How Do You Falsify a Question?: Crucial Tests versus Crucial Demonstrations

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1. Introduction

How is the deep disagreement between what Kuhn characterized as paradigms ultimately resolved and how do we interpret such debates epistemically? A close analysis of the Ox-Phos Controversy in bioenergetics from the 1960s and 70s (§§2-3 below) suggests that one justifies a set of questions through an ensemble of empirical demonstrations. This contrasts to decisions between theoretical alternatives through 'crucial experiments'. When viewed along with other historical episodes, this case suggests a philosophical category of 'demonstrations', distinguished from crucial tests and complementary in justificatory status to falsifying instances. The distinction also suggests specific strategies for scientists (§4).

The Ox-Phos Controversy is an especially valuable case for studying theory development and scientific change, and for investigating the problems of disagreement, originally highlighted by Kuhn, where two incompatible conceptual or experimental gestalts converge on the same empirical domain (see also Gilbert and Mulkay 1984a; 1984b; Robinson 1984; 1986; Rowen 1986; Weber 1986; 1991; Allchin 1990; 1991). The debate in this case centered on perhaps the most significant stage of energy-processing in the cell, oxidative phosphorylation, or ox-phos (pronounced as an assonant, nearly rhyming 'OX-FOSS'). The basic problem was how energy was transferred from the stage where we ultimately use the oxygen we breathe to the stage where we produce ATP, the molecule that provides energy for virtually all our cellular functions. Though originating in a relatively specialized area of biochemistry, the controversy soon surfaced in introductory biology texts (e.g., Keeton 1972; Dyson 1975; Becker 1977; Curtis 1979) where normally only consensual knowledge is presented. The reconceptualization that emerged was the occasion for the 1978 Nobel Prize in Chemistry, awarded to Peter Mitchell for his 'chemiosmotic theory' (Ernster 1979).

Originally, in the 1950s, energy was regarded as passing from chemical bond to chemical bond through a series of enzymatic reactions—much like buckets of water along a fire brigade. For chemists, the experimental challenge was crudely to tear apart the cell, isolate its essential components—especially a set of high-energy intermediate compounds—combine them all together again in a test tube and thus reconstitute the re-

PSA 1992, Volume 1, pp. 74-88

action system *in vitro*. Peter Mitchell, on the other hand, conceived the intermediate energy state as an electrochemical gradient of ions across the two sides of the membrane in which the system was embedded. Under this 'chemiosmotic' scheme, the directional orientation of components in the membrane and the structural integrity of the membrane were critical. They set the tasks of mapping the vectorial structure, and measuring the gradients and membrane permeability. Two overlapping, but incompatible causal networks were thus proposed. And they emerged from two divergent sets of questions, research goals and even domains of supposedly relevant phenomena: no single discriminating or "crucial" experiment was possible (see also reviews by Greville 1969; Racker 1970). Experimental findings were, nonetheless, collectively effective in resolving the dispute—and it is critical epistemically to understand how.

2. Falsification and Anomaly-Localization Revisited

First, however, one needs to understand how empirical results failed to be effective. In particular, one needs to appreciate how anomalous results, or potentially 'falsifying' instances, were variously interpreted. Researchers differed in their theoretical backgrounds or cognitive resources and, therefore, in the questions they asked. The variable response suggests that the role of experiment merely in reducing theoretical alternatives depends on those questions, and that we may look for other ways that they shape the development of theory or research programs or help in arbitrating disputes.

Among philosophers, the notion of simple or 'naive' falsification has been heavily criticized and largely abandoned (e.g., Lakatos 1970). Among scientists, however, the concept may function as a heuristic and rhetorical device. At least this was the case in the ox-phos episode. Peter Mitchell, for example, self-consciously framed his novel chemiosmotic hypothesis according to Popperian principles (Boyer et al. 1977, p.996; Mitchell 1980, pp.184-190; 1981a, p.17; 1981b, p.611); others, as well, appealed to the scientific authority of falsification (Azzone 1972; Huszagh and Infante 1989). Still others argued, without explicit philosophical reference, that single experiments were decisive against the opposing theory (e.g., Chance and Mela 1966; Chance, Lee and Mela 1967; Slater 1967; Tupper and Tedeschi 1969).

But it is also clear that the researchers often did not follow their own advice or adhere rigidly to their own rhetorical claims. Elements of Mitchell's original hypothesis, for example, failed repeatedly in the early development of the theory to match actual observations. Data about the direction of the energy gradient (Mitchell 1961b), the magnitude of the gradient (Mitchell 1966a) and an important intermediate ion ratio (the H^+/O ratio; Mitchell 1966a) all challenged Mitchell's initial proposals—and the discrepancies certainly did not escape the notice of critics (Chance, Lee and Schoener 1966; Slater 1966; 1967; 1971). Yet Mitchell persisted in his broader, more general program. One particularly recalcitrant problem was the arrangement of molecular components (including the cytochrome b pair) that would provide the necessary directional orientation (redox loop) that Mitchell postulated. Mitchell admitted later that the data "had always been regarded by [Britton] Chance and other people as anomalous." At one point, in fact, he was "feeling more and more that this might be a point where we could succeed in falsifying the chemiosmotic hypothesis" (1980 interview, quoted in Weber 1991). Yet Mitchell did not capitulate. On the verge of crisis (perhaps), Mitchell dramatically revised the theory and introduced an arguably *ad hoc* concept (the "Q cycle"), deemed later by some as the most elegant achievement of Mitchell's theorizing (Weber 1991; Slater 1981). In brief—and perhaps to no philosopher's surprise—this episode exhibited no falsifications in large-scale intertheoretic debate.

The most informative lessons one can draw here, however, are not about naive falsification, but about how the anomalous results were interpreted. Indeed, many results did not match theoretical expectations. In Lakatos' more 'sophisticated' version of falsification (1970), anomalous results should lead to progressive theory development. But Lakatos did not consider the dynamics of such a process, and he notably failed to address the common criticism that mere falsification does not identify where in a network of concepts and methods, items need to be amended. By contrast, Glymour (1975), Wimsatt (1980; 1981) and Darden (1991), for example, have each offered a rich repertoire of methods by which we might indeed isolate or localize the weaknesses in a theory. The special virtue of such analyses is that they avoid abstract and often vague scales of 'progressiveness' or 'problem-solving power' and aim to identify specifically where justification is or is not warranted. These procedures thus carry the bulk of the epistemic work, at least within a research programme or tradition. The ox-phos case suggests, however, that even the application of such methods is heavily context-dependent, or based on a researcher's orientation, and thus may be ineffective in resolving the deepest forms of intertheoretic disagreement. That is, the interpretation of anomalies and the localization of problems may themselves depend on the questions one asks.

When Mitchell originally introduced his chemiosmotic alternative, for example, he noted six "facts" that he claimed were anomalous or epistemically threatening to the conventional chemical hypothesis. Chemists acknowledged the same "facts," but interpreted them quite differently. Mitchell had observed that "it is not clear why phosphorylation [the last step in the energy-transfer process] should be so closely associated with membranous structures" (1961b, p.145). For Mitchell, the membrane separated inside from outside and was essential in preserving an energized gradient. The chemists certainly recognized the close association of the process with membranes and at first they considered it a "nuisance" in their efforts to reconstruct the enzyme system *in vitro* (Cooper and Lehninger 1957). Albert Lehninger later suggested, however, that the severe technical problems might reveal "a biological necessity for structural organization of these catalysts in a moderately rigid, geometrically organized constellation" (1960, p.952): the "problem" thus dissolved into a promising research enterprise. Where Mitchell saw a defeating anomaly or falsifying instance, chemists saw instead the exciting potential for exploring a whole new dimension of biological organization.

Most dramatically, the chemical hypothesis postulated a series of high-energy intermediate compounds, none of which had been isolated or identified. For Mitchell they were not found—nor would they ever be found—because, simply, they did not exist. He claimed one had to stop asking how energy was passed only from chemical bond to chemical bond, and ask instead how it could be channeled through the movement of protons or ions and, say, create a membrane gradient. Chemists, however, advanced numerous reasons—mostly technical—why the energy-rich intermediates were, as Mitchell had phrased it, "elusive to identification" (1961b, p.144): they would only need to exist in small concentrations; they were unstable and short-lived; and/or they were tightly bound to other molecules in the membrane (Griffiths 1963; 1965; Chance, Lee and Mela 1967; Greville 1969). David Griffiths characterized the experimental task of isolation as "formidable" (1963, p.1064)—though Efraim Racker only went so far as to call it "rather formidable" (1970, p.137). Chemists had localized the "anomaly" of the high-energy intermediates in yet unsolved technical puzzles. Mitchell, on the other hand, had "localized" it globally, in the whole way investigations were conceived. There was accord on the experimental record, but not on how to interpret the problems it generated.

Finally, from the reciprocal view, one may note that anomalies for the chemiosmotic approach were also not interpreted uniformly. E.C. Slater, one of the most prominent critics of Mitchell's framework, described how the sequence of components that he studied and knew best (the cytochrome b complex) simply could not be made to fit any version of the chemiosmotic hypothesis (1971, pp.44-45). For him, there was no reason to consider the alternative any further: this single mismatch was enough to reject even the plausibility of the chemiosmotic enterprise. As noted above, Mitchell took the conceptual deficit quite seriously—but decidedly not as a reason for abandoning his problem frame. Instead, he used it as an occasion to focus on several anomalous results and to reconstruct a more acceptable answer to the same set of questions about vectorial chemistry. In this and the other cases cited above, Mitchell preserved his orientation to the problem in the face of anomalous findings and worked on details, while others saw the failures as justification to disregard it entirely.

Close analysis of the ox-phos case suggests that observations were not so "theory-laden" that anomalous data could not be recognized. Researchers did repeatedly reassess (and even revise) their beliefs based on the evidence. Rarely, however, did researchers abandon the questions that motivated their research. One finds, in fact, that these central questions, more than mere theoretical commitment, guided their varying responses to the same data. This suggests that we might best characterize each research enterprise *interrogatively*, by its problem-field, problem-frame or set of *questions* (see also Laudan 1977; Nickles 1980, pp.33-38; Allchin 1990).[1] By regarding the questions as primary, one can understand exactly how the response to anomalous results was shaped in each case. The philosophical lesson is perhaps best expressed in my title, through a query at once rhetorical and self-referential: namely, "how do you falsify a question?"

3. The 'Crucial' Role of Demonstrations

The epistemic challenge in profound scientific debate, then, may sometimes become articulating how one unasks a question—or comes to ask a different one (the "replacement problem" of Nickles 1981, pp.95-96). Philosophically, one wants to understand how one justifies a question or set of related questions, particularly in relation to others that may be similar.

One may examine, for instance, how scientists legitimate individual research projects in the opening section of their papers. Research studies are generally formally embedded in a context of experimental practice and extant theory (Griesemer and Wimsatt 1989), so that their results are positioned at important junctures for channeling reasoning or resources (Knorr-Cetina 1981, Chap. 6; Latour 1987, pp.108-121). An analysis of the ox-phos case (below) allows one to see further how such arguments are received, not merely presented—that is, how researchers can construe experiments as warranting certain questions and, indirectly, their pursuit.

The most dramatic feature of even sophisticated versions of falsification is their use of editing, selection, or other eliminative procedures (Hacking 1983, pp.3-5). Justification is always unfinished, and a burden of further proof thus perpetually remains. While effective within a research enterprise, such methods (as shown above) may be ineffective where questions themselves are "in question". One may also see experiments, however, in their more positive or productive role (e.g., Franklin 1986). How do empirical studies generate or create justification, rather than merely limit or qualify it?

In the ox-phos episode, controversy was resolved through numerous studies, which are now entering a canon of "classic experiments" in the field of bioenergetics. One of the earliest and now most renowned set of experiments was originally done as part of another, intersecting research enterprise. André Jagendorf and his coworkers at Cornell merely measured (or "observed") the energized gradient earlier only hypothesized by Mitchell. Soon thereafter, however, they induced an artificial gradient by plunging the membrane-bound vesicles into an "acid-bath" (Jagendorf and Uribe 1966). The sudden pH differential across the membrane generated the final energy product, ATP, where there was no natural source of energy—and under conditions that were not at the time considered to exist in nature. As a result of these acid-bath experiments, researchers—in this case, even Mitchell himself—began to address the chemiosmotic framework in ox-phos more seriously: why?

The acid-bath experiments were dramatic, in part, because they followed an interventive strategy (Hacking 1983). More importantly, though, they revealed a hitherto unknown phenomenon that could well be related to the central question of energy transfer in the cell. By showing how the membrane gradient was causally connected to ox-phos, the novel results potentially altered the range of relevant phenomena or 'domain' *(sensu* Shapere 1974) included in any complete theory or explanation of ox-phos. In so doing, they also thrust chemiosmotic questions, which addressed precisely this reconfigured domain, into explicit consideration. The acid-bath experiments did not limit or qualify the chemiosmotic theory or problem-field; rather, they legitimated them. They played an epistemic role complementary to that of falsification: namely, ***demonstration.***

In many conceptions of scientific explanation or justification, confirmation of predictions (or specifically novel or "risky" predictions) plays a central role (e.g., Hempel 1966). In this capacity, a 'demonstration' may likewise function as an empirical "benchmark" for a conceptual or theoretical "map." But the role of the acid-bath studies went much deeper: they began to redraw the very boundary of phenomena then considered relevant to ox-phos. They did so by showing empirically how a conceptually new class or type of phenomena was causally connected to those already known. Before the acid-bath experiments, the range of relevant phenomena according to the chemiosmotic approach was merely plausible theoretically; afterwards—through a largely ostensive exercise—it was also plausible experimentally. Jagendorf's results 'demonstrated', at least within a local domain, that the framework for posing chemiosmotic questions was empirically well framed.[2]

The major role of the acid-bath demonstrations, however, was not merely along a single theory-evidence axis. Jagendorf's results were also key in moving intertheoretic debate "downstream" and, ultimately, in resolving the controversy. Still, the acid-bath studies were not structured as a crucial test between the two available alternatives. The conditions of the experiments did not even strictly address the standard chemical interpretation, and certainly did not directly falsify or challenge any element of its approach. That is, they were not designed to evaluate the two problem-fields symmetrically, or in parallel. But the demonstrations did challenge the chemical approach indirectly. As Robinson (1984) has noted, the acid-bath experiments forced chemists to retreat from their assumptions or claims about the irrelevance of chemiosmotic concerns. Originally, they conceived energy transfer in terms of a chemical intermediate with a high-energy bond (scheme A, following page), while the chemiosmotic framework used a membrane gradient (scheme B):

(A) oxidation ⟷ high-energy intermediate ⟷ phosphorylation (ATP)

(B) oxidation ⟷ membrane gradient ⟷ phosphorylation (ATP)

(C) oxidation ⟷ high-energy intermediate ⟷ phosphorylation (ATP)
↕
membrane gradient

Jagendorf's findings obliged proponents of the chemical hypothesis to acknowledge the phenomenon as causally relevant, though initially they regarded it as only a peripheral or side reaction (scheme C; see Chance, Lee and Mela 1967, pp. 1341-42; Slater 1967, pp. 321-22). Disagreement persisted, but on a substantially different issue. The former issue was whether gradients were part of the domain of ox-phos; the new issue was whether the central mechanism of ox-phos could occur without gradients. That is, the epistemic task of debate had shifted— "crucially" so—from establishing warrant for the chemiosmotic approach, to finding ways to dismiss it (if possible). The demonstrations, though not designed as discriminating two-way tests, nevertheless carried the horizon of debate forward and, notably, also shifted the burden of proof.

While Jagendorf's demonstrations were crucial within a local domain, they did not justify the entire chemiosmotic enterprise across its whole domain or in all areas of application. Data on artificial gradients could not shift empirical contexts and be applied to claims about, say, the specific directional arrangement of components in the membrane—despite the prior construction of a hypothesis which linked them. Further evidence was necessary to legitimate Mitchell's claims that other local domains were also simultaneously relevant to the domains already well understood. There were thus many demonstrations, each crucial in establishing an empirical benchmark in a different local domain. These included: showing that the membrane was relatively impermeable and could thus preserve a gradient, once formed (Mitchell 1961a; Mitchell and Moyle 1965a; 1967); finding that the intermediate gradient was crudely quantitatively positioned (H^+/O and H^+/ATP ratios; Mitchell and Moyle 1965a); measuring the membrane gradient more definitively using the movement of synthetic ions (Skulachev 1970); and demonstrating that elements from systems that had evolved in divergent organisms, when recombined in a chimeric vesicle, could function as an ensemble (Oesterfelt and Stoeckenius 1973; Racker and Stoeckenius 1974). Mitchell (1966c) could also draw on earlier studies, not previously considered relevant: Lee and Ernster (1966) had in a different context noted that the membrane was "sided" or had different features on either side; and further results indicated that vesicles that were inside-out with respect to each other behaved differently (by generating reversed gradients). Each of these demonstrations served to anchor the chemiosmotic "map" to the empirical landscape or, perhaps more appropriately expressed, served as crucial knots in tying a new causal network together. One should note, additionally, that this process of shifting questions or problem-fields was not one of gradually increased support according to some single abstract scale of justification or a set of increasingly rigorous evaluative standards (Laudan and Laudan 1989). Rather, the task of justification was distributed across several local domains, with each initial demonstration playing a crucial role in warranting the questions for further investigation in the respective range of phenomena.[3]

The existence of 'crucial demonstrations' did not exclude, of course, the possibility for counter demonstrations. In fact, there were several experimental findings that had the potential, at least initially, to reclaim for the conventional chemists some of their threatened domain. Among these were several claims to have successfully isolated or identified the high-energy intermediate molecule which, according to the chemiosmotic formulation, did not exist. In each case, however, the results—though "repeatable" even now—did not fit into their proposed locus in the larger causal nexus. That is, the context of the claims could not be substantiated, and the findings were attributed to either different or highly circumscribed domain (Allchin 1991, pp.180-195). Even failing in their broader implications, though, the claims represented critical turns in the development of the field and provided momentary warrant or plausibility for further investigation.

Another domain of opportunity for defenders of the rear guard was in demonstrating ox-phos without closed membranes. Mitchell's questions implied that ox-phos could not (or would not?) take place in open, ruptured vesicles or with only fragments of membrane. Such a universal prohibitive claim was difficult to defend. One could only appeal, as Mitchell (1961b; 1966a) did, to the prolonged absence to the contrary. By contrast, the chemiosmotic hypothesis would be "immediately and irrevocably refuted," according to Greville, if oxidative phosphorylation could be demonstrated in a solution without closed membrane compartments (1969, p.71). Thus, one researcher was able to note the exceptional attention given to one reported finding:

> I remember in [a] meeting in 1972, somebody had written an abstract saying that they had demonstrated oxidative phosphorylation in a membrane-free system derived from a bacterium . . . . Normally these ten-minute papers, not many people attend. But I noticed that the room was filled, and the usual anti-chemiosmotic gang were all there like vultures. But the evidence that there were no membranes there wasn't very satisfactory. You could see them going away a little disappointed (Gilbert and Mulkay 1984b, p.29).

In fact, several claims to have demonstrated membrane-free ox-phos were published (e.g., Painter and Hunter 1970; Wilson et al. 1972; Komai et al. 1976; Tedeschi 1980) and each became a focal point of attention. Despite the obvious interest in such findings (due to their potential import epistemically), the results could not be repeated, in some cases "in several laboratories" (Racker and Horstman 1972). A demonstration, even if "crucial" in its implications, still had to survive further investigation or continued development (see also Hacking 1983, pp.249-50). Promising experimental results that suggested a problem or domain was plausible or worth pursuing never guaranteed generating the "right" answers. The demonstrations did, however, fuel further significant research.

Given the dual justificatory-suggestive status of demonstrations, one may be tempted to use them to construct some sort of comprehensive criterion of progressiveness, probable belief, or level of opportunity or novel problems posed, by which the alternative research enterprises in the ox-phos episode were (or should have been) assessed. But this would blatantly disregard the situatedness of each demonstration in validating certain questions only locally and in warranting only specific prospective work. Again, the domain or scope of justification for each demonstration was local, or limited to phenomena that (with given experience) could be classified as similar (recalling, perhaps, problems posed by Goodman, 1978, about exemplification and "fair samples" and, 1965, about induction classes). Still, the multiple demonstrations cited above were collectively effective at discriminating between the two hypotheses, or research programs. Because the two traditions in ox-phos were conceptually and experimentally incompatible—that is, incapable of recombining their parts (see also Allchin 1990; 1991, pp.168-241)—they interacted as wholes. Yet warrant for each was nevertheless established piecemeal. It was the domain, or substrate of study, however, that was "reduced" or decomposed, not the problems or conceptual framework (as in Simon 1969; Wimsatt 1980). There were no crucial tests between the major theories or even between individual corresponding concepts. But there were demonstrations that, as research successfully deepened, established for each research lineage authority over certain relatively distinct local domains.

Attention to specific local domains is critical because it allows one to understand fully how the controversy was resolved. When debate finally subsided, marked by an exceptional joint six-author review article (Boyer et al. 1977), both research traditions remained: how? The demonstrations detailed above had largely vindicated the novel chemiosmotic problem-field, which now formed the central framework for interpreting the transfer of energy from oxidation to phosphorylation. The original chemical lineage, its concepts and problems, however, also persisted— though in a substantially more limited domain associated with certain finer-scale problems. Indeed, even a third major hypothesis (Boyer's conformational hypothesis), representing yet another lineage, contributed to the final interpretation. In this case, there was no single solution to the problem(s) of ox-phos; there was only resolution among originally competing, overlapping explanations. One must jettison the either-or, winner-take-all terms of most models of theory-choice (e.g., Kuhn 1962; Lakatos 1970; Laudan 1977; Howson and Urbach 1989) and instead characterize the outcome as a differentiation of domain or the distribution of authority among several theories (see also Whitt 1990, pp.474-476, for similar conclusions in the case of the debate between Dalton and Berzelius over the proper problem domains in chemistry). The experiments had established by exemplification or ostension the constellation of local domains appropriate to each set of questions. In this sense, the demonstrations were "crucial" not only in guiding research, but also in resolving disagreement. That is, they not only justified each theory, especially in the face of criticism, but they allowed one to partition the domain into separate contexts or domains, and thereby resolve the deep interparadigm conflict.

4. From Historical Case to Philosophical Principle and Scientific Strategy

In detailing the acid-bath experiments, etc., from the ox-phos episode, I hope that other historical examples that resonate with them will be highlighted. Runcorn's polar wandering curves, in the context of criticisms of continental drift; Lavoisier's measurements of the conservation of mass during combustion specifically when oxygen was included, in the context of interpretations using phlogiston; and Young's documentation and measurement of light interference, in the context of wave-corpuscular questions—as three immediate cases—were all critical to intertheoretic debate. All filled the role of concrete, domain-claiming demonstrations that promoted certain conceptions of the problem while not specifically refuting alternatives. If I do not analyze these or other examples here in the same depth as the ox-phos episode, they can nevertheless indicate that the case I have presented is hardly unique; and I trust that the details of the ox-phos case "demonstrate" their particular significance.

But further, one may hope to generalize from these cases, using historical clues as an occasion to develop more formal philosophical principles (e.g., Darden 1991; Wimsatt 1987; 1992). Discussion of the ox-phos episode above has been oriented, in fact, specifically to highlight the significant epistemic features of what I am calling 'demonstrations'. A demonstration may be seen as a uniquely significant sample confirmation, and (as noted above) complementary in its role to a (model) falsifying instance or anomaly. That is, while falsifications function negatively, selectively, or eliminatively to reduce theory (as noted above), demonstrations function positively or constructively to expand extant theory. Their primary feature is to legitimate a conceptual frame with respect to a certain local domain, perhaps construed as a variable or a dimension or range of measurement. In this respect, demonstrations in science may be compared with precedents in the development of common law. Largely through ostension or exemplification, the demonstration also makes a question or problem-frame concretely plausible, thus indirectly giving warrant to continued pursuit of similar or related questions in that area.

Just as one cannot always effectively localize a falsifying instance in a body of theory and method, so, too, one cannot explicitly distribute the "credit" of a demonstration. But when the demonstration is successful (inherently so—see below), there is little immediate need to isolate any unreliable, unnecessary or redundant element, if any exists: the system works, the problem-frame is effective at getting solutions, and there is no explicit error to handicap further research. The productive result validates the ensemble of factors as an integrated ensemble.

A demonstration may be further distinguished from a 'test'. Tests exhibit symmetry, or parallel experimental conditions, based on the alternative answers for a given question at issue; thus (when it approaches the ideal of their unambiguous design) a test's outcome can distinguish clearly between two or more conceptual alternatives. In the ox-phos case, such tests were able to address (within the chemical framework) such questions as whether there were one or more steps in the energy-transfer sequence; how many molecules were involved; at what step phosphate was introduced, etc. (Allchin 1990, pp.55-57; 1991, pp.144-167). (I assume that the notion of a test, or a 'crucial test', is familiar and that one need not enumerate its features further.) One may note, however, that tests may only be effective within paradigms—that is, for cases where questions are shared and background conditions are similar for each alternative solution.

A demonstration, by contrast, is distinctly asymmetrical. That is, the alternative results of the experiment do not have equal import. If "successful," a demonstration is meaningful: it shows, sometimes quite dramatically, how a (the) problem can be solved. If unsuccessful, however, the experiment implies very little other than perhaps a lack of imagination or luck in finding the right combination of experimental procedures and theoretical parameters (note Kuhn on puzzle-solving, 1962, esp. p.37). The acid-bath demonstrations, for example, strongly legitimated the chemiosmotic problem-frame, while the failure to isolate the high-energy intermediates, while clearly frustrating to the chemists, did not directly "falsify" or challenge their questions.

In the context of disagreement, the acid-bath and other demonstrations each represented a sort of "territorial claim" in their respective domains. (This is an especially apt metaphor where one construes theories or their models as conceptual maps). As noted above (§3), the claim essentially shifted the burden of proof. For the opposing chemists to re-claim their territory, they would have to "advance" an even stronger alternative explanation, or show how the original result was merely a residual artifact (by demonstrating how the proposed cause could be screened off by another variable in producing the same result; Salmon 1984). Interparadigm debate (between problem-fields) thus proceeded by escalation. Demonstrations, each with more rigorous experimental demands or forms of completeness, may be stacked on a local domain, much as chess pieces may all be concentrated on a particular board-position—until the demonstrative resources one research tradition can muster are depleted. This would be the case, at least, where domains overlapped and could not be partitioned or more finely "resolved" as they were in the ox-phos controversy.

Lastly, one may expect that demonstrations may also function much like Kuhnian exemplars (1962, pp.viii,10,23,80-81). That is, they may serve as explicit points of departure for further research, notably where the demonstration makes an unexplored domain accessible through an effective problem-frame or set of questions. Identifying certain experiments as having such a role fits comfortably with characterizations of scientists as pursuing "the path of opportunity" or (perhaps more cynically) as "opportunists" (Pickering 1984, p.10; in the ox-phos case, Robinson 1984).

The concept of a 'demonstration', then, when set against the notions of 'tests' and falsifying anomalies, provides a way to interpret interparadigm debate, as exemplified in the ox-phos controversy. But the touchstone of value for this concept, like any philosophical concept of science, may be whether it provides tools or strategies to practice science more effectively. Indeed, the distinction between tests and demonstrations and the role of demonstrations in warranting questions suggests that scientists cannot neglect the context in which they present their arguments: only certain types of experiments will be effective in different modes of disagreement. Where researchers share questions or problem-fields, one expects that crucial tests will be effective. Where they must argue for the questions themselves, however, one must not attempt to construct crucial 'tests'—or worse, try to falsify an opponent's questions: one must *demonstrate*. Of course, scientists must recognize the signals of the two forms of debate—not always obvious. This study thus complements a diagnosis of disagreement that distinguishes between intra- and interparadigm debate (Allchin 1990)—and aids scientists in identifying the specific contexts under which demonstrations versus tests will be crucially effective in resolving disagreement.

Notes

[1]The interrogative orientation relates to Kuhn's sense of a paradigm as a problem-field (1962, pp. 103, 147-48, 155, 157), and contrasts to a 'hard core' (*sensu* Lakatos, 1970) of conceptual commitments. The focus here clearly points to the need for philosophers to reflect more deeply on the nature of 'problems' (how they originate, how they are considered solved, etc.), how problems may be framed differently, and how they may fit in ensembles of problem-fields—work suggested and begun, for example, by Shapere (1974) and Nickles (1981). This orientation may also guide a reading of Kuhnian 'incommensurability' as the clash between questions or problem-fields, not meanings or world views (Allchin 1990).

[2]This may, in turn, have contributed to an image of "promise" for the chemiosmotic problem-field. But my emphasis here is not on the cognitive roles of surprise, prior expectation, or opportunity in posing new questions (though these may have occurred secondarily—see Robinson 1984). Rather, I want to underscore the justificatory role of the demonstrations. By concretely embodying answers when questions were posed in a certain way, they exemplified how further experimental phenomena could be constructed "downstream" and, equally important, they shifted the burden of proof (see below).

[3]Support may also be seen as distributed across a community of researchers, where each member is viewed as having different cognitive resources and commitments to explaining different local domains (Giere 1988; Whitt 1990, pp. 476-479; Allchin 1991, pp. 278-291). In this episode, one may be especially impressed by the case of E.C. Slater, whose trenchant criticism against the chemiosmotic hypothesis persisted until he became satisfied that it solved the "35-year-old paradox" of the cytochrome b complex, the portion of the system that he studied and knew most intimately (Slater 1981a).

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