Reason Enough?
More on Parity-Violation Experiments and Electroweak Gauge Theory

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In recent years a unified strategy in dealing with constructivism has been emerging in the writings of historians and philosophers of science. In my own experience, the strategy is exemplified in the long critiques of all or parts of my book, *Constructing Quarks* (*CQ*), set out by Paul Roth, Peter Galison and Allan Franklin. These critiques have two common features. First, the substance of constructivist claims is more or less ignored, in favour a fictional version that simply asserts the opposite of what the critic wants to affirm, which is, second, that the evolution of science should be grasped in terms of some relatively simple and unsituated concept of 'reason' (or 'logic' or 'persuasive argument').[1] Allan Franklin's discussion of the history of parity-violation experiments in atomic and high-energy physics exemplifies both of these features.[2] Concerning the first, the position he attributes to *CQ* is summarised as a pure negative: Pickering, he says, 'obviously doubts that science is a reasonable enterprise based on valid experimental or observational evidence' (165).[3] This negative is set up to lead into its inverse, which is Franklin's own position: that science is a reasonable enterprise based on valid etc etc. I resent Franklin's gloss of my analysis of science, as I resent Roth's and Galison's, but I largely let that pass here. My main concern is with Franklin's conception of 'reason' and what that concept can accomplish in helping us understand science. I think it is important to set out my position on this at least once in my life, because I believe that ideas like Franklin's are actually the main impediment to understanding what constructivism amounts to, what it claims, and what problems it addresses. Until the weaknesses of positions like his are exposed, there is, I think, little hope of useful dialogue between constructivists and large sections of the history and philosophy of science community.

The thrust of my argument is simple. Whatever the image that Franklin's rhetoric might conjure up in the minds of his audience, it is untrue that I deny that science is a reasonable enterprise, or that evidence has a constitutive role to play in the production of scientific knowledge. The problem I see in Franklin's understanding of science is rather that there are *too many reasons* to be found in science, and that these reasons point in all sorts of directions. They cannot therefore be understood as unproblematic explanations of why science proceeds historically in one direction rather than another. Rather than arguing this assertion in the abstract, I want to discuss the specific passage in the history of particle physics that Franklin takes to establish his own position,

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but first I need to clarify one aspect of that position.[4] Franklin campaigns under the banner of what he calls the 'evidence model', but there are actually two threads to his argument. He wants to suggest, first, that there is some especially 'reasonable' and theory-independent way of extracting 'reliable' empirical conclusions from a confused field of evidence. And, once such an extraction has been made, he wants to suggest that certain implications follow for theory-choice — implications that are caught up in his 'evidence model'. I take these threads in turn.[5]

1. What is the evidence?

The case in dispute concerns the discovery of parity-violating effects in electron-hadron interactions. It is not disputed that conflicting evidence on this topic was offered by experimentalists in the period 1976 to 1981, evidence that came from a range of bench-top atomic-physics experiments and one high-energy physics experiment, experiment E122, performed at the Stanford Linear Accelerator Center (SLAC). The question is, how did the scientific community make sense of this field of evidence? Franklin presents his own way of making sense of it, which he regards as especially reasonable. My feeling is, though, that there were any number of reasonable ways of making sense of the data, none of which can be especially singled out as better than the others. Since my strategy is simply to make a non-exhaustive list of reasonable alternatives, I rehearse Franklin's way first and number it '1'.

1) Franklin notes that the atomic-physics experiments in question were very difficult, were plagued with interpretative and systematic errors and were incapable of agreeing with one another: some reported the expected parity-violating effects predicted by the Weinberg-Salam (WS) model, others reported that any such effects were much smaller than the model predicted, if they existed at all. Franklin's suggestion is that the most reasonable stance towards these experiments as a class is to forget about them, to regard the results pro and con the WS model as, in effect, neutralising one another. He then argues that the remaining high-energy physics experiment, E122 at SLAC, was, instead, trustworthy, and therefore it was reasonable to accept the findings of E122, namely that parity violation in electron-hadron interactions does indeed occur at the rate predicted by the WS model. As he puts it, parodying a passage from *CQ*: 'Scientists *chose*, on the basis of reliable experimental evidence provided by the SLAC E122 experiment, to accept the Weinberg-Salam theory. They *chose* to leave an apparent, but also quite uncertain, anomaly in the atomic parity violation experiments for future investigation' (192).

My first comment on this phase of Franklin's essay is that it is hard to see that anything especially philosophical is at stake. Franklin, it seems to me, simply offers us an artful reading of the scientific publications designed to lead up to the conclusion he wants to reach. If there is some system behind his commentary, he does not make it explicit. However, my intention is not to argue with him about such niceties, so I move on to my second comment. It is that Franklin's way of evaluating the evidence in question seems to me quite reasonable. I cannot, in the abstract, see anything wrong with reasoning about this confused field of data as he does. What I dispute about Franklin's reasonableness, though, is its uniqueness. To make this point, I continue with a list of some other ways of thinking about the same data that also appear reasonable.

2) Franklin asserts, in the passage quoted above, that the physics community in fact reasoned as he does about the evidence on parity violation, but this is not quite correct.[6] He is right to say that E122 decided the issue for most physicists, but he is wrong if he imagines that this was accompanied by a reasoned judgement that the

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atomic-physics experiments neutralised one another. Rather, the presumption within the physics community was that something was wrong with just those atomic-physics experiments that failed to detect the parity violation that E122 had found. Thus, in what I think was the first major review talk to follow the announcement of E122's results, the reviewer discounted the negative findings on parity-violation (from groups working at the Universities of Oxford and Washington) while including the positive findings (from groups at Novosibirsk and Berkeley) in his calculations of the phenomenological parameters describing the electroweak interaction. Oddly enough, Franklin discusses this review in his essay (quoting from *CQ*), as he does another authoritative review talk from a workshop on neutral-current interactions in atoms held two months later. There, as Franklin notes, the reviewer concluded: '*As a conclusion on this bismuth session, one can say that parity violation has been observed roughly with the magnitude predicted by the Weinberg-Salam model*' (173). Just to be clear, let me state that the bismuth experiments in question were the atomic-physics experiments performed at Oxford, Washington and Novosibirsk, and that the first two arrived at null results; only the Novosibirsk group had data consistent with the predictions of the WS model.[7]

So, it seems reasonable to say that the physics community did not reason quite as Franklin does, reasonable though his way of reasoning is. Where Franklin feels that the atomic-physics experiments neutralised one another, the physicists themselves excluded from their calculations those experiments that produced evidence against the WS model, while including the experiments that went along with the model. Should we therefore say that the physics community reasoned unreasonably? I don't think so. What I do think is that to see that their reasoning was reasonable one has to recognise that a kind of dichotomous logic was at work: either the WS model was right about parity violation or it was wrong. If it was right, as indicated by E122, then those atomic-physics experiments that failed to find parity violation had somehow to be in error and should not be taken into account. Again, this reasoning seems quite defensible to me. What seems less defensible is the artful way in which Franklin in his essay glosses over the gap, already evident there, between his own reasoning and that of the physics community. He has to do this, though, because pointing to the dichotomous logic at work in the actual reasoning of the physics community draws attention to the importance of theoretical context in this instance of scientists' reasoning about evidence. Back to this in a moment; first, some more reasons.

3) A lot hinges upon E122 in Franklin's reasoning, as it did in the physics community's. No-one disputes that E122's findings just about settled matters. Does that mean that the performance and interpretation of E122 was itself beyond dispute? As far as Franklin is concerned, the answer is yes, and he gives his reasons for thinking so. He recites at great length the checks that the SLAC experimenters gave as reasons for believing that their parity-violating signal was genuine and not an artefact of their apparatus (176-80). I agree with Franklin that such checking is important for scientists to do, and for science-studies to think about. I agree that it has a constitutive role in scientists persuasion of themselves and others of the reliability of their findings. It is quite reasonable to feel the force of arguments based upon such checks. But it is at this point that I start to feel that Franklin's rhetoric is somewhat disingenuous. I note, for example, that the atomic-physics experimenters, even those who reported that parity-violation did not exist, did checks too — though Franklin makes no mention of them in his essay.[8] If checking is all that is at stake, I wonder whether there is any special reason for trusting E122 and putting it in a special category all by itself. Why wouldn't it be reasonable to lump E122 together with the atomic-physics experiments and let them all neutralise one another? Actually, I think it would be reasonable to proceed thus, and hence to conclude that there was no reliable data to be had on parity-violation in the

period under consideration. And to reinforce this conclusion — to make it seem even more reasonable—I can point out just how anomalous an experiment E122 was in the history of particle physics and in the history of science in general.

Experiment E122 was performed just once and then disassembled. No experiment like it has been performed since, and no experiment like it seems likely ever to be performed. Now, twelve years later, the findings of E122 stand as the sole record of parity-violation in high-energy electron scattering. As I pointed out in *CQ*, this is a situation as far removed as can be from the standard philosophical paradigm of intersubjectively replicated evidence. If I suggested that this in itself were sufficient grounds for reasonable doubt about the findings of E122, would I just be playing a philosopher's game with no relevance to the real world? Here I can borrow some more rhetoric from Franklin's paper. He concludes with some moral tales concerning theoretical presuppositions which insinuated themselves into famous experiments. Fortunately, he says, 'the importance of the experiments led to many repetitions and to the correction of these early results' (1988, p. 28). Where does that leave E122? If I were a physicist who in 1978 had said that I preferred to wait for E122 to be replicated before making up my mind up parity violation, would I have been acting unreasonably? I think not.[9]

4) Suppose that despite qualms concerning the replicability of the findings of E122 one were inclined to accept them. And suppose further one were sufficiently impressed by the competence of Patrick Sandars, the leader of the Oxford atomic-physics collaboration, to accept their null-result at face value. One might, for example, have been so intimidated by Sandars in the undergraduate teaching laboratory that one decided to become a theoretical physicist — as I was and did. Having become such a theorist, would it then be unreasonable to try to find some variant of electroweak gauge theory that could reconcile the null-result of the Oxford and Washington atomic-physics experiments with the positive findings of E122? Again, I think this course of action would be reasonable, and could be made to seem even more so by, for example, noting the very large error bars on the pro-WS Berkeley data, and by undermining the pro-WS Novosibirsk data by pointing to the dubious track record of Soviet physicists in bench-top experiments that attempt to address topics of interest in particle physics (on the rest masses of neutrinos, for example). Franklin writes as if the possibility of devising such alternative models had been ruled out once and for all by experiment E122 (180). But though it is true that E122 analysed their data in a way that displayed the improbability of a particular class of variant gauge theories, the so-called 'hybrid models', I do not believe that it would have been impossible to devise yet more variants.[10]

So, I have listed four quite different ways of reasoning about the evidence available on electron-hadron parity violation in the late 1970s: Franklin's way, that discounts all the findings of the atomic-physics experiments; the physicists' way, that discounted only those atomic-physics experiments that disagreed with the WS model; a line that challenges E122 on the grounds of replicability; and a line that credits both E122 and the Washington-Oxford atomic-physics experiments and tries to reconcile them. As I said before, this list is not exhaustive — it is easy enough to think of yet more ways of reasoning about the data — but it is enough, I think, to call into question Franklin's suggestion that there is some uniquely reasonable way of figuring out what Nature was trying to tell physicists in this instance. And thus, as I see it, an explanatory problem remains open — that of understanding why, from my indefinite list, physicists in fact took the second option, crediting the pro-WS data as they did. One can exclaim, 'but it's reasonable!' until one is blue in the face, but the problem remains.[11]

One last point in this connection. It requires no great powers of the imagination to construct the list of reasonable readings of the data that I have just given. One question that arises in my mind is therefore that of why Franklin and the other critics of constructivism are so obsessed with the first option. The answer is, I think, a moral one. The second and fourth items on my list (though not the third) implicate theory in a straightforward way in the assessment of evidence; and Franklin *et al* seem to have a moral conviction residing at a level *beyond reason* that such implication of theory is a bad thing. Evidently I do not share their morality, but I would make two comments concerning it. First, it would perhaps be possible to devise a system of institutional arrangements in which the moral purity of the empirical base of science could be maintained as Franklin desires. The products of that system would not, however, be the same as those of what we presently call science. Mainstream quantitative US sociology might emerge pretty much unscathed, but a lot of modern physics would be ruled unscientific. Second, in accordance with the inversion strategy I mentioned at the beginning, Franklin and his fellow moralists portray the constructivist enemy as their own opposite, the veritable Anti-Christ of epistemology. If morality requires the purity of the empirical base, then constructivists are accused of insisting on its desecration. If we suggest that evidence is not everything, then it must be nothing; all evidence is just an imposition of theoretical prejudice. We are the dreaded 'theory-firsters' in Peter Galison's forgettable phrase. Against this I remark that my list of reasonable ways of proceeding includes items 1 and 3 as well as 2 and 4. If scientists behave like 'theory-firsters', as particle physicists increasingly did in the period covered by *CQ*, don't shoot the messenger.

2. The evidence model?

The first thread of Franklin's argument was intended to establish a pure realm of evidence for science, in preparation for the second thread concerning theory-choice and the 'evidence model'. I have already suggested that the clean split between assessing evidence and choosing theories is hard to maintain in the instance under dispute. As I argued in *CQ*, it seems clear that the physics community evaluated evidence and made their choice of theory both at the same time, quite reasonably, as described under the second item on my list. Thus, I suspect that parity violation is not a very perspicuous choice of example for advancing the Franklinian cause. But I want to make things difficult for myself in what follows, by pretending that the experiments straightforwardly supported the predictions of the Weinberg-Salam model. My reason for doing so is to highlight some further shortcomings of Franklin's 'evidence model'.

What is the 'evidence model'? 'The evidence model', says Franklin, 'explains adherence to scientific beliefs in terms of their relationship to valid experimental evidence' (162). Thus, in the present instance, and forgetting about the problem of deciding what the 'valid' evidence was, it was reasonable to adhere to belief in the WS model, since the predictions of the model stood in a relation of agreement to the evidence. Now, as usual, I am happy to assent to this assertion, it seems reasonable enough to me. But still I feel that something fishy is going on here, which I can best summarise by saying that Franklin's 'evidence model' *affects to explain much more than it actually does*. The key question to consider is: what follows from the evidence model? what are the implications for future practice of reasonable belief grounded in evidence? In the case in point, after E122 physicists took the WS model for granted as a trustworthy description of the world of electroweak phenomena and largely ceased to explore alternative possibilities. So perhaps this is what Franklin understands as the reasonable behaviour that follows from the 'evidence model'. It sounds reasonable to me. But it is important to recognise that as a general prescription for scientific practice, this understanding of the 'evidence model' is a recipe for vicious conservatism.

To emphasise this point, I turn to what appears to be the knockout punch at the end of Franklin's paper. 'If the social constructivist view were correct', he says, 'then one would expect to find at least one episode in which the decision of the scientific community went against the weight of the experimental evidence. No such episode has been provided' (this volume).[12] I think there is something desperately wrong with this formulation. I can think of many examples that run counter to it. Consider the two great theoretical conjectures that run through the history of what I call the 'new physics' of elementary particles — the quark model and gauge theory. The quark model postulated the existence of particles carrying third-integral electric charges which were known not to exist from half-a-century's worth of experimentation, and the electroweak gauge theory as laid out by Salam and Weinberg in 1967 immediately predicted the existence of weak neutral currents which were known not to exist from many observations of K-decays and neutrino interactions. It was crucial to the history of modern particle physics that both of these models were initially elaborated in the face of, not with the support of, the available evidence.

How could Franklin respond to this observation? He could start, I suppose, by defending the letter of the passage quoted above and insisting that 'the scientific community' did not accept the quark or Weinberg-Salam models in their early years; only certain subsections of the community jumped on the relevant bandwagons. But then, what might one say about those subsections? Here Franklin's closing remark — 'that scientists, being human, are fallible and do not always behave as they ought to, should surprise no one' (this volume) — might come into play. The founders of the theoretical wing of the new physics — including heavyweights like Nobel laureates Gell-Mann, Weinberg and Salam — were indeed human, and in this instance they were exercising their prerogative and acting unreasonably. But I doubt whether the most rabid empiricist would want to go that far. Franklin's proper response to these important episodes would be, I imagine, that these laureates and their followers did have reasons for elaborating theories that appeared to be false. But what might these reasons be? Presumably either that the evidence that made the theories false was itself suspect — in which case we are back where we started, with my observation that there is more than one reasonable way of reasoning about a field of evidence — or that there is a further category of reasons that bear upon theoretical practice that just escapes the 'evidence model'. Either way, my point is established: there are just too many reasons around for reason to stand as an explanation of the development of science. My conclusion concerning Franklin's 'evidence model' is therefore either that it implies the kind of conservatism that would have ruled out the development of the quark model and gauge theory — and thus, incidentally, would have made the historical episode under discussion unthinkable — or that it is toothless as explanation: it explains utterances of belief in very special circumstances and nothing else.[13]

3. The trouble with reason

So far, I have been querying Franklin's way of thinking about science on its own terms. This is an exercise that is necessary from time to time. But I want to close by stepping outside Franklin's chosen frame. I offer three brief statements to indicate what I see as general problems of Franklin-type analyses of reason.

- Franklin's argument about the episode under discussion has enough plausibility to make it worth disputing. That would not have been the case if the topic had been, say, the development of QCD, the gauge theory of the strong interaction. There the relation between theory and experiment was so different from the theory-testing paradigm of traditional philosophy that Franklin's evidence model could have gained no purchase whatsoever (Pickering 1984, pp. 309-46). From this one could

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conclude that the development of QCD was itself unreasonable, or, as I do, that the theory-testing paradigm is in general a misleading starting point for thinking about science.

- Franklin's model of the scientist is that of a static reasoner, a weigher of evidence and a comparer of evidence and predictions. I do not deny that scientists engage in such practices, but I do deny that such a model can take us very far in understanding science. The model is just too thin. Most importantly it conceals the temporal dimension of scientific practice, the fact that scientists live not just in the present but in the past and future as well — in a field of goals and histories. This was what I tried to grasp and analyse in *CQ* in my model of what I called the dynamics of practice — although you would never guess it from Franklin's critique (or Galison's). I do not claim to have said the last word on this subject then or since, but I do continue to claim that attention to the temporality of practice is necessary if one wants to understand why, in the midst of the proliferation of reasons, science develops as it does.[14]

- An exclusive focus on scientific reasoning contributes to the strange blindness of traditional philosophy to the material dimension of science. In *CQ* I remarked upon the gross shift in the material practices of experimenters that was part and parcel of the establishment of QCD (1984, 347-82), and the more subtle shifts that accompanied the development of electroweak gauge theory, including those surrounding the establishment of parity violation in electron-hadron interactions.[15] I concluded that scientists had to learn proper ways of conducting themselves in the material world at the same time as they learned how to think about it. I continue to find this a striking and important observation, though, of course, it can find no expression in a philosophical discourse organised just around reason. For me, this indicates that we need a new philosophy (Pickering 1990).

Notes

[1]Roth and Barrett (1990), Galison (1987); for my replies see Pickering (1990, forthcoming a). For a similar critique of constructivism more generally, see Giere (1988) and for my critique of the critique, Pickering (forthcoming b).

[2]To avoid confusion, I should explain that in 1988 Allan Franklin invited me to participate in a 1990 PSA symposium organised around his account of the parity-violation experiments (Franklin 1988). That account subsequently appeared as Chapter 8 of his book (1990) though with certain changes (none of any substance as far as my arguments are concerned). A highly abridged version of Franklin's argument appears in this volume. My essay was written as a response to Franklin (1988) but, except where indicated, page number citations in what follows are to the book (1990). Pickering (1984, pp. 290-302) is my account of the historical developments presently under discussion.

[3]This formulation appears at the end of a sequence of glosses and translations which begins with quotations from Trevor Pinch and continues via references to 'interests' (162-3) — an analytical concept that I have self-consciously abstained from using since around 1980.

[4]Two further lines of argumentation also support my conclusion that there are too many reasons in science. The first relates to my own experience in exploring the history of particle physics. In the course of that research I have met and often interviewed and collected documentation from many 'deviant' scientists — scientists who reject, say, all or parts of the present gauge-theory orthodoxy. These deviants prove to have better worked out substantive and philosophical reasons for their heretical attitudes (which differ widely from one to the other) than the proponents of the orthodoxy. Secondly, there is now a pretty extensive literature on the analysis of scientists' discourse and argumentation. The image of the scientific actor that emerges from this literature is that of a person artfully constructing reasons for particular purposes, and not of a Franklinian automaton ruled by reasons beyond her control (Gilbert and Mulkay 1984). It seems to me that both Franklin's present essay and the atomic-physics publications discussed below would be extremely fruitful sites for the study and documentation of situated reason-giving.

[5]This is an appropriate point to confess that Franklin has found a substantive error in *CQ*'s account of the experiments in question: he is right that the 1977 publications from Washington and Oxford made no reference to hybrid unified electroweak models, though, as he concedes, such models were discussed in the literature of the time (169). On another putative error, I think the mistake is Franklin's. When I stated in *CQ* that 'The details of the [Novosibirsk atomic-physics experiment] were not known to Western physicists', it is clear from the context that I was referring to the period up to and including the review talk by F. Dydak given at the European Physical Society Conference, 27 June-4 July 1979. My statement is thus not refuted, as Franklin supposes (171), by the presence of the Soviet physicists at the meeting held in Cargèse, 10-14 September 1979. My other supposed errors are dealt with in the text and notes below.

[6]Also: 'The physics community chose to await further developments in the atomic parity violating experiments, which, as I have shown, were uncertain' (180).

[7]Franklin also mentions a third review talk — Commins and Bucksbaum (1980) — and states that 'They [Commins and Bucksbaum] regarded the situation with regard to the bismuth results as unresolved' (p. 173, note 16). In fact, that review treated the Washington and Oxford experiments as having no data at all, while including the positive findings from Novosibirsk and Berkeley in its calculations of the parameters of the electroweak interaction and concluding that these parameters were in 'very satisfactory agreement' with the WS model (Commins and Bucksbaum, pp. 38-41, 48-51, quotation at 51).

[8]Thus, for example, Franklin dwells on the systematic errors reported in the 1977 account of the Oxford atomic-physics experiment (169), but makes no mention of the passage immediately following where the experimenters discuss their procedure for getting round these errors: they randomly interspersed their measurements on bismuth vapour with measurements on a dummy tube not containing bismuth (Baird *et al.* 1977, 800). The rhetorical potential of the artful pruning of quotations is nicely brought out in Ashmore (1988, 138-9).

[9]As Franklin puts, 'Unlike statistical errors, which can be calculated precisely, systematic errors are both extremely difficult to detect and to estimate' (191). How fortunate there were none of the latter in E122. As a speculation, I offer the thought that the uncritical reception of E122 may have depended somewhat on the 'politics of experiment'. Parity violation was the last of a series of major discoveries made at

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SLAC during the 1970s; to challenge E122 would have been to challenge the credibility of the laboratory itself (and, of course, to challenge both gratuitously, since there was no other accelerator at which comparable measurements could be made). Possibly connected with this speculation is the fact that the early reports of the findings in E122 in the scientific press credit the experiment to Richard Taylor, while the leader and moving force of the E122 collaboration was actually Charles Prescott (see, for example, *Times* (1978), *New Scientist* (1978), Walgate (1978) and *Physics Bulletin* (1978); on Prescott's role, see Pickering (1984, 298-9). Taylor was the experimenter who had led the collaboration responsible for the discovery of scaling at SLAC in the late 1960s (Pickering 1984, 127-31).

[10]Open-ended recipes for the construction of variations on the electroweak theme had first been written down in 1972: see Pickering (1984, 181).

[11]At various points in his essay, Franklin writes as if the issue between us concerns 'evidential weight' rather than 'reason'. Thus, 'The issue seems to turn on the relative evidential weight one assigns to the original Oxford and Washington atomic physics results and to the SLAC E122 experiment . . . Pickering seems to regard them as having equal weight. I *do not*' (174), '*Pickering claims that the decision of theory choice* excluded evidence, of equal weight, that argued against that choice' (1988, p. 27), 'I believe he [Pickering] made an incorrect judgement on the relative evidential weight of the two different experiments' (1988, p. 29, note 3). It is understandable that a Bayesian might want to speak of 'evidential weight' as if it could be read off the surface of a published text (see Franklin 1986, Ch. 4), but my remarks here on 'reason' can serve equally well to demonstrate the problematic nature of 'evidential weight'.

[12]In this connection Franklin mentions studies of the discovery of the weak neutral current by myself and Peter Galison, before stating without argument that he believes Galison's account 'supports the evidence model' and is 'more persuasive' (164). The interested reader might consult the works cited by myself and Galison, as well as Pickering (1989).

[13]Note that this conclusion applies to all static articulations of 'reason' (see below), including Bayesianism, and not just to the particular version of the 'evidence model' that is proposed in Franklin's present essay.

[14]For more recent discussions of my understanding of the dynamics of scientific practice, see Pickering (1990, forthcoming a). Pickering (1990) contains a discussion of how the static dimension of scientific reasoning can integrated with an understanding of the dynamics of practice.

[15]In this connection, certain developments concerning the Washington atomic-physics experiment might repay detailed attention, though I failed to mention them in *CQ*. As Franklin notes, the Washington group published new data in 1981 that agreed with the predictions of the WS model. There they included a table listing the different experimental runs that they had performed since their earliest experiments on bismuth vapour (Hollister *et al.* 1981, p. 645, Table I). It is interesting to note that these runs were categorised by the kind of laser used. The earliest run had used a parametric oscillator (this resulted in the 1976 publication reporting a null-result) as had the second run (which resulted in an even tighter upper limit on the extent of parity violation, as reported in their 1977 publication). The group then switched to using a gallium-aluminium-arsenide laser diode: they first used a transverse-junction-stripe diode, which led to measurements confirming the null-results of the 1977 publication and which were reported in an unpublished PhD thesis; then they worked with two other stripe

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diodes and a channeled-substrate-planar diode and obtained results consistent with the WS model. The results from the positive experiments (with the second and third stripe diodes and with the planar diode) were averaged to give the stated result of the 1981 paper. There is, then, a *prima facie* case for thinking of the history of this experiment as being that of the tuning of experimental techniques to the production of credible phenomena: the experimenters were finding out what kind of laser to use, in a very particular sense, in the course of their material and interpretative practice.

Although not strictly relevant, I cannot resist two further remarks concerning the Washington experiment. First, I note that gallium arsenide was also the source of polarised electrons for experiment E122. Second, though the Washington group never remarked upon it, the measured positive effect reported in the 1981 publication was actually within the quoted experimental error of their 1976 null-result (they quote values of a quantity 10^8R stated to be -10.4 ± 1.7 and -8 ± 3 respectively; their Table I, which I am reading here, is a masterpiece of obfuscation). What turned a null-result into a confirmation of theory was that calculations of the expected effect had decreased by a factor of two between 1976 and 1981.

References

Ashmore, M. (1988), "The Life and Opinions of a Replication Claim: Reflexivity and Symmetry in the Sociology of Scientific Knowledge", in *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge*, S. Woolgar (ed.). Beverly Hills and London: Sage, pp. 125-54.

Baird, P.E.G. *et al.* (1977), "Search for Parity-Nonconserving Optical Rotation in Atomic Bismuth", *Physical Review Letters* 39: 798-801.

Commins, E. and Bucksbaum, P. (1980), "The Parity Non-Conserving Electron-Nucleon Interaction", *Annual Reviews of Nuclear and Particle Science* 30: 1-52.

Franklin, A. (1986), *The Neglect of Experiment*. Cambridge: Cambridge University Press.

______. (1988), "The Way Mutants Meet Their Deaths: The Case of Atomic Parity Violation Experiments". Unpublished draft, University of Colorado, Boulder, dated 11 Feb. 1988.

______. (1990), *Experiment, Right or Wrong*. Cambridge: Cambridge University Press.

______. (this volume), "Do Mutants Have to be Slain, or Do They Die of Natural Causes? The Case of Atomic Parity Violation Experiments".

Galison, P. (1987), *How Experiments End*. Chicago: University of Chicago Press.

Giere, R.N. (1988), *Explaining Science: A Cognitive Approach*. Chicago: University of Chicago Press.

Gilbert, G.N. and Mulkay, M. (1984), *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*. Cambridge: Cambridge University Press.

Hollister, J.H. *et al.* (1981), "Measurement of Parity Nonconservation in Atomic Bismuth", *Physical Review Letters* 46: 643-6.

New Scientist (1978), "Major Boost for Unified Theory", 22 June, p. 824.

Physics Bulletin (1978), "Left at Last?", 29: 396.

Pickering, A. (1984), *Constructing Quarks: A Sociological History of Particle Physics*. Chicago and Edinburgh: University of Chicago Press/Edinburgh University Press.

________. (1989), "Editing and Epistemology: Three Accounts of the Discovery of the Weak Neutral Current", in *Knowledge and Society: Studies in the Sociology of Science, Past and Present*, Vol. 8, L. Hargens, R. A. Jones and A. Pickering (eds). Greenwich, CT: JAI Press, pp. 217-232.

________. (1990), "Knowledge, Practice and Mere Construction", *Social Studies of Science* 20: 682-729.

________. (forthcoming a), "Beyond Constraint: The Temporality of Practice and the Historicity of Knowledge", to appear in *Philosophical and Historiographic Problems about Small-Scale Experiments*, J. Buchwald (ed.).

________. (forthcoming b), "Philosophy Naturalized a Bit", to appear in *Social Studies of Science* 21(3).

Roth, P.A. and Barrett, R.B. (1990), "Deconstructing Quarks: Rethinking Sociological Constructions of Science", *Social Studies of Science* 20: 579-632.

Times (1978), "Physics: Confirmation of Unified Theory", 16 June.

Walgate, R. (1978), "Success for Unified Field Theory", *Nature* 273: 584.

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