Allan Franklin's Transcendental Physics

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Does Allan Franklin's study of atomic parity-violation experiments provide convincing evidence against social constructivism? According to Franklin (1990a, p. 2), "when questions of theory choice, confirmation, or refutation are raised they are answered on the basis of valid experimental evidence . . . [and] there are good reasons for belief in the validity of that evidence." Franklin asserts that social constructivists take the opposite position: "They would say that it is not the experimental results, but rather the social and/or cognitive interests of the scientists, that must be used in the explanation." Having set up the contrasting positions, he then asks the reader, "which of us is telling the more plausible story?" (Franklin 1990b, p. 163). Consistent with his evidence model, he proposes to discriminate between the two opposed positions by consulting historical evidence. He describes the results of two different sets of experiments on atomic parity-violations, and assesses the extent to which the experimental data match predictions based on the Weinberg-Salam unified theory of electroweak interactions. The earlier experiments, performed in the mid-1970s at Oxford and Washington did not support the W-S theory, whereas the later experiments, performed in 1979 in the Soviet Union and at Berkeley and SLAC, supported that theory. Franklin emphasizes that the later experiments used different arrays of equipment, as well as a variety of procedures for checking results, eliminating backgrounds, and ruling-out possible sources of artifact.

Franklin argues that the physics community's eventual preference for the later (SLAC, etc.) experiments was "reasonable" (i.e., justified by evidence and procedural rationality). He argues that this preference was not based on absolute grounds, but that it was justified by the superior "weight" of the evidence. As he reconstructs the situation in the 1970's particle physics community, physicists recognized at the time that the Oxford-Washington experiments were problematic for several reasons. Not only did the experimental results conflict with predictions based on an accepted theory, the calculations of the theoretically predicted effects of passing polarized light through bismuth vapor were uncertain, and the experimental techniques were untried. "These were extremely difficult experiments, beset with systematic errors of approximately the same size as the predicted effects" (Franklin 1990b, p. 176). In contrast, the later experiments were more convincing, not only because they seemed to support the W-S theory, but because they employed more reliable procedures which produced

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less systematic error in the experimental results. Franklin goes on to give a nauseatingly detailed account for those of us who are not trained in physics, and if we take his word for it (and I have no reason not to) particle physicists had good reason to accept the USSR-Berkeley-SLAC experiments, even though they found no "fatal flaws" in the results of the Oxford-Washington experiments. He draws two related conclusions from this: (1) The evidence model accounts for the historical episode, and (2) the evidence model is supported by the historical evidence to a greater extent than is the social constructivist view. I can imagine that both of these claims are disputable, although I will only take issue with the second claim. I am not going to try to support the converse of Franklin's argument, i.e., by claiming that social constructivism accounts for the evidence better than does the evidence model. Instead, I shall question the way Franklin initially sets up the opposition between his evidence model and a social constructivist position. In my view, Franklin's attempt to use an historical case study to settle the realist-constructivist debate is symptomatic of his more general inattention to the difference between epistemological argumentation and situated practical reasoning. However diligently and competently he describes the atomic parity-violation experiments and their results, he miscasts the position he says he is arguing against and the choice between his position and social constructivism is undecidable on that basis.

1. The Evidence for the Evidence Model

Franklin argues (1990b, p. 163) that his "evidence model applies to both science and the study of science." In a footnote (n. 3, p. 163) he acknowledges that "Some readers may worry that I am using the evidence model to decide whether or not an evidence model applies to science." This is not a serious problem, he says, because there are no guarantees "that the view that scientists use such a model will be supported by the evidence." As far as he is concerned, the relationship between an epistemological position and an historical case study is analogous to that between predictions based upon a physical theory and relevant experimental evidence. This analogy is a fairly "thin" one, especially if we accept what Franklin has to say about the "epistemology of experiment." According to him, experimental instruments and techniques incorporate checks, triangulation procedures, and other strategies for establishing the validity of results. However, it is not clear to me how his schematic reconstructions of experiments could themselves be comparable to the material practices and assessments of evidence they describe. Franklin approvingly cites Peter Galison's (1987) argument to the effect that the modern particle physics community houses separate material cultures in which theorists, instrument makers, and experimentalists hone their skills and develop their collective interests. This situation, Galison argues, offers practical constraints on the testing of theories, since the experimentalists and instrument makers do not simply follow after the demands of the theorists, but act in accordance with their own distinctive traditions. However, there is no comparable independence between Franklin's articulation of his evidence model and the narrative descriptions he uses as evidence for it. He crafts both of them from within the same literary space. Nor is his general epistemological model precisely constrained by the sorts of material, institutional, and practical conditions that Galison identifies in the particle physics community. In brief, while Franklin's evidence model may be well argued and convincingly documented, his documentary methods do not incorporate anything like the practical and social constraints that Galison identifies for physics experiments.

Franklin tells us that particular scientists had "good reasons" for acting as they did in the cases he reconstructs, and that they accepted evidence because it was "valid". He employs a Bayesian approach to reconstruct scientists' probability judgments

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about the relationship between hypotheses and evidence, but he admits (Franklin 1990a, p. 100) that this procedure does not reflect the actual judgments scientists made at the time. And, I would add, in many instances it is questionable whether it makes sense even to use rough probability estimates to reconstruct their judgments. A great deal turns upon what exactly might be meant by such terms as "good reasons" and "valid evidence," as both expressions permit a wide range of applications to particular cases. Franklin's case description supports his initial claim, but his story is motivated by and organized around the epistemological lesson he uses it to elaborate, and I fully expect that his constructivist interlocutors would be able to recite a different story of the "same" case supporting their claims. Even if Franklin and Galison are correct when they say that theories and experimental results in physics are not "so plastic that they can always be brought into agreement with each other" (Franklin 1990a, pp. 158-9, n. 25), I would not say this about the relationship between general epistemological claims and historical case descriptions. So, I have considerable doubt about the way Franklin sets up his study as a "test" of what he seems to think are two mutually exclusive epistemological theories.

2. Sociology of Scientific Knowledge Explanations

Franklin presents a strict opposition between his evidence model and the social constructivist "view" (note that he does not call it a "model"). The evidence model states that scientists' choices are based on "good reasons" and "valid evidence," whereas he leads us to think that social constructivists believe the opposite, that such choices are based on poor reasons, or no reasons at all, and that scientists disregard experimental evidence. Consider two of his characterizations: (a) "Obviously I do not agree with the social constructivists that all pictures of the world are equally good" (Franklin 1990b, p. 163); and (b) "[Pickering (1984)] obviously doubts that science is a reasonable enterprise based on valid experimental or observational evidence" (p. 165).

Franklin does not always characterize social constructivism so starkly, but the way he phrases these two characterizations maximizes the contrast between the explanations given by his evidence model and and those given by social constructivists. In his view, the social constructivists entirely discount the role of experimental evidence, and they treat theory-laden "interests" as the sole basis for the construction and interpretation of experimental results. The contrast between the two positions, as Franklin presents them, can be concisely stated as follows:

EVIDENCE MODEL: theory choice, confirmation, and refutation are made on the basis of valid experimental evidence.

INTEREST MODEL: theory choice, confirmation, and refutation are *not* made on the basis of valid experimental evidence; instead, they are based on social interests.

This way of setting up the comparison implies that actions based on social interests are incompatible with evidential justification. Although Franklin's set up may facilitate a clear choice between the two positions, it gives a very misleading picture of what the social constructionists have argued. Social constructivism is not a single position, but for the most part Franklin focuses only on two loosely organized "programmes" in British sociology of science: the Edinburgh School's "strong programme" in the sociology of knowledge (Barnes 1974; Bloor 1976), and a related "empirical relativist" case-study approach (Collins 1985; Pinch 1986). In David Bloor's (1976, p. 1) terms, the programmatic aim is to investigate, and sometimes to explain, "the very content and nature of scientific knowledge."

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The strong programme in the sociology scientific knowledge built upon Karl Mannheim's (1936) *Wissensoziologie*, by proposing to "strengthen" its domain of application. Mannheim was concerned with the question of how to demonstrate the relationship between historical systems of knowledge and their "existential" conditions. In a famous passage, he suggested how this could be done:

> The existential determination of thought may be regarded as a demonstrated fact in those realms of thought in which we can show (a) that the process of knowing does not actually develop historically in accordance with immanent laws, that it does not follow only from the "nature of things" or from "pure logical possibilities", and that it is not driven by an "inner dialectic". On the contrary, the emergence and crystallization of actual thought is influenced in many decisive points by extra-theoretical factors of the most diverse sort. These may be called, in contradistinction to purely theoretical factors, existential factors. This existential determination of thought will also have to be regarded as a fact (b) if the influence of these existential factors on the concrete content of knowledge is of more than mere peripheral importance, if they are relevant not only to the genesis of ideas, but penetrate into their forms and content and if, furthermore, they decisively determine its scope and the intensity of our experience and observation, i.e. that which we formerly referred to as the "perspective" of the subject. (Mannheim 1936, pp. 239-40)

Mannheim acknowledged that not all knowledge is equally amenable to this mode of explanation, since some of the propositions in mathematics and the "exact" sciences seem to be immanently accountable. The historical stability and consensual use of a statement like "two times two equals four" makes it impossible to show how the content of the statement reflects the particular social position of its users (Mannheim 1936, p. 272).[1] The form of the statement "gives no clue as to when, where, and by whom it was formulated," unlike an artistic work whose composition can give art historians many clues for assigning it to a particular artist or genre of art, associating it with historically relative stylistic conventions, and explicating the relevant artistic community's presuppositions about the nature of the artistic subject. Similarly, a text or argument in the social sciences can easily be traced to a "school" or "perspective" like Marxism, functionalism, or rational-choice theory.

Bloor and other social constructivists take issue with Mannheim's apparent "exemption" of mathematics and the exact sciences from the purview of the sociology of knowledge, and they argue that this exemption is a consequence of his belief in the transcendent reality of mathematical objects and natural laws. As I understand Mannheim's position, however, he is not subscribing to an absolutist position on the "nature of things" or "pure logical possibilities," any more than he is endorsing a Hegelian conception of the "inner dialectic" of ideas. Instead, he is discussing the requirements for *demonstrating* the "existential determination of thought" against the claims of various absolutist and transcendental philosophies. Naturalism, logical determinism, and dialectics challenge the sociology of knowledge with obstinate arguments generated from within, or on behalf of, rival philosophical commitments. Such arguments are not easily displaced, and Mannheim recommends a methodical procedure for accomplishing their displacement in particular cases. This procedure has two basic steps:

(a) Employing historical comparisons to show that an "immanent theory" cannot entirely explain the contents and historical development of the system of knowledge in which it is situated. This procedure is used to demonstrate that such a theory cannot unequivocally and exhaustively attribute the state of its knowledge at any given time to "the nature of things," "pure logical possibilities," or an "inner dialectic".

(b) Specifying the social conditions (the local historical milieux, class interests and group 'mentalities', rhetorical strategies, etc.) that influenced the development and content of the given state of knowledge.

Mannheim strongly opposed transcendental and absolutist philosophies, so it might seem that he would dismiss the very possibility that knowledge *could ever* "develop historically in accordance with immanent laws." Nevertheless, he was unable to find a way to demonstrate that an expression like "2 x 2 = 4" could be explained by extra-theoretical "existential" factors. Bloor, Barnes, Collins and other contemporary sociologists of knowledge addressed this problem by drawing upon a variety of sources to supplement and broaden Mannheim's explanatory program. The "strong programme" in the sociology of knowledge retained Mannheim's basic two-step form of demonstration, while modifying it to cover science and mathematics. With appropriate modifications of Mannheim's terms, adherents to the strong programme sought to show that:

(a) While scientists and mathematicians may act in accordance with the immanent logic of theory, their actions are not unequivocally determined by "nature of things" or "pure logical possibilities." On the contrary, the extension of a mathematical rule or scientific theory is determined by socialized judgments and practical interests that limit the field of acceptable applications.

(b) The influence of social factors on the concrete content of scientific and mathematical knowledge is of more than peripheral importance. Intra- and extra-scientific factors influence the acceptance of theories and the interpretation of experimental evidence.

Sociologists of scientific knowledge who adhere to the strong programme often accomplish the first step with the aid of arguments from philosophy of science about the underdetermination of theories by facts and the theory-ladenness of observation, and they make use of more general skeptical arguments about the relationship between signs and meanings.[2] Radicalizing Kuhn, they tend to view historical controversies as particularly illuminating phenomena. Their descriptions of controversies demonstrate that consensus is essentially fragile, that controversies end without being definitively settled, and that stable scientific fields often include disgruntled members who ascribe the consensus in their fields to 'mere' conformity. Historical and ethnographic documentation provides the necessary leverage for contesting the unequivocal determinacy of the "nature of things" or "pure logical possibilities," and for demonstrating the contingent nature of consensus within particular disciplines. The second step is accomplished by using diverse sources from sociology, anthropology and the philosophy of language. Bloor (1976), for instance, calls upon Durkheim's basic method for linking the symbolic content of religious ritual and magical belief to the structural divisions within the tribe. He and Barnes (1983) update Durkheim's second-hand anthropology by making use of Mary Douglas' cognitive anthropology, and particularly her "grid-group" scheme for linking the properties of a group to the cognitive style of its members' beliefs and arguments. Barnes, Bloor, and Collins also make use of Mary Hesse's (1974) "network" approach to the organization and entrenchment of culturally-specific classificatory schemes. This approach enables a demonstration of non-arbitrary (i.e., relational) variations between the configurations of similar semantic domains in different knowledge communities.

Franklin's main target is Andy Pickering's (1984) *Constructing Quarks*, an exhaustively detailed and innovative exemplar of the strong programme's research policies. As its title suggest, the study reviews the series of theoretical and experimental developments since the 1960's that culminated in the establishment of what Pickering calls the "quark/gauge theory world-view." This world-view was populated by new theoretical entities, including "quarks," which were said to be fundamental constituents of protons and neutrons. Gauge theory provided incentive for particle physicists to pursue funding for increasingly massive and powerful instruments to "penetrate" more deeply into the inner structure of matter. In line with the strong programme's two-step method of demonstration, Pickering (1984, p. 6) contests what he calls "the scientist's version" of the immanent development of a series of experiments supporting the new theories on the composition of matter. He cites the familiar philosophical arguments on the underdetermination of theories by experimental facts, and he also argues that the "facts" themselves are "deeply problematic". This, he says, is because the factual status of experimental data depends upon fallible judgments about whether equipment was functioning properly, effective controls were made, and relevant signals were correctly distinguished from noisy backgrounds. Moreover, he argues, the "factual" sense and meaning of the data were developed through the use of models, analogies, and simulations which aligned those data with theoretical pre-conceptions. Pickering proposes that the relation between theory and experimental data is one of "tuning" or "symbiosis" rather than independent verification of theory by fact. His historical account demonstrates the "potential for legitimate dissent" on questions of experimental procedure and theoretical interpretation of data.[3] He describes the debates between different research groups, and uses their discrepant accounts as a basis for demonstrating the multiplicity of possible interpretations of the relevant experimental events and their theoretical implications. To explain how scientists manage to accomplish experimental interpretations and theory-choices he introduces a concept of "opportunism in context," a way of describing how physicists pursue the particular experimental-interpretive pathways that enable them to exercise their professional skills and make use of the most "interesting" of the available theoretical developments.

Pickering's study is distinguished by its close attention to experimental practices and instrumentation. He discusses the available designs for bubble-chamber apparatus, methods for interpreting traces of sub-atomic particles, and computer simulation procedures used in experiments on "weak neutral currents". His pragmatic focus is consistent with a recent trend in social studies of science toward descriptions of experimental instrumentation, technique, and analysis (Shapin and Schaffer 1985; Gooding 1988). The more abstract, theory-based conception of knowledge familiar from earlier socio-historical studies is gradually turning into a more particularistic conception of the material sites, artifacts, and techniques of 'knowledge production'. The focus is more intensive and "internal" (in the non-rationalist sense), as the aim is to identify the pragmatic strategies and informal judgments made at the worksite when researchers sort through "messy" arrays of data and decide whether equipment is working properly.

As I understand it, Franklin's characterization of the strong programme's thesis is more than a little overdrawn, and in my view his historiographic "test" does not actually discriminate between mutually exclusive accounts of the relationship between theory and experiment. Social constructivists do *not* say that experimental evidence is irrelevant to theory choice, confirmation, and refutation. Nor do they argue that there are no good reasons for belief in the validity of evidence. Instead, they argue that experimental evidence does not *compel* acceptance of a single theory, or, in Pickering's (1984, p. 5) terms, "experiment cannot *oblige* scientists to make a particular choice of theories." Franklin apparently does not disagree with Pickering on this point. Franklin (1990a, p. 147) agrees, at least partially, with the Duhem-Quine underdetermination thesis, which states, as he puts it, that "no finite set of confirming instances can entail a universal statement," and he gives a homely example: "No matter how many white swans one sees it does not entail that 'all swans are white.' A single instance can, however, refute a universal statement. Observation of a single black swan refutes 'all swans are white'." (I am not so sure about the latter part of this statement, that a single black swan would refute the statement "all swans are white." Unless the anomalous instance were determined to be representative of a coherent population, variety or species of swan, it would most likely be viewed as a freak, mutant, or victim of an oil spill.) Franklin then says that, for him, "compel" means "having good reasons for belief," and he characterizes these good reasons in terms of pragmatic strategies and plausibility judgments. It is not clear to me whether this version of experimental "compulsion" is incompatible with the constructivist position, since constructivist explanations only require that, as mentioned earlier, scientific developments "do not follow only from the 'nature of things' or from 'pure logical possibilities'." The important words here are "only" and "pure". The explanatory program does not prohibit the possibility that scientists use the ruling-out and reality-testing strategies that Franklin describes, it only requires that these not be treated as absolutely compelling or exclusive grounds for belief.

3. "Good Reasons" and Valid Evidence

What is at stake in the debate between social constructivism and realism can perhaps be clarified by focusing upon two related questions: (a) whether "good reasons" for accepting evidence imply validity, and (b) whether sociologists and historians of science should take a partisan position on the scientific arguments they study.

(a) The first question concerns whether the fact that theory choice, confirmation, and refutation are made on the basis of experimental evidence justifies the conclusion that such evidence is "valid". From a sociological point of view, the uncontroversial fact that scientists typically give reasons for their choices — reasons that they hope will be accepted as good reasons for the validity of the evidence — does not justify treating such reasons as causes for consensual belief in the validity of the evidence. In my reading, "valid" is a gratuitous term in Franklin's assertion that theory choices are settled "on the basis of valid experimental evidence." Even if we grant that scientists base their theoretical judgments on experimental evidence, and that retrospective analyses of the evidence show systematic patterns consistent with such judgments, are we compelled to conclude that the evidence was "valid"? As Hacking (1983, p. 54) puts it, "To add 'and photons are real', after Einstein has finished, is to add nothing to the understanding. . . . If the explainer protests, saying that Einstein himself asserted the existence of photons, then he is begging the question. For the debate between realist and anti-realist is whether the adequacy of Einstein's theory of the photon does require that the photons be real."

Sociologists and historians of science have described numerous cases where members of scientific communities come to agree that particular experimental or observational results are valid. So, for instance, I take it that in the case of atomic parity violation experiments, Pickering and Franklin both agree that as Franklin (1990b, p. 165) puts it, "By 1979 the Weinberg-Salam theory was regarded by the high-energy physics community as established." But where Franklin wants to explain the establishment of this theory by citing the validity of experimental evidence for it, Pickering aims to describe the historical process without initially making assumptions about which evidences were or were not valid.

(b) Instead of opposing or rejecting evidential accounts of theory choices, sociologists of knowledge try to remain uncommitted to the extant 'beliefs' in the communities they study. This policy dates back at least to Mannheim's (1936) attempt to distinguish the sociology of knowledge from an epistemologically "relativist" position by saying that relativism retains an absolutist standard of evaluation when it confuses the insight that 'all knowledge is relative to the knower's situation' with the conclusion that 'all knowledge-claims must be doubted.' Presuming to doubt *all* knowledge is no less absolutist than presuming that there must be a ground for all true knowledge. So, instead of advocating relativism, Mannheim argued for a "relationist" concept of knowledge. Rather than opting for a radically individualistic conception of knowledge, he proposed that particular ideas are situated within historical and social circumstances. Such ideas might not be justifiable, according to absolutist standards of rationality, but this should not discount their adequacy in terms of the relevant epistemic community's categorical judgments and validity claims. For Mannheim, "relational" knowledge — knowledge cultivated within a living community of understandings — could be dynamic without necessarily being arbitrary, and he argued that a "non-evaluative general total conception of ideology" could be attained.

> The non-evaluative general total conception of ideology is to be found primarily in those historical investigations, where, provisionally and for the sake of the simplification of the problem, no judgments are pronounced as to the correctness of the ideas to be treated. . . . The task of a study of ideology, which tries to be free from value-judgments is to understand the narrowness of each individual point of view and the interplay between these distinctive attitudes in the total social process (Mannheim 1936, p. 80).

Although contemporary sociologists of science are critical of many of Mannheim's views, they share his aim to "step back" from the systems of knowledge studied without discounting the cultural and pragmatic 'validity' of that knowledge. Franklin (1990b, p. 162) quotes a line from Trevor Pinch (1986, p. 8), saying about scientific "beliefs" that "*many pictures* can be painted, and furthermore, . . . the sociologist of science cannot say that any picture is a better representation of Nature than any other." Pinch is not saying that "all pictures of the world are equally good" (Franklin, 1990b, p. 163) nor is he denying that normative appraisals of the evidence are part of the picture. Instead, he is making a point about the relationship between the sociology of knowledge and the scientific fields it studies. The important phrase in his remark is "the sociologist of science cannot say . . .." Pickering makes a similar point about history of physics. He argues that the historian's descriptive task is different from the naturalistic endeavors of the scientists studied. Historians write about human actions, whereas physicists attempt to "discover the structure of nature" (Pickering 1984, p. 8). Like other constructivist sociologists of science, Pinch and Pickering recommend that sociologists should attempt to study the contemporaneous actions of scientists, while remaining detached from the scientists' naturalistic commitments.

Franklin has fewer qualms about retrospective descriptions based on currently accepted physics, in part because he accepts the distinction between context of discovery (or, as he prefers, the context of "pursuit") and context of justification. He says he is interested mainly in justification of physicists' choices, and he comfortably makes assertions like the following: "During the 1960s events now attributed to weak-neutral currents were seen but were attributed to neutron background. At the time there was no theoretical prediction of such currents" (Franklin 1990b, p. 164). From his point of view, what physicists later took to be the case can be used to define what physicists had "seen" in the 1960's. Social constructivists have a different aim entirely, as they try to describe the operative conditions under which scientists perform their collective activities. From their point of view it is absurd to say that physicists had "seen" evidences of weak-neutral currents before they had the relevant concept. Instead, it would be more appropriate to say that they *saw* fluctuations in the neutron background. The descriptive task would then be to follow the series of theoretical innovations, experiments, negotiations, arguments, controversies, and the like, from which the 'discovery of weak neutral currents' eventually emerged. Such a history would not reproduce the physicists' historicized account of the discovery, it would attempt to recover the series of scientists' actions together with their historicized achievement (Garfinkel, Lynch, and Livingston 1981).

One can certainly question whether historians and sociologists can indeed detach themselves from retrospective understandings, and one can surely doubt that it is possible to describe scientists' actions without making judgments about the correctness of those actions and the theoretical entities they implicate. But it misses the point to read Pinch or Pickering to be saying that evidence is irrelevant to theory choice or that all theories are equally acceptable. Franklin's reconstructions of experiments on parity-violation experiments do not refute their claims, since they do not make the claims he refutes. In a way, he is speaking as one of the "natives" Pinch and Pickering study when he insists that evidences are "valid" and that there are "good reasons" for the choices scientists make.

4. Transcendental Physics

To return to the question with which I began this paper: Does Franklin's evidence compel us to favor his evidence model instead of Pickering's constructivist model of "opportunism in context"? My answer is "No," since Franklin's evidence is not independent of the articulation of his model, and the position he tries to persuade us to reject is a caricature of a constructivist argument. Short of awarding Franklin with a decisive victory, we could perhaps consider giving him more modest credit. But to do this, we need to locate where exactly his account differs from those of his constructivist interlocutors. Franklin's major challenge to constructivism seems to be that the evidence provided by the Washington-Oxford experiments was doubtful from the beginning, and that this was recognized at the time by members of the physics community. According to him, the uncertainties in the calculations and in the experimental procedure were such that physicists did not accept the results with a great deal of confidence. When the results from the 1979 experiments were presented, according to Franklin, physicists had clear procedural and evidential grounds for preferring them to the Washington-Oxford results. He argues that researchers forged ahead on the basis of relative (but not absolute) assurance that the evidence supporting the W-S theory was stronger than the evidence against it, and later experiments further justified their judgments. So, according to Franklin they had "good reasons" *at the time* for favoring the experiments supportive of the W-S theory. His analytic procedures mute this claim somewhat, since his retrospective assessment of the evidence confuses the issue of what physicists at the time made of the relevant experimental results.

At this point, to assess (or contest) Franklin's claim seems to require historical research about just how physicists understood the Washington-Oxford experiments in the late 1970s. Franklin does supply some testimony about this, but he confuses the issue with his overriding concern about whether the physicist's choices were, in the end, justified. From his account, we do not get any sense of there being a diversity of views in the mid-1970's particle physics community, nor does he give us a dynamic picture of the various arguments that may have circulated within that community and of the temporal "careers" of those arguments. His narration of experimental practices is rather static, as Robert Ackermann pointed out in a review of Franklin's (1986) earlier book:

> An irony of Franklin's book is that an actual experimental set-up is only portrayed once in its fully contingent form, and that in the glorious confusion of apparatus in the dust jacket photograph. Inside, as in all 'histories' of experimentation, experimental set-ups are given in schematic diagrams that portray the theory of how apparatus could work so as to produce meaningful data, and observational data are represented in the smoothed form gathered from properly working apparatus . . . (Ackerman 1989, p. 188)

I think the same could be said for Franklin's more recent reconstruction of the atomic parity violation experiments. Although I am not skeptical about Franklin's claim that scientists orient to evidence and have good reasons for acting as they do, I think it is worth distinguishing the reasons particular scientists give for their judgments from an account of how an idealized reasoner would assess reconstructed arrays of experimental data. This does not come down to a difference between contexts of discovery and justification, since provisional justifications are constructed when scientists progressively work through the contingencies in a novel experimental situation. Franklin's inventories of experimental checks, calibrations of equipment, and so forth, suggest some of the ways in which experimentalists construct justifications, but again he bases his "epistemology of experiment" on abstracted experimental designs stated in written reports. To gain an appreciation of the contingent production of "live" experimentation, perhaps an example will help. The example I will use is not drawn from particle physics, nor is it an example of an experiment. The example is taken from an account of a discovery in astronomy, and I use it here because it provides a simple and dramatic demonstration of a progression of actions unfolding in time in a "scientific" research situation.

5. An Excursion Into Astronomy

Below, I have reproduced portions of a transcript of a conversation that was recorded aboard NASA's Kuiper Airborne Observatory, while it flew over the Indian Ocean in March 1977. The transcripts were presented in an article published that same year by three of the members of the team (Elliot, Dunham, and Millis 1977, pp. 414-15). According to the researchers, their expedition was designed to record high-quality photo-electric light curves of a star (SAO 158687) as it was eclipsed by the planet Uranus. They believed that the data would enable them to find how the temperature and other properties of the planet's atmosphere changed with height above its surface, and by coordinating their observations with those from a few ground-based observatories, they hoped to be able to get precise measures of the diameter and oblateness of the planet. On the appointed night, they flew along a path calculated to be in the shadow of the eclipse, and they set the telescope on the star and begin recording its light curve on a chart recorder. About a half-hour before the predicted eclipse, the following conversation ensued:

(The main speakers in the transcript include Jim Elliot, principal investigator; Ted Dunham, data recorder; Jim McClenahan, NASA mission director; Al Meyer, telescope operator; and Pete Kuhn, meteorologist. Fourteen other members of the NASA team and flight crew were also aboard.)

Dunham:	What was that? What was that?
Elliot:	What?
Dunham:	This!
Elliot:	I dunno. Was there a tracker glitch?
Meyer:	Nothing here.

Dunham: Uh-oh. No, I don't think it's anything here, it's clearly duplicated in both channels.
Elliot: Yeah, I mean, clouds, or . . .?
Meyer: Ask Pete.
Dunham: Pete, what's your water vapor?
Kuhn: Eight point nine.
Dunham: Well, that's pretty low.
McClenahan: What happened?
Elliot: Well, we got a dip in the signal here, which was either due to a loss or momentary glitch in the tracker, or a cloud whipping through.
Dunham: Okay, I think somebody should have the responsibility of always watching the focal plane there. I suppose that a lot of people are.
Elliot: But no one caught that one.
McClenahan: Nobody caught that one. . . .

The transcript continues after a break of a minute or two:

Dunham: OK, I got a deep short spike here.
Elliot: I wonder if we're getting any clouds?
Dunham: No, Pete said we had . . . microns of water.
Kuhn: There's no clouds; I mean, truthfully, there's nothing up here.
Elliot: Well, maybe this is a D ring. ["This comment, which causes general laughter, was prompted by a team joke: If we didn't ob serve an occultation, we could use the data to put an upper limit on the optical depth of a hypothetical ring around Uranus!" — From Elliot *et al.*, (1977) p. 414.]
Dunham: With a normal optical depth of three, right? Another one.
Elliot: Yeah, those are real — I guess.
Oishi: Boy, that was a deep one.
Kuhn: Yep. There's no indication of any fog at all.
McClenahan: Doesn't seem to be any bore-sight shifting.
Elliot: Yeah, that's good. I think we're getting real — could be small bodies — the satellite plane is face on, or it could be just small bodies like thin rings.

. . .

Elliot: Maybe it's something to do with Uranus, because they seem to be about the same amplitude on that scale. Nominal occultation in twenty minutes.
Mink: Right.
Dunham: Another one!
Elliot: It's definitely the star being occulted somehow.

. . .

Dunham: There's another one!

The full eclipse occurred on schedule, and afterwards the researchers found out that their recording of the 'dips' was corroborated by Perth Observatory. Two days later, one of the researchers noticed that the dips in the light curve before and after the eclipse matched up almost perfectly, and this seemed to indicate that they were very thin rings, and not satellites (moons) as they had previously thought. Although Herschel claimed to see Uranus' rings when he discovered the planet in the late eighteenth century, this later was dismissed as an impossibility.

Ostensively, this excerpt from a "live" sequence of observations supports Franklin's thesis. The succession of phenomenal "dips" and the observing team's intervening checks on water vapor and instrument tracking can be cited as examples of an "epistemology of experiment." The transcript enables us to follow, in rapid succession, how a surprising and singular anomaly becomes progressively "attached to nature" as the team deploys its specialized skills and monitors the equipment to eliminate the possibility of a tracking error or cloud interference. The succession of dips provides a kind of naturally occurring basis for ruling out possible interpretations of the prior dips and honing in on a more restricted set of phenomenal possibilities. The astronomers themselves seem to subscribe to Franklin's thesis. In their published article, Elliot, Dunham, and Millis (1977, p. 414) give a 'Franklinian' interpretation of the transcript. Alongside the transcript they present commentaries on what they later determined they were seeing at the time. For instance, just before Dunham exclaims "What was that?" the article tells us that "First secondary occultation appears on the chart record, but is not noticed for almost a minute." They also identify "Delta ring occultation" at the point in the transcript where Dunham remarks, "OK, I got a deep short spike here." This ex-post facto identification of what the researchers were "really" looking at enables readers to gain an ironic appreciation of the transcripted fact that Elliot's remark "Well, maybe this is a D ring" at the time draws laughter from his colleagues. A determination made two days later — that the "dips" were evidence of occultations of the star by planetary rings — is used in the article to define what the researchers saw and failed to see at the time.

This way of conceptualizing the "actual events" in the sequence may seem plausible, natural, and even irresistible. Nevertheless, it is not an accurate historical description, if by "historical description" is meant an account that identifies the significance historical agents "attach to" the events in their life-world at a particular time. Instead, what we might call a "transcendental" vantage point equips the reader with a fore-knowledge of what was determined afterwards; a fore-knowledge that consequently acts as a backdrop for defining what the speakers in the transcript were "really" seeing. Franklin's account of parity-violation experiments is a slightly refracted version of such a transcendental history, as he does not hesitate to use a practically and historically-established account to identify how specific historical agents managed to achieve it. Moreover, Franklin tends to treat observation and reasoning as monological phenomena, whereas the transcript allows us to appreciate that observation and reasoning were accomplished by what might be called an observing assemblage; a multi-receptive, multi-bodied, and internally communicating socio-technical unit. The voices in the transcript testify to various coordinative and communicative actions that occur as the observing assemblage adjusts and reshapes itself in light of the latest in the series of "dips". Actions occur simultaneously on several fronts, through a flexible distribution of specialists and readable technologies coordinated by supervisory remarks and commands. What is especially impressive about the observing assemblage's organic maneuvers is that they are inextricable from the practical situation; a situation that includes the airborne observatory, along with its trajectory, its equipment, its staff, and its particular mission. The "epistemological" strategies are deeply embedded in that practical situation.

6. Conclusion

I have argued that Franklin's dispute with constructivism is miscast before he even begins to talk physics. Constructivists do not deny the role of practice, materials, and evidence. Nor do they say that all evidence is equally good or that scientists do not act without having good reasons. As Wittgenstein (1969, §105) asserts,

> All testing, all confirmation and disconfirmation of a hypothesis takes place already within a system. And this system is not a more or less arbitrary and doubt-

ful point of departure for all our arguments: no, it belongs to the essence of what we call an argument. The system is not so much the point of departure, as the element in which arguments have their life.

When uncertainties emerge they are resolved (when they are resolved) in reference to an unquestioned background. The background for scientific experiments includes accepted theories, concepts, procedures, and organizational arrangements, as well as entities and forces that are assumed to exist, laws that are held to be invariant, general and particular conceptions of how the experimental instruments are designed to operate and how they are operating now, assessments of the competency of the staff, and trust of previous runs of the experiment. Some of these hold fast as unquestioned bases for noticing and resolving particular uncertainties. On the surface, this seems to support Franklin's argument, but upon further examination it does not. Wittgenstein resists making any suggestion that those things that we hold fast, or that we use as practical "tests" for assessing more tenuous matters, are therefore "real" or "certain" in some metaphysical sense. Franklin seems to want to upgrade the *praxeological validity* (Garfinkel et al., 1988, p. 22) of experimental practices into "epistemological strategies," and while this may seem warranted by the case materials he presents, it has the effect of detaching these strategies from the local equipmental and technical environments that enable and at the same time frustrate scientists' attempts to make experiments work.

Notes

[1]Also see Mannheim (1936, p. 79). Stephen Turner (1981, p. 231, n. 3) observes that, contrary to what is assumed in many criticisms, Mannheim's exemption of the truths of arithmetic from sociological explanation was not made "on the ground of a criterion of 'rationality'."

[2]For a concise account of the use of the underdetermination and theory-ladenness theses in sociology of scientific knowledge, see Knorr-Cetina and Mulkay (1983).

[3]Pickering's training as a physicist is indispensable for this procedure, since it enables him to make claimably "legitimate" counterfactual assessments on what the experimenters he examines *could have* concluded. His competency enables him to avoid engaging in the sort of armchair relativism where general philosophical arguments assure the possibility of interpretative alternatives to those considered by the actual participants in an historical case. So, in a sense, his account is also a "scientist's version" although one that expresses perhaps an unusual set of theoretical and methodological commitments.

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