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The Ambiguous Legacy of Kuhn's *Structure* for Normative Philosophy of Science

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Six decades after its original publication, the legacy of Kuhn's *Structure of Scientific Revolutions* (hereafter, *Structure*) for analytic philosophy of science remains ambiguous. On the one hand, *Structure* (SSR-1) was the key work—along with Quine's "Two Dogma's of Empiricism" (Quine 1951)—that contributed to the demise of logical empiricism in the 1960s and 1970s. Moreover, *Structure* modeled a novel methodological approach for doing philosophy of science, which spearheaded the 'historical turn' in philosophy of science and the rise of the history and philosophy of science (HPS) tradition. In terms of legacy, *Structure* was undoubtedly successful in shifting the methodological assumptions of post-positivist philosophy of science towards historical analyses and away from logical analyses. On the other hand, the methodological assumptions of *Structure* introduced unclarity regarding how philosophers of science should address normative issues and what kinds of normative questions they should address. Prior to *Structure*, analytic philosophers of science were preoccupied with addressing normative questions, such as the demarcation problem (e.g., Popper 1935/1959; Carnap 1936, 1937, 1956; Hempel 1950). After the publication of *Structure*, attention shifted away from such general ('universalist') philosophy of science issues and attempts to address normative questions in an ahistorical manner became unfashionable.

This chapter examines the legacy of Kuhn's *Structure* for normative philosophy of science. As an argument regarding the history of 20th century philosophy of science, I contend that the main legacy of *Structure* was destructive: *Structure* shifted philosophy of science away from addressing general normative philosophical issues (e.g., the demarcation problem, empirical testability) towards more deflationary and local approaches to normative issues. This is evident in the first generation of post-*Structure* philosophers of science in the 1980s and 1990s, who adopted a pluralist approach to HPS. As a metaphilosophical argument regarding the methods adopted in HPS, I argue that there are a plurality of legitimate philosophical methodologies for inferring normative claims from historical cases. I frame this argument as a response to Pitt's dilemma of case studies. I reject Pitt's dilemma for its presupposition of an unrealistic and unfruitful standard (viz., epistemic certainty) for assessing HPS arguments and its analysis of philosophical methodology at the level of *individual arguments*. Pitt's dilemma is most usefully understood as identifying potential points of criticism for HPS arguments.

The chapter begins with an examination of Kuhn's normative philosophy of science in *Structure* and his position that historical cases provide *evidence* for philosophical claims. Kuhn's philosophical methodology is insufficiently articulated, and his utilization of case studies is subject to objections (viz., interpretative bias, hasty generalization) implied by Pitt's dilemma. I subsequently examine four post-Kuhnian methodological perspectives: (1) Ian Hacking's particularism, (2) Helen Longino's practice-based approach, (3) Michael Friedman's neo-Kantianism, and (4) Hasok Chang's complementary science. These views suggest alternative methodological strategies in HPS for addressing normative issues. I conclude by articulating some outstanding methodological challenges for the pluralist tradition of HPS—associated with the Stanford and Minnesota schools of philosophy of science—that emerged in the 1980s and remains influential.

Kuhn's Philosophical Methodology and Its Problems

The topic of this chapter is encapsulated by a question that Paul Feyerabend posed to Kuhn about whether *Structure* presents normative arguments. I critically examine Kuhn's response that his normative arguments (e.g., science should solve paradigm-prescribed puzzles) are supported by historical cases. I subsequently discuss problems with Kuhn's methodology with reference to the dilemma of case studies.

Kuhn's Normative Philosophy of Science

At the 1965 International Colloquium of Philosophy of Science (also see Hoyningen-Huene 2006b), Feyerabend asked whether *Structure* intended to present normative prescriptions or historical descriptions:

> Whenever I read Kuhn, I am troubled by the following question: are we here presented with *methodological prescriptions*, which tell the scientists how to proceed; or are we given a *description*, void of any evaluative element, of those activities which are generally called 'scientific'? Kuhn's writings . . . do not lead to a straightforward answer. They are *ambiguous* in the sense that they are compatible with, and lend support to, both interpretations. (Feyerabend 1970, 199, emphasis in original)

In response to Feyerabend, Kuhn was characteristically nuanced and murky:

> The answer is that, of course, they should be read in both ways at once. If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their field is to flourish. The structure of my argument is simple and, I think, unexceptional: scientists behave in the following ways; those modes of behavior have (here theory enters) the following functions; in the absence of an alternative mode *that would serve similar functions*, scientists should behave essentially

> as they do if their concern is to improve scientific knowledge. (Kuhn 1970, 237, emphasis in original)

In the postscript added to the second edition of *Structure* in 1970, Kuhn puts the point as follows:

> [*Structure*] presents a . . . theory about the nature of science, and . . . the theory has consequences for the ways in which scientists should behave if their enterprise is to succeed. Though it need not be right, any more than any other theory, *it provides a legitimate basis for reiterated 'oughts' and 'shoulds.'* Conversely, one set of reasons for taking the theory seriously is that scientists, whose methods have been developed and selected for their success, do in fact behave as the theory says they should. *My descriptive generalizations are evidence for the* [normative] *theory precisely because they can also be derived from it*, whereas on other views of the nature of science they constitute anomalous behavior.
>
> The circularity of that argument is not . . . viscous. The consequences of the viewpoint being discussed are not exhausted by the observations [i.e., case studies] upon which it rested at the start. (SSR-4, 206-207, emphasis added)

Kuhn indicates that his theory of scientific change—wherein science progresses from stages of normal science to revolutionary science—is both descriptive and prescriptive.[1] As a normative theory, this implies that scientists *should* engage in puzzle-solving or 'mop up work' (i.e., dogmatic and narrow empirical research intended to support, elaborate, and expand the currently favored paradigm) characteristic of normal science (SSR-4, Ch. 3) *until* a competing paradigm emerges that has greater puzzle-solving power.[2] For Kuhn (SSR-4, Ch. 13), science is *rational* and genuinely scientific insofar as scientists commit themselves to the best available paradigm with respect to (current or future) *puzzle-solving power*. From a methodological standpoint, Kuhn's descriptions of (putatively successful) scientific revolutions provide the basis (and 'evidence') for his normative claims. Kuhn argues that this 'derivation' of normative from descriptive claims is not question-begging ('viscously circular') because his normative generalizations can be 'tested' against other successful cases of paradigm change.

The Dilemma of Case Studies

Kuhn's favored methodology of deriving normative (philosophical) generalizations from particular case studies is limited insofar as it requires impartial selection (and accurate

[1] An alternative, arguably more candid, statement of Kuhn's methodology is found in a letter that Feyerabend wrote to Kuhn in the early 1960s. Feyerabend paraphrases and quotes Kuhn's response (from an unpublished letter from Kuhn to Feyerabend) to his prescriptive-descriptive question as follows: "You say that you are not interested in a prescriptive methodology, but in a 'more realistic notion of the practice actually used successfully in physical science'" (Feyerabend to Kuhn, cited in Hoyningen-Huene 2006b, 617). On this understanding, *Structure* was more concerned with presenting an accurate historical description of successful scientific practices than articulating philosophical prescriptions about how science should proceed. In their correspondence, Feyerabend chides Kuhn for assuming that his description of historical facts is *value-free*.

[2] *Pace*-Kuhn, Feyerabend (1975) argues that science should be a pluralistic ('anarchistic') enterprise that constantly encourages a proliferation of inconsistent methods and theories. Feyerabend (1970) premises his argument for pluralism on the assumption that normal science, as Kuhn describes it, does not exist (Tsou 2003b).

description) of historical cases and a sufficient number of cases to support normative generalizations. Pitt (2001) presents these limitations as a dilemma (p. 373):

(1) Either historical cases are selected because they support a philosophical conclusion, or they are randomly selected.
(2) If cases are selected because they support a philosophical conclusion, then there is no assurance the historical data has not been manipulated to support the conclusion.
(3) If case studies are randomly selected, then it is unclear how many cases are needed to support a philosophical conclusion.
(4) Thus, the use of cases to support philosophical conclusions is either (potentially) biased or (potentially) a hasty generalization.

Pitt's dilemma implies that philosophical arguments based on case studies fall prey to the fallacies of question-begging and/or hasty generalization. Hesse (1982) anticipated this problem when she remarked that "the case history approach is liable to fall into either dogmatic partisanship for a single theory of science, or into lack of generality" (Hesse 1982, 5).

Pitt's dilemma is particularly salient for *Structure* since Kuhn's analysis appears to violate both horns. In *Structure*, Kuhn supports his theory primarily through three cases: the Copernican revolution, the chemical revolution, and the Einsteinian revolution. Kuhn presents these cases to support the (descriptive) generalization that large-scale scientific change is discontinuous and non-cumulative insofar as revolutions involve the *replacement* (in whole or in part) of an established paradigm with an incommensurable one (SSR-4, 92). Regarding the question-begging charge, critics argue that Kuhn's historical descriptions are inaccurate and presented through the lens of his theory of scientific revolutions (Shapere 1964).[3] For example, Kuhn's presentation of the Einsteinian revolution neglects continuities (e.g., Newton's laws are derivable as a limiting case in special relativity), while exaggerating discontinuities (Earman 1993).[4] Moreover, Kuhn's presentation of this revolution as a *forced choice* between 'incommensurable paradigms' that would define physics is a misleading. The Einsteinian revolution did not involve a *replacement* of Newtonian mechanics (e.g., Newton's theory is still used widely by scientists and engineers), but the recognition of the limited domain of application for Newton's theory (e.g., classical mechanics applies to objects on earth moving slower than the speed of light). Regarding the hasty generalization charge, a larger or more random selection of scientific cases might conflict with the philosophical conclusions that Kuhn reaches. For example, analysis of the transition from views of blending inheritance to Mendelian inheritance (Olby 1966/1985) might reveal a more continuous and cumulative view of scientific revolutions than the narrative suggested in *Structure*. Similarly, examination of the shift from behaviorist psychology to cognitive psychology might yield a much more cumulative and continuous picture of scientific change (e.g., behaviorist methods of classical and operant conditioning are retained in cognitive psychology and remain central in contemporary neuroscience) than the 'replacement

[3] Pitt's question-begging objection focuses on bias in *case selection*. Herein, I extend this objection to include bias in *case interpretation*. A related but distinct issue that I do not address concerns whether Kuhn's philosophical methodology commits a naturalistic fallacy (Giere 1973; Schickore 2011; Schindler 2013).
[4] Kuhn (SSR-4, 101-102) contends that these continuities are *negated* by changes in the meaning of terms (e.g., 'mass') across theories. Chang (2012b) argues persuasively against the significance of meaning incommensurability, pointing out that the meanings of theoretical terms frequently change with theory revisions.

view' suggested in *Structure*. The presence of such alternative historical narratives highlights the significance of the hasty generalization objection.

Post-Kuhnian Approaches to Philosophy of Science

The following section examines some post-Kuhnian methodological approaches to philosophy of science: Ian Hacking's particularism, Longino's practice-based approach, Michael Friedman's neo-Kantianism, and Hasok Chang's complementary science. These approaches fall broadly within the HPS tradition inspired by Kuhn but offer more clearly articulated methodological positions on normative questions.[5]

Hacking's Particularism

Ian Hacking is a central member of the Stanford School of philosophy of science (Suppes 1978; Cartwright 1983, 1999; Hacking 1983, 1992; Galison 1987; Dupré 1993), which is characterized by a commitment to scientific pluralism, disunity of science, and practice-based accounts of science (Ludwig and Ruphy 2021; Cat 2022). In their overarching commitment to disunity of science and rejection of universalist accounts, the Stanford School opposes both the logical empiricists' unity of science ideal and Kuhn's (monistic) attempt to present a global (or 'universal') account of scientific change (cf. Wray 2021b).

The philosophical methodology that characterizes Hacking's various philosophical projects is a form of philosophical particularism, which eschews drawing *universal* claims about science.[6] Hacking's methodological particularism recommends drawing particular philosophical conclusions from historical cases and avoiding generalizations about science. This approach is motivated to address the second horn of Pitt's methodological dilemma. By closely examining particular case studies, philosophers can avoid making invalid inferences by drawing qualified and local normative conclusions.

Hacking's particularism is evident in his analysis of scientific objectivity ("Let's Not Talk about Objectivity"), where he urges philosophers to stop discussing objectivity *in the abstract* (Hacking 2015). Hacking distinguishes two different types of questions about objectivity:

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[5] My survey examines work associated with three of the most prominent methodologically-oriented HPS societies: *The International Society for History of Philosophy of Science* (HOPOS), *The Society for Philosophy of Science in Practice* (SPSP) and *The Committee for Integrated HPS* (&HPS). Friedman offers an example of HOPOS methodology; Hacking and Longino provide contrasting examples of methodologies adopted in SPSP; and Chang presents a prominent example of SPSP and &HPS methodology (Chang is a co-founder of both societies). SPSP and &HPS are contemporary societies that represent the pluralist/ disunity of science tradition of HPS that emerged in the 1980s (Ludwig and Ruphy 2021).

[6] Hacking's particularism is inspired by J. L. Austin's ordinary language philosophy and Michel Foucault's 'history of the present' (Hacking 2002). Tsou (2015) notes particularist (i.e., bottom-up) aspects of Kuhn's philosophical methodology in *Structure*. Kuhn's case study methodology reflects his training in James Bryant Conant's General Education of Science curriculum at Harvard (Wray 2021a, Ch. 2). For a fascinating historical examination of Kuhn and Conant's collaboration in the context of the Cold War, see Reisch (2019).

> (1) 'ground level questions': specific questions about particular cases that have some bearing on the objectivity of science (e.g., 'Can we trust medical research when it is funded by pharmaceutical companies?')
> (2) 'second-story questions': general questions about objectivity that assume that objectivity is a stable epistemic ideal (e.g., 'What is scientific objectivity?')

Hacking (2015) argues that philosophers should stop talking about (2) and only talk about (1), which motivates his imperative: "let's get down to work on cases, not generalities" (Hacking 2015, 29). Hacking regards abstract philosophical ideals (e.g., 'objectivity,' 'truth,' 'facts') as "elevator words" that exist at a higher level than actual things in the world (Hacking 1999, 22-24). He objects to philosophical analysis of such words because they are circularly defined and use of them gives the false impression that there are stable meanings for such terms. His favored methodology analyzes these words *as they appear in various historical sites* (Hacking 2002).

Hacking's particularism is also salient in his defense of entity (or experimental) realism. Hacking distinguishes between realism about theories (as true or false) and realism about entities (as existing or not). Hacking eschews questions about realism about theories, but he maintains that questions about realism about entities are answerable. Hacking's argument for entity realism emphasizes the use and manipulation of theoretical entities as a basis for believing in their reality. He argues that we have good evidence for the reality of an entity under specific circumstances:

> The experimenter is convinced of the reality of entities some of whose causal properties are sufficiently well understood that they can be used to interfere elsewhere in nature. One is impressed by entities that one can use to test conjectures about other more hypothetical entities. (Hacking 1982, 75)

Hacking contends that we have evidence for the reality of 'unobservable' entities that experimenters can *control* and *manipulate* to study other parts of nature: "We understand the effects, we understand the causes, and we use these to find out about something else" (Hacking 1983, 24). Hacking draws this normative conclusion based primarily on a single case study, viz., experimenters' manipulation of electrons in the use of electron guns to study quarks. He writes:

> We are completely convinced of the reality of electrons when we regularly set out to build — and often enough succeed in building — new kinds of device[s] that use various well-understood causal properties of electrons to interfere in other more hypothetical parts of nature. (Hacking 1983, 265, emphasis removed)

This leads to Hacking's well-known slogan for entity realism: "If you can spray them, then they are real" (Hacking 1983, 22). Hacking's analysis indicates the qualified and limited normative conclusions he is willing to draw from case studies. As a methodological strategy, Hacking's particularism grabs the second horn of Pitt's dilemma by qualifying and limiting the scope of normative claims away from universal questions (e.g., when are scientific theories true?) towards more particular and local normative questions (e.g., when do we have evidence that the unobservable entities posited by theories exist?).

Longino's Practice-Based Methodology

Whereas Hacking eschews discussing scientific ideals in the abstract, Longino (1990) articulates a general social account of objectivity that opposes Kuhn's account of scientific objectivity. Her practice-based methodology is important to examine since it represents a common methodology adopted in contemporary HPS.

Longino rejects Kuhn's 'theory choice' framework (Kuhn 1973/1977), which examines questions of objectivity in terms of *choices* that scientists make between incommensurable paradigms. In Kuhn's framework, theory-choice is objective if there is a *stable* set of *epistemic values* (e.g., empirical accuracy, predictive power, explanatory scope, simplicity, fruitfulness) shared by proponents of competing paradigms, which provides an intersubjective basis for making *mechanical* ('algorithmic') *decisions* between competing paradigms. Kuhn argues that paradigm choice is never fully objective in this sense because competing paradigms are premised on incommensurable value-sets (e.g., specific values are interpreted differently or weighted more heavily within a paradigm).[7] Longino rejects Kuhn's (rational-choice) framework for its individualistic assumptions and failure to reflect actual scientific practices. In her social framework, objectivity is a characteristic of scientific communities, rather than an individual's reasons for favoring a paradigm. A scientific community is objective, i.e., free from bias, when its knowledge claims are presented in a public domain and subject to criticism.

Longino assumes that non-epistemic *contextual values* (i.e., socially-situated background beliefs and assumptions) are a ubiquitous feature of science, and they determine what counts as scientific evidence in a historical context. Despite the inevitable presence of contextual values, scientific communities are objective if they allow for *transformative criticism*, i.e., criticism that can lead to the revision of contextual values:

> As long as background beliefs can be articulated and subjected to criticism from the scientific community, they can be defended, modified, or abandoned in response to such criticism. As long as this kind of response is possible, the incorporation of hypotheses into the canon of scientific knowledge can be independent of any individual subjective preferences. (Longino 1990, 73-74)

Scientific communities are objective to the extent that they encourage—and are responsive to—criticism (especially criticism of contextual values).

Longino argues that features of more objective scientific communities include recognized avenues for criticism (e.g., peer reviewed journals and conferences), the presence of public shared scientific standards (e.g., empirical accuracy) that guide criticism, responsiveness to criticism, and equality of intellectual authority. On this view,

> Objectivity is dependent upon the depth and scope of the transformative interrogation that occurs in any given scientific community. This communitywide process ensures … that the hypotheses ultimately accepted … do not reflect a single individual's idiosyncratic assumptions … To say that a theory … was accepted on the basis of objective methods [entitles us to say that the theory] reflects the critically achieved consensus of the scientific community… [I]t's not clear we should hope for

[7] For criticism of Kuhn's analysis, see Laudan (1984) and McMullin (1993).

anything better. (Longino 1990, 79)

For Longino, objectivity is constituted by social mechanisms which encourage a critical scientific community; by contrast, a community is subjective (i.e., biased) when there are inadequate mechanisms to prevent its theories from reflecting the idiosyncratic assumptions of a few individuals. Whereas Kuhn identifies objectivity with *a shared (or agreed upon) perspective of epistemic values* that provide *clear criteria* (or rules) for choosing among competing paradigms, Longino identifies objectivity with the presence of *a plurality of conflicting perspectives* that facilitate criticism of *contextual values*.

From a methodological perspective, Longino endorses a Kuhn-inspired practice-based methodology, which includes conceptual analysis and the understanding of scientific reasoning "as a practice rather than as the disembodied application of a set of rules" (Longino 1990, 13). Longino utilizes this Kuhnian standard to criticize Kuhn's analysis of objectivity for being artificial and disengaged with actual scientific practices. Longino's methodology is subject to the first horn of Pitt's dilemma insofar as she selectively chooses scientific cases to support normative claims. Longino's strategy involves: (i) explicating an operational definition of objectivity, and (ii) providing contrastive examples, which exemplify objective and subjective aspects of scientific communities. While Hacking opposes (i) as 'circular,' Longino's definition can be assessed in terms of whether it picks out a desirable epistemic feature of science (viz., a community's encouragement of self-criticism) that captures a useful meaning of 'objectivity.' While Pitt opposes (ii) as a conclusion-driven selection of cases, Longino's examples (e.g., androcentric research on sex differences) can be assessed in terms of whether they accurately illuminate desirable ('objective') and undesirable ('subjective') features of science, and whether there are counterexamples. Longino's strategy for avoiding the question-begging objection is to be conservative in her selection of cases relative to her normative conclusions. She points to relatively *uncontroversial* instances (e.g., peer review, replication of studies) and counterinstances (e.g., politically motivated scientific communities) of her ideal of objectivity. In this regard, Longino's use of history is closer to traditional *a priori* normative approaches, which utilize cases primarily as anecdotal ('toy') examples that illustrate epistemic virtues and vices of science. Hence, her methodology can be criticized for engaging in the 'anecdotal'/ Whiggish use of history rejected by Kuhn (cf. Solomon 2001, Ch. 3).

Friedman's Neo-Kantianism

Michael Friedman argues for anti-Kuhnian conclusions on the basis of the history of science and the history of philosophy of science. Friedman is troubled by the anti-rationalist and relativist conclusions suggested by Kuhn's analysis of scientific change,[8] and he advances a neo-Kantian account of scientific rationality.

From a methodological perspective, Friedman draws both from the history of science and the history of philosophy of science as resources for supporting normative philosophical claims. One of the main normative arguments that Friedman presents is that the development of modern physics required theoretical principles that have a relativized *a priori* status. In the history of

[8] The relativist implications of *Structure* were heartily embraced by sociologists of scientific knowledge (see Wray 2011).

logical empiricism, the importance of relativized *a priori* principles were recognized by Reichenbach and Carnap. Friedman writes:

> Careful attention to the actual historical development of science . . . shows that relativized a priori principles of just the kind Carnap was aiming at are central to our scientific theories. Although Carnap may have failed in giving a precise logical characterization or explication of such principles, it does not follow that the *phenomenon* he was attempting to characterize does not exist. On the contrary, everything that we know about the history of science . . . indicates that precisely this phenomenon is an absolutely fundamental feature of science. (Friedman 2001, 41, emphasis in original)

Friedman argues that in the history of mathematical physics, from Newtonian mechanics to the general theory of relativity, physical theories have required the presupposition of relativized *a priori* principles. The epistemic function of these principles is that they are *constitutive* of empirical laws and tests insofar as they make "the precise mathematical formulation and empirical application of the theories in question first possible" (Friedman 2001, 40). In the history of 20th century philosophy of science, Friedman argues that the special status of such principles was not only recognized by Reichenbach and Carnap, but also by Kuhn, in his 'Kantian' assumption that empirical observations and tests require the presupposition of a paradigm (cf. Hoyningen-Huene 1993; Patton 2011).

Friedman's neo-Kantian account suggests that modern physical theories are distinguishable into three asymmetrically functioning parts:

> (1) A *mathematical part*: basic mathematical and geometrical principles that describe the spatio-temporal framework.
> (2) A *mechanical part*: principles that coordinate the mathematical part to the empirical part.
> (3) An *empirical part*: empirical and physical principles that use the theories in the mathematical part to formulate empirical laws to describe concrete phenomena.

For Friedman, (1) and (2) are relativized *a priori* in the sense that—*taken together*—they are *constitutive* of the empirical part. In Newtonian mechanics, e.g., Euclidean geometry (1) and the laws of motion (2) have a relativized *a priori* status insofar as they are constitutive of empirical laws such as the law of universal gravitation (3). In special relativity, the geometry of Minkowski space-time (1) and the light principle (2) are constitutive of empirical laws such as Maxwell's equations (3). In general relativity, the theory of semi-Riemannian space-time manifolds (1) and the principle of equivalence (2) are constitutive of empirical laws such as Einstein's equations for the gravitational field (3).

Friedman's argument for the rationality of scientific change is framed in terms of relative *a priori* principles and draws on Habermas' distinction between instrumental and communicative rationality:

(1) Instrumental rationality (subjective): the capacity to engage in effective means-ends reasoning (where an agreed-upon goal is assumed).

(2) Communicative rationality (intersubjective): the capacity to engage in deliberative argumentative reasoning (where there is no agreed-upon goal) aimed at bringing about consensus.

In Kuhn's account, communicative rationality is limited to rationality *within* a paradigm (i.e., normal science). Friedman argues that there is an important sense in which communicative rationality, in the sense of achieving a consensus or agreement, is achieved *between* different paradigms (i.e., revolutionary science). For Friedman, the history of mathematical physics illustrates that earlier constitutive principles appear as limiting cases in later theories (which hold under specified conditions) and the concepts of succeeding constitutive frameworks evolve continuously, by a series of natural transformations, from previous constitutive frameworks (cf. Worrall 1989). He writes: "we can thus view the evolution of succeeding paradigms . . . as a convergent series . . . in which we successively refine our constitutive principles in the direction of even greater generality and adequacy" (Friedman 2001, 63).

Friedman's philosophical methodology, like Kuhn's, aims to infer normative conclusions about science by selecting case studies that exemplify an argument. As such, it falls prey to the interpretative bias objection expressed by the first horn of Pitt's dilemma. Conversely, it is notable that Friedman aims to counter Kuhn's view by providing a *more accurate description* of the history of physics. Friedman's analysis also falls prey to the second horn of Pitt's dilemma. While Friedman's analysis may generate normative claims about the rationality of mathematical physics, it is unclear how generalizable this conclusion is to science more generally.[9]

Chang's Complementary Science

Hasok Chang is one of the most vocal advocates for pluralism and integrated HPS. Chang's methodological ideal of complementary science is particularly important to examine since it is motivated precisely to address the methodological issues discussed in this chapter.

Chang (2004) presents complementary science as an *ideal function* that HPS should serve. The aim of complementary science is to articulate normative claims about science through historical and philosophical investigation. In this ideal, there is not a strong disciplinary distinction between history, philosophy, and science. Unlike more orthodox methodological approaches in HPS that aim to produce meta-scientific knowledge (e.g., a general account of scientific explanation or scientific change), complementary science aims to contribute to scientific knowledge itself:

> [Complementary science] contributes to scientific knowledge through historical and philosophical investigations. Complementary science asks scientific questions that are excluded from current specialist science. It begins by re-examining the obvious, by asking why we accept the basic truths of science. . . . Because many things are protected from questioning and criticism in specialist science, its demonstrated effectiveness is also unavoidably accompanied by a degree of dogmatism and a narrowness of focus that can actually result in a loss of knowledge. History and philosophy of science in its 'complementary' mode can ameliorate the situation. (Chang 2004, 3)

[9] Friedman (2001, 124-129) briefly discusses how his neo-Kantian framework applies to sciences such as chemistry and biology, but his discussion does little to engage with actual chemical or biological science. For a discussion of constitutive principles in biology, see Tsou (2010) and Luchetti (2021).

In its complementary function, HPS can recover useful ideas and facts lost in the scientific record ('Kuhn losses'), address foundational questions concerning present science, and explore alternative conceptual systems and lines of experimental inquiry for future science. If these investigations are successful, they will complement and enrich current specialist science. Normative claims (e.g., concerning good or bad science) generated by complementary science are articulated at the level of scientific knowledge itself, rather than at a level removed from science.

As a methodological perspective, complementary science addresses Pitt's dilemma by rejecting the terms in which it is framed. Specifically, Chang rejects the assumption that history deals with the particular and philosophy deals with the general:

> [Kuhn] never specified a clear method for the history–philosophy interaction, and without such a method we are condemned to the dilemma between making unwarranted generalizations from historical cases and doing entirely "local" histories with no bearing on an overall understanding of the scientific process.
>
> In attempting to transcend this dilemma, I believe that the first thing we need to do is to see if we can get beyond an inductive view of the history–philosophy relation, which takes history as *particular* and philosophy as *general*. Of course, we cannot get away from inductive thinking entirely, but it is instructive to try seeing the history–philosophy relation as one between the *concrete* and the *abstract*, instead of one between the particular and the general. Abstract [philosophical] ideas are needed for the understanding of *any* concrete [historical] episode. . . . Any concrete account requires abstract notions [e.g., 'confirmed,' 'observation,' 'measurement']. . . . If we extract abstract [normative] insights from the account of a specific concrete episode . . . , that is not so much a process of *generalization*, as an *articulation* of what was already put into it. (Chang 2012a, 110, emphasis in original)

Chang challenges Kuhn's stance that—given their divergent aims—history of science and philosophy of science cannot be practiced at the same time (Kuhn 1977). He also challenges Pitt's assumption that history is concerned with particular cases, whereas philosophy is concerned with general (normative) concepts. Against these views, complementary science advocates an integrated view of HPS, wherein concrete historical episodes and abstract philosophical concepts stand in a mutually-dependent relationship (cf. Burian 2001). With respect to the dilemma of case studies, Chang's position recommends resisting an entrenched assumption of HPS, assumed by Kuhn, Pitt, and others, that historical episodes are particular pieces of *evidence* that support general philosophical conclusions. Rather, we should view normative arguments as the articulation of abstract ideas that are embedded in concrete historical episodes.

Methodological Challenges for HPS

One distinctive feature of the HPS community in the 1980s and 1990s—one generation removed from *Structure*—is its pluralistic outlook (Cartwright 1983, 1999; Hacking 1983; Galison 1987; Longino 1990; Dupre 1993; Galison and Stump 1996; Giere 1999; Wylie 1999). In advocating scientific pluralism, disunity of science, anti-reductionism, and methodological engagement with the history of science and scientific practices, post-Kuhnian HPS engaged with a broader range of sciences besides physics (especially biology) and addressed a broader range of philosophical topics (e.g., values in science, models, mechanisms). If the methodological assumptions associated with Stanford and Minnesota Schools of pluralism (Kellert, Longino, and Waters 2006; Solomon and Richardson 2005; Cat 2021) provide a fair representation of the methodological commitments of the HPS community, then post-Kuhnian HPS is decidedly more Feyerabendian in character than Kuhnian.[10]

From a metaphilosophical perspective, several methodological challenges for post-Kuhnian HPS—especially the pluralistic tradition that emerged in the 1980s—can be gleaned from the methodological approaches examined in this chapter. While no decisive conclusions can be drawn from such a limited survey, I articulate three related methodological challenges for post-Kuhnian HPS: historicism, the relationship between case studies and normative arguments, and the lack of canonical problems.

Historicism

One methodological challenge for HPS concerns the possibility of articulating non-historicist and non-relativist arguments about science. Although Kuhn (SSR-4, 203-205) adamantly denied charges of relativism, Popper (1970, 55) correctly diagnosed Kuhn's theory as a species of 'historical relativism.' Kuhn (and Feyerabend) assumed a form of historicism that eschewed attempts to articulate (stable) transhistorical normative scientific ideals (e.g., 'truth,' 'empirical evidence,' 'rationality').[11] This chapter suggests that the historicism championed by Kuhn and Feyerabend in the 1960s and 1970s evolved into the pluralistic style of HPS practiced in the 1980s and 1990s.

In the context of 21[st] century HPS, historicism is most explicitly endorsed in the subfield of 'historical epistemology' (Hacking 1975, 1992, 2002; Galison 1987; Daston 1994; Davidson 2001; Daston and Galison 2007).[12] In contrast with traditional normative philosophy of science, which defends *stable normative scientific ideals* (e.g., 'probability,' 'experimentation,' 'empirical evidence'), historical epistemology assumes that epistemic and scientific ideals are *unstable* and examines the emergence and evolution of such ideals. In the (yet) broader context of 21[st] century philosophy of science, historicist approaches—such as historical epistemology—are somewhat marginal.[13] At present, philosophy of science embraces a plurality of

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[10] The pluralist/ disunity of science consensus arrived at in 1980s and 1990s was not self-consciously inspired by Feyerabend. Rather, pluralism was an umbrella concept that post-Kuhnian philosophers of science invoked to articulate *non-positivist* positions on a variety of issues (Richardson 2006; Ludwig and Ruphy, 2021). Despite resonances with Feyerabend's views on pluralism, anti-reductionism, and disunity of science, the programmatic statements of the Stanford (Suppes 1978) and Minnesota (Kellert, Longino, and Waters 2006) schools of pluralism do not acknowledge Feyerabend as an influence (cf. Hacking 1983, 14; Dupré 1993, 10; Longino 1990, Ch. 2). For discussion of Feyerabend's mixed legacy in HPS, see Lloyd (1996), Preston, Munévar, and Lamb (2000), Sankey (2020), Brown and Kidd (2016), and Shaw and Bschr (2021).

[11] For discussion of Kuhn's historicism, see Bird (2015) and Nickles (2017). In contrast with Kuhn's distaste for relativism, Feyerabend (1978, 1987, 1989) increasingly embraced historicism and relativism in his post-*Against Method* works (Preston 1997a, 1997b; Kusch 2016; Brown and Kidd 2011).

[12] For critical discussion, see Kusch (2010) and Feest and Sturm (2016).

[13] The historicism popularized by Kuhn and Feyerabend in the 1960s and 1970s eventually gave way—especially through the 'science wars'—to a realist reaction by philosophers of science in the 1990s and 2000s (Nickles 2017). This realist response also reflects the increased influence of (Quinean) naturalistic methodological approaches to

methodological approaches, including historical, formal, naturalistic, and social approaches. However, historicist considerations are relevant for any HPS analysis that defends *general normative arguments* through historical cases. For example, Friedman (2001, 64-65) is acutely aware of historicist considerations in his defense of scientific rationality. His response to historicism involves understanding scientific rationality as a Kantian *regulative ideal of reason*: "We can . . . view our present scientific community . . . as an approximation to a [Peircean] final, ideal community of inquiry . . . that has achieved a universal, trans-historical communicative rationality on the basis of the fully general and adequate constitutive principles reached in the ideal limit of scientific progress" (Friedman 2001, 64). Regardless of its merits as a response to historical relativism (see Richardson 2002; Tsou 2003a), Friedman should be commended for his sensitivity to historicist issues and transparency in responding to them. Few HPS analyses are so explicit.

How Historical Cases Support Philosophical Arguments

A related methodological challenge for HPS is how historical cases can support normative positions on the most general philosophy of science issues, such as the demarcation problem.[14] One of the historical legacies of *Structure* is that it shifted the attention of philosophers of science away from *general* philosophy of science issues towards more qualified topics (e.g., analyses limited to the special sciences) and local normative issues (e.g., structural realism, mechanisms, inductive risk). To the point of this chapter, *Structure* shifted philosophers away from traditional normative questions concerning the *justification of scientific knowledge*. In my opinion, this state of affairs is odd and regrettable. In the 21st century, epistemic questions concerning what distinguishes genuine science from non-science remain salient in various socially relevant contexts (e.g., debates about climate change models or mRNA vaccines) and philosophers have little to contribute beyond arguments proposed over five decades ago.[15] Others express similar concerns that the disunity of science/ pluralist consensus reached in the 1980s and 1990s fragmented HPS and threatened the very possibility of doing general normative philosophy of science (Reisch 1998; Magnus 2013).

The challenge of how to support normative philosophical arguments with cases is brought into focus by Pitt's dilemma. For Pitt, the reliability of the case study approach is limited because one can always question: (i) the theoretical neutrality in which cases are selected and interpreted, and (ii) whether the selected cases provide adequate inductive support for general philosophical conclusions. Pitt's dilemma should be rejected in its implicit presupposition of an unreasonably lofty epistemic standard, viz., the demand for epistemic certainty or infallibility. Pitt's dilemma

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philosophy of science (Tsou forthcoming). It is worth noting that Kuhn's methodology in *Structure* was naturalistic insofar as it demanded *accurate reconstructions of scientific practices* with the aid of *a posteriori* sciences (e.g., history, psychology). Bird (2000, 2002, 2004) argues that Kuhn's naturalism was the most promising feature of *Structure*; he laments the fact that Kuhn shifted away from naturalism in post-*Structure* works (e.g., Kuhn 2000) towards more traditional *a priori* philosophical methods (cf. Shan, 2020).

[14] Roth (2013) notes the irony that, despite the immense methodological influence of *Structure*, post-Kuhnian philosophers of science have paid relatively little attention to issues of historical explanation.

[15] For some post-Kuhnian analyses of the demarcation problem, see Hoyningen-Huene (2013) and Pigliucci and Boudry (2013). Kuhn's historicized answer to the demarcation problem (i.e., science is distinguished by the presence of a paradigm) and normative account of science (i.e., scientists should prefer paradigms that are better at solving puzzles than rivals) are articulated at *too general of a descriptive level* to evaluate the scientific status of individual fields.

only has force, *as an argument against using historical cases*, if the goal of philosophical analysis is to infer—*with absolute certainty*—normative conclusions. In this regard, Pitt's dilemma applies equally to science as it does to HPS insofar as scientific conclusions of *a given study* are inferred from particular observations, experiments, or tests. Just as the fallibility of a single scientific study does not speak against inferring of general theories from particular observations, the fallibility of generalizations drawn in a single HPS study does not speak against inferring general conclusions from particular case studies. If case studies provide defeasible evidence in support of scientific generalizations, including normative generalizations, and HPS arguments are subject to criticism (e.g., regarding the selection or interpretation of cases), then there is nothing particularly problematic about the case study approach. Following Chang, normative philosophical arguments can be regarded as *abstract generalizations* that are instantiated in and illustrated by concrete historical episodes. In HPS, the challenge is to articulate normative arguments that avoid "naïve abstraction while working at a greater level of generality than the specific sciences" (Magnus 2013, 51).

Canonical Normative Problems for HPS

A final challenge for HPS concerns the lack of consensus about canonical normative issues (cf. Galison 2008). In the logical empiricist tradition (e.g., Hempel 1966), philosophers of science worked collectively on a family of interrelated normative issues (e.g., the demarcation problem, induction in science, empirical testability and confirmation, scientific explanation, laws of nature) that focused on the epistemic question of what *justifies* scientific knowledge. *Structure* introduced ambiguity, not only with respect to the methodologies that philosophers of science should adopt, but for the kinds of normative questions philosophers of science should address. In *Structure*, Kuhn was preoccupied with what appeared to be a set of descriptive issues (e.g., the continuity and discontinuity in scientific change, science as puzzle-solving, incommensurability). In rejecting the logical empiricists' distinction between the contexts of discovery and justification, Kuhn's historical approach questioned the very assumption that philosophers could address questions about scientific justification, independent from their historical contexts (Hoyningen-Huene 2006a). The pluralism that characterizes much of post-Kuhnian HPS provides little assurance that an emerging consensus on canonical normative issues is forthcoming.

Conclusion

This chapter examined the legacy of Kuhn's *Structure* in the history of 20th century philosophy of science. From the perspective of normative philosophy of science, the legacy of *Structure* was primarily destructive: *Structure* brought into question the very idea of defending ahistorical normative ideals of science and shifted historians and philosophers of science towards more deflationary and localized normative projects. This deflationary impact of *Structure* need not be viewed as a regressive episode in the history of philosophy of science nor be cause for methodological anxiety. Rather, *Structure* opened up new methodological possibilities and opportunities for HPS to articulate normative arguments.

From the perspective of contemporary 21st century philosophy of science, historians and philosophers of science face ongoing methodological challenges for articulating and supporting normative arguments about science based on historical (and present) cases of science. Three outstanding methodological challenges for contemporary HPS include: (1) articulating non-historicist and non-relativist arguments about science, (2) articulating general ('universal') features of good and bad science, and (3) articulating canonical issues for HPS. These challenges are particularly salient for the pluralistic (quasi-Feyerabendian) tradition of HPS that gained prominence in the generation once removed from *Structure*. My analysis does not imply that philosophers of science should develop detailed responses to such challenges. Rather, these challenges are highlighted as outstanding methodological issues bestowed on HPS by *Structure* that provide further opportunities for methodological innovation and evolution.

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