Chapter 2

Ways of Integrating HPS: Top-down, Bottom-up, and Iterations

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Abstract: Philosophy of science and history of science have been unable to integrate in a meaningful fashion. The major difficulty has been the question of how the history of science can inform the philosophy of science. By making several distinctions to characterize the type of philosophy of science relevant for integrated HPS, I show how traditional approaches to integration failed. These include a top-down and a bottom-up philosophical approach to integrated HPS. I then present a more fruitful way of integrating the disciplines, that of iterations.

Keywords: integrated history and philosophy of science; inductivism; hypothetico-deductivism; epistemic iterations

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1. Introduction

Philosophy of science (PS) and history of science (HS) have been put at odds with one another whenever their arranged marriage has been put in question. Yet, there have been many advocating for an intimate relation between the two disciplines in the form of integrated history and philosophy of science (&HPS), and many times this has resulted in failure. The two primary questions to ask in this interdisciplinary field are: What does the *history* of science have to offer the philosophy of science? What does the *philosophy* of science have to offer the history of science? The latter question has often had many answers and is not nearly as problematic as the former question. Many answers to the former question have been given, and theories of how the history of science can improve our understanding of science, even in the modern day, have often

been unsuccessful. It is essential that both questions have answers to form a mutually beneficial cross-disciplinary study of science through historical and philosophical avenues. I hope to review some of the failures in integrating the history and philosophy of science and shed light on a promising way of integrating history and philosophy of science – iterations.

2. Characterizing Philosophy of Science for &HPS

One major issue that has plagued &HPS has been the ill-defined character of PS. Arguments against the possibility of &HPS have often tacitly assumed certain characterizations of PS which have led to conclusions that only apply to these specific characterizations.[1] The sentiment that there is a need for a proper characterization of PS has also been noticed by other authors, but no immediate solution has been presented (Schickore, 2011a, p. 469; also, Richardson, 1992, p. 42). In this section, I set out to characterize PS for &HPS by making several distinctions: first a metaphysics/epistemology distinction, then a PS/HS relevant concepts distinction within epistemology, and a normative/descriptive distinction within &HPS concepts. Lastly, I present some examples.

The initial distinction to be made is between the metaphysical and the epistemological sides of PS (this topic is also covered in Barseghyan & Shaw, 2022, in this collection). &HPS will in general be more interested in the epistemological side of things, but simply distinguishing between metaphysics and epistemology is insufficient to properly characterize the type of PS most important to &HPS. On the metaphysical end, we generally have PS intermingling with scientific ontological concepts, their definitions and related theories. These include debates about realism and anti-realism, theories of causation, the nature of space and time, evidence, error, replication, observation, etc. These are to be contrasted with the more epistemological PS that more closely considers knowledge and its acquisition. Questions are covered on acceptance, theory choice, justification, discovery, etc. However, these two types of PS are not disjoint. Many metaphysical questions interact with epistemological ones. For example, the topic of evidence is relevant to both metaphysics and epistemology, and as such, cannot be placed exclusively under the banner of either of them. &HPS does however seem to be primarily

[1] See for instance Nickles, 1986, p. 255. Here Nickles argues that if there is no fixed normative methodology of science, there can be no general theory of scientific knowledge. This is conflating the normative and descriptive theories of scientific knowledge. Although a normative one may be an untenable enterprise, perhaps a descriptive theory of scientific knowledge can be achieved.

concerned with epistemology, and how science is practiced, but one needs to draw further insight into which concepts are relevant for &HPS.

Historians of science for the most part do not concern themselves with many debates in the philosophy of science in their writings. There are many philosophical concepts that have no immediate value for HS: error, replication, realism, and probability, to list a few. This does not mean that HS is not interested in any PS concepts. Here I aim to outline but a few of these mutually relevant concepts between HS and PS. For instance, the question of demarcation is important both for HS and PS. What is science? Without at least some basic intuition as to an answer for that question, neither a historian nor a philosopher can proceed to practice HS or PS, as they would have no idea what it is they are studying! Defining science has tremendous historical value as it answers the question of where to draw the line between history *of science* and general history. Topics like these are critically important and discussing them in &HPS can benefit both the historical and philosophical disciplines. Moreover, these relevant concepts are not limited to demarcation by any means, questions such as: What constitutes scientific change? What are the constituent elements of science? Are there any patterns in science? How is science done? These are all questions that are crucial for both HS and PS. Some of these topics, such as patterns in science, can only be properly studied via both historical and philosophical avenues. There are both diachronic and synchronic studies in &HPS, and these diachronic studies cannot be tackled by PS alone, but require some form of historical research. These mutual philosophical questions will form the basis of my discussion of &HPS.

There is also a more traditional distinction to be made–the normative/descriptive distinction of PS. Within the literature, the normative character of PS has been overwhelmingly stressed and the concept of a descriptive PS has been introduced, but not properly separated from normative PS. PS has had several primary goals regarding science: the interpretation of science; an understanding of science; and most problematically, a prescription for the practice of science. One can do PS by interpreting science, e.g., what does quantum mechanics have to say about determinism? This is of course a topic for &HPS, but it is not a topic that is of particular interest to HS. Understanding science, on the other hand, is quite a different story. HS has a lot to gain from philosophical understandings of science. It can refine what the relevant artifacts are for a study of science, a well-defined taxonomy can be offered for HS, the question of what a scientific narrative consists of begins to have an answer and many more benefits become apparent when PS offers an understanding of science.

Notice, however, that all the benefits HS can draw from a philosophical understanding of science are descriptive, and *not* normative. It can even be said that HS requires some form of descriptive PS (a framework or language) to

function, but it surely does not require any PS norms saying how science ought to be done. For example, the question of the Aristotelian-Medieval community's rationality in the face of Galileo's observations has at times been mislabeled as an irrational process based on modern scientific standards, which ignored the episodes historical context (Barseghyan & Shaw, 2022). Normative PS (at least its non-descriptive component) offers little to HS.

Normative PS does however have a place. Although normative PS has often been incredibly historically incorrect and met with criticism[2], it can in principle offer something to scientists. A question such as "what theory *should* be chosen considering the evidence?" can be answered philosophically and potentially even have scientific output. Koertge, for instance, paraphrases Marx as saying "the duty of philosophy of science is to improve scientific practice, not describe it" (Koertge, 1976, p. 367). This certainly adds purpose to normative PS as a more analytic guiding hand for science. Normative PS certainly has a place, but it seems that descriptive PS is the one that matters for &HPS.

I shall label the descriptive philosophy of science, as theories of scientific change (TSC), and the normative philosophy of science as methodology (MTD). The TSC/MTD distinction has been lightly covered by other authors historically, but has not seen a more concrete characterization until more recently.[3] I have found that these distinctions have not been sufficiently clear in separating the normative components of PS from the descriptive, while the TSC/MTD distinction is well-defined in this regard. To gain an intuition for this separation, I provide some examples.

I shall present one illustration of a seemingly pure MTD, an illustration of a mixed case that elucidates why the normative/descriptive distinction in PS has been problematic, and finally an illustration of a seemingly pure TSC case.

Bayesianism and the base-rate fallacy offer an example of a pure MTD. Historically, and currently, it is common that many people commit the base rate fallacy (Dicken, 2013, p. 565). Under Bayesian theory, they ascribe incorrect

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[2] Just think of Popper's falsificationism, Carnap's verificationism, probability calculus, Kuhnian paradigms, and many more historical philosophies of science that have prescribed certain scientific behaviors. All of these have often been met with distaste from historians of science, actual scientists, and other philosophers of science. However, my aim is not to discuss this topic here.

[3] A recent characterization for the descriptive/normative PS distinction can be found in Scholl (2018). Barseghyan (2015, pp. xi-xvi) discusses the distinction between TSC and MTD. Burian (1977) makes an evaluative/prescriptive distinction, while Brown (1988) makes a normative/naturalized epistemology distinction. Other authors like Laudan (1990) and Giere (1985) are explicit in their normative or descriptive commitments in naturalized epistemology.

probability assessments to situations. Does it matter to Bayesianism that people have commonly committed the base-rate fallacy? No. Bayesianism does not attempt to explain *how* people reason, it is prescriptive in the sense that it tells you what is more probable and perhaps carries the normative baggage that you *should* believe what is more probable. Clearly, Bayesianism is an MTD in this regard, it does not seek to describe science or human reasoning, but it seeks to prescribe arriving at certain conclusions over others based on probabilities (a method). Bayesianism in this sense is not descriptive and hence not a TSC, but a pure MTD.

Next, consider Lakatos's research programmes. Let's focus in on a certain claim: that ad hoc hypotheses (any of the types) are regressive and scientists should avoid them. This is clearly a normative statement which qualifies Lakatos as having created an MTD. But, is that all that Lakatos is saying? No, he is not only saying that scientists should avoid ad hoc hypotheses, but that scientists *do* avoid them. This is a descriptive statement about how science functions. Such a statement could be put into a TSC. Although analyzing Lakatos is merely an illustration in this paper, I invite the reader to consider the whole *Methodology of Scientific Research Programmes* and ask whether the statements are both prescribing norms for science and describing actual science, or simply one of the two. It seems that almost all of Lakatos's claims are both descriptive and prescriptive for science. Lakatos's MSRP thus serves a dual purpose as a TSC and an MTD. This, to me, is why there has been a major conflation of normative/descriptive PS in the HPS literature. This mixing of descriptive and normative PS has been seen in other authors as well such as Popper, Kuhn, and other traditional names in HPS.

On the other hand, Scientonomy from Barseghyan (2015) is a pure TSC. Scientonomy's aim is to be a "science of science" and only offer descriptive claims on what science is, what are its elements, what constitutes scientific change and what the patterns are in scientific change. It does not attempt to prescribe anything to scientists. It follows from a tradition of naturalized philosophy; however, it does not have any normative output. Scientonomy only aims to describe the scientific process, and not prescribe how science ought to be practiced. This makes it a promising venture for those seeking to do &HPS.

3. Food for Thought

In this brief section, I outline an obvious way that HS can be relevant to PS, and that is simply through giving philosophers cases to think about. Garber gives an accurate and charming description of this relevance (Garber, 1986, p. 111):

> It is fair to say that much philosophy of science in the positivist tradition has been armchair philosophy of science; the idea is that we sit down in

> a comfortable armchair and think very hard about confirmation, explanation and the like. History of science brings the armchair to the dining table, as it were, and provides food for thought. Despite the fact that it is theory-bound, the history of science can be very valuable in eliciting our intuitions about good and bad science, and in showing us scientific procedures worthy of consideration and practical test that we might otherwise miss.

This kind of interaction between HS and PS works both for a TSC and an MTD. In general, HS provides a "broad range of examples, conceptual tensions and puzzles, which shed new light on present understandings of epistemic terms and might lead to conceptual discoveries" (Schickore, 2002, pp. 453-454). These examples and puzzles may be used in an MTD to find problems or counterexamples of certain accounts, say, for example, of error which lead to a deeper understanding of those terms and how they should be understood and prescribed.

Popular philosophical theses perhaps have stemmed from simply using HS as food for thought. Theses such as the Duhem-Quine thesis, auxiliary hypotheses, incommensurability, the non-cumulative character of science, scientific revolutions, etc. All seem to have stemmed from some inspiration (not necessarily an inductive or hypothetico-deductive inspiration) from the HS.

I hope it is clear that HS can at least serve as food for thought for PS, but it would be a shame if that was all there is to it, and most likely not enough rationale to make an &HPS department or field if this were the only thing HS had to offer for PS. In the next sections, I will cover several approaches on &HPS.

4. Top-down &HPS

Having offered several distinctions that serve as guidelines for what PS is suitable for &HPS, I now take a look at ways history can inform PS by starting with top-down philosophy. The MTD/TSC distinction shall be used extensively to contrast the traditional normative way of practicing philosophy of science, from the descriptive style. Lakatos (1970) introduced a way of integrating history and philosophy of science – his rational reconstructions. Originally inspired by practices in science of taking a hypothesis and making it confront the data, Lakatos perhaps wanted to apply the hypothetico-deductive method of science to the history of science. An aspect of PS for Lakatos was its historiographical input. PS would be able to tell historians what details to focus on in historical episodes and what factors play a role in science and its change. His method for &HPS would go as follows: a methodology (PS) is confronted with the historical data (HS), then it would be possible to judge the PS via the historical reconstruction it produces and how well it fits with the historical data

(Lakatos, 1970, p. 109). Then, *somehow*, the better reconstruction will be chosen as king.

Within this section, I wish to cover a more general version of the so-called rational reconstructions, viz., top-down philosophy. Top-down philosophy is the process where one takes a philosophical thesis and "tests" it against the historical record. I will show how applying a top-down approach to MTDs leads to many problems, while it is far less problematic for TSCs.

For MTDs, one takes a normative prescription and looks at the HS for some form of confirmation/falsification of it. Some scale can be used to compare competing normative theories. This top-down approach has many complications for an MTD. From the previous discussion, one aspect of MTD is that it aims to improve scientific practice. If that is the case, we then arrive at the problem that the HS is clearly in the past, and does not have within itself a novel improvement to scientific practice. At best, one can argue for the revival of a historical scientific practice that has been discontinued. Cases like this however seem to be few and far in between. It seems counterproductive for an MTD to use the HS in order to justify its claims. An MTD wishes to guide science in novel fashions, and not describe previous failures or successes. Thus, it seems that a top-down philosophy cannot be used effectively to test MTDs for their guiding benefits to science, but this is but a small problem compared to the next for an MTD.

The next problem is circularity (Brown, 1988, p. 54; Giere, 1985, p. 333). If one uses some MTD to make a historical reconstruction, and then uses that reconstruction to justify itself, isn't that circular? Is it not the case that the historical reconstruction will be inevitably shoehorned and by this vice, enable its own confirmation? After all, the reconstruction may simply relegate the disconfirming evidence to irrational factors in the historical data.[4] Even if we grant a means to avoiding this problem of shoehorning, what about comparing competing MTDs? What method would we use to choose between the competing MTDs? MTDs *are* our means of choosing between theories, and if we decide to use an MTD in order to pick between MTDs, then our results will always be skewed. We cannot justify the use of an MTD in choosing MTDs as that would be circular. This circularity seems inevitable, but maybe we can make a concession to make it work.

Let us try using a third-party method to pick between MTDs and their respective rational reconstructions. Perhaps we can pick something like the hypothetico-deductive method (à la VPI Project) and use that? If we do this,

[4] This is comically seen in Lakatos (1970, p. 107) who relegates the disconfirming history to the footnotes.

maybe we can decide on which reconstruction is better, but the problem remains when we are testing this MTD of theory choice itself. Giere summarizes this issue quite well: "history of science cannot be regarded as providing empirical evidence for a philosophical account of empirical validation" (Giere, 1973, p. 294) because that would presuppose an account of validation. There seems to be no escape from circularity or contradiction within such reasoning, even by using a third-party MTD to arbitrate choice of competing MTDs.

Perhaps the most common objection of testing an MTD with historical reconstructions is the "is-ought" problem (Brown, 1988, p. 55). In the case of a reconstruction, are we not using descriptive historical data to justify/falsify a normative MTD? We are. It is generally taken that no descriptive statements can have bearing on normative statements directly, but I shall leave a more in-depth discussion of this problem for the next section where I tackle bottom-up philosophy.

I now switch my attention to using top-down philosophy for a TSC. Since a TSC is descriptive, it may attempt to mirror some hypothetico-deductive reasoning as seen in science. We take a descriptive thesis, reconstruct some historical episodes using this descriptive thesis and compare it to other reconstructions stemming from different theses. It is clear that as a descriptive science, a TSC will be able to benefit from such a historical study of science, it does not need to guide science like an MTD.

Do we run into circularity when doing top-down philosophy with a TSC? Let us consider a hypothetical scenario. Suppose we have several theses on how theory choice takes place in science and we wish to compare them. What will we use to choose between these competing theses? Evidently, it is some normative criteria. These criteria will be whatever the academics consider to be the method to use at the time, the *modern* method. Notice that this modern method is chosen by the academic community as being the best available at the time. Whatever methods are being tested in history, are the historical methods, which may or may not coincide with the modern method. It is crucial to note that the modern method is not using HS to justify itself, it is already taken for granted, and is merely being used as a tool for generating descriptions of science.

There are, however two remaining problems for TSCs in top-down philosophy: *shoehorning* and *cherry-picking*. Perhaps a historical episode is shoehorned by the TSC to promote itself, or perhaps certain historical episodes are cherry-picked to support a thesis in a TSC. After all, history is vast and one can find a confirming or a disconfirming instance for almost any descriptive statement (Nickles, 1986, p. 256, shares this sentiment). However, one is not limited to a single framework of historical constructions as one can have multiple competing TSCs all reconstructing the same historical episodes, and we can pick between

them without a problem. This essentially allows us to deem certain TSCs as better. Thus, shoehorning can be avoided by a proliferation of competing TSCs. Cherry-picking is a problem to all empirical studies. Eventually, methods are developed in order to diminish its effects. Consider something like large historical studies for a TSC, or even randomized ones. Although they are not currently possible, one of the goals of developing a TSC is to be able to get these large coherent databases in order to do these kinds of historical and randomized studies (Scholl & Räz, 2016, p. 230). Both shoehorning and cherry-picking are resolvable over time, as the field of &HPS grows. Although currently these problems are real, the key point here is that one day this cross-disciplinary study of &HPS may become more empirically sound.

Several strides have been taken in this direction. We already have a good example of what competing historical reconstructions could look like. Martinez-Ordaz & Estrada-Gonzales (2018) have already done something of the sort. In their work, they focused on a historical episode in parasitology on the dilemma between biogenesis and heterogenesis of parasitic worms. They reconstruct the same historical episode in three ways called stories A, B, and C. Story A reconstructs this episode as a supporting case of social factors being the only determinants of theory choice. They label this a poor historical construction as it offers little reinforcement for the thesis of social factors being the only factors in theory choice, as it merely says that in the absence of rational factors, social factors were used to choose between biogenesis and heterogenesis. Story B takes a different route and paints a picture of inconsistency toleration between biogenesis and heterogenesis, them being both accepted, but in different empirical domains. They discuss that this is a confirming case, but there is no explicit mention within the historical sources that there was any sort of inconsistency toleration, merely these two (a priori) inconsistent theories seemed to have been both accepted at one point in the medical community. Lastly, in story C, the episode is reconstructed as a case of increase in problem-solving ability. The authors claim that this is the best story as the conclusion is well-reinforced.

As can be seen, from this type of work, we have several competing reconstructions that can all be compared and judged by an &HPS community. Which one is better is not inherent, but to be decided within the community. At the same time, it is not necessary to choose a better reconstruction; one can even keep several so long as they are compatible. So, it seems that MTDs fundamentally struggle with a top-down approach, while TSCs can overcome their deficiencies with time.

5. Bottom-up &HPS

After covering the top-down approach to &HPS, let's have a look at its counterpart. Bottom-up &HPS is the idea to start with historical data and then generalize to some PS. This general concept was often called naturalized philosophy (for example, Giere, 1985; see also Laudan, 1990, who offers a form of naturalized epistemology with normative conclusions). In &HPS, this process involves inferring general statements from historical case studies in science. I shall examine the problems of bottom-up &HPS and notice a fatal flaw of this approach, namely its inability to deal with theory-ladenness on its own.

To start, I will deal with an issue that is common to any conception of bottom-up &HPS, that is, do we only generalize from HS or from what else? The question was brought up whether we should have the "bottom" be composed of purely HS, or if it should include cognitive science, psychology, sociology of science or any other disciplines (Giere, 2011, p. 62). What these disciplines share in common is that they all have some kind of import into the functioning of science. Since we study science as done by humans, cognitive science and psychology may reveal certain behaviors or reasoning patterns that we all use, these patterns could then have some kind of import into how science functions. An example could be our tendency to visualize things and how theories with "better" visual representations came to be accepted by communities. Similarly, sociology might offer many other generalizations relevant to science, e.g., the fact that we publish our research in books or write things down may influence the preservation of scientific ideas. So, to restate the question asked by Schickore (2011a, p. 465), is HS the correct "bottom" to naturalize "up" from? Well, that's up to the &HPS community to decide. However, one can say that there is no reason to exclude any of the given disciplines and they can all complement one another. We need not only do historical generalizations, or only sociological generalizations, all of these generalizations can offer different relevant bases and will (if anything) ameliorate our understanding of science (and its philosophy). So, the choice of "bottom" is not so problematic, but bottom-up approaches have numerous other problems.

I now turn to bottom-up &HPS with MTD. The idea is as follow, we start with some historical data and we want to generalize into some normative statements. The major proponent of such an approach was Laudan (1990), who made a scheme of "hypothetical imperatives" in order to move from historical data to normative statements. The main logic of how this would work is the ability to make theses on "why science worked" (Laudan, 1989, p. 216). If you can say why science worked in one episode, maybe you will be able to say what should be done if you want science to work in the same way again (what means should you employ to reach what end). This is heavily discussed within the literature and I will not focus on Laudan himself. I consider the more general

case of simply starting with some historical data and trying to reach a normative output.

I begin with the fundamental problem, the is-ought problem. Historical data is necessarily descriptive, so wouldn't going from historical data to MTD be committing the naturalistic fallacy? The is-ought problem can be expressed as "there is no consistent set of purely descriptive premises *D* from which a purely normative conclusion *N* follows, which is not logically true" (Schurz, 1997, p. 11). Bottom-up &HPS seems to be clearly disregarding the is-ought problem which constitutes committing the naturalistic fallacy. But, what if we had some type of bridge principle to go from descriptive propositions to normative ones? One must first appreciate that analytically true bridge principles are not possible (Schurz, 1997, pp. 276-277). What we are left with is synthetic bridge principles, i.e., not necessarily true normative theories that in conjunction with descriptive statements can generate further normative statements (Schurz, 1997, pp. 31, 277-285). These synthetic bridge principles cannot be justified a priori, but we can still accept them and make use of them (just like a scientific theory). Here is a simple example of a synthetic bridge principle: if you promise to do something, then you should do it. If one made use of such a bridge principle in HS, one could find instances when scientists promised something, then conclude that they should do that thing they promised. However, of course, such a bridge principle would be of no interest to most philosophers. Laudan had his own idea of a bridge principle. If one could identify why a scientific mean worked at achieving some scientific end (let's assume that this distinction is unproblematic), then one would be able to get a statement of the form: if you want such and such aim, do such and such mean (a hypothetical imperative). But, why would we choose Laudan's bridge principle over any other one? The decision cannot be a priori, and it would be a shame to call it arbitrary as well. We could of course, under the method of the &HPS community come to agree on a choice of synthetic bridge principle and make use of it. It is conceivable, it would circumvent confronting is-ought problem, but is there any other problem it introduces?

Perhaps there is. From the previous discussion, I noted that one task of MTD was to improve science in some way, but would doing bottom-up &HPS with a synthetic principle be able to achieve that? We run into the same problem as with top-down, an improvement to current scientific practices is almost certainly not going to be found within HS and we will be unable to inductively generalize from historical data in reaching such an improvement. At best, we will be able to generalize some MTDs of the past, find some common trans-historical ones and make historical theses, rather than ones that have any kind of applicable normative output. These trans-historical MTDs come in the form of seeming platitudes, such as predictive power should increase over time. This,

I believe, makes the bottom-up conception a bit dry for an MTD even with a synthetic bridge principle.

The main problem I address is that of the theory-ladenness of historical data. Just like how in scientific observation, we need concepts to make sense of percepts, in HS it is required to have some theory to make sense of the historical record (the documents, artifacts, etc.). As Garber put it, "the history of science (as opposed to our normative judgments about cases in the history of science) cannot be construed as evidence [for any PS]" (Garber, 1986, p. 106). It is appreciated that every historian must work in some framework in order to construct any type of narrative or historical piece, they must assume some kind of *Weltphilosophie* and arrive at their own judgments (Hanson, 1962, p. 574). There is no stand-alone historical data, it must be interpreted via some theory. How can we then proceed to doing HS in a coherent fashion? We must adopt some descriptive theory to describe the HS, namely a TSC. There is of course no a priori TSC to use for neutral historical data. We are forced to be explicit in our commitments, but they may change eventually, and history will then need to be revised or rewritten. Bottom-up &HPS will need to assume at least some form of framework (at least a taxonomy) in order to do any HS. Doing so will no longer be bottom-up &HPS, but instead will involve top-down &HPS since we are assuming some TSC before getting to any HS.

This exactly mirrors the problem in actual science. Science never inducts from neutral data, it's always theory-laden. One needs to start with some conceptual framework to make sense of the data. In the same way, theory-ladenness in &HPS needs to start with some conceptual framework. This of course does not mean we are trapped in this framework, nor that it is the only one we can use. As before, we can reconstruct things in different ways and accept several reconstructions. One important thing to note is that historians frequently omit being explicit with their given framework (Hull, 1992, p. 472; historians typically assume some theory of rationality in passing and use it to explain changes in intellectual history). Having a TSC is indispensable in this regard, as having a consistent vocabulary, precise meanings to words used within a historical narrative, an understanding of the epistemic processes present in science, etc. are all crucial components that are lacking in particularist approaches to HS. In order to properly make use of bottom-up &HPS, we will need to couple it with a top-down approach.

6. Iterations

Both the top-down and bottom-up methods of &HPS seem to have been based on some form of hypothetical-deductivism and inductivism respectively from

the sciences. But, are these methods truly how science proceeds? Schickore would say that top-down and bottom-up are merely modes of presentation of scientific research rather than a mode of the production of scientific knowledge (Schickore, 2011a, p. 473). It is instead proposed that science actually follows something along the lines of an iterative fine-tuning of theoretical concepts that interpret observational data, which then in turn gives insight to new theoretical concepts and the process repeats itself. I shall call such an iterative production of knowledge simply as "iterations".[5] I shall present several examples of such iterations, including a scientonomic conception.

Let us start with Mill's inductivism as presented in Knowles (2003). Here Knowles applies Mill's inductivism in the context of scientific norms to show that they are fine-tuned in the same fashion as observations and hypotheses are fine-tuned (Knowles, 2003, pp. 71-73). That is, one has a working hypothesis, produces some observations with that hypothesis, evaluates those observations and uses them to justify the next hypothesis. Similarly, this can be applied to norms on the next (meta-)level up. One has some norms and evaluates the success of the hypotheses accepted under such norms, and the results of this evaluation justify new norms. Such a process is an iteration between norms and successes of hypotheses. In Figure 2.1, directions of justification/support are depicted by the sloping/vertical arrows, while the bold horizontal arrow indicates "the progress (assumed to be) being made over time" (Knowles, 2003, p. 71):

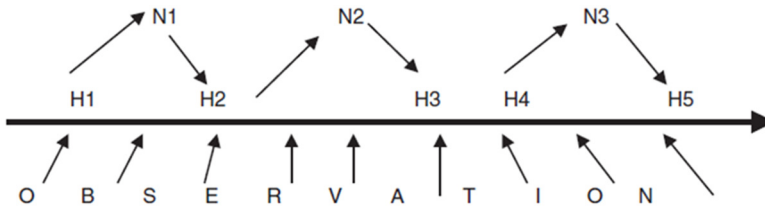

Figure 2.1: Knowles' diagram

[5] This steals from Hasok Chang's naming, but I believe it is a nice picture of how it works. Similar conceptions of iterations have been studied by numerous authors, and they all have a similar structure.

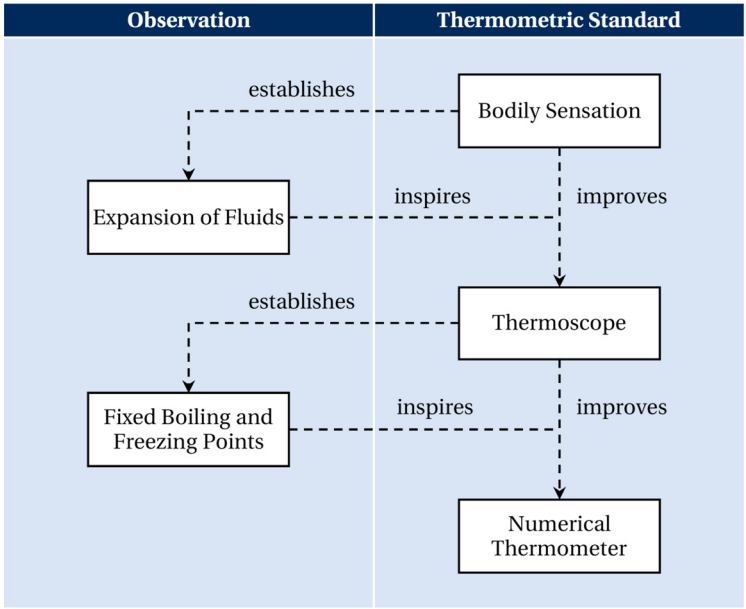

Figure 2.2: A general form of iteration

Chang (2011) also presents us with an iteration. He calls it an epistemic iteration and applies it to the nomic problem of measurement. In the context of temperature measurement, the epistemic iteration progresses as follows. One starts with a method to measure temperature, then tests it out (perhaps in comparison to other methods) and evaluates the results, which inspire fine-tunings of the next measurement method, and the process repeats. The idea is that over each iteration, you fine-tune your temperature measurement method even further, eventually arriving at more and more suitable measurement methods. Epistemic iterations are not limited to measurement methods, one can apply them to any process of inquiry. You start with some ill-defined methods or concepts, test them out, re-evaluate your methods or concepts based on the results of the previous testing, and repeat. Hence, Chang has a general form of iteration (Figure 2.2).

Scholl & Räz (2016) also have their own version of helical iterations called cyclical HPS. In this case, abstract philosophical concepts enlighten historical episodes which provide concrete results that inform further abstract concepts, and the process repeats. Although they do repeat Chang's idea, they now show how it can be applied to &HPS in specific.

Next is Schickore's hermeneutic-critical style. It can be summarized as follows (Schickore, 2011a, p. 471):

Initial case judgments – judgments that identify portions of the historical record as noteworthy – and provisional analytic concepts are gradually reconciled until they are brought into equilibrium.

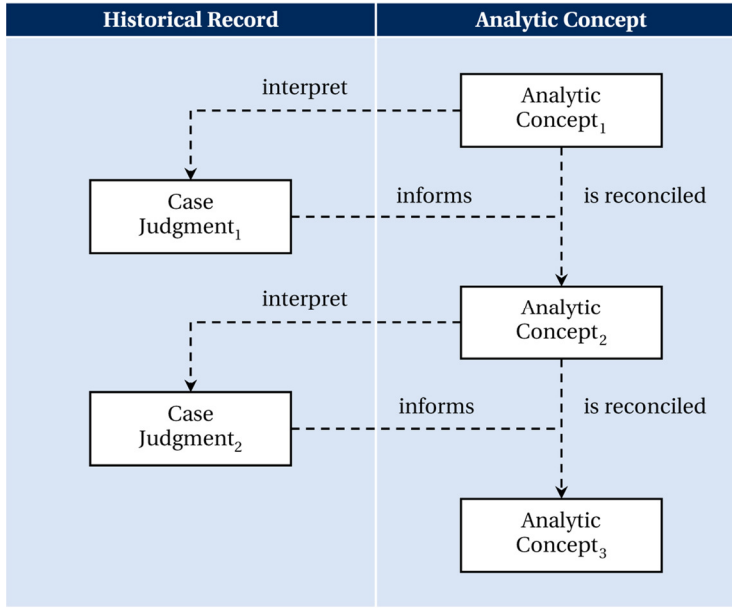

Figure 2.3: Iteration in &HPS

Although Schickore dismisses top-down and bottom-up philosophy as modes of presentation, I believe there is a clear connection between those two and the alternative iterative process Schickore suggests. It seems that by taking a provisional concept, one is effectively doing some form of top-down &HPS, at least locally, as they assume a framework and use it to interpret an episode. This interpretation is then inductively used to refine the concepts, this step consists of the bottom-up portion of Schickore's iterative process. As such, Schickore's hermeneutic-critical style (Figure 2.3) seems to be a sequence of local top-down and bottom-up steps, used to gradually refine philosophical concepts using historical research.

Lastly, I present the scientonomic conception of iterations. The second and third scientonomic laws predict an iterative process. By the second law, theories come to be accepted through a satisfactory assessment by the method of the time (Patton, Overgaard, & Barseghyan, 2017). Then, by the third law, methods come to be revised through the newly accepted theories (Sebastien, 2016). This illustrates an iterative process where methods are fine-tuned by

newly accepted theories. An example from clinical epidemiology is visualized in Figure 2.4.

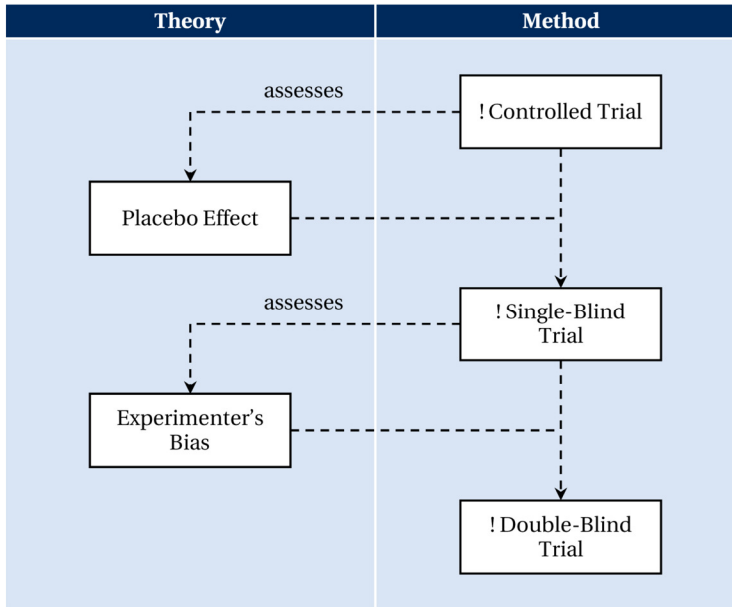

Figure 2.4: An example of iteration from clinical epidemiology

We see here that the method of control trials is able to identify there being a placebo effect. This in turn inspires a new method, namely that of a single-blind trial, making for the first iterative development. The single-blind trial is then able to identify the experimenter's bias, which makes the double-blind trial deducible by the third law. This makes for two iterative steps of scientonomic iterations.

Overall, I propose a general conception of iterations inspired by these examples. All the examples mimic a form of scientific production of knowledge, as an iterative interaction between theory and evidence. A theory is used to interpret evidence, which is then used to (dis)confirm the theory itself, or other competing theories. Iterations are summarized diagrammatically in Figure 2.5.

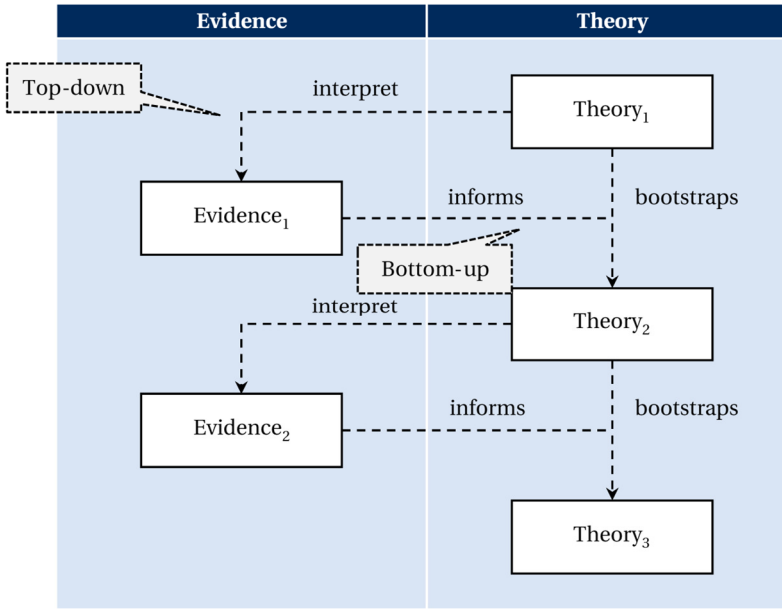

Figure 2.5: Iterations summarized

Notice that we have two types of steps here. Theories may interpret evidence; this constitutes a top-down step of the iteration. Then, this evidence may be used to inform new theories; which constitutes a bottom-up step of the iteration. This makes the iterative production of knowledge not restricted to either top-down or bottom-up approaches, but actually integrates the two in a seamless fashion. We avoid the problem of theory-ladenness of bottom-up approaches, as the prior top-down step sets up a framework to interpret the evidence. We further are able to appropriately generalize from the evidence without having to propose new hypotheses seemingly out of nowhere, the hypotheses are informed by prior evidence. Lastly, we see how prior theories may build upon themselves in what has been typically called a 'bootstrapping' step, though it should be clear that this is a misnomer in the case of iterations as interpreted evidence (along with theories) plays a role in this step. This process generalizes the iterative production of knowledge.

All of these approaches to iterations have been able to fruitfully study either science itself, or offer means of studying science. Iterations avoid the nefarious problems present for top-down and bottom-up approaches to studying the history of science, and have been shown to offer interesting takes on historical episodes. I hope that this recognition of the iterative processes present in studies of science is able to further progress &HPS as a field. I believe that through iterations between PS, acting as theory, and HS, acting as evidence,

&HPS can be united into fruitful areas of study, as the exemplary authors discussed have proven.

7. Conclusion

In explicitly characterizing the relevant PS for &HPS I proceeded to study the methods by which the HS informs PS. I discuss HS as food for thought for PS, a top-down approach to &HPS, a bottom-up approach to &HPS, and finally culminate in a discussion of iterations. I hope to have shown that iterations are present both in science and in studies of science. Moreover, I hope to have shown that iterations serve as an interesting instrument for studying epistemic continuities as they identify a powerful mechanism by which the progression of knowledge can be facilitated.

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