theoretical alternative possibilities to those they confidently embraced on eliminative grounds?

Having this two-fold historical problem in mind, Stanford proceeds to critically examine historical sources, not only selecting the textual evidence that crucially suggests an affirmative answer to questions (a)-(b), but also using the philosophical vocabulary of "empirical evidence", "eliminative inference" and "serious theoretical alternatives/possibilities" to describe and explain actors' thoughts and intentions. In other words, the problem of unconceived alternatives is guiding Stanford's historical analysis; this philosophical claim provides the terminology to account for the episode and hence solve the two-fold historical problem.

These explanatory variables are also crucial concerning the structure of Stanford's historical narrative. He frames the span of the episode according to the following points:

- (i) Focusing on a "hard-case" from biological science, Stanford characterises the theoretical account each historical actor developed and defended to explain inheritance phenomena, offering a narrative that describes a process of successive replacement of one explanation by the other. In doing so, Stanford reconstructs the process of this theorising by focusing on three actors' seminal work.
- (ii) Without hindsight, Stanford goes on to establish a "historical thesis" concerning eliminative inference. As historical actors' pronouncements themselves suggest, each actor took his own hypothesis as providing the only conceivable explanation of phenomena given the evidence available at the time.
- (iii) With hindsight, Stanford finally establishes a "historical thesis" concerning the problem of unconceived alternatives. Historical actors were facing this epistemic predicament simply because they were unable to consider successor alternative theoretical explanations of the same phenomena, which would have been perfectly conceivable and acceptable as serious candidates in light of the available evidence at the time. In doing so, Stanford answers question (b) affirmatively.

Whilst Stanford makes points (i)-(ii) by directly appealing to textual evidence from historical actors and the relevant historical scholarship, it arguably seems that he infers the point (iii) retrospectively from the fact that successor alternative explanations were a crucial unconceived alternative. In this way, his historical reconstruction depicts the successor theory as remaining unconceived by the earlier actor. Although the successor theory was well-supported by the evidence available at the time, and only later considered and accepted by the scientific community, that same theory would have perfectly been conceivable and acceptable by the earlier actor as a serious contender.

Under this narrative structure, Stanford applies the problem of unconceived alternatives to interpret the historical episode. This thesis enables him to explain the episode as the intellectual efforts to know the "fundamental mechanism of inheritance" as facing the epistemic predicament of current transient underdetermination. He thus begins with Darwin's pangenesis, then considers Galton's stirp as a live alternative to Darwin's pangenesis, and finally introduces Weismann's germ-plasm as the latest possibility that replaced Darwin's and Galton's accounts. Stanford goes on to show that Darwin took pangenesis as the only conceivable explanation of phenomena even though he failed to conceive of Galton's central idea of common-cause mechanisms of inheritance. Likewise, Galton believed in the stirp as the best explanation of inheritance mechanism, despite being unable to conceive of a direct and contextualist account of heredity similarity. Finally, whilst Weismann took the idea of germinal specificity as the only possible explanation of the mechanism being responsible for genetic and ontogenetic processes, his account remained invariantist just like Galton's, and did not characterise the heredity material as an expendable resource. In short, the historical thesis concerning the problem of unconceived alternatives explains why these actors were unable to anticipate some central assumptions underlying modern genetics and ontogeny.

Not surprisingly, Stanford applies the philosophical terminology associated with eliminative inference and the problem of unconceived alternatives to write the historical narrative. His historical study is itself theory-laden with explicit philosophical commitments. For instance, Stanford (2006) characterises Darwin's reasoning underwriting his confidence in pangenesis as follows:

Darwin himself conceived of the support for his hypothesis as eliminative in character: the central virtue he claims for pangenesis is that it alone offers a "positive" or "distinct" idea capable of explaining and unifying a wide variety of the phenomena of heredity and generation "which at present stand absolutely isolated." Furthermore, Darwin here reports that this eliminative foundation was sufficient to lead him to "fully believe" in the literal truth of at least the theory's central claim that each cell does indeed throw off gemmules destined to develop into cells like the one from which they arose. (p. 65; my emphasis)

Once Darwin's pronouncements presumably indicate that pangenesis was taken to be the only conceivable theoretical explanation of heredity similarity in terms of a causal chain between parents and offspring, Stanford (2006) infers the lesson of the problem of unconceived alternatives:

Instead the most natural conclusion to draw from the historical evidence is that Darwin simply failed to conceive of or consider the entire class of theoretical alternatives to pangenesis picked out by this idea, notwithstanding the fact that it offered an equally promising strategy for explaining what he took to be the central phenomena of inheritance and generation. (p. 75; my emphasis)

A similar role for the problem of unconceived alternatives is found in Stanford's rendering of Galton's stirp theory. He lavishes special attention on Galton's four conditions for any theory of inheritance, namely that organic units of bodies originate in separate germs, that "the stirp contains a host of germs", "that the undeveloped germs retain their vitality", and that mutual affinities and repulsions among germs bring about bodies' organisation (Galton in Stanford 2006, p. 88). These conditions ultimately explain Galton's confidence in his maturational and invariant conception of inheritance in terms of eliminative inference, since he "does not propose this commoncause mechanism for heredity in a hypothetical or tentative way" (Stanford 2006, p. 87). Galton instead believed that the stirp theory meets the four conditions as "necessary consequences" that make it the only possible explanation. The first and four conditions committed him to a "maturational" conception of heredity, thus making him unable to consider a "directive" alternative: "Galton's confidence in the necessity of his first and fourth consequences seems rooted in his failure to conceive of even the possibility of any directive alternative to the maturational conception of particulate heredity he shared with Darwin" (Stanford 2006, p. 89). And the second a third conditions Galton postulated show "his failure to consider any alternative to what we might call an invariant conception of heredity" —e.g., a "contextualist conception of heredity" (Stanford 2006, p. 89).

Regarding Galton and the problem of unconceived alternatives, Stanford (2006) thus notes:

[H]e never conceived of the possibility of directive or contextual accounts of particulate inheritance, or indeed of any alternatives to the maturational and invariant aspects of his own conception, despite the fact that the phenomena to which he appealed supported a directive and/or contextual version of his own theory equally well [...]. [I]t seems we must conclude that just as Darwin failed to conceive of the very possibility of any common-cause mechanism for inheritance, after surmounting this conceptual obstacle Galton failed in turn to conceive of any alternatives to the maturational and invariant aspects of his own account of particulate inheritance. (p. 100; my emphasis)

As far as Weismann's germ-plasm theory is concerned, Stanford stresses the explanatory indispensability of germinal specificity to understanding cellular control for ontogenetic development and differentiation, whereby the germ-plasm was conceived of as a productive rather than as expendable material that can be disintegrated and affected by its local environment:

Weismann believes not only that the progressive disintegration of the germplasm into diverse constituent elements is in fact the process by which germinal specificity is achieved, but also that this is the only possible mechanism by which the germ-plasm could control the cell from within the nucleus to produce the kind of cellular differentiation actually observed over the course of ontogeny. (Stanford 2006, p. 115; my emphasis)

Accordingly, Stanford (2006) infers that the problem of unconceived alternatives also affects Weismann's epistemic situation:

And these features of Weismann's account and the arguments he makes for them illustrate the important respects in which *he himself remains unable to conceive of any alternative to an invariant conception of heredity, despite the clear progress he was able to make over Galton's imaginative imprisonment by this conception.* (p. 126; my emphasis)

To sum up: Stanford's reconstruction of these historical cases clearly depends upon philosophical theorising relating to the problem of unconceived alternatives. This is the criterion Stanford employs to select and interpret the corresponding textual material, as well as the philosophical claim being supported by such a historical study.

4.4.2 Stanford's abstraction and idealisation

Having characterised Stanford's historical analysis in some detail, I turn now to examine the extent to which the *manipulation argument* afflicts NIB in the sense that Stanford's historical study would involve objectionable forms of abstraction and idealisation (§1.3.1).

Regarding *abstraction*, Stanford's analysis noticeably focuses on the problem of unconceived alternatives across the length of his historical study. Also, his reconstruction amounts to *intellectual history*, which is drawn upon a big narrative depicting a succession of theories proposed by individual scientists. Thus, Stanford is abstracting from a more complex historical situation, since his historical narrative involves few actors as central figures and lavishes attention on the conceptual products of their minds. For instance, Stanford (2006) disregards the role of practices and material culture relating to the experiments he considers in some detail —e.g., Galton's transfusion experiments (pp. 80-6) and Driesch's fertilised eggs experiments (pp. 105-135). He dismisses the cultural context in the production, evaluation, and transmission of the theoretical alternatives about the fundamental mechanism of inheritance he addresses. And he accounts for the epistemic connections of actors' scientific beliefs *regardless of* (*not because of*) the social organisation, values and interests of scientific communities in theorising about inheritance (Stanford 2006, pp. 85-6).

Curiously, Stanford (2006) himself affirms that the problem of unconceived alternatives has to do with "theorists" rather than with "theories" (p. 44), and that individual scientists face this epistemic predicament simply because it happens primarily at the level of scientific communities (p. 129). However, Stanford appeals exclusively to "intellectual" factors to support these two statements. He characterises theorists as "thinkers" in terms of their "cognitive capacities" and their outcomes. In reasoning on the basis of "eliminative inference" and considering the "empirical evidence" alone, scientific minds such as Darwin, Galton and Weismann failed to "conceive of" or "consider" serious "theoretical possibilities" due to a lack of "imagination". In this way, Stanford does not include actors' pronouncements about metaphysical, religious, and political commitments in his analysis of textual evidence, and establishes no causal connection between these pronouncements and the rich cultural and political contexts of England and Germany in the 19th century.

I am reluctant to see this as an absolutely objectionable form of abstraction, however. For one thing, the elements of the historical situation Stanford is leaving out are not required to make the philosophical point about the problem of unconceived alternatives, even if some of them could enrich the historical narrative and reinforce that philosophical thesis. The textual sources plausibly seem to support the claim that actors reasoned according to eliminative inference and were unable to exhaust the space of alternative explanations of the same evidence available at the time.

For another thing, it seems that Stanford's narrative is not leaving the *historical process* out. Here the problem of unconceived alternatives refers to a historical pattern in which scientists were devising explanations of biological inheritance for almost one century. For Stanford (2017), "historical record reveals [...] a robust pattern of theoretical succession in which such foundational theories are accepted on the strength of a given body of evidence, only to be ultimately superseded by alternatives that were also well-confirmed by that evidence but nonetheless simply remained unconceived at the time of the earlier theory's acceptance" (p. 213). And if Stanford's (2017) inductive projection works, this historical pattern also offers an "uniformitarian vision of the past, present, and future of scientific inquiry itself" (p. 218). Thus, Stanford does pay attention to the *temporal dimension* of science. The problem of unconceived alternatives can justifiably be seen as a claim about scientific change that explains the process of theoretical change and acceptance in a particular domain of biological enquiry. Furthermore, the strategy of historical ostension illustrates how historical evidence concerning scientific dynamics informs the scientific realism debate.

Whilst Stanford's selective strategies seem not to involve any objectionable form of abstraction, the issue concerning *idealisation* is slightly more complicated. It seems that Stanford *distorts* historical facts as he exaggerates the explanatory role of the problem of unconceived alternatives. His historical narrative retrospectively compares theories of inheritance from Darwin through Galton to Weismann to show that actors faced this epistemic predicament. He therefore poses this problem as explaining why actors accepted each theory as "the lone contender" to explain the phenomena of inheritance (Stanford 2006, p. 16). In taking the problem of unconceived alternatives to be the *relevant explanans* of actors' confidence in the truth of their respective theories, his account is based upon the following counterfactual claim: if actors had conceived of the later accepted alternative explanation they failed to think about, then actors would not have accepted the early explanation they confidently embraced in the first place. It therefore seems that Stanford commits a *distortion of emphasis* by isolating "a single philosophical or methodological point as the key to an adequate explanation" (Brooke 1981, p. 257) —which is the problem of unconceived alternatives.

Naturally, this emphasis on the philosophical point leads Stanford to exclude the occurrence of further factors that could also explain the epistemic status Darwin, Galton and Weismann attributed to their theories —factors which would have had a relevant

influence on their transient acceptance and inconceivability. Even if textual evidence plausibly suggests that actors could have conceived of the theoretical alternatives despite the fact they did not do so, and even if the benefits of hindsight show that they failed to conceive of these alternatives, Stanford appears to be reducing the reason for the acceptance and replacement of theories of inheritance to the problem of unconceived alternatives. It therefore seems that Stanford also commits a *differentiation failure* "by imposing a monolithic rational structure on a complex historical situation" (Brooke 1981, p. 252) —which corresponds to the historical pattern the problem of unconceived alternatives describes.

Notice, however, that Stanford prevents the problem of the *priority of internal history* from arising. Contrary to rational reconstructions, Stanford explains the "error" or "failure" of actors in exhausting the space of alternatives by appealing to *epistemic reasons*. Given his own intellectualist approach, Stanford (2006) indicates that such a failure stems from a "conceptual obstacle" (p. 100). The priority of internal history boils down to explaining "failure" in terms of "external" factors (e.g., Lakatos 1976; Laudan 1977; Newton-Smith 1981), but Stanford nonetheless explains "errors" by appealing to actors' epistemic reasons that are quite "internal" elements.

In defence of Stanford's distortion, it can be put forward that *prima facie*, enriching contextualisation needs not be a desideratum for the philosophical approach to history. This clearly differs from mainstream historiography of science in both aims and levels of historical representation. As a philosopher, Stanford aims to draw philosophical lessons from historical information. He is interested in telling something relevant about philosophical issues based upon what we know about history. For this purpose, he does not need to produce a rich, detailed and concrete representation of historical events that captures them in all their complexity with (approximate) accuracy. Rather, he focuses on those aspects of the historical episode that are salient and relevant to the philosophical point he wants to make.

What seems to be objectionable about philosophical distortion are those cases in which the distortion creates an inconsistency between the idealised account and the detailed historical reconstruction of the same episode. This typically occurs when the philosophical point is a clear *false assumption* that was introduced in the narrative, thereby creating a *tension between explanatory factors*. However, notice that if the problem of unconceived alternatives were capturing a *real historical pattern* —albeit by leaving other different factors out,— then this would be enough explanation for philosophical purposes, without curtailing the account's historical adequacy. Furthermore, Stanford opens the door for other explanatory factors to enrich the historical understanding of the episode in terms of the problem of unconceived alternatives. He points out curiously that "unless we find some reason to think that this pattern depends on idiosyncrasies of the personalities or period involved in this particular case [...], even this single series of historical episodes may go a considerable distance toward showing that we are in possession of a quite general challenge to scientific realism" (Stanford 2006, p. 47). This suggests that the problem of unconceived alternatives is quite compatible with other explanatory factors.

The general point is this: philosophers typically distort complex historical situations, but their representations are nonetheless admissible just in case they harmonise with rather than contradict a "historiographical" representation of the same episode that is much richer in detail. Put otherwise, the *minimalist idealisation* here needs not be *de-idealised* provided the philosophical point is not historically false. But idealisation could even be de-idealised to enrich the philosophical explanation itself, as long as the other explanatory factors being introduced are quite compatible with the philosophical point that was originally emphasised.

There is also a worry about *fictionalisation* (\$1.3.3), which has to do with how Stanford employs textual material. Regarding the *reliability of sources*, it is unfair to claim that Stanford did not appeal to primary textual evidence or that he disregarded the current historical scholarship. It is helpful here to examine Stanford's methodological pronouncements about his use of sources, and then confront these pronouncements with what one does find in his writings. Stanford (2006) makes it explicit which and how those sources were consulted:

Here and throughout I have tried to restrict my use of the secondary literature concerning this period in the history of science to classic discussions in the field whose central contentions still appear to be widely accepted, rather than to the unavoidably more contentious claims embodied in more recent historical scholarship. As will become clear in what follows, however, the direct evidence I adduce in support of the problem of unconceived alternatives in the work of Darwin, Galton, and Weismann is drawn almost exclusively from primary sources, rather than from this secondary literature. Of course, if more recent developments in the historical scholarship concerning this period undermine either my reading of the primary sources or the use I have made of them in trying to establish the general significance of the problem of unconceived alternatives itself, I trust that my colleagues in the history of science will set me straight. (pp. 75-6 n.1; my emphasis) On closer inspection, I have found that Stanford did critically use textual material, thereby confirming his pronouncements (see Table 2 in Appendix A). This material encompasses primary sources (i.e., unpublished texts of historical archives and published works of critical editions) and secondary ones (i.e., translation of published works and professional historical scholarship). Using original and multiple sources is what historians of science like I. B. Cohen (1974) expect to find in any philosophical use of history that makes historical statements. It follows that Stanford's work arguably relies upon reliable sources as far as NIB is concerned.

Even so, a further question remains: does Stanford decontextualise textual material by inserting it into the "philosophical context" of the problem of unconceived alternatives? Or better: does Stanford commit any objectionable form of anachronism in interpreting textual sources and give no account of the generation-process of such sources?

Regarding *anachronism*, it is obvious that Stanford's philosophical terminology does not belong to the category-system of historical actors. The problem of unconceived alternatives is not an actor's category, but Stanford nonetheless employs this philosophical claim to describe an actual historical pattern. This claim is therefore an analyst's category. Not all analyst's categories are invariably objectionable. For instance, whilst the concept of "Scientific Revolution" is not an actor's category, it is historically adequate in describing a process of transformation by which modern science originated. Whether the problem of unconceived alternatives is adequate for writing the history of the episode in question could be determined by seeing how historical sources support such a category in constructing a consistent narrative. That is, the category would be acceptable as long as it were creating no conflict between the category and the original context at stake.

In this respect, Stanford's (2006) analysis relies upon primary textual evidence as he indicates that "the direct evidence I adduce in support of the problem of unconceived alternatives in the work of Darwin, Galton, and Weismann is drawn almost exclusively from primary sources" (pp. 75-6 n.1). On these grounds, Stanford offers a consistent story of theorising about inheritance as being afflicted by this problem. Put otherwise, the problem of unconceived alternatives appears to be plausibly inferred from using historical sources correctly. In turn, this allows Stanford to write a narrative of how the historical episode came about (i.e., as an analyst's interpretation), even though the problem of unconceived alternatives is not explicitly identifiable in the historical situation (i.e., as an actors' category). For this reason, it seems that Stanford's historical study is not committing any anachronistic sin. His purpose is not to impose

the philosophical point, but rather illuminate a key aspect of the historical situation on its basis.

Now, the worry about *sources decontextualisation* rather complains that Stanford provides no account of the generation-process of Darwin's, Galton's and Weismann's writings when he extracts some key passages and cites them as evidence of his philosophical point. Textual evidence would therefore be speaking about the problem of unconceived alternatives in Stanford's contrived context rather than in the original historical one. I want to discuss this objection by examining the case study of Galton in some detail.

As seen before, Stanford explains Galton's belief in stirp theory in terms of the problem of unconceived alternatives: he only employs textual sources purported to show that Galton considered this "maturational" and "invariant" theory as the only explanation (given the empirical evidence that was available for him), *because* Galton was unable to conceive of both "directive" and "contextualist" theoretical alternatives that also were well-confirmed. This amounts to an intellectualist historical account of Galton's scientific reasoning, examining textual sources to find the actor's epistemic reasons as formulated in explicit pronouncements. This approach, however, says nothing about the link between the actor's pronouncements and the surrounding circumstances that could elucidate the generation-process of his writings. These circumstances typically include social causes such as shared goals and interests, which can be relative to either scientists' narrow circle of specialists or the wider societies they inhabit.

Cowan (1977) establishes such a link between Galton's writings and the context. She contextualises Galton's ideas on heredity as being influenced primarily by socialpolitical motivations. Cowan focuses his analysis on Galton's (1865) *Hereditary Talent* and Character, an article in which he defends the idea of mental heredity —i.e., that human's mental qualities and capacities are transmitted from one generation to the next. The eugenic movement relied upon this idea and embraced a scientific theory that conceives of human talents as *innate* from heredity rather than acquired by the influence of environmental causes. On this basis, eugenics supported a social policy concerned with the improvement of race, aiming "to promote the fertility of the better types which the nation contains, whilst diminishing the birth rate amongst those which are inferior" (Darwin 1926, p. 138).

For Cowan, eugenics led Galton to develop his scientific ideas. She argues that Galton "had convinced himself of the validity of mental heredity, not because he thought it was a solution to a great scientific problem, but because he was fascinated by the social programs that could be built around it" (Cowan 1977, p. 140). Also, Galton's

commitments to eugenics as an ideology elucidate his stirp theory, since "the idea of continuity of germ plasm was absolutely essential to Galton's eugenic scheme" (Cowan 1977, p. 142). To underpin this rendering, Cowan not only identifies Galton's eugenic pronouncements, but also inserts Galton's writings into such ideological context. She thus indicates that "when read as a political rather than a scientific tract 'Hereditary Talent and Character' makes more sense" (Cowan 1977, p. 146), noting that it was published in a place that "was a magazine of general cultural and political interest, representing a fairly conservative constituency in 1865" (p. 146). In this way, Cowan (1977) accounts for the generation-process of Galton's writings, thereby drawing the strong conclusion that for Galton the "achievement of a eugenic society was a more important goal than the achievement of scientific truth" (p. 147).

MacKenzie (1981) also contextualises Galton's pronouncements by appealing to social context. He uses the tools of SSK to argue that "the eugenic objectives of Galton, Pearson and Fisher were closely connected to their science" (p. 11). This historical study is recognised above all by offering a *symmetric* analysis of the controversy between Pearson and Yule over statistical association measurement, explaining the disagreement as one involving different epistemic goals that "can be related to different attitudes to eugenics" (MacKenzie 1981, p. 13). Most importantly, MacKenzie establishes a causal connection between scientific knowledge (i.e., statistical ideas and related eugenic beliefs) and the social context (i.e., the middle-class of professionals in 19th-century British society). This connection suggests that "eugenics did not merely motivate their statistical work but affected its content" to the effect that it was "partially determined by eugenic objectives" (MacKenzie 1981, p. 12). MacKenzie (1981, p. 68) arrives at that same conclusion concerning Galton. He explains, for instance, that Galton's epistemic goal was to understand "the statistical dependence of two variables" in terms of reversion and correlation, that is, "the effect of the [mental] characteristics of one generation on that of the next" (MacKenzie 1981, p. 71), which was central to eugenics theory. Accordingly, since "Galton's eugenics reflected the social interests of the group of elite professionals to which he belonged" (MacKenzie 1981, p. 72), such interests ultimately explain why he developed his statistical ideas in the way he did.

Unlike these two social histories, Stanford suggests that his intellectualist account of Galton's views on heredity is sufficient to explain his scientific reasoning, thereby making the appeal to social factors irrelevant or redundant. Stanford indicates this point especially with respect to the disagreement between Darwin's pangenesis and Galton's transfusion experiments on rabbits. He contends that even if this experimental result is understood as a case of holist underdetermination, the actor's decisions on which auxiliary hypotheses to change were by no means determined by social causes:

Not even Darwin's famous network of powerful scientific allies seem to have regarded his suggestion in response to the transfusion experiments that gemmules might be diffused independently of the blood vessels as the least bit convincing, and even Galton (whose personal loyalties, not to mention his clearest route to authority, advancement, and social power in the context of nineteenth-century British science, clearly lay with Darwin himself) went on to consider instead the suggestion that the gemmules of pangenesis might be only *temporary* residents of the blood (see LLL II 161). (Stanford 2006, p. 85)

In other words, Darwin was unable to protect the pangenesis theory from empirical refutation, even though "it is hard to imagine either a figure enjoying greater authority, power, or social recourses in late nineteenth-century biological science than Darwin" Stanford 2006, p. 86). In a similar vein, Bulmer (1999) argues that appealing to the actor's judgements exclusively is appropriate when it comes to explaining why Galton rejected the hypothesis that gemmules were transported through blood; this rejection "was based on his failure to demonstrate the existence of these elements in the blood, and on his skepticism about the importance of the inheritance of acquired character" (p. 279). Accordingly, Bulmer (1999) complains that social causes are dispensable to see why Galton dismissed the role of the inheritance of acquired characters in 1865:

Ruth Cowan has argued that Galton's rejection of the inheritance of acquired characters was sociopolitical, being determined by his eugenic convictions, but a simpler explanation seems equally convincing. The passage quoted above is well-argued and suggests that Galton had thought carefully about the evidence for the inheritance of acquired characters, and concluded on internal grounds that it was weak. (p. 269)

Thus, it suffices to cite the actor's epistemic reasons to understand their reasoning. This justifies extracting parts of textual material to underpin a historical argument without shedding light on why actors wrote what they wrote. If this reading is correct, Stanford does not provide any contextualisation of the sources he employs and he is therefore vulnerable to the charge of having potentially falsified textual material.

I think that Stanford could escape from this accusation, however. This comes from realising from the outset that "understanding what the actors themselves thought is valuable, but it is not necessarily the best way of understanding why they thought or did what they did" (Chang 2021b, p. 101). More specifically, it happens that actor's epistemic reasons are themselves insufficient explanatory variables when they underdetermine rival scientific positions (Bloor 2011a, pp. 403-4) or when they are unable to elucidate contingent courses of action —i.e., why actors used such epistemic reasons in the specific way they did (Bloor 1984).

Besides this general point, social factors are neither irrelevant nor even dispensable for Stanford's analysis, since they arguably reinforce his case for the problem of unconceived alternatives. Elsewhere, Stanford (2019) himself appeals to social causes that explain the specific ways in which contemporary science is even more vulnerable to this problem, such as "the professionalization of science in the middle decades of the nineteenth century, the shift to state support of academic science through peer-reviewed proposals for particular research projects following World War II, and the ongoing acceleration and expansion of so-called 'Big Science'" (p. 3917). It is nonetheless curious that Stanford (2006) does not recognise that social histories of Galton's ideas on heredity can equally support his philosophical account of the same historical case. Fortunately, Stanford (2009) notes that in constructing NIB he avoided any "exploration of the reality and consequences of our repeated failures to conceive of the full range of well-confirmed theoretical alternatives to any particular account of the (presumably heterogeneous and untidy) sources of those failures" (p. 382). Such an account, which would be provided by social history of science, amounts to an "empirical exploration of the various dynamical processes that help explain how and why unconceived alternatives remain unconceived by particular (human!) scientists and scientific communities" (Stanford 2009, p. 381). On these grounds, he naturally claims that social history would "complement my aims in *Exceeding Our Grasp*, rather than compete with them" (Stanford 2009, p. 381).

Thus, any "philosophical" contextualisation of textual sources is deemed objectionable just in case it creates a *tension between contexts*, that is, between the *actor's perspective* (i.e., scientists' epistemic reasons) and the *analyst's perspective* (i.e., those social causes the historian connects to epistemic reasons). To my mind, Stanford's and Bulmer's accounts of Galton's work on heredity do not conflict with Cowan's and MacKenzie's reconstructions —at least regarding the problem of unconceived alternatives.

My foregoing discussion of Stanford's abstraction and idealisations makes it plausible to claim that these forms of manipulating historical material are not dangerous, because they do not generate unreliable historical data. If so, Stanford's reasoning would not be self-serving and NIB would therefore confirm the problem of unconceived alternatives. Of course, this is only a conditional claim. The reasons I have offered for the antecedent of this conditional statement are by no means conclusive. However, they confidently protect Stanford's *direct proof* from the problem of *manipulation of history* unless given further, stronger reasons for the contrary.

4.5 Conclusion

Here is a summary of the chapter. I examined the strategy of historical ostension in Stanford's two-fold historical challenge to explanationist scientific realism. The *indirect* proof seeks to disconfirm selective realism based upon CIB, whilst the *direct proof* rests on NIB to confirm the problem of unconceived alternatives itself. I explored whether *historical pluralism* undermines the former and *manipulation of history* endangers the latter.

Firstly, I blocked the challenge of historical pluralism, concluding categorically that CIB disconfirms selective realism. I not only rejected Chakravartty's verdict of the selective realism debate concerning the caloric episode, but also cast doubt on the sceptical generalisation that historiographical standards "are too weak to settle all historiographical conflicts" and therefore that "historical case studies typically cannot settle philosophical conflicts" (Kinzel 2015, p. 55). Since there exists at least one case of resolvable philosophical disagreement based upon historiographical standards, one is justifiably prompted to expect that historical criticism can resolve many other philosophical debates that are apparently intractable.

Secondly, I mitigated the threat of manipulating history, concluding conditionally that NIB confirms the problem of unconceived alternatives if Stanford's abstraction and idealisations turn out not to be objectionable. I gave three reasons. First, Stanford's historical case study is abstract, but it is historically adequate in light of textual evidence and rescues the historical context and process. Second, Stanford's case study distorts complex historical situations, but it does not bring about any tension between his distorted reconstruction and more detailed historical case studies of the same episode. Third and finally, Stanford uses reliable sources, but introduces some anachronisms without contextualising the textual material. Despite this, Stanford's categories of analysis allow him to write a consistent narrative that is well-supported by textual sources, where the intellectualism underwriting the narrative creates no conflict between the original historical context and the philosophical one in which Stanford inserts the actors' pronouncements. If Stanford's strategy of historical ostension neither is undermined by historical pluralism nor manipulation of history, then this instance of evidential reasoning in HPS escapes from the *circularity argument*. But the story does not end up here. The sceptic has the *unsuitability argument* as an ace up her sleeve, contending that historical evidence does not support philosophical claims because history and philosophy are incompatible. In the next chapter, I will draw upon Chang's work to tackle this second sceptical argument.

Chapter 5

Integrated HPS

Abstract

This chapter aims to rebut the philosophical unsuitability of history. I discuss the conflict argument purported to support what I called metaphysical unsuitability. According to this argument, history and philosophy are essentially opposed to one another, since philosophers embrace scientific absolutism and historians instead assume scientific non-absolutism. I resolve this inherent tension of integrated HPS based upon Chang's metapragmatism — i.e., using pragmatist epistemology to validate philosophical theorising itself. My solution consists in characterising the inherent tension as a form of operational incoherence, the solution of which requires adjusting the philosophy of science upon which integrated HPS is conducted. After arguing that standard scientific realism involves absolutism and therefore creates the operational incoherence of integrated HPS, I claim that activist realism involves non-absolutism, thus making integrated HPS an operationally coherent practice. It follows that whilst scientific realists must admit that historical studies are philosophically useless, activist realists make the history of science suitable for philosophical work. I close this chapter by critically comparing Chang's integrated HPS with Kuhn's, Hacking's and Feyerabend's approaches to HPS.

5.1 Introduction

My purpose in this chapter is to resolve the inherent tension of integrated HPS that sanctions the *unsuitability argument*. The conflict between history and philosophy is worth solving for a simple reason: the use of historical case studies to establish philosophical claims about science is precluded if the history of science is unsuitable for philosophical theorising due to the incompatibility between the two disciplines. Alas, the inherent tension of integrated HPS would also undermine the very possibility of understanding science by bringing together history and philosophy.

I argue that the *unsuitability argument* does not jeopardise evidential reasoning in HPS. My proposed response to this argument focuses on rebutting the conflict argument

that bolsters *metaphysical unsuitability*, which states that there is an inherent tension of integrated HPS because history and philosophy are intrinsically incompatible. To do this, I draw upon Chang's work as a case study. I resolve the inherent tension of integrated HPS based upon metapragmatism. Metapragmatism is nothing but pragmatist epistemology as applied to validate philosophical theorising itself. I argue against standard scientific realism and for activist realism using this epistemology. My argument appeals to the notion of operational coherence to evaluate these two competing philosophical theories in the domain of integrated HPS. According to this criterion, we assess the acceptability of *theories* by examining the extent to which they facilitate epistemic coherent activities in relevant empirical domains.

When it comes to the domain of science, Chang (2022) has already argued that standard realism is a theory that makes scientific practice operationally incoherent, since it attributes to science an unrealistic aim —i.e., attaining the absolute truth about ultimate reality. On the contrary, activist realism is a theory that converts scientific research into an operationally coherent enterprise that seeks to produce knowledge for learning truths about realities, which is a realistic aim to achieve.

Here I use pragmatist epistemology in a similar way, showing how standard realism leads to the same undesirable consequence in the domain of integrated HPS. I claim that the reliance upon this philosophical theory makes integrated HPS operationally incoherent —i.e., it does not facilitate operationally coherent activities in pursuing the aim of *understanding science by bringing together history and philosophy*. However, I also affirm that the reliance upon activist realism facilitates the operational coherence of integrated HPS —i.e., this theory does allow conducting "history as philosophy" in an operationally coherent way.

The chapter is structured as follows. In Section 5.2 I introduce Chang's pragmatist epistemology. I spell out the normative notion of operational coherence, showing how it provides three criteria for evaluating epistemic activities, concepts, and theories in all empirical domains. In section 5.3 I explain how to both characterise and solve the inherent tension of integrated HPS based upon pragmatist epistemology. I claim that the conflict between history and philosophy can justifiably be seen as a form of operational incoherence. Thus, a solution to this problem requires an iterative process of aim-oriented adjustment of integrated HPS, which consists in changing the philosophical theory being adopted. In the remainder of the chapter, I properly develop this pragmatist strategy. In section 5.4 I argue that the reliance upon standard realism creates the conflict between history and philosophy, thus making integrated HPS an operationally incoherent practice. In section 5.5 I show how the reliance upon activist realism resolves the conflict that standard realism brings about. In this way, I pragmatically invalidate standard realism whilst validating activist realism. I suggest that HPS-theorists must replace standard realism with activist realism if they aim to make integrated HPS operationally coherent. I therefore claim that activist realism entitles us to reject the *unsuitability argument* by removing the basis in which *metaphysical unsuitability* is rooted. Finally, in section 5.6 I outline Chang's formulation of integrated HPS, indicating some of its parallels with other conceptions of HPS that have even exerted an influence upon Chang's thought, namely, those of Kuhn, Hacking, and Feyerabend. I explore their approaches to HPS as their philosophical commitments to non-absolutism are concerned. This helps to elucidate the extent to which Chang's work can justifiably be seen as an up-to-date case of integration between history and philosophy, which ultimately succeeds in tackling the philosophical unsuitability of history.

5.2 Pragmatist epistemology

Chang develops a conception of knowledge as ability based upon the notion of "active knowledge". His "action-based view of knowledge" is primarily concerned with "knowledge in terms of what people do" (Chang 2022, p. 12). As far as science is concerned, the concept of active knowledge highlights the *practice* of science, which is analysed in terms of "systems of practice" together with their constituent "epistemic activities" that pursue inherent "knowledge-related aims". For Chang (2022), "an epistemic activity is a knowledge-related activity, aimed at acquiring, assessing or using knowledge. A system of practice is a network of activities that function coherently together" (p. 16). So what scientists do when they interact with the world is to conduct epistemic activities aiming at achieving some knowledge-related goals. Correspondingly, a set of epistemic activities constitutes a system of practice to the extent that scientists set them up in relation to corresponding desirable "system-level aims" (Chang 2022, p. 37).

On these grounds, Chang proposes the criterion of *operational coherence* to evaluate science as embedded in systems of practice. Operational coherence is the *quality* of active knowledge, which means that the *epistemic value* of epistemic activities and systems of practice consists in their being operationally coherent. There are two ways of understanding operational coherence. First, operational coherence is *aimoriented coordination*. This designates a *state* of epistemic activities in which they are coordinated in such a way that a particular aim would be achieved successfully. In

this case, operational coherence is a *property* of what people do in linking epistemic activities with corresponding aims, such that such activities lead epistemic agents to attain their aims. Chang (2022) points out that "the coherence of an activity is not some mysterious harmony between things in themselves, but it is a matter of how *we* bring together things and actions in order to achieve our aims" (pp. 45-6). Hence active knowledge involves not only coordinated epistemic activities vis-à-vis aims, but also and foremost epistemic agents who have desirable goals to achieve and can coordinate activities in the first place.

Second, operational coherence is *pragmatic understanding*. This is meant to say that a system of practice is operationally coherent provided that what people do to achieve an aim *makes sense* in order to succeed. So operational coherence is a *normative status* that either analysts or agents themselves impose on systems of practice in understanding how epistemic activities will work out. Here operational coherence is a value that people *attribute* to epistemic activities. Chang (2022) puts it this way:

Operational coherence is a hermeneutical notion, concerning a pragmatic kind of understanding. What is *operationally coherent* is what makes sense for us to *do*, and 'sense' here is framed by our aims [...]. The success of an activity is not *caused* by its coherence; rather, the coherence of an activity *consists* in doing what is sensible to do if one wants to succeed. Coherence is *design* for success, and that design is based on empirical learning: it makes *sense* to do what we think will succeed, and it doesn't make sense to do what we think is unlikely to succeed. Coherent activities are carefully designed so that they *would* work. (p. 44)

Both ways of understanding operational coherence are two sides of the same coin. The *state* of operational coherence corresponds to activities being well-designed to achieve an aim. The *status* of operational coherence is ascribed to activities that people understood as well-designed for success, so that it makes sense to pursue them in order to achieve something.

So construed, operational coherence provides a criterion for assessing the standing of active knowledge, thus determining whether the system of practice under evaluation has a positive epistemic status. This *epistemic* criterion can be formulated as follows:

EPISTEMIC COHERENCE: A system of practice is operationally coherent *iff* its constituent epistemic activities are aim-oriented coordinated and either analysts or epistemic agents themselves have a pragmatic understanding of this fact.

The idea is that a system of practice is *epistemically good* so long as its constituent epistemic activities feature operational coherence. Such epistemic criterion might thus be seen as primary, because operational coherence is directly related to epistemic activities and systems of practice.

Furthermore, operational coherence gives a pragmatist notion of reality and truth. Which entities are *real* and which propositions are *true* is something that ultimately depends upon using the corresponding concepts and propositions in epistemic activities that are operationally coherent.

Regarding reality, Chang (2022) claims that "an entity is real to the extent that there are operationally coherent activities that can be performed by relying significantly on its existence and its properties" (p. 121). This is a definition of "reality" in terms of necessary and sufficient conditions, as Chang (2022) puts it forward:

I am not claiming that the definition of "real" I am proposing here encompasses every existing usage of the world. Rather, I am proposing that the capacity to support coherent activities is what we *should* mean by "real", because I think it will be conducive to productive discourse, while being reasonably faithful to enough of the actual usage currently embedded in various practices. So what I am engaged in can be seen as a project of explication as conceived by Carnap, or an attempt at "conceptual engineering." (p. 122)

This pragmatist definition captures the conditions under which an entity is real, thus constituting the unique meaningful concept of reality. On these grounds, an *ontological* criterion to assess instances of success in conceptual reference is this:

ONTOLOGICAL COHERENCE: An entity is real *iff* the concept referring to this entity is indispensable for the operational coherence of the epistemic activities in which that concept is being used.

This criterion validates scientific concepts pragmatically. If using a particular concept is either dangerous or even dispensable for an epistemic activity to feature operational coherence, then scientists are entitled not to be realist about the existence of the entity which the concept refers to. Instead, if the concept being used is indispensable for the operational coherence of that same activity, then scientists are entitled to adopt a realist commitment to the entity in question.

As far as truth is concerned, Chang (2022) proposes the notion of "truth-byoperational-coherence", according to which "a proposition is true to the extent that there are operationally coherent activities that can be performed by relying on it" (p. 167). Unlike the pragmatic definition of reality, "the definition above gives a sufficient condition for truth, but not a necessary condition", since "giving a sufficient condition for truth makes for a concept that we can use" (Chang 2017, p. 114). Here Chang (2017) is adopting an *alethic pluralism* based upon a realisation that "not giving a necessary condition allows that there may be other ways in which the concept is meaningful" (p. 114). However, there is a sense in which truth-by-operational-coherence does provide sufficient and necessary conditions for truth, namely regarding what he calls *empirical (primary) truth*: "I do want to propose that truth-by-operational-coherence is what constitutes primary truth in empirical domains" (Chang 2022, p. 167).

This pragmatist definition of truth provides an *alethic* criterion for validating the truth value of propositions:

ALETHIC COHERENCE: A proposition p is true *if* p is indispensable for the operational coherence of the epistemic activities in which p is being used.

As for the specific validation of empirical propositions, the proper formulation of that same criterion is the following:

ALETHIC COHERENCE:* An empirical proposition p is primarily true *iff* p is indispensable for the operational coherence of the epistemic activities in which p is being used.

Truth is obviously a property of propositional knowledge. But as Chang (2022, p. 22) argues, propositional knowledge is grounded in and contributes to active knowledge. Propositions —like concepts— are employed in the context of epistemic activities, and the use of propositional knowledge can improve epistemic activities to the extent that indispensable propositions enhance operational coherence. Scientists are therefore entitled to believe in those propositions that play a necessary role within operationally coherent activities, although not to believe in propositions whose role is either dangerous or even dispensable for the operational coherence of those same activities.

So construed, pragmatist epistemology is a theory with general scope. This means that it "will concern itself with all practices relating to knowledge" (Chang 2022, p. 67), which includes both science and integrated HPS itself. As an epistemic practice producing (empirical) knowledge of science, integrated HPS can be figured out in terms

of active knowledge and evaluated according to operational coherence. This therefore allows the pragmatist to give a pragmatic diagnosis of, and a solution to, the inherent tension of integrated HPS itself.

5.3 A pragmatist diagnosis of metaphysical unsuitability

From a pragmatist perspective, the research field of "science studies" (broadly conceived) can be seen as a plurality of systems of practice. This plurality includes history of science and philosophy of science as separate systems, each of them as constituted by epistemic activities pursuing corresponding aims, and relying upon elements such as theoretical claims and methodological principles. Correspondingly, integrated HPS counts as a particular type of inter-system interaction in science studies, resulting from attempting to achieve the general aim of understanding science by doing "work that is both historical and philosophical at the same time" (&HPS website). Put things so, the epistemic status of integrated HPS will ultimately depend upon whether the epistemic activities pursuing such a general aim are operationally coherent —i.e., whether those activities exhibit aim-oriented coordination, and HPS-theorists have pragmatic understanding of this fact.

The pragmatist will take the inherent tension of integrated HPS as a reason to claim that this practice has no positive epistemic status. Given that the *conflict argument* (§2.2) leads to thinking that the general aim of integrated HPS is not feasible whatsoever, the pragmatist will conclude that this practice is operationally incoherent. That is to say, the realisation that history and philosophy are intrinsically opposed to one another entitles the pragmatist to conclude that it does not make sense for HPS-theorists to coordinate historical and philosophical activities in pursuing the aim of understanding science. Firstly, there cannot be coordination if there is an incompatibility between history and philosophy, so the epistemic activities of integrated HPS do not exhibit aim-oriented coordination. Secondly, the conflict between the two disciplines implies that those activities cannot be understood as conducive to success —i.e., there is no pragmatic understanding of how integrated HPS will ultimately work out. If this is correct, then the pragmatist will reformulate the *conflict argument* as follows:

The incoherence argument:

- 1. *Absolutism*. Philosophy is committed to essentialism, universalism, and theoreticism about science.
- 2. *Non-absolutism*. History is committed to contingentism, localism, and practicalism about science.
- 3. *Inherent tension*. These two sets of commitments conflict with one another. This conflict generates a defective conjunction between history and philosophy.
- 4. *Operational incoherence*. If there is a defective conjunction between history and philosophy, then HPS lacks aim-oriented coordination and HPS-theorists have no pragmatic understanding.
- 5. *Conclusion*. HPS lacks aim-oriented coordination and HPS-theorists have no pragmatic understanding. (Modus ponens 3, 4.)

Again, the point is that the history-philosophy incompatibility can arguably be seen as a certain type of operational incoherence. The aim of understanding science in both historical and philosophical terms cannot be accomplished unless bringing together history and philosophy. However, it is very hard to see how HPS-theorists can fit together the elements and aspects of both disciplines in order to successfully achieve such an aim. The inherent tension, then, turns out to be a defective conjunction of integrated HPS, whereby the work of doing "history as philosophy" is not well-designed to account for science. As long as integrated HPS lacks operational coherence, it has no positive epistemic status.

HPS-theorists will be upset with this negative verdict on the status of integrated HPS. Fortunately, I think that the *incoherence argument* above is not conclusive, since the operational incoherence this ascribes to integrated HPS does have a promising solution. To see this, I propose to follow a pragmatist strategy to handle this problem —which seems very plausible given the pragmatist characterisation of the problem itself.

The pragmatist strategy is based upon three steps. The first step is to figure out where the incoherence of HPS is coming from. It seems that the ultimate source of operational incoherence of a certain epistemic practice can be relative to the constitutive general aim (when this is simply unrealistic), but also to the epistemic activities themselves (when these are not well-designed to succeed), or to the theoretical assumptions upon which the research is conducted (when the epistemic practice is based upon mutually contradictory claims). Naturally, the proposed solution to the problem will be adjusting at least one of these three elements in integrated HPS —i.e., altering either the general aim, or some epistemic activities, or some theoretical claims.
It is not hard to find the source. The *incoherence argument* suggests that this emerges from the metaphysical commitments about science, which also have methodological implications in how integrated HPS is conducted. These commitments are mutually contradictory beliefs about the nature of science, to wit: absolutism on the philosophy side, and non-absolutism on the history one. Thus, it is the *reliance* upon conflicting *theoretical claims* that creates the operational incoherence of integrated HPS.

The second step for solving the problem is picking up the relevant theoretical claims to be revised. Notice that the *incoherence argument* is compelling so long as the first two premises are true —i.e., only when it is the case that philosophy of science involves absolutism and history of science does not. Considering this, the pragmatist can block the conclusion of the argument by denying at least one of the two premises: either that philosophy of science involves absolutism, or that history of science involves non-absolutism. This obviously opens these two possibilities to argue for: either philosophy of science does not entail absolutism, or history of science does entail absolutism. In both cases, the conflict between metaphysical commitments about science —and hence the defective conjunction of history and philosophy— ultimately disappears.

In my view, it arguably seems more productive to revise the metaphysical commitments about science which has been adopted on the philosophy side. First, the *incoherence argument* presupposes that there is no third way between absolutism and non-absolutism vis-à-vis the metaphysical commitments. I will concede this assumption even though it could be contested. The conflict absolutism vs. non-absolutism is typically conceived of as a mutually exclusive and jointly exhaustive dichotomy in the relativism debate. Boghossian (2006, 2020) and Bloor (2011b, 2020) agree on this point at least. For current purposes, it can be accepted such a dilemma as far as metaphysical commitments about science are concerned.

Second, history of science does not need to give up non-absolutism. As shown previously (§2.2), several mainstream historians think that the historiography of science does not involve absolutism, and that encouraging absolutism in a normative fashion would even undermine some standards of good historical analysis. Finally and foremost, philosophy of science is quite compatible with non-absolutism. After all, there can be some philosophies of science not committed to essentialism, universalism, and theoreticism about science.

Thus, whilst it is plausible to believe that non-absolutism is essential to historical discipline, it is far from obvious that absolutism is necessary to "philosophy" of science.

Defending that history of science is (or should be) committed to absolutism could also be done, but I will not follow this option here. Rather, I want to suggest that it can justifiably be accepted that philosophy of science does not need to involve absolutism, so the pragmatist can deny the first premise of the *incoherence argument* —i.e., philosophy embraces essentialism, universalism, and theoreticism about science. This move is sufficient to not sanction the unpleasant conclusion that integrated HPS lacks operational coherence.

The third and final step for solving the problem is to change the philosophy of science, thereby adopting a philosophical theory committed to non-absolutism. To do so, I will turn to examine two competing contemporary philosophies of science that crucially differ from one another regarding their corresponding metaphysical commitments about science. On the one hand, there is standard realism, which is committed to absolutism. On the other hand, the pragmatist proposes activist realism, which is instead committed to non-absolutism. Whilst standard realism opens the door to the *incoherence argument*, activist realism can successfully block such inference.

Before turning to examine these two philosophical theories, it is worth emphasising that the validation of philosophical theorising is not a separate task from the very practice of integrated HPS. For the pragmatist, the epistemic evaluation of philosophical claims takes place in a process of *epistemic iteration* (Chang 2022, §5.5) yielding an aim-oriented adjustment (Chang 2022, \$1.5) of the epistemic practice itself. The expected result is to make actual epistemic progress, which requires both creating and increasing the operational coherence of integrated HPS. Thus, changing the theory HPS-theorists adopt on the philosophy side in order to handle the defective conjunction of integrated HPS is nothing but an *adjustment* in the theoretical assumptions on which this practice is conducted. In other words, the epistemic evaluation of philosophy is also an activity that can result in modifying the relevant philosophical premises upon which integrated HPS is based. Facing the problem that the incoherence argument poses to HPS-theorists, the pragmatist will therefore abandon the philosophical assumptions under which integrated HPS does not work out, thereby introducing philosophical claims that ultimately support a practice that is operationally coherent. In this case, the HPS-theorist and the pragmatist philosopher is the same person too.

To that end, the pragmatist can justifiably apply the three criteria from operational coherence to evaluate activities, concepts, and claims in the domain of integrated HPS. She therefore asks the following triad of questions in accordance with these criteria:

- (A) Are HPS-activities operationally coherent? In other words, does integrated HPS feature aim-oriented coordination and pragmatic understanding? (EPISTEMIMC COHERENCE)
- (B) Do philosophical concepts refer to real entities? That is to say, do these concepts facilitate coherent epistemic activities in integrated HPS? (ONTOLOGICAL COHERENCE)
- (C) Are philosophical claims true? Specifically, are these claims true-by-operationalcoherence? (ALETHIC COHERENCE)

I will therefore examine the extent to which standard realism and activist realism either facilitate or not the operational coherence of integrated HPS. In doing so, I will answer these three questions regarding the two cases.

5.4 Invalidating standard realism

Put roughly, standard realism is the view that science aims at attaining the *absolute* truth about ultimate reality. To understand this claim, Chang (2022) characterises this philosophy as consisting of these five tenets:

- 1. There is mind-independent reality.
- 2. Truth consists in a correspondence between statements (or theories) and reality.
- 3. It is possible to obtain knowledge about mind-independent reality.
- 4. Attaining truth about reality [...] is an essential aim of science.
- 5. Modern science has been largely successful in this aim. (p. 69)

So construed, standard realism is an instance of absolutist philosophy of science. To see this, consider the following conception of absolutism regarding scientific truth and ontology, according to which scientific knowledge is rendered absolute so long as:

ESSENTIALISM: Scientific truth and ontology are temporally necessary. **UNIVERSALISM:** Scientific truth and ontology neither vary in scope nor degree. **THEORETICISM:** Propositional knowledge is the primary unit of analysis.

The tenets of standard realism are concordant with these absolutist principles. For one

thing, standard realist maintains that mind-independent reality is already prefigurated (tenet 1). For Psillos (1999), the metaphysical thesis of realism is that "the world has a definite and mind-independent natural-kind structure" (p. xvii). This squares with an absolutist conception of *objectivity*, according to which there are objective natural facts whose existence is entirely independent of human knowledge.

Additionally, standard realism is committed to a conception of truth as correspondence, which is a metaphysical theory of truth (tenet 2). On this view, the world is the truth-maker of scientific theories, which is mean to say that "instead of projecting a structure onto the world, scientific theories, and scientific theorising in general, discover and map out an already structured and mind-independent world" (Psillos 1999, p. 17). Put otherwise, standard realists reject an epistemic theory of truth, since "to say that a theory is true is to say that it corresponds to reality", thereby rescuing the idea that "the world is *independent* of theories, beliefs, warrants, epistemic practices and so on" (Psillos 2017, pp. 24-5). The central idea, then, is that the truth of theories ultimately depends upon metaphysical conditions about how the world is, not upon epistemic conditions related to the justification of scientific theories. Empirical success is an indicator of truth but not the truth-maker of theories. This also squares with an absolutist conception of *monism*, according to which there is a uniquely true theory of the world, provided that truth is a correspondence of theory with a prefigured mind-independent reality.

Furthermore, standard realism defends that science aims the truth (tenet 4), and that scientists can and does attain the uniquely true theory (tenets 3 and 5). For Psillos (1999), "'epistemic optimism' of scientific realism intends to stress that it is reasonable, at least occasionally, to believe that science has achieved theoretical truth" (p. 18). So attaining the truth is not only the central aim of science, but also its most valuable achievement. This idea squares with an absolutist conception of *knowability*, according to which it is possible to get the theory that correctly describes the world. Finally, this also fits an absolutist *optimism* —i.e., the belief there are at least some true scientific claims, which will therefore "last forever" (Vickers 2022).

In establishing these parallels among the five tenets of standard realism and those absolutist notions of objectivity, monism, knowability, and optimism, I followed some recent work on metaepistemology (e.g., Bland 2018; Boghossian 2001, 2006; Carter and McKenna 2021; Kusch 2021; Seidel 2014). On these construals, it seems plausible to take standard realism as committed to essentialism, universalism, and theoreticism relating to scientific ontology and truth. This philosophical theory is *essentialist* because it couches prefigurated reality as not contingent on scientific practice, which is to say that "scientific theories are answerable to the world and are made true by the world" (Psillos 2017, p. 24). Scientific ontology does not change because scientific theories change. Accordingly, if it is admitted that there is an "asymmetric dependence of the theories on the world" (Psillos 2017, p. 24), and that the world is knowable by science, then scientific knowledge will not change so long as it provides the true description of the world. Once the approximately true theory is obtained, what would be expected concerning scientific change is a minor adjustment of claims and concepts rather than a radical rejection and replacement of theories (Vickers 2022, pp. 10-3). Scientific truth remains essentially the same, admitting little refinements notwithstanding.

Standard realism is also *universalist* because human beings share and interact with the same world. Therefore, scientific truths are valid here and everywhere, and the content of scientific concepts does not vary across different contexts. For instance, the claim that "DNA has a double helix structure" is true in, say, London and Buenos Aires. Similarly, the nature of the electron is the same, say, in Mexico City and Cambridge, and "electron" refers to the same kind of entity where there is mass-energy.

Regarding *theoreticism*, standard realism is a view of science "concerning scientific theories and their relation to the world" (Psillos 2017, p. 20). Scientific realists are interested in understanding how scientists can get epistemic access to the world through theoretical representations, thus seeing whether believing in scientific theories is ultimately warranted. Of course, realists also pay crucial attention to the practice of science, but only because of the ways in which the features and elements of scientific practice are conducive to true scientific theories. Standard scientific realism is a theory-centered view of science.

Thus, the reliance upon standard realism makes integrated HPS an operationally incoherent practice. Under the above interpretation, this philosophical theory turns out to be an instance of the first premise of the *incoherence argument* —i.e., standard realism involves essentialism, universalism, and theoreticism about science. This makes realism as conflicting with the scientific non-absolutism of historiographical practice, thereby creating a defective conjunction between history and *this* philosophy. Arabatzis has already noted this problem as confronting those scientific realists who want to write and take seriously the history of science. For instance, he affirms that "given the realist's belief that (mature) science has developed against a stable ontological background, they are forced to portray past scientific terms and their modern descendants as referring to the same entities" (Arabatzis 2001, p. 539). This adoption of scientific realism

led HPS-theorists to commit "anachronisms and misinterpretations of past scientific practice" (Arabatzis 2001, p. 540), which ultimately "hampers the integration of history and philosophy of science" (Arabatzis 2018, p. 36).

The pragmatist lesson is that it is far from clear how to do history as (realist) philosophy in a *coherent* way. This is how the pragmatist invalidates standard realism by answering the triad of questions related to the three criteria from operational coherence:

(A) Does 'standard' integrated HPS feature aim-oriented coordination and pragmatic understanding?

The answer would be "no". As argued previously, there is no fitting-together between standard realism and the non-absolutism of history. In other words, standard realists are forced to accept the unpleasant conclusion that integrated HPS is not operationally coherent, provided they conduct this practice assuming standard realism on the philosophy side. Realism-based integrated HPS cannot therefore fulfil EPISTEMIC COHERENCE.

(B) Do 'standard' concepts facilitate coherent epistemic activities in integrated HPS?

The answer would be "no", at least regarding some central meta-scientific categories this theory postulates. For instance, Psillos' concept of "referential continuity" would lead HPS-theorists "to embrace Whiggism, a historiographical stance rejected by the overwhelming majority of historians of science" (Arabatzis 2018, p. 36). As some philosophical concepts do not facilitate the coherence of doing history as philosophy, HPS-theorists are entitled to consider as non-real those entities which such concepts refer to. I suggest reading Putnam's criticism of metaphysical realism in these terms. His claim that the notion of "correspondence" concerning truth is "empty" (Putnam 1995, p. 10) amounts to saying that this philosophical category refers to nothing real about the nature of scientific truth. Therefore, some 'standard' concepts do not fulfil ONTOLOGICAL COHERENCE.

(C) Is standard realism true-by-operational-coherence?

The answer would be "no", as long as standard realism opens the door to the *incoherence* argument. Since the epistemic activities of integrated HPS cannot be coherently performed by relying upon this philosophical theory, standard realism is not empirically true to an extent —i.e., the theory is unable to offer an accurate description of actual (past) science. Therefore, standard realism does not fulfil ALETHIC COHERENCE.*

5.5 Validating activist realism

Let me now turn to examine activist realism. This philosophical theory gives an entirely different image of science in pragmatist terms —one in which scientific ontology and truth are devoid of absolutist gloss. Put roughly, activist realism affirms that science aims at *creating more and better knowledge, thus learning many truths about many realities.* Limitations of space prevent me from addressing very properly standard realism, let alone its critical comparison with activist realism. (For a systematic discussion, see Chang (2022, Chs. 2, 5). Suffice it to highlight some central aspects to contrast both positions.

Activist realism differs from standard realism in being *realistic*, which means "to pursue aims that we can have some hope of achieving, at least aims that we can meaningfully work towards" (2022, p. 206). But activist realism is not only able to explain how scientific practice can produce knowledge of the world. This also contrasts with standard realism in being *activist*, "commitment to do whatever we can in order to extend and enhance our knowledge concerning realities, as much as possible in the context of other aims and values" (2022, p. 209). So activist realism also seeks to promote actual scientific progress.

For Chang, the central problem confronting standard realism is that it makes scientific practice operationally incoherent, since the intended goal of attaining the absolute truth about ultimate reality cannot be accomplished. It is not possible to coordinate activities to acquire knowledge of an inaccessible reality, discover prefigurated entities in it, and formulate propositions holding a correspondence relation to it. Neither does it make sense for scientists to pursue activities aiming at getting the final theory of the world. Standard realism makes science as lacking pragmatic understanding and would therefore obstruct scientific progress.

On the contrary, scientific ontology and truth are perfectly achievable aims according to activist realism. Regarding scientific ontology, Chang (2022) points out that "the term 'reality' should be reserved for things that we can meaningfully interact with, not for some inaccessible realm of Being that we only entertain in our abstract thought" (p. 207). As for scientific truth, he emphasises that science seeks nothing but "the pursuit of operational kinds of truth —namely, truth-by-operational-coherence, and secondary truth based on that" (Chang 2022, p. 206). Empirical truths differ from *the* metaphysical truth defended by scientific realists because "these are the kinds of truth we can attain in the here-and-now; we can work on improving the truths we have, and we can clearly tell when we are making progress in that task" (Chang 2022, p. 206). Hence Chang (2022) concludes that "my concepts make truth about real entities a very realistic aim to achieve, dissolving a central difficulty concerning standard scientific realism" (p. 207).

For current purposes, the difference between these two philosophies of science is relevant to see how activist realism is committed to non-absolutism, thereby becoming helpful to counter the *incoherence argument*. Specifically, the pragmatist conception of scientific knowledge, reality and truth squares with those metaphysical commitments that historical discipline involves. Rather than creating a defective conjunction in the core of integrated HPS, philosophers who advocate activist realism can perfectly agree with the image of science offered by historians, and vice versa. It follows that there is no intractable tension between *this* philosophy of science and the history of science whatsoever.

To see this, consider the following conception of non-absolutism concerning scientific truth and ontology, according to which scientific knowledge is not rendered absolute so long as:

CONTINGENTISM: Scientific truths and ontologies change over time. **LOCALISM:** Scientific truths and ontologies are domain-relative. **PRACTICALISM:** Active knowledge is the primary unit of analysis.

The three criteria from operational coherence are concordant with these non-absolutist tenets. For one thing, Chang makes it explicit that operational coherence is by no means an absolutist concept, indicating that "in not appealing to an absolute standard or authority for [pragmatic] understanding, my view may be considered a relativist one", and adding however that "relativism in the sense of rejecting absolutes is not a crude and bankrupt doctrine" (Chang 2022, p. 47. n. 30).

In addition, activist realism exhibits each non-absolutist tenet more specifically. Regarding CONTINGENTISM, Chang accepts that scientific knowledge, reality and truth are not immutable, but rather subject to temporal change —after taking a serious historical look at science. Scientific growth consists in actively changing our epistemic activities and aims to make cognitive progress in several productive ways. He calls this process an "aim-oriented adjustment", which takes place by means of "epistemic iteration" (Chang 2022, pp. 245-6). In commenting C. I. Lewis's view of scientific change, Chang (2022) states the following concerning ONTOLOGICAL COHERENCE: "there is no final point or destination of development, which is to say that nothing we regard as real should be regarded as absolutely and exclusively and eternally real" (p. 147). Similarly, Chang (2022) also adopts a contingentist account concerning ALETHIC COHERENCE, insisting that "it would be useful for us to get into the *habit* of always asking 'where/when is this statement true?' as an antidote to absolutist and universalist tendencies" (p. 172).

In respect of LOCALISM, Chang believes that scientific knowledge, ontology, and truth are local in the sense of being relative to domains. He points out that "domain here may be a spatio-temporal region, but more generally I intend the term to refer to all kinds of conditions that affect the coherence of an activity, pointing to a rather general type of context-dependence" (Chang 2022, p. 147). So operational coherence is a matter of degrees and something that epistemic activities exhibit in some cases but not in others. It happens that an epistemic activity that succeeds in achieving an aim in a certain domain can fail to do so in a different specific domain.

ONTOLOGICAL COHERENCE also incorporates the idea that "the reality of entities is not only a matter of degrees, but also something pertaining to specific domains" (Chang 2022, p. 147). Likewise, ALETHIC COHERENCE takes truth as a qualitative property to the effect that "it is the quality of truth itself that is a matter of degrees" (Chang 2022, p. 171). But truth is also a quality relative to domains, which implies that "a statement that is true in a certain domain can easily fail to be true in other domains (i.e., it may not support coherent activities there)" (Chang 2022, p. 172). This certainly counts as a relativist conception of truth, once noted that "another factor that makes truth non-absolute is its finite scope, or domain-specificity" (Chang 2022, p. 171).

Finally, PRACTICALISM is the most explicit assumption of activist realism. As mentioned previously, active knowledge has to do with the practice of science. Scientific research demands coordination among individual epistemic agents to develop aimoriented activities that require the use of both theoretical and material resources. It follows that the material culture and social organisation of science are explanatory central, too. This constitutes a methodological difference between activist realism and those epistemological approaches taking propositional knowledge as the primary unit of analysis. In positing active knowledge as a fundamental category, Chang (2022) brings to light a further difficulty with standard realism as based upon the propositional view of knowledge, namely, that "it obliges us to disregard many kinds of things that we readily regard as 'knowing'" (p. 17). Given that propositional knowledge is grounded but in active knowledge, he sets aside standard realism with the caveat that "I am not suggesting that the proposition-focused orthodox epistemology is *wrong*, but I do think that it is *limiting*" (Chang 2022, p. 17). At this point, it can be seen how the reliance upon activist realism allows the pragmatist to successfully rebut the *incoherence argument*. Recall that the proposed strategy was to adjust the first premise of it —i.e., philosophy of science adopts essentialism, universalism, and theoreticism about science. In doing so, this premise was replaced with the claim that activist realism adopts contingentism, localism, and practicalism about science. As a result, the pragmatist can construe a new argument to sanction the positive conclusion that history and philosophy are compatible, thus facilitating the operational coherence of integrated HPS. Here is the new argument:

The coherence argument:

- 1. *Absolutism.* Philosophy (activist realism in particular) is committed to contingentism, localism, and practicalism about science.
- 2. *Non-absolutism.* History is committed to contingentism, localism, and practicalism about science.
- 3. *Inherent concordance*. These two sets of commitments are concordant with one another. This concordance generates an effective conjunction between history and philosophy.
- 4. *Operational coherence* If there is an effective conjunction between history and philosophy, then HPS can exhibit aim-oriented coordination and HPS-theorists can have pragmatic understanding.
- 5. *Conclusion.* HPS can exhibit aim-oriented coordination and HPS-theorists can have pragmatic understanding. (Modus ponens 3, 4.)

Having done so, this is how the pragmatist validates activist realism itself by answering the triad of questions related to the criteria from operational coherence:

(A) Does 'activist' integrated HPS feature aim-oriented coordination and pragmatic understanding?

The answer would be "yes". After all, a fitting-together between activist realism and the non-absolutism of historical discourse is found. Indeed, with activist realism Chang (in conversation) is proposing a better philosophy of science that historians can safely and productively engage with. This implies that setting aside standard realism from philosophy of science can only help in bringing philosophy and history closer together again. As it is an effective conjunction between activist realism and the history of science, HPS-theorists can conduct operationally coherent activities in integrated HPS. Thus, 'activist' integrated HPS fulfils EPISTEMIC COHERENCE.

(B) Do 'activist' concepts facilitate coherent epistemic activities in integrated HPS?

The answer would be "yes". Chang argues that some of his pragmatist categories have shown to be quite useful as historiographical units of analysis. For instance, he points out that the concept of "systems of practice" concretely provides a better understanding of the Oxygen vs Phlogiston "theories" in the late 18th-century Chemical Revolution (Chang 2012), and of the Chemical vs Contact "theories" in the 19th-century Battery Science. This philosophical category facilitates a good history-writing about these historical episodes of scientific controversies, so HPS-theorists are entitled to take systems of practice as a real kind of entity, and the corresponding concept as successfully referring to the rich, complex nature of science itself. Therefore, 'activist' concepts can fulfil ONTOLOGICAL COHERENCE.

(C) Is activist realism true-by-operational-coherence?

The answer would be "yes", as long as it closes the door to the *incoherence argument*. The epistemic activities of integrated HPS can be coherently performed by relying upon this philosophical theory, so activist realism is empirically true to an extent. Chang (in conversation) confidently thinks that activist realism gives an image of science that today's historian can happily work with, which suggests that doing history as (activist) philosophy has offered an accurate description of past science. Therefore, activist realism itself fulfils ALETHIC COHERENCE*.

It is worth noting that this pragmatic validation of activist realism is not the only way of tackling the *conflict argument* that underpins *metaphysical unsuitability*. Besides Chang's integrated HPS, there are other authors whose conceptions of HPS involve non-absolutist philosophical commitments, so they would also be able to handle this issue in a similar way. This raises the following question: why did I take Chang's work as a special case of suitable disciplinary integration between history and philosophy rather than other representative works in the field? In the next section, I will answer this concern. After characterising how Chang conceives of integrated HPS, I will critically compare his view with those of three seminal authors: Kuhn, Hacking, and Feyerabend.

5.6 Integrating history and philosophy

A key aspect of Chang's conception of HPS is his reiterated insistence on *disciplinary* integration. For him, HPS encompasses work that is both historical and philosophical, not merely an "interaction" or "juxtaposition" of the history of science and the philosophy of science as *autonomous* disciplines. This idea is unpacked in two senses, seeing history as philosophy and philosophy as history. In the first sense, historyframing, historical analysis is inherently philosophical in that philosophical categories and assumptions are used to frame historical narratives; philosophical abstraction facilitates and improves the historical understanding of scientific episodes. In the second sense, *philosophy-basing*, philosophical analysis is inherently historical in that the study of history leads to generating abstract categories about science; historical reconstructions enhance the articulation of new philosophical ideas. Thus, when one is reconstructing concrete episodes, one is using a philosophical framework to understand historical actor's beliefs and actions. In turn, philosophy-based historical work leads one to revise the philosophical categories previously used and create new concepts for understanding those and new episodes. Thus, Chang (2004) notes that "it becomes difficult to see where philosophy ends and history begins or vice versa" (p. 240), thereby suggesting that "this process works out best if the historian and the philosopher is the same person doing both at the same time" (Chang 2011, p. 122).

More precisely, Chang (2011, pp. 121-3) indicates that integrated HPS operates at three levels of engagement. First, history and philosophy are in *necessary engagement* just in case history gives philosophy its "subject matter" and philosophy provides history with "conceptual frameworks". Second, history and philosophy are in *critical* engagement when the former provides "counterexamples" to philosophical theories and the latter offers "corrections" to historiographical perspectives. Third, history and philosophy are in *heuristic engagement* in that historians produce "new concepts for better understanding of puzzling episodes" and philosophers discover "new historical facts to remove philosophical puzzles". So construed, this three-level procedure aims to satisfy both the historiographical goal of accounting for puzzling episodes of past science and the philosophical purpose of elucidating fundamental epistemological issues. Arabatzis (2017) draws a distinction between "historical philosophy of science" and "philosophical *history* of science" as two types of integrated HPS, "the former aims at philosophical enlightenment, whereas the latter is motivated by historiographical concerns" (p. 70). Presumably, Chang's work amounts to "historical philosophy" and "philosophical history" alike.

This summarised characterisation of Chang's approach suffices to point out how it differs from other representative conceptions of HPS. I want to compare Chang with Kuhn, Hacking, and Feyerabend by examining a couple of aspects of their positions: the integrative character of their works and the non-absolutist philosophical commitments they involve. Limitations of space prevent me from discussing these authors' approaches to HPS in any detail. However, I will comment on their works by focusing on what is most important for current purposes.

Regarding the issue of *disciplinary integration*, Kuhn (1977) maintained as an HPS-theorists that history and philosophy cannot be integrated: "no one can practice them both at the same time" (p. 5); integrated HPS is like the failed attempt to "educe a duck-rabbit" (p. 6). He thus invites historians and philosophers to sustain "a dialogue between fields without subverting the disciplinary basis of either" (p. 4). Furthermore, Kuhn aligns with I. B. Cohen (1974) when he considers that history contributes to philosophy, but not vice versa: "I do not think current philosophy of science would be improved if history played a larger background role in its preparation" (Kuhn 1977, p. 12).

As an HPS-practitioner, Kuhn's position is less clear. Some of his work arguably amounts to philosophy-framed history, whilst his more influential contribution was in history-informed philosophy. For instance, in *The Copernican Revolution*, Kuhn (1957, pp. 39-41, 76, 135-43, 155, 172) writes an intellectual history of the Copernican episode presupposing categories of his model of scientific change that are not philosophically innocent, such as "revolution", "conceptual scheme", "crisis", "innovation", and "aesthetic" considerations. In *The Structure*, he draws upon his historical account of Copernicus to make philosophical points concerning "paradigms", "anomalies", "revolutionary science", "external factors", and so forth (Kuhn 1962, pp. 68-9, 75-6, 157). Kuhn (2000) depicts himself as someone who does "history for philosophical purposes" (p. 276); who addresses philosophical problems by doing historical research (Kuhn 1970b, p. 236).

Was Kuhn doing history and philosophy but not the two at once? He would answer that he was simply alternating between the two disciplines, "working from time to time on historical problems and attacking philosophical issues in between" (Kuhn 1977, p. 5). Certainly, *The Copernican Revolution* is not a book of philosophy, and *The Structure* presumably addresses "in between" some problems related to scientific rationality and change. Hacking defines himself as a professional philosopher. He characterises his work as a "new historicism" in philosophy, which squares with the so-called historical epistemology. This is the project of analysing philosophical ideas by understanding their historical origin, seeing "many philosophical problems as being essentially constituted in history" (Hacking 2004b, p. 63). As philosophy that uses the "history of the present", it aims to understand how we came to think what we now think vis-à-vis philosophical issues.

On this view, Hacking's HPS falls short of being philosophical history. Hacking (2012) is not interested in reconstructing scientific episodes: "the styles project uses the past as a way to understand the present" and "in itself it adds no new content to the history of science" (p. 600). Hacking's work is rather historical philosophy, in which he emphasises that the relation between history and philosophy is asymmetrical: no history, no philosophy. Philosophical criticism needs historical research as "the philosopher who conceives of the sciences as a human production and even invention requires the historian to show that analytic concepts have application" (Hacking 2004c, p. 198). Despite this asymmetric dependence of philosophy upon history, Hacking suggests that philosophy will be quite useful for historical analysis. For instance, the "style of reasoning" category can be employed to frame historical accounts and has "suggested historical research to others" (Hacking 2012, p. 600). Furthermore, good history typically involves philosophical commitments: "every sound history is imbued with philosophical concepts about human knowledge, nature, and our conception of it" (Hacking 2004c, p. 199).

Thus, whilst history clearly contributes to philosophy according to the three threelevels of engagement, Hacking's work is not history of science —even though his philosophical ideas can potentially contribute to historical research in both critical and heuristic terms. (For a detailed discussion of Hacking's conception of HPS, see Simos and Arabatzis (2021).)

Feyerabend's contribution is even more difficult to encapsulate, as his scare pronouncements on HPS "speak exceedingly softly", and they hardly coincide with his practice. In *Against Method*, Feyerabend (1993[1975]) makes some points concerning scientific rationality by using the history of science systematically, which he describes as "my attempt to draw methodological conclusions from historical examples" (p. 147). He draws primarily upon Galileo's defence of Copernicanism and the Copernican Revolution more generally, appealing to a great deal of both primary sources and secondary ones that were authoritative at that time. Feyerabend (1993[1975]) also mentions further historical episodes, noting for instance that "each of the examples of footnotes 3-17 can be used as a basis for case studies of the kind to be carried out in Chapters 6-12 (Galileo and the Copernican Revolution)" (p. 46 n. 20).

Using history is so necessary for the philosophy of science because historical data shows that "science is much more 'sloppy' and 'irrational' than its methodological image" (Feyerabend 1993[1975], pp. 157-8), which results from rejecting the common assumption that "the elements of our knowledge [...] are *timeless entities*" (Feyerabend 1993[1975], p. 106). He admittedly uses those case studies as *counterexamples* of methodological theories, from which he concludes that "rationalism" in the philosophy of science ultimately obstructs scientific progress. Feyerabend (1993[1975], Ch. 14) *confronts* his reconstruction of Copernicus and some Copernicans with several methodologies, showing that these philosophical theories are historically inadequate. He thus claims that "Copernicanism and other 'rational' views exist today only because reason was overruled at some time in their past" (Feyerabend 1993[1975], p. 116). More generally, Feyerabend (1993[1975]) points out: "wherever we look, whatever examples we consider" in the historical record, methodologies "give an inadequate account of the past development of science and are liable to hinder science in the future" (p. 157).

Whilst Against Method strongly suggests that historical case studies play a necessary, evidential role for (un)justifying philosophical claims, Feyerabend's self-reflective pronouncements seem to oppose reading his position in such terms. For one thing, he observes regarding his critique of rationalism that "this argument [...] does not depend on the historical material which I have presented" (Feyerabend 1993[1975], p. 117). The philosophical lesson that "a conflict between reason and the preconditions of progress is *possible*" can be equally drawn upon his case study of Galileo, even "if it turns out to be a fairy-tale" rather than "historically correct" (1993[1975], p. 117). For another, in his correspondence with Kuhn, Feyerabend makes it explicit that "reference to history plays no role whatever in the arguments used for establishing and defending (or attacking) a certain set of methodological rules" (in Hoyningen-Huene 2006, p. 619).

In view of this, Feyerabend's practice seems to involve "necessary" and "critical" engagements *from history to philosophy*, but some of his pronouncements as an HPS-theorist suggests quite the opposite. Meanwhile, it is far from clear what heuristic role historical case studies could play for philosophical theorising, and vice versa.

Now consider the issue of *scientific non-absolutism*. Kuhn shares the idea that philosophy is absolutist whereas history is not, as some of his pronouncements presumably suggest ($\S2.2$). Regarding *essentialism* and *universalism*, it is important to recall Kuhn's (1977) remark that philosophy aims "to discover and state what is true at all

times and places" (p. 5), that "philosophy's business is with rational reconstruction" seeking to capture the "essentials" of science (p. 14). Concerning his own work, Kuhn (1970b) thus claims that "I am no less concerned with rational reconstruction, with the discovery of essentials, than are philosophers of science" (p. 236). As for theoreticism, Kuhn's philosophical work was primarily concerned with scientific knowledge, with "the cognitive status of [...] theories" (1970b, p. 236). His ideas developed in *The Structure* concerning the rich composition of scientific paradigms and the structure paradigmatic revolutions would be ultimately oriented to explaining the epistemological success of science. And Kuhn also took a deeper turn to theoreticist issues in his late writings, focusing more on the conceptual aspects of scientific theories, shifting from scientific communities and exemplars as cognitive abilities to conceptual meanings, theoretical languages, theoretical translations, theory-choice, and so forth (Bird 2002, 2005). In contrast to this reading, Rouse (2003) put forward a "practicalist" interpretation of Kuhn's philosophy, which takes its central concepts as referring to science as a scientific activity. As such, Rouse's account is not properly an exegesis of Kuhn's work, but rather a fruitful way to employ Kuhn's ideas to conduct the philosophy of science as focused on practices. All in all, even if Kuhn's philosophy can justifiably be *understood* as involving practicalist elements (Kindi 2013), this is not to deny the theoreticist facet of his early and later works.

Hacking's position concerning the conflict between absolutism and non-absolutism is ambivalent. On the one hand, it exhibits non-absolutist aspects. It is certainly *localist* to conceive of scientific ontology and truth as relative to styles of reasoning: "there are neither sentences that are candidates for truth, nor independently identified objects to be correct about, prior to the development of a style of reasoning" (Hacking 2004c, pp. 188-9). So, Hacking (2004c) goes on to claim, "the truth of a sentence (of a kind introduced by a style of reasoning) is what we find out by reasoning using that style" (p. 191). Also, styles emerge from social-historical circumstances: "every style comes into being by little microsocial interactions and negotiations" (Hacking 2004c, p. 188). This aligns with Hacking's (1983) contingentist view that "knowledge itself is a historically evolving entity" (p. 17). He believes that styles of reasoning arise, develop and are replaced over time, thereby noting that "I am inclined to go with the contingency theorists among historians on all these points" (Hacking 2004c, p. 195). And his "anarcho-rationalism" considers that old styles "began to stabilize but also continued to evolve in an endless cycle of contingencies" (Hacking 2012, p. 600) and "new styles of reasoning will continue to evolve" (Hacking 2004a, p. 163). Finally, Hacking's *practicalism* is obviously much more explicit. Recall his experimental realism

is a philosophy centred on the practice of science: "reality has more to do with what we do in the world than with what we think about it" (Hacking 1983, p. 17). Similarly, Hacking's (2012) later characterisation of styles or reasoning in terms of "styles of scientific thinking and doing" seeks to make it explicit that "reasoning is also practical as well as theoretical; it involves as much doing as thinking" (p. 600).

Despite this, Hacking's position also involves absolutist components. Regarding *universalism*, his idea is that styles of reasoning come to be both autonomous and universal. Hacking (2004c) maintains that "styles of reasoning become autonomous of their origins and their originators" (p. 189), meaning that "each style has become independent of its own history" and "has become what we think of as a rather timeless canon of objectivity" (p. 188). Moreover, styles of reasoning are universal in two pivotal senses. First, they are distributed here and everywhere: "styles de pensée s'exportent très facilement de l'Europe et de ses anciennes colonies américaines à différentes parties du monde, et finalement, au monde tout entire" (Hacking 2006, p. 4). And this stems from science's capacity to globalise itself, since "les sciences, leurs connaissances, leurs méthodes et même leurs institutions sont hautement exportables" (Hacking 2006, p. 4). Second, styles are universal as they ultimately rely upon human cognitive faculties "that are presumed to be universal, they have become part of the heritage of our species" (Hacking 2012, p. 600).

Hacking also seems to adopt an *essentialism* concerning scientific ontology and truth. First, each style of reasoning introduces and individuates a new class of objects, but "this does not mean that objects of the class did not exist before there was a way to investigate them. That is nonsense" (Hacking 2012, p. 606). Styles define domains of ontology, but they do not create objects themselves; the reality of a certain entity does not depend upon how it is conceptualised within a given style. Moreover, it seems that a metaphysical conception of truth underlies Hacking's characterisation of scientific and non-scientific statements. For him the truth value of some propositions is not style-dependent, "what we might call pre-style or unreasoned sentences, including the maligned category of observation sentences" (Hacking 2004c, p. 191). But even the truth value of scientific claims is also independent of styles: "the actual truth value of those sentences is external to the style: what is true in no way depends upon the style of reasoning. The truth does not depend on how we think" (Hacking 1992, p. 135).

Lastly, Hacking's *practicalism* is restricted. This stresses the role of self-stabilisation techniques of styles and material culture of experimentation but at the expense of leaving out the social dimension of science. Hacking (2004c) considers that social factors explain the emergence of styles, but not those techniques by which styles come

to stabilise themselves: "a style becomes autonomous of the local microsocial incidents that brought it into being" (p. 196). Once styles emerge from specific social-historical conditions, they detach from those factors and are therefore able to achieve both self-stabilisation and self-authentication. Regarding the laboratory style, Hacking also suggests that social components are "external" to experimental practice. He displaces the role of human agency by assuming that material culture is primarily the "internal" element of the laboratory style. Not surprisingly, Hacking (2004c) indicates that the protagonist of the historical origin of experimentation, "as both Bruno Latour (1990) and I (1991) have observed, is not a person but an instrument, the apparatus, the air pump" (p. 185).

Arguably, "Hacking aims at maintaining a middle position, on the one hand, between contingency and inevitabilism, and, on the other, between internalism and externalism" (Simos and Arabatzis 2021, p. 163). For some people (in conversation), it is a virtue of Hacking's view that it could therefore go beyond the opposition between scientific absolutism and non-absolutism. Instead, I consider that Hacking's ambivalence is nothing but a sign that his work was afflicted by a fundamental tension vis-à-vis metaphysical commitments about science. This tension is even reflected in some interpretative scholarship about styles of reasoning. For Carter and Gordon (2014), Hacking considers that he is a relativist, but his position does not amount to relativism. By contrast, Kusch (2010) argues that Hacking says that he is not a relativist, but his position amounts to relativism. Moreover, Simos and Arabatzis (2021) point out that this tension does afflict Hacking's work on styles of reasoning, thereby jeopardising his alleged middle position. They even add that Hacking shows his inclination to ultimately adopt absolutism when it comes to explaining the stabilisation of styles: "his philosophical and metahistorical commitments compromise his position towards a more inevitabilist and internalist orientation" (Simos and Arabatzis 2021, p. 163).

As far as Feyerabend's non-absolutism is concerned, he advanced certain forms of relativism in some writings, but he demurred in his later work. Feyerabend's departure from relativism is based upon a sort of quietism concerning theories of science. For him, relativism is an epistemological theory among many, but "there cannot be any theory of knowledge [...], there can at most be a (rather incomplete) history of the ways in which knowledge has changed in the past" (Feyerabend 1993[1975], p. 269). He thus goes on to claim that "I now reject a philosophical relativism that provides a definition or a theory of truth and/or reality" (Feyerabend 1991, p. 513). And he equally rejects absolutism for the same reason: "relativism is as much of a chimaera as absolutism, its cantankerous twin" (Feyerabend 1991, p. 515). Going beyond this

opposition, Feyerabend (1991) was rather inclined to embrace a kind of "pluralism", where "different forms of life and knowledge are possible because reality permits and even encourages them and not because 'truth' and 'reality' are relative notions" (p. 516).

Interestingly, though, Feyerabend's quietism concerning philosophical theorising arguably relies upon two non-absolutist commitments, to wit: the complexity of reality and the unpredictability of history. Regarding the former, Feyerabend (1993[1975] draws a parallel between science and history: "history of science will be as complex, chaotic, full of mistakes, and entertaining as the ideas it contains", such as scientific ideas "in turn will be as complex, chaotic, full of mistakes, and entertaining as the ideas, and entertaining as are the minds of those who invented them" (p. 11). Regarding the latter, Feyerabend (1993[1975]) comments the historian Butterfield and points out that "history is full of 'accidents and conjunctures and curious juxtapositions of events' and it demonstrates to us the 'complexity of human change and the unpredictable character of the ultimate consequences of any given act or decision of men" (p. 9).

These two ideas square with scientific non-absolutism. Feyerabend's (1993[1975] contingentism is reflected in the thought that "science is a complex and heterogeneous historical process" (p. 106), insisting that such complexity is inherent to temporal change: "many of the conflicts and contradictions which occur in science are due to this heterogeneity of the material, to this 'unevenness' of the historical development, as a Marxist would say" (p. 107). Feyerabend (1993[1975]) makes that same point when he characterises the interaction between traditions: "in all these cases we have a practice, or a tradition, we have certain influences upon it, emerging from another practice or tradition and we observe a change" (p. 215). And he explains that "the change may lead to a slight modification of the original practice, it may eliminate it, it may result in a tradition that barely resembles either of the interacting elements" (Feyerabend 1993[1975], p. 215). Most importantly, Feyerabend (1993[1975]) notes that a "pluralistic methodology" would lead us to reject any idea of convergence relating to scientific progress: "knowledge so conceived is not a series of self-consistent theories that converges towards an ideal view; it is not a gradual approach to the truth" (p. 21). (See Kidd (2024) for a detailed discussion on contingentism in Feverabend.)

Likewise, Feyerabend qualifies as a *localist* as he is concerned with "cultural diversity and cultural change" (Feyerabend 1987, p. 1), and conceives of science as a "complex historical-anthropological phenomena" (Feyerabend 1993[1975], p. 206). As an epistemological anarchist, "the only thing he opposes positively and absolutely are universal standards, universal laws, universal ideas such as Truth, Reason, [...]" (Feyerabend 1993[1975], p. 189). In fact, one problem with rationalism is its universalism: "the belief in a unique set of standards that has always led to success and will always lead to success is nothing but a chimera" (Feyerabend 1993[1975], p. 160). This is because sciences "may proceed in an orderly way but the patterns that occur are not stable and cannot be universalised" (Feyerabend 1987, p. 11). Accordingly, Feyerabend (1987) points out that "knowledge is a local commodity designed to satisfy local needs and to solve local problems" (p. 28).

Whilst Feyerabend was concerned with scientific rationality as understood in terms of the relation between theories and evidence, some of his pronouncements involve *practicalism* in important ways. Firstly, he sees HPS-practitioners as anthropologist, who not only "explore the way in which scientists actually deal with their surroundings", but first and foremost "examine the actual shape of their product, viz. 'knowledge', and the way in which this product changes as a result of decisions and actions in complex social and material conditions" (Feyerabend 1993[1975], p. 197). Secondly, Feverabend (1987) stresses the role of "independent social developments, political (institutional) pressures, and powerplay" (p. 31) in scientific reasoning. During the Copernican Revolution, for instance, "accepted methodological rules are put aside because of social requirements (patrons need to be persuaded by means more effective than argument)" (Feyerabend 1993[1975], p. 120). Thirdly, his "practical relativism" articulates a view of "open exchange" among conflicting traditions, in which "much of what we know about people, their habits, idiosyncrasies, and prejudices, arises from interactions (between people) that are shaped by social customs and individual preferences; this knowledge is 'subjective' and 'relative'" (Feyerabend 1987, p. 28). Finally, Feyerabend (1991) seems to connect all these ideas with his conception of reality as an "agent of research" when he asks: "what are the elements of research? People, groups of people, instruments —and these are all parts of the world" (p. 514).

Thus, Feyerabend's quietism concerning relativism in no way prevents one from giving a "history" of his relevant non-absolutist commitments about science. A good conclusion might be drawn by Feyerabend (1991) himself: "while I confess to be a fervent relativist in some senses, I certainly am not a relativist in others" (p. 507). (For a detailed discussion on relativism in Feyerabend, see Kusch (2016).)

Up to this point, I have examined Kuhn's, Hacking's and Feyerabend's approaches to HPS and the non-absolutism that supposedly underlies their philosophies of science. On this basis, I want to close this section by showing how Chang's integrated HPS differs from these authors in three crucial respects. To begin with, *Chang's pursuit of integrated HPS is explicit and involves more elements.* First, Chang takes HPS as a discipline in its own, different from both the history of science (that not being philosophy-framed) and the philosophy of science (that not being history-informed). Second, his view differs from those approaches in HPS that only amount to either historical philosophy or philosophical history. His integrated HPS encompasses three-levels of engagement that incorporate historical philosophy and philosophical history alike. Third and finally, Chang believes that such integrated HPS can be accomplished *in practice*.

Moreover, *Chang's metaphysical commitments are coherent and perfectly non-absolutist*. We have seen that Kuhn held an absolutist view of philosophy and even he incorporated absolutist ideas within his own philosophical work. Hacking was ambivalent between absolutism and non-absolutism, more oriented to admit absolutism when he attempted to account for the so-called "paradox" of styles of reasoning —i.e., "qu'ils sont à la fois historiques et universels" (Hacking 2006, p. 4). Feyerabend's quietism led him to refuse any classification of his position as either absolutist or non-absolutist, but he nonetheless defended non-absolutist theses in his writings. Much on the contrary, Chang's philosophy embraces contingentism, localism, and practicalism about science at once and without many caveats.

The last point to notice is that *Chang's philosophy of science offers a solution* to metaphysical unsuitability itself. For one thing, Kuhn agreed on the complaint that history and philosophy conflict with one another, Hacking's HPS as based upon styles of reasoning suffered from this conflict, and Feyerabend's quietism is an explicit renouncement to seeing how his relativism could also close the door to metaphysical unsuitability successfully. Needless to say, Chang (2021a) himself recognises how Feyerabend has influenced in a great deal his own work on normative scientific pluralism.

For another thing, it is far from obvious how the Kuhnian model of paradigms and scientific change, styles of thinking and doing, and epistemological anarchism or pluralism would tackle *metaphysical unsuitability* in the domain of HPS. Of course, this exercise of "reflexivity" regarding each of these authors can be done in future research. For current purposes, however, suffices it to claim that Chang's pragmatist epistemology diagnoses the inherent tension of integrated HPS in terms of operational coherence and solves that problem by adopting activist realism, as I have argued along these pages.

5.7 Conclusion

In this chapter, my main purpose has been to rebut the *conflict argument* purported to support the philosophical unsuitability of history. To that end, I employed Chang's pragmatist epistemology, offering a pragmatist diagnosis of and solution to the problem of the inherent tension of integrated HPS. By characterising the conflict between history and philosophy as a form of operational incoherence, I proposed to restore the operational coherence of integrated HPS by changing the philosophy of science being adopted. Given that standard realism curtails the operational coherence of integrated HPS, HPS-theorists must replace this philosophical theory with activist realism, as long as the reliance upon this philosophical theory facilitates such operational coherence.

My suggestion is therefore the following: if one wants to bring together history and philosophy to sustain philosophical theses based upon historical case studies, then one can adopt activist realism in the philosophy side. My line of argument for this suggestion has two desirable results.

Firstly, I have vindicated the suitability of historical studies for the philosophy of science with the *coherence argument*. Unfortunately for the sceptic, the *conflict argument* underpinning *metaphysical unsuitability* fails to jeopardise the feasibility of integrated HPS. Secondly, in resolving the inherent tension of integrated HPS with activist realism, I also have rescued evidential reasoning in HPS from the problem that history of science cannot be evidence for philosophical claims. Thus, the sceptic cannot longer appeal to *metaphysical unsuitability* to call into question the use of historical case studies to establish philosophical conclusions about science. Put differently, in showing that history and philosophy are mutually compatible in metaphysical terms, activist realists can block inferring the conclusion that historical evidence does not support philosophical positions from the claim that historical case studies have no philosophical work to do.

I also attempted to make it plausible that Chang's approach to HPS is not the only alternative available, but it constitutes an up-to-date representative account of integrated HPS. This account serves this aim better than Kuhn's, Hacking's, and Feyerabend's contributions. Although these authors are representative beyond any doubt, they are nowhere near as good as Chang in formulating and defending how integrated HPS works out by adopting non-absolutist philosophical commitments.

Of course, it remains the question of whether evidential reasoning in HPS (and Chang's approach in particular) can escape from the objection from *epistemic unsuitability* (§2.3). This complains that historical data are ill-suited to sustain general and normative philosophical conclusions. In the next and concluding chapter, I will begin

by proposing some ways in which Lakatos, Stanford, and Chang could counter this another form of unsuitability.

Chapter 6

Concluding remarks

Abstract

This last chapter sketches an epistemology of HPS. I do this by formulating the epistemological lessons derived from my case-based argumentative strategy as developed in the previous chapters. In parallel to that, I want to make explicit some theoretical and methodological commitments that anchored my analysis. This serves to judge the scope of my argument and how this doctoral thesis contributes to the current debate on the foundations of HPS. To do so, I explain the character of the arguments supporting my epistemological lessons about evidential reasoning in HPS. Then I cover some contentious issues concerning the selection criteria for my three case studies, spelling out the methodological role of the so-called (meta-)alternation in my characterisation and defence of evidential reasoning in HPS. Before that, I briefly explore how the three works examined in my case studies manage to block the unsuitability argument as based upon epistemic unsuitability.

6.1 Time for epistemic unsuitability

Hitherto I have taken it for granted that the conflict argument underlying *metaphysical* unsuitability is the strongest reason for the philosophical unsuitability of history (§2.2). My case study of Chang's work was designed to tackle this variety of unsuitability (Chapter 5). Since the objections underpinning *epistemic unsuitability* are weaker in challenging the three works in HPS I have scrutinised, I decided not to pursue an in-depth examination of it in a separate case study. In this section, however, I am going to sketch how the works in question would handle this issue, thereby justifying the emphasis I put on the metaphysical formulation of the problem.

Consider firstly the *non-privileged of history* (§2.3.1). It is clear that using historical evidence is indispensable for Lakatos, Stanford, and Chang to establish their corresponding philosophical conclusions. Without the historical evidence, the acceptability of these conclusions is at best low and at worst null.

In the case of Lakatos, historical evidence is needed to criticise theories of rationality alongside logical and epistemological standards. Whilst falsificationism is "logically impeccable" (FMSRP, p. 108), conventionalism "is not easy prey to logical or epistemological criticism" (FMSRP, p. 129). However, these philosophical theories "can be falsified as rational reconstructions of history with the help of the sort of historiographical criticism" (FMSRP, p. 129). In addition, Lakatos' methodology proposes that the evaluation of scientific research programmes (and *mutatis mutandis* historiographical ones) involves lavishing attention on the developmental processes of diachronical series of theories; both theoretical and empirical progresses are to be judged historically.

The indispensability of history for Stanford's historical challenge to scientific realism is even more salient. Stanford (2017) points out emphatically that "the most persistent and influential challenges to this realist view have been motivated by exploring the historical record of scientific inquiry itself" (p. 212). For instance, historical evidence arguably shows that scientific realists cannot simply dismiss the underdetermination thesis as having no bearing on scientific knowledge and practice: "the historical record of scientific inquiry itself offers us a compelling reason to think that Duhem's challenge is a serious one" (Stanford 2006, p. 28). History is thus required for justifying not only the pessimistic induction, but also the problem of unconceived alternatives.

Regarding Chang's integrated HPS, it is obvious that philosophy needs history, and vice versa. It is "history giving philosophy its very subject matter, and philosophy providing the necessary conceptual framework" (Chang 2011, p. 123). Assuming that "historians can *generate* new concepts and ways of thinking that philosophers may not come up with from their entirely abstract work" (Chang 2011, p. 121), Chang makes it explicit that historical work is the place in which he has articulated his philosophical ideas. Specifically, activist realism facilitates accounting for "puzzling episodes" like the Battery Science and the Chemical Revolution, which in turn serve as counterexamples of standard realism. Finally, history is also indispensable for HPS as complementary science, provided "history serves as the supplier of forgotten questions and answers" (Chang 2004, p. 240).

All in all, Lakatos draws upon historical episodes not only because *logical analysis is insufficient* in some important cases, but also because *scientific rationality is essentially diachronical.* Stanford has shown that underdetermination is a serious conundrum as couched in terms of the problem of unconceived alternatives, which describes a *historical pattern* in the course of fundamental sciences and constitutes an epistemic predicament that *contemporary scientists* do face. And Chang needs history to do better philosophy of science, which especially includes studying neglected science as a source of alternative questions and methods for contemporary scientific work. Whilst historical evidence is arguably indispensable for the type of philosophical conclusions at stake, it is less obvious that logical analysis and other types of empirical data can lead to drawing those same conclusions —e.g., the evidence provided by cognitive science and the (non-historical) sociology of science.

What about the *naturalistic fallacy*? ($\S2.3.2$). The objection takes it for granted that (i) there is a logical distinction between a norm having authority and that norm having been taken or accepted as such, hence (ii) scientific practice is not a source of epistemic normativity. That is, the objection argues that epistemic norms can be established independently of the best examples of rationality, which presumably are to be found in successful science. Naturally, a promising way of countering this objection consists in contesting presuppositions (i)-(ii). One can appeal here to philosophical naturalism. Giere (2008) notes against presupposition (i) that "one must resist the non-naturalistic urge to seek beyond nature or history for something further on which to ground our moral and other normative judgments" (p. 218). As for presupposition (ii), he thus contends that "there is no naturalistic distinction between a social practice being regarded as normative and its somehow really being normative" (p. 218). Regarding scientific normativity, philosophical naturalism takes the evaluative activity of scientists and philosophers as an empirical phenomenon, the study of which is conducted according to the same methodological criteria that are employed for evaluating scientific knowledge.

Stanford arguably embraces this view. To begin with, the scientific realism debate assumes naturalistic commitments. Scientific realism is itself an explanatory theory of science to be tested empirically (e.g., Clarke and Lyons 2010; Douven and van Brakel 1995; McAllister 2023). As relied upon the inference to the best explanation, scientific realism counts as a hypothesis that would better explain the empirical success of science. Thus, realists make evaluative judgments about both scientific claims and scientific realism itself by appealing to empirical adequacy and explanatory power as a theoretical virtue.

Playing the same game, Stanford believes that historical case studies as secondorder evidence disconfirm scientific realism and call into question those scientific beliefs about fundamental realities. The problem of unconceived alternatives explains our epistemic situation regarding fundamental scientific knowledge. This problem suggests a normative judgment, namely that we have no good reasons for believing in current fundamental scientific theories as being approximately true. And this claim is ultimately based upon empirical evidence about how science has gone and presumably will go from here on.

Moreover, Stanford (2016) calls "integrative naturalism" his own meta-philosophical view. According to this, there is no foundational point from which epistemic judgments acquire normative force. Rather, one just has good and bad empirical evidence for claims about how the world is and about how we interact epistemically with it:

For such integrative naturalists, understanding what our best scientific theories are telling us about the world and understanding how we go about entheorizing that world in the first place are not distinct challenges: both are part of the overarching and more fundamental challenge of trying to simultaneously understand both the world and our own place within it. (Stanford 2016, p. 93)

Stanford thus rejects the presuppositions (i)-(ii) underlying the naturalistic fallacy objection. If scientists and non-specialists do not need any foundational point to evaluate their beliefs, Stanford's philosophy of science does not need it either.

Chang's position deserves a similar verdict. Integrated HPS investigates science as an epistemic endeavour, seeking to improve knowledge-acquisition practices. This approach is concerned with scientific normativity in two ways, "it captures something good about the norms that have actually governed science, and promotes that good by articulating, defending and developing it for future practice" (Chang 2012, p. 205). Chang (in conversation) shares the view that science has an inherent normative dimension worth investigating in historical and philosophical terms. This implies that science itself is the best place to look at its epistemic rationality. But it is in no way to be a naturalist in the sense of adopting "an unthinking deference to science" (Chang 2012, p. 248). Although Chang prefers to call his position "pragmatism" rather than "naturalism", his pragmatist philosophy involves rejecting presuppositions (i)-(ii). For the pragmatist every epistemic practice is empirical, so there is a continuity between philosophy and science. Scientists acquire empirical knowledge about the world whereas HPS-practitioners produce empirical knowledge about scientific practice. In Chang's (forthcoming) words, "we learn from experience, and experience includes the experience of inquiry, so we also learn from the experience of learning, which means that we learn how to learn" (p. 6). In short, normative judgments about scientific practice cannot be established in a non-empirical way, which arguably motivates to pursue HPS as "complementary science."

As for Lakatos, it is far from clear whether his position concerning scientific rationality is afflicted by the naturalistic fallacy. He claims that "one tries to compare this rational reconstruction with actual history and to criticize both one's rational reconstruction for lack of historicity and the actual history for lack of rationality" (FMSRP, p. 53). On the one hand, Lakatos seems to accept presuppositions (i)-(ii). Normative judgments of rationality are third-world premises, whereby methodologies of science are criticised in terms of rational reconstructions that involve normative claims. On the other hand, Lakatos seems to give up those presuppositions. He defends a "pluralist system of authority, partly because the wisdom of the scientific jury and its case law has not been, and cannot be, fully articulated by the philosopher's statute law, and partly because the philosopher's statute law may occasionally be right when the scientist' judgment fails" (HSRR, p. 137). This means that when methodological theories and actual scientific practice diverge, philosophers and the scientific elite can be blame alike. But if one concedes the idea, as I have suggested (§3.3.2), that scientists' epistemic judgments reflect the epistemic facts, then the distinction between having authority and taken to be authoritative is merely logical, and scientific practice is an actual source of epistemic normativity notwithstanding.

Finally, the objection from hasty generalisation (§2.3.3) attacks primarily inductive uses of historical evidence. Neither Lakatos nor Chang use the history of science in that manner. Lakatos defends a "quasi-empirical" theory of epistemic justification that also applies to historiographical research programmes. On this view, historical evidence disproves rather than proves philosophical theories of rationality. Epistemic justification "is not the transmission of truth but rather the retransmission of falsity" (REPM?, p. 28), which moves "from the basic statements 'upwards' towards the hypothesis –logic here is an organon of criticism" (REPM?, p. 29). As seen in Chapter 3, Lakatos' procedure is to offer historical episodes that cannot be rationally explained by the methodology of science under test. For this aim, the sample size and the heterogeneity of the historical cases being presented are of minor importance.

Meanwhile, Chang (2011) criticises to employ *particular* historical cases to establish *general* conclusions via enumerative induction, proposing instead to "seeing the history-philosophy relation as one between the *concrete* and the *abstract*" (p. 110). Again, philosophy provides abstract ideas to conceptually frame historical episodes, which are concrete instantiations of such ideas. Whilst this procedure is first and foremost one of conceptual articulation and historical understanding, it does not exclude philosophy-framed historical accounts from having a justificatory role. Chang (2011) notes that "an abstraction becomes *general* only when it has been *applied* widely. Successful application functions as confirmation, but without the presumption of universality in

what is confirmed" (p. 111). This seems to restore an inductive view again, but one renouncing to make perfectly general claims about science.

On the contrary, the strategy of historical ostension is inductive in character. Stanford's (2006) historical challenge explicitly involves an "enumerative induction projecting directly from past cases of failure to conceive of serious theoretical alternatives to future ones in any case" (p. 45). At first glance, it is very hard for Stanford to escape from hasty generalisation. Consider however a more charitable reading of Stanford's reasoning, which focuses on three aspects of it.

Firstly, the problem of unconceived alternatives should not be taken as a grand generalisation about science because it is largely domain-specific, as Stanford's commitment to *localism* suggests. I will unpack this non-absolutist aspect of Stanford's philosophy in the next section. Secondly, the sample size of the evidence supporting his historical challenge seems to be "big enough"; CIB plus NIB constitutes eight historical cases in total rather than "one case or even two or three". Yet, other scholars have enriched the scientific realism debate by discussing some of these and further historical cases (e.g., Lyons and Vickers 2021). And thirdly, Stanford does not think that such historical episodes are quite heterogeneous among them. The strategy of historical ostension articulates philosophical points "by indicating a range of actual historical cases in which they hold the relevant properties or relationships to be exemplified" (Stanford 2017, p. 215). The cases involved in CIB and NIB are homogeneous *exemplars* as they are "concrete historical examples of successful past theories that contemporary theoretical orthodoxy judges to be 'not even approximately true'" (2017, p. 215).

The foregoing discussion shows how the three works in HPS I have examined can tackle *epistemic unsuitability*. They already have the resources to succeed in responding to these objections or at least in mitigating their effects. As such, *epistemic unsuitability* is not as strong as its metaphysical counterpart.

Having done so, I am now in a position to formulate the epistemological lessons that derive from my case-based argumentative strategy as developed in the preceding chapters. This helps to characterise the abstract epistemological argument I am underpinning in this doctoral thesis more clearly.

6.2 Epistemological lessons

Here I outline the epistemology of HPS. Drawing upon three case studies of works in HPS, I have discussed two sceptical arguments purported to challenge the epistemic status of evidential reasoning in HPS. The works in question are *hard and paradigm*

cases, from which I provide both a *confutation* of the two sceptical arguments and present a *plausibility proof* concerning the epistemic status of evidential reasoning in HPS.

My confutation is grounded in a proof by counterexample. Both sceptical arguments conclude that *historical evidence does not support philosophical claims*. My case studies, however, contradict this conclusion. The proof thus takes this more technical form:

Confutation:

- 1. Historical evidence does not support philosophical claims. (Sceptical conclusion.)
- 2. As for historiographical research programmes, rational reconstructions do not support methodologies of science. (Universal instantiation 1.)
- 3. As for historiographical research programmes, rational reconstructions support methodologies of science. (Case study (1).)
- 4. As for historiographical research programmes, rational reconstructions do not support methodologies of science and rational reconstructions do support methodologies of science. (Conjunction 2, 3.)
- 5. Therefore, it is false that historical evidence does not support philosophical claims. (Reductio ad absurdum 1, 4.)

Here I am considering my case study of Lakatos' work as a counterexample of the sceptical conclusion, as long as it relies upon the *circularity argument*. My proposed typology of independent historical evidence sanctions the claim that rational reconstructions support methodologies of science. However, notice that my case studies of the strategy of historical ostension and integrated HPS are counterexamples of the sceptical conclusion in that same way. The former contradicts the sceptical conclusion as based upon the *circularity argument*, whilst the latter contradicts the conclusion as established by the *unsuitability argument*. Therefore, *confutation* offers the following anti-sceptical antidote as far as my three case studies are concerned:

Case-bound conclusion: The *circularity argument* fails to undermine the epistemic status of both Lakatos' and Stanford's works, whilst the *unsuitability argument* is unable to defeat the epistemic status of Chang's work.

Presumably, the sceptical conclusion that historical evidence does not support philosophical claims is an universal, epistemological claim. This is so because the *circularity* argument and the unsuitability argument are construed under principled reasons that deductively sanction the same sceptical conclusion. But even if the sceptical conclusion is admittedly taken to be a robust generalisation claim, *confutation* is nonetheless sufficient to limit its scope. Under these two readings of the proof, my *case-bound conclusion* obtains in any case.

Now, confutation logically permits to make an existential generalisation to the effect that there is historical evidence that supports philosophical claims. As such, this existential generalisation is only a *possibility claim*. This is primarily supported by an *actuality proof*, in which my case studies are positive instances of such a claim. For instance, if (it is the case that) rational reconstructions support methodologies of science, then (it is possible that) historical evidence supports philosophical claims. In addition, the *possibility claim* can be justified with the positive theses I am abstracting from my three case studies. Such theses are the following:

- (T1) Historical reconstructions satisfy types of independent evidence when they support philosophical theories (as elicited from historiographical research programmes).
- (T2) Standards relating to historical adequacy are crucial to assessing reconstructions of historical episodes in order to settle philosophical disagreements (as concluded from the strategy of historical ostension).
- **(T3)** Non-absolutist philosophies of science are concordant with the historiography of science and therefore facilitate bringing together history and philosophy (as established from integrated HPS).

T1-T3 are also existential generalisations statements that encapsulate some general aspects of evidential reasoning in HPS. I have identified in, and abstracted from, my three case studies some epistemic properties of this practice. These properties can be couched in the following terms:

Epistemic properties: There are (i) types of independent historical evidence, (ii) historiographical standards for criticising reconstructions and adjudicating philosophical disagreements, and (iii) philosophical theories quite compatible with historical research.

Arguably, these properties are *qualities* of evidential reasoning in HPS —i.e., they constitute a mark of its *success*. This means that a particular work in HPS is an instance of *epistemically valuable evidential reasoning* provided this work features at least

one of such properties (i)-(iii). Similarly, this conception delivers three corresponding *epistemic desiderata* for HPS-practitioners, which have a bearing on how evidential reasoning in HPS can avoid the charge of being epistemically objectionable. By the same token, instances of epistemically valuable evidential reasoning are expected to meet the following triad of desiderata:

- (a) Desideratum of independent historical evidence: For the relation of evidential support not to be viciously circular, HPS-practitioners must employ historical reconstructions that constitute independent historical evidence in the proposed forms.
- (b) Desideratum of historical criticism: For the relation of evidential support not to be viciously circular, HPS-practitioners must critically evaluate historical reconstructions by applying historiographical standards, thus enhancing "a greater sensitivity to the canons of history" (Cohen 1974, p. 312).
- (c) Desideratum of concordance between disciplinary principles: For historical data to be suitable for philosophical theorising, *HPS-practitioners must rely their philosophy of science upon non-absolutist fundamental commitments, as long as these cornerstone principles are concordant with those of historiographical practice.*

These epistemic desiderata are conditional judgments connecting means and ends: if one's objective is X, then one must do Y. For instance, "if one wants to get a rocket to the moon, then one should rely on classical mechanics" (Giere 2011, p. 61). I am adopting a hypothetical conception of rationality, according to which being "rational" is to use a mean that is known to be effective in achieving a particular aim. On these grounds, I suggest to rephrase the proposed *possibility claim* as follows:

Ampliative conclusion: There are further instances of evidential reasoning in HPS that meet at least one of the three epistemic desiderata, in which historical evidence therefore supports philosophical claims.

Apart from pointing to a bare possibility, the *ampliative conclusion* turns out to be highly plausible. There are at least three ways of bearing out its plausibility; feasible reasons that make it sensible to accept this conclusion. Let me elaborate on this in more detail.

First reason: The proposed epistemic desiderata can justifiably be extrapolated by considering my sample of case studies.

Each of these three works in HPS showcases a particular epistemic property that is nonetheless found in another work that belongs to the same sample. Consider the strategy of historical ostension as an example. Whilst Stanford's reasoning first and foremost meets *desideratum* (b), it also fulfils *desideratum* (a) —i.e., the direct proof for the historical challenge to scientific realism involves types of independent historical evidence.

In his response to Forber (2008) and Godfrey-Smith (2008), Stanford (2015) gives additional historical evidence to support the problem of unconceived alternatives that meets *type-independent evidence*. These authors contend that there are good reasons for thinking that current scientific theorising is not (or less) vulnerable to recurrent transient underdetermination, since communities of scientists are more capable of exhausting the space of serious theoretical possibilities than their individual members. Further, today's scientific communities reliably apply eliminative inference as they are much better organised in articulating theoretical alternatives than past scientific communities.

Interestingly, Stanford (2015) replies that "those historical transformations of the scientific enterprise independently regarded by historians of science as most profound and fundamental [...] each served instead to increase rather than decrease the vulnerability of the resulting scientific communities to the problem" (pp. 868-9; my emphasis). For Stanford, the historical evidence concerning the process that gave cause to conservative social organisation within contemporary scientific practice shows not only that Stanford's inductive projection as based upon NIB is appropriate, but also suggests that the underdetermination predicament is currently more pervasive. The factors that explain conservative social organisation in scientific practice include "the professionalization of science in the middle decades of the nineteenth century, the shift to peer-reviewed funding of academic science by the state following World War II, and the ongoing expansion of so-called big science" (Stanford 2015, pp. 868-9). Stanford thus proposes that contemporary science restricts many more scientists' explorations of divergent research agendas and articulations of alternative theoretical explanations. Most importantly, this historical evidence is independent of the historical cases involved in the NIB, since this evidence rather stems from the historiography of science and social studies of science and technology (e.g., Bowler and Morus 2005; Chubin and Hackett 1990; Daniels 1967; De Solla Price 1963; Shapin 2008).

That same historical evidence can be employed as higher-order independent evidence. For one thing, calibration would change the degree of confirmation of the problem of unconceived alternatives if Stanford's NIB were *calibrated* with historical reconstructions of the same episode coming from the historiography of science. Stanford's reply seems to illustrate how sociology-based historical reconstructions are surrogates of his own historical study of inheritance theorising. Moreover, the problem of unconceived alternatives would become a robust philosophical conclusion if it were drawing upon historical reconstructions whose theoretical sources are independent of one another. Stanford's reply can justifiably be taken as involving a *robustness argument*: the problem of unconceived alternatives is a robust philosophical conclusion to the extent that it is supported by concordant historical case studies that were reconstructed from independent theoretical sources —i.e., NIB and historians' research concerning the emergence of contemporary scientific institution. Lastly, it is worth recalling Stanford's use of reliable, primary sources alongside authoritative secondary studies, mainly his emphasis on the close correspondence between his own historical study and the historical scholarship when he asserts that "I trust that my colleagues in the history of science will set me straight" (Stanford 2006, pp. 75-6 n. 1).

Consider now a second example, which illustrates how Stanford's argument also meets *desideratum* (c)—i.e., his historical challenge to scientific realism is non-absolutist as it involves contingentism, localism, and practicalism about science.

Stanford (2021) points out that the "particularist turn" in philosophy of science over the last decades has led philosophers to abandon the essentialist and universalist pretensions they once embraced. This attitude is reflected in his own philosophical position. The problem of unconceived alternatives is a general claim describing a historical pattern, but its domain of application nonetheless corresponds to a local, contingent feature of scientific practice.

Regarding *localism*, recurrent transient underdetermination only afflicts a particular use of eliminative forms of scientific reasoning by scientific communities in the context of fundamental theories of physics and biology. This means that "the evidence we have does not support a blanket challenge to all eliminative inferences or to eliminative inferences in every epistemic context" (Stanford 2006, p. 46). Further, the problem of unconceived alternatives does not go beyond the scientific theories and communities as these are depicted by the case studies involved in NIB.

As for *contingentism*, Stanford argues that recurrent transient underdetermination could even disappear: the future course of fundamental scientific theorising could be otherwise, when the epistemic warrant for a given scientific belief would license to adopt a realist commitment. To show this, Stanford (2011) draws upon a historical case concerning the acceptance of the hypothesis of organic fossil origins. He argues that scientists in the modern era were facing the problem of unconceived alternatives because eliminative inference was the warrant they had for the hypothesis that fossils are the remains of once-living organisms. Now, the experimental evidence obtained from "taphonomy" in the 20th century ultimately changed the type of evidential support for that hypothesis: the warrant ceased to be eliminative in character, passing instead to be grounded in "projective evidence" (Stanford 2011, p. 15). Evolutionary biologists are thus entitled to be realists about the hypothesis of organic fossil origins, since there was a change in the type of reasoning supporting this scientific claim. More generally, the problem of unconceived alternatives could therefore be otherwise in the future as the forms of scientific reasoning for the same given theory are modified. This is compatible with having inductive grounds to expect that future science will be vulnerable to this problem, as Stanford maintains.

This case study of the hypothesis of fossil origins also leads to reject essentialist and universalist theories of scientific confirmation. Stanford (2011) indicates that "philosophers of science have long sought the holy grail of the logical form of scientific confirmation", adding however that "such accounts either misrepresent or ignore something important about the heterogeneous ways in which scientific hypotheses can be supported by evidence, and [...] the search for any single such account may be misguided in any case" (p. 989).

Finally, Stanford admittedly embraces *practicalism*. As mentioned before, Stanford uses the history of the emergence of "professional science", "big science", and "technoscience" to defend that the vulnerability to the problem of unconceived alternatives is much higher for contemporary scientific communities. Here the conceptual link between the practical and the epistemic is clear. In particular, a species of social order (i.e., conservative forms of scientific work) determines the epistemic reliability of a species of scientific reasoning (i.e., eliminative inference in the context of fundamental scientific research). This is a good example of how historical studies that portray science as a social practice become quite informative in understanding scientific reasoning and belief.

Second reason: The proposed epistemic desiderata can justifiably be extrapolated given that my sample involves hard and paradigm cases.

Hard cases put epistemic desiderata to the severe test. I take for granted the idea that "hard cases demonstrate the power of a principle, and they show that the same
principle can plausibly handle a host of similar but less difficult cases" (Scholl and Räz 2016, p. 77). Similarly, paradigm cases are typical, successful instances of epistemic desiderata. I assume that paradigm cases give "some optimism that many concepts, once developed and refined, can be transferred from them to other cases" (Scholl and Räz 2016, p. 79). On these grounds, I argue that Lakatos' and Stanford's works are hard cases vis-à-vis the *circularity argument*, and that integrated HPS is a paradigm case in relation to the *unsuitability argument*.

Consider firstly the hard cases. Historiographical research programmes involve *fictional history* concerning the rational reconstructions of episodes. Kuhn (1970a) already noted that "what Lakatos conceives as history is not history at all but philosophy fabricating examples" (p. 143). It is not therefore clear how Lakatos' view could avoid the accusation of manipulating history. Moreover, Lakatos assumes that *confirmation entails theory-choice* in testing methodologies of science. Recall that "tests are —at least— three cornered fights between rival theories and experiment" (FMSRP, p. 31). In consequence, it would be very difficult for historiographical research programmes to escape from the problem of historical pluralism.

The strategy of historical ostension also seems to fall prey to both problems. Whilst Stanford believes that the caloric episode does not suit the aims of selective realism, realists such as Vickers and Chakravartty contend that no historical episode —caloric in particular— can adjudicate the selective realism debate. Additionally, in Chapter 4 I suggested a hitherto unexplored way of defusing Stanford's direct proof for the historical challenge, which consists in arguing that his philosophical approach to history ultimately manipulates history in constructing NIB. Curiously enough, philosophicallyminded critics of Stanford's position have not paid enough attention to his *historical challenge*, the weight of which is definitely rooted in the evidential use of historical cases.

Consider now how integrated HPS works out as a paradigm case. It is wellrecognised that Chang's work has been the most explicit, committed effort to integrate historical and philosophical approaches in recent years (e.g., Kusch 2015, p. 70). This project requires history and philosophy to be compatible and the former therefore to be appropriate for the latter, and vice versa. So construed, integrated HPS is an up-to-date exemplar of the disciplinary integration, which nonetheless the unsuitability argument is purported to challenge. For this reason, it will be very difficult to accommodate Chang's work as a case for the philosophical suitability of the history of science if metaphysical unsuitability is correct; this would therefore pose a serious obstacle to the feasibility of integrated HPS. Conversely, if integrated HPS succeeds in tackling *metaphysical unsuitability*, one can reasonably expect that similar proposed forms of disciplinary integration will not be afflicted by this sceptical conundrum either.

This triad of works, then, cannot be dismissed in the discussion over evidential reasoning in HPS. If the sceptical arguments were undermining such cases, then they would be easy targets for scepticism. However, I already argued that these works manage to block the corresponding sceptical argument. Thus, as the works in question are hard and paradigm cases that demonstrate the power of T1-T3, such theses can plausibly be expected to apply to other works whose epistemic status has not been clearly and directly threatened by the *normative thesis* underwriting the sceptical position. This ultimately speaks in favour of the plausibility of my *ampliative conclusion*.

Third reason: The proposed epistemic desiderata can justifiably be extrapolated given that HPS-critics have not yet proven that such extrapolation fails.

Given my three case studies, it is clear that HPS-critics now have the burden of proof. I mentioned in the Introduction that the sceptical position may be understood as involving two assumptions vis-à-vis the epistemic status of evidential reasoning in HPS. The strong assumption claims that evidential reasoning is epistemically objectionable until proven otherwise. This demands HPS-partitioners to provide an antecedent justification for the justificatory role of historical evidence. This is too demanding, however. Has Giere given good reasons to accept the normative thesis in the first place? I am reluctant to believe that evidential reasoning is guilty until proven innocent —such as people are not guilty until proven otherwise in a legal context.

Thus, the weak assumption instead involves that *evidential reasoning holds a positive epistemic status until proven otherwise*. This means that unless there are stronger reasons for judging that historical evidence does not support philosophical claims, HPS-practitioners are not obligated to give evidential reasoning up in a lack of reasons for proving otherwise. Conversely, HPS-practitioners must abandon evidential reasoning just in case there are good reasons for concluding that this practice is fraudulent in epistemic terms.

HPS-critics take for granted this second point on the basis of the two sceptical arguments. This point demands HPS-critics to provide an antecedent justification of evidential reasoning given that sceptical arguments constitute a defeater for its epistemic status. This is primarily what I attempted to do in these pages: defeat the defeater! If I succeeded in this task, the situation is now the one described by the first point, namely that HPS-practitioners are not obligated to give evidential reasoning

up because there are no good reasons for doing so. In this situation, HPS-critics are those who need to make further arguments to demonstrate that evidential reasoning is epistemically objectionable.

It is not trivial to clarify the dialectics concerning the *normative thesis* in this way, since it has implications for the plausibility of my *ampliative conclusion*. I take it for granted that both critics and practitioners share the view that the method to both examine and assess evidential reasoning in HPS is by drawing upon case studies. This naturalistic approach makes the *ampliative conclusion* a hypothesis to be tested, which requires producing more case studies of case studies. Surely, the other works in HPS I listed in the Introduction as counterexamples of the *descriptive thesis* are entirely worth examining and evaluating. Addressing such works would be a good starting point for future research. But it is important to note something about this starting point. Having conceded the weak assumption, HPS-critics now have the burden of proof. Until they manage to demonstrate that a host of works in HPS are epistemically objectionable and must therefore be abandoned, those works plausibly fulfil the proposed epistemic desiderata. Evidential reasoning in HPS is innocent until proven guilty.

In the meantime, HPS-practitioners should spend some energy examining alleged shortcomings in their characterisation and defence of evidential reasoning in HPS. By covering some contentious issues about my argumentative strategy, I now want to clarify more precisely my terminological, theoretical, and methodological commitments.

6.3 Pressupositions and implications

The first issue concerns the selection of cases and the scope of the epistemological lessons. Lyons and Vickers (2021) convincingly claim concerning the historical challenge to scientific realism that "the history of science is a big place, and it was never plausible that all the important lessons for the debate could be drawn from just three cases" (p. 2). A similar point can be made regarding my own case-based argumentative strategy. Whilst HPS is not as big as science, it is also an enterprise very rich and diverse. Worse yet, I have presented just three case studies of case studies! Two questions therefore immediately arise:

- (Q1) Which were my criteria for sampling these cases rather than others?
- (Q2) What is the evidential weight of just three case studies vis-à-vis my epistemological lessons?

Regarding both questions, someone would object that my sampling involves *cherrypicking*, whilst contesting that using three case studies amounts to *hasty generalisation*. To respond to this, I want to make it explicit my sampling criteria in the first place.

The three works in HPS I have examined are relevant and representative. That is, they arguably portray some common aspects in virtue of which they are good examples of evidential reasoning in HPS and hence potential victims of the sceptical challenge.

The first aspect is the *indispensability of history to draw unexpected philosophical* conclusions. As mentioned before, the works in question need historical evidence to support their respective philosophical claims. But also notice that philosophical lessons from history seem to be surprising claims about science. These lessons are unexpected in that they challenge commonsensical meta-scientific views that several philosophers and scientists themselves widely accept. And the cogency of these claims cannot be properly estimated by ignoring the historical record. For instance, it is quite contrary to common sense about science that there is no such thing as an isolated and conclusive refutation of scientific theories, that inference to the best explanation fails in fundamental sciences, and that there are multiple truths about many realities, etc. Likewise, the works I mentioned in the Introduction also make these surprising statements: epistemic standards are just local and contingent, there exist general patterns of scientific change, and replication is not invariably the gold standard of experimentation, etc. Taking a serious look at the history of science has led to originally formulating and/or reasonably accepting such unexpected claims. Of course, this is in no way to suggest an *exclusiveness* of history. That historical evidence is necessarily central does not prevent the same philosophical conclusions from obtaining warrant from different sources.

The second aspect is that *HPS is conceived of in continuity with scientific research*. In different ways, the works in question seem to embrace a scientific conception of HPS itself. The point is that both research *in* science and *about* science are at least structurally identical in that they produce empirical knowledge. Accordingly, the epistemic evaluation of philosophical claims is similar to that of scientific hypothesis; both epistemic practices involve evidential reasoning for that matter. For instance, given that HPS is scientific in character, historians and philosophers provide "second-order evidence" for and against scientific claims (e.g. Psillos 2011, p. 188; Vickers 2022). Regarding my three case studies, Lakatos considers that historiographical programmes work like scientific ones, and that philosophers can legitimately criticise the "scientific elite" sometimes (HSRR, p. 137). Stanford believes that the problem of unconceived alternatives justifies not to believe in current fundamental scientific theories, and his

integrative naturalism sees science and HPS as "part of the overarching and more fundamental challenge of trying to simultaneously understand both the world and our own place within it" (Stanford 2016, p. 93). Finally, Chang's (2004) "complementary science" is "a continuation of science by other means" (p. 249). His project aims at considering scientific problems that current science is neglecting, so "HPS can generate scientific knowledge" (p. 237) with criticism of specialist science that contributes to epistemic progress. Thus, the point with the continuity between science and HPS is to stress the fact that HPS-theorising is structurally identical to HPS-practice, in the same way that the latter is structurally identical to scientific reasoning. Naturally, I have used case studies to draw epistemological lessons about evidential reasoning in HPS itself.

A third aspect concerns the *diversity of cases*. Although the works in question share common features that characterise evidential reasoning in HPS, it is worth noticing that they diverge to one another in some important respects. These works differ in how they conceive of the relation of evidential support between history and philosophy, whose relata are historical data and philosophical theories. For historiographical research programmes, historical data involve rational reconstructions of the actual history of science. The strategy of historical ostension employs historical data steaming from historical cases that depict a *historical pattern*. Meanwhile, integrated HPS draws upon historical information resulting from framing *historical episodes* in philosophical terms. Regarding philosophical theories, Lakatos proposes that these are *normative* theories of scientific rationality. Stanford takes the problem of unconceived alternatives as an *explanatory* philosophical claim about the limits of human reasoning. And Chang couches philosophical theories in terms of conceptual *abstractions* that provide understanding of concrete scientific practice. Finally, the relation of evidential support between history and philosophy is also figured out in different ways. It is quasi-empirical according to Lakatos, *inductive and explanatory* for Stanford, and *iterative* from Chang's point of view. The idea is that diversity of cases increases the confidence in the claim that historical evidence supports philosophical claims, since three different ways of portraying evidential reasoning in HPS point towards the same general conclusion.

A final aspect is that the works in question are *hard and paradigm cases* vis-àvis the sceptical arguments, as I explained before. The *hardness* of Lakatos' and Stanford's works not only leads to take the sceptical challenge very seriously, but it also gives a very strong reason for the plausibility of my positive theses T1-T2 and the corresponding epistemic desiderata. If one wants to test an epistemological position, the sampling should include those cases that are very risky to be epistemically objectionable. Meanwhile, the *paradigmatic* character of integrated HPS gives good reasons for the plausibility of T3 and the corresponding epistemic desideratum, as long as Chang's approach succeeds in bringing together history and philosophy by embracing non-absolutist, philosophical commitments. Again, if one wants to test an epistemological position, the sampling should offer a representative instance of the epistemological point to be made, which will function as a starting point to articulate and judge other cases as featuring that same point.

With this in view, I would respond to the accusation of cherry-picking and hasty generalisation as follows. The objection is that my case-based argumentative strategy cannot support my epistemological lessons. I might be accused of drawing each positive thesis as a general epistemological claim upon a single case study, of using the conjunction of three cases to support the ampliative conclusion, and of conveniently selecting each case to make my point.

To see why this objection is misleading, notice firstly that my three cases are typical targets of the two sceptical arguments in the sense of having been rejected as fraudulent on their basis. However, I am defeating both arguments by showing that each of these works in HPS does hold the relevant epistemic property, thereby meeting the corresponding desideratum. In this way, my analysis plausibly suggests that new works in HPS —i.e., similar cases that have not been the typical target of scepticism can meet the proposed epistemic desiderata. Thus, my own argument does not involve cherry-picking as long as HPS-critics themselves have arguably selected these three cases in the first place; these are crucial cases for current purposes.

Similarly, my strategy does not fall prey to hasty generalisation because I am not casting the argument in terms of induction. The following conclusion is certainly ampliative but not inductive: there are further instances of evidential reasoning in HPS that meet at least one of these epistemic desiderata, in which historical evidence therefore supports philosophical claims. The point is that this conclusion is a possibility claim that is highly plausible. I am claiming that my positive theses T1-T3 can plausibly be true about other works in HPS, not that such works do meet the proposed epistemic desiderata.

To defend this ampliative proposal, my argument has three worth recalling components. First, it gives a *confutation* as rooted in a proof by counterexample. Second, it states the *possibility claim* that is justified by the positive theses T1-T3 I abstracted from the corresponding case studies, which capture three epistemic properties and desiderata vis-à-vis evidential reasoning in HPS. And third, it involves a threefold plausibility proof to underpin my *ampliative conclusion*. This is how my epistemological lessons are warranted, which helps to judge the actual scope of my case-based strategy. It would suffice to answer questions Q1-Q2.

A second contentious issue related to my case-based strategy concerns the commitments I was adopting. As the epistemology of HPS cannot work in a vacuum, HPS-theorists usually look at extant works in HPS to receive some insight. Forging my case studies to solve the problem posed by the two sceptical arguments, I adopted the so-called *(meta-)alternation* as a methodological approach. This is a pivotal component of relativism (Collins and Yearley 1992, p. 301) and pluralism (Chang 2012, p. 265). To handle research problems, HPS-practitioners can learn to alternate between different scientific perspectives. Similarly, HPS-theorists can learn to alternate not only between multiple works in HPS, but also across other academic fields that are found in the vicinity.

My case-based argumentative strategy was an exercise of alternation. I forced myself to switch from Lakatos through Stanford to Chang, and from history of science through philosophy of science to analytic epistemology, back and forth. This painstaking alternation ultimately informed how evidential reasoning in HPS was conceptually characterised and epistemically defended here.

I emulated Stanford's strategy of historical ostension in elucidating evidential reasoning in HPS. Rather than providing definitions in terms of necessary and sufficient conditions, I have drawn upon my case studies to characterise via ostension what it is to employ historical case studies as evidence that supports philosophical claims. As a result, I portrayed some family-resemblance aspects of the works I examined, which squares with a very general yet useful enough idea of "evidence" already proposed by epistemologists (e.g., Conee and Feldman 1985, 2004) and general philosophers of science (e.g., Achinstein 2001; Haack 1993; Kosso 1992). This notion encapsulates a key aspect of evidential reasoning in HPS, namely that *historical studies play a justificatory role for philosophical theorising*. In turn, this general characterisation is informative in being consistent with the particular views of Lakatos, Stanford and Chang about evidential reasoning in science and HPS alike.

Of course, I am not suggesting that traditional philosophical analysis lacks value. Instead, my point is that, for current purposes, there was no need for doing explication work regarding concepts like "historical case study", "evidential support", and "philosophical theory". Likewise, it was not necessary to delve into debates regarding the possibility of historical knowledge or the nature of epistemic justification, to name just two examples. Furthermore, analytic epistemology was primarily useful here to formulate more precisely the central theses and arguments being discussed; this approach facilitated the covering and understanding of the issues at stake much better.

In parallel with emulating Stanford to grasp the generality of evidential reasoning in HPS, I followed Chang's pluralism to highlight the specificity of this reasoning in each case study. My reconstruction of the two sceptical arguments showcases the relevant and distinct aspects of historical case studies and philosophical theories, which purportedly give room for the problems of vicious circularity and the unsuitability of history. Also, I put an emphasis on the history of early modern science and the Copernican revolution because this is the history that I know best. Assuming a pluralist attitude, I even tried to give an important voice to mainstream historians of science within this debate, whose relationship with HPS-practitioners and philosophers of science has been uncollaborative (if not hostile) for decades. A quick dismissal of those historians' reasons is far from being pluralist.

Furthermore, I attempted to understand and evaluate each work in HPS in its own terms as much as possible. This is particularly salient in my case study of Chang's work, where I switched to pragmatist epistemology in order to diagnose and resolve the inherent tension of integrated HPS. Although activist realism is not certainly the only non-absolutist philosophy of science on the table, this philosophical theory can nonetheless handle *metaphysical unsuitability* in a very coherent, comprehensive way. I think this is the conclusion that epistemic pluralists \hat{a} la Chang should arrive at.

At least tacitly, I also incorporated Lakatos' idea of constructive criticism in framing my discussion of the two sceptical arguments. Constructive criticism proceeds by rejecting positions and offering better alternatives. The history of epistemology can justifiably be seen as the attempts to say something *affirmative* about the nature of knowledge and rational belief, but only by *denying* scepticism —i.e., the thesis that knowledge and rational belief are impossible to achieve. Similarly, I attempted to vindicate the epistemic status of evidential reasoning in HPS by defusing the sceptical arguments. This constructive criticism arguably closes the door to the *normative thesis*, whilst shedding light on those qualities of evidential reasoning in HPS that makes this practice epistemically valuable. Surely, this doctoral thesis would critically contribute to recent debates about the use of historical case studies in the philosophy of science, and those concerning the feasibility of integrating history and philosophy of science more generally.

All in all, these pages are how alternation looks like in practice, showing the extent to which this methodological approach is fruitful for self-reflecting upon HPS. It is hard to disagree with sociologists when they note that promiscuity is a receipt for education (Collins and Yearley 1992, p. 302). I would however add that promiscuity also benefits enquiry itself. Like Chang (2012, p. xxi), I believe that "disciplinary and professional boundaries are not important to me" either.

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Appendix A

Tables of Chapter 4

Theory-ladenness of Stanford's historical study											
X _i	Philosophical term (and	Darwin		Galton		Weismann		Study			
	cognates)	f _i	n _i	fi	n _i	f _i	n _i	$\sum f$	$\sum n$		
1	(Un)conceived	69	18,3	50	19,1	132	24	251	16,8		
2	(Dis)confirmation/elimination	12	3,17	12	4,58	33	5,99	57	3,82		
3	Alternative/posibility	61	16,1	60	22,9	157	28,5	278	18,6		
4	Evidence	12	3,17	12	4,58	14	2,54	38	2,55		
5	Experiment	11	2,91	33	12,6	5	0,91	49	3,29		
6	Explanation	46	12,2	12	4,58	16	2,9	74	4,96		
7	Failure	26	6,88	17	6,49	42	7,62	85	5,7		
8	Hypothesis	20	5,29	30	11,5	5	0,91	55	3,69		
9	Mechanism	17	4,5	6	2,29	24	4,36	47	3,15		
10	New induction	7	1,85	1	0,38	3	0,54	11	0,74		
11	Scientific community	3	0,79	3	1,15	10	1,81	16	1,07		
12	Scientific realism	1	0,26	1	0,38	1	0,18	3	0,2		
13	Seriousness	20	5,29	16	6,11	16	2,9	52	3,49		
14	Theorists	15	3,97	0	0	15	2,72	30	2,01		
15	Theory	58	15,3	9	3,44	78	14,2	145	9,73		
	Total terms (M)	378	100	262	100	551	100	1491	100		
Т	otal data (graphical forms)	2905		2312		2682		7899			
	Total pages (interval)	29 (51-79)		25 (80-104)		36 (105-140)		90 (51-140)			

Table 1. Frequency data:



Fig. A.1 The table lists (X) the key philosophical terms Stanford used in reconstructing the historical cases of NIB, showing the number of times each term occurs according to absolute frequency (f), relative frequency (n), and accumulated values (Σ) . **CRATILO®**

Table 2. Historical material:

	Historical sources of the new inductive base								
Primary sources						Secondary sources			
Historical	Unpublis	shed work		Publishe	d work				
actor	Archive	Quotation	Original texts	Quotation	Translations	Quotation	Scholarship	Quotation	
	Cambridge University Library [L] D. to Hooker	~ [Ch. 3] 67 (n.12)	VAP [1868, 1st American edition]	[Ch. 3] 61, 62, 63, 64, 69, 70, 71, 72 (n.7,14,16) [Ch. 4] 81, 87 (n.3,5)	-	~	Bowler 1989	[Ch. 3] 52, 53, 54, 60, 62 (n.6,22) [Ch. 5] 107, 119 (n.1,4,8,18)	
			MLD [1903, F. Darwin & A. Seward (eds.)]	[Ch. 3] 14, 19 (n.9)			Bulmer 2004	[Ch. 3] (n.4,22)	
			[L] D. to Hooker	[Ch. 3] 61, 70, 71 (n.16)			Callender 1988	[Ch. 3] 62	
			[L] D. to Wallace	[Ch. 3] 66			Coleman 1965	[Ch. 5] 119 (n.1,2,3,5,8,10,19,23)	
			[L] D. to G. Bentham	[Ch. 3] 66			Coleman 1973	[Ch. 3] 56	
			[L] D. to Müller	[Ch. 3] 66, 67, 68				[Ch. 5] 106, 107, 109	
			[L] D. to Hildebrand	[Ch. 3] 67			Cowan 1985	[Ch. 3] 60, 71, 74 (n.18)	
Darwin			[L] D. to De Candolle	[Ch. 3] (n.10)	-		01 100 1000	[Ch. 4] 82 (n.1,24,25)	
	Staatsbibliothek zu Berlin—Preußisc ber Kulnuchesitz	[Ch. 3] 67 (n.12)	[L] D. to Weir	[Ch. 3] 67	-		Churchill 1968	Ch.5 107, 108 (n.1,3,7,9,12,21,29)	
			LD (1887 (1950) E. Dannia	[Ch. 3] /1	-		Churchill 1970	[Ch.5] (n.29)	
			(ed.)]	[Ch. 3]	_		Churchill 1987	[Ch. 3] 62	
			[L] D. to Hooker	[Ch. 3] 64, 66, 67 (n.7,13)	-		Duna 1965	[Ch. 5] 106, 119 (n.22)	
			[L] D. to Huxley	[Ch. 3] 65, 67 (n.7)	-		Dunn 1965	[Ch. 5] 52, 60, 75	
			[L] D. to Gray	[Ch. 3] 66, 67	-		Endershy 2003	[Ch. 3] 69 (n.6)	
			[L] D. to Ogle	[Ch. 3] 67 (n.7)			Gasking 1967	[Ch. 3] 52, 53, 54, 55, 60	
			[L] D. to Lyell	[Ch. 3] (n.6)			Gayon 1998	[Ch. 3] 60, 62 (n.15,16,22)	
	[L] D. to J. V. Car		2002 [?] [L] D. to Huxley	[Ch. 3] 67	1		Geison 1969	[Ch. 3] 60, 69, 71 (n.6,7)	
			BR [1871-1872]	[Ch. 4] 86, 87, 91, 92, 93, 94, 95, 96 (n.11,12,20,21,24)				[Ch. 3] 56	
			TH [1875]	[Ch. 4] 80, 83, 86, 87, 88, 90, 93, 94, 95, 96, 97			Gould 1977	[Ch. 5] 107	
			NI [1889]	(n.2,10,13,14,15,16,18,22) [Ch. 4] 91, 92, 94, 95, 96, 99	-		Hodge 1985	[Ch. 3] 63 (n.22)	
			LLL [1889]	[Ch. 4] 81, 85 (n.2.3.4.8.12)	1		Hodge 1989	[Ch. 3] 62, 63	
			[L] Darwin to G. (nov. 4)	[Ch. 3] 73, 74			Larson 1979	[Ch. 3] 55	
			[L] Darwin to G. (nov. 5)	[Ch. 3] 74			Lenoir 1981	[Ch. 3] 58	
Galton			[L] Darwin to G. (dec. 19)	[Ch. 4] 97			Lenoir 1982	[Ch. 3] 55, 56	
			[L] Darwin to Henrietta	[Ch. 4] 81			Maienschein 1986	[Ch. 3] 52	
			[L] G. to Darwin (apr. 25)	[Ch. 4] 82	_		Mazzolini and Roe 1986	[Ch. 3] 54 (n.2,3)	
			ILLG to Darwin (dec. 18)	ICb. 41.97			Pearson (LLL)	[Ch. 4] 82 (n.4)	
			[L] G. to Darwin (dec. 19)	[Ch. 4] 97, 98			Olby 1963	[Ch. 3] 65 (n.16.25)	
			[L] Darwin in Nature	[Ch. 4] 82, 83			Olby 1979	[Ch. 3] 62	
			[L] Galton in Nature	[Ch. 4] 82			Olby 1985	[Ch. 3] 60, 61, 62 (n.14,16,17,25)	
			1865	[Ch. 3] 72				[Ch. 3] 60, 62, 65, 71, 72, 75	
			1870-1871	[Ch. 4] 81, 83, 84			Robinson 1979	[Ch. 4] 82, 86 (n.2)	
					GP [1892 (1983), W.	[Ch. 5] 107, 108, 109, 111,		[Ch. 5] 106, 108, 119, 130 (n.1,2,5,13,18,26)	
					Newton Parker & H.	112, 113, 114, 115, 116,	Roc 1981	[Ch. 3] 54, 55 (n.3)	
					Essays [1891-2, E. B.	117, 118, 120, 121, 122,	Winther 2000	[[Ch. 3] (n.15)	
Weismann					Poulton, S. Schönland, and A. E.	[Ch. 5]			
					Shipley (eds. & trans.)]				
					1885	[Ch. 5] 106 (n.7,14)			
					1887	[Ch. 5] (n.7)			
					1890	[Ch. 5] (n.5)			
De Vries					1896 1889 [1910, C. S. Gager	[Ch. 5] (n.24) [Ch. 5] (n.18)			
Driesch			1894	ICb. 51 119	(trans.)		1		
Haeckel			1866	[Ch. 5] (n.9)			1		
Hertwig					1896 [P. Chalmers Mitchell	[Ch. 5] 119	1		
Spencer			1864	[Ch. 3] 61	(trans.)]		1		
N- P					1828 [1951, T. H. Huxley	101 11 12 10	1		
Von Baer					(trans.), T. Hall (ed.)]	[Ch. 5] 57, 58			

Fig. A.2 The table classifies all the historical material Stanford employed in reconstructing the historical cases of NIB according to these criteria: primary and secondary sources (unpublished work, published work, and translations);
corresponding authors (historical actor and secondary scholarship); type of text (archive (e.g., Cambridge University Library), books (e.g., VAP), papers (e.g., TH), and letters ([L]); and quotation place ([chapter] page(s)).