

Chapter 8

Beyond Case-Studies: History as Philosophy

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8.1 The Trouble with Case-Studies, and the Active Philosophical Function of History

What can we conclude from a mere handful of case-studies? This has been a vexing question for integrated history and philosophy of science (HPS). The field of HPS has witnessed too many hasty philosophical generalizations based on a small number of conveniently chosen case-studies. This was seen as detrimental to philosophy and history both. On the philosophical side, case-studies may end up as empty gestures parading as evidence confirming one's pre-existing biases about the nature of science and its methods. At best what we get is "grand conclusions by induction from absurdly small samples", in Richard Burian's words (2001, 388). The deeper problem, as Joseph Pitt (2001, 374) put it, is that "if philosophers wish to use historical cases to bolster their positions, then . . . we will have to figure out how to relate the history to the philosophical point without begging the question." On the historical side, even philosophically sympathetic historians despaired of the oversimplifications that philosophers were apt to make of complex historical material through the case-study approach. John Hedley Brooke's complaint is typical and apt (1981, 257): "When the circumstances and the problems were so complex, the isolation of a single philosophical or methodological point as the key to an adequate explanation must lead to a distortion of emphasis." I believe that the neglect to clarify the nature of the history–philosophy relationship in case-studies has contributed decisively to a widespread disillusionment with the whole HPS enterprise.

Emblematic of this disillusionment is Thomas Kuhn. He had a particularly sharp reaction against Imre Lakatos's explicit treatment of history as testing-ground for philosophical theories of scientific method, with his predilection for rational reconstructions of history. Against Lakatos's dictum (1971, 91) that "history of science without philosophy of science is blind", Kuhn retorted (1971, 143): "What Lakatos

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conceives as history is not history at all but philosophy fabricating examples.” But how did Kuhn, who thought that even science did not have paradigm-independent facts, assume that historical facts could be discerned without a philosophical framework? Kuhn’s answer, given five years later: “The historian’s problem is not simply that the facts do not speak for themselves but that, unlike the scientist’s data, they speak exceedingly softly. Quiet is required if they are to be heard at all.” (Kuhn 1980, 183) Kuhn argued that history of science and philosophy of science had fundamentally different goals, and therefore “no one can practice them both at the same time” (1977a, 5). All this is quite ironic coming from the man who seemed to cross the history–philosophy boundary so freely, and whose work was probably more responsible than anyone else’s for encouraging the drawing of philosophical conclusions from historical cases. Wasn’t it Kuhn who had uttered these famous words as the very first sentence of *The Structure of Scientific Revolutions* (1962, 1): “History . . . could produce a decisive transformation in the image of science by which we are now possessed”? Perhaps, as pioneers often do, he felt that he had unleashed a Frankenstein, and the tone of his latter-day condemnation of “historical philosophy of science” is consistent with this interpretation (Kuhn [1991] 2000). Kuhn did agree, of course, that philosophy of science should ultimately be informed by some knowledge of scientific practice (historical or contemporary). But he 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 ideas are needed for the understanding of *any* concrete episode, so we could not avoid them even if we only ever had one episode to deal with. We cannot understand scientists’ actions, not to mention judge them, without considering them in abstract terms (such as “confirmed”, “coherent”, “observation”, “measurement”, “explanation”, “simple”, “novel”, “self-interested”, “curiosity-driven”, “collective”, “honest”, “open-minded”, etc.). Any concrete account requires abstract notions in the characterization of the relevant events, characters, circumstances and decisions. If we extract abstract insights from the account of a specific concrete episode that we have produced ourselves, that is not so much a process of *generalization*, as an *articulation* of what was already put into it.¹ To highlight this change of perspective, I prefer to speak of historical “episodes” rather than “cases”. When we have an episode of *The Simpsons*, or *Buffy*

¹It may even be an act of self-analysis, in case the episode was initially narrated without a good awareness of the abstractions that guided its construction.

the Vampire Slayer, or what have you, the episode is not really a case or an example of whatever the general idea of the show might be. Rather, the episode is a concrete instantiation of the general concepts (the characters, the setting, the type of events to be expected, etc.), and each episode also contributes to the articulation of the general concepts. To be sure, this analogy is very imperfect, but it does express something relevant about the relation between concrete historical episodes and abstract philosophical conceptions.

Philosophers will be pleased but not surprised to hear that abstract conceptions are necessary for the telling of history. The more novel idea I would like to propose here concerns the opposite direction of dependency—doing history can help our philosophizing, even as we refrain from making crude inductions. When there are no ready-made philosophical concepts through which a given historical episode can be properly understood, the historian needs to craft new abstract philosophical concepts. *This necessity should not be resisted or avoided, but actively embraced as a great intellectual opportunity.* Instead of sitting back and complaining that existing philosophical frameworks are inadequate for understanding real history, historians can actively engage in the creation of new philosophical ideas through their concrete investigations. If the historians won't do it, philosophers can jump in. Regardless of the official affiliation of the scholars involved, we will have history and philosophy being done at the same time, in spite of Kuhn's verdict on the impossibility of this enterprise. History-writing can be a very effective method of philosophical discovery.

Once an abstract idea has been generated, it needs to show its worth in two different ways. First of all, its *cogency* needs to be demonstrated through further abstract considerations and arguments. This is where philosophy takes up the reins again, to examine carefully what has been generated through historiographical necessity. If the abstract idea is deemed to be cogent in itself, then its range of applicability needs to be checked. This can only be done by trying to the framing of various other concrete episodes, with history in the driving seat again. An abstraction becomes *general* only when it has been *applied* widely. Successful application functions as confirmation, but without the presumption of universality in what is confirmed. This is consistent with Burian's view on how a bottom-up approach to case-studies can contribute to the establishment of "regional standards" rather than "universal methodologies or epistemologies" (Burian 2001, 400).

I will illustrate these claims through two investigations in HPS from my own recent work. In both cases, I am not going to present significant new content that is not already in my other publications, but I do hope to present some significant new reflections on the existing material. This also raises a question of reflexivity, already noted by Brooke and Burian: how can we use case-studies to show how to go beyond case-studies?! I hope my answer will be clear as I go on: it is through wrestling with these concrete studies that I crafted my abstract insights about the history–philosophy relation, as the existing ideas concerning that relation did not seem to be helpful.

8.2 Temperature Measurement and Epistemic Iteration

8.2.1 Circularity and Reliability in Measurement

The first episode comes from my work on the history and philosophy of temperature measurement (Chang 2004, esp. chapter 2). When I started this project, I was thinking about the theory-ladenness of observation, and the question of how observations can be justified despite theory-ladenness without falling into a vicious circularity. Having got tired of trying to explain quantum physics to people who didn’t really want to know,² I wanted to deal with simpler scientific material. I thought, temperature would be nice, since it is a fundamental physical concept that everyone has some sense of. At least everyone knows how to use a basic thermometer.

Does something as simple as an ordinary mercury thermometer rely on theory? Yes: The key assumption is that mercury expands uniformly (or, linearly) with increasing temperature. Think about how simple thermometers are made. We take a glass tube partly filled with mercury. We put it into freezing water, see how far the mercury comes up, and mark that point 0; we put it into boiling water, and mark the position of the mercury column 100; then we divide up the interval evenly to make a scale, calling the halfway point between zero and hundred “50”, and so on. So what we assume is that when the temperature is exactly 50°C, the mercury comes up exactly to the halfway point. But is that true? Table 8.1 shows some simple data from the mid-nineteenth century, which illustrates why practicing scientists would have worried about this philosophical-sounding problem. What this table shows is that thermometers filled with different liquids will differ very seriously in the middle of the scale, even if they are graduated to agree exactly at the fixed points. How can we tell which one is correct?

A good physicist will think that assumptions about the behavior of thermometric fluids should of course be tested by experiment. So we take some data, and make a plot of the volume (height) of mercury against temperature, and see if the points lie on a straight line. But how would we get the temperature values, unless we already had a thermometer we can trust, which is exactly what we don’t have yet? We could

Table 8.1 Comparison of thermometers filled with different liquids

Mercury (°C)	Alcohol	Water
0	0	0
25	22	5
50	44	26
75	70	57
100	100	100

Data from Lamé (1836, 1: 208)

²For instance, Chang (1995) addressed these concerns as they pertain to energy measurements in early quantum physics; almost exactly the same issues are played out in temperature measurement, as discussed in Chang (2004, chapter 3).

try using a different kind of thermometer to take the temperature values, but then we would have to ask how we know *that* thermometer to be correct. Alternatively, we could try to make a theoretical argument about whether mercury expands linearly or not, but that would require a detailed theory of thermal physics, which we would, in all likelihood, have to test by experiments that involve the measurement of temperature.

Like a good philosopher, I made an abstract and precise formulation of the problem:

- (i) We want to measure quantity X ;
- (ii) If quantity X is not directly observable, we infer it from another quantity Y , which is directly observable.
- (iii) For this inference we need a law that expresses X as a function of Y .
- (iv) But the form of this function cannot be discovered or tested empirically, because that would involve knowing the values of both Y and X , but X is the unknown variable that we are trying to measure.

I called this “the problem of nomic measurement” (Chang 2004, 59, 89–90). The precise formulation of the problem made me realize two things: first, it must be a general problem pertaining to nearly every attempt to justify a method of measurement; second, there was no apparent solution to it!

So I was about to give up. Then it occurred to me: “But they have *done* this!” Scientists today do know exactly how mercury expands with temperature (and they tell us that the expansion is far from linear). Someone, sometime, must have figured out how to solve the problem, and the least I could do was to go and look at the actual *history* to learn how it was done. So I began to look up the history of thermometry, in which I became immersed for nearly a decade. Clearly, right from the start I approached this history with a philosophical problem in mind. But I did not go in with a particular philosophical view (hypothesis, even) to confirm, or refute. Rather, I went to the history with an unresolved philosophical question, with a necessarily open mind about what answers I might find. This is a mode of work consonant with Burian’s response to Pitt: “case studies, properly deployed, illustrate styles of scientific work and modes of argumentation that are not well handled by currently standard philosophical analyses” (Burian 2001, abstract, 383).

Looking deeper into the history at first deepened my philosophical puzzle rather than resolve it. Every trail I followed into the past looking for the moment of rational foundation of thermometry seemed to lead into a thicket, not a clearing. I couldn’t find any satisfactory foundationalist answers on how thermometers were validated, which is what I was after. Some scientists did propose foundationalist solutions, but they were all duly shot down by other scientists. For instance, Jean-André De Luc’s “method of mixtures” seemed to offer a solution: mix up freezing water (at 0°C by definition) and boiling water (at 100°C by definition) in the ratio of $a:b$ by weight; and the resulting mixture should be at $[b/(a+b)]^{\circ}\text{C}$, and whichever thermometer indicates that temperature is correct (see Chang 2004, 60–68, 93–94 for details). With this method De Luc nearly generated a consensus on the foundations

of thermometry, but then his critics eventually pointed out that his method assumed that the specific heat of water was constant, which was a groundless assumption. Worse than that, it was impossible to test that assumption of constant specific heat, without reliable temperature data. De Luc's critics destroyed his solution, without offering a better one.

What became clear through such examples was the failure of foundationalism, which was the basic philosophical framework with which I began this work, without even realizing that clearly. Now, with my limited philosophical imagination, I thought that if foundationalism did not work, then the fallback position could only be coherentism. Quine's web of belief and Neurath's boat beckoned, as I drowned in the historical mess. But I also found that the primary literature I was looking at clearly conveyed a sense of *progress* in thermometry, which could not easily be captured in straightforward coherentism in which the only validation of a proposition comes from its logical consistency with other propositions we believe. I had a great difficulty in making sense of this historical episode, as long as my understanding was limited by the philosophical framework defined by the opposition between foundationalism and coherentism. My limited philosophical imagination was stuck in a false dilemma: justification is by a self-evident foundation, or it is merely circular.

8.2.2 *Epistemic Iteration and Progressive Coherentism*

I managed to get out of that impasse by crafting the idea of "epistemic iteration". The concept of iteration allowed me to see that the circularity in question was more like a helix, in which scientists began by *presuming* the validity of an untested measurement method, starting a process of inquiry which eventually doubled back on itself to refine and correct its own starting assumptions. Epistemic iteration is a process in which we create successive stages of knowledge, each building on the preceding one, in order to enhance the achievement of certain epistemic goals, such as precision, consistency, scope, explanatory power, simplicity, and so on. We do not start with indubitable facts, or unrevisable axioms. Instead, we start with a system of knowledge that we recognize as imperfect or even faulty, which is used for its own improvement. No fixed algorithm tells us how to proceed. But we have the impetus and constraints provided by the epistemic values and aims that we adopt. (The idea of epistemic iteration is explained fully in (Chang 2004, chapter 5); I will only provide an introductory summary here.)

Inventing the concept of epistemic iteration helped me capture the historical development of thermometry more sensibly. First, people started with sensation, assuming that the feeling of hot and cold corresponded to high and low temperatures. Then they created "thermoscopes", by finding certain materials that seemed to expand and contract reliably as things felt hotter and colder. Thermoscopes extended the observable temperature range, and they were also more sensitive than human sensation. When thermoscopes were well enough established, they also began to *correct* sensations, as people ceded epistemic authority about temperature

to these simple machines: so you might say “I feel colder but the mercury is not moving—I must be getting a fever.” And so on. This kind of refinement and self-correction happened again when people made numerical thermometers starting from thermoscopes, by attaching scales to them.

The making of numerical thermometers led to further iterative improvements. Although there never was any indubitable reason for believing that any particular thermometric substance expanded uniformly, various numerical thermometers were made, each on the presumption of uniformity, and eventually those that did not return self-consistent numbers were rejected; Victor Regnault’s work in the 1840s was crucial in that development. The most self-consistent thermometric fluid turned out to be air, and the readings of air-thermometers provided the basis for William Thomson’s thermodynamic theory and the Joule–Thomson experiment, with the results of which the physicists corrected the air-thermometer used for that very experiment. In this whole process there was progress *without* a firm foundation, in terms of increasing precision, consistency, and scope of the measurements.

So, that was the origin of my ideas about epistemic iteration, born out of historiographical necessity. The next step was to examine, develop and apply these ideas more broadly. As I do not have the space to go through the details of the doctrine of “progressive coherentism” that I developed (see Chang 2007), I will just show you a metaphorical picture of it (Fig. 8.1). I mentioned my disillusionment with foundationalism. The traditional foundationalist picture of building knowledge on a firm ground does not work, since there is no such thing in empirical science

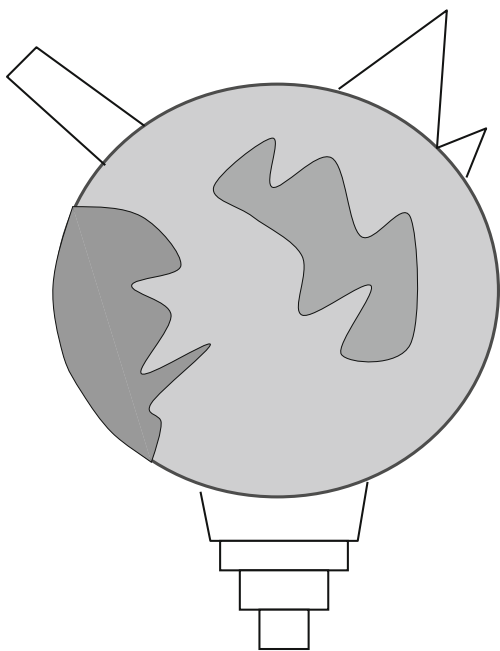


Fig. 8.1 The progressive-coherentist metaphor of building on a round earth

corresponding to that firm ground. But foundationalists have actually been sitting on the perfect metaphor for the kind of ideas I want to express—we only have to remember that the earth is not flat. In real construction, we don't build upward on a flat earth, but outward on a round earth; that is how the metaphor has to work, too. There are no fixed points and no up-and-down in the universe. We build on the earth not because it is firmly fixed anywhere, but because it is a large and dense body that attracts other things, and we happen to live on it.

I have found this framework of progressive coherentism quite useful in thinking about other historical episodes, so at least to my own mind it has proven its broad applicability. The clearest applications are to other cases of nomic measurements. A number of other system in which numerical precision is improved also seem to rely on a process of epistemic iteration. I have also briefly indicated how progressive coherentism applies to the development of chemical analysis (Chang 2007, section 4). Even instances of scientific revolutions could be illuminated by seeing them in progressive-coherentist light, since new paradigms do build, partly and at least initially, on the empirical basis of the older paradigms that they replace.

8.3 The Chemical Revolution: Pluralism and Systems of Practice

8.3.1 *The Chemical Revolution as a Puzzle*

The second episode I want to discuss is the Chemical Revolution. This comes from current research that will be incorporated into my forthcoming book, titled *Is Water H₂O?* This was research born out of teaching. I covered the Chemical Revolution quite routinely in my classes, but always had to leave it as a puzzle yet to be explained. The more I myself learned about it, the less I could convince myself that there were good enough reasons for a clear majority of chemists to go over to Lavoisier's new chemistry. As a quick reminder for the incredulous, here is one very simple case which illustrates how the two theories could explain the same range of phenomena equally well. This is the case of the composition of water.³ What everyone agreed on was the observation of the following reaction:



Lavoisier interpreted this as a straightforward proof that water was a compound:



³Priestley maintained his preference for the phlogiston theory until his death, and it is quite telling that his last stance was a book titled *The Doctrine of Phlogiston Established and That of the Composition of Water Refuted*, published in 1803. By that time, most others had converted to Lavoisier's theory.

Isn't this enough to refute the phlogiston theory, which held that water was an element? Not so, as there is a coherent alternative interpretation based on phlogistonist ideas (advanced by Cavendish and Priestly):

$$\textit{Dephlogisticated water} + \textit{Phlogisticated water} \rightarrow \textit{Water}$$

And there are many other similar instances of empirical equivalence between the two theories.

So how do we explain the mass-conversion of late eighteenth-century chemists to the Lavoisierian doctrine? It is possible that scientific change happens due to rhetoric, interests, ideology or fashion, but I think in this case those factors give only a partial explanation. Yes, it was Joseph Priestley in Britain that led the resistance to the new "French chemistry", but this was the same Priestley who was hounded out of his native land for his support of the French Revolution, the same revolution that would two years later guillotine Lavoisier. And so on. The contextual picture is very complex, and will not give us a simple explanation of anything. (In this day and age, we might have to say: when *external* explanations are insufficient, we must invoke *internal* ones.) In any case, as philosophers we may well want to retain the question of whether the decisions that the past scientists took were scientifically justified or not.

Elsewhere (Chang 2009, 2010) I have argued in some detail that none of the standard philosophical criteria show sufficiently that the phlogiston theory was clearly inferior to the oxygen theory. Just to summarize the outcome here: it is not the case that the phlogiston theory was in the end refuted by empirical evidence while Lavoisier's theory was not. It is, for one thing, not the case that the phlogiston theorists had to assign negative weight to phlogiston (only a very few did). The idea that the phlogiston theory lacked empirical adequacy compared to Lavoisier's theory does not stand up to scrutiny, even in Philip Kitcher's (1993) very subtle version of it. Perhaps the most common and fatal failure in the standard discourse is the ignoring of the many empirical problems that Lavoisier's theory had. It is also not the case that Lavoisier's theory was inherently simpler than the phlogiston theory, despite Andrew Pyle's (2000) sophisticated defence of this idea. Even Pyle's version of the argument is ultimately founded in the sense that the phlogiston theory unnecessarily complicated things by postulating the existence of an unobservable and redundant substance, namely phlogiston; this argument ignores the fact that Lavoisier also had to postulate just such a substance, caloric, which he proudly put at the head of his table of simple substances.

I think the best integrated-HPS work we have on this subject is still Alan Musgrave's paper of 1976. Musgrave starts by rejecting some old misconceptions (1976, 182–86), anticipating the difficulties of the later works just cited, too. But I don't believe Musgrave's own story works, either. His Lakatosian answer proposes that the crucial factor was the comparative progressiveness of the two competing research programmes. It was rational for chemists to abandon the phlogiston programme because it stopped making successful new predictions and only resorted to ad hoc hypotheses. Musgrave maintains (1976, 205): "Between 1770 and 1785 the

oxygen programme. . . developed coherently and each new version was theoretically and empirically progressive, whereas after 1770 the phlogiston programme did neither.” But Musgrave himself states (1976, 199): “While Lavoisier was failing, Priestley was having great success with the 1766 version of phlogistonism. . . the most impressive experiment of all came in early 1783.” This was the confirmation of the phlogistonist prediction that a calx would be reduced to metal by heating in inflammable air. To support the Lakatosian argument, then, we need to find successful novel predictions that Lavoisier made around 1783 or later. Where are these predictions? Musgrave doesn’t tell us. Was he thinking of the prediction that the oxidation of inflammable air (hydrogen) would produce an acid?⁴ Or the prediction that muriatic (hydrochloric) acid would be decomposed into oxygen and the “muriatic radical”, which even appears in Lavoisier’s list of elements?

8.3.2 Epistemic Pluralism

Faced with these philosophical failures, I started looking for a different framework for understanding the Chemical Revolution. The most promising one seemed to be theory-choice on the basis of epistemic values, as implicit in Kuhn’s *Structure of Scientific Revolutions* but articulated more clearly in his later paper “Objectivity, Value Judgment, and Theory Choice” (1977b). My preliminary conclusion in those terms was that the opposing camps of chemists did uphold different epistemic values. For example, Priestley had an ideal of completeness in recording and explaining every little thing happening in his lab (which he achieved by twisting his theory if necessary), while Lavoisier pursued clean and elegant theoretical uniformity. There was a clear trade-off between these two values, as Kuhn said we should often expect. So it makes sense that many people followed Lavoisier in pursuing uniformity, while some remained with Priestley in maintaining a stronger respect for all the messiness and particularity of phenomena. There was no compelling objective argument that uniformity was more important (or truth-conducive) than completeness. And so on.

If I had stopped there, the payoff from this case-study would have been a small vindication of the Kuhnian view on scientific change against the more traditional view that theory-change in science occurs (or should occur) only for reasons that all good scientists can commonly accept. That would have been a reasonable enough outcome, but I was left with a strong sense of unfinished business. Having studied the Chemical Revolution in depth and detail, my feeling was that if dissenters like Priestley dissented because they had different yet respectable values, that dissent should have been allowed, even fostered. If the phlogiston theory had its own distinct merits, it should have been kept. But it’s not that I thought Lavoisier’s theory should not have been developed and adopted. *They should have both lived*. That was the new insight that the case of the Chemical Revolution had finally forced upon me: a full-fledged pluralism that says multiple theories should have been kept, not a

⁴Again, Musgrave himself points out this failed Lavoisierian prediction, and how long he had struggled with it (1976, 199–200).

petulant relativism that just says whichever theory would have been just as good or bad.

Thus I found that an immediate implication of the Kuhnian conclusion on the Chemical Revolution went against a central assumption in Kuhn's philosophy of science, which has been shared by most of his enemies, and had been accepted implicitly by myself. This assumption is monism. Kuhn maintained (as a matter of both fact and ideal) that in the normal state of science there was one paradigm dominating any given discipline. When normal science goes into a crisis this dominance is challenged, and when there is a revolution the dominant paradigm is overthrown and replaced by a new paradigm, which then enjoys its own era of monopolistic dominance. One paradigm or the other must win in the revolutionary struggle, even if there aren't unequivocally good reasons for the winner to win; that is the way it is, and that is the way it should be.

Rejecting this monist assumption and adopting a pluralist framework of analysis has improved and enriched my account of the Chemical Revolution, presented in full in Chang (forthcoming, [chapter 1](#)). Pluralism is of course not an entirely new idea (and epistemic iteration, too, has been a common enough idea, in various guises), but I believe that it has never seriously been articulated in the context of the historiography of the Chemical Revolution. Freed from the monist straightjacket, my puzzle about the dominance of Lavoisier now receives a new framing as a question about the failure of pluralism. Both epistemic and social factors are smoothly incorporated into the story of the triumph of Lavoisierian dogmatism, with myself as the historian reserving the right to retrospective dissent. Within a pluralist framework it is also easier to make sense of the wide variety of reactions to the rise of Lavoisier, of the various forms that each theory could and did take, and of the interactions between the two theories and their variations. Moving beyond that stage of work, I have also made a full articulation of pluralism in science and its merits (*ibid.*, [chapter 5](#)), and I look forward to the task of applying a pluralist framework to the study of other historical episodes.

8.3.3 *Systems of Practice*

Thinking about the Chemical Revolution has led me to another major philosophical innovation, which is to take *systems of practice* instead of theories as units of epistemic appraisal. Thoughts related to this had bubbled up in my mind on various occasions, but it was the Chemical Revolution episode that made them crystallize. I have already mentioned how I departed from the traditional model of theory-choice in which different theories vie for the title of one exclusive truth, in favor of one in which theories are appraised in terms of various epistemic values. Setting truth aside, as an inoperable aim, I wanted to learn how to make sense of how scientists actually make theory-choice. Therefore considering various kinds of success rather than the ultimate truth, I also came to see that it was not theories in themselves, but what one did with theories that was successful or unsuccessful—and, of course, that success came not only in the realm of theory but also in the realm of

experiment, instrumentation, modeling, simulation, and many other activities. Each of these epistemic activities has a distinct kind of aim, and can be assessed in relation to how successful it is in meeting its own aim. So it becomes necessary to consider how various epistemic activities fit coherently together with each other, to form a scientific system of practice (or, a system of knowledge). This is similar to a Kuhnian paradigm (in the sense of “disciplinary matrix” rather than “exemplar”), but I refrain from using the term “paradigm”, both because Kuhn did not specify how different elements of it came together, and also because there are strong monist connotations to the Kuhnian term due to his assumption of the monopoly that a paradigm does and should enjoy in normal science.

To be more precise: an *epistemic activity* is a coherent set of mental or physical actions (or operations) that are intended to contribute to the production or improvement of knowledge in a particular way, in accordance with some discernible rules (though the rules may be unarticulated). It is important to keep in our view the aims that scientists are trying to achieve in each and every situation. The presence and operation of an identifiable aim (even if not articulated explicitly by the actors themselves) is what distinguishes actions and activities from mere physical happenings involving human bodies. Common types of epistemic activities include measurement, detection, prediction, hypothesis testing, etc. When we start thinking of scientific work as a collection of activities, an immediately obvious thing is the sheer variety in the types of epistemic activities that scientists engage in. Here is a partial list of types of epistemic activities: describing, predicting, explaining, hypothesizing, testing, observing, detecting, measuring, classifying, representing, modelling, simulating, synthesizing, analyzing, abstracting, idealizing.

Epistemic activities normally do not, and should not, occur in isolation. Rather, each one tends to be practiced in relation to others, constituting a whole system. A scientific *system of practice* is formed by a coherent and interacting set of epistemic activities performed with a view to achieve certain aims. It is the overall aims of a system of practice that define what it means for the system to be coherent. For instance, Lavoisier created a system of chemistry whose main activities included making various chemical reactions involving gases, tracking chemical substances through weight-measurement, classifying compounds according to their compositions, and analyzing organic substances by combustion. The overall aims of this system included determining the composition of various substances, and explaining chemical reactions in terms of the composition of the substances. The coherence of a system goes beyond mere consistency between the propositions involved in its activities; rather, coherence consists in various activities coming together in an effective way toward the achievement of the aims of the system. Coherence comes in degrees and different shapes, and it is necessarily a less precise concept than consistency, which comes well defined through logical axioms.

It may seem difficult to make a sharp distinction between epistemic activities and systems of practice, and this is intentional. When I distinguish higher and lower levels of description, that is only relative and context-dependent. Each epistemic activity is in itself a system of activities. For example, the combustion-analysis of a chemical substance consists of various simpler activities, such as burning, the absorption of combustion-products using other chemicals, weighing,

and percentage-calculations. And even those component activities in themselves consist of other activities (the act of weight consisting in the placing of samples and weights on balance-pans, reading the number off the scale, etc.). In each situation in which we study a body of scientific practice, I am proposing to call the overall object a *system*; when it is desired that we should study more closely different aspects of that system, we can analyze the system into different *subordinate activities*, without implying a reductionist metaphysics in which a system is made by a simple addition of various activities which do not really have any connection with each other.

At least in the Anglophone traditions, philosophical analyses of science have been unduly limited by the common habit of viewing science as a body of propositions, focusing on the truth-value of those propositions and the logical relationships between them. This has led to the neglect of experimentation and other non-verbal and non-propositional dimensions of science in philosophical analyses. Many historians, sociologists and philosophers have pointed out this problem, but so far no clear alternative philosophical framework has been agreed upon to provide a language for fuller analyses of scientific practice. In an attempt to make improvements on this situation, the first step to take is to go beyond talking about theories and theory-choice, while not neglecting the theoretical dimension of science.

I have only just begun to articulate this way of analyzing science (see Chang, forthcoming, [chapter 1](#), section B1.1). There is still much philosophical work to do in examining the cogency of the concepts of epistemic activity and system of practice. I expect to be able to draw much useful inspiration from pragmatist philosophy, among other sources. However, I have already begun to apply these abstract ideas to frame further historical episodes, starting with [chapters 2 and 3](#) of the forthcoming book. There seems much promise in this direction.

8.4 Concluding Remarks

I started by problematizing the common use of historical case-studies in relation to philosophy of science. And then I tried to illustrate one particular mode of history–philosophy interaction, which can be schematized as follows:

Existing philosophical framework

- Historiographical puzzle: an episode that is difficult to understand
- Search for a new philosophical framework
- Better understanding of the episode, in the new philosophical framework
- Further development of the new philosophical framework
- Application of the new framework to other episodes

We start with an existing philosophical framework, and find historiographical puzzles, namely episodes that are difficult to describe and understand. In attempts to find an apposite description of these episodes, historians can *generate* new concepts and ways of thinking that philosophers may not come up with from their entirely

abstract work. And then philosophers can take up the new abstract ideas generated through the historical work, and develop and apply them further. In this business it is difficult to say whether a given investigator is working as a historian or a philosopher, but that is just the point. Contrary to Kuhn, I think this process works out best if the historian and the philosopher *is* the same person doing both at the same time. Now, it may be objected that *good* philosophers should be able to come up with good philosophical ideas without the help of historiographical stimulus, but in reality I think abstract philosophical thinking has not been very effective in generating ideas that are useful for the analysis of scientific practice. And at any rate, I do not see anything wrong with getting help where we can, rather than insisting on restricting the source of our ideas.

There is also a mirror-image of this process, in which philosophy helps history:
Existing historiography

- Philosophical puzzle: a set of putative actions/decisions by past scientists that does not make sense
- Search for better historiography
- Removal of philosophical puzzle
- Empirical work to complete new historical account
- Reflections on other related history

This is a reminder that when there is a tension between history and philosophy, it is not always philosophy that is to blame, or at least not entirely. (This is similar to how a conflict between theory and observation does not necessarily mean that the theory is incorrect.) In fact, I believe that such correction of history through philosophical puzzlement is also necessary in the case of the Chemical Revolution (see Chang 2010).

I believe that both of these processes have already been at work in much of the best research in integrated HPS, and I hope that my articulation will help them continue in more conscious, thorough and methodologically sound ways. Finally, I would like to close by placing these processes into a broader framework of history–philosophy interaction. Table 8.2 summarizes what I have worked out so far.

Table 8.2 Modes of history–philosophy engagement

Mode of engagement	History gives philosophy	Philosophy gives history
Necessary	Subject matter (“philosophy of science without history of science is empty”)	Conceptual framework (“history of science without philosophy of science is blind”)
Critical	Counter-examples	Detection of implausibility
Heuristic	New concepts for better understanding of puzzling episodes	Discovery of new historical facts to remove philosophical puzzles
??? (“other things”)		

There are at least three modes of history–philosophy engagement. First, they are indeed necessary for each other, history giving philosophy its very subject matter, and philosophy providing the necessary conceptual framework. Then there is the critical function that history and philosophy can serve for each other. And what I have addressed in this paper is mainly the heuristic function. My table, just like Francis Bacon’s list of “instances meeting in the nature of heat”, ends with a bunch of question-marks, or, “other things” (Bacon 2000, 111). That is my way of indicating that we are at the start, not at the end, of our business.⁵

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