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Contents

Review Essay

Experimental Questions

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It has been slightly more than ten years since Ian Hacking asked, "Do we see through a microscope?" (1981) and started the process of redressing the balance between experiment and theory in the history, philosophy, and sociology of science. Prior to Hacking's pioneering work, the study of science was theory dominated. It was so much so that, in Kuhn's history of the origins of quantum mechanics, *Black-Body Theory and the Quantum Discontinuity* (1978), there is almost no mention of the experiments on black-body radiation that showed the discrepancy between theory and experiment and led to Planck's introduction of the quantum of action. The experimenters, Lummer, Pringsheim, Rubens, and Kurlbaum, are peripheral figures. There is no discussion of what the experiments were or how the data were acquired. One never sees a graph showing the discrepancy between the

This essay will survey some of the most important work on experiment done during the last decade. I will concentrate on the new questions asked and the answers proposed. I will also briefly comment on several of the important books and papers written on the subject during this period. The books will include Robert Ackermann, *Data, Instruments, and Theory* (1985); Peter Galison, *How Experiments End* (1987); David Gooding, Trevor Pinch, and Simon Schaffer, eds., *The Uses of Experiment* (1989); Ian Hacking, *Representing and Intervening* (1983); Andrew Pickering (ed.), *Science as Practice and Culture* (1992); and Trevor Pinch, *Confronting Nature* (1986). This is not intended to be a comprehensive survey, but rather one that gives my own view of the important work that has been done. I should emphasize that I am not a disinterested commentator, but rather an active participant, with my own rather strong views. Part of this work was supported by a faculty fellowship and grant-in-aid from the Council on Research and Creative Work, Graduate School, University of Colorado, and I thank the council for its support. This material is based on work partially supported by the National Science Foundation under grant no. DIR-9024819. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the author and do not necessarily reflect the views of the National Science Foundation.

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experimental results and the theoretical prediction. The title of the book indicates what Kuhn thinks is most important—theory.

Theory also dominated on the philosophical side. Popper, for example, states, "Theory dominates experimental work from its initial planning up to the finishing touches in the laboratory" (1959, p. 107). One of the interesting points made in recent work is that an experiment does not end when the data have been acquired. Considerable analysis is required before data are considered to be an experimental result (Bogen and Woodward 1988). Even those philosophers who gave experiment some significance seemed to have a repertoire of only three historical examples: Galileo and the Leaning Tower, Young's interference experiment, and the Michelson-Morley experiment. Even in these cases, the accounts of the experiments seemed to be more mythical than actual.

Hacking offers another interesting example of theory dominance. He notes that Penzias and Wilson discovered the uniform 3K background radiation while working on antennas and radiotelescopes. Although they were unaware of it when they performed their experiments, this meshed quite nicely with contemporaneous theoretical work that such radiation was produced by the Big Bang. Yet in subsequent history of the episode theory dominates: "Radioastronomers believed that if they could aim a very sensitive receiver at a blank part of the sky, a region that appeared to be empty, it might be possible to determine whether or not the theorists were correct" (Branley 1979, p. 100).

Since that beginning we have realized that there is no simple answer to Hacking's seemingly simple question. We have also found a host of related questions concerning experiment and its roles, as well as a wide variety of answers proposed to those questions. Some of these questions are: (1) How do scientists decide that an experiment is completed and report the result? (2) Do the presuppositions of scientists affect the results they report and, thereby, bias decisions concerning theory? (3) Do reasons other than experimental evidence enter into the evaluation of theory? (4) Does experiment have a life of its own, independent of theory? (5) What is the relation between theory and experimental evidence? (6) What is the status of theoretical entities contained in a well-confirmed scientific theory?[1] and (7) Do major changes in theory and experiment occur at the same time? Others would, no doubt, offer a somewhat different list of new questions

1. This is a rather old question, but new answers have been proposed since Hacking's original work.

concerning experiment, but it is clear that there are both new questions and new answers.

1. Experimental Results

Hacking's original question really asked how do we come to believe in an experimental result obtained with a complex experimental apparatus? How do we distinguish a valid result from an artifact created by that apparatus? He provided an extended answer in the second half, the intervening section, of *Representing and Intervening* (1983). He pointed out that, even though an experimental apparatus is laden with, at the very least, the theory of the apparatus, observations remain robust despite changes in the theory of the apparatus. His illustration is the continuous belief in microscope images despite the major change in the theory of the microscope when Abbe pointed out the importance of diffraction in its operation. One reason Hacking gave for this is that in making such observations the experimenters intervened. They manipulated the object under observation. Thus, in looking at a cell through a microscope, one might inject fluid or stain the specimen. One expects the cell to change shape or color when this is done. Observing the predicted effect strengthens our belief both in the proper operation of the microscope and in the observation. This is true in general. Observing the predicted effect of an intervention strengthens our belief both in the proper operation of the experimental apparatus and in the observations made with it.

Hacking also discussed the strengthening of one's belief in an observation by independent confirmation. The fact that the same pattern of dots is seen with "different" microscopes, that is, ordinary, polarizing, phase contrast, fluorescence, interference, electron, acoustic, and so forth, argues for the validity of the observation. One might question whether or not "different" is theory laden. After all, it is our theory of light and the microscope that allows us to consider these microscopes "different." Nevertheless, the argument goes through. Hacking correctly argues that it would be a preposterous coincidence if the same pattern of dots were produced in two totally different kinds of physical systems. Different apparatuses have different backgrounds and systematic errors, making the coincidence, if it is an artifact, most unlikely. If it is a correct result, and the instruments are working properly, the coincidence of results is understandable.

Hacking's answer is correct as far as it goes. It is, however, incomplete. What happens when one can perform the experiment with only one type of apparatus, such as an electron microscope or a radiotelescope, or when intervention is either impossible or extremely difficult?

Other strategies are needed to validate the observation. These may include (1) experimental checks and calibration, in which the experimental apparatus reproduces known phenomena; (2) reproducing artifacts that are known in advance to be present; (3) elimination of plausible sources of error and alternative explanations of the result (the Sherlock Holmes strategy); (4) using the results themselves to argue for their validity; (5) using an independently well-corroborated theory of the phenomena to explain the results; (6) using an apparatus based on a well-corroborated theory; and (7) using statistical arguments.[2] One should emphasize here that these strategies provide good reasons for belief in experimental results but do not guarantee that the results are correct. Experiment is fallible.

In *How Experiments End* (1987), Peter Galison extended the discussion of experiment to more complex situations. In his histories of the measurements of the gyromagnetic ratio of the electron, of the discovery of the muon, and of the discovery of weak-neutral currents, he considered a series of experiments measuring a single quantity, a set of different experiments culminating in a discovery, and two high-energy physics experiments done by large groups, with complex experimental apparatus.

Galison's view is that experiments end when the experimenters believe that they have a result that will stand up in court. A result that I believe will include, and has included, the use of the epistemological strategies discussed earlier. Thus, David Cline, one of the weak-neutral current experimenters, remarked, "At present I don't see how to make these effects [the weak-neutral current event candidates] go away" (p. 235). I note here that Cline had spent a good part of his career performing experiments that showed that weak-neutral currents do not exist. His comment is therefore extremely telling against those who think that career interests have a major influence on experimental results. His earlier experiments were not, in fact, wrong. They were done on strangeness-changing weak interactions, in which neutral currents do not occur, while the experiment under discussion involved strangeness-conserving interactions, in which they do. It was realized later that this was an important distinction.

Galison emphasizes that, within a large experimental group, different members of the group may find different pieces of evidence most convincing. Thus, in the Gargamelle weak-neutral current experiment,

2. See Franklin (1986, chap. 6; 1990, chap. 6) and Franklin and Howson (1984, 1988) for details of these strategies, along with a discussion of how they fit into a Bayesian philosophy of science.

several group members found the single photograph of a neutrino-electron scattering particularly important, while for others the difference in spatial distribution between the observed neutral current candidates and the neutron background was decisive. Galison attributes this, in large part, to differences in experimental traditions, in which scientists develop skill in using certain types of instruments or apparatus. In particle physics, for example, there is the tradition of visual detectors, such as the cloud chamber or the bubble chamber, in contrast to the electronic tradition of Geiger and scintillation counters and spark chambers. Scientists within the visual tradition tend to prefer "golden events" that clearly demonstrate the phenomenon in question, while those in the electronic tradition tend to find statistical arguments more persuasive and important than individual events.

Galison points out that major changes in theory and in experimental practice and instruments do not necessarily occur at the same time. This persistence of experimental results provides continuity across these conceptual changes. Thus, the experiments on the gyromagnetic ratio spanned classical electromagnetism, Bohr's old quantum theory, and the new quantum mechanics of Heisenberg and Schrödinger. Robert Ackermann has offered a similar view in his discussion of scientific instruments.

> The advantages of a scientific instrument are that it cannot change theories. Instruments embody theories, to be sure, or we wouldn't have any grasp of the significance of their operation. . . . Instruments create an invariant relationship between their operations and the world, at least when we abstract from the expertise involved in their correct use. When our theories change, we may conceive of the significance of the instrument and the world with which it is interacting differently, and the datum of an instrument may change in significance, but the datum can nonetheless stay the same and will typically be expected to do so. An instrument reads 2 when exposed to some phenomenon. After a change in theory,[3] it will continue to show the same reading, even though we may take the reading to be no

3. It might be useful here to distinguish between the theory of the apparatus and the theory of the phenomenon. Ackermann is talking primarily about the later. It may not always be possible to separate these two theories. The analysis of the data obtained from an instrument may very well involve the theory of the phenomenon, but that does not necessarily cast doubt on the validity of the experimental result.

> longer important, or to tell us something other than what we thought originally. [Ackermann 1985, p. 33]

Although Ackermann discusses the stability and robustness of results produced by instruments, he does not discuss in detail why one should believe in such results. For example, an instrument may very well produce incorrect results consistently.

Galison also discusses other aspects of the interaction between experiment and theory. Theory may influence what is considered to be a real effect, demanding explanation, and what is considered background. In the discovery-of-the-muon episode, he argues that the calculation of Oppenheimer and Carlson, which showed that showers were to be expected in the passage of electrons through matter, left the penetrating particles, later shown to be muons, as the problem. Prior to their work, physicists thought the showering particles were the problem, whereas the penetrating particles seemed to be understood.

The role of theory as an "enabling theory," one that allows calculation or estimation of the size of the expected effect and also the size of expected backgrounds, is also discussed by Galison. Such a theory can help to determine whether or not an experiment is feasible. He also emphasizes that elimination of background that might simulate or mask an effect is central to the experimental enterprise and not a peripheral activity. In the case of the weak-neutral current experiments the existence of the currents depended crucially on showing that the event candidates could not all be due to neutron background.[4]

There is also a danger that the design of an experiment may preclude observation of a phenomenon. Galison points out that the original design of one of the neutral current experiments, which included a muon trigger, would not have allowed the observation of neutral currents. In its original form the experiment was designed to observe charged currents, which produced a high-energy muon. Neutral currents do not. Therefore, having a muon trigger precluded their observation. Only after the theoretical importance of the search for neutral currents was emphasized to the experimenters was the trigger changed. Changing the design did not, of course, guarantee that neutral currents would be observed.

Galison also shows that the theoretical presuppositions of the experimenters may enter into the decision to end an experiment and re-

4. For another episode in which the elimination of background was crucial see my 1990 discussion of the measurement of the K^{+}_{e2} branching ratio (Franklin 1990, pp. 115–31).

port the result. Einstein and de Haas ended their search for systematic errors when their value for the gyromagnetic ratio of the electron, $g = 1$, agreed with their theoretical model of orbiting electrons. This effect of presuppositions might cause one to be skeptical of both experimental results and their role in theory evaluation. Galison's history shows, however, that, in this case, the importance of the measurement led to many repetitions of the measurement. This resulted in an agreed-on result that disagreed with theoretical expectations. Scientists do not always find what they are anticipating.

Pickering and others have raised objections to the view that experimental results are accepted on the basis of epistemological arguments. They point out that "a sufficiently determined critic can always find a reason to dispute any alleged 'result'" (MacKenzie 1989, p. 412). In this view the reasons offered for accepting results are the future utility of such results for both theoretical and experimental practice and the agreement of such results with the existing community commitments. In discussing the discovery of weak-neutral currents, Pickering states, "Quite simply, particle physicists accepted the existence of the neutral current because they could see how to ply their trade more profitably in a world in which the neutral current was real" (Pickering 1984*b*, p. 87). He further states that "scientific communities tend to reject data that conflict with group commitments and, obversely, to adjust their experimental techniques to tune in on phenomena consistent with those commitments" (Pickering 1981, p. 236).[5] The emphasis on future utility and existing commitments is clear. These two criteria do not necessarily agree. For example, there are episodes in the history of science in which more opportunity for future work is provided by the overthrow of existing theory (see, e.g., the history of the overthrow of parity conservation and of CP symmetry [Franklin 1986, chaps. 1, 3]).

Pickering has recently offered a different view of experimental results. In this view the material procedure including the experimental apparatus itself along with setting it up, running it, and monitoring its operation; the theoretical model of that apparatus; and the theoretical model of the phenomena under investigation are all plastic resources that the investigator brings into relations of mutual support (Pickering 1987, 1989): "Achieving such relations of mutual support is, I suggest, the defining characteristic of the successful experiment" (1987, p. 199). His example is Morpurgo's search for free quarks, or fractional charges

5. This has led Peter Galison to refer to such scholars as "theory firsters" (quoted in Pickering 1991, p. 463).

of $\pm 1/3\ e$ or $\pm 2/3\ e$, where e is the charge of the electron (see also Gooding 1992). Morpurgo used a modern Millikan-type apparatus and initially found a continuous distribution of charge values. Following some tinkering with the apparatus, Morpurgo found that if he separated the capacitor plates he obtained only integral values of charge: "After some theoretical analysis, Morpurgo concluded that he now had his apparatus working properly, and reported his failure to find any evidence for fractional charges" (Pickering 1987, p. 197).

Pickering has made the important point that experimental apparatuses rarely work properly when they are first operated, and that some adjustment, or tinkering, is required before it does. He has also correctly pointed out that the theory of the apparatus and the theory of the phenomena can, and do, form part of the argument for the validity of an experimental result. He has, however, overemphasized theory. It was known, from Millikan onward, that fractional charges, if they exist at all, are very rare in comparison with integral charges. The failure of Morpurgo's apparatus to find integral charges indicated quite strongly, despite his initial theoretical analysis, that it was not an accurate charge-measuring device. Only after tinkering, when the apparatus measured integral charges and thus passed a crucial experimental check, could one legitimately trust its measurements of charge. Although the modified theoretical analysis may have helped to clarify this, it was the experimental check that was crucial. There is more to an experimental apparatus than its theoretical analysis.

Ackermann has offered a modification of Pickering's view. He suggests that the experimental apparatus itself is a less plastic resource than either the theoretical model of the apparatus or that of the phenomenon: "To repeat, changes in A [the apparatus] can often be seen (in real time, without waiting for accommodation by B [the theoretical model of the apparatus]) as improvements, whereas 'improvements' in B don't begin to count unless A is actually altered and realizes the improvements conjectured. It's conceivable that this small asymmetry can account, ultimately, for large scale directions of scientific progress and for the objectivity and rationality of those directions" (Ackermann 1991, p. 456).

Hacking (1992) has also offered a more complex version of Pickering's later view. He suggests that the results of mature laboratory science achieve stability and are self-vindicating when the elements of laboratory science are brought into mutual consistency and support. These are (1) ideas: questions, background knowledge, systematic theory, topical hypotheses, and modeling of the apparatus; (2) things: target, source of modification, detectors, tools, and data generators; and

(3) marks and the manipulation of marks: data, data assessment, data reduction, data analysis, and interpretation. "Stable laboratory science arises when theories and laboratory equipment evolve in such a way that they match each other and are mutually self-vindicating" (1992, p. 56). "We invent devices that produce data and isolate or create phenomena, and a network of different levels of theory is true to these phenomena. Conversely we may in the end count them as phenomena only when the data can be interpreted by theory" (1992, pp. 57–58). One might ask whether or not such mutual adjustment between theory and experimental results can always be achieved? What happens when an experimental result is produced by an apparatus on which several of the epistemological strategies, discussed earlier, have been successfully applied, and the result is in disagreement with our theory of the phenomenon? One might also ask how Hacking's view that phenomena are accepted only when the data can be interpreted by theory can be reconciled with his view that experiment often has a life of its own. The careful reader might object that Hacking says only that data may be rejected if they cannot be interpreted by theory. A problem remains. Without some guidance as to when data should be accepted or rejected, Hacking's view seems to lack content.

Hacking himself worries about what happens when a laboratory science that is true to the phenomena generated in the laboratory, thanks to mutual adjustment and self-vindication, is successfully applied to the world outside the laboratory. Does this argue for the "truth" of the science? In Hacking's view it does not. If laboratory science does produce happy effects in the "untamed world, . . . it is not the truth of anything that causes or explains the happy effects" (p. 60).

Thus, there is a rather severe disagreement on the reasons for the acceptance of experimental results. For some, like Galison and myself, it is because of epistemological arguments. For others, like Pickering, the reasons are utility for future practice and agreement with existing theoretical commitments. Although the history of science shows that the overthrow of a well-accepted theory leads to an enormous amount of theoretical and experimental work, proponents of this view seem to accept it as unproblematical that it is always agreement with existing theory that has more future utility. Hacking and Pickering also suggest that experimental results are accepted on the basis of the mutual adjustment of elements, which includes the theory of the phenomenon.

Nevertheless, everyone seems to agree that a consensus does arise on experimental results. The question then is, How are these results used?

2. The Roles of Experiment

Although experiment often takes its importance from its relation to theory, Hacking pointed out that it often has a life of its own, independent of theory. He notes the pristine observations of Carolyn Herschel's discovery of comets and William Herschel's work on "radiant heat." He offers an interesting counterexample to Popper's view in Davy's observation of the gas emitted by algae and the flaring of a taper in this gas. Davy had no theory of the phenomenon. Similarly, one may note the nineteenth-century measurements of atomic spectra and the work on the masses and properties on elementary particles during the 1960s. Both of these sequences were conducted without any guiding theory.

In deciding what experimental investigation to pursue, scientists may very well be influenced by the equipment available and their own ability to use that equipment (See McKinney 1992; Franklin 1993*a*, 1993*b*). Thus, when the Mann-O'Neill collaboration was doing high-energy physics experiments at the Princeton-Pennsylvania Accelerator during the late 1960s, the sequence of experiments was (1) measurement of the K^+ decay rates, (2) measurement of the K^+_{e2} branching ratio, and (3) measurement of the form factor in K^+_{e3} decay. These experiments were performed with basically the same experimental apparatus, but with relatively minor modifications for each particular experiment. By the end of the sequence the experimenters had become quite expert in the use of the apparatus and knowledgeable about the backgrounds and experimental problems. We might refer to this as "instrumental loyalty" and the "recycling of expertise." This meshes nicely with Galison's view of experimental traditions. Scientists, both theorists and experimentalists, tend to pursue experiments and problems in which their training and expertise can be used.

Hacking also notes the "noteworthy observations" on Iceland Spar by Bartholin, on diffraction by Hooke and Grimaldi, and on the dispersion of light by Newton: "Now of course Bartholin, Grimaldi, Hooke, and Newton were not mindless empiricists without an 'idea' in their heads. They saw what they saw because they were curious, inquisitive, and reflective people. They were attempting to form theories. But in all these cases it is clear that the observations preceded any formulation of theory" (Hacking 1983, p. 156). (Contrast this with his view, discussed earlier, on the importance of theory in the interpretation of results.) In all of these cases we may say that these were observations waiting for, or perhaps even calling for, a theory. The discovery of any unexpected phenomenon, such as the discovery of superconductivity early in the twentieth century, calls for a theoretical explanation.

Experiment can do more than just indicate the need for a new theory. It may sometimes give a hint as to the structure of the theory required. The discovery of the Meissner effect, the exclusion of magnetic fields from the interior of a superconductor, was a crucial step in the development of a theory of superconductivity because it emphasized the importance of magnetism in the phenomenon.[6] Similarly, Pauli had shown that there were only five possible relativistically invariant forms for the four-Fermion weak interaction. The angular-correlation beta-decay experiments of the 1940s and 1950s were crucial in deciding which of these mathematical forms was correct. It is interesting that the most important of these experiments, the He^6 angular-correlation experiment of Rustad and Ruby, which had established that the form of the interaction was a combination of scalar and tensor (*S* and *T*), was later shown to be incorrect. When the error was found and the experimental apparatus improved, the conclusion was that the interaction was vector and axial vector (*V* and *A*).[7]

Still, one may fairly say that one of the most important roles of experiment is in the evaluation of theory—theory choice, confirmation, and refutation. The history of science abounds with such examples. The Compton effect and the photon theory of light, experiments on beta decay and the discovery of the violation of left-right symmetry in nature, the bending of starlight and the advance of the perihelion of Mercury and Einstein's general theory of relativity, and the discovery of CP symmetry violation by showing that the long-lived K^0 meson decayed into two pions, are modern examples.

Some scholars, however, deny that experiment has a legitimate role in the evaluation of theory. I have alluded to this earlier when discussing the view of Pickering and Hacking that experimental results are accepted when theory and experiment are mutually adjusted. Pickering states, "It is *unproblematic* that scientists produce accounts of the world that they find comprehensible; given their cultural resources, only singular incompetence could have prevented [high-energy physicists] from producing an understandable version of reality at any point in their history" (Pickering 1984, p. 413). If, as they claim, this adjustment not only is always possible, but is, in fact, done, then

6. The Meissner effect also conclusively demonstrates that superconductivity is a well-defined thermodynamic state of matter, and that thermodynamic and statistical mechanical theories can be applied in its analysis.

7. See Franklin (1990, pp. 55–59, 78–82) for details.

experiment cannot evaluate theory. Similarly, if experimental results are accepted on the basis of utility for future practice or agreement with existing commitments, as Pickering and others have claimed, then there is no legitimate role for experiment in theory evaluation. I believe they are wrong. I also believe that the historical studies support my view.

On a general level, Galison (1987, p. 11) has argued that there are three problems with this view: (1) "It is unfair to look to experimental arguments for ironclad implications and then, upon finding that experiments do not have logically impelled conclusions, to ascribe experimentalists' beliefs entirely to 'interests.'" (2) "Interest theory exaggerates the flexibility of theory. It is not just 'singular incompetence' that prevents arbitrary numbers of viable accounts of particle physics to co-exist—mathematical and physical constraints are not easily brushed aside." And (3) "Interest theorists do not attend to the constraints on experimentalists' conclusions that are imposed by the skills and techniques of their work."

I would emphasize the physical constraints. The world may just not coincide with our existing, accepted theory, and no amount of reasonable adjustment will make it do so. Pickering himself seems to acknowledge an aspect of this: "In his pragmatic material interaction with his apparatus, Morpurgo was interacting with, and learning about, something which resisted him, which was not himself, nor his culture: it was material reality. . . . The point that I want to emphasize is that this eventuation [whether or not fractional charges existed] was not entirely under Morpurgo's [or anyone's] control: it was a product of Morpurgo's immersion, through the medium of his experiment, in the *real*" (Pickering 1987, p. 197, emphasis added).[8]

Two episodes from the recent history of physics have been examined from these very different perspectives: what one might call the "evidence model" position of Galison and myself, and the "social constructivist" view of Pickering and others. These episodes are the discovery of weak-neutral currents and the experiments on atomic parity violation and their relation to the Weinberg-Salam unified theory of electroweak interactions.

I will not offer any explicit evaluation of the competing accounts of these two episodes. Obviously, I am not an impartial observer. The im-

8. Pickering's views are grounded in two traditional problems in the philosophy of science: (1) the Duhem-Quine problem, the problem of the localization of refutation, and (2) the underdetermination of theory by evidence. These issues are too complex, and have been discussed extensively elsewhere, to be treated briefly in this essay. For some recent discussion see Nelson (1992) and Franklin (1990, 1993*b*).

portant point is that detailed accounts have been written from both points of view. In each case, the studies presented contain sufficient detail so that by comparing both accounts the reader can make their own informed choice.

In the case of weak-neutral currents, events now attributed to such currents were found during the 1960s but were attributed to neutron background. At the time there was no theoretical prediction of such currents, and several experimental searches for them had produced null results. Later, after the Weinberg-Salam unified theory of electroweak interactions, which had other independent experimental support, had predicted the existence of these currents, two experiments, the Gargamelle heavy-liquid experiment at CERN and the E1A spark-chamber experiment at Fermilab, found evidence for the existence of the neutral currents.

Pickering explains this as the mutual adjustment of the theory of the phenomena, the experimental apparatus, and the theory of that apparatus. He notes that accepted experimental practice had changed, so that events that were regarded during the 1960s as unproblematic background were regarded as evidence for the existence of neutral currents in the 1970s. What had changed was the evidential context—the predictions of such currents by the Weinberg-Salam theory. As noted earlier, Pickering regards the reason for this as the future utility for theoretical and experimental practice: "Quite simply, particle physicists accepted the existence of the neutral current because they could see how to ply their trade more profitably in a world in which the neutral current was real" (Pickering 1984*b*, p. 87).

Galison disagrees. He does not deny that the evidential context had changed, or that the Weinberg-Salam theory was an important factor in both the pursuit and design of the experiment. He argues, however, that it was the construction of "arguments that would stand up in court" that decided the issue of the existence of weak-neutral currents. He documents the enormous effort that went into producing those arguments. Certainly, the theory of the phenomena was a guide for the experimenters, but it could not guarantee that events would be found in the bubble or spark chambers.

In the case of the experiments on atomic parity violation, there are also two different historical accounts (Pickering 1984*a*, 1991 and Franklin 1990, chap. 8; 1993*b*). I will begin with what is agreed on by both Pickering and myself. In 1976 and 1977, experimental groups at Oxford University and at the University of Washington reported results that disagreed with the predictions of the Weinberg-Salam unified theory of electroweak interactions. At the time the theory had other experimental support but was not universally accepted. In 1978 and 1979 a group at the Stanford Linear Accelerator (the SLAC E122 experiment) reported results that confirmed the Weinberg-Salam theory, and, on the basis of those results, combined with the previous support, the scientific community accepted the Weinberg-Salam theory.

At this point the accounts diverge. Pickering notes that the Weinberg-Salam theory was regarded as established despite the fact that "there had between no *intrinsic* change in the status of the Washington-Oxford experiments" (Pickering 1984*a*, p. 301). In his view, "particle physicists *chose* to accept the results of the SLAC experiment, chose to interpret them in terms of the standard model (rather than some alternative which might reconcile them with the atomic physics results), and therefore *chose* to regard the Washington-Oxford experiments as somehow defective in performance or interpretation" (1984*a*, p. 301. Pickering regards the Washington-Oxford results and those of SLAC E122 as having the same evidential weight and maintains that the reason the physics community chose to accept the SLAC results and the Weinberg-Salam theory it supported was that they provided more opportunity for future work and were also consistent with existing commitments.

My view is quite different. I regard the two experimental results as having quite different evidential weights. The initial Washington-Oxford results (later ones agreed with the Weinberg-Salam theory) used new and untested experimental apparatus and had large systematic uncertainties (as large as the predicted effects). In addition, their initially reported results were internally inconsistent and by 1979 there were other atomic parity violation results that confirmed the Weinberg-Salam theory. The overall situation with respect to the atomic parity results was quite uncertain. The SLAC experiment, on the other hand, although also using new techniques, had been very carefully checked and had far more evidential weight. As Bouchiat remarked, "I would like to say that I have been very much impressed by the care with which systematic errors have been treated in the experiment [SLAC E122]. It is certainly an example to be followed by all people working in this very difficult field" (Bouchiat 1980, pp. 359–60). Faced with this situation the physics community chose to accept the SLAC results, which supported the Weinberg-Salam theory, and to await further developments on the uncertain atomic parity violation results.[9]

9. For comments and criticism on these papers see Ackermann (1991), Lynch (1991), and Pickering (1991). For my response see Franklin (1993*b*). I also note here that the E122 experiment did conclusively refute the so-called hybrid model, which reconciled the atomic parity results with the high-energy results.

Ackermann (1988) has offered a middle-ground position on the interaction of experiment and theory and scientific progress. He initially proposes a symmetrical view, in which "science involves a dialectical interplay of theory and experiment, any particular advance of science involving a projection of one, and then revision and check by the other" (p. 327). He suggests, however, that there is, in fact, an asymmetry:

> Now we have a hint of legitimate asymmetry within the original confrontations produced by the flanking movements of scientific history. When theoretical conjectures clash, they may be resolved by logic or aesthetic considerations, but if not, resolution of the clash must await an experimental decision. When experiments produce clashing data, they may be resolved by theoretical judgment, but if not, resolution of the clash must await an experimental decision. In both cases, theory may fail us as the motor of progress, but experiment can always be varied by tinkering with the instruments, providing a path into the future. [Ackermann 1988, p. 334]

Experiment also plays a role in answering questions concerning the status of theoretical entities contained in our theories, and in the status of those theories themselves. This is the old realism-antirealism debate, but new answers have been offered recently.

Hacking (1983) and Cartwright (1983) have offered a view of entity realism in which the entities can have real status, but not theories. Hacking emphasizes manipulability: "If you can spray them then they are real" (1983, p. 23). More formally he argues, "*We are completely convinced of the reality of electrons when we regularly set out to build—and often enough succeed in building—new kinds of device that use various well-understood causal properties of electrons to interfere in other more hypothetical parts of nature*" (1983, p. 265). Cartwright also emphasizes the causal properties of entities. In discussing tracks in a cloud chamber she remarks that if there are no electrons there is no reason for the tracks to exist. I believe that Cartwright is painting with too broad a brush. If she wants to say that it is an electron track rather than a proton track, she must invoke a theory of ionization. This gives the theory the same epistemological status as the electron.

McKinney (1991) has suggested that Hacking has overemphasized manipulability. In his study of polywater he argues that experimenting on an entity may also provide grounds for belief in its existence. Morrison (1990) has argued that experimenters may manipulate entities without necessarily believing in their existence. One might also argue that the activities in a laboratory make little sense without a realist attitude toward the objects of experiment. The new attitude toward experiment has changed the discussion of scientific realism.

Trevor Pinch's study (1986) of Davis's experiment to measure the flux of solar neutrinos discusses the interaction of theory and experiment and its effect on securing funding for a large experiment, a subject rarely discussed. What Davis had was a neutrino detector. (See Shapere 1982 for another discussion of this experiment and its relation to "direct" observation.) It was only by placing it in the evidential context of Bahcall's solar model that the experiment became a crucial test of nuclear astrophysics, and an important and fundable experiment.

The results disagreed with Bahcall's calculations and led to various attempts to explain the discrepancy: casting doubt on the radiochemistry of the detector by Jacobs, questioning the nuclear cross-sections or the solar model used in the theoretical calculations, or considering the possibility of neutrino oscillations. In his detailed discussion of Jacobs's criticism, Pinch demonstrates that determined critics can maintain their belief, but only at the cost of appearing more and more unreasonable. Davis and others tested Jacobs's suggestions, even when they regarded them as extremely unlikely, and rejected them on the basis of experimental evidence. Interestingly, the solar neutrino problem persists today, although the discrepancy between theory and experiment is smaller. Instant rationality is not always possible.

Following Hacking's interventionist view of experiment, David Gooding (1990) has argued admirably for the importance of human agency in discussions of experiment. He presents examples from the history of electromagnetism in the nineteenth century, particularly the work of Faraday, to support his view that agency is essential to exploratory observation and experimental testing.

3. Discussion

It is clear that the study of experiment has come of age. There have been conferences devoted solely to it, and no recent meeting of the Philosophy of Science Association seems complete without at least one session devoted to experiment. It is also clear that there is no consensus either on experimental results or on the complex interaction between experiment and theory. If the reader requires further evidence of this they need only consult three recent essay collections, *Theory and Experiment* (Batens and van Bendegem 1988), *The Uses of Experiment* (Gooding, Pinch, and Schaffer 1989), and *Science as Practice and Culture* (Pickering 1992).

There is also no clear answer to Hacking's question, "Which comes first, theory or experiment?" We have seen that theory enters into the design of experiments, helps to decide what is a real effect and what is background, provides questions for experiments to answer, and provides the framework in which experiments are interpreted. Experiment provides results for theory to explain, may call for a new theory or give hints to the structure of that theory, may provide evidence for the existence of theoretical entities, and provides the means by which theories are evaluated.

Despite the disagreements, I believe that progress has been made. Experiment has been restored to its rightful place in the study of science. We have a new set of questions to answer, and an even greater number of proposed answers. There are also a considerable number of detailed studies of episodes involving experiment in the actual practice of science, which not only have provided those answers, but will provide the basis for further study and speculation. It is a long way from "All swans are white" to experiments on weak-neutral currents.

I regard the lack of consensus as a strength, rather than as a weakness, of the field. The absence of an accepted framework (dare I say paradigm) can only encourage further study. As Pickering might put it, "We have accepted the lack of a single framework because we can ply our trade more profitably in a world in which no such framework exists." My own view is that sufficient evidence exists to support the "evidence model," but not everyone agrees. I have no explanation for this. Perhaps the "evidence model" does not apply to the study of science.

Where are we likely to go? Thus far, most of the work has studied physics, where the theoretical predictions and mathematical structures are clear. Although much remains to be done, I believe that our work to date has provided a framework for the study of other sciences, particularly biology, in which the theory-evidence relation seems less clear and well defined.[10]

Lummer and Pringsheim are not dead after all.

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10. This has already begun. For an example of this see Rasmussen's (1993) discussion of the bacterial mesosome and electron microscopy. At the 1992 meeting of the Philosophy of Science Association, three other scholars discussed their current work on experiment and biology with me.

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