Context-Dependent Anomalies and Strategies for Resolving Disagreement

A Case in Empirical Philosophy of Science

Douglas Allchin

Abstract The interpretation and analysis of anomalies is itself theory-dependent, as illustrated in the case of the ox phos debate in biochemistry in the 1960s. Here, the perceived threat of six anomalies to an existing research lineage depended on perspective, or Kuhnian paradigm. The ambiguous status of anomalies sharpens the problem of Kuhnian incommensurability. But analysis of the details of the historical case—one way to pursue an empirical philosophy of science—also indicate a possible solution. The asymmetric organization of multiple anomalies strongly indicated that disagreement had shifted from an intraparadigm to an interparadigm level, where modes of effective argument and use of evidence differ. This diagnostic awareness of the type of disagreement can orient discourse and allow investigators to develop and present evidence appropriately. I briefly extend the results of this historical case analysis to Darwin's synthesis and to gendered bias in craniology, to indicate the prospective generality of the analysis of anomaly asymmetry.

Keywords Empirical philosophy of science · Anomalies · Kuhn · Incommensurability · Error types · Strategies

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D. Allchin (✉)
Minnesota Center for the Philosophy of Science, University of Minnesota, Minneapolis, MN 55455, USA
e-mail: allch001@umn.edu

D. Allchin
Science Studies Program, Århus University, Aarhus, Denmark

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1 Introduction

How can history contribute to an empirical philosophy of science? In particular, how can one bridge the gap between abstractly normative and concretely descriptive accounts? Here, I offer a case with one prospective solution.

At one level, any methodological question about science is necessarily empirical: does the idealized method proposed by philosophers actually work in practice? In what contexts, or under what circumstances? The relation between history of science and philosophy of science has always been viewed as somewhat problematic, even if also fruitful (Brush 2007; Losee 1987; Nickles 1995). Nonetheless, several major efforts have effectively demonstrated the value of "testing" philosophical propositions through analysis of historical cases (Brush 2015; Donovan et al. 1988; Hull 1993 ; Losee 1972, 2005). Similarly, one might ask whether, based on history, the epistemological dimension of social norms envisioned by Merton (1973), Hull (1988), or Longino (1990) are, or can be, realized in practice (Jukola this volume). Indeed, good historical analysis may well shape an impression of what epistemological goals are achievable, or what one can realistically target. Empirical perspectives support a naturalized epistemology, sensitive to the abilities and limits of human cognition (Bechtel and Richardson 2010; Callebaut 1993; Wimsatt 2007). In these approaches, history provides the evidence for assessing the validity and scope of philosophical theories about how science should, can, or does function.

Another approach, which I explore here, is to adopt standard philosophical norms about scientific knowledge (consider such familiar benchmarks as reliability, simplicity, explanatory power, predictiveness, or novelty), while remaining uncommitted about the possible methods for achieving them in practice. Here, philosophy may offer epistemological, or normative, aims and justifications—the "whys". However, history answers the epistemic, or descriptive, questions—the "hows" of scientific practice (Losee 1972, 1987). That is, philosophy stipulates the ultimate values; nitty-gritty history, the proximal mechanisms. Product and process differ. For example, one may aim for consistency between theory and evidence. But in practice, experimental findings may not align with theoretical predictions. That is, anomalies may emerge. Such inconsistencies are ideally resolved. But philosophers generally do not prescribe how such anomalies are resolved. Through an analysis of history, however, and by documenting many examples of resolving anomalies in Mendelian genetics, Darden (1991) was able to generate a practical repertoire of potential strategies that might guide scientists on other occasions in the future. Similarly, Bechtel and Richardson (2010) acknowledged reductive explanation as a conceptual goal, but considered a large sample of historical cases in order to articulate just how scientists typically do this successfully in practice. The descriptive work of history makes the normative perspective of philosophy more complete and applicable.

To illustrate this approach further, I consider how the ox phos debate in cellular biochemistry in the 1960s might inform classic philosophical problems about Kuhnian paradigm shifts (for a fuller account, see Allchin 1991). This episode exemplifies well the type of dramatic theoretical and methodological gulfs or alternative gestalts described by Kuhn (1970; see also Hoyningen-Heune 1993; Allchin 1992, 1994; Weber 2002). In stormy rhetoric participants seemed (as informed by a retrospective view) to blindly talk past each other. Their discourse exhibited vividly the challenges of Kuhnian incommensurability where commitments to alternative problem fields differ and evidence could not be measured using comparable assumptions or benchmarks. Ultimately, the participants did resolve the disagreement after a decade and a half of debate, by redefining and differentiating the empirical domains, or scope, of the conflicting theories and their corresponding suites of experimental practices (Allchin 1994, 1996, 1997).

Kuhn maintained that interparadigm disagreement, aggravated by the challenge of incommensurability in discourse, is eventually resolved rationally, although he was not able to fully articulate just how, at least to the satisfaction of many skeptics and critics. How, indeed, does one interpret and resolve problematic interparadigm disagreements from the historically situated perspective of science-in-the-making? That is an empirical question, with important overtones at for general philosophical conceptions. The ultimate epistemic aim, here, may be achieved in part through concrete historical analysis. The proximal historical aim is to interpret how practitioners could transition from apparently irreconcilably conflicting views to acceptably complementary views. Namely, once the debate had begun, how could researchers interact productively to resolve it?

In the case of the ox phos controversy, viewed retrospectively, one particular problem was especially noteworthy. Earlier, I characterized how effective evidence-based argumentation differs for intraparadigm versus interparadigm disagreement (Allchin 1991, 1992, 1994). For example, crucial either-or tests may be possible within a paradigm, where assumptions and background knowledge are stable. But where problem fields and assumptions diverge, as in an interparadigm context, one must rely more on demonstrations, which merely display the explanatory power of a theory without decisively ruling our specific alternatives (Allchin 1994; Robinson 1984). Throughout much of the ox-phos debate, however, chemists engaged in intraparadigmatic arguments, trying (unsuccessfully) to resolve interparadigmamic discord. By misframing the discourse, and relying on implicit assumptions that were not shared, they tended to talk past each other. While one may easily see this in retrospect, it is less clear how participants in the midst of such historical developments may recognize the circumstances. This practical problem, while based on a philosophical understanding, calls for empirical analysis of history. How does someone know when disagreement has shifted from an intraparadigm to an interparadigm level, changing the terms of evidential argumentation? What diagnostic clues are available?

As one examines the case closely with these factors in mind, one finds that the dire sketch Kuhn provided of conceptual change was, ironically, somewhat optimistic. He regarded anomalies as well defined, able to leverage a "crisis." In the ox phos case, however, the interpretation or analysis of anomalies itself depended on theoretical context (Gilbert and Mulkay 1984). That is, an anomaly for one scientist was not necessarily the same anomaly for another—and may not have seemed anomalous at all. Philosophically, one might find here occasion to further characterize the difficulties of incommensurability, or to criticize and revise Kuhn's model. That would treat history as evidence for informing philosophical theories (first approach above). However, this is not my primary goal. Rather, history can also afford a more active role in informing and enriching philosophical perspectives. One can analyze the history and discover—not test—how the disagreement was, ultimately, resolved. In addition, this understanding could help inform science in practice. One can solve the Kuhnian problem of incommensurability and inter-paradigm disagreement empirically, not conceptually. Still, an answer, once discerned, can certainly be framed (retrospectively) with a philosophical flourish, deepening our abstract conceptual understanding of Kuhnian-type episodes in science.

2 Interpreting the Anomalies of Ox Phos

Let us enter the case in 1961.[1] Hans Krebs has elucidated the reactions of the citric acid cycle. Fritz Lipmann has described the central role of phosphate bonds, notably in ATP, as an energy carrier in the cell. David Keilin has helped identify the cytochrome chain that transforms energy from the Krebs cycle to ATP. For the last decade, research has focused on deciphering these final energy reactions that use oxygen and produce ATP: oxidative phosphorylation, or ox phos. The general consensus is that there are more, yet unknown, chemical reactions with many high-energy intermediate compounds along the reaction pathway. Yet in the eight years since they were formally proposed in 1953, no one has found them.

At this time, Peter Mitchell introduced a remarkably different theory, which would ultimately earn him a Nobel Prize in 1978: what he called the chemiosmotic hypothesis. In his original 1961 paper, in a deceptively modest four column-inches of text citing twenty articles, Mitchell presented six anomalies: "facts," he said, "… that are generally acknowledged to be difficult to reconcile with this orthodox (chemical) view" (1961, 144). It was almost a textbook definition of anomalies. These six anomalies, Mitchell suggested, collectively prompted doubt in the reigning concepts about the high-energy intermediates, and instead supported his alternative interpretation, based on electrochemical membrane gradients. What interests us most, however, is not how other chemists weighed the evidence Mitchell presented or considered the relative merit of alternative theories. Rather, of interest is how they first interpreted, or gave meaning to, these experimental "facts"—and how this makes philosophical thinking about anomalies more complex.

[1]For a more complete account of the entire ox phos episode, see Allchin (1991, 1997), and Weber (1991).

First, Mitchell noted that loss of the ATP product on one side of the mitochondrial membrane led to changes in the equilibrium of the reactions on the other side of the membrane. Mitchell contended that moving hydrogen ions across the membrane was central to the energy reactions—and here he emphasized how his conception could explain this particular effect, bridging the two sides of the membrane. But while chemists acknowledged this fact, they did not see it as threatening their view. They saw ox-phos, like all chemical reactions, as reversible. When one uses the product, equilibrium shifts. There was no broken expectation, no inadequate explanation. No anomaly, here, at least.

Second, Mitchell noted, the proposed high-energy intermediates of the reaction series were "elusive to identification". In classic scientific understatement, he had implied, of course, that there were no intermediates at all. Rather, the intermediate energy stage was a build-up of protons outside the membrane: an electrochemical pH gradient. Those studying ox phos were arguing about whether such intermediates were phosphorylated, or whether there was a second non-phosphorylated intermediate, so Mitchell's claim seemed to betray a fundamental confusion. Moreover, from recent reports, biochemists seemed on the verge of isolating the intermediates. They were likely short-lived and thus hard to isolate experimentally, especially if embedded in the membrane. This was a technical puzzle so typical of Kuhnian normal science, not a theoretical failure—and certainly not epistemically threatening (Allchin 1997).

Third, Mitchell noted, structurally intact membranes seemed essential. For Mitchell, the membrane preserved the pH energy gradient. Here, chemists did consider this problematic—but only experimentally. The conventional research, epitomized in the work of Krebs, Lipmann and others, targeted enzymes in aqueous solutions. The ox-phos components, however, were located in the mitochondrial membrane, a hydrophobic (or oil-like) environment. Researchers could not isolate the components while still functional. For biochemists, the challenge was largely another technical puzzle of normal science: to discover how to isolate enzymes intact from membrane-like structures. Later, Lehninger (1960) viewed the membrane more positively: "There may be a biological necessity for structural organization of these catalysts in a moderately rigid, geometrically organized constellation in the membrane." The membrane might hold enzymes in close proximity and proper orientation. The implied remedy, as before, was to search experimentally for ways to prepare such complex membrane-bound structures. The same acknowledged "fact"—the structural integrity of membranes—had two quite different meanings: one as a technical puzzle, the other as threatening theory and the way of doing ox phos science.

Fourth, Mitchell noted that many compounds interfered with ox phos, but they seemed to share no specific chemical characteristic. Mitchell noted, however, that these compounds were all soluble in the membrane's oil-like environment. They could thus enter the membrane and transport protons (or other charged particles), dissipating the pH gradient. For chemists, the solubility could certainly explain how the compounds entered the membrane. But understanding how they worked required more specific elucidation of their structure. Mitchell seemed to miss the critical features, which might not be known until all the reactions and their enzymes had been studied. Nor did anything dictate one common mechanism for all the chemicals.

Fifth on Mitchell's list: mitochondria would swell and shrink during ox-phos. According to the chemiosmotic view, the movement of various ions caused the corresponding osmotic movement of water. While such osmotic effects were not uncommon, they were more familiar to the lipid biochemists who studied membranes. Biochemists studying energy-related reactions focused primarily on enzymes and protein chemistry. Osmotic phenomena fell outside their concerns. Swelling might occur incidentally, as a by-product, but hardly seemed relevant to how the enzymes functioned. Here, Mitchell and the other chemists addressed different potentially relevant variables.

Last among Mitchell's list of anomalies: reactants and products did not always exhibit integer ratios. When studying chemistry, we all learned to balance chemical equations. Reactants relate to products in whole number ratios. Chemists observed that this "rule" was occasionally broken for mitochondria. For Mitchell, even if the reactions creating the gradient followed exact ratios, the pH gradient of the membrane could "leak" any amount. Other chemists acknowledged, for their part, that intermediate products might be used in other reactions, altering observed ratios. The uneven ratios, so commonly observed, reflected experimental static, or noise, not meaningful signal. Technical mastery would eventually dissolve this artifact—another puzzle for normal science. Once again, the chemists isolated Mitchell's "anomaly" to experimental methods, not theoretical concepts (Allchin 1997).

So, there were six anomalies. All could agree in 1961 about the basic "facts" or experimental observations. Yet where Mitchell saw many fundamental counter instances and explanatory flaws, chemists perceived only a handful of familiar technical puzzles and sometimes no problem at all. Mitchell saw the anomalies as evidence for a revolutionary new theory. Other chemists saw only Kuhnian normal science. Mitchell's anomalies were only anomalous using the chemiosmotic perspective as an interpretive guide. The meaning of the six anomalies was context-dependent. That is, while all agreed there was a latent error inherent in the accepted experimental results, they disagreed about how to localize, and thus clearly identify, that error. Of course, this should surprise no one. Anomalies, like any observation, may be theory-laden, or interpreted contextually. Accordingly, Lightman and Gingerich (1992), observed that anomalies do not begin with internal contradictions, but rather when a new paradigm introduces an alternative perspective that exposes them. The meaning, not merely the acknowledgement, of anomalies seems theory-dependent.

The history thus indicates how Kuhn's initial philosophical conception (although itself based on historical study) was rough or incomplete. Empirical historical analysis refines the philosophical concept. Here, the problem of incommensurability becomes even worse. According to Kuhn, an accumulation of anomalies leads to crisis. They reveal weaknesses in the paradigm that eventually lead to questioning it and developing a successor. In the ox-phos case, however, the view from within the established paradigm seemed to eclipse the type of awareness that Kuhn suggested becomes inevitable. Worse, perhaps, where Mitchell saw the opening of a new paradigm, other chemists saw only the continuity of normal science and puzzle-solving. Indeed, the divergent views seem to epitomize Kuhnian incommen-surability, but an incommensurability based on problem fields and views of relevance more than on linguistic references or communication woes (Allchin 1990). How, then, can anomalies lead to scientific change? How can one correct an error if researchers are blinded to its "meaning"—that is, if the interpretation of anomalies is itself theory-laden? Mere philosophical reflection does not necessarily solve the problem. History—the empirical dimension—has an additional role in profiling the solution, to which my discussion now turns.

3 Resolving Disagreement About Anomalies

Darden (1991) has suggested a set of strategies for resolving anomalies. They are not normative "methods," or algorithmic rules, in the conventional sense. They are possible solutions to explore. They are "strategies" derived from a more or less descriptive historical analysis, then formalized in a philosophical perspective. They do not guarantee results, but provide guidance whose potential value is warranted by historical experience. Darden's strategies on anomalies, however, were oriented exclusively to theory change, for cases where the "problem" is identifiably theoretical. In the ox phos case, as just noted, some chemists saw the problem as conceptual, others as experimental. One needs a broader perspective here.

As exemplified in the ox-phos case, one cannot always immediately isolate an individual anomaly unambiguously. Yet if one assumes that every anomaly exposes a latent "error" to be remedied, then to isolate anomalies or resolve disagreement, one may profit from a complete inventory of generalized error types. In contrast to Darden's focus on revising theories only, one may find that error types range from the material or experimental to the conceptual or discursive-social (Allchin 2001). The appearance of an anomaly does not itself indicate whether to localize the problem in the lab, in the theory, in cognitive or cultural biases, or in some other element of scientific practice. The problem in the ox phos case was how researchers, despite their divergent interpretations of the relevant error type(s), could communicate and argue effectively about them. How could Mitchell (or others) frame their evidence to be persuasive?

Here, the detailed historical perspective highlights an important clue in the pattern of the anomalies themselves. This was distinct from how each was interpreted. That is, the anomalies have a character as an ensemble, rather than individually. From Mitchell's chemiosmotic perspective, they formed a unified syndrome. They all implicated the relevance of the membrane. The flaws, as Mitchell framed them, were systematic. They functioned together as a half-dozen anomalies. From the extant perspective in ox-phos, on the other hand, these were six separate anomalies. In this case, six of one was not the same as a half-dozen of the other. This distinctive asymmetry was critical. While it did not provide a definitive solution, it showed the path to a solution. It indicated how to address the underlying disagreement.

What did the asymmetry mean? Today, in retrospect, we might say one could weigh the two theories by applying a standard philosophical norm of simplicity, consilience, coherence, or conceptual economy, and decide that adopting the new theory solved everything all at once (Janssen 2001). Namely, using the age-old Occam's razor, the chemiosmotic perspective was the clear "winner". Indeed, many researchers were impressed by the coherence of the chemiosmotic gestalt and began to entertain it seriously or reorient their research trajectories (Robinson 1984). Here, then, using the historical analysis, one could supplement Darden's catalog: namely, use a meta-analysis of multiple anomalies or error-types to identify a common error. This strategy echoes one sketched by Glymour (1980) for a more conventional hypothetico-deductive (logical) framework. Namely, when multiple observations or results do not match separate theoretical predictions, check shared boundary conditions or auxiliary hypotheses supporting those predictions as probably incorrect. Just as independent observations or lines of reasoning from multiple sources may provide robust support for a particular conclusion, so too they may indicate a robust weakness, vulnerability, or error (Wimsatt 2007, pp. 43-74). Thus, a potential strategy, exhibited through an empirical analysis of this case, might be: "Search for an intersection of prospective error types among many anomalies." In this view, a half-dozen anomalies would be inherently more informative than six.—And perhaps decisive.

However, the fully empirical approach I am profiling proceeds differently. One must work philosophically from within the historical perspective, or science-in-the-making (Latour 1987). Namely, philosophical analysis can be biased by retrospect. One cannot fruitfully trump the situated perspectives of the researchers. In 1961, the evidence is not yet fully in. Mitchell could be wrong. Searching for a common root error is merely a strategy, not a final evaluative judgment, or normative rule. Our analysis must thus focus instead on the discursive dimension. How were the different perspectives reconciled through further evidence? In this case, researchers needed to know how to present their findings effectively for others to understand, and for them to have persuasive merit.

While Mitchell did not necessarily resolve all the anomalies at the outset, he did, nonetheless, dramatically change the discursive landscape. He had shown how the anomalies could be related. The chemiosmotic view resolved all the anomalies at once, by adopting a new theory, or conceptual gestalt (as described above). The conventional chemist who resolved one anomaly, still had five others to resolve. For example, showing that the membrane functioned as scaffolding for protein interaction would not thereby solve the anomaly of the missing intermediates, and vice versa. Piecemeal solutions for each anomaly no longer sufficed.

The six/half-dozen asymmetry was the critical contextual signal. Its significance was in indicating that discourse had shifted from intraparadigm to interparadigm comparisons. It did not yet resolve the disagreement. When Mitchell showed a plausible role for the membrane in all cases, he essentially destabilized the background assumptions that had guided earlier experimental reasoning and interpretation. Those assumptions could no longer be regarded as unproblematically justified. The asymmetric stacking of anomalies reflected this altered epistemic environment.

Returning to the historical perspective, how did this shift affect the researcher? Generally, when two theoretical alternatives present themselves, an investigator hopes to test them against each other under controlled conditions, isolating the variable in question against a stable background. However, in the context of contrasting paradigms, one can no longer make such narrow parallel comparisons. Mitchell could present experimental evidence that supported his view, but not that simultaneously excluded the chemists' interpretations as "wrong". Indeed, his criticisms fell relatively flat because chemists felt no need in 1961 to abandon their own interpretations. How could he present evidence, then, for the integrated nature of the six anomalies? Mitchell and others had to show, or demonstrate, that the chemiosmotic perspective was cogent and fruitful, and solved relevant problems (for more details, see Allchin 1992). The focus becomes experimental demonstrations, without undo concern for explicit comparisons or discounting of alternatives. In an interparadigm context, the appropriate strategy is demonstration. The asymmetric stacking of anomalies—six of one, a half-dozen of another—was essentially a diagnostic signal of, rather than a particular solution to, the shift in argumentation and experimental strategy to the interparadigm level. The framing of this significant diagnostic signal is the concrete outcome of an empirical historical approach in this case. This strategy was not obvious from a purely abstract perspective, I contend. Nonetheless, it emerges from a detailed analysis sensitive to historical perspective.

4 Conclusion

The case of asymmetry in anomalies in ox-phos, between six-of-one and a-half-dozen-of-the-other, then, may serve to illustrate a particular fruitful use of history in an empirical philosophy of science. For example, historical analysis may provide important specific "hows" where the philosophical "whys" are already established. The analysis may yield scientifically fruitful strategies, sensitive to context, such as, "When the interpretation of multiple anomalies differ (some viewing them as independent and others as conceptually unified), assume interparadigm discourse and adopt a strategy of experimental demonstration." Here, history has a creative role in developing—not merely assessing or contextualizing—philosophical principles. That is, the ox phos case helps illustrate how historical analysis can refine, and possibly revise, philosophical concepts; how history can go beyond conventional philosophical norms by articulating them in authentic scientific practice; and, most importantly, how history can help profile research strategies. As demonstrated in this case, history can contribute to the middle zone between abstract philosophical norms and concrete historical descriptions, where the "hows" are as important to scientists as the "whys."

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