Three epistemological desiderata for HPS practitioners

Dubian Cañas

dubiandrescanas@gmail.com

2025 IUHPST Essay Prize

Abstract

In this paper, I outline an epistemology of evidential reasoning in the history and philosophy of science (HPS). Drawing upon some prominent works in HPS as case studies, I formulate three novel epistemological desiderata for using historical case studies as evidence for philosophical claims about science, to wit: independent historical evidence, metahistorical criticism, and disciplinary alignment. These desiderata pick out some epistemic qualities and contribute to the achievement of the primary goal of evidential reasoning, which is to confer justification upon philosophical conclusions on the basis of historical evidence. In his way, my proposed epistemology tackles the "methodological" problem of vicious circularity and the "metaphysical" problem of disciplinary unsuitability that allegedly jeopardise HPS practice, thereby vindicating its positive epistemic status.

1 Introduction

Evidential reasoning in the history and philosophy of science (HPS) consists in using historical case studies as evidence for philosophical claims about science. This methodology has played a pivotal role in recent debates over scientific realism, underdetermination, epistemic relativism, scientific pluralism, experimental reasoning and scientific change, to name just a few. While using historical evidence to support philosophical conclusions is a consolidated practice in HPS, sensible scepticism about evidential reasoning exists. Several philosophers and historians have argued that this practice is epistemically objectionable and must therefore be abandoned. Methodological and metaphysical arguments bolster their

sceptical position. The methodological objection involves a worry about vicious circularity: "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 for the metaphysical objection, sceptics insist on the philosophical unsuitability of the history of science: historical case studies are unable to sustain philosophical claims because "history of science and philosophy of science are intrinsically opposed to one another" (Dresow 2020, p. 1). No doubt, these two lines of criticism pose a serious sceptical challenge to HPS practitioners: if at least one of these objections is correct, it undermines their confidence in employing evidential reasoning when it comes to studying science in both historical and philosophical terms.

In this paper, I outline a novel epistemology of evidential reasoning in HPS that tackles the problems of vicious circularity and disciplinary unsuitability. This epistemology encompasses a triad of epistemological desiderata for HPS practitioners. These desiderata capture some epistemic qualities of evidential reasoning and contribute to the achievement of its primary epistemic goal —i.e., the justification of philosophical claims with historical case studies. I claim that there is a *desideratum of independent historical evidence*, a *desideratum of metahistorical criticism*, and a *desideratum of disciplinary alignment*. These epistemic qualities make evidential reasoning epistemically valuable and constitute a mark of its success.

My proposed epistemology is not an armchair exercise in the methodology of HPS. I suggest that the epistemological desiderata are applied by HPS practitioners and can be found in actual examples of evidential reasoning. To flesh out these desiderata, I draw upon three prominent works in HPS as case studies: Lakatos's historiographical research programmes, Stanford's historical challenge to scientific realism, and Chang's pragmatist philosophy of science. Although such case studies are primarily illustrations of each desideratum, my argument stemmed from an iterative analysis. I have examined *concrete* HPS works in order to articulate desiderata as *abstract* epistemic principles.

In Section 2, I reconstruct the sceptical arguments from vicious circularity and disciplinary unsuitability. Then I offer an abstract formulation of the epistemological desiderata, indicating how they handle the twofold sceptical challenge. In section 3, I show the applicability of the desideratum of independent historical evidence based upon Lakatos's plea for testing philosophical theories against historical evidence. In section 4, I illustrate the

desideratum of metahistorical criticism in light of the selective scientific realism debate concerning the caloric theory of heat between Stanford and Psillos. In section 5, I make a vivid sense of the desideratum of disciplinary alignment by delving into Chang's activist realism and its relation to the historiography of science. In conclusions, I pinpoint the scope of my proposed epistemology and its contribution to current debates on the foundations of HPS.

2 Epistemic scepticism and desiderata

Evidential reasoning is still prevalent in HPS research, whereas sceptical objections against this practice turn out to be sensible and pressing. A variety of current works confidently employ historical case studies as evidence for supporting influential philosophical theses. For instance, Lyons and Vickers (2021) note that "the realism debate ought to be informed by a rich diversity of historical cases" to draw "new lessons concerning the ways in which false theoretical ideas have sometimes led to success, or the ways in which old theoretical ideas 'live on', in a different form, in successor theory" (p. 2). Chang (2012) 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 complementary science: 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" (p. 254). Regarding epistemic relativism, Kusch (2021) states that historical case studies are "taken to support the 'historicist' generalization that justification is invariably local, contingent, and relative" (p. 53). Scientonomy also involves evidential reasoning in studying the dynamics of science: the history of scientific change explains individual historical episodes, provides the theory of scientific change with historical data, and tests the general hypotheses of the theory of scientific change (Barseghyan 2015, p. 75). As a last example, Franklin draws upon some case studies concerning the rational acceptability of experimental results that were not replicated, using this historical evidence to make "the point that replicating an experimental result is not a necessary condition for its acceptability" because "sometimes once is enough for acceptability" (Franklin and Laymon 2021, p. 9).

Of course, sceptics would not be impressed with these examples. They complain that evidential reasoning is epistemically objectionable and should therefore be abandoned. Thus, as Giere (1973) noted, HPS practitioners "will not be content merely to practice their art but

will make repeated efforts to explain and argue the rationale of their approach" (p. 291). Sceptics insist that evidential reasoning is beset by a couple of intractable problems that make this practice fraudulent, namely vicious circularity and disciplinary unsuitability.

The methodological objection from vicious circularity states that historical evidence does not support philosophical claims because historical case studies are dependant on philosophical theory:

The vicious circularity argument:

(i) For historical evidence to support philosophical claims, historical case studies must be independent of the philosophical claims being supported. [Epistemic condition.]

(ii) Historical case studies are laden with philosophical theory. [Sceptical claim.]

(iii) Given that historical case studies are laden with philosophical theory, historical case studies are not independent of the philosophical claims being supported. [Sceptical hypothesis.]

(iv) Therefore, historical evidence does not support philosophical claims. [Via (i)-(iii).]

Bolinska and Martin (2020) clarify specific ways in which historical case studies hinge on philosophical theory. They show that HPS practitioners invariably rely upon philosophical commitments in the construction, selection, interpretation, and application of historical case studies as evidence for philosophical claims. This 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 circular reasoning would make HPS practitioners "able to prove absolutely anything, however intuitively unjustifiably" (Boghossian 2001, p. 11). Another consequence is that evidential reasoning is *dialectically ineffective*. For Pitt (2001), it is not clear "how to relate the history to the philosophical point without begging the question" (p. 374). Permitting circular reasoning implies that I cannot persuade you that my historical case study supports my favoured 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).

In this line, Kinzel (2015, 2016) offers an argument from *historical pluralism* for concluding that historical evidence is unable to resolve HPS disagreements. This argument states that history-writing depends upon philosophical theory and that more than one historical reconstruction of the same scientific episodes exists, whereby there is no neutral way of deciding between competing philosophical positions and the corresponding historical accounts. Given that historical case studies are laden with rival philosophical theories, there is no shared evidential base and philosophical conflicts cannot therefore be *empirically adjudicated*. Kinzel also argues that disagreements cannot be *non-empirically adjudicated*, in the sense of appealing to neutral and strong criteria besides historical evidence. She rejects philosophical criticism because prior philosophical commitments "themselves are theory-laden" and therefore "highly contested issues" (Kinzel 2015, p. 55). Likewise, she takes issue with metahistorical criticism on the grounds that historiographical standards "are too weak to settle all historiographical conflicts", so "historical case studies typically cannot settle philosophical conflicts" (Kinzel 2015, p. 55).

Moreover, the metaphysical objection from disciplinary unsuitability states that historical case studies cannot be used as evidence for philosophical claims because history is inappropriate for philosophy:

The disciplinary unsuitability argument:

(v) For historical evidence to support philosophical claims, history of science must be suitable for philosophical theorising. [Epistemic condition.]

(vi) History and philosophy are fundamentally incompatible. [Sceptical claim.]

(vii) Due to the incompatibility between history and philosophy, history of science is not suitable for philosophical theorising. [Sceptical hypothesis.]

(viii) Therefore, historical evidence does not support philosophical claims. [Via (v)-(vii).]

Several authors have suggested that history and philosophy are intrinsically opposed to one another because historians and philosophers adopt conflicting, fundamental commitments about science that frame how each discipline is conducted. This disciplinary conflict is couched in terms of a tension between scientific absolutism vs. non-absolutism. Absolutism encompasses essentialism, universalism, and theoreticism about science, and non-absolutism

involves contingentism, localism, and practicalism about science. Thus, philosophical analysis is essentialist, universalist, and theoreticist, whereas historical analysis is contingentist, localist, and praticalist.

These two sets of metaphysical commitments are in conflict with one another in different ways. The *modality conflict* between history and philosophy concerns the question of whether science has properties that are temporally 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 across different contexts. Finally, the *quality conflict* underlies the issue of whether representational products of science (e.g., concepts, beliefs, theories, etc.) are the only relevant unit of analysis for explaining science. Consequently, the modality conflict entails 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). The quantity conflict underwrites the view 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). And the quality conflict leads to the conclusion 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). As a result, a tension between fundamental disciplinary principles emerges, which makes historical case studies ill-suited to sustain philosophical claims.

The arguments from vicious circularity and disciplinary unsuitability call into question the feasibility of the general goal of evidential reasoning. The former intends to undermine the epistemic condition of the independence of evidence for historical case studies, and the latter targets the epistemic condition of disciplinary compatibility for the history-philosophy relation. In what follows, however, I argue that three epistemological desiderata are conducive to the primary goal of evidential reasoning; they lead evidential reasoning to fulfil the epistemic conditions of evidential independence and disciplinary compatibility. By applying these desiderata, HPS practitioners can therefore close the door to the twofold sceptical challenge. An abstract formulation of each desideratum is the following:

(D1) *Independent historical evidence:* Historical case studies satisfy types of independent

evidence when they support philosophical claims.

(D2) *Metahistorical criticism:* Standards of historical adequacy facilitate the evaluation of historical case studies in order to settle philosophical disagreements.

(D3) *Disciplinary alignment:* Non-absolutist philosophies of science are concordant with the historiography of science and therefore enable bringing together history and philosophy.

These desiderata pick out some *epistemic qualities* of evidential reasoning. I maintain that there are types of independent historical evidence, historiographical standards for criticising case studies that adjudicate philosophical disputes, and philosophical theorising quite compatible with historical research. Such qualities makes evidential reasoning *epistemically valuable* and constitute a mark of its *success*. Furthermore, these desiderata make the twofold sceptical challenge tractable. I show how desiderata D1 and D2 allow HPS practitioners to counter the vicious circularity argument in two different ways, whereas desideratum D3 allows them to reject the disciplinary unsuitability argument.

I now turn to consider what these epistemological desiderata amount to in action. By employing three prominent HPS works as case studies, I illustrate how HPS practitioners apply the epistemological desiderata and their works exhibit the corresponding epistemic quality.

3 (D1) Independent historical evidence

This desideratum is articulated upon Lakatos's view of evidential reasoning in terms of historiographical research programmes. My case-based analysis sketches a typology of independent historical evidence that is informed by how Lakatos uses historical case studies to assess normative methodologies of science —i.e., philosophical theories of scientific rationality. Lakatos classifies historical case studies into two classes. First, they are *rational reconstructions* when historical episodes are selected and interpreted in terms of some methodology of science. Second, some case studies amount to *actual history of science* when episodes are written in terms of "what really happened in the past". These two classes permit HPS practitioners "to criticise both one's rational reconstruction for lack of historicity and the actual history for lack of rationality" (Lakatos 1978a, p. 53). Furthermore, methodologies of

science are evaluated on the rational reconstructions they generate, where the best methodology can save the largest number of historical episodes as rational (Lakatos 1976, pp. 1, 31).

At first glance, sceptics might object that Lakatos's proposal is afflicted by vicious circularity. First, rational reconstructions are laden with methodologies of science (Lakatos 1976, n. 60). Second, a plurality of competing methodologies of science exists —e.g., inductivism, conventionalism, falsificationism, and the methodology of scientific research programmes (Lakatos 1976, p. 2; 1978b, pp. 191-2). It follows that methodologies cannot be critically compared with historical case studies. On careful examination, however, this accusation is uncharitable. Looking at how Lakatos employs the history of the Copernican revolution to critically compare rival methodologies, I find different forms in which historical case studies are independent of the philosophical theories being supported, considering how rational reconstructions of episodes relate to the actual history of science. There are at least three forms of evidential independence that are relative to both the *kinds of theory* used to produce historical case studies and how they *interact* with one another. The first variety of independent evidence can be characterised as follows:

Type-independent evidence: Historical case studies are theory-laden, but not laden with methodologies of science.

While historical reconstructions are theory-laden, much of the history of science is not laden with philosophies of science. In Lakatos's terminology, not all historical cases are written with theories of rationality; their reconstruction does not entail any methodology. Case studies that are not methodology-laden typically come from what he calls professional historiography of science. These case studies are relevant to assess methodologies of science insofar as they permit HPS practitioners to see that methodologies are historically inadequate. Lakatos uses *type-independent evidence* to show that history of science does not bear out rival methodologies.

There are plausible reasons for thinking that case studies from professional historiography constitute *type-independent evidence*, including those Lakatos uses. For one thing, mainstream historians have explicitly argued that they do not (and should not) employ philosophical theories in history-writing. Shapin (1982), for instance, states that "it would

be quite incorrect to regard empirical literature as if it were merely a 'testing' of some theoretical programme", adding that "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). I. B. Cohen (1974) criticises the assumption that philosophical theorising is helpful for historians: "I personally find great difficulty in ascertaining which books or articles dealing with the philosophy of science may be useful for historical inquiries" (p. 312). To figure out the alleged necessity of philosophy for history "seems well beyond the philosophical capacities of almost all historians" (pp. 313). Finally, Rossi (1986) explicitly rejects the use of theories of rationality as conceptual frameworks, noting that "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" (p. 208). For him, historians are fundamentally interested in studying "temporal processes rather than 'logical substitutes'" (Rossi 1986, p. 209). 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).

This general aspect of historiography aside, it is the fact that the works of professional historians that Lakatos appeals to are not rational reconstructions. Rather, Lakatos uses them as if they were providing information about the actual history of science, against which rational reconstructions are criticised for lack of historical adequacy. Consider some examples from his co-authored case study of the Copernican revolution (Lakatos and Zahar 1973).

Lakatos claims that *verificationism* cannot explain the Copernican episode 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" (Lakatos and Zahar 1973, p. 357). He appeals to Gingerich's (1973, as cited in Lakatos and Zahar 1973, p. 357 n. 7) paper to support this critical judgement. 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" (Lakatos and Zahar 1973, p. 362). The historical studies he uses include Price's and Ravetz's articles (Price 1959, as cited in

Lakatos and Zahar 1973, p. 362, and Lakatos 1976, p. 27 n. 102; Ravetz 1965, as cited in Lakatos and Zahar 1973, p. 363 n. 30), as well as I. B. Cohen's, Koestler's, and Dreyer's big narratives (Cohen 1960, as cited in Lakatos 1976, p. 28 n. 103; Koestler 1959; Dreyer 1953, as cited in Lakatos 1978a, 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" (Lakatos and Zahar 1973, p. 358), thus suggesting that "the alleged superiority of Reinhold's Prutenic tables over the Alfonsine ones could not provide the crucial test" (Lakatos and Zahar 1973, p. 359). Likewise, Lakatos points out that *elitism* fails to explain the heliocentric achievement because Kuhn's reconstruction makes 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" (Lakatos and Zahar 1973, p. 366 n. 43).

To favour his methodology of research programmes, Lakatos focuses on the equant's elimination as one indicator of progressive problemshift in Copernicus's theory. Ptolemy introduced the equant 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" (Lakatos and Zahar 1973, p. 374). Lakatos supports his verdict with some passages from Copernicus's *Commentariolus* and Neugebauer's (1968, as cited in Lakatos and Zahar 1973, pp. 374, 378) historical account, which was the canonical interpretation of the Copernican revolution in professional historiography at that time (Swerdlow 1973).

In *type-independent evidence*, the theory reconstructing the historical episode and the methodology under test are quite independent because the case study is not a rational reconstruction. This breaks the vicious circularity in evidential reasoning. Since the historical reconstruction is not based upon the *type theory* being tested, competing rational reconstructions can be ranked using historical case studies that are not methodology-laden, thus making it possible to adjudicate between competing methodologies in terms of maximising rational explanations of past scientific episodes.

Suppose now a situation in which no *type-independent evidence* is available to support

theories of rationality —i.e., when HPS practitioners intend to test methodologies of science in light of rational reconstructions exclusively. In this case, HPS practitioners can use rational reconstructions that do not depend upon the methodology being tested, but upon a different methodology. This involves the following second form of independent evidence:

Token-independent evidence: Historical case studies are laden with methodologies of science, but the methodology that reconstructs historical episodes is not the methodology being tested.

As an example, consider Lakatos's recourse to Kuhn's (1957) study of Copernicus to bolster the claim that the Copernican revolution was not the result of theory choice in terms of simplicity. Lakatos points 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'" (Lakatos 1976, 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* prevents the relation of evidential support from being viciously circular. Since the rational reconstruction is not based upon the *token theory* being tested, methodologies can be critically compared with historical case studies that are methodology-laden. Remarkably, HPS practitioners can also reinforce the epistemic support that such rational reconstructions confer upon methodologies by appealing to historical case studies that are not methodology-laden. This involves another way in which *token-independent evidence* and *type-independent evidence* interact with one another, which captures this third form of independent evidence:

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: "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 being tested is found to be *robust*.

a. Historical calibration. This strategy consists in taking a *historical* reconstruction that does not depend upon any methodology of science as a surrogate for a *rational* reconstruction supporting the theory of rationality under test. For instance, Lakatos and Zahar's (1973) case study of Copernicus can be calibrated by appealing to current historical scholarship. Historians (Goldstein 2002; Westman 2011) have recently argued that Copernicus aimed to solve the problem of planetary order rather than to eliminate the heuristics of equant points. After all, geocentric models dispensing with equants were commonly designed and used by Muslim astronomers in the early 13th century, and Copernicus did equip his heliocentric modelling with some equants in the technical books of *De Revolutionibus*. Goldstein (2002) demonstrates that "the equant was an astronomical problem whose solution did not impinge on cosmological issues" (p. 220), and its elimination "was done only after he [Copernicus] made an initial commitment to a heliocentric system" (p. 221).

This updated account contradicts Lakatos's 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" (Lakatos and Zahar 1973, 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's reconstruction fails the test from historical calibration.

Consider now what happens with Zahar's reconstruction. This rests on a modified version of Lakatos's theory of rationality, which reformulates 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's astronomy predicts planetary order by determining the distance-period relationship among planets, arguing that this prediction meets conditions (i)-(iii). Lakatos points out that "Zahar's account thus explains Copernicus' achievement as constituting genuine progress compared with Ptolemy" (Lakatos and Zahar 1973, p. 380).

Zahar's reconstruction agrees with current historiography in that Copernicus solved the problem of planetary order. However, Goldstein (2002) claims that "Copernicus argued explicitly against the Ptolemaic system as violating the distance-period 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.

b. 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 under 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. While Kuhn's *elitism* presumably qualifies as a theory of rationality, Gingerich's theoretical commitments do not. Gingerich's (1975) reconstruction is instead a case in point of conceptual history: "What has struck Copernicus is a new cosmological vision, a grand aesthetic view of the structure of the Universe" (p. 90). This conceptual account contradicts Kuhn's elitist reconstruction in that the Copernican revolution was not brought about by a paradigm-crisis: "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) Despite this, Gingerich's rendering agrees with Kuhn about another particular conclusion relevant to *conventionalism*, namely that Copernican astronomy was not simpler (Gingerich 1975, pp. 87-8; Kuhn 1957, p. 169).

The inadequacy of *conventionalism*, then, is grounded in an argument of historical robustness —which involves one historical reconstruction that is methodology-laden and another 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. Consider again my foregoing negative assessment of Lakatos and Zahar's case study. This verdict becomes robust relative to both Goldstein's (2002) and Westman's (2011) reconstructions of Copernicus. 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 introduces social factors for explaining the Copernican episode as an answer to the problem of planetary order. 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). Copernicus's theory facilitated the restoration of 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).

Goldstein and Westman therefore 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. This is a robustness argument *against* Lakatos's theory of rationality. However, it could also be a robustness argument *for* a different theory. 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 would provide a positive assessment for a methodology defending that eliminative inference is a cornerstone of scientific reasoning (e.g., McCain and Kampourakis 2020).

The lesson from my case study of Lakatos's work is that rational reconstructions can be independent of the methodology of science being tested in several ways. This is an epistemic quality of Lakatos's evidential reasoning that is well encapsulated by the desideratum of independent historical evidence. This desideratum vindicates the use of historical case

studies to *empirically adjudicate* disagreements between philosophical claims, thus blocking the sceptical hypothesis of the vicious circularity argument —i.e., the theory-ladenness of historical case studies entails that they are not independent evidence.

4 (D2) Metahistorical criticism

What about the sceptical contention that disagreements cannot be *non-empirically adjudicated* on philosophical and metahistorical grounds? Kinzel objects that additional philosophical arguments and historiographical standards are unable to settle disagreements. Against this view, Bolinska and Martin (2020) suggest how philosophical arguments give an antecedent justification of prior philosophical commitments that breaks vicious circularity. In this section, I show how metahistorical criticism also serves this aim. The second epistemological desideratum highlights the role of historiographical standards for evaluating the quality of historical evidence in order to settle philosophical conflicts. These standards permit HPS practitioners to rank conflicting philosophical positions in terms of the historical adequacy of case studies.

I articulate this desideratum with Stanford's historical critique of Psillos's reconstruction of the caloric theory of heat in the context of the selective realism debate. Following Kinzel, sceptics points out that historical accounts of caloric are biased by the philosophical positions at stake (Vickers 2017, pp. 49-50), thus contending that historiographical standards cannot resolve the disagreement between Stanford and Psillos: "the mere existence of shared standards does not yield determinate answers regarding which side in the dispute about caloric is correct" (Chakravartty 2017, p. 27). Instead, I show here how historiographical standards play a crucial role in adjudicating this dispute in terms of the quality of the historical reconstructions of the caloric episode. My case-based argument puts forward the following points. First, there is a standard of diachronical historical adequacy in the mainstream 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 vis-à-vis the competing philosophical positions about caloric. This standard is implied by the fact that selective realism must be tested against past scientists' judgements of selective confirmation. Third, when this standard is applied, the textual material suggests that scientists' judgements of selective confirmation about caloric 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 Stanford's non-realist position rather than Psillos's selective realism.

a. Independence

Historical adequacy is the primary value of historical case studies. Historical research aims to provide factual knowledge about past science and philosophical conclusions should rely upon historical facts rather than upon "imagined history", as I. B. Cohen (1974) calls it. If historical research intends to tell narratives of "what really happened in the past", then good historical case studies must meet some historiographical criteria underwriting historical adequacy. These criteria are encapsulated by the following standard:

Historical adequacy: To obtain evidence from good historical reconstructions, historians must reconstruct the historical situation in its original context by carefully attending to the available textual material.

This standard presupposes three salient elements. The first element is *diachronical 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. Kragh (1987) notes that "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" (p. 90).

The second element involves a *rejection of 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). 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 arguably adopt the diachronical perspective. 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). Diachronical analysis is a natural way of making historical investigations worth pursuing. As I. B. Cohen (1974) argues, presentism ultimately denies "the reason for studying history in the first place" (p. 349).

The third element focuses on the *critical use of textual evidence* in diachronical analysis. Since historians want to understand historical actors' thoughts and actions, the most direct source of information is to be found in what those actors have registered in a textual form. Obviously, this does not exclude other types of historical sources. For Kragh, using available textual evidence permits picking out the relevant explanatory variables to understand the historical situation in its original context and integrity. Otherwise, "much history of science commits anachronistic sins by streamlining and clarifying past thoughts far beyond what is justified by textual evidence" (Kragh 1987, 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 (Ashplant and Wilson 1988).

Thus, historical adequacy conveys some generally agreed-upon historiographical criteria in the mainstream historiography of science. This standard calls for a diachronical 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.

b. Relevance

Historical adequacy is also relevant to HPS. In this case, the standard determines the extent to which competing renderings of historical cases can support selective realism, indicating which historical reconstruction of the same episode is more (in)adequate in historical terms.

Roughly, selective realists seek to establish a criterion for identifying the "central core" of scientific theories, compounded by elements of past theories responsible for empirical

success and retained through theory change. However, they must have a principled way of individuating such a "central core" that is to be independent of what has been preserved until now. If realists make a retrospective judgement based upon what we know today, they adopt an anachronistic and teleological historiographical perspective in interpreting past theories. Realists are prompted to take 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. Second, selective realism begs the question, since retrospective judgements take it for granted that current scientific theories are successful and hence approximately true.

Both Psillos and Stanford acknowledge that the evidential support historical evidence provides to selective realism must avoid the trivial historical retention and the commitment to the truth of current theories. Psillos (1999) claims that "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", since past "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" (p. 107). He suggests that realists must find a criterion for selective confirmation in past scientific judgements, which requires scrutinising the available textual evidence. Similarly, Stanford (2006) thinks that retrospective judgements 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). Stanford (2006) thus demands realists 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 (p. 168).

Thus, Psillos and Stanford would perfectly agree on the claim that the success of selective realism implies a diachronical analysis, examining scientists' pronouncements about the epistemic status of their theories carefully. This methodological maneuver boils down to understanding judgements of selective confirmation in terms of historical actors themselves.

c. Application

Consider finally how *historical adequacy* is applied to resolve the selective realism debate concerning caloric. Stanford criticises Psillos's account of this episode by arguing that it is not historically defensible. Yet, he does so for reasons that Psillos would himself concede. Although Stanford celebrates that Psillos's historical method to approach selective realism 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 criticism uses further textual evidence 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 acknowledges the authenticity of the passages from Black and Lavoisier that Psillos cites 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 argues that the epistemic attitude of both actors towards the cause of heat as a material substance was 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 accepted that the caloric theory was more empirically probable than the rival, dynamical theory. However, this does not suffice to believe in the caloric theory because it was facing important problems: it cannot explain some experimental results, provides no complete explanation of heat phenomena, and 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 two theories. Finally, Psillos (1999) points 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 *intellectual context* in which Black made these statements, he 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 argues that Black's cautious epistemic attitude should itself be treated with due caution. For one thing, "Black advocates an official hostility toward all theories and theorizing in general" (Stanford 2006, p. 175). To support 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, it is understandable why Black thought that the available evidence makes more probable the caloric theory, even though experimental method for Newtonians says nothing about the issue of whether the cause of heat is a material substance. This Newtonian commitment is not meant to claim that the caloric theory was not well-supported by empirical evidence as compared to its rival. Stanford (2006) also argues that Black himself "rejected the dynamical theory in light of the evidence" (p. 176). Just as Psillos cites Rumford for illustrating the explanatory limitations of the caloric theory to account for the weight of caloric and the production of heat by friction between solid bodies, Stanford cites Black himself —and McKie and Heathcote's (1935) historical account— to illustrate the explanatory deficits of the dynamical theory to accommodate his "discoveries concerning latent heat" (Stanford 2006, p. 176). On this basis, Stanford concludes that Black's judgement of selective confirmation regarding the dynamical theory features his rejection of the belief in the cause of heat 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's use of Lavoisier's pronouncements. According to Psillos (1999), calorimetry considerations in the *Mémoire sur la Chaleur* (Lavoisier and Laplace 1783/1982) reveal an 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 focuses on the appropriate contextualisation of the passage Psillos cites. Attending to the *context of production and transmission* of the *Mémoire sur la Chaleur* according to its contents and potential audience, Stanford stresses actors' strategies of persuasion to understand why Lavoisier and Laplace opened the *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; see also 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 both scientists nonetheless embraced 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 caloric is belief because the theory is able to explain several phenomena and remains differentially confirmed by the evidence. Complementing with historical scholarship, Stanford shows that for Lavoisier caloric explains chemical combination (*Mémoire sur la Chaleur*), is well-supported by the existing evidence (*Mémoires de Chimie* and Donovan 1993), confirms the oxygen theory rather than the phlogiston theory (Guerlac 1976 and Morris 1972), and is the cause of most of thermic phenomena (*Traité de Chimie*).

Stanford's (2006) general conclusion from his criticism of Psillos's narrative is that "I do not see how the textual evidence can be reconciled with Psillos's 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, p. 119)" (p. 179). He draws this conclusion after doing what *historical adequacy* demands in the context of historical analysis, namely taking into account and critically interpreting all the relevant textual material in the appropriate historical context. While Psillos's rendering of caloric involves a diachronical analysis that rejects a presentist approach to the episode, his narrative falls short of making critical use of sources. For this reason, Psillos's case study is not historically adequate. He thus cannot appeal to this historical case to support his selective realism, because the caloric episode shows that past scientists' judgements about the ontological status of caloric were not reliable and are not therefore prospectively applicable. The historical evidence concerning this episode does not support Psillos's realist approach.

The lesson from my case study of the selective realism debate concerning caloric is that standards of historical adequacy are strong for HPS disagreements to reach a resolution on their basis. The desideratum of metahistorical criticism captures this epistemic quality of Stanford's evidential reasoning that facilitates the *non-empirical adjudication* of disagreements between philosophical claims, which is another way of rebutting the sceptical

hypothesis of the vicious circularity argument.

5 (D3) Disciplinary alignment

The third and final epistemological desideratum stems from exploring how philosophy of science can be compatible with history of science. My claim is that the alleged disciplinary conflict between history and philosophy emerges if the philosophy of science being adopted involves scientific absolutism. In other words, non-absolutist philosophies of science are concordant with the non-absolutism of history. I analyse Chang's (2022) activist realism and how this philosophy gets in close alignment with historiographical practice. My case-based argument discusses activist realism and standard scientific realism, two competing contemporary philosophies of science that differ fundamentally from one another. I show that standard realism embraces absolutism, whereas activist realism is instead committed to non-absolutism. Thus, adopting standard realism creates the philosophical unsuitability of history but activist realism makes historical case studies suitable for sustaining philosophical claims.

a. Standard realism meets absolutism. 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 realists maintain 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). In this view, the world is the truth-maker of scientific theories, meaning 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 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 that 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 the absolutist notions of objectivity, monism, knowability, and optimism, I drew upon 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.

Standard realism 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, say, in Medellín and London. Similarly, the nature of the electron is the same, say, in Cambridge and Singapore, 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 realism is a theory-centred view of science.

Given that standard realism involves essentialism, universalism, and theoreticism about science, this philosophy conflicts with the scientific non-absolutism of historiographical practice. Arabatzis has already noted this problem as confronting those scientific realists who

want to write and take the history of science seriously. 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 practitioners 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).

b. Activist realism meets non-absolutism. Activist realism gives an entirely different image of science in pragmatist terms —where scientific ontology and truth are devoid of absolutist gloss. Roughly, this philosophical theory affirms that science aims at *creating more and better knowledge, thus learning many truths about many realities*. So it 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" (Chang 2022, p. 206). Also, activist realism 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" (Chang 2022, p. 209).

Chang motivates activist realism with the notion of "operational coherence". This is characterised as the *quality* of active knowledge, so the epistemic value of epistemic activities consists in their being operationally coherent. Operational coherence is *aim-oriented coordination* in that "it is a matter of how *we* bring together things and actions in order to achieve our aims" (Chang 2022, pp. 45-6). Additionally, operational coherence amounts to *pragmatic understanding* as "what is *operationally coherent* is what makes sense for us to *do*", that is, "the coherence of an activity *consists* in doing what is sensible to do if one wants to succeed" (Chang 2022, p. 44). The former sense of operational coherence refers to activities being well-designed to achieve an aim, and the latter sense is ascribed to activities that epistemic agents understand as well-designed for success.

So construed, operational coherence provides pragmatist criteria for knowledge, truth, and reality. *Epistemic coherence* evaluates the status of active knowledge and proposes that a system of practice is operationally coherent *iff* its constituent epistemic activities are aim-oriented coordinated and epistemic agents have a pragmatic understanding of this fact. *Ontological coherence* evaluates the referential success of scientific concepts by establishing 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" (Chang 2022, p. 121). Finally, *alethic coherence* evaluates the truth-value of scientific propositions in terms of "truth-by-operational-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" (Chang 2022, p. 167).

Noticeably, this pragmatist conception of scientific knowledge, reality, and truth squares with the fundamental commitments that historical discipline involves. Rather than creating an inherent conflict between history and philosophy, philosophers who advocate activist realism can perfectly agree with historians' non-absolutism, and vice versa. To see this, consider the following conception of non-absolutism regarding 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 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. Chang (2022) states concerning *ontological coherence* that "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 of *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 (2022) believes that scientific knowledge, ontology, and truth are local in the sense of being relative to domains: "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" (p. 147). So operational coherence is a matter of degrees and something that epistemic activities exhibit in some cases but not in others. *Ontological coherence* 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 it is 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. Active knowledge has to do with the practice of science. Scientific research demands coordination among epistemic agents to develop aim-oriented activities that require the use of both theoretical and material resources. It follows that the material culture and social organisation of science are also explanatory central. 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 facilitates to align history and philosophy, provided their sets of fundamental commitments about science are now concordant with one another. Since activist realism adopts contingentism, localism, and practicalism about science, the alleged disciplinary conflict ultimately disappears. Chang

(personal communication) himself thinks that he is proposing a better philosophy of science that historians can safely and productively engage with.

The lesson from this case study is that non-absolutist philosophies of science make historical case studies quite suitable for philosophical theorising. Chang's evidential reasoning holds this epistemic quality as it relies upon activist realism. So this case reveals how the desideratum of disciplinary alignment serves to counter the sceptical claim underlying the disciplinary unsuitability argument, namely that history and philosophy are intrinsically opposed to one another.

6 Concluding remarks

Hitherto I have sketched an epistemology that conveys three epistemological desiderata for evidential reasoning in HPS. This epistemology vindicates the positive epistemic status of this practice by tackling the twofold sceptical challenge to the justificatory role of historical case studies. While I have articulated these desiderata with only three case studies, there are good reasons to expect that they can be found in other HPS works and should be applied by other HPS practitioners.

Firstly, my case-based analysis involves *hard* and *paradigm* cases. Hard cases put epistemic desiderata to the severe test. I take it for granted 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). Lakatos's and Stanford's works can justifiably be seen as hard cases, since sceptics have argued that they both lack the epistemic qualities corresponding to desiderata D1 and D2, respectively. Chang's work is rather a paradigm case, which serves as an exemplar of desideratum D3 in accordance with his prominent effort to engage with *integrated* HPS.

Secondly, my proposed desiderata supply HPS practitioners with abstract *epistemic principles* that make it explicit how to exercise methodological care. For the relation of evidential support not to be viciously circular, HPS-practitioners (P1) *must employ historical case studies that constitute independent historical evidence in the proposed forms*, and they (P2) *must*

critically evaluate historical case studies by applying historiographical standards that enhance a greater sensitivity to the canons of history. Likewise, for historical data to be suitable for philosophical theorising, HPS-practitioners (P3) *must rely their philosophy of science upon non-absolutist fundamental commitments, as long as these cornerstone principles align with those of historiographical practice.* These epistemic principles are conditional judgements 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). A naturalist, hypothetical conception of rationality is adopted here, according to which being "rational" is to use means that are known to be effective in achieving particular aims.

All in all, my proposed epistemology not only clarifies what HPS practitioners already implicitly endorse but also works towards a more explicit and self-aware practice in the field. This epistemological framework would be invaluable for those aiming to employ historical case studies to support philosophical conclusions, ensuring that case studies are epistemically sound and relevant. This epistemology is surely subject to future developments and refinements, and I expect it will enrich fruitful discussion of methodological issues surrounding HPS.

References

Arabatzis, T. (2001). Can a historian of science be a scientific realist? *Philosophy of Science*, *68*(S3), S531–S541. http://www.jstor.org/stable/3080971.

Arabatzis, T. (2018). Engaging philosophically with the history of science: Two challenges for scientific realism. *Spontaneous Generations*, *9*(1), 35–37. https://doi.org/10.4245/sponge.v9i1.27095.

Ashplant, T., & Wilson, A. (1988). Present-centred history and the problem of historical knowledge. *The Historical Journal*, *31*(2), 253–274. http://www.jstor.org/stable/2639213.

Barseghyan, H. (2015). *The Laws of Scientific Change*. Springer.

Bland, S. (2018). *Epistemic Relativism and Scepticism*. Springer.

Boghossian, P. (2001). How are objective epistemic reasons possible? *Philosophical Studies*, *106*(1/2), 1–40. http://www.jstor.org/stable/4321190.

Boghossian, P. (2006). *Fear of Knowledge: Against Relativism and Constructivism*. Clarendon Press.

Bolinska, A., & Martin, J. D. (2020). Negotiating history: Contingency, canonicity, and case studies. *Studies in History and Philosophy of Science Part A*, *80*, 37–46. https://doi.org/10.1016/j.shpsa.2019.05.003

Burian, R. M. (2001). The dilemma of case studies resolved: The virtues of using case studies in the history and philosophy of science. *Perspectives on Science*, *9*(4), 383–404. https://doi.org/10.1162/106361401760375794.

Caneva, K. L. (2011). What in truth divides historians and philosophers of science? In S. Mauskopf & T. Schmaltz (Eds.), *Integrating history and philosophy of science: Problems and prospects* (pp. 49–57). Springer. https://doi.org/10.1007/978-94-007-1745-9.

Carter, J. A., & McKenna, R. (2021). Absolutism, relativism and metaepistemology. *Erkenntnis*, *86*(5), 1139–1159. https://doi.org/10.1007/s10670-019-00147-w.

Chakravartty, A. (2017). *Scientific Ontology: Integrating Naturalized Metaphysics and Voluntarist Epistemology*. Oxford University Press. https://doi.org/10.1093/oso/9780190651459.001.0001

Chang, H. (2003). Preservative realism and its discontents: Revisiting caloric. *Philosophy of Science*, *70*(5), 902–912. https://doi.org/10.1086/377376.

Chang, H. (2012). *Is Water H2O? Evidence, Realism and Pluralism*. Springer. https://doi.org/10.1007/978-94-007-3932-1

Chang, H. (2022). *Realism for realistic people: A new pragmatist philosophy of science*. Cambridge University Press. https://doi.org/10.1017/9781108635738.

Cohen, I. B. (1960). *The birth of a new physics*. Anchor Books Doubleday.

Cohen, I. B. (1974). History and the philosopher of science. In F. Suppe (Ed.), *The structure of scientific theories* (pp. 308–349). University of Illinois Press.

Dear, P. (1995). *Discipline and experience: The mathematical way in the scientific revolution*. University of Chicago Press.

Dear, P. (2011). Philosophy of science and its historical reconstructions. In S. Mauskopf & T. Schmaltz (Eds.), *Integrating history and philosophy of science: Problems and prospects* (pp. 67–82). Springer. https://doi.org/10.1007/978-94-007-1745-9.

Donovan, A. (1993). *Antoine lavoisier: Science, administration and revolution*. Cambridge University Press.

Dresow, M. (2020). History and philosophy of science after the practice-turn: From inherent tension to local integration. *Studies in History and Philosophy of Science*, *82*, 57–65. https://doi.org/10.1016/j.shpsa.2020.01.001.

Dreyer, J. L. E. (1953). *A history of astronomy from thales to kepler*. Courier Corporation.

Franklin, A. (1997). Calibration. *Perspectives on Science*, *5*(1), 31–80. https://doi.org/10.1162/posc_a_00518

Franklin, A., Anderson, M., Brock, D., Coleman, S., Downing, J., & Gruvander, A. e. a. (1989). Can a theory-laden observation test the theory? *The British Journal for the Philosophy of Science*, *40*(2), 229–231. http://www.jstor.org/stable/68751.

Franklin, A., & Laymon, R. (2021). *Once can be enough*. Springer.

Fuller, S. (1991). Is history and philosophy of science withering on the vine? *Philosophy of the Social Sciences*, *21*(2), 149–174. https://doi.org/10.1177/004839319102100201.

Giere, R. N. (1973). History and philosophy of science: Intimate relationship or marriage of convenience? *The British Journal for the Philosophy of Science*, *24*(3), 282–297. https://doi.org/10.1093/bjps/24.3.282.

Giere, R. N. (2011). History and philosophy of science: Thirty-five years later. In S. Mauskopf & T. Schmaltz (Eds.), *Integrating history and philosophy of science: Problems and prospects* (pp. 59–65). Springer. https://doi.org/10.1007/978-94-007-1745-9.

Gingerich, O. (1973). The Copernican Celebration. *Science Year*, *1973*, 266–67.

Gingerich, O. (1975). "Crisis" versus aesthetic in the Copernican Revolution. *Vistas in Astronomy*, *17*, 85-95. https://doi.org/10.1016/0083-6656(75)90050–1.

Goldstein, B. R. (2002). Copernicus and the origin of his heliocentric system. *Journal for the History of Astronomy*, *33*(3), 219–235. https://doi.org/10.1177/002182860203300301.

Guerlac, H. (1976). *Chemistry as a branch of physics: Laplace's collaboration with lavoisier*. Princeton University Press.

Henry, J. (2002). *The scientific revolution and the origins of modern science*. Bloomsbury Publishing.

Kinzel, K. (2015). Narrative and evidence: How can case studies from the history of science support claims in the philosophy of science? *Studies in History and Philosophy of Science Part A*, *49*, 48–57. https://doi.org/10.1016/j.shpsa.2014.12.001.

Kinzel, K. (2016). Pluralism in historiography: A case study of case studies. In T. Sauer & R. Scholl (Eds.), *The philosophy of historical case studies* (pp. 123–149). Springer. https://doi.org/10.1007/978-3-319-30229-4-7.

Koestler, A. (1959). *The sleepwalkers: A history of man's changing vision of the universe*. Hutchinson.

Koyré, A. (1963). Commentaries (problems in the historiography of science). In A. C. Crombie (Ed.), *Scientific change; historical studies in the intellectual, social, and technical conditions for scientific discovery and technical invention, from antiquity to the present* (pp. 847–857). Heinemann.

Kragh, H. (1987). *An introduction to the historiography of science*. Cambridge University Press.

Kuhn, T. S. (1957). *The copernican revolution: Planetary astronomy in the development of western thought*. Harvard University Press.

Kuhn, T. S. (1977). The relation between the history and the philosophy of science. In *The essential tension: Selected studies in scientific tradition and change* (pp. 3–30). University of Chicago Press.

Kusch, M. (2021). *Relativism in the philosophy of science*. Cambridge University Press. https://doi.org/10.1017/9781108979504.

Kuukkanen, J.-M. (2016). Historicism and the failure of HPS. *Studies in History and Philosophy of Science Part A*, *55*, 3–11. https://doi.org/10.1016/j.shpsa.2015.08.002.

Lakatos, I. (1976). History of science and its rational reconstructions. In C. Howson (Ed.), *Method and appraisal in the physical sciences* (pp. 1–39). Cambridge University Press.

Lakatos, I. (1978a). Falsification and the methodology of scientific research programmes. In J. Worrall & G. Currie (Eds.), *The methodology of scientific research programmes. philosophical papers (vol. 1)* (pp. 8–101). Cambridge University Press.

Lakatos, I. (1978b). A postscript on history of science and its rational reconstructions. In J. Worrall & G. Currie (Eds.), *The methodology of scientific research programmes. philosophical papers (vol. 1)* (pp. 189–192). Cambridge University Press.

Lakatos, I., & Zahar, E. (1973). Why did Copernicus' research program supersede Ptolemy's? In R. Westman (Ed.), *The copernican achievement* (pp. 354–383). University of California Press.

Laudan, L. (1989). Thoughts on HPS: 20 years later. *Studies in History and Philosophy of Science Part A*, *20*(1), 9–13. https://doi.org/10.1016/0039-3681(89)90030-7.

Laudan, L. (1990). The history of science and the philosophy of science. In R. Olby, G. Cantor, J. Christie, & J. Hodge (Eds.), *Companion to the history of modern science* (pp. 47–59). Routledge.

Laudan, L., & Laudan, R. (2016). The re-emergence of hyphenated history-and-philosophy-of-science and the testing of theories of scientific change. *Studies in History and Philosophy of Science, Part A*, *59*, 74–77.https://doi.org/10.1016/j.shpsa.2016.06.009.

Lyons, T. D., & Vickers, P. (2021). *Contemporary scientific realism: The challenge from the history of science*. Oxford University Press. https://doi.org/10.1093/oso/9780190946814.001.0001

McCain, K., & Kampourakis, K. (2020). *What is scientific knowledge? an introduction to contemporary epistemology of science*. Routledge. https://doi.org/10.4324/9780203703809.

Miller, D. M. (2011). The history and philosophy of science history. In S. Mauskopf & T. Schmaltz (Eds.), *Integrating history and philosophy of science: Problems and prospects* (pp. 29–48). Springer. https://doi.org/10.1007/978-94-007-1745-9.

Morris, R. J. (1972). Lavoisier and the caloric theory. *The British Journal for the History of Science*, *6*(1), 1–38.

Neugebauer, O. (1968). On the planetary theory of Copernicus. *Vistas in Astronomy*, *10*, 89–104.

Pinnick, C., & Gale, G. (2000). Philosophy of science and history of science: A troubling interaction. *Journal for General Philosophy of Science / Zeitschrift für allgemeine Wissenschaftstheorie*, *31*(1), 109–125. http://www.jstor.org/stable/25171167.

Pitt, J. (2001). The Dilemma of Case Studies: Toward a Heraclitian Philosophy of Science. *Perspectives on Science*, *9*(4), 373–382. https://doi.org/10.1162/106361401760375785.

Psillos, S. (1999). *Scientific realism: How science tracks truth*. Routledge.

Psillos, S. (2017). The realist turn in the philosophy of science. In J. Saatsi (Ed.), *The routledge handbook of scientific realism* (pp. 20–34). Routledge. https://doi.org/10.4324/9780203712498.

Ravetz, J. R. (1965). *Astronomy and cosmology in the achievement of Nicolaus Copernicus.* Ossolineum.

Rossi, P. (1986). *I ragni e le formiche: Un'apologia della storia della scienza*. I'mulino.

Schickore, J. (2011). More Thoughts on HPS: Another 20 Years Later. *Perspectives on Science*, *19*(4), 453–481. https://doi.org/10.1162/POSC_a_00049.

Schickore, J. (2018). Explication work for science and philosophy. *Journal of the Philosophy of History*, *12*(2), 191-211. https://doi.org/10.1163/18722636–12341387.

Scholl, R., & Räz, T. (2016). Towards a methodology for integrated history and philosophy of science. In T. Räz & R. Scholl (Eds.), *The philosophy of historical case studies*. Springer.

Seidel, M. (2014). *Epistemic relativism: A constructive critique*. Springer. `https://doi.org/10.1057/9781137377890.`

Shapin, S. (1982). History of science and its sociological reconstructions. *History of Science*, *20*(3), 157–211. https://doi.org/10.1177/007327538202000301.

Shapin, S. (1996). *The scientific revolution*. University of Chicago press.

Shapin, S. (2010). *Never pure: Historical studies of science as if it was produced by people with bodies, situated in time, space, culture, and society, and struggling for credibility and authority*. JHU Press.

Solla Price, D. J. (1959). Contra Copernicus: A critical re-estimation of the mathematical planetary theory of Ptolemy, Copernicus and Kepler. *Critical problems in the history of science*, 197–218.

Stanford, P. K. (2006). *Exceeding our grasp: Science, history, and the problem of unconceived alternatives*. Oxford University Press.

Stegenga, J., & Menon, T. (2017). Robustness and independent evidence. *Philosophy of Science*, *84*(3), 414–435. https://doi.org/10.1086/692141.

Steinle, F., & Burian, R. M. (2002). Special issue: History of science and philosophy of science. *Perspectives on Science*, *10*, 391–397.

Swerdlow, N. M. (1973). The derivation and first draft of Copernicus's planetary theory: A translation of the Commentariolus with commentary. *Proceedings of the American Philosophical Society*, *117*(6), 423–512. http://www.jstor.org/stable/986461.

Vicedo, M. (1992). Is the history of science relevant to the philosophy of science? *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, *1992*(2), 490–496. https://doi.org/10.1086/psaprocbienmeetp.1992.2.192862.

Vickers, P. (2017). Historical challenges to realism. In J. Saatsi (Ed.), *The routledge handbook of scientific realism* (pp. 48–59). Routledge. https://doi.org/10.4324/9780203712498.

Vickers, P. (2022). *Identifying future-proof science*. Oxford University Press.

Westman, R. S. (2011). *The Copernican Question: Prognostication, Skepticism, and Celestial Order*. University of California Press. https://doi.org/10.1525/california/9780520254817.001.0001

Westman, R. S. (2013). The Copernican Question revisited: A reply to Noel Swerdlow and John Heilbron. *Perspectives on Science*, *21*(1), 100–136. https://doi.org/10.1162/POSC_a_00087

Zahar, E. (1976). Why did Einstein's programme supersede Lorentz's? In C. Howson (Ed.), *Method and appraisal in the physical sciences* (pp. 211–275). Cambridge University Press.