PDF version of the entry on
Experiment in Physics
http://plato.stanford.edu/archives/win2008/entries/physics-experiment/
from the Winter 2008 Edition of the

Stanford Encyclopedia of Philosophy

Library of Congress Catalog Data
ISSN: 1095-5054

Experiment in Physics

First published Mon Oct 5, 1998; substantive revision Fri Jul 6, 2007

Physics, and natural science in general, is a reasonable enterprise based on valid experimental evidence, criticism, and rational discussion. It provides us with knowledge of the physical world, and it is experiment that provides the evidence that grounds this knowledge. Experiment plays many roles in science. One of its important roles is to test theories and to provide the basis for scientific knowledge.[1] It can also call for a new theory, either by showing that an accepted theory is incorrect, or by exhibiting a new phenomenon that is in need of explanation. Experiment can provide hints toward the structure or mathematical form of a theory and it can provide evidence for the existence of the entities involved in our theories. Finally, it may also have a life of its own, independent of theory. Scientists may investigate a phenomenon just because it looks interesting. Such experiments may provide evidence for a future theory to explain. [Examples of these different roles will be presented below.] As we shall see below, a single experiment may play several of these roles at once.

If experiment is to play these important roles in science then we must have good reasons to believe experimental results, for science is a fallible enterprise. Theoretical calculations, experimental results, or the comparison between experiment and theory may all be wrong. Science is more complex than “The scientist proposes, Nature disposes.” It may not always be clear what the scientist is proposing. Theories often need to be articulated and clarified. It also may not be clear how Nature is disposing. Experiments may not always give clear-cut results, and may even disagree for a time.

In what follows, the reader will find an epistemology of experiment, a set of strategies that provides reasonable belief in experimental results. Scientific knowledge can then be reasonably based on these experimental results.

---

1. Experimental Results

1.1 The Case For Learning From Experiment

1.1.1 An Epistemology of Experiment

It has been two decades since Ian Hacking asked, “Do we see through a microscope?” (Hacking 1981). Hacking's question really asked how do we come to believe in an experimental result obtained with a complex experimental apparatus? How do we distinguish between a valid result[2] and an artifact created by that apparatus? If experiment is to play all of the important roles in science mentioned above and to provide the evidential basis for scientific knowledge, then we must have good reasons to believe in those results. Hacking provided an extended answer in the second half 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 or in the theory of the phenomenon. His illustration was the sustained 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 into the cell or stain the specimen. One expects the cell to change shape or color when this is done. Observing the predicted effect strengthens our belief in both 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 in both 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—dense bodies in cells—is seen with "different" microscopes, (e.g. ordinary, polarizing, phase-contrast, fluorescence, interference, electron, acoustic etc.) argues for the validity of the observation. One might question whether "different" is a theory-laden term. After all, it is our theory of light and of the microscope that allows us to consider these microscopes as different from each other. Nevertheless, the argument holds. 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 radio telescope, or when intervention is either impossible or extremely difficult? Other strategies are needed to validate the observation.[3] These may include:

1. Experimental checks and calibration, in which the experimental apparatus reproduces known phenomena. For example, if we wish to argue that the spectrum of a substance obtained with a new type of spectrometer is correct, we might check that this new spectrometer could reproduce the known Balmer series in hydrogen. If we correctly observe the Balmer Series then we strengthen our belief that the spectrometer is working properly. This also strengthens our belief in the results obtained with that spectrometer. If the check fails then we have good reason to question the results obtained with that apparatus.
2. Reproducing artifacts that are known in advance to be present. An example of this comes from experiments to measure the infrared spectra of organic molecules (Randall et al. 1949). It was not always possible to prepare a pure sample of such material. Sometimes the experimenters had to place the substance in an oil paste or in solution. In such cases, one expects to observe the spectrum of the oil or the solvent, superimposed on that of the substance. One can then compare the composite spectrum with the known spectrum of the oil or the solvent. Observation then of this artifact gives confidence in other measurements made with the spectrometer.
3. Elimination of plausible sources of error and alternative explanations of the result (the Sherlock Holmes strategy).[4] Thus, when scientists claimed to have observed electric discharges in the rings of Saturn, they argued for their result by showing that it could not have been caused by defects in the telemetry, interaction with the environment of Saturn, lightning, or dust. The only remaining explanation of their result was that it was due to electric discharges in the rings—there was no other plausible explanation of the observation. (In addition, the same result was observed by both Voyager 1 and Voyager 2. This provided independent confirmation. Often, several epistemological strategies are used in the same experiment.)

4. Using the results themselves to argue for their validity. Consider the problem of Galileo's telescopic observations of the moons of Jupiter. Although one might very well believe that his primitive, early telescope might have produced spurious spots of light, it is extremely implausible that the telescope would create images that they would appear to be a eclipses and other phenomena consistent with the motions of a small planetary system. It would have been even more implausible to believe that the created spots would satisfy Kepler's Third Law (R^3/T^2 = constant). A similar argument was used by Robert Millikan to support his observation of the quantization of electric charge and his measurement of the charge of the electron. Millikan remarked, "The total number of changes which we have observed would be between one and two thousand, and *in not one single instance has there been any change which did not represent the advent upon the drop of one definite invariable quantity of electricity or a very small multiple of that quantity*" (Millikan 1911, p. 360). In both of these cases one is arguing that there was no plausible malfunction of the apparatus, or background, that would explain the observations.
5. Using an independently well-corroborated theory of the phenomena to explain the results. This was illustrated in the discovery of the $W^{\pm}$, the charged intermediate vector boson required by the Weinberg-Salam unified theory of electroweak interactions. Although these experiments used very complex apparatuses and used other epistemological strategies (for details see (Franklin 1986, pp. 170-72)). I believe that the agreement of the observations with the theoretical predictions of the particle properties helped to validate the experimental results. In this case the particle candidates were observed in events that contained an electron with high transverse momentum and in which there were no particle jets, just as predicted by the theory. In addition, the measured particle mass of 81 ± 5 GeV/c^2 and $80^{+10}{}_{-6}$, GeV/c^2, found in the two experiments (note the independent confirmation also), was in good agreement with the theoretical prediction of 82 ± 2.4 GeV/c^2. It was very improbable that any background effect, which might mimic the presence of the particle, would be in agreement with theory.
6. Using an apparatus based on a well-corroborated theory. In this case the support for the theory inspires confidence in the apparatus based on that theory. This is the case with the electron microscope and the radio telescope, whose operations are based on a well-supported theories, although other strategies are also used to validate the observations made with these instruments.
7. Using statistical arguments. An interesting example of this arose in the 1960s when the search for new particles and resonances occupied a substantial fraction of the time and effort of those physicists working in experimental high-energy physics. The usual technique was to plot the number of events observed as a function of the invariant mass of the final-state particles and to look for bumps above a smooth background. The usual informal criterion for the presence of a new particle was that it resulted in a three standard-deviation effect above the background, a result that had a probability of 0.27% of occurring in a single bin. This criterion was later changed to four standard deviations, which had a probability of 0.0064% when it was pointed out that the number of graphs plotted each year by high-energy physicists made it rather probable, on statistical grounds, that a three standard-deviation effect would be observed.

These strategies along with Hacking's intervention and independent confirmation constitute an epistemology of experiment. They provide us with good reasons for belief in experimental results, They do not, however, guarantee that the results are correct. There are many experiments in which these strategies are applied, but whose results are later shown to be incorrect (examples will be presented below). Experiment is fallible. Neither are these strategies exclusive or exhaustive. No single one of them, or fixed combination of them, guarantees the validity of an experimental result. Physicists use as many of the strategies as they can conveniently apply in any given experiment.

1.1.2 Galison's Elaboration

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, the discovery of the muon, and 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 performed 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 includes 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" (Galison, 1987, p. 235).

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, several group members found the single photograph of a neutrino-electron scattering event particularly important, whereas 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, whereas those in the electronic tradition tend to find statistical arguments more persuasive and important than individual events. (For further discussion of this issue see Galison (1997)).

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 Schrodinger. 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,[5] it will continue to show the same reading, even though we may take the reading to be no longer important, or to tell us something other

than what we thought originally (Ackermann 1985, p. 33).

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 his discussion of the discovery of the muon, 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 unexplained phenomenon. 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," (i.e., 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. (See also (Franklin 1995b) and the discussion of the Stern-Gerlach experiment below). Such a theory can help to determine whether an experiment is feasible. Galison 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.[6]

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 produce 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 report 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-upon result that disagreed with theoretical expectations.

Recently, Galison has modified his views. In *Image and Logic*, an extended study of instrumentation in 20th-century high-energy physics, Galison (1997) has extended his argument that there are two distinct experimental traditions within that field—the visual (or image) tradition and the electronic (or logic) tradition. The image tradition uses detectors such as cloud chambers or bubble chanbers, which provide detailed and extensive information about each individual event. The electronic detectors used by the logic tradition, such as geiger counters, scintillation counters, and spark chambers, provide less detailed information about individual events, but detect more events. Galison's view is that experimenters working in these two traditions form distinct epistemic and linguistic groups that rely on different forms of argument. The visual tradition emphasizes the single "golden" event. "On the image side resides a deep-seated commitment to the 'golden event': the single picture of such clarity and distinctness that it commands acceptance." (Galison, 1997, p. 22) "The golden event was the exemplar of the image tradition: an individual instance so complete and well defined, so 'manifestly' free of distortion and background that no further data had to be involved" (p. 23). Because the individual events provided in the logic detectors containded less detailed information than the pictures of the visual tradition, statistical arguments based on large numbers of events were required.

Kent Staley (1999) disagrees. He argues that the two traditions are not as distinct as Galison believes:

> I show that discoveries in both traditions have employed the same statistical [I would add "and/or probabilistic"] form of argument, even when basing discovery claims on single, golden events. Where Galison sees an epistemic divide between two communities that can only be bridged by creole- or pidgin-like 'interlanguage,' there is in fact a shared commitment to a statistical form of experimental argument. (p. 96).

Staley believes that although there is certainly epistemic continuity within a given tradition, there is also a continuity between the traditions. This does not, I believe, mean that the shared commitmeny comprises all of the arguments offered in any particular instance, but rather that the same methods are often used by both communities. Galison does not deny that statistical methods are used in the image tradition, but he thinks that they are relatively unimportant. "While statistics could certainly be used within the image tradition, it was by no means necessary for most applications" (Galison, 1997, p. 451). In contrast, Galison believes that arguments in the logic tradition "were inherently and inalienably statistical. Estimation of probable errors and the statistical excess over background is not a side issue in these detectors—it is central to the possibilty of any demonstration at all" (p. 451).

Although a detailed discussion of the disagreement between Staley and Galison would take us too far from the subject of this essay, they both agree that arguments are offered for the correctness of experimental results. Their disagreement concerns the nature of those arguments. (For

further discussion see Franklin, (2002), pp. 9-17).

1.2 The Case Against Learning From Experiment

1.2.1 Collins and the Experimenters' Regress

Collins, 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). Harry Collins, for example, is well known for his skepticism concerning both experimental results and evidence. He develops an argument that he calls the "experimenters' regress" (Collins 1985, chapter 4, pp. 79–111): What scientists take to be a correct result is one obtained with a good, that is, properly functioning, experimental apparatus. But a good experimental apparatus is simply one that gives correct results. Collins claims that there are no formal criteria that one can apply to decide whether or not an experimental apparatus is working properly. In particular, he argues that calibrating an experimental apparatus by using a surrogate signal cannot provide an independent reason for considering the apparatus to be reliable.

In Collins' view the regress is eventually broken by negotiation within the appropriate scientific community, a process driven by factors such as the career, social, and cognitive interests of the scientists, and the perceived utility for future work, but one that is not decided by what we might call epistemological criteria, or reasoned judgment. Thus, Collins concludes that his regress raises serious questions concerning both experimental evidence and its use in the evaluation of scientific hypotheses and theories. Indeed, if no way out of the regress can be found, then he has a point.

Collins strongest candidate for an example of the experimenters' regress is presented in his history of the early attempts to detect gravitational radiation, or gravity waves. (For more detailed discussion of this episode see (Collins 1985; 1994; Franklin 1994; 1997a) In this case, the physics community was forced to compare Weber's claims that he had observed gravity waves with the reports from six other experiments that failed to detect them. On the one hand, Collins argues that the decision between these conflicting experimental results could not be made on epistemological or methodological grounds—he claims that the six negative experiments could not legitimately be regarded as replications[7] and hence become less impressive. On the other hand, Weber's apparatus, precisely because the experiments used a new type of apparatus to try to detect a hitherto unobserved phenomenon,[8] could not be subjected to standard calibration techniques.

The results presented by Weber's critics were not only more numerous, but they had also been carefully cross-checked. The groups had exchanged both data and analysis programs and confirmed their results. The critics had also investigated whether or not their analysis procedure, the use of a linear algorithm, could account for their failure to observe Weber's reported results. They had used Weber's preferred procedure, a nonlinear algorithm, to analyze their own data, and still found no sign of an effect. They had also calibrated their experimental apparatuses by inserting acoustic pulses of known energy and finding that they could detect a signal. Weber, on the other hand, as well as his critics using his analysis procedure, could not detect such calibration pulses.

There were, in addition, several other serious questions raised about Weber's analysis procedures. These included an admitted programming error that generated spurious coincidences between Weber's two detectors, possible selection bias by Weber, Weber's report of coincidences between two detectors when the data had been taken four hours apart, and whether or not Weber's experimental apparatus could produce the narrow

coincidences claimed.

It seems clear that the critics' results were far more credible than Weber's. They had checked their results by independent confirmation, which included the sharing of data and analysis programs. They had also eliminated a plausible source of error, that of the pulses being longer than expected, by analyzing their results using the nonlinear algorithm and by explicitly searching for such long pulses.[9] They had also calibrated their apparatuses by injecting pulses of known energy and observing the output.

Contrary to Collins, I believe that the scientific community made a reasoned judgment and rejected Weber's results and accepted those of his critics. Although no formal rules were applied (e.g. if you make four errors, rather than three, your results lack credibility; or if there are five, but not six, conflicting results, your work is still credible) the procedure was reasonable.

Pickering has argued that the reasons 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. (1984b, p. 87)
>
> 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. (1981, p. 236)

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, for example, the history of the overthrow of parity conservation and of CP symmetry discussed below and in (Franklin 1986, Ch. 1, 3)).

1.2.2 Pickering on Communal Opportunism and Plastic Resources

Pickering has recently offered a different view of experimental results. In his 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; Pickering 1989). He says:

> Achieving such relations of mutual support is, I suggest, the defining characteristic of the successful experiment. (1987, p. 199)

He uses Morpurgo's search for free quarks, or fractional charges of 1/3 *e* or 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 goes on to note that Morpurgo did not tinker with the two competing theories of the phenomena then on offer, those of integral and fractional charge:

> The initial source of doubt about the adequacy of the early stages of the experiment was precisely the fact that their findings—continuously distributed charges—were consonant with neither of the phenomenal models which Morpurgo was prepared to countenance. And what motivated the search for a new instrumental model was Morpurgo's eventual success in producing findings in accordance with one of the phenomenal models he was willing to accept
>
> The conclusion of Morpurgo's first series of experiments, then, and the production of the observation report which they sustained, was marked by bringing into relations of mutual support of the three elements I have discussed: the material form of the apparatus and the two conceptual models, one instrumental and the other phenomenal. Achieving such relations of mutual support is, I suggest, the defining charactersitic of the successful experiment. (p. 199)

Pickering has made several important and valid points concerning experiment. Most importantly, he has emphasized that an experimental apparatus is initially rarely capable of producing a valid experimental results and that some adjustment, or tinkering, is required before it does. He has also recognized that both the theory of the apparatus and the theory of the phenomena can enter into the production of a valid experimental result. What I wish to question, however, is the emphasis he places on these theoretical components. From Millikan onwards, experiments had strongly supported the existence of a fundamental unit of charge and charge quantization. The failure of Morpurgo's apparatus produce measurements of integral charge indicated that it was not operating properly and that his theoretical understanding of it was faulty. It was the failure to produce measurements in agreement with what was already known (i.e., the failure of an important experimental check) that caused doubts about Morpurgo's measurements. This was true regardless of the theoretical models available, or those that Morpurgo was willing to accept. It was only when Morpurgo's apparatus could reproduce known measurements that it could be trusted and used to search for fractional charge. To be sure, Pickering has allowed a role for the natural world in the production of the experimental result, but it does not seem to be decisive.

1.2.3 Critical Responses to Pickering

Ackermann has offered a modification of Pickering's view. He suggests that the experimental apparatus itself is a less plastic resource then 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 only as phenomena only when the data can be interpreted by theory. (pp. 57–8)

One might ask whether 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? Accepted theories can be refuted. Several examples will be presented below.

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” (1992, p. 60).

1.2.4 Pickering and the Dance of Agency

Recently Pickering has offered a somewhat revised account of science. “My basic image of science is a performative one, in which the performances the doings of human and material agency come to the fore.

Scientists are human agents in a field of material agency which they struggle to capture in machines (Pickering, 1995, p. 21)." He then discusses the complex interaction between human and material agency, which I interpret as the interaction between experimenters, their apparatus, and the natural world.

> The dance of agency, seen asymmetrically from the human end, thus takes the form of a *dialectic of resistance and accommodations,* where resistance denotes the failure to achieve an intended capture of agency in practice, and accommodation an active human strategy of response to resistance, which can include revisions to goals and intentions as well as to the material form of the machine in question and to the human frame of gestures and social relations that surround it (p. 22).“

Pickering's idea of resistance is illustrated by Morpurgo's observation of continuous, rather than integral or fractional, electrical charge, which did not agree with his expectations. Morpurgo's accommodation consisted of changing his experimental apparatus by using a larger separation between his plates, and also by modifying his theoretical account of the apparatus. That being done, integral charges were observed and the result stabilized by the mutual agreement of the apparatus, the theory of the apparatus, and the theory of the phenomenon. Pickering notes that ”the outcomes depend on how the world is (p. 182).“ ”In this way, then, *how the material world is* leaks into and infects our representations of it in a nontrivial and consequential fashion. My analysis thus displays an intimate and responsive engagement between scientific knowledge and the material world that is integral to scientific practice (p. 183).“

Nevertheless there is something confusing about Pickering's invocation of the natural world. Although Pickering acknowledges the importance of the natural world, his use of the term ”infects“ seems to indicate that he

isn't entirely happy with this. Nor does the natural world seem to have much efficacy. It never seems to be decisive in any of Pickering's case studies. Recall that he argued that physicists accepted the existence of weak neutral currents because ”they could ply their trade more profitably in a world in which the neutral current was real.“ In his account, Morpurgo's observation of continuous charge is important only because it disagrees with his theoretical models of the phenomenon. The fact that it disagreed with numerous previous observations of integral charge doesn't seem to matter. This is further illustrated by Pickering's discussion of the conflict between Morpurgo and Fairbank. As we have seen, Morpurgo reported that he did not observe fractional electrical charges. On the other hand, in the late 1970s and early 1980s, Fairbank and his collaborators published a series of papers in which they claimed to have observed fractional charges (See, for example, LaRue, Phillips et al. 1981). Faced with this discord Pickering concludes,

> In Chapter 3, I traced out Morpurgo's route to his findings in terms of the particular vectors of cultural extension that he pursued, the particular resistances and accommodations thus precipitated, and the particular interactive stabilizations he achieved. The same could be done, I am sure, in respect of Fairbank. And these tracings are all that needs to said about their divergence. It just happened that the contingencies of resistance and accommodation worked out differently in the two instances. Differences like these are, I think, continually bubbling up in practice, without any special causes behind them (pp. 211-212).

The natural world seems to have disappeared from Pickering's account. There is a real question here as to whether or not fractional charges exist in nature. The conclusions reached by Fairbank and by Morpurgo about their existence cannot both be correct. It seems insufficient to merely state, as Pickering does, that Fairbank and Morpurgo achieved their individual stabilizations and to leave the conflict unresolved. (Pickering does comment that one could follow the subsequent history and see how the conflict was resolved, and he does give some brief statements about it, but its resolution is not important for him). At the very least, I believe, one should consider the actions of the scientific community. Scientific knowledge is not determined individually, but communally. Pickering seems to acknowledge this. ”One might, therefore, want to set up a metric and say that items of scientific knowledge are more or less objective depending on the extent to which they are threaded into the rest of scientific culture, socially stabilized over time, and so on. I can see nothing wrong with thinking this way…. (p. 196).“ The fact that Fairbank believed in the existence of fractional electrical charges, or that Weber strongly believed that he had observed gravity waves, does not make them right. These are questions about the natural world that can be resolved. Either fractional charges and gravity waves exist or they don't, or to be more cautious we might say that we have good reasons to support our claims about their existence, or we do not.

Another issue neglected by Pickering is the question of whether a particular mutual adjustment of theory, of the apparatus or the phenomenon, and the experimental apparatus and evidence is justified. Pickering seems to believe that any such adjustment that provides stabilization, either for an individual or for the community, is acceptable. I do not. Experimenters sometimes exclude data and engage in selective analysis procedures in producing experimental results. These practices are, at the very least, questionable as is the use of the results produced by such practices in science. There are, I believe, procedures in the normal practice of science that provide safeguards against them. (For details see Franklin, 2002, Section 1).

The difference between our attitudes toward the resolution of discord is one of the important distinctions between my view of science and

Pickering's. I do not believe it is sufficient simply to say that the resolution is socially stabilized. I want to know how that resolution was achieved and what were the reasons offered for that resolution. If we are faced with discordant experimental results and both experimenters have offered reasonable arguments for their correctness, then clearly more work is needed. It seems reasonable, in such cases, for the physics community to search for an error in one, or both, of the experiments.

Pickering discusses yet another difference between our views. He sees traditional philosophy of science as regarding objectivity ”as stemming from a peculiar kind of mental hygiene or policing of thought. This police function relates specifically to theory choice in science, which,… is usually discussed in terms of the rational rules or methods responsible for closure in theoretical debate (p. 197).“ He goes on to remark that,

> The most action in recent methodological thought has centered on attempts like Allan Franklin's to extend the methodological approach to experiments by setting up a set of rules for their proper performance. Franklin thus seeks to extend classical discussions of objectivity to the empirical base of science (a topic hitherto neglected in the philosophical tradition but one that, of course the mangle [Pickering's view] also addresses). For an argument between myself and Franklin on the same lines as that laid out below, see (Franklin 1990, Chapter 8; Franklin 1991); and (Pickering 1991); and for commentaries related to that debate, (Ackermann 1991) and (Lynch 1991) (p. 197).”

For further discussion see (Franklin 1993b)). Although I agree that my epistemology of experiment is designed to offer good reasons for belief in experimental results, I do not agree with Pickering that they are a set of rules. I regard them as a set of strategies, from which physicists choose, in order to argue for the correctness of their results. As noted above, I do not think the strategies offered are either exclusive or exhaustive.

There is another point of disagreement between Pickering and myself. He claims to be dealing with the practice of science, and yet he excludes certain practices from his discussions. One scientific practice is the application of the epistemological strategies I have outlined above to argue for the correctness of an experimental results. In fact, one of the essential features of an experimental paper is the presentation of such arguments. I note further that writing such papers, a performative act, is also a scientific practice and it would seem reasonable to examine both the structure and content of those papers.

1.2.5 Hacking's The Social Construction of What?

Recently Ian Hacking (1999, chapter 3) has provided an incisive and interesting discussion of the issues that divide the constructivists (Collins, Pickering, etc.) from the rationalists, like myself. He sets out three sticking points between the two views: 1) contingency, 2) nominalism, and 3) external explanations of stability.

Contingency is the idea that science is not predetermined, that it could have developed in any one of several successful ways. This is the view adopted by constructivists. Hacking illustrates this with Pickering's account of high-energy physics during the 1970s during which the quark model came to dominate. (See Pickering 1984a).

> The constructionist maintains a contingency thesis. In the case of physics, (a) physics theoretical, experimental, material) could have developed in, for example, a nonquarky way, and, by the detailed standards that would have evolved with this alternative physics, could have been as successful as recent physics has been by its detailed standards. Moreover, (b) there is no sense in which this imagined physics would be equivalent to present physics. The

> physicist denies that. (Hacking 1999, pp. 78-79).

> To sum up Pickering's doctrine: there could have been a research program as successful ("progressive") as that of high-energy physics in the 1970s, but with different theories, phenomenology, schematic descriptions of apparatus, and apparatus, and with a different, and progressive, series of robust fits between these ingredients. Moreover and this is something badly in need of clarification the "different" physics would not have been equivalent to present physics. Not logically incompatible with, just different.

> The constructionist about (the idea) of quarks thus claims that the upshot of this process of accommodation and resistance is not fully predetermined. Laboratory work requires that we get a robust fit between apparatus, beliefs about the apparatus, interpretations and analyses of data, and theories. *Before a robust fit has been achieved, it is not determined what that fit will be. Not determined by how the world is, not determined by technology now in existence, not determined by the social practices of scientists, not determined by interests or networks, not determined by genius, not determined by anything* (pp. 72-73, emphasis added).

Much depends here on what Hacking means by "determined." If he means entailed then I agree with him. I doubt that the world, or more properly, what we can learn about it, entails a unique theory. If not, as seems more plausible, he means that the way the world is places no restrictions on that successful science, then I disagree strongly. I would certainly wish to argue that the way the world is restricts the kinds of theories that will fit the phenomena, the kinds of apparatus we can build, and the results we can obtain with such apparatuses. To think otherwise seems silly. Consider a homey example, it seems to me highly unlikely, an understatement, that someone can come up with a successful theory in which objects whose density is greater than that of air fall upwards. This is not, I believe, a caricature of the view Hacking describes. Describing Pickering's view, he states, “Physics did not need to take a route that involved Maxwell's Equations, the Second Law of Thermodynamics, or the present values of the velocity of light (p. 70).” Although I have some sympathy for this view as regards Maxwell's Equations or the Second Law of Thermodynamics, I do not agree about the value of the speed of light. That is determined by the way the world is. Any successful theory of light must give that value for its speed.

At the other extreme are the “inevitablists,” among whom Hacking classifies most scientists. He cites Sheldon Glashow, a Nobel Prize winner, “Any intelligent alien anywhere would have come upon the same logical system as we have to explain the structure of protons and the nature of supernovae (Glashow 1992, p. 28).”

Another difference between Pickering and myself on contingency concerns the question of not whether an alternative is possible, but rather whether there are reasons why that alternative should be pursued. Pickering seems to identify *can* with *ought*.

In the late 1970s there was a disagreement between the results of low-energy experiments on atomic parity violation (the violation of left-right symmetry) performed at the University of Washington and at Oxford University and the result of a high-energy experiment on the scattering of polarized electrons from deuterium (the SLAC E122 experiment). The atomic-parity violation experiments failed to observe the parity-violating effects predicted by the Weinberg- Salam (W-S) unified theory of electroweak interactions, whereas the SLAC experiment observed the predicted effect. In my view, these early atomic physics results were quite uncertain in themselves and that uncertainty was increased by positive

results obtained in similar experiments at Berkeley and Novosibirsk. At the time the theory had other evidential support, but was not universally accepted. Pickering and I are in agreement that the W-S theory was accepted on the basis of the SLAC E122 result. We differ dramatically in our discussions of the experiments. Our difference on contingency concerns a particular theoretical alternative that was proposed at the time to explain the discrepancy between the experimental results.

Pickering asked why a theorist might not have attempted to find a variant of electroweak gauge theory that might have reconciled the Washington-Oxford atomic parity results with the positive E122 result. (What such a theorist was supposed to do with the supportive atomic parity results later provided by experiments at Berkeley and at Novosibirsk is never mentioned). “But though it is true that E122 analysed their data in a way that displayed the improbability [the probability of the fit to the hybrid model was 6×10^{-4}] 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” (Pickering 1991, p. 462). Pickering notes that open-ended recipes for constructing such variants had been written down as early as 1972 (p. 467). I agree that it would have been possible to do so, but one may ask whether or not a scientist might have wished to do so. If the scientist agreed with my view that the SLAC E122 experiment provided considerable evidential weight in support of the W-S theory and that a set of conflicting and uncertain results from atomic parity-violation experiments gave an equivocal answer on that support, what reason would they have had to invent an alternative?

This is not to suggest that scientists do not, or should not, engage in speculation, but rather that there was no necessity to do so in this case. Theorists often do propose alternatives to existing, well-confirmed theories.

Constructivist case studies always seem to result in the support of existing, accepted theory (Pickering 1984a; 1984b; 1991; Collins 1985; Collins and Pinch 1993). One criticism implied in such cases is that alternatives are not considered, that the hypothesis space of acceptable alternatives is either very small or empty. I don't believe this is correct. Thus, when the experiment of Christenson et al. (1964) detected $K^o{}_2$ decay into two pions, which seemed to show that CP symmetry (combined particle-antiparticle and space inversion symmetry) was violated, no fewer than 10 alternatives were offered. These included (1) the cosmological model resulting from the local dysymmetry of matter and antimatter, (2) external fields, (3) the decay of the $K^o{}_2$ into a $K^o{}_1$ with the subsequent decay of the $K^o{}_1$ into two pions, which was allowed by the symmetry, (4) the emission of another neutral particle, “the paritino,” in the $K^o{}_2$ decay, similar to the emission of the neutrino in beta decay, (5) that one of the pions emitted in the decay was in fact a “spion,” a pion with spin one rather than zero, (6) that the decay was due to another neutral particle, the L, produced coherently with the K^o, (7) the existence of a “shadow” universe, which interacted with out universe only through the weak interactions, and that the decay seen was the decay of the “shadow $K^o{}_2$,” (8) the failure of the exponential decay law, 9) the failure of the principle of superposition in quantum mechanics, and 10) that the decay pions were not bosons.

As one can see, the limits placed on alternatives were not very stringent. By the end of 1967, all of the alternatives had been tested and found wanting, leaving CP symmetry unprotected. Here the differing judgments of the scientific community about what was worth proposing and pursuing led to a wide variety of alternatives being tested.

Hacking's second sticking point is nominalism, or name-ism. He notes that in its most extreme form nominalism denies that there is anything in common or peculiar to objects selected by a name, such as “Douglas fir” other than that they are called Douglas fir. Opponents contend that good names, or good accounts of nature, tell us something correct about the world. This is related to the realism-antirealism debate concerning the status of unobservable entities that has plagued philosophers for millennia. For example Bas van Fraassen (1980), an antirealist, holds that we have no grounds for belief in unobservable entities such as the electron and that accepting theories about the electron means only that we believe that the things the theory says about observables is true. A realist claims that electrons really exist and that as, for example, Wilfred Sellars remarked, “to have good reason for holding a theory is *ipso facto* to have good reason for holding that the entities postulated by the theory exist (Sellars 1962, p. 97).” In Hacking's view a scientific nominalist is more radical than an antirealist and is just as skeptical about fir trees as they are about electrons. A nominalist further believes that the structures we conceive of are properties of our representations of the world and not of the world itself. Hacking refers to opponents of that view as inherent structuralists.

Hacking also remarks that this point is related to the question of “scientific facts.” Thus, constructivists Latour and Woolgar originally entitled their book *Laboratory Life: The Social Construction of Scientific Facts* (1979). Andrew Pickering entitled his history of the quark model *Constructing Quarks* (Pickering 1984a). Physicists argue that this demeans their work. Steven Weinberg, a realist and a physicist, criticized Pickering's title by noting that no mountaineer would ever name a book *Constructing Everest*. For Weinberg, quarks and Mount Everest have the same ontological status. They are both facts about the world. Hacking argues that constructivists do not, despite appearances, believe that facts do not exist, or that there is no such thing as reality. He cites Latour and Woolgar “that ‘out-there-ness' is a *consequence* of scientific work rather than its cause (Latour and Woolgar 1986, p. 180).” I agree with Hacking when he concludes that,

> Latour and Woolgar were surely right. We should not *explain* why some people believe that *p* by saying that *p* is true, or corresponds to a fact, or the facts. For example: someone believes that the universe began with what for brevity we call a big bang. A host of reasons now supports this belief. But after you have listed all the reasons, you should not add, as if it were an additional reason for believing in the big bang, 'and it is true that the universe began with a big bang.' Or 'and it is a fact.'This observation has nothing peculiarly to do with social construction. It could equally have been advanced by an old-fashioned philosopher of language. It is a remark about the grammar of the verb 'to explain' (Hacking 1999, pp. 80–81).

I would add, however, that the reasons Hacking cites as supporting that belief are given to us by valid experimental evidence and not by the social and personal interests of scientists. I'm not sure that Latour and Woolgar would agree. My own position is one that one might reasonably call conjectural realism. I believe that we have good reasons to believe in facts, and in the entities involved in our theories, always remembering, of course, that science is fallible.

Hacking's third sticking point is the external explanations of stability.

> The constructionist holds that explanations for the stability of scientific belief involve, at least in part, elements that are external to the content of science. These elements typically include social factors, interests, networks, or however they be described. Opponents hold that whatever be the context of discovery, the explanation of stability is internal to the science itself (Hacking 1999, p. 92).
>
> Rationalists think that most science proceeds as it does in the light

> of good reasons produced by research. Some bodies of knowledge become stable because of the wealth of good theoretical and experimental reasons that can be adduced for them. Constructivists think that the reasons are not decisive for the course of science. Nelson (1994) concludes that this issue will never be decided. Rationalists, at least retrospectively, can always adduce reasons that satisfy them. Constructivists, with equal ingenuity, can always find to their own satisfaction an openness where the upshot of research is settled by something other than reason. Something external. That is one way of saying we have found an irresoluble "sticking point" (pp. 91-92)

Thus, there is a rather severe disagreement on the reasons for the acceptance of experimental results. For some, like Staley, 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.

2. The Roles of Experiment

2.1 A Life of Its Own

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, William Herschel's work on “radiant heat,” and Davy's observation of the gas emitted by algae and the flaring of a taper in that gas. In none of these cases did the experimenter have any theory of the phenomenon under investigation. One may also 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 guidance from 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 (McKinney 1992). 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 $K^+{}_{e3}$ branching ratio and decay spectrum, (3) measurement of the $K^+{}_{e2}$ branching ratio, and (4) 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. This allowed the group to successfully perform the technically more difficult experiments later in the sequence. We might refer to this as “instrumental loyalty” and the “recycling of expertise” (Franklin 1997b). 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 remarks on 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, 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). 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 calls for a theoretical explanation.

2.2 Confirmation and Refutation

Nevertheless several of the important roles of experiment involve its relation to theory. Experiment may confirm a theory, refute a theory, or give hints to the mathematical structure of a theory.

2.2.1 The Discovery of Parity Nonconservation: A Crucial Experiment

Let us consider first an episode in which the relation between theory and experiment was clear and straightforward. This was a “crucial” experiment, one that decided unequivocally between two competing theories, or classes of theory. The episode was that of the discovery that parity, mirror-reflection symmetry or left-right symmetry, is not conserved in the weak interactions. (For details of this episode see Franklin (1986, Ch. 1) and Appendix 1). Experiments showed that in the beta decay of nuclei the number of electrons emitted in the same direction as the nuclear spin was different from the number emitted opoosite to the spin direction. This was a clear demonstartion of parity vilation in the weak interactions.

2.2.2 The Discovery of CP Violation: A Persuasive Experiment

After the discovery of parity and charge conjugation nonconservation, and following a suggestion by Landau, physicists considered CP (combined parity and particle-antiparticle symmetry), which was still conserved in the experiments, as the appropriate symmetry. One consequence of this scheme, if CP were conserved, was that the K_1^o meson could decay into two pions, whereas the K_2^o meson could not.[10] Thus, observation of the decay of K_2^o into two pions would indicate CP violation. The decay was observed by a group at Princeton University. Although several alternative explanations were offered, experiments eliminated each of the alternatives leaving only CP violation as an explanation of the experimental result. (For details of this episode see Franklin (1986, Ch. 3) and Appendix 2.)

2.2.3 The Discovery of Bose-Einstein Condensation: Confirmation After 70 Years

In both of the episodes discussed previously, those of parity nonconservation and of CP violation, we saw a decision between two competing classes of theories. This episode, the discovery of Bose-Einstein condensation (BEC), illustrates the confirmation of a specific theoretical prediction 70 years after the theoretical prediction was first made. Bose (1924) and Einstein (1924; 1925) predicted that a gas of noninteracting bosonic atoms will, below a certain temperature, suddenly develop a macroscopic population in the lowest energy quantum state.[11] (For details of this episode see Appendix 3.)

2.3 Complications

In the three episodes discussed in the previous section, the relation between experiment and theory was clear. The experiments gave unequivocal results and there was no ambiguity about what theory was predicting. None of the conclusions reached has since been questioned. Parity and CP symmetry are violated in the weak interactions and Bose-

Einstein condensation is an accepted phenomenon. In the practice of science things are often more complex. Experimental results may be in conflict, or may even be incorrect. Theoretical calculations may also be in error or a correct theory may be incorrectly applied. There are even cases in which both experiment and theory are wrong. As noted earlier, science is fallible. In this section I will briefly discuss several episodes which illustrate these complexities.

2.3.1 The Fall of the Fifth Force

The episode of the fifth force is the case of a refutation of an hypoothesis, but only after a disagreement between experimental results was resolved. The “Fifth Force” was a proposed modification of Newton's Law of Universal Gravitation. The initial experiments gave conflicting results: one supported the existence of the Fifth Force whereas the other argued against it. After numerous repetitions of the experiment, the discord was resolved and a consensus reached that the Fifth Force did not exist. (For details of this episode see Appendix 4.)

2.3.2 Right Experiment, Wrong Theory: The Stern-Gerlach Experiment[12]

The Stern-Gerlach experiment was regarded as crucial at the time it was performed, but, in fact, wasn't. In the view of the physics community it decided the issue between two theories, refuting one and supporting the other. In the light of later work, however, the refutation stood, but the confirmation was questionable. In fact, the experimental result posed problems for the theory it had seemingly confirmed. A new theory was proposed and although the Stern-Gerlach result initially also posed problems for the new theory, after a modification of that new theory, the result confirmed it. In a sense, it was crucial after all. It just took some time.

The Stern-Gerlach experiment provides evidence for the existence of electron spin. These experimental results were first published in 1922, although the idea of electron spin wasn't proposed by Goudsmit and Uhlenbeck until 1925 (1925; 1926). One might say that electron spin was discovered before it was invented. (For details of this episode see Appendix 5).

2.3.3 Sometimes Refutation Doesn't Work: The Double-Scattering of Electrons

In the last section we saw some of the difficulty inherent in experiment-theory comparison. One is sometimes faced with the question of whether the experimental apparatus satisfies the conditions required by theory, or conversely, whether the appropriate theory is being compared to the experimental result. A case in point is the history of experiments on the double-scattering of electrons by heavy nuclei (Mott scattering) during the 1930s and the relation of these results to Dirac's theory of the electron, an episode in which the question of whether or not the experiment satisfied the conditions of the theoretical calculation was central. Initially, experiments disagreed with Mott's calculation, casting doubt on the underlying Dirac theory. After more than a decade of work, both experimental and theoretical, it was realized that there was a background effect in the experiments that masked the predicted effect. When the background was eliminated experiment and theory agreed. (Appendix 6)

2.4 Other Roles

2.4.1 Evidence for a New Entity: J.J. Thomson and the Electron

Experiment can also provide us with evidence for the existence of the entities involved in our theories. J.J. Thomson's experiments on cathode

rays provided grounds for belief in the existence of electrons. (For details of this episode see Appendix 7).

2.4.2 The Articulation of Theory: Weak Interactions

Experiment can also help to articulate a theory. Experiments on beta decay during from the 1930s to the 1950s detremined the precise mathematical form of Fermi's theory of beta decay. (For details of this episode see Appendix 8.)

2.5 Some Thoughts on Experiment in Biology

2.5.1 Epistemological Strategies and the Peppered Moth Experiment

One comment that has been made concerning the philosophy of experiment is that all of the examples are taken from physics and are therefore limited. In this section I will suggest that these discussions also apply to biology.

Although all of the illustrations of the epistemology of experiment come from physics, David Rudge (1998; 2001) has shown that they are also used in biology. His example is Kettlewell's (1955; 1956; 1958) evolutionary biology experiments on the Peppered Moth, *Biston betularia*. The *typical* form of the moth has a pale speckled appearance and there are two darker forms, f. *carbonaria*, which is nearly black, and f. *insularia*, which is intermediate in color. The *typical* form of the moth was most prevalent in the British Isles and Europe until the middle of the nineteenth century. At that time things began to change. Increasing industrial pollution had both darkened the surfaces of trees and rocks and had also killed the lichen cover of the forests downwind of pollution sources. Coincident with these changes, naturalists had found that rare, darker forms of several moth species, in particular the Peppered Moth, had become common in areas downwind of pollution sources.

Kettlewell attempted to test a selectionist explanation of this phenomenon. E.B. Ford (1937; 1940) had suggested a two-part explanation of this effect: 1) darker moths had a superior physiology and 2) the spread of the melanic gene was confined to industrial areas because the darker color made *carbonaria* more conspicuous to avian predators in rural areas and less conspicuous in polluted areas. Kettlewell believed that Ford had established the superior viability of darker moths and he wanted to test the hypothesis that the darker form of the moth was less conspicuous to predators in industrial areas.

Kettlewell's investigations consisted of three parts. In the first part he used human observers to investigate whether his proposed scoring method would be accurate in assessing the relative conspicuousness of different types of moths against different backgrounds. The tests showed that moths on “correct” backgrounds, *typical* on lichen covered backgrounds and dark moths on soot-blackened backgrounds were almost always judged inconspicuous, whereas moths on “incorrect” backgrounds were judged conspicuous.

The second step involved releasing birds into a cage containing all three types of moth and both soot-blackened and lichen covered pieces of bark as resting places. After some difficulties (see Rudge 1998 for details), Kettlewell found that birds prey on moths in an order of conspicuousness similar to that gauged by human observers.

The third step was to investigate whether birds preferentially prey on conspicuous moths in the wild. Kettlewell used a mark-release-recapture experiment in both a polluted environment (Birmingham) and later in an unpolluted wood. He released 630 marked male moths of all three types in an area near Birmingham, which contained predators and natural boundaries. He then recaptured the moths using two different types of trap, each containing virgin females of all three types to guard against the

possibility of pheromone differences.

Kettlewell found that *carbonaria* was twice as likely to survive in soot-darkened environments (27.5 percent) as was *typical* (12.7 percent). He worried, however, that his results might be an artifact of his experimental procedures. Perhaps the traps used were more attractive to one type of moth, that one form of moth was more likely to migrate, or that one type of moth just lived longer. He eliminated the first alternative by showing that the recapture rates were the same for both types of trap. The use of natural boundaries and traps placed beyond those boundaries eliminated the second, and previous experiments had shown no differences in longevity. Further experiments in polluted environments confirmed that *carbonaria* was twice as likely to survive as *typical*. An experiment in an unpolluted environment showed that *typical* was three times as likely to survive as *carbonaria*. Kettlewell concluded that such selection was the cause of the prevalence of *carbonaria* in polluted environments.

Rudge also demonstrates that the strategies used by Kettlewell are those described above in the epistemology of experiment. His examples are given in Table 1. (For more details see Rudge 1998).

Epistemological strategies	Examples from Kettlewell
1. Experimental checks and calibration in which the apparatus reproduces known phenomena.	Use of the scoring experiment to verify that the proposed scoring methods would be feasible and objective.
2. Reproducing artifacts that are known in advance to be present.	Analysis of recapture figures for endemic *betularia* populations.
3. Elimination of plausible sources of background and alternative explanations of the result.	Use of natural barriers to minimize migration.
4. Using the results themselves to argue for their validity.	Filming the birds preying on the moths.
5. Using an independently well-corroborated theory of the phenomenon to explain the results.	Use of Ford's theory of the spread of industrial melanism.
6. Using an apparatus based on a well- corroborated theory.	Use of Fisher, Ford, and Shepard techniques. [The mark-release-capture method had been used in several earlier experiments]
7. Using statistical arguments.	Use and analysis of large numbers of moths.
8. Blind analysis	Not used.
9. Intervention, in which the experimenter manipulates the object under observation	Not present
10. Independent confirmation using different experiments.	Use of two different types of traps to recapture the moths.

Table 1. Examples of epistemological strategies used by experimentalists in evolutionary biology, from H.B.D. Kettlewell's (1955, 1956, 1958) investigations of industrial melanism. (See Rudge 1998).

2.5.2 The Meselson-Stahl Experiment: “The Most Beautiful Experiment in Biology”

The roles that experiment plays in physics are also those it plays in biology. In the previous section we have seen that Kettlewell's experiments both test and confirm a theory. I discussed earlier a set of crucial experiments that decided between two competing classes of theories, those that conserved parity and those that did not. In this section I will discuss an experiment that decided among three competing mechanisms for the replication of DNA, the molecule now believed to be responsible for heredity. This is another crucial experiment. It strongly supported one proposed mechanism and argued against the other two. (For details of this episode see (Holmes 2001)).

In 1953 Francis Crick and James Watson proposed a three-dimensional structure for deoxyribonucleic acid (DNA) (Watson and Crick 1953a). Their proposed structure consisted of two polynucleotide chains helically wound about a common axis. This was the famous “Double Helix”. The chains were bound together by combinations of four nitrogen bases — adenine, thymine, cytosine, and guanine. Because of structural requirements only the base pairs adenine-thymine and cytosine-guanine are allowed. Each chain is thus complementary to the other. If there is an adenine base at a location in one chain there is a thymine base at the same location on the other chain, and vice versa. The same applies to cytosine and guanine. The order of the bases along a chain is not, however, restricted in any way, and it is the precise sequence of bases that carries the genetic information.

The significance of the proposed structure was not lost on Watson and Crick when they made their suggestion. They remarked, “It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material."

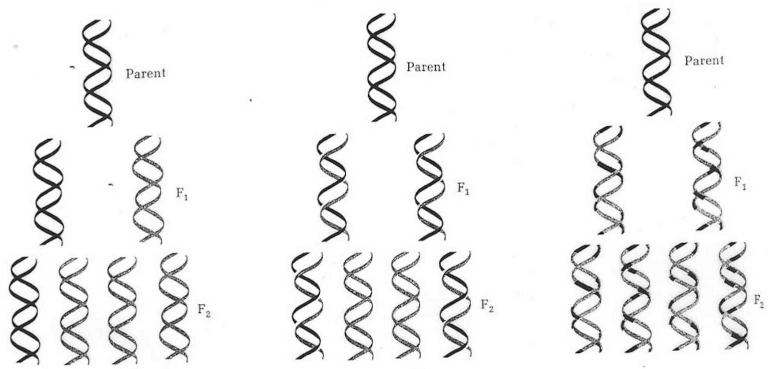

Figure 21: Possible mechanisms for DNA replication. (Left) Conservative replication. Each of the two strands of the parent DNA is replicated to yield the unchanged parent DNA and one newly synthesized DNA. The second generation consists of one parent DNA and three new DNAs. (Center) Semiconservative replication. Each first generation DNA molecule contains one strand of the parent DNA and one newly synthesized strand. The second generation consists of two hybrid DNAs and two new DNAs. (Right) Dispersive replication. The parent chains break at intervals, and the parental segments combine with new segments to form the daughter chains. The darker segments are parental DNA and the lighter segments are newly synthesized DNA. From Lehninger (1975).

If DNA was to play this crucial role in genetics, then there must be a mechanism for the replication of the molecule. Within a short period of time following the Watson-Crick suggestion, three different mechanisms for the replication of the DNA molecule were proposed (Delbruck and Stent 1957). These are illustrated in Figure 21. The first, proposed by Gunther Stent and known as conservative replication, suggested that each

of the two strands of the parent DNA molecule is replicated in new material. This yields a first generation which consists of the original parent DNA molecule and one newly-synthesized DNA molecule. The second generation will consist of the parental DNA and three new DNAs.

The second proposed mechanism, known as semiconservative replication is when each strand of the parental DNA acts as a template for a second newly-synthesized complementary strand, which then combines with the original strand to form a DNA molecule. This was proposed by Watson and Crick (1953b). The first generation consists of two hybrid molecules, each of which contains one strand of parental DNA and one newly synthesized strand. The second generation consists of two hybrid molecules and two totally new DNAs. The third mechanism, proposed by Max Delbruck, was dispersive replication, in which the parental DNA chains break at intervals and the parental segments combine with new segments to form the daughter strands.

In this section I will discuss the experiment performed by Matthew Meselson and Franklin Stahl, which has been called "the most beautiful experiment in biology", and which was designed to answer the question of the correct DNA replication mechanism (Meselson and Stahl 1958). Meselson and Stahl described their proposed method. "We anticipated that a label which imparts to the DNA molecule an increased density might permit an analysis of this distribution by sedimentation techniques. To this end a method was developed for the detection of small density differences among macromolecules. By use of this method, we have observed the distribution of the heavy nitrogen isotope ^{15}N among molecules of DNA following the transfer of a uniformly ^{15}N-labeled, exponentially growing bacterial population to a growth medium containing the ordinary nitrogen isotope ^{14}N" (Meselson and Stahl 1958, pp. 671-672).

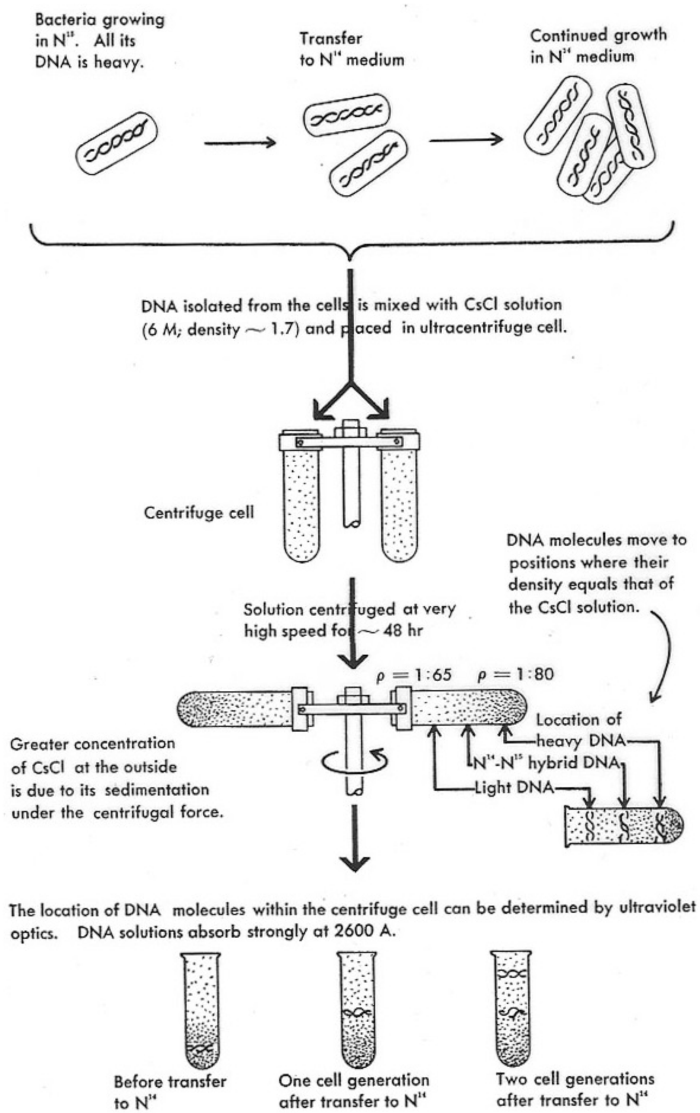

Figure 22: Schematic representation of the Meselson-Stahl experiment. From Watson (1965).

The experiment is described schematically in Figure 22. Meselson and Stahl placed a sample of DNA in a solution of cesium chloride. As the sample is rotated at high speed the denser material travels further away from the axis of rotation than does the less dense material. This results in a solution of cesium chloride that has increasing density as one goes further away from the axis of rotation. The DNA reaches equilibrium at the position where its density equals that of the solution. Meselson and Stahl grew *E. coli* bacteria in a medium that contained ammonium chloride (NH_4Cl) as the sole source of nitrogen. They did this for media that contained either ^{14}N, ordinary nitrogen, or ^{15}N, a heavier isotope. By destroying the cell membranes they could obtain samples of DNA which contained either ^{14}N or ^{15}N. They first showed that they could indeed separate the two different mass molecules of DNA by centrifugation (Figure 23). The separation of the two types of DNA is clear in both the photograph obtained by absorbing ultraviolet light and in the graph showing the intensity of the signal, obtained with a densitometer. In addition, the separation between the two peaks suggested that they would be able to distinguish an intermediate band composed of hybrid DNA from the heavy and light bands. These early results argued both that the experimental apparatus was working properly and that all of the results obtained were correct. It is difficult to imagine either an apparatus malfunction or a source of experimental background that could reproduce those results. This is similar, although certainly not identical, to Galileo's observation of the moons of Jupiter or to Millikan's measurement of the charge of the electron. In both of those episodes it was the results themselves that argued for their correctness.

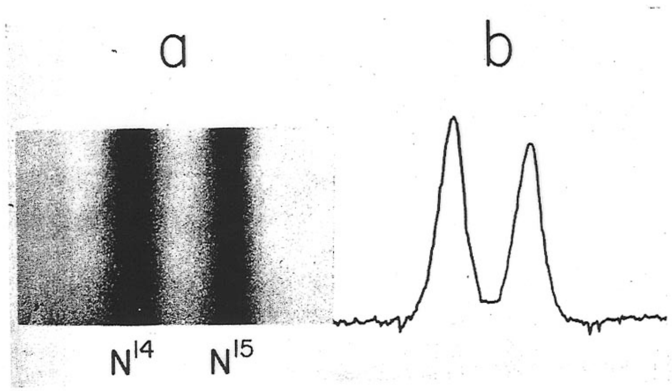

Figure 23: The separation of ^{14}N DNA from ^{15}N DNA by centrifugation. The band on the left is ^{14}N DNA and that on the right is from ^{15}N DNA. From Meselson and Stahl (1958).

Meselson and Stahl then produced a sample of *E coli* bacteria containing only ^{15}N by growing it in a medium containing only ammonium chloride with ^{15}N ($^{15}NH_4Cl$) for fourteen generations. They then abruptly changed the medium to ^{14}N by adding a tenfold excess of $^{14}NH_4CL$. Samples were taken just before the addition of ^{14}N and at intervals afterward for several generations. The cell membranes were broken to release the DNA into the solution and the samples were centrifuged and ultraviolet absorption photographs taken. In addition, the photographs were scanned with a recording densitometer. The results are shown in Figure 24, showing both the photographs and the densitometer traces. The figure shows that one starts only with heavy (fully-labeled) DNA. As time proceeds one sees more and more half-labeled DNA, until at one generation time only half-labeled DNA is present. "Subsequently only half labeled DNA and completely unlabeled DNA are found. When two generation times have elapsed after the addition of ^{14}N half-labeled and

unlabeled DNA are present in equal amounts" (p. 676). (This is exactly what the semiconservative replication mechanism predicts). By four generations the sample consists almost entirely of unlabeled DNA. A test of the conclusion that the DNA in the intermediate density band was half labeled was provided by examination of a sample containing equal amounts of generations 0 and 1.9. If the semiconservative mechanism is correct then Generation 1.9 should have approximately equal amounts of unlabeled and half-labeled DNA, whereas Generation 0 contains only fully-labeled DNA. As one can see, there are three clear density bands and Meselson and Stahl found that the intermediate band was centered at (50 ± 2) percent of the difference between the ^{14}N and ^{15}N bands, shown in the bottom photograph (Generations 0 and 4.1). This is precisely what one would expect if that DNA were half labeled.

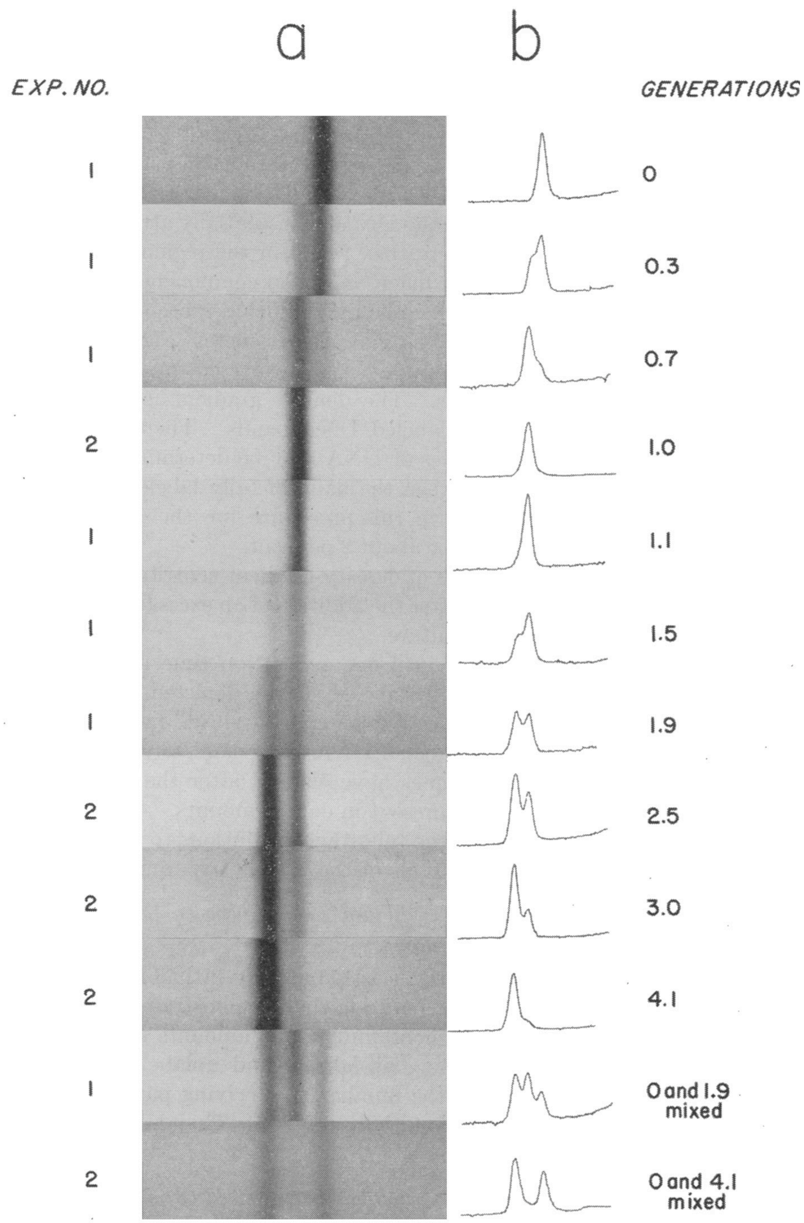

Figure 24: (Left) Utraviolet absorption photographs showing DNA bands from centrifugation of DNA from *E. Coli* sampled at various times after the addition of an excess of ^{14}N substrates to a growing ^{15}N culture. (Right) Densitometer traces of the photographs. The initial sample is all heavy (^{15}N DNA). As time proceeds a second intermediate band begins to appear until at one generation all of the sample is of intermediate mass (Hybrid DNA). At longer times a band of light DNA appears, until at 4.1 generations the sample is almost all lighter DNA. This is exactly what is predicted by the Watson-Crick semiconservative mechanism. From Meselson and Stahl (1958)

Meselson and Stahl stated their results as follows, "The nitrogen of DNA is divided equally between two subunits which remain intact through many generations…. Following replication, each daughter molecule has received one parental subunit" (p. 676).

Meselson and Stahl also noted the implications of their work for deciding among the proposed mechanisms for DNA replication. In a section labeled "The Watson-Crick Model" they noted that, "This [the structure of the DNA molecule] suggested to Watson and Crick a definite and structurally plausible hypothesis for the duplication of the DNA molecule. According to this idea, the two chains separate, exposing the hydrogen-bonding sites of the bases. Then, in accord with base-pairing restrictions, each chain serves as a template for the synthesis of its complement. Accordingly, each daughter molecule contains one of the parental chains paired with a newly synthesized chain…. The results of the present experiment are in exact accord with the expectations of the Watson-Crick model for DNA replication" (pp. 677-678).

It also showed that the dispersive replication mechanism proposed by Delbruck, which had smaller subunits, was incorrect. "Since the apparent molecular weight of the subunits so obtained is found to be close to half that of the intact molecule, it may be further concluded that the subunits of the DNA molecule which are conserved at duplication are single, continuous structures. The scheme for DNA duplication proposed by Delbruck is thereby ruled out" (p. 681). Later work by John Cairns and others showed that the subunits of DNA were the entire single polynucleotide chains of the Watson-Crick model of DNA structure.

The Meselson-Stahl experiment is a crucial experiment in biology. It decided between three proposed mechanisms for the replication of DNA. It supported the Watson-Crick semiconservative mechanism and eliminated the conservative and dispersive mechanisms. It played a similar role in biology to that of the experiments that demonstrated the nonconservation of parity did in physics. Thus, we have seen evidence that experiment plays similar roles in both biology and physics and also that the same epistemological strategies are used in both disciplines.

3. Conclusion

In this essay varying views on the nature of experimental results have been presented. Some argue that the acceptance of experimental results is based on epistemological arguments, whereas others base acceptance on future utility, social interests, or agreement with existing community commitments. Everyone agrees , however, that for whatever reasons, a consensus is reached on experimental results. These results then play many important roles in physics and we have examined several of these roles, although certainly not all of them. We have seen experiment deciding between two competing theories, calling for a new theory, confirming a theory, refuting a theory, providing evidence that determined the mathematical form of a theory, and providing evidence for the existence of an elementary particle involved in an accepted theory. We have also seen that experiment has a life of its own, independent of theory. If, as I believe, epistemological procedures provide grounds for reasonable belief in experimental results, then experiment can legitimately play the roles I have discussed and can provide the basis for scientific knowledge.

Bibliography

Principal Works:

- Ackermann, R. 1985. *Data, Instruments and Theory*. Princeton, N.J.: Princeton University Press.
- —–. 1991. "Allan Franklin, Right or Wrong". *PSA 1990, Volume 2*. A. Fine, M. Forbes and L. Wessels (Ed.). East Lansing, MI, Philosophy of Science Association:451-457.
- Adelberger, E.G. 1989. "High-Sensitivity Hillside Results from the Eot-Wash Experiment". *Tests of Fundamental Laws in Physics: Ninth Moriond Workshop*. O. Fackler and J. Tran Thanh Van (Ed.). Les Arcs, France, Editions Frontieres:485-499.
- Anderson, M.H., J.R. Ensher, M.R. Matthews, et al. 1995. "Observation of Bose-Einstein Condensation in a Dilute Atomic Vapor". *Science* 269: 198-201.
- Bell, J.S. and J. Perring 1964. "2pi Decay of the K_2o Meson". *Physical Review Letters* 13: 348-349.
- Bennett, W.R. 1989. "Modulated-Source Eotvos Experiment at Little Goose Lock". *Physical Review Letters* 62: 365-368.
- Bizzeti, P.G., A.M. Bizzeti-Sona, T. Fazzini, et al. 1989a. "Search for a Composition Dependent Fifth Force: Results of the Vallambrosa Experiment". *Tran Thanh Van, J*. O. Fackler (Ed.). Gif Sur Yvette, Editions Frontieres.
- Bizzeti, P.G., A.M. Bizzeti-Sona, T. Fazzini, et al. 1989b. "Search for a Composition-dependent Fifth Force". *Physical Review Letters* 62: 2901-2904.

- Bose, S. 1924. “Plancks Gesetz und Lichtquantenhypothese”. *Zeitschrift fur Physik* 26(1924): 178-181.
- Burnett, K. 1995. “An Intimate Gathering of Bosons”. *Science* 269: 182-183.
- Cartwright, N. 1983. *How the Laws of Physics Lie*. Oxford: Oxford University Press.
- Chase, C. 1929. “A Test for Polarization in a beam of Electrons by Scattering”. *Physical Review* 34: 1069-1074.
- —–. 1930. “The Scattering of Fast Electrons by Metals. II. Polarization by Double Scattering at Right Angles”. *Physical Review* 36: 1060-1065.
- Christenson, J.H., J.W. Cronin, V.L. Fitch, et al. 1964. “Evidence for the 2pi Decay of the $K^o{}_2$ Meson”. *Physical Review Letters* 13: 138-140.
- Collins, H. 1985. *Changing Order: Replication and Induction in Scientific Practice*. London: Sage Publications.
- —–. 1994. “A Strong Confirmation of the Experimenters' Regress”. *Studies in History and Philosophy of Modern Physics* 25(3): 493-503.
- Collins, H. and Pinch, T. 1993. *The Golem: What Everyone Should Know About Science*. Cambridge: Cambridge University Press.
- Conan Doyle, A. 1967. “The Sign of Four”. *The Annotated Sherlock Holmes*. W. S. Barrington-Gould (Ed.). New York: Clarkson N. Potter.
- Cowsik, R., N. Krishnan, S.N. Tandor, et al. 1988. “Limit on the Strength of Intermediate-Range Forces Coupling to Isospin”. *Physical Review Letters* 61(2179-2181).
- Cowsik, R., N. Krishnan, S.N. Tandor, et al. 1990. “Strength of Intermediate-Range Forces Coupling to Isospin”. *Physical Review Letters* 64: 336-339.
- de Groot, S.R. and H.A. Tolhoek 1950. “On the Theory of Beta-Radioactivity I: The Use of Linear Combinations of Invariants in the Interaction Hamiltonian”. *Physica* 16: 456-480.
- Delbruck, M. and G. S. Stent 1957. On the Mechanism of DNA Replication. The Chemical Basis of Heredity. W. D. McElroy and B. Glass. Baltimore: Johns Hopkins Press: 699-736.
- Dymond, E.G. 1931. “Polarisation of a Beam of Electrons by Scattering”. *Nature* 128: 149.
- —–. 1932. “On the Polarisation of Electrons by Scattering”. *Proceedings of the Royal Society (London)* A136: 638-651.
- —–. 1934. “On the Polarization of Electrons by Scattering. II.”. *Proceedings of the Royal Society (London)* A145: 657-668.
- Einstein, A. 1924. “Quantentheorie des einatomigen idealen gases”. *Sitzungsberischte der Preussische Akademie der Wissenschaften, Berlin*: 261-267.
- —–. 1925. “Quantentheorie des einatomigen idealen gases”. *Sitzungsberichte der Preussische Akadmie der Wissenschaften, Berlin*: 3-14.
- Everett, A.E. 1965. “Evidence on the Existence of Shadow Pions in K^+ Decay”. *Physical Review Letters* 14: 615-616.
- Fermi, E. 1934. “Attempt at a Theory of Beta-Rays”. *Il Nuovo Cimento* 11: 1-21.
- Feynman, R.P. and M. Gell-Mann 1958. “Theory of the Fermi Interaction”. *Physical Review* 109: 193-198.
- Feynman, R.P., R.B. Leighton and M. Sands 1963. *The Feynman Lectures on Physics*. Reading, MA: Addison-Wesley Publishing Company.
- Fierz, M. 1937. “Zur Fermischen Theorie des -Zerfalls”. *Zeitschrift fur Physik* 104: 553-565.
- Fischbach, E., S. Aronson, C. Talmadge, et al. 1986. “Reanalysis of the Eötvös Experiment”. *Physical Review Letters* 56: 3-6.
- Fitch, V.L. 1981. “The Discovery of Charge-Conjugation Parity

Asymmetry". *Science* 212: 989-993.

- Fitch, V.L., M.V. Isaila and M.A. Palmer 1988. "Limits on the Existence of a Material-dependent Intermediate-Range Force". *Physical Review Letters* 60: 1801-1804.
- Ford, E. B. 1937. "Problems of Heredity in the Lepidoptera." *Biological Reviews* **12**: 461-503.
- Ford, E. B. 1940. "Genetic Research on the Lepidoptera." *Annals of Eugenics* **10**: 227-252.
- Ford, K.W. 1968. *Basic Physics*. Lexington: Xerox.
- Franklin, A. 1986. *The Neglect of Experiment*. Cambridge: Cambridge University Press.
- —–. 1990. *Experiment, Right or Wrong*. Cambridge: Cambridge University Press.
- —–. 1991. "Do Mutants Have to Be Slain, or Do They Die of Natutal Causes." *PSA 1990, Volume 2*. A. Fine, M. Forbes, and L. Wessels. East Lansing, MI: Philosophy of Science Association, 2: 487-494.
- —–. 1993a. *The Rise and Fall of the Fifth Force: Discovery, Pursuit, and Justification in Modern Physics*. New York: American Institute of Physics.
- —–. 1993b. "Discovery, Pursuit, and Justification." *Perspectives on Science* 1:252-284.
- —–. 1994. "How to Avoid the Experimenters' Regress". *Studies in the History and Philosophy of Science* 25: 97-121.
- —–. 1995a. "The Resolution of Discordant Results". *Perspectives on Science* 3: 346-420.
- —–. 1995b. "Laws and Experiment". *Laws of Nature*. F. Weinert (Ed.). Berlin, De Gruyter:191-207.
- —–. 1996. "There Are No Antirealists in the Laboratory". *Realism and Anti-Realism in the Philosophy of Science*. R. S. Cohen, R. Hilpinen and Q. Renzong (Ed.). Dordrecht, Kluwer Academic Publishers**:** 131-148.
- —–. 1997a. "Calibration". *Perspectives on Science* 5: 31-80.
- —–. 1997b. "Recycling Expertise and Instrumental Loyalty". *Philosophy of Science* 64 (4 (Supp.)): S42-S52.
- —–. 1997c. "Are There Really Electrons? Experiment and Reality". *Physics Today* 50(10): 26-33.
- —–. 2002. *Selectivity and Discord: Two Problems of Experiment* Pittsburgh: University of Pittsburgh Press.
- Franklin, A. and C. Howson 1984. "Why Do Scientists Prefer to Vary Their Experiments?". *Studies in History and Philosophy of Science* 15: 51-62.
- Franklin, A. and C. Howson 1988. "It Probably is a Valid Experimental Result: A Bayesian Approach to the Epistemology of Experiment". *Studies in the History and Philosophy of Science* 19: 419-427.
- Friedman, J.L. and V.L. Telegdi 1957. "Nuclear Emulsion Evidence for Parity Nonconservation in the Decay Chain pi - mu-e". *Physical Review* 105: 1681-1682.
- Galison, P. 1987. *How Experiments End*. Chicago: University of Chicago Press.
- —–. 1997. *Image and Logic*. Chicago: University of Chicago Press.
- Gamow, G. and E. Teller 1936. "Selection Rules for the - Disintegration". *Physical Review* 49: 895-899.
- Garwin, R.L., L.M. Lederman and M. Weinrich 1957. "Observation of the Failure of Conservation of Parity and Charge Conjugation in Meson Decays: The Magnetic Moment of the Free Muon". *Physical Review* 105: 1415-1417.
- Gerlach, W. and O. Stern 1922a. "Der experimentelle Nachweis der Richtungsquantelung". *Zeitschrift fur Physik* 9: 349-352.
- Gerlach, W. and O. Stern 1924. "Uber die Richtungsquantelung im Magnetfeld". *Annalen der Physik* 74: 673-699.

- Glashow, S. 1992. "The Death of Science?" *The End of Science? Attack and Defense*. R.J. Elvee. Lanham, MD.: University Press of America
- Gooding, D. 1992. "Putting Agency Back Into Experiment". *Science as Practice and Culture*. A. Pickering (Ed.). Chicago, University of Chicago Press**:** 65-112.
- Hacking, I. 1981. "Do We See Through a Microscope". *Pacific Philosophical Quarterly* 63: 305-322.
- —–. 1983. *Representing and Intervening*. Cambridge: Cambridge University Press.
- —–. 1992. "The Self-Vindication of the Laboratory Sciences". *Science as Practice and Culture*. A. Pickering (Ed.). Chicago, University of Chicago Press:29-64.
- —–. 1999. *The Social Construction of What?* Cambridge, MA: Harvard University Press.
- Halpern, O. and J. Schwinger 1935. "On the Polarization of Electrons by Double Scattering". *Physical Review* 48: 109-110.
- Hamilton, D.R. 1947. "Electron-Neutrino Angular Correlation in Beta-Decay". *Physical Review* 71: 456-457.
- Hellmann, H. 1935. "Bemerkung zur Polarisierung von Elektronenwellen durch Streuung". *Zeitschrift fur Physik* 96: 247-250.
- Hermannsfeldt, W.B., R.L. Burman, P. Stahelin, et al. 1958. "Determination of the Gamow-Teller Beta-Decay Interaction from the Decay of Helium-6". *Physical Review Letters* 1: 61-63.
- Holmes, F. L. (2001). *Meselson, Stahl, and the Replication of DNA, A History of "The Most Beautiful Experiment in Biology"*. New Haven: Yale University Press.
- Kettlewell, H. B. D. (1955). "Selection Experiments on Industrial Melanism in the Lepidoptera." *Heredity* **9**: 323-342.
- Kettlewell, H. B. D. (1956). "Further Selection Experiments on Industrial Melanism in the Lepidoptera." *Heredity* **10**: 287-301.
- Kettlewell, H. B. D. (1958). "A Survey of the Frequencies of *Biston betularia* (L.) (Lep.) and its Melanic Forms in Great Britain." *Heredity* **12**: 51-72.
- Kofoed-Hansen, O. 1955. "Neutrino Recoil Experiments". *Beta- and Gamma-Ray Spectroscopy*. K. Siegbahn (Ed.). New York, Interscience:357-372.
- Konopinski, E. and G. Uhlenbeck 1935. "On the Fermi Theory of Radioactivity". *Physical Review* 48: 7-12.
- Konopinski, E.J. and L.M. Langer 1953. "The Experimental Clarification of the Theory of -Decay". *Annual Reviews of Nuclear Science* 2: 261-304.
- Konopinski, E.J. and G.E. Uhlenbeck 1941. "On the Theory of Beta-Radioactivity". *Physical Review* 60: 308-320.
- Langer, L.M., J.W. Motz and H.C. Price 1950. "Low Energy Beta-Ray Spectra: Pm^{147} S^{35}". *Physical Review* 77: 798-805.
- Langer, L.M. and H.C. Price 1949. "Shape of the Beta-Spectrum of the Forbidden Transition of Yttrium 91". *Physical Review* 75: 1109.
- Langstroth, G.O. 1932. "Electron Polarisation". *Proceedings of the Royal Society (London)* A136: 558-568.
- LaRue, G.S., J.D. Phillips, and W.M. Fairbank. "Observation of Fractional Charge of (1/3)*e* on Matter". *Physical Review Letters* 46: 967-970.
- Latour, B. and S. Woolgar. 1979. *Laboratory Life: The Social Construction of Scientific Facts*. Beverly Hills: Sage.
- Latour, B. and S. Woolgar. 1986. *Laboratory Life: The Construction of Scientific Facts*. Princeton: Princeton University Press.
- Lee, T.D. and C.N. Yang 1956. "Question of Parity Nonconservation in Weak Interactions". *Physical Review* 104: 254-258.
- Lehninger, A. L. 1975. *Biochemistry*. New York: Worth Publishers.
- Lynch, M. 1991. "Allan Franklin's Transcendental Physics." *PSA*

1990, Volume 2. A. Fine, M. Forbes, and L. Wessels. East Lansing, MI: Philosophy of Science Association, 2: 471-485.

- MacKenzie, D. 1989. "From Kwajelein to Armageddon? Testing and the Social Construction of Missile Accuracy". *The Uses of Experiment*. D. Gooding, T. Pinch and S. Shaffer (Ed.). Cambridge, Cambridge University Press: 409-435.
- Mayer, M.G., S.A. Moszkowski and L.W. Nordheim 1951. "Nuclear Shell Structure and Beta Decay. I. Odd A Nuclei". *Reviews of Modern Physics* 23: 315-321.
- McKinney, W. (1992). Plausibility and Experiment: Investigations in the Context of Pursuit. History and Philosophy of Science. Bloomington, IN, Indiana.
- Mehra, J. and H. Rechenberg 1982. *The Historical Development of Quantum Theory*. New York: Springer-Verlag.
- Meselson, M. and F. W. Stahl 1958. "The Replication of DNA in Escherichia Coli." *Proceedings of the National Academy of Sciences (USA)* 44: 671-682.
- Millikan, R.A. 1911. "The Isolation of an Ion, A Precision Measurement of Its Charge, and the Correction of Stokes's Law". *Physical Review* 32: 349-397.
- Morrison, M. 1990. "Theory, Intervention, and Realism". *Synthese* 82: 1-22.
- Mott, N.F. 1929. "Scattering of Fast Electrons by Atomic Nuclei". *Proceedings of the Royal Society (London)* A124: 425-442.
- —–. 1931. "Polarization of a Beam of Electrons by Scattering". *Nature*
- Nelson, A. 1994. "How Could Scientific Facts be Socially Constructed?". *Studies in History and Philosophy of Science* 25(4): 535-547.
- —–. 1932. "Tha Polarisation of Electrons by Double Scattering". *Proceedings of the Royal Society (London)* A135: 429-458.
- Nelson, P.G., D.M. Graham and R.D. Newman 1990. "Search for an Intermediate-Range Composition-dependent Force Coupling to N-Z". *Physical Review D* 42: 963-976.
- Nelson, A. 1994. "How Could Scientific Facts be Socially Constructed?". *Studies in History and Philosophy of Science* 25(4): 535-547.
- Newman, R., D. Graham and P. Nelson 1989. "A "Fifth Force" Search for Differential Accleration of Lead and Copper toward Lead". *Tests of Fundamental Laws in Physics: Ninth Moriond Workshop*. O. Fackler and J. Tran Thanh Van (Ed.). Gif sur Yvette, Editions Frontieres:459-472.
- Nishijima, K. and M.J. Saffouri 1965. "CP Invariance and the Shadow Universe". *Physical Review Letters* 14: 205-207.
- Pais, A. 1982. *Subtle is the Lord…* Oxford: Oxford University Press.
- Pauli, W. 1933. "Die Allgemeinen Prinzipen der Wellenmechanik". *Handbuch der Physik* 24: 83-272.
- Petschek, A.G. and R.E. Marshak 1952. "The -Decay of Radium E and the Pseusoscalar Interaction". *Physical Review* 85: 698-699.
- Pickering, A. 1981. "The Hunting of the Quark". *Isis* 72: 216-236.
- —–. 1984a. *Constructing Quarks*. Chicago: University of Chicago Press.
- —–. 1984b. "Against Putting the Phenomena First: The Discovery of the Weak Neutral Current". *Studies in the History and Philosophy of Science* 15: 85-117.
- —–. 1987. "Against Correspondence: A Constructivist View of Experiment and the Real". *PSA 1986*. A. Fine and P. Machamer (Ed.). Pittsburgh, Philosophy of Science Association. 2: 196-206.
- —–. 1989. "Living in the Material World: On Realism and Experimental Practice." *The Uses of Experiment*. D. Gooding, T. Pinch and S. Schaffer (Ed.). Cambridge, Cambridge University Press: 275-297.

- ——. 1991. "Reason Enough? More on Parity Violation Experiments and Electroweak Guage Theory." *PSA 1990, Volume 2*. A. Fine, M. Forbes, and L. Wessels. East Lansing, MI: Philosophy of Science Association, 2: 459-469.
- ——. 1995. *The Mangle of Practice*. Chicago: University of Chicago Press.
- Prentki, J. 1965. *CP Violation*. Oxford International Conference on Elementary Particles, Oxford, England.
- Pursey, D.L. 1951. "The Interaction in the Theory of Beta Decay". *Philosophical Magazine* 42: 1193-1208.
- Raab, F.J. 1987. "Search for an Intermediate-Range Interaction: Results of the Eot-Wash I Experiment". *New and Exotic Phenomena: Seventh Moriond Workshop*. O. Fackler and J. Tran Thanh Van (Ed.). Les Arcs, France, Editions Frontieres: 567-577.
- Randall, H.M., R.G. Fowler, N. Fuson, et al. 1949. *Infrared Determination of Organic Structures*. New York: Van Nostrand.
- Richter, H. 1937. "Zweimalige Streuung schneller Elektronen". *Annalen der Physik* 28: 533-554.
- Ridley, B.W. (1954). Nuclear Recoil in Beta Decay. Physics. Cambridge, Cambridge University.
- Rose, M.E. and H.A. Bethe 1939. "On the Absence of Polarization in Electron Scattering". *Physical Review* 55: 277-289.
- Rudge, D. W. (1998). "A Bayesian Analysis of Strategies in Evolutionary Biology." *Perspectives on Science* **6**: 341-360.
- Rudge, D. W. (2001). "Kettlewell from an Error Statistician's Point of View." *Perspectives on Science* **9**: 59-77.
- Rupp, E. 1929. "Versuche zur Frage nach einer Polarisation der Elektronenwelle". *Zetschrift fur Physik* 53: 548-552.
- ——. 1930a. "Ueber eine unsymmetrische Winkelverteilung zweifach reflektierter Elektronen". *Zeitschrift fur Physik* 61: 158-169.
- ——. 1930b. "Ueber eine unsymmetrische Winkelverteilung zweifach reflektierter Elektronen". *Naturwissenschaften* 18: 207.
- ——. 1931. "Direkte Photographie der Ionisierung in Isolierstoffen". *Naturwissenschaften* 19: 109.
- ——. 1932a. "Versuche zum Nachweis einer Polarisation der Elektronen". *Physickalsche Zeitschrift* 33: 158-164.
- ——. 1932b. "Neure Versuche zur Polarisation der Elektronen". *Physikalische Zeitschrift* 33: 937-940.
- ——. 1932c. "Ueber die Polarisation der Elektronen bei zweimaliger 90^{o} - Streuung". *Zeitschrift fur Physik* 79: 642-654.
- ——. 1934. "Polarisation der Elektronen an freien Atomen". *Zeitschrift fur Physik* 88: 242-246.
- Rustad, B.M. and S.L. Ruby 1953. "Correlation between Electron and Recoil Nucleus in He^6 Decay". *Physical Review* 89: 880-881.
- Rustad, B.M. and S.L. Ruby 1955. "Gamow-Teller Interaction in the Decay of He^6". *Physical Review* 97: 991-1002.
- Sargent, B.W. 1932. "Energy Distribution Curves of the Disintegration Electrons". *Proceedings of the Cambridge Philosophical Society* 24: 538-553.
- ——. 1933. "The Maximum Energy of the -Rays from Uranium X and other Bodies". *Proceeddings of the Royal Society (London)* A139: 659-673.
- Sauter, F. 1933. "Ueber den Mottschen Polarisationseffekt bei der Streuun von Elektronen an Atomen". *Annalen der Physik* 18: 61-80.
- Sellars, W. 1962. *Science, Perception, and Reality*. New York: Humanities Press.
- Sherr, R. and J. Gerhart 1952. "Gamma Radiation of C^{10}". *Physical Review* 86: 619.
- Sherr, R., H.R. Muether and M.G. White 1949. "Radioactivity of C^{10} and O^{14}". *Physical Review* 75: 282-292.
- Smith, A.M. 1951. "Forbidden Beta-Ray Spectra". *Physical Review* 82: 955-956.

- Staley, K. 1999 “Golden Events and Statistics: What's Wrong with Galison's Image/Logic Distinction.” *Perspectives on Science* 7: 196-230.
- Stern, O. 1921. “Ein Weg zur experimentellen Prufung Richtungsquantelung im Magnet feld”. *Zeitschrift fur Physik* 7: 249-253.
- Stubbs, C.W., E.G. Adelberger, B.R. Heckel, et al. 1989. “Limits on Composition-dependent Interactions using a Laboratory Source: Is There a ”Fifth Force?“”. *Physical Review Letters* 62: 609-612.
- Stubbs, C.W., E.G. Adelberger, F.J. Raab, et al. 1987. “Search for an Intermediate-Range Interaction”. *Physical Review Letters* 58: 1070-1073.
- Sudarshan, E.C.G. and R.E. Marshak 1958. “Chirality Invariance and the Universal Fermi Interaction”. *Physical Review* 109: 1860-1862.
- Thieberger, P. 1987a. “Search for a Substance-Dependent Force with a New Differential Accelerometer”. *Physical Review Letters* 58: 1066-1069.
- Thomson, G.P. 1933. “Polarisation of Electrons”. *Nature* 132: 1006.
- ——. 1934. “Experiment on the Polarization of Electrons”. *Philosophical Magazine* 17: 1058-1071.
- Thomson, J.J. 1897. “Cathode Rays”. *Philosophical Magazine* 44: 293-316.
- Uhlenbeck, G.E. and S. Goudsmit 1925. “Ersetzung der Hypothese von unmechanischen Zwang durch eine Forderung bezuglich des inneren Verhaltens jedes einzelnen Elektrons”. *Naturwissenschaften* 13: 953-954.
- Uhlenbeck, G.E. and S. Goudsmit 1926. “Spinning Electrons and the Structure of Spectra”. *Nature* 117: 264-265.
- van Fraassen, B. 1980. *The Scientific Image*. Oxford: Clarendon Press.
- Watson, J. D. 1965. *Molecular Biology of the Gene*. New York: W.A. Benjamin, Inc.
- Watson, J. D. and F. H. C. Crick (1953a). “A Structure for Deoxyribose Nucleic Acid.” *Nature* 171: 737.
- Watson, J. D. and F. H. C. Crick (1953b). “Genetical Implications of the Structure of Deoxyribonucleic Acid.” *Nature* 171: 964-967.
- Weinert, F. 1995. “Wrong Theory—Right Experiment: The Significance of the Stren-Gerlach Experiments”. *Studies in History and Philosophy of Modern Physics* 26B(1): 75-86.
- Winter, J. 1936. “Sur la polarisation des ondes de Dirac”. *Academie des Science, Paris, Comptes rendus hebdomadaires des seances* 202: 1265-1266.
- Wu, C.S. 1955. “The Interaction in Beta-Decay”. *Beta- and Gamma-Ray Spectroscopy*. K. Siegbahn (Ed.). New York, Interscience: 314-356.
- Wu, C.S., E. Ambler, R.W. Hayward, et al. 1957. “Experimental Test of Parity Nonconservation in Beta Decay”. *Physical Review* 105: 1413-1415.
- Wu, C.S. and A. Schwarzschild (1958). A Critical Examination of the He^6 Recoil Experiment of Rustad and Ruby. New York, Columbia University.

Other Suggested Reading

- Ackermann, R. 1988. “Experiments as the Motor of Scientific Progress”. *Social Epistemology* 2: 327-335.
- Batens, D. and J.P. Van Bendegem, Eds. 1988. *Theory and Experiment*. Dordrecht: D. Reidel Publishing Company.
- Bogen, J. and J. Woodward 1988. “Saving the Phenomena”. *The Philosophical Review* 97: 303-352.
- Burian, R. M. (1992). “How the Choice of Experimental Organism Matters: Biological Practices and Discipline Boundaries.” *Synthese* **92**: 151-166.

- Burian, R. M. (1993). “How the Choice of Experimental Organism Matters: Epistemological Reflections on an Aspect of Biological Practice.” *Journal of the History of Biology* **26**: 351-367.
- Burian, R. M. (1993b). “Technique, Task Definition, and the Transition from Genetics to Molecular Genetics: Aspects of the Work on Protein Synthesis in the Laboratories of J. Monod and P. Zamecnik.” *Journal of the History of Biology* **26**: 387-407.
- Burian, R. M. (1995). Comments on Rheinberger. *Concepots, Theories, and Rationality in the Biological Sciences*. G. Wolters, J. G. Lennox and P. McLasughlin. Pittsburgh: University of Pittsburgh Press**:** 123-136.
- Gooding, D. 1990. *Experiment and the Making of Meaning*. Dordrecht: Kluwer Academic Publishers.
- Gooding, D., T. Pinch and S. Schaffer, Eds. 1989. *The Uses of Experiment*. Cambridge: Cambridge University Press.
- Koertge, N., Ed. 1998. *A House Built on Sand: Exposing Postmodernist Myths About Science*. Oxford: Oxford University Press.
- Nelson, A. 1994. “How Could Scientific Facts be Socially Constructed?”. *Studies in History and Philosophy of Science* 25(4): 535-547.
- Pickering, A., Ed. 1992. *Science as Practice and Culture*. Chicago: University of Chicago Press.
- Pickering, A. 1995. *The Mangle of Practice*. Chicago: University of Chicago Press.
- Pinch, T. 1986. *Confronting Nature*. Dordrecht: Reidel.
- Rasmussen, N. 1993. “Facts, Artifacts, and Mesosomes: Practicing Epistemology with the Electron Microscope”. *Studies in History and Philosophy of Science* 24: 227-265.
- Rheinberger, H.-J. (1997). *Toward a History of Epistemic Things*. Stanford: Stanford University Press.

- Shapere, D. 1982. “The Concept of Observation in Science and Philosophy”. *Philosophy of Science* 49: 482-525.
- Weber, M. (2005). *Philosophy of Experimental Biology*. Cambridge: Cambridge University Press.

Other Internet Resources

[Please contact the author with suggestions.]

Related Entries

confirmation | logic: inductive | rationalism vs. empiricism | scientific method | scientific realism

Acknowledgments

I am grateful to Professor Carl Craver for both his comments on the manuscript and for his suggestions for further reading.

Supplement to Experiment in Physics

Appendix 1: The Discovery of Parity Nonconservation

Let us consider first an episode in which the relation between theory and experiment was clear and straightforward. This was a "crucial" experiment, one that decided unequivocally between two competing theories, or classes of theory. The episode was that of the discovery that parity, mirror-reflection symmetry or left-right symmetry, is not conserved in the weak interactions. (For details of this episode see Franklin (1986, Ch. 1)). Parity conservation was a well-established and strongly-believed principle of physics. As students of introductory physics learn, if we wish to determine the magnetic force between two currents we first determine the direction of the magnetic field due to the first current, and then determine the force exerted on the second current by that field. We use two Right-Hand Rules. We get exactly the same answer, however, if we use two Left-Hand Rules, This is left-right symmetry, or parity conservation, in electromagnetism.

In the early 1950s physicists were faced with a problem known as the "τ-θ" puzzle. Based on one set of criteria, that of mass and lifetime, two elementary particles (the tau and the theta) appeared to be the same, whereas on another set of criteria, that of spin and intrinsic parity, they appeared to be different. T.D. Lee and C.N. Yang (1956) realized that the problem would be solved, and that the two particles would be different decay modes of the same particle, if parity were not conserved in the decay of the particles, a weak interaction. They examined the evidence for parity conservation and found, to their surprise, that although there was strong evidence that parity was conserved in the strong (nuclear) and electromagnetic interactions, there was, in fact, no supporting evidence that it was conserved in the weak interaction. It had never been tested.

Lee and Yang suggested several experiments that would test their hypothesis that parity was not conserved in the weak interactions. One was the β decay of oriented nuclei (Figure 1). Consider a collection of radioactive nuclei, all of whose spins point in the same direction. Suppose also that the electron given off in the radioactive decay of the nucleus is always emitted in a direction opposite to the spin of the nucleus In the mirror the electron is emitted in the same direction as the spin. The mirror image of the decay is different from the real decay. This would violate parity conservation, or mirror symmetry. Parity would be conserved only if, in the decay of a collection of nuclei, equal numbers of electrons were emitted in both directions. This was the experimental test performed by C.S. Wu and her collaborators (1957). They aligned

$Cobalt^{60}$ nuclei and counted the number of decay electrons in the two directions, along the nuclear spin and opposite to the spin. Their results are shown in Figure 2 and indicate clearly that more electrons are emitted opposite to the spin than along the spin. Parity is not conserved.

Two other experiments, reported at the same time, on the sequential decay pi meson decays to mu meson decays to electron also showed parity nonconservation (Friedman and Telegdi 1957; Garwin, Lederman and Weinrich 1957). These three experiments decided between two classes of theories--that is, between those theories that conserve parity and those that do not. They refuted the theories in which parity was conserved and supported or confirmed those in which it wasn't. These experiments also demonstrated that charge conjugation, or particle-antiparticle, symmetry was violated in the weak interactions and called for a new theory of decay and the weak interactions. It is fair to say that when a physicist learned the results of these experiments they were convinced that parity was not conserved in the weak interactions.

Return to Experiment in Physics

Supplement to Experiment in Physics

Figure 1

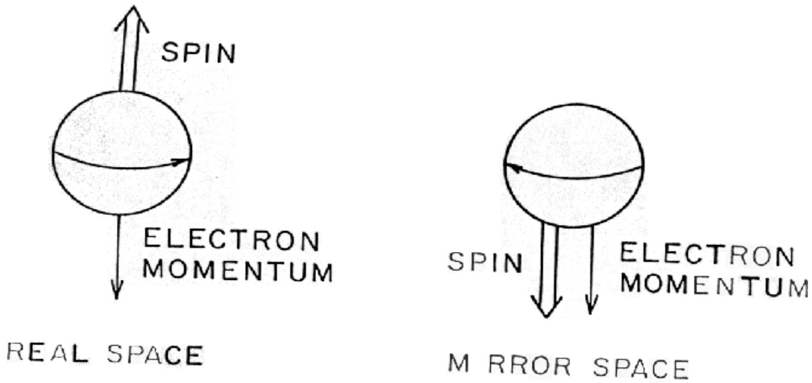

Figure 1. Nuclear spin and momentum of the decay electron in decay in both real space and in mirror space.

Supplement to Experiment in Physics

Figure 2

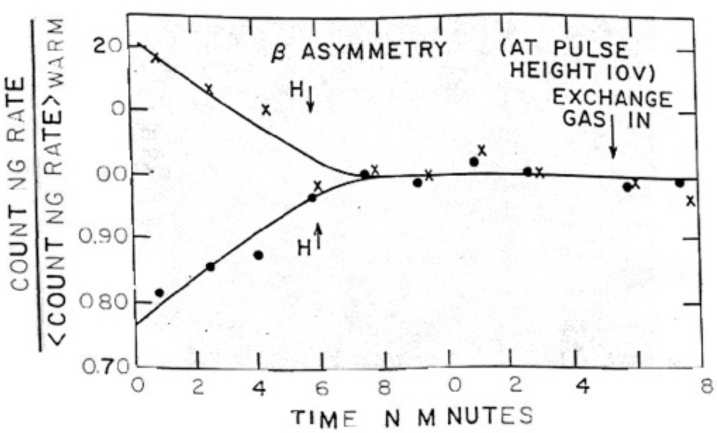

Figure 2. Relative counting rates for particles from the decay of oriented ^{60}Co nuclei for different nuclear orientations (field directions). There is a

clear asymmetry with more particles being emitted opposite to the spin direction. From Wu et al. (1957).

Supplement to Experiment in Physics

Appendix 2: The Discovery of CP Violation: A Persuasive Experiment

A group at Princeton University, led by Cronin and Fitch, decided to test CP conservation. The experimenters were quite aware of the relevance of their experiment to the question of CP violation, but they did not expect to observe it. As Val Fitch, one of the group leaders remarked, "Not many of our colleagues would have given credit for studying CP invariance, but we did so anyway" (Fitch 1981, p. 991). A preliminary estimate indicated that the CP phase of the experiment would detect about 7500 K_2^o decays and thus reduce the limit on CP violation from the then current limit of 1/300 (0.3%) to 1/7500 (For details of this episode see Franklin (1986, Ch. 3)).

The experimental beam contained only K_2^o mesons. (The K_1^o meson has a much shorter lifetime than the K_2^o meson, so that if we start with a beam containing both types of particles, after a time only the K_2^o mesons will remain). The experimental apparatus detected two charged particles from the decay of the K_2^o meson· The vector momentum of each of the two decay products from the K_2^o beam and the invariant mass m* were computed assuming that each product had the mass of a pion:

$$m^* = [(E_1 + E_2)^2 - (\mathbf{p}_1 + \mathbf{p}_2)^2]^{1/2},$$

where E and **p** are the energy and vector momenta of the pions, respectively. If both particles were indeed pions from K_2^o decay, m* would equal the K_2^o mass. The experimenters also computed the vector sum of the two momenta and the angle between this sum and the direction of the K_2^o beam. This angle should be zero for two-body decays, but not, in general, for three-body decays.

This was exactly what the Princeton group observed (Christenson et al. 1964). As seen clearly in Figure 3, there is a peak at the K^o mass, 498 MeV/c^2, for events with cos(θ) greater than 0.9999 (cos(θ) approximately equal to 1 means θ is approximately equal to 0). No such peak is seen in the mass regions just above or just below the K^o mass. The experimenters reported a total of 45 ± 9 two-pion K_2^o decays out of a total of 22,700 K_2^o decays. This was a branching ratio of (1.95 ± 0.2) x 10^{-3}, or approximately 0.2 percent.

The most obvious interpretation of the Princeton result was that CP symmetry was violated. This was the view taken in three out of four theoretical papers written during the period immediately following the report of that result. The Princeton result had persuaded most of the physics community that CP symmetry was violated. The remaining theoretical papers offered alternative explanations.[1] These alternatives relied on one or more of three arguments: (1) the Princeton results are caused by a CP asymmetry (the local preponderance of matter over antimatter) in the environment of the experiment, (2) K_2^o decay into two pions does not necessarily imply CP violation, and (3) the Princeton observations did not arise from two-pion K_2^o decay. This last argument can divided into the assertions that (3a) the decaying particle was not a K_2^o meson, (3b) the decay products were not pions, and (3c) another unobserved particle was emitted in the decay. Included in these alternatives were three suggestions that cast doubt on well-supported fundamental assumptions of modern physics. These were: (1) pions are not bosons, (2) the principle of superposition in quantum mechanics is violated, and (3) the exponential decay law fails. Although by the end of 1967 all of these alternatives had been experimentally tested and found

wanting, the majority of the physics community had accepted CP violation by the end of 1965, even though all the tests had not yet been completed. As Prentki, a theoretical particle physicist, remarked, this was because in some cases "the price one has to pay in order to save CP becomes extremely high," and because other alternatives were "even more unpleasant"(Prentki 1965).

This is an example of what one might call a pragmatic solution to the Duhem-Quine problem.[2] The alternative explanations and the auxiliary hypotheses were refuted, leaving CP violation unprotected. One might worry that other plausible alternatives were never suggested or considered. This is not a serious problem in the actual practice of physics. No fewer than ten alternative explanations of the Princeton result were offered, and not all of them were very plausible. Had others been suggested they, too, would have been considered by the physics community. Consider the model of Nishijima and Saffouri (1965). They explained two-pion K_2^o decay by the existence of a "shadow" universe in touch with our "real" universe only through the weak interactions. They attributed to the two pion decay observed to the decay of the $K^{o'}$ from the shadow universe. This implausible model was not merely considered, it was also experimentally tested. Everett (1965) noted that if the $K^{o'}$, the shadow K^o postulated by Nishijima and Saffouri existed, then a shadow pion should also exist, and the decays of the K^+ into a positive pion and a neutral pion and of the K^+ into a positive pion and a neutral shadow pion and should occur with equal rates. The presence of the shadow pion could be detected by measuring the ordinary K^+ branching ratio in two different experiments, one in which the neutral pion was detected and one in which it was not. If the shadow pion existed the two measurements would differ. They didn't. There was no shadow pion and thus, no $K^{o'}$.

What was the difference between the episodes of parity nonconservation and CP violation. In the former parity nonconservation was immediately accepted. No alternative explanations were offered. There was a convincing and decisive set of experiments. In the latter at least ten alternatives were proposed, and although CP violation was accepted rather quickly, the alternatives were tested. In both cases there are only two classes of theories, those that conserve parity or CP, and those that do not. The difference lies in the length and complexity of the derivation linking the hypothesis to the experimental result, or to the number of auxiliary hypotheses required for the derivation. In the case of parity nonconservation the experiment could be seen by inspection to violate mirror symmetry (See Figure 1). In the CP episode what was observed was K_2^o decay into two pions. In order to connect this observation to CP conservation one had to assume (1) the principle of superposition, (2) that the exponential decay law held to 300 lifetimes, (3) that the decay particles were both "real" pions and that pions were bosons, (4) that no other particle was emitted in the decay, (5) that no other similar particle was produced, and (6) that there were no external conditions present that might regenerate K_1^o mesons. It was these auxiliary assumptions that were tested and eliminated as alternative explanations by subsequent experiments.

The discovery of CP violation called for a theoretical explanation, a call that is still unanswered.

Return to Experiment in Physics

Supplement to Experiment in Physics

Figure 3

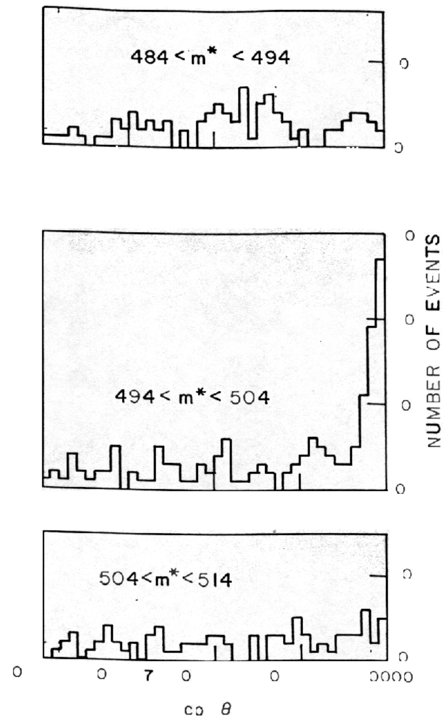

Figure 3. Angular distributions in three mass ranges for events with cos(theta) > 0.9995. From Christenson et al. (1964).

Supplement to Experiment in Physics

Appendix 3: The Discovery of Bose-Einstein Condensation: Confirmation After 70 Years

In both of the episodes discussed previously, those of parity nonconservation and of CP violation, we saw a decision between two competing classes of theories. This episode, the discovery of Bose-Einstein condensation (BEC), illustrates the confirmation of a specific theoretical prediction 70 years after the theoretical prediction was first made. Bose (1924) and Einstein (1924; 1925) predicted that a gas of noninteracting bosonic atoms will, below a certain temperature, suddenly develop a macroscopic population in the lowest energy quantum state.[1] An interesting aspect of this episode is that the phenomenon in question had never been observed previously. This raises an interesting epistemological problem. How do you know you have observed something that has never been seen before?

Elementary particles can be divided onto two classes: bosons with integral spin (0, 1 ,2, ...), and fermions with half-integral spin (1/2, 3/2, 5/2, ...). Fermions, such as electrons obey the Pauli Exclusion Principle. Two fermions cannot be in the same quantum mechanical state. This explains the shell structure of electrons in atoms and the periodic table. On the other hand, any number of bosons can occupy the same state. At sufficiently low temperatures, when thermal motions are very small, there is a strong tendency for a group of bosons to all go into the same state.

The experiment that first demonstrated the existence of BEC was done by Carl Wieman, Eric Cornell, and their collaborators (Anderson et al. 1995). The experimental apparatus is shown is Figure 4. In outline the experiment was as follows. A sample of ^{87}Rb atoms was cooled in a magneto-optical trap. It was then loaded into a magnetic trap and further cooled by evaporation. The condensate was formed and the trap removed, allowing the condensate to expand. The expanded condensate was illuminated with laser light and the resulting shadow of the cloud was imaged, digitized, and stored.[2]

The experimental results are shown in Figures 5 - 7. Figure 5 shows the velocity distribution of the rubidium gas cloud (a) just before the appearance of the condensate, (b) just after, and (c) after further evaporation of the cloud has left a sample of nearly pure condensate. This figure also shows the spatial distribution of the gas. Although the

measurement process destroyed the condensate sample, the entire process can be repeated so that one can measure the cloud at different stages. Figure 6 shows the peak density of the gas as a function of the RF frequency used to excite the atoms into a non-confined state and to assist the cooling by evaporation). There is a sharp increase in density at a frequency of 4.23 MHz. This indicates the appearance of Bose-Einstein condensation. As the sample is further cooled one expects to observe a two-component cloud with a dense central condensate surrounded by a diffuse non-condensate. This is seen clearly in both Figures 5 and 7. Figure 7 shows horizontal sections of the rubidium cloud. At 4.71 MHz, above the transition temperature, one sees only a broad thermal distribution. Beginning at 4.23 MHz one sees the appearance of a sharp central peak, the Bose-Einstein condensate, above the thermal distribution. At 4.11 MHz the cloud is almost a pure condensate.

There are three clear indications of the presence of Bose-Einstein condensation: (1) the velocity distribution of the gas shows two distinct components, (2) the sudden increase in density as the temperature decreases, and (3) the elliptical shape of the velocity distribution (Figure 5). The velocity distribution should be elliptical because for the harmonic trap used, the force in the z direction was eight times larger than in the x and y directions. No phenomenon other than Bose-Einstein condensation could plausibly explain these results

This result was sufficiently credible that Keith Burnett, an atomic physicist at Oxford University remarked, in the same issue of *Science* in which Wieman and Cornell reported their result, "In short, they have observed the phenomenon called Bose-Einstein condensation (BEC) in a gas of atoms for the first time. The term Holy Grail seems quite appropriate given the singular importance of this discovery" (Burnett 1995, p. 182).

A theoretical prediction had been confirmed after 70 years.

Return to Experiment in Physics

Supplement to Experiment in Physics

Figure 4

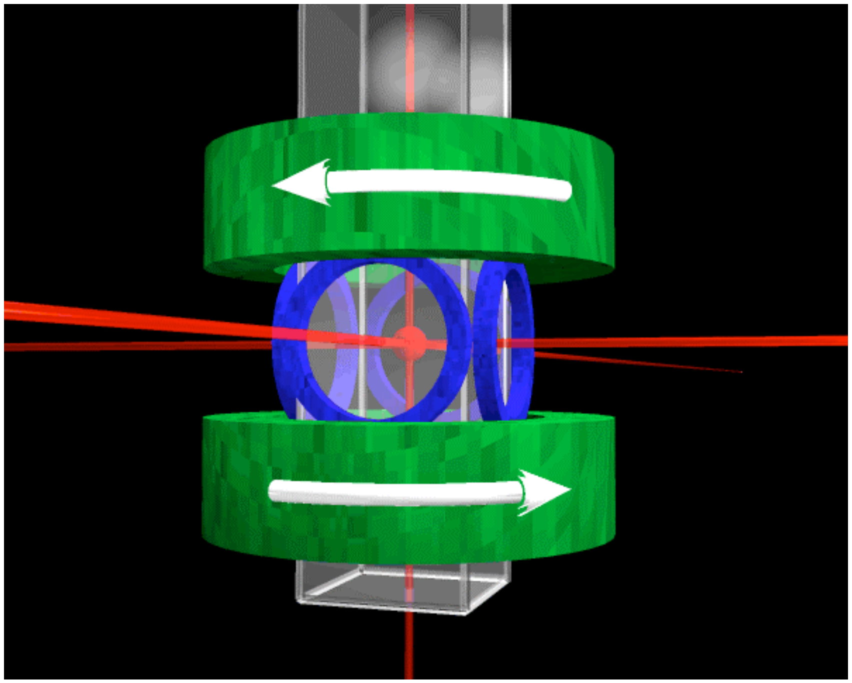

Figure 4. Schematic of the BEC apparatus. From Anderson et al. (1995).

Supplement to Experiment in Physics

Figure 5

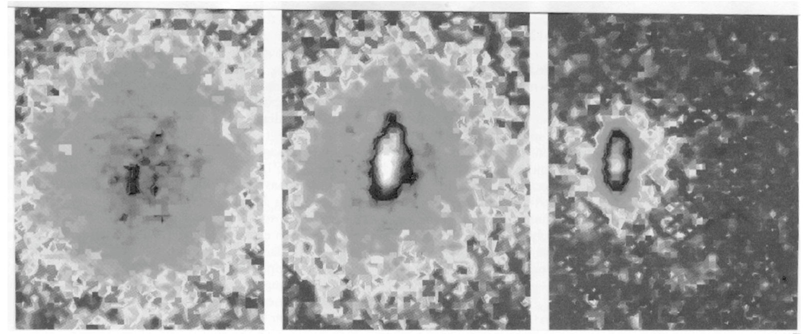

Figure 5. False color images of the velocity distribution of the rubidium BEC cloud (from the left): just before the appearance of the condensate, just after the appearance of the condensate, and after further evaporation has left a sample of nearly pure condensate. From Anderson et al. (1995).

Supplement to Experiment in Physics

Figure 6

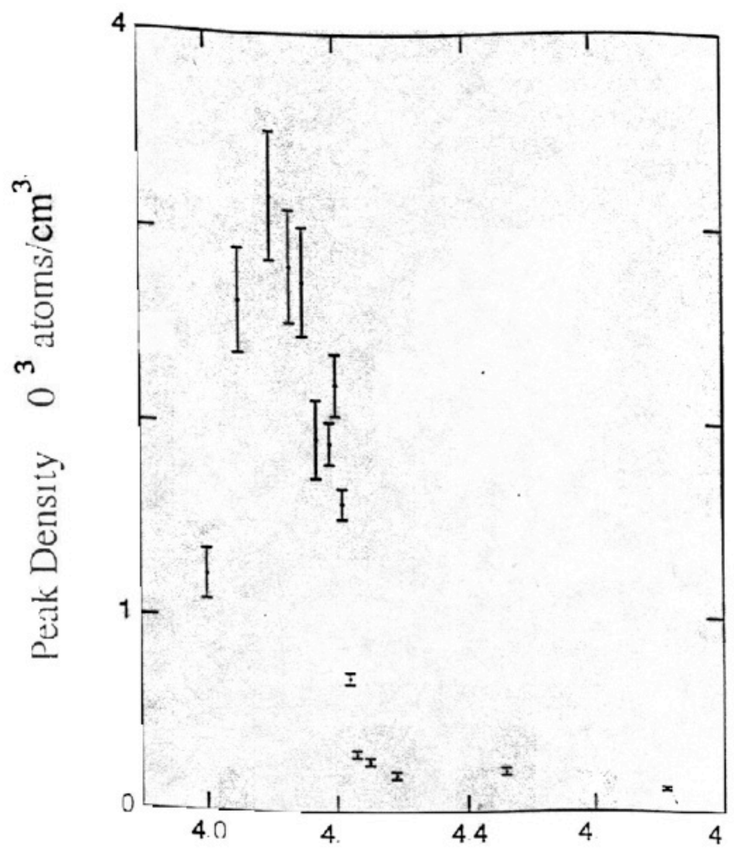

Figure 6. Peak density at the center of the sample as a function of the final depth of the evaporative cut on the RF frequency. As evaporation progresses to smaller values of the frequency, the cloud shrinks and cools, causing a modest increase in peak density until the frequency reaches 4.23 MHz. The sudden discontinuity at 4.23 MHz indicates the first appearance of the high-density condensate as the cloud undergoes a phase transition. From Anderson et al. (1995).

Supplement to Experiment in Physics

Figure 7

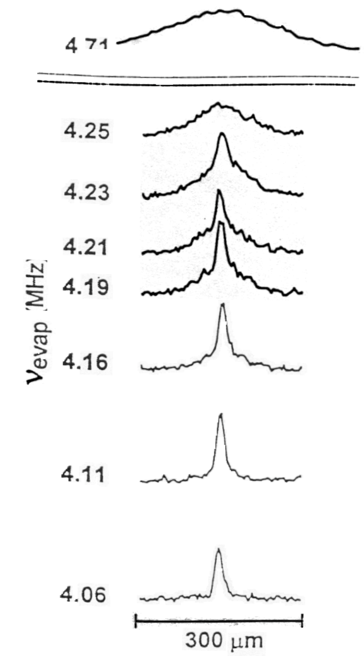

Figure 7. Horizontal sections taken through the velocity distribution at progressively lower values of the RF frequency show the appearance of the condensate fraction. From Anderson et al. (1995).

Supplement to Experiment in Physics

Appendix 4: The Fall of the Fifth Force

In this episode we will examine a case of the refutation of a hypothesis, but only after a disagreement between experimental results was resolved.

The "Fifth Force" was a proposed modification of Newton's Law of Universal Gravitation. The initial experiments gave conflicting results: one supported the existence of the Fifth Force whereas the other regued against it. After numerous repetitions of the experiment, the discord was resolved and a consensus reached that the Fifth Force Did not exist. A reanalysis of the original Eötvös experiment[1] by Fischbach and his collaborators (1986) had shown a suggestive deviation from the law of gravity. The Fifth Force, in contrast to the famous Galileo experiment, depended on the composition of the objects. Thus, the Fifth Force between a copper mass and an aluminum mass would differ from that between a copper mass and a lead mass. Fischbach and collaborators also suggested modifying the gravitational potential between two masses from

$$V = -Gm_1m_2/r$$

to

$$V = -Gm_1m_2/r\ [1 + (\alpha)e^{-r/\lambda}],$$

where the second term gives the Fifth Force with strength α and range λ. The reanalysis also suggested that α was approximately 0.01 and λ was approximately 100m. (For details of this episode see (Franklin 1993)).

In this episode, we have a hitherto unobserved phenomenon along with discordant experimental results. The first two experiments on the Fifth Force gave contradictory answers. One experiment supported the existence of the Fifth Force, whereas the other found no evidence for it. The first experiment, that of Peter Thieberger (1987a) looked for a composition-dependent force using a new type of experimental apparatus, which measured the differential acceleration between copper and water. The experiment was conducted near the edge of the Palisades cliff in New Jersey to enhance the effect of an intermediate-range force. The

experimental apparatus is shown in Figure 8. The horizontal acceleration of the copper sphere relative to the water can be determined by measuring the steady-state velocity of the sphere and applying Stokes' law for motion in a resistive medium. Thieberger's results are shown in Figure 9. The sphere clearly has a velocity, indicating the presence of a force. Thieberger concluded, "The present results are compatible with the existence of a medium-range, substance-dependent force" (p. 1068).

The second experiment, by the whimsically named Eöt-Wash group, was also designed to look for a substance-dependent, intermediate range force (Raab 1987; Stubbs et al. 1987). The apparatus was located on a hillside on the University of Washington campus, in Seattle (Figure 10). If the hill attracted the copper and beryllium bodies differently, then the torsion pendulum would experience a net torque. This torque could be observed by measuring shifts in the equilibrium angle of the torsion pendulum as the pendulum was moved relative to a fixed geophysical point. Their experimental results are shown in Figure 11. The theoretical curves were calculated with the assumed values of 0.01 and 100m, for the Fifth Force parameters α and λ, respectively. These were the best values for the parameters at the time. There is no evidence for such a Fifth Force in this experiment.

The problem was, however, that both experiments appeared to be carefully done, with no apparent mistakes in either experiment. Ultimately, the discord between Thieberger's result and that of the Eöt-Wash group was resolved by an overwhelming preponderance of evidence in favor of the Eöt-Wash result (The issue was actually more complex. There were also discordant results on the distance dependence of the Fifth Force. For details see Franklin (1993; 1995a)). The subsequent history is an illustration of one way in which the scientific community deals with conflicting experimental evidence. Rather than making an immediate decision as to which were the valid results, this seemed extremely difficult to do on methodological or epistemological grounds, the community chose to await further measurements and analysis before coming to any conclusion about the evidence. The torsion-balance experiments of Eöt-Wash were repeated by others including (Cowsik et al. 1988; Fitch, Isaila and Palmer 1988; Adelberger 1989; Bennett 1989; Newman, Graham and Nelson 1989; Stubbs et al. 1989; Cowsik et al. 1990; Nelson, Graham and Newman 1990). These repetitions, in different locations and using different substances, gave consistently negative results. In addition, Bizzeti and collaborators (1989a; 1989b), using a float apparatus similar to that of Thieberger, also obtained results showing no evidence of a Fifth Force. There is, in fact, no explanation of either Thieberger's original, presumably incorrect, results. The scientific community has chosen, I believe quite reasonably, to regard the preponderance of negative results as conclusive.[2] Experiment had shown that there is no Fifth Force.

Return to Experiment in Physics

Supplement to Experiment in Physics

Figure 8

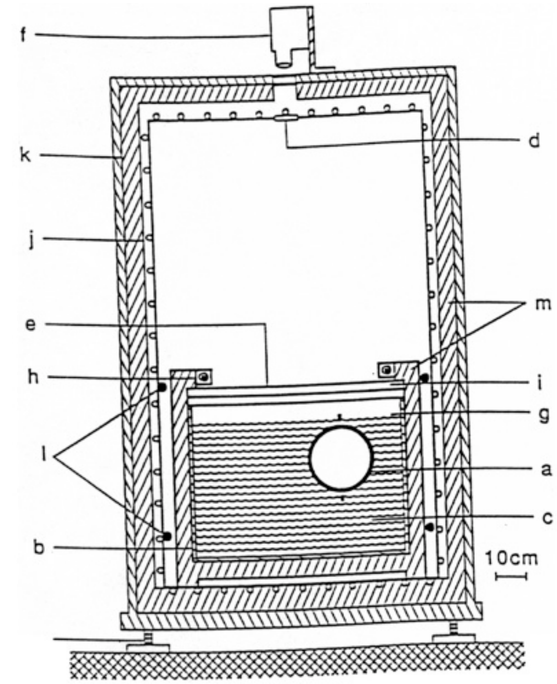

Figure 8. Schematic diagram of the differential accelerometer used in Thieberger's experiment. A precisely balanced hollow copper sphere (a) floats in a copper-lined tank (b) filled with distilled water (c). The sphere can be viewed through windows (d) and (e) by means of a television camera (f). The multiple-pane window (e) is provided with a transparent x-y coordinate grid for position determination on top with a fine copper mesh (g) on the bottom. The sphere is illuminated for one second per hour by four lamps (h) provided with infrared filters (i). Constant temperature is maintained by mea ns of a thermostatically controlled copper shield (j) surrounded by a wooden box lined with Styrofoam insulation (m). The Mumetal shield (k) reduces possible effects du e to magnetic field gradients and four circular coils (l) are used for positioning the sphere through forces due to ac-produced eddy currents, and for dc

tests. From Thieberger (1987).

Supplement to Experiment in Physics

Figure 9

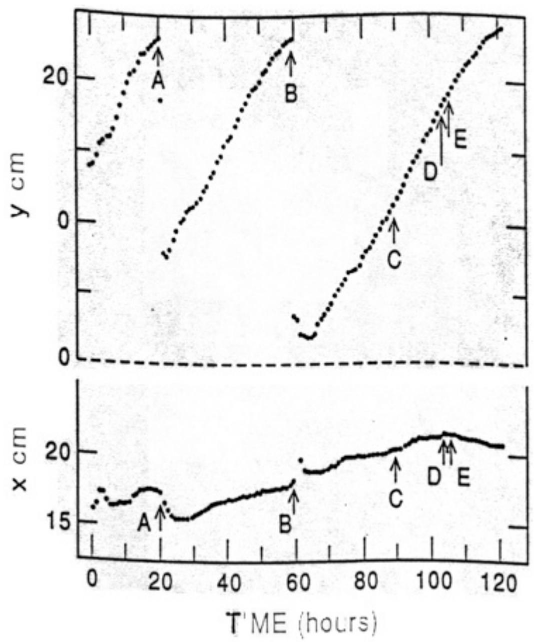

Figure 9. Position of the center of the sphere as a function of time. The y axis points away from the cliff. The position of the sphere was reset at points A and B by engaging the coils shown in Figure 21. From Thieberger (1987).

Supplement to Experiment in Physics

Figure 10

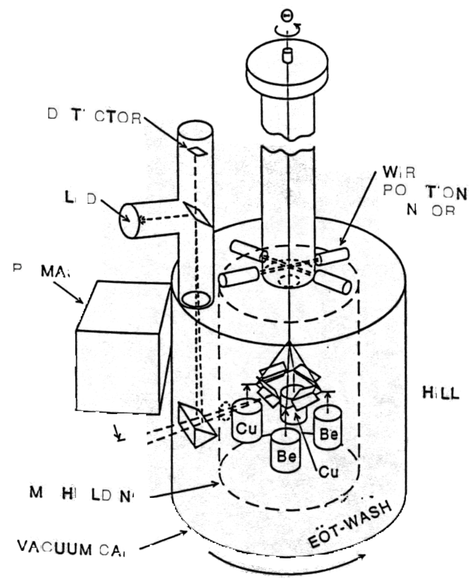

Figure 10. Schematic view of the University of Washington torsion pendulum experiment. The Helmholtz coils are not shown. From Stubbs et al. (1987).

Supplement to Experiment in Physics

Figure 11

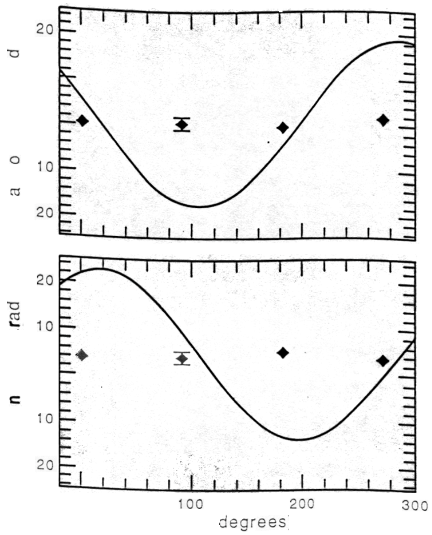

Figure 11. Deflection signal as a function of . The theoretical curves correspond to the signal expected for alpha = 0.01 and lambda = 100m. From Raab (1987).

Supplement to Experiment in Physics

Appendix 5: Right Experiment, Wrong Theory: The Stern-Gerlach Experiment

From the time of Ampere onward, molecular currents were regarded as giving rise to magnetic moments. In the nuclear model of the atom the electron orbits the nucleus. This circular current results in a magnetic moment. The atom behaves as if it were a tiny magnet. In the Stern-Gerlach experiment a beam of silver atoms passed through an inhomogeneous magnetic field (Figure 12). In Larmor's classical theory there was no preferential direction for the direction of the magnetic

moment and so one predicted that the beam of silver atoms would show a maximum in the center of the beam. In Sommerfeld's quantum theory an atom in a state with angular momentum equal to one (L = 1) would have a magnetic moment with two components relative to the direction of the magnetic field, $\pm eh/4m_e$. (Bohr had argued that only two spatial components were allowed). In an inhomogeneous magnetic field, H, the force on the magnetic moment μ will be μ_z x (Gradient of the magnetic field in the z direction), where $\mu_z = \pm eh/4m_e$, where e is the charge of the electron, m_e is its mass, h is Planck's constant, and z is the field direction. Thus, depending on the orientation of the magnetic moment relative to the magnetic field there will be either an attractive or repulsive force and the beam will split into two components, exhibiting spatial quantization. There will be a minimum at the center of the beam. "According to quantum theory μ_z can only be $\pm (e/2m_e)(h/2\pi)$. In this case the spot on the receiving plate will therefore be split into two, each of them having the same size but half the intensity of the original spot" (Stern 1921, p. 252, JM) This difference in prediction between the Larmor and Sommerfeld theories was what Stern and Gerlach planned to use to distinguish between the two theories. Stern remarked that "the experiment, if it can be carried out, (will result) in a clear-cut decision between the quantum-theoretical and the classical view" (Stern 1921, FW).

Sommerfeld's theory also acted as an enabling theory for the experiment. It provided an estimate of the size of the magnetic moment of the atoms so that Stern could begin calculations to see if the experiment was feasible. Stern calculated, for example, that a magnetic field gradient of 10^4 Gauss per centimeter would be sufficient to produce deflections that would give detectable separations of the beam components. He asked Gerlach if he could produce such a gradient. Gerlach responded affirmatively, and said he could do even better. The experiment seemed feasible. A sketch of the apparatus is shown in Figure 12. The silver atoms pass through the inhomogeneous magnetic field. If the beam is spatially quantized, as Sommerfeld predicted, two spots should be observed on the screen. (The sketch shows the beam splitting into three components, which would be expected in modern quantum theory for an atom with angular momentum equal to one). I note that Sommerfeld's theory was incorrect, illustrating the point that an enabling theory need not be correct to be useful.

A preliminary result reported by Stern and Gerlach did not show splitting of the beam into components. It did, however, show a broadened beam spot. They concluded that although they had not demonstrated spatial quantization, they had provided "evidence that the silver atom possesses a magnetic moment." Stern and Gerlach made improvements in the apparatus, particularly in replacing a round beam slit by a rectangular one that gave a much higher intensity. The results are shown in Figure 13 (Gerlach and Stern 1922a). There is an intensity minimum in the center of the pattern, and the separation of the beam into two components is clearly seen. This result seemed to confirm Sommerfeld's quantum-theoretical prediction of spatial quantization. Pauli, a notoriously skeptical physicist, remarked, "Hopefully now even the incredulous Stern will be convinced about directional quantization" (in a letter from Pauli to Gerlach 17 February 1922). Pauli's view was shared by the physics community. Nevertheless the Stern-Gerlach result posed a problem for the Bohr-Sommerfeld theory of the atom. Stern and Gerlach had assumed that the silver atoms were in an angular momentum state with angular momentum equal to one (L = 1). In fact, the atoms are in an L = 0 state, for which no splitting of the beam would be expected in either the classical or the quantum theory. Stern and Gerlach had not considered this possibility. Had they done so they might not have done the experiment. The later, or new, quantum theory developed by Heisenberg, Schrodinger, and others, predicted that for an L = 1 state the beam should split into three components as shown in Figure 12. The magnetic moment of the atom

would be either 0 or ± eh/(4π x m). Thus, if the silver atoms were in an L = 1 state as Stern and Gerlach had assumed, their result, showing two beam components, also posed a problem for the new quantum theory. This was solved when Uhlenbeck and Goudsmit (1925, 1926) proposed that the electron had an intrinsic angular momentum or spin equal to h/4π. This is analogous to the earth having orbital angular momentum about the sun and also an intrinsic angular momentum due to its rotation on its own axis. In an atom the electron will have a total angular momentum **J** = **L** + **S**, where **L** is the orbital angular momentum and **S** is the spin of the electron. For silver atoms in an L = 0 state the electron would have only its spin angular momentum and one would expect the beam to split into two components. Goudsmit and Uhlenbeck suggested the idea of electron spin to explain features in atomic spectra such as the anomalous Zeeman effect, the splitting of spectral lines in a magnetic field into more components than could be accommodated by the Bohr-Sommerfeld theory of the atom. Although the Stern-Gerlach results were known, and would certainly have provided strong support for the idea of electron spin, Goudsmit and Uhlenbeck made no mention of the result.

The Stern-Gerlach experiment was initially regarded as a crucial test between the classical theory of the atom and the Bohr-Sommerfeld theory. In a sense it was, because it showed clearly that spatial quantization existed, a phenomenon that could be accommodated only within a quantum mechanical theory. It decided between the two classes of theories, the classical and the quantum mechanical. With respect to the particular quantum theory of Bohr and Sommerfeld, however, it wasn't crucial, although it was regarded as such at the time, because that theory predicted no splitting for a beam of silver atoms in the ground state (L = 0). The theory had been wrongly applied. The two-component result was also problematic for the new quantum theory, which also predicts no splitting for an angular momentum zero state and three components for an L = 1 state. Only after the suggestion of electron spin did the Stern-

Gerlach result confirm the new theory.

Although the interpretation of the experimental result was incorrect for a time, the result itself remained quite robust through the theory change from the old to the new quantum theory. It is important to remember that experimental results do not change when accepted theory changes, although certainly, as we have seen, their interpretation may change. Gerlach and Stern emphasized this point themselves.

Apart from any theory, it can be stated, as a pure result of the experiment, and as far as the exactitude of our experiments allows us to say so, that silver atoms in a magnetic field have only *two discrete* values of the component of the magnetic moment in the direction of the field strength; both have the same absolute value with each half of the atoms having a positive and a negative sign respectively (Gerlach and Stern 1924, pp. 690-691, FW)

Experimental results, as well as experiments, also have a life of their own, independent of theory.

Return to Experiment in Physics

Supplement to Experiment in Physics

Figure 12

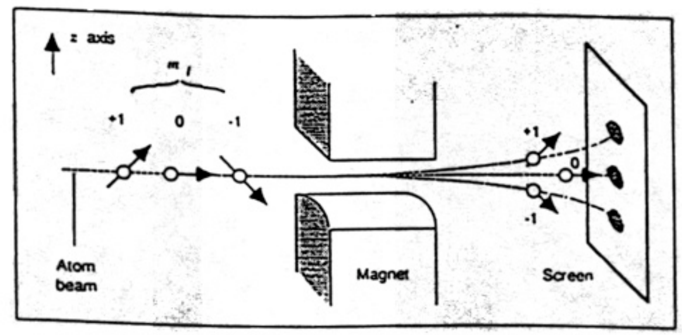

Figure 12. Sketch of the Stern-Gerlach experimental apparatus. The result expected for atoms in an l = 1 state (three components) is shown. From Weinert (1995).

Supplement to Experiment in Physics

Figure 13

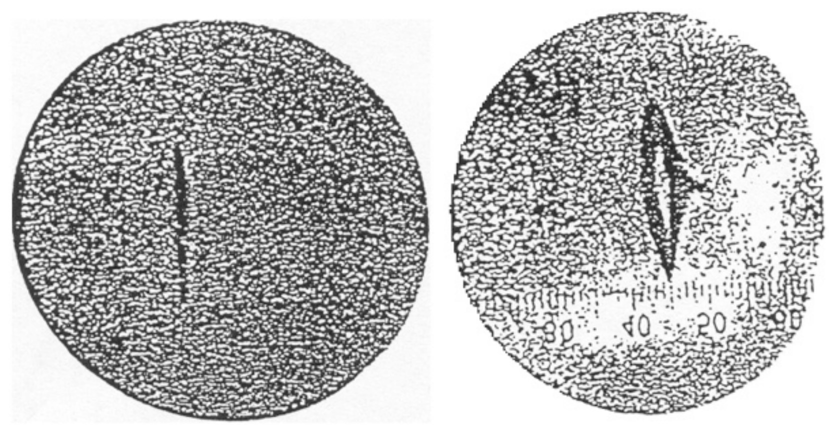

Figure 13. The experimental result of the Stern-Gerlach experiment. The beam has split into two components. From Gerlach and Stern (1922a).

Supplement to Experiment in Physics

Appendix 6: Sometimes Refutation Doesn't Work: The Double-Scattering of Electrons

In 1929, Mott (1929, and later 1931, 1932) calculated, on the basis of Dirac's theory of the electron, that there would be a forward-backward asymmetry of approximately 10% in the double scattering of electrons from heavy nuclei. Mott clearly specified the conditions that would have to be satisfied in order to observe this effect. One had to double scatter relativistic electrons at large angles (90^{o}) from heavy nuclei (most calculations assumed a nuclear charge Z approximately 80). The first scatter would polarize the electrons and the second scatter would analyze the produced polarization, giving rise to an asymmetry.

The earliest experiment that discussed Mott's calculation was performed by Chase (1929). He observed a 4% asymmetry in the double scattering of electrons but attributed it to a difference in the path that the electrons followed. His subsequent experiment (Chase 1930) reported a 1.5% effect, and this time did attribute it to Mott scattering. Most experiments during the early 1930s, showed no polarization effects, although some of them did not satisfy the conditions for Mott scattering (For details see Franklin (1986, Ch. 2)). The sole positive results were provided by experiments done by Rupp (1929; 1930a; 1930b; 1931; 1932a; 1932b; 1932c). Rupp's 1932 experiment first scattered electrons at 90^{o} from a gold foil, followed by a 90^{o} scatter from a gold wire. He found a 3-4% asymmetry at an electron energy of 130 keV and an asymmetry of 9-10% at 250 keV. These results , although positive, were in quantitative disagreement with Mott's prediction of 15.5% at 127 keV and 14% at 204 keV (Mott 1931). Dymond (1931) also reported a positive result, but one that was five times smaller than the theoretical prediction.

Mott and the rest of the electron-scattering community were quite aware of both the confused nature of the experimental results, and of the apparent discrepancy between experiment and theory. Langstroth (1932) reviewed the situation and commented on the difficulty of experiment-theory comparison when one deals with real, as opposed to ideal, experiments. "In view of the fact that practical conditions may be immensely more complicated than those of Mott's theory, it is not surprising that it does not furnish a guide, even in a qualitative way, to all of the above experiments. This may be due to (a) the fact that a large proportion of the beam scattered from a thick target consists of electrons which have undergone more than one collision, (b) the insufficiency of the theoretical model, (c) the inclusion of extraneous effects in the experimental results" (pp. 566-67).

The situation became even more confused when Dymond(1932) published a detailed account of his experiment, which restated his positive, but discrepant, result. Adding to the confusion was the fact Dymond's experiment seemed to satisfy all the conditions for Mott scattering. Rupp (1934) continued his work, this time using thallium vapor rather than gold targets, and again found a positive result. G.P. Thomson (1933), on the other hand, found no effect. At approximately the same time Sauter (1933) redid Mott's calculations and obtained identical results. He also considered whether or not screening by atomic electrons could cancel the predicted effect and found that it could not. If things weren't difficult enough, they got worse when Dymond (1934) published a full repudiation of his earlier results. He had found a considerable and variable experimental asymmetry in his apparatus, and concluded that he had not, in fact, observed any polarization effect. Dymond also considered possible reasons for the theory-experiment discrepancy including inelastic, stray, and plural scattering, and nuclear screening and rejected them all. He concluded, "We are driven to the conclusion that the theoretical results are wrong. There is no reason to believe that the work of Mott is incorrect;... It seems not improbable, therefore, that the divergence of theory from experiment has a more deep-seated cause, and that the Dirac wave equation needs modification in order to account successfully for the absence of polarization" (Dymond 1932, p. 666).

G.P. Thomson (1934) also published a comprehensive review of the field. He reported no effects of the type found by Rupp and he found a forward-backward ratio of (0.996 ± 0.01) in comparison to Mott's prediction of 1.15.Thomson also concluded that there was a serious discrepancy between theory and experiment.

Faced with this apparent theory-experiment discrepancy, theorists sought either to modify Dirac's theory or to propose a new theory, and thus accommodate the experimental results. Hellmann (1935), Halpern and Schwinger (1935), and Winter (1936) offered modifications of the Coulomb potential, each of which had the effect that it "annihilates the polarization effect completely." Although each of the theoretical calculations predicted null results from double scattering experiments, they were not regarded as solving the problem. One might speculate that this was because these modifications had no physical or theoretical underpinning. They seemed invented solely for the purpose of explaining the experimental results.

Experimental work also continued. The situation became even more confused when Rupp (1935) withdrew several of his results on electron scattering This eliminated the most positive results supporting Mott's theory.[1] In 1937 Richter published what he regarded as the definitive experiment on the double scattering of electrons. He claimed to have satisfied the conditions of Mott's calculation exactly and had found no effect. He concluded that "Despite all the favorable conditions of the experiment, however, no sign of the Mott effect could be observed. *With this experimental finding, Mott's theory of the double scattering of electrons from the atomic nucleus can no longer be maintained*. It cannot be decided here how much Dirac's theory of electron spin, which is at the basis of Mott's theory, and its other applications are implicated through the denial of Mott's theory" (Richter 1937, p. 554). The discrepancy was further confirmed by the theoretical work of Rose and Bethe (1939). They examined various ways of trying to eliminate the discrepancy and concluded that "the discrepancy between theory and experiment remains -- perhaps more glaring than before" (p. 278).

Thus, at the end of 1939 there was a clear discrepancy between Dirac theory, as used by Mott, and the experimental results on the double scattering of electrons. Yet the theory was not regarded as refuted. Why was this? The reason is that, at the time, Dirac theory, and only Dirac theory, predicted the existence of the positron (a positive electron). This particle had been discovered in 1932 and had provided very strong support for Dirac theory. In comparison with this success, the discrepancy in electron scattering, along with another small discrepancy in the spectrum of hydrogen, just did not have sufficient evidential weight. The unique, and confirmed, prediction of the positron outweighed these discrepancies. It isn't easy to refute a strongly confirmed theory. Neither is it impossible as demonstrated by the histories of both parity nonconservation and CP violation discussed earlier.

Interestingly, it was the experimental results that were wrong. In the early 1940s experimental work showed that the way in which the experiments were performed during the 1930s had precluded the possibility of observing the polarization effects predicted by Mott. In order to avoid problems with multiple scattering the experimenters had scattered the electrons from the front surface of the targets. Unfortunately this made the effects of plural scattering, a few large scatters rather than just one as required by Mott, very large. The symmetric plural scattering swamped the predicted polarization effect. When the experimental apparatuses were changed to eliminate this problem the discrepancy disappeared.[2] Mott's theory was then supported by the experimental evidence.

We have seen here a classic case of the Duhem-Quine problem and how the physics community attempted to solve it. There was a clear discrepancy between the experimental results and the predictions of a well-confirmed theory. The experiments were redone to check the results, with careful attention to the experimental conditions required by the theory. Theorists checked on whether or not other effects might mask the predicted polarization effect. Other theorists offered competing explanations. Ultimately a solution was found.

Does the fact that Dirac theory was not regarded as refuted even though experiment clearly disagreed with its predictions mean that physicists disregard negative results whenever it suits their purposes? Do physicists really tune in on existing community commitments, as some social constructivists would have it, and overlook negative evidence? The answer is no. There is no indication in this episode that the negative evidence was disregarded. The physics community examined the theory in the light of all the available experimental evidence, weighed its importance, and then made a decision. I note that even though Dirac theory remained relatively unscathed, both experimental and theoretical work continued until the problem was solved. The discrepancy was not hidden from view, nor was it ignored.

Return to Experiment in Physics

Supplement to Experiment in Physics

Appendix 7: Evidence for a New Entity: J.J. Thomson and the Electron

In discussing the existence of electrons Ian Hacking has written, "So far as I'm concerned, if you can spray them then they are real" (Hacking 1983, p. 23). He went on to elaborate this view. "We are completely convinced of the reality of electrons when we 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" (p. 265).

Hacking worried that the simple manipulation of the first quotation, the changing of the charge on an oil drop or on a superconducting niobium sphere, which involves only the charge of the electron, was insufficient grounds for belief in electrons. His second illustration, which he believed more convincing because it involved several properties of the electron, was that of Peggy II, a source of polarized electrons built at the Stanford Linear Accelerator Center in the late 1970s. Peggy II provided polarized electrons for an experiment that scattered electrons off deuterium to investigate the weak neutral current. Although I agree with Hacking that manipulability can often provide us with grounds for belief in a theoretical entity,[1] his illustration comes far too late. Physicists were manipulating the electron in Hacking's sense in the early twentieth century.[2] They believed in the existence of electrons well before Peggy II, and I will argue that they had good reasons for that belief.[3]

The position I adopt is one that might reasonably be called "conjectural" realism. It is conjectural because, despite having good reasons for belief in the existence of an entity or in the truth of a scientific law, we might be wrong. At one time scientists had good reason to believe in phlogiston and caloric, substances we now have good reason to believe don't exist. My position includes both Sellars' view that "to have good reason for holding a theory is *ipso facto* to have good reason for holding that the entities postulated by the theory exist" (Sellars 1962, p. 97), and the "entity realism" proposed by Cartwright (1983) and by Hacking (1983).

Both Hacking, as noted above, and Cartwright emphasize the manipulability of an entity as a criterion for belief in its existence. Cartwright also stresses causal reasoning as part of her belief in entities. In her discussion of the operation of a cloud chamber she states, "...if there are no electrons in the cloud chamber, I do not know why the tracks are there" (Cartwright, 1983, p.99). In other words, if such entities don't exist then we have no plausible causal story to tell. Both Hacking and Cartwright grant existence to entities such as electrons, but do not grant "real" status to either laws or theories, which may postulate or apply to such entities.

In contrast to both Cartwright and Hacking, I suggest that we can also have good reasons for belief in the laws and theories governing the behavior of the entities, and that several of their illustrations implicitly involve such laws.[4] I have argued elsewhere for belief in the reality of scientific laws (Franklin 1996). In this section I shall concentrate on the reality and existence of entities, in particular, the electron. I agree with both Hacking and Cartwright that we can go beyond Sellars and have good reasons for belief in entities even without laws. Hacking and Cartwright emphasize experimenting *with* entities. I will argue that experimenting *on* entities and measuring their properties can also provide grounds for belief in their existence.

In this section I will discuss the grounds for belief in the existence of the electron by examining J.J. Thomson's experiments on cathode rays. His 1897 experiment on cathode rays is generally regarded as the "discovery" of the electron.

The purpose of J.J. Thomson's experiments was clearly stated in the introduction to his 1897 paper.

The experiments discussed in this paper were undertaken in the hope of gaining some information as to the nature of Cathode Rays. The most diverse opinions are held as to these rays; according to the almost unanimous opinion of German physicists they are due to some process in the aether to which -- inasmuch as in a uniform magnetic field their course is circular and not rectilinear -- no phenomenon hitherto observed is analogous: another view of these rays is that, so far from being wholly aetherial, they are in fact wholly material, and that they mark the paths of particles of matter charged with negative electricity (Thomson 1897, p. 293).

Thomson's first order of business was to show that the cathode rays carried negative charge. This had presumably been shown previously by Perrin. Perrin placed two coaxial metal cylinders, insulated from one another, in front of a plane cathode. The cylinders each had a small hole through which the cathode rays could pass onto the inner cylinder. The outer cylinder was grounded. When cathode rays passed into the inner cylinder an electroscope attached to it showed the presence of a negative electrical charge. When the cathode rays were magnetically deflected so that they did not pass through the holes, no charge was detected. "Now the supporters of the aetherial theory do not deny that electrified particles are shot off from the cathode; they deny, however, that these charged particles have any more to do with the cathode rays than a rifle-ball has with the flash when a rifle is fired" (Thomson 1897, p. 294).

Thomson repeated the experiment, but in a form that was not open to that objection. The apparatus is shown in Figure 14]. The two coaxial cylinders with holes are shown. The outer cylinder was grounded and the inner one attached to an electrometer to detect any charge. The cathode rays from A pass into the bulb, but would not enter the holes in the cylinders unless deflected by a magnetic field.

When the cathode rays (whose path was traced by the phosphorescence on the glass) did not fall on the slit, the electrical charge sent to the electrometer when the induction coil producing the rays was set in action was small and irregular; when, however, the rays were bent by a magnet so as to fall on the slit there was a large charge of negative electricity sent to the electrometer.... If the rays were so much bent by the magnet that they overshot the slits in the cylinder, the charge passing into the cylinder fell again to a very small fraction of its value when the aim was true. *Thus this experiment shows that however we twist and deflect the cathode rays by magnetic forces, the negative electrification follows the same path as the rays, and that this negative electrification is indissolubly connected with the cathode rays* (Thomson 1897, p. 294-295, emphasis added).

This experiment also demonstrated that cathode rays were deflected by a magnetic field in exactly the way one would expect if they were negatively charged material particles.[5]

There was, however, a problem for the view that cathode rays were negatively charged particles. Several experiments, in particular those of Hertz, had failed to observe the deflection of cathode rays by an electrostatic field. Thomson proceeded to answer this objection. His apparatus is shown in Figure 15]. Cathode rays from C pass through a slit in the anode A, and through another slit at B. They then passed between plates D and E and produced a narrow well-defined phosphorescent patch at the end of the tube, which also had a scale attached to measure any deflection. When Hertz had performed the experiment he had found no deflection when a potential difference was applied across D and E. He concluded that the electrostatic properties of the cathode ray are either *nil* or very feeble. Thomson admitted that when he first performed the experiment he also saw no effect. "on repeating this experiment [that of Hertz] I at first got the same result [no deflection], but subsequent experiments showed that the absence of deflexion is due to the conductivity conferred on the rarefied gas by the cathode rays.[6] On measuring this conductivity it was found that it diminished very rapidly as the exhaustion increased; it seemed that on trying Hertz's experiment at very high exhaustion there might be a chance of detecting the deflexion of the cathode rays by an electrostatic force" (Thomson 1897, p. 296). Thomson did perform the experiment at lower pressure [higher exhaustion] and observed the deflection.[7]

Thomson concluded:

> As the cathode rays carry a charge of negative electricity, are deflected by an electrostatic force as if they were negatively electrified, and are acted on by a magnetic force in just the way in which this force would act on a negatively electrified body moving along the path of these rays, I can see no escape from the conclusion that they are charges of negative electricity carried by particles of matter. (Thomson 1897, p. 302)[8]

Having established that cathode rays were negatively charged material particles, Thomson went on to discuss what the particles were. "What are these particles? are they atoms, or molecules, or matter in a still finer state of subdivision" (p. 302). To investigate this question Thomson made measurements on the charge to mass ratio of cathode rays. Thomson's method used both the electrostatic and magnetic deflection of the cathode rays.[9] The apparatus is shown in]. It also included a magnetic field that could be created perpendicular to both the electric field and the trajectory of the cathode rays.

Let us consider a beam of particles of mass m charge e, and velocity v. Suppose the beam passes through an electric field F in the region between plates D and E, which has a length L. The time for a particle to pass through this region $t = L/v$. The electric force on the particle is Fe and its acceleration $a = Fe/m$. The deflection d at the end of the region is given by

$d = ½ at^2 = ½(eF/m)L^2/v^2$

Now consider a situation in which the beam of cathode rays simultaneously pass through both F and a magnetic field B in the same region. Thomson adjusted B so that the beam was undeflected. thus the magnetic force was equal to the electrostatic force.

$evB = eF$ or $v = F/B$.

This determined the velocity of the beam. Thus, $e/m = 2dF/B^2L^2$

Each of the quantities in the above expression was measured so the e/m or m/e could be determined.

Using this method Thomson found a value of m/e of $(1.29 \pm 0.17) \times 10^{-7}$. This value was independent of both the gas in the tube and of the metal used in the cathode, suggesting that the particles were constituents of the atoms of all substances. It was also far smaller, by a factor of 1000, than the smallest value previously obtained, 10^{-4}, that of the hydrogen ion in electrolysis.

Thomson remarked that this might be due to the smallness of m or to the largeness of e. He argued that m was small citing Lenard's work on the range of cathode rays in air. The range, which is related to the mean free path for collisions, and which depends on the size of the object, was 0.5 cm. The mean free path for molecules in air was approximately 10^{-5} cm. If the cathode ray traveled so much farther than a molecule before colliding with an air molecule, Thomson argued that it must be much smaller than a molecule.[10]

Thomson had shown that cathode rays behave as one would expect negatively charged material particles to behave. They deposited negative charge on an electrometer, and were deflected by both electric and

magnetic fields in the appropriate direction for a negative charge. In addition the value for the mass to charge ratio was far smaller than the smallest value previously obtained, that of the hydrogen ion. If the charge were the same as that on the hydrogen ion, the mass would be far less. In addition, the cathode rays traveled farther in air than did molecules, also implying that they were smaller than an atom or molecule. Thomson concluded that these negatively charged particles were constituents of atoms. In other words, Thomson's experiments had given us good reasons to believe in the existence of electrons.

Return to Experiment in Physics

Supplement to Experiment in Physics

Figure 14

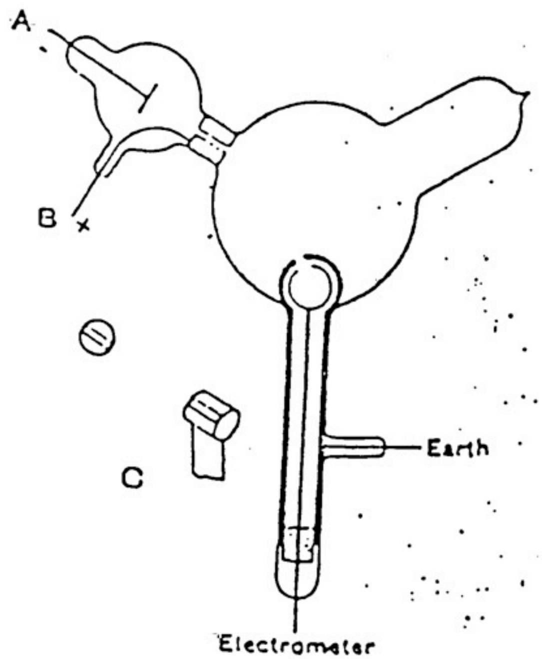

Figure 14. Thomson's apparatus for demonstrating that cathode rays have negative charge. The slits in the cylinders are shown. From Thomson (1897).

Supplement to Experiment in Physics

Figure 15

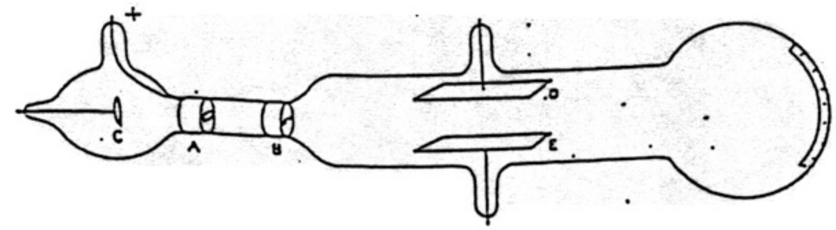

Figure 15. Thomson's apparatus for demonstrating that cathode rays are deflected by an electric field. It was also used to measure *m/e*. From Thomson (1897).

Supplement to Experiment in Physics

Appendix 8: The Articulation of Theory: Weak Interactions

Radioactivity, the spontaneous decay of a substance, produces alpha particles (positively charged helium nuclei), or beta particles (electrons), or gamma rays (high energy electromagnetic radiation). It was discovered in 1896 by Henri Becquerel. Experimental work on the energy of the electrons emitted in β decay began in the early twentieth century, and the observed continuous energy spectrum posed a problem. If β decay were a two-body decay (for example, neutron → proton + electron) then applying the laws of conservation of energy and of conservation of momentum requires that the energy of the electron have a unique value, not a continuous spectrum.[1] Thus, the observed continuous energy spectrum cast doubt on both of these conservation laws. Physicists speculated that perhaps the electrons lost energy in escaping the substance, with different electrons losing different amounts of energy, thus accounting for the energy spectrum. Careful experiments showed that this was not the case so the problem remained. In the early 1930s Pauli suggested that a low-mass neutral particle, named by Fermi as the neutrino, was also emitted in β decay.[2] This solved the problem of the continuous energy spectrum because in a three-body decay (neutron proton + electron + neutrino) the energy of the electron was no longer required to be unique. The electron could have a continuous energy spectrum and the conservation laws were saved.[3]

In 1934 Fermi proposed a new theory of β decay that incorporated this new particle (Fermi 1934). He added a perturbation energy due to the decay interaction to the Hamiltonian describing the nuclear system. Pauli (1933) had previously shown that the perturbation could have only five different forms if the Hamiltonian is to be relativistically invariant. These are S, the scalar interaction; P, pseudoscalar; V, vector; A, axial vector; and T, tensor. Fermi knew this but chose, in analogy with electromagnetic theory, to use only the vector interaction. His theory initially received support from the work of Sargent (1932; 1933) and others. There remained, however, the question of whether or not the other forms of the interaction also entered into the Hamiltonian.[4] In this episode we shall see how experiment helped to determine the mathematical form of the weak interaction.

Gamow and Teller (1936) soon proposed a modification of Fermi's vector theory. Fermi's theory had originally required a selection rule, the change in J = 0, where J is the angular momentum of the nucleus, and did not include the effects of nuclear spin. Gamow and Teller included nuclear spin and obtained selection rules, change in J = 0, ±1 for allowed transitions, with no 0--0 transitions allowed. The Gamow-Teller modification required either a tensor or an axial vector form of the interaction. Their theory helped to solve some of the difficulties that arose in assigning nuclear spins using only the Fermi selection rule. At then end of the 1930s there was support for Fermi's theory with some preference for the Gamow-Teller selections rules and the tensor interaction.

The work of Fierz (1937) helped to restrict the allowable forms of the interaction. He showed that if both S and V interactions were present in the allowed -decay interaction, or both A and T, then there would be an interference term of the form 1 + a/W in the allowed beta-decay spectrum, where W is the electron energy. This term vanished if the admixtures were not present. The failure to observe these interference terms showed that the decay interaction did not contain both S and V, or both A and T.

The presence of either the T or A form of the interaction in at least part of the beta-decay interaction was shown by Mayer, Moszkowski, and Nordheim (1951). They found twenty five decays for which the change in J was 0, ±1, with no parity change. These decays could only occur if the A or T forms were present. Their conclusion depended on the correct assignment of nuclear spins which, although reliable, still retained some uncertainty. Further evidence, which did not depend on knowledge of the nuclear spins, came from an examination of the spectra of unique forbidden transitions.[5] These were n-times forbidden transitions in which the change in nuclear spin was n + 1. These transitions require the presence of either A or T. In addition, only a single form of the interaction makes any appreciable contribution to the decay. This allows the prediction of the shape of the spectrum for such transitions. Konopinski and Uhlenbeck (1941) showed that for an n-times forbidden transition the spectrum would be that of an allowed transition multiplied by an energy dependent term $a_n(W)$. For a first-forbidden transition $a_1 = C[(W^2 - m^2c^4) + (W_o - W)^2]$. The spectrum for ^{91}Y measured by Langer and Price (1949) (Figure 16) shows the clear presence of either the A or T forms of the interaction. The spectrum requires the energy-dependent correction.

Evidence in favor of the presence of either the S or V forms of the interaction was provided by Sherr, Muether, and White (1949)and by

Sherr and Gerhart (1952). They observed the decay of ^{14}O to an excited state of ^{14}N, $^{14}N^{*}$. They argued that both ^{14}O and $^{14}N^{*}$ had spin 0. This required the presence of either S or V because the decay was forbidden by A and T. (Recall that the Gamow-Teller selection rules specified no 0 to 0 transitions).

Further progress in isolating the particular forms of the interaction was made by examining the spectra of once-forbidden transitions. Here too, interference effects, similar to those predicted by Fierz, were also expected. A. Smith (1951)and Pursey (1951) found that the spectrum for these transitions would contain energy dependent terms of the form G_VG_T/W, G_AG_P/W, and G_SG_A/W, where the G's are the coupling constants for the various interactions, and W is the electron energy. The linear spectrum found for ^{147}Pm demonstrated the absence of these terms (Langer, Motz and Price 1950).

Let us summarize the situation. There were five allowable forms of the decay interaction; S, T, A, V, P. The failure to observe Fierz interference showed that the interaction could not contain both S and V or both A and T. Experiments showing the presence of Gamow-Teller selections rules and on unique forbidden transitions had shown that either A or T must be present. The decay of ^{14}O to $^{14}N^{*}$ had demonstrated that either S or V must also be present. This restricted the forms of the interaction to STP, SAP, VTP, or VAP or doublets taken from these combinations. The absence of interference terms in the once-forbidden spectra eliminated the VT, SA, and AP combinations. VP was eliminated because it did not allow Gamow-Teller transitions. This left only the STP triplet or the VA doublet as the possible interactions.

The spectrum of RaE provided the decisive evidence. Petschek and Marshak (1952) analyzed the spectrum of RaE and found that the only interaction that would give a good fit to the spectrum was a combination

of T and P. This was, in fact, the only evidence favoring the presence of the P interaction. This led Konopinski and Langer (1953), in their 1953 review article on β decay to conclude that, "As we shall interpret the evidence here, the correct law must be what is known as an STP combination (1953, p. 261)."

Unfortunately, the evidence from the RaE spectrum had led the physics community astray. Petschek and Marshak had noted that their conclusion was quite sensitive to assumptions made in their calculation. "Thus, an error in the finite radius correction of approximately 0.1 percent leads to an error of up to 25% in $C_{1(T+P)}$ [the theoretical correction term]." Further theoretical analysis cast doubt on their assumptions, but all of this became moot when K. Smith[6] measured the spin of RaE and found it to be one, incompatible with the Petschek-Marshak analysis.

The demise of the RaE evidence removed the necessity of including the P interaction in the theory of β decay, and left the decision between the STP and VA combinations unresolved. The dilemma was resolved by evidence provided by angular-correlation experiments, particularly that from the experiment on ^{6}He by Rustad and Ruby (1953; 1955)

(a) Angular Correlation Experiments. Angular correlation experiments are those in which both the decay electron and the recoil nucleus from β decay are detected in coincidence. The experiments measured the distribution in angle between the electron and the recoil nucleus for a fixed range of electron energy, or measured the energy spectrum of either the electron or the nucleus at a fixed angle between them. These quantities are quite sensitive to the form of the decay interaction and became decisive pieces of evidence in the search for the form of the decay interaction. Hamilton (1947) calculated the form of the angular distribution expected for both allowed and forbidden decays, assuming only one type of interaction (S, V, T, A, P) was present. He found, for allowed transitions, that the angular distributions for the specific forms of the interaction would be different. A more general treatment was given by de Groot and Tolhoek (1950). They found that the general form of the angular distribution for allowed decays depended on the combination of the particular forms of the interactions in the decay Hamiltonian. For single forms their results agreed with those of Hamilton.

The most important of the experiments performed at this time was the measurement of the angular correlation in the decay of ^{6}He. This decay was a pure Gamow-Teller transition and thus was sensitive to the amounts of A and T present in the decay interaction. The decisive experiment was that of Rustad and Ruby (1953; 1955). This experiment was regarded as establishing that the Gamow-Teller part of the interaction was predominantly tensor. This was the conclusion reached in several review papers on the nature of β decay. (Ridley 1954; Kofoed-Hansen 1955; Wu 1955). The experimental apparatus is shown in Figure 17. The definition of the decay volume was extremely important. In order to measure the angular correlation one must know the position of the decay so that one can measure the angle between the electron and the recoil nucleus. The decay volume for the helium gas in this experiment was defined by a 180 microgram/cm^2 aluminum hemisphere and the pumping diaphragm. Rustad and Ruby (1953) presented two experimental results. The first was the coincidence rate as a function of the angle between the electron and the recoil nucleus for electrons in the energy range (2.5 - 4.0) mc^2. The second was the the energy spectrum of the decay electrons with the angle between the electron and the recoil nucleus fixed at 180^o. Both results are shown in Figure 18 along with the predicted results for A and T, respectively. The dominance of the tensor interaction is clear. This conclusion was made more emphatic in their 1955 paper which included more details of the experiment and even more data. The later results, shown in Figure 19, clearly demonstrate the superior fit of the tensor interaction.

The Rustad-Ruby result, along with several others, established that the Gamow-Teller part of the decay interaction was tensor and that the decay interaction was STP, or ST, rather than VA. We have seen clearly in this episode the fruitful interaction between experiment and theory. Theoretical predictions became more precise and were tested experimentally until the form of the weak interaction was found. Fermi's theory of β decay had been confirmed. It had also been established that the interaction was a combination of scalar, tensor, and pseudoscalar (STP).

(b) Epilogue. It would be nice to report that such a simple, satisfying story, with its happy ending was the last word. It wasn't. Work continued on angular correlation experiments and the happy agreement was soon destroyed (Franklin 1990, Chapter 3). Things became more complex with the discovery of parity nonconservation in the weak interactions, including β decay. Sudarshan and Marshak (1958) and Feynman and Gell-Mann (1958) showed that only a V-A interaction was compatible with parity nonconservation. If there was to be a single interaction describing all the weak interactions then there was a serious conflict between this work and the Rustad-Ruby result. This led Wu and Schwarzschild (1958) to reexamine and reanalyze the Rustad-Ruby experiment. They found, by calculation and by constructing a physical analogue of the gas system, that a considerable fraction of the helium gas (approximately 12%) was not in the decay volume. This changed the result for the angular correlation considerably and cast doubt on the Rustad-Ruby result.[7] The ^{6}He angular correlation experiment was redone, correcting the problem with the gas target, and the new result is shown in Figure 20 (Hermannsfeldt et al. 1958). It clearly favors A, the axial vector interaction. Once again, physics was both fallible and corrigible. This new result on ^{6}He combined with the discovery of parity nonconservation established that the form of the weak interaction was V-

A.

Return to Experiment in Physics

Supplement to Experiment in Physics

Figure 16

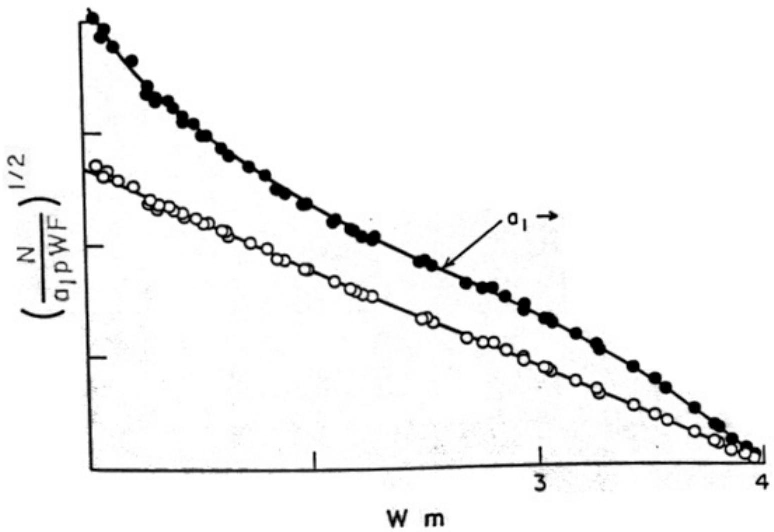

Figure 16. The unique, once-forbidden spectrum of ^{91}Y. The best theoretical fit is that which gives a straight line. The Fermi theory alone, $a_1 = 1$, does not give a straight line. The correction factor $a_1 = C[(W^2 - m_o c^2) + (W_o - W)^2]$, does give a linear plot. From Konopinski and Langer (1953)

Supplement to Experiment in Physics

Figure 17

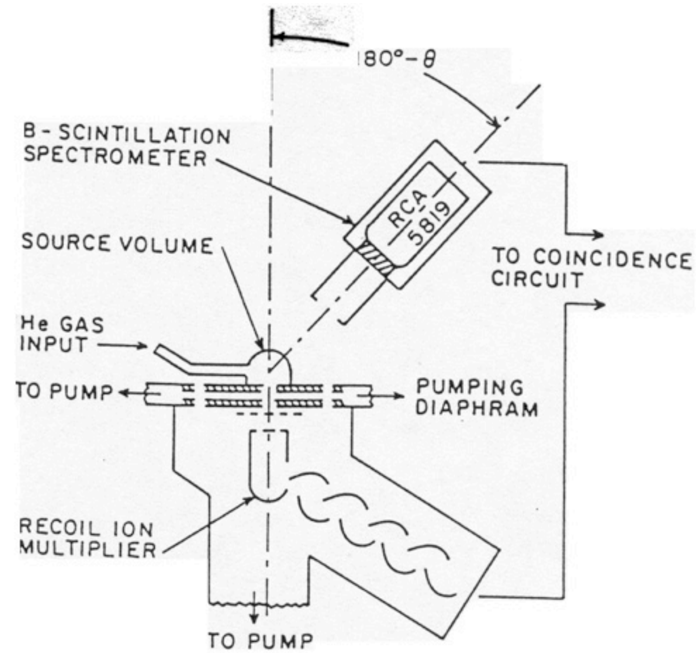

Figure 17. Schematic view of the experimental apparatus for the ^6He angular correlation experiment of Rustad and Ruby (1953; 1955)

Supplement to Experiment in Physics

Figure 18

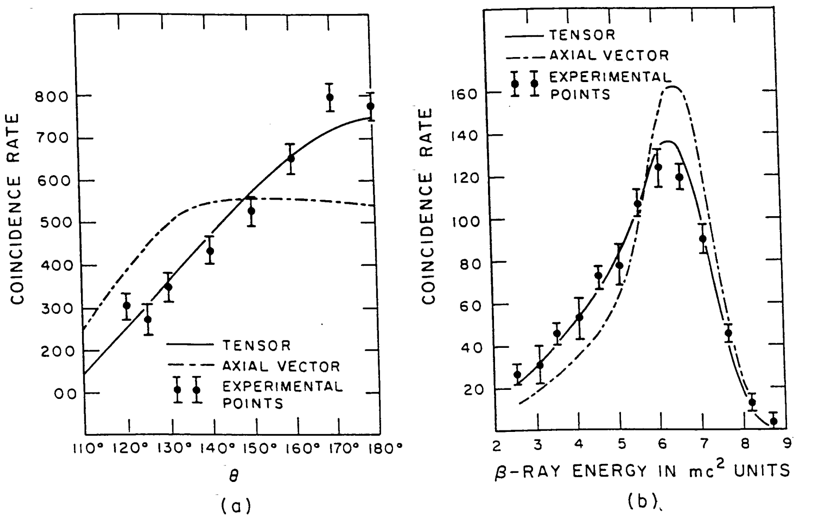

Figure 18. (a) Coincidence counting rate versus angle between the electron and the recoil nucleus, for electrons in the energy range 2.5-4.0 mc^2. (b) Coincidence counting rate versus electron energy for an angle of 180^{o} between the electron and the recoil nucleus. From Rustad and Ruby (1953) .

Supplement to Experiment in Physics

Figure 19

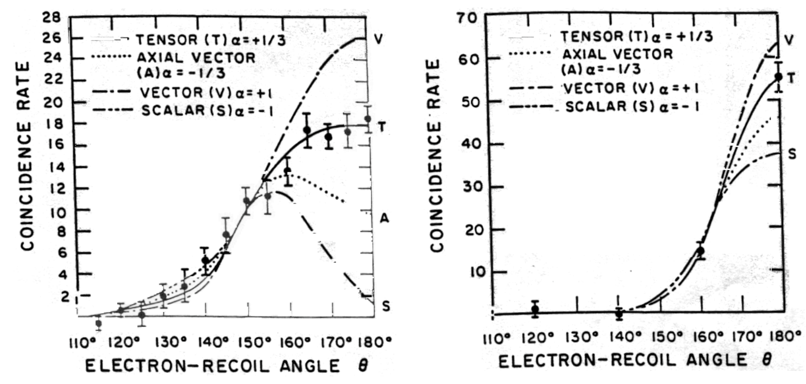

Figure 19. Coincidence counting rate versus angle between the electron and the recoil nucleus for (a) electrons in the energy range 4.5-5.5 mc^2 and (b) electrons in the energy range 5.5-7.5 mc^2. From Rustad and Ruby (1955).

Supplement to Experiment in Physics

Figure 20

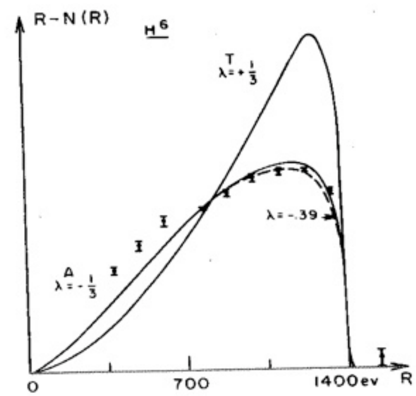

Figure 20. Energy spectrum of recoil ions from ^{35}A decay. From (Hermannsfeldt et al. 1958).

Notes to Experiment in Physics

1 As the late Richard Feynman, one of the leading theoretical physicists of the twentieth century, wrote:

> The principle of science, the definition, almost, is the following: *The test of all knowledge is experiment*. Experiment is the *sole judge* of scientific 'truth'.
> (Feynman, Leighton and Sands 1963, p. 1-1)

In these postmodern times this might seem to be an old-fashioned view, but it is, I believe, correct. Not everyone would agree. As Andy Pickering has remarked,

> ...there is no obligation upon anyone framing a view of the world to take account of what twentieth-century science has to say.
> (Pickering 1984a, p. 413)

2. By valid, I mean that the experimental result has been argued for in the correct way, by use of epistemological strategies such as those discussed below.

3. See Franklin (1986, Ch. 6; and, 1990, Ch. 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

4. As Holmes remarked to Watson, "How often have I said to you that when you have eliminated the impossible, whatever remains, *however improbable*, must be the truth." (Conan Doyle 1967, p. 638)

5. 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 doesn't necessarily cast doubt on the validity of the experimental result.

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

7. Collins offers two arguments concerning the difficulty, if not the virtual impossibility of replication. The first is philosophical. What does it mean to replicate an experiment? In what way is the replication similar to the original experiment? A rough and ready answer is that the replication measures the same physical quantity. Whether or not it, in fact, does so can, I believe, be argued for on reasonable grounds, as discussed earlier.

Collins' second argument is pragmatic. This is the fact that in practice it is often difficult to get an experimental apparatus, even one known to be similar to another, to work properly. Collins illustrates this with his account of Harrison's attempts to construct two versions of a TEA leaser (Transverse Excited Atmospheric) (Collins 1985, pp. 51-78). Despite the fact that Harrison had previous experience with such lasers, and had excellent contacts with experts in the field, he had great difficulty in building the lasers. Hence the difficulty of replication.

Ultimately Harrison found errors in his apparatus and once these were corrected the lasers operated properly. As Collins admits, "...in the case of the TEA laser the circle was readily broken. The ability of the laser to vaporize concrete, or whatever, comprised a universally agreed criterion of experimental quality. There was never any doubt that the laser ought to be able to work and never any doubt about when one was working and when it was not.” (Collins 1985, p. 84)

Although Collins seems to regard Harrison's problems with replication as casting light on the episode of gravity waves, as support for the experimenters' regress, and as casting doubt on experimental evidence in general, it really doesn't work. As Collins admits (see quote in last paragraph), the replication was clearly demonstrable. One may wonder what role Collins thinks this episode plays in his argument.

8. In more detailed discussions of this episode, Franklin (1994, 1997a), I argued that the gravity wave experiment is not at all typical of physics experiments. In most experiments, as illustrated in those essays, the adequacy of the surrogate signal used in the calibration of the experimental apparatus is clear and unproblematical. In cases where it is questionable considerable effort is devoted to establishing the adequacy of that surrogate signal. Although Collins has chosen an atypical example I believe that the questions he raises about calibration in general and about this particular episode of gravity wave experiments should be answered.

9. Weber had suggested that the actual gravity wave pulses were longer that expected, and that the nonlinear analysis algorithm was more efficient at detecting such pulses.

10. The K_1^o and K_2^o mesons were elementary particles with the same charge, mass, and intinsic spin. They did, however, differ with respect to the CP operator. The K_1^o and K_2^o mesons were eigenstates of the CP operator with eigenvalues CP = +1 and -1, respectively.

11. Bose's paper had originally been rejected by the *Philosophical Magazine*. He then sent it, in English, to Einstein with a request that if Einstein thought the paper merited publication that he would arrange for publication in the *Zeitschrift fur Physik*. Einstein personally translated the

paper and submitted it to the *Zeitschrift fur Physik*, adding a translator's note, “In my opinion, Bose's derivation of the Planck formula constitutes an important advance. The method used here also yields the quantum theory of the ideal gas, as I shall discuss elsewhere in more detail” (Pais 1982, p. 423). This discussion appeared in Einstein's own papers of 1924 and 1925. For details see Pais (1982, Ch. 23).

12. This section is based on the accounts given by Weinert (1995) and by Mehra and Rechenberg (1982). Translations from the German were provided by these authors and are indicated by initials in the text.

Notes to Appendix 2

1. I surveyed eighty such theoretical papers. Sixty accepted the Princeton result as evidence for either CP violation or apparent CP violation. Even those that offered alternative explanations of the result were not necessarily indications that the authors did not accept CP violation. One should distinguish between interesting speculations and serious suggestions. The latter are characterized by a commitment to their truth. I note that T.D. Lee was author, or co-author, of three of these theoretical papers. Two offered alternative explanations of the Princeton result and one proposed a model that *avoided* CP violation. Lee was not seriously committed to the truth of any of them. Bell and Perring, authors of one of the alternatives, remarked, “Before a more mundane explanation is found *it is amusing to speculate* that it might be a local effect due to the dyssymmetry of the environment, namely the local preponderance of matter over antimatter” (Bell and Perring 1964, p. 348, emphasis added).

2. In the modus tollens if *h* entails *e* then “not *e*” entails not *h*. Duhem and Quine pointed out that it is really *h* and *b,* where *b* is background knowledge and auxiliary hypotheses, that entails *e*. Thus “not *e*” entails “*h*” or “*b*” and one doesn't know where to place the blame.

Notes to Appendix 3

1. Bose's paper had originally been rejected by the *Philosophical Magazine*. He then sent it, in English, to Einstein with a request that if Einstein thought the paper merited publication that he would arrange for publication in the *Zeitschrift fur Physik*. Einstein personally translated the paper and submitted it to the *Zeitschrift fur Physik*, adding a translator's note, "In my opinion, Bose's derivation of the Planck formula constitutes an important advance. The method used here also yields the quantum theory of the ideal gas, as I shall discuss elsewhere in more detail" (Pais 1982, p. 423). This discussion appeared in Einstein's own papers of 1924 and 1925. For details see Pais (1982, Ch. 23).

2. One difficulty with using rubidium is that at very low temperatures rubidium should be a solid. (In fact, rubidium is a solid at room temperature). Wieman, Cornell and their collaborators avoided this difficulty by creating a system that does not reach a true equilibrium. The vapor sample created equilibrates to a thermal distribution as a spin polarized gas, but takes a very long time to reach its true equilibrium state as a solid. At the low temperatures and density of the experiment the rubidium remains as a metastable super-saturated vapor for a long time.

Notes to Appendix 4

1. The original Eötvös experiment was designed to measure the ratio of the gravitational mass to the inertial mass of different substances. Eötvös found the ratio to be one, to within approximately one part in a million. Fischbach and his collaborators reanalyzed Eötvös' data and found a composition dependent effect, which they interpreted as evidence for a Fifth Force.

2. It is a fact of experimental life that experiments rarely work when they are initially turned on and that experimental results can be wrong, even if

there is no apparent error. It is not necessary to know the exact source of an error in order to discount or to distrust a particular experimental result. Its disagreement with numerous other results can, I believe, be sufficient.

Notes to Appendix 6

1. Rupp's withdrawal included a note from a psychiatrist stating that Rupp had suffered from a mental illness and could not distinguish between fantasy and reality.

2. The problem with the hydrogen spectrum was not solved until the later discovery of the anomalous magnetic moment of the electron in the 1950s.

Notes to Appendix 7

1. Morrison (1990) has argued that manipulability is not sufficient to establish belief in an entity. She discusses particle physics experiments in which particle beams were viewed not only as particles, but also as beams of quarks, the constituents of the particles, even though the physicists involved had no belief in the existence of quarks. Although I believe that Morrison's argument is correct in this particular case, I do think that manipulability can, and often does, give us good reason to believe in an entity. See, for example, the discussion of the microscope in Hacking (1983).

2. Millikan, for example, used the properties of electrons emitted in the photoelectric effect to measure *h*, Planck's constant. Stern and Gerlach, as discussed below, used the properties of the electron to investigate spatial quantization, and also discovered evidence for electron spin.

3. For more details of this episode, including a discussion of other early twentieth century experiments, see (Franklin 1997c).

4. In Cartwright's discussion of the electron track in the cloud chamber, for example, she can identify the track as an electron track rather than as a proton track only because she has made an implicit commitment to the law of ionization for charged particles, and it's dependence on the mass and velocity of the particles.

5. Thomson also demonstrated the magnetic deflection of cathode rays in a separate experiment.

6. Thomson actually investigated the conductivity of the gas in the tube under varying pressure conditions. See Thomson (1897, pp. 298-300).

7. I shall return to this when I discuss Thomson's measurement of *e*/*m* for the electron.

8. Thomson's argument is the "duck argument." If it looks like a duck, quacks like a duck, and waddles like a duck, then we have good reason to believe that it is a duck. One need only reconstitute the argument using "it" as cathode rays and negatively charged particles as ducks.

9. Thomson actually used two different methods to determine the charge to mass ratio. The other method used the total charge carried by a beam of cathode rays in a fixed period of time, the total energy carried by the beam in that sane time, and the radius of curvature of the particles in a known magnetic field. Thomson regarded the method discussed in the text as more reliable and this is the method shown in most modern physics textbooks.

10. Not everything Thomson concluded is in agreement with modern views. Although he believed that the electron was a constituent of atoms, he thought that it was the primordial atom from which all atoms were constructed, similar to Prout's view that all atoms were constructed from hydrogen atoms. He also suggested that the charge on the electron might be larger than that of the hydrogen ion.

Notes to Appendix 8

1. The conservation of momentum also requires that the electron and proton have equal and opposite momenta, for a neutron decay at rest. They will be emitted in opposite directions.

2. Pauli's suggestion was first made in a December 4, 1930 letter to the radioactive group at a regional meeting in Tuebingen.

> Dear Radioactive Ladies and Gentlemen:
>
> I beg you to receive graciously the bearer of this letter who will report to you in detail how I have hit on a desperate was to escape from the problems of the "wrong" statistics of the N and Li^6 nuclei and of the continuous beta spectrum in order to save the "even-odd" rule of statistics and the law of conservation of energy. Namely the possibility that electrically neutral particles, which I would like to call neutrons [the particle we call the neutron, which is about the same mass as the proton, was discovered in 1932 by Chadwick. Pauli's "neutron" is our "neutrino."] might exist inside nuclei; these would have spin 1/2, would obey the exclusion principle, and would in addition duffer from photons through the fact that they would not travel at the speed of light. The mass of the neutron ought to be about the same order of magnitude as the electron mass, and in any case could not be greater than 0.01 proton masses. The continuous beta spectrum would then become understandable by assuming that in beta decay a neutron is always emitted along with the electron, in such a way that the sum of the energies of the neutron and electron is a constant.

Now, the question is, what forces act on the neutron? The most likely model for the neutron seems to me, on wave mechanical grounds, to be the assumption that the motionless neutron is a magnetic dipole with a certain magnetic moment μ (the bearer of this letter can supply details). The experiments demand that the ionizing power of such a neutron cannot exceed that of a gamma ray, and therefore μ probably cannot be greater than $e(10^{-13}\text{cm})$. [*e* is the charge of the electron].

At the moment I do not dare to publish anything about this idea, so I first turn trustingly to you, dear radioactive friends, with the question: how could such a neutron be experimentally identified if it possessed about the same penetrating power as a gamma ray or perhaps 10 times greater penetrating power?

I admit that my way out may look rather improbable at first since if the neutron existed it would have been seen long ago. But nothing ventured, nothing gained. The gravity of the situation with the continuous beta spectrum was illuminated by a remark by my distinguished predecessor in office, Mr. DeBye, who recently said to me in Brussels, “Oh, that's a problem like the new taxes; one had best not think about it at all.” So one ought to discuss seriously any way that may lead to salvation. Well, dear radioactive friends, weigh it and pass sentence! Unfortunately, I cannot appear personally in Tubingen, for I cannot get away from Zurich on account of a ball which is held here on the night of December 6-7. With best regards to you and to Mr. Baek,

Your most obedient servant,
W. Pauli

(Quoted in Ford 1968, p. 849.)

This was a serious suggestion and although Pauli did not get all the properties of the neutrino right his suggestion was the basis of further work.

3. With a three-body decay the electron and the proton also didn't have to come off back to back. This observation was not made until the late 1930s. Assigning the neutrino a spin, intrinsic angular momentum, of h/4 also preserved the law of conservation of angular momentum.

4. The actual history is more complex. For a time, an alternative theory of decay, proposed by Konopinski and Uhlenbeck (1935) was better supported by the experimental evidence than was Fermi's theory. It was subsequently shown that both the experimental results and the theoretical calculations were wrong and that Fermi's theory was , in fact, supported by the evidence. For details see (Franklin 1990).

5. Allowed transitions are those for which both the electron and neutrino wavefunctions could be considered constant over nuclear dimensions. Forbidden transitions are those that included higher order terms in the perturbation series expansion of the matrix element.

6. I have been unable to find a published reference to this measurement. It is cited as a private communication in the literature.

7. In a post-deadline paper presented at the January 1958 meeting of the American Physical Society, Rustad and Ruby suggested that their earlier result might be wrong. There are no abstracts of post-deadline papers, but the talk was cited in the literature. Ruby remembers the tone of the paper as *mea culpa* (private communication).