Introduction

Scientists need no convincing that experiment plays an essential role in science. It provides the basis for theory choice, confirms or refutes hypotheses or theories, and sometimes calls for new theories. These are only a few of its roles and, as Ian Hacking (1983) has pointed out, experiment often has a life of its own. Nevertheless, I called my previous book *The Neglect of Experiment* (1986). Who was neglecting experiment? Certainly not scientists. I believed then that it was historians, philosophers, and sociologists of science. Even among those who acknowledged the importance of experimental results there tended to be an almost mythological treatment of a few standard exemplary experiments, such as Galileo and the Leaning Tower, Young's double slit interference experiment, and the Michelson-Morley experiment. Actual experiments were rarely discussed.

Fortunately, this is no longer the case. One of the most interesting and exciting trends in history, philosophy, and sociology of science in the 1980s has been the study of and emphasis on actual experiments. Philosophers such as Dudley Shapere (1982), Ian Hacking (1983), Nancy Cartwright (1983), and Robert Ackermann (1985) have used the actual practice of science to analyze and illuminate what good science should be. Historians such as Bruce Wheaton (1983), Peter Galison (1987), and Roger Stuewer (1975) have not only provided us with detailed studies of particular experiments, but have also given us new perspectives on the role of real experiments. Sociologists of science have added to our knowledge by their detailed studies of experiments. Although, as discussed in detail later, I disagree with their view that science is merely a social construction, there is no doubt that the work of Harry Collins

(1976), Andy Pickering (1984a, 1984b), and Trevor Pinch (1986) has enhanced our knowledge of experiment.[1]

Hacking began the discussion of how we come to believe rationally, or reasonably, in experimental results. Cartwright's emphasis on the actual practice of science has given us new insights into the nature of scientific explanation. Galison emphasizes the continuity of scientific instruments and of experimental apparatus and practice. He notes that changes in these rarely occur at the same time as major theoretical changes. Thus, they provide a continuous empirical basis for science, one that the usual theory-dominated accounts fail to give. Ackermann has also discussed the role of instruments in providing stable data.

In this book I will argue that the practice of science is reasonable.[2] This has been implicit in my previous work and I will discuss it explicitly here, and contrast it to the view that science is merely a social construction. The view of science I propose is what one might call an "evidence model" of science. I suggest that when questions of theory choice, confirmation, or refutation are raised they are answered on the basis of valid experimental evidence. I will also argue that there are good reasons for belief in the validity of that evidence. This is both a descriptive and a normative view. I believe that the history of science, as illustrated in the episodes presented here and in my (1986) book, demonstrates that scientists behave this way. I also believe that this is the way they should behave. The Bayesian approach to the philosophy of science, which I believe is both a fruitful way of looking at science and also part of a theory of rationality, requires that observation of evidence entailed by a hypothesis strengthen our belief in that hypothesis. Although I know of no episodes in the history of science in which scientific decisions have gone against the weight of evidence,[3] I think that scientists in that case would have been unreasonable.[4]

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[1] I have restricted myself here to discussions of twentieth-century experiments. There have also been several conferences and books devoted to the study of theory and experiment. See Achinstein and Hannaway (1985), Batens and van Bendegem (1988), and Gooding, Pinch, and Schaffer (1988).

[2] I will use the term "reasonable" rather than "rational" because I do not have a complete theory of rationality. I do believe, however, that part of such a theory includes the idea that observation of evidence entailed by a hypothesis should strengthen our belief in the hypothesis.

[3] I am referring here to the context of justification and not to the context of

There will, of course, be cases in which the evidence available at a given time may not be sufficient to decide an issue, or in which there are arguments as to what constitutes relevant evidence or on the validity of the evidence, but this is to be expected. Such episodes themselves assume an evidence model. Even in cases of scientific fraud, those who engage in the fraud are behaving in the way suggested by an evidence model by forging, cooking, or trimming their results to support their views. (See Franklin 1986, ch. 8, for details of some episodes of scientific fraud.)

I will continue my study of two questions. (1) What role does and should experiment play in the choice between competing theories and in the confirmation or refutation of theories and hypotheses? (2) How do we come to believe reasonably in experimental results? I hope to provide a fuller and more detailed philosophy of experiment than I have previously, including a somewhat different approach to the Duhem-Quine problem, or the localization of support or refutation. I will show that my previously presented epistemology of experiment, a set of strategies for reasonable belief in experimental results, can be explicated in terms of Bayesian confirmation theory. I will further argue that this, as well as other evidence, makes Bayesianism a fruitful way to look at science. I will also provide additional historical case studies, because I believe that the philosophy of science benefits from the study of actual science. One major difference will be the consideration of the fallibility and corrigibility of experimental results. In my previous case studies I dealt with episodes in which the experimental results are still regarded as valid and correct by the physics community. For example, subsequent work has not cast doubt on the experiments that demonstrated the nonconservation of parity or *CP* violation.[5] (See Franklin 1986, chs. 1 and 3, for details.)

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discovery or to what one might call the context of pursuit, the decision of a scientist to pursue a certain program of research.

[4] Bayesianism requires that you change your degree of belief using conditionalization. If you don't conditionalize there are bets that can be made against you such that you will always lose money.

[5] Even though the average value of the *CP*-violating parameter, η_{+-}, has changed from $(1.95 \pm 0.03) \times 10^{-3}$ to $(2.27 \pm 0.022) \times 10^{-3}$ the existence of *CP* violation has not been questioned. This change in the average value of this quantity is also an example of experimental fallibility. See Chapter 6 for

Episodes such as these often occur in the history of physics, giving us confidence in the strategies used to validate experimental results. Often, however, the history of physics shows that experimental results are both fallible and corrigible. In dealing with the complex interaction between theory and experiment the old cliché "Man proposes, Nature disposes" has been shown to be far from adequate. John Worrall's (1982) account of the nineteenth-century experiments that attempted to measure the pressure of light shows that it is not only difficult to know what man is proposing, but that it is also difficult to learn what Nature's disposition is. Not only did the experimental results change, but also their theoretical interpretation. The studies of the experiments that demonstrated the existence of weak neutral currents, by Galison (1987, ch. 4) and by Pickering (1984b), illustrate the same point. During the 1960s, events were seen that, in the light of later theoretical developments and experimental studies, are now interpreted as providing evidence for the existence of weak neutral currents. At the time, however, they were thought to be caused by neutron background. During the 1930s, experimental results were reported that, in retrospect, illustrate nonconservation of parity (Franklin 1986, ch. 2.) Their significance was not realized by the experimenters or by anyone else for more than twenty-five years. It was only after the discovery of parity nonconservation in the 1950s that these results were reinterpreted. In addition, history shows that the experimental results were first thought to be valid, then believed to be an instrumental artifact, then again thought to be correct, then believed again to be an artifact, and are currently thought to be correct. The interpretation and validation of experimental results is not a simple task.

In this study I will present detailed histories of two such episodes: (1) the interaction of experiment and theory in the development of the theory of weak interactions from Fermi's theory in 1934 to the *V-A* theory of 1957 and (2) atomic parity violating experiments in the 1970s and 1980s and their interaction with the Weinberg-Salam unified theory of electroweak interactions. In these episodes we will see not only that experimental results can

details. The original result of Christenson et al. (1964) of $(2.0 \pm 0.4) \times 10^{-3}$ is consistent with either average value.

be wrong, but also that theoretical calculations and the comparison between experiment and theory can also be incorrect.

Does the fallibility and corrigibility of experiment, of theory, and of their comparison affect our answers to the two questions posed earlier? Can we still maintain that experiment plays a legitimate role in theory choice and confirmation? Can we still argue that there are good strategies for reasonable belief in experimental results? I believe the answer to all of these questions is yes, and my arguments follow.

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I

EXPERIMENT AND THE DEVELOPMENT OF THE THEORY OF WEAK INTERACTIONS: FROM FERMI TO *V-A*

1

Fermi's theory

The fallibility and corrigibility of experimental results, of theoretical calculation, and of the comparison between experiment and theory will be amply illustrated in the episode to be discussed. This section will deal with the relation between experiment and theory in the field of weak interactions during the period between Fermi's proposal of his theory of β decay in 1934 and the acceptance of the *V-A* theory of weak interactions in 1959. Part of the fascination of this story is that the *V-A* theory appeared to be refuted by existing experimental evidence at the time it was proposed by Sudarshan and Marshak (1957) and by Feynman and Gell-Mann (1958). The authors, themselves, recognized this and suggested that the experimental results might be wrong, a suggestion that turned out to be correct. Nevertheless the theory was proposed because it seemed to be the only available candidate for a universal theory of the weak interaction. In this section I will examine the origin and development of this idea of a universal theory of the weak interaction to the acceptance of the *V-A* theory as such a theory.

Fermi's (1934a, 1934b) theory of β decay was introduced in 1934. It was not the first quantitative theory of β decay. Beck and Sitte (1933) had formulated an earlier theory using Dirac's prediction of the positron. According to their 1933 model an electron–positron pair was created. The positron was absorbed by the nucleus and the electron emitted (or vice versa). This theory had currency for a short time, but was rejected on experimental grounds, to be discussed below. Fermi assumed the existence of the neutrino, then recently proposed by Pauli,[1] and

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[1] Pauli had originally called the particle the neutron, but following Chadwick's discovery of a heavy neutral particle, Fermi coined the name neutrino, or little neutral one.

used the method of second quantization. He also assumed, with Heisenberg, that the nucleus contained only protons and neutrons and that the electron and the neutrino were created at the instant of decay. This was because no theory available at that time[2] could explain how an electron and a neutrino could be bound inside the nucleus. Fermi added a perturbing energy due to the decay interaction to the Hamiltonian of the nuclear system. In modern notation this perturbation is of the form

$$H_{if} = G\,[U^*_f \phi_e(r)\phi_\nu(r)]\; O_x U_i, \tag{1.1}$$

where U_i and U_f describe the initial and final states of the nucleus, ϕ_e and ϕ_ν are the electron and antineutrino wavefunctions, respectively, and O_x is a mathematical operator.

Pauli (1933) had previously shown that O_x can take on only five forms if the Hamiltonian is to be relativistically invariant. We identify these as *S*, the scalar interaction; *P*, pseudoscalar; *V*, polar vector; *A*, axial vector; and *T*, tensor.[3] Fermi knew this, but, in analogy with electromagnetic theory, and because his calculations were in agreement with experiment, he chose to use only the vector form of the interaction. He also considered only what he called "allowed" transitions, those for which the electron and neutrino wavefunctions could be considered constant over nuclear dimensions. He recognized that "forbidden" transitions

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[2] There are some contemporary theories that do allow an electron and a neutrino to be bound inside a nucleus. See, for example, Barut (1980, 1982). These theories are not widely accepted within the physics community.

[3] We wish to consider the relativistically invariant combinations of the wave functions. Let U_f and U_i represent the initial and final nuclear states and ϕ_e and ϕ_ν be the electron and antineutrino wavefunctions, respectively. Let Q be an operator which, when applied to the wavefunction describing the initial nuclear state, substitutes for it one in which a proton replaces a neutron. Q^* causes the nucleon to make the opposite transition. The five allowable interactions are:

Scalar: $S = (U^*_f \beta Q_k U_i)\,(\phi^*_e \beta \phi_\nu)$,
Vector: $V = (U^*_f Q_k U_i)\,(\phi^*_e\, \phi_\nu) - (U^*_f \alpha Q_k U_i)\,(\phi^*_e \alpha \phi_\nu)$,
Tensor: $T = (U^*_f \beta\sigma Q_k U_i)\,(\phi^*_e \beta\sigma \phi_\nu) + (U^*_f \beta\alpha Q_k U_i)\,(\phi^*_e \beta\alpha \phi_\nu)$,
Axial Vector: $A = (U^*_f \sigma Q_k U_i)\,(\phi^*_e \sigma \phi_\nu) - (U^*_f \gamma_5 Q_k U_i)\,(\phi^*_e \gamma_5 \phi_\nu)$,
Pseudoscalar: $P = (U^*_f \gamma_5 Q_k U_i)\,(\phi^*_e \beta\gamma_5 \phi\nu)$.

α is a vector whose three components are the Dirac matrices. σ differs from the usual Pauli spin matrices only in being doubled to four rows and four columns. β is the fourth Dirac matrix, and $\gamma_5 = -i\alpha_x\alpha_y\alpha_z$. See Konopinski (1943) for details.

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would also exist. The rate of such transitions would be much reduced and the shape of the spectrum would differ from that of the allowed transitions. He found that for allowed transitions certain selection rules would apply. These included: no change in the angular momentum of the nucleus, $\Delta J = 0$, and no change in the parity (space reflection properties) of the nuclear states. He also found for such transitions, assuming the mass of the neutrino was zero, that

$$P(W)\,dW = G^2\,|M|^2\,f(Z,W)\,(W_o - W)^2\,(W^2 - 1)^{1/2}\,W\,dW, \tag{1.2}$$

where W is the energy of the electron (in units of m_ec^2), W_o is the maximum energy allowed, $P(W)$ is the probability of the emission of an electron with energy W, and $f(Z,W)$ is a function giving the effect of the Coulomb field of the nucleus on the emission of electrons. It was later shown that for allowed transitions the energy dependence of the β spectrum was independent of the choice of interaction (Konopinski 1943). Fermi also showed that the value of $F(Z,W_o)\tau_o$ should be approximately constant for each type of transition, that is, allowed, first forbidden, second forbidden, and so forth. $F(Z,W_o)$ is the integral of the energy distribution and τ_o is the lifetime of the transition. Fermi cited already published experimental results in support of his theory, in particular the work of Sargent on the shape of β-decay spectra (Sargent 1932) and on decay constants and maximum electron energies (Sargent 1933). Sargent had found that if he plotted the logarithm of the disintegration constants (inversely proportional to the lifetime) against the logarithm of the maximum electron energy, the data for all measured decays fell into two distinct groups, known in the later literature as Sargent curves (Figure 1.1). Although Sargent had remarked that, "At present the significance of this general relation is not apparent" (1933, p. 671), that was what Fermi's theory required, namely, that $F\tau_o$ is approximately constant for each type of decay. (Note that the value of F depends on W_o, the maximum electron energy.) Fermi associated the two Sargent curves with the allowed and first forbidden transitions, in analogy with electromagnetic dipole and quadrupole radiation. The general shape of the observed spectra also agreed with Fermi's model.

Although Fermi's theory had received some confirmation, it

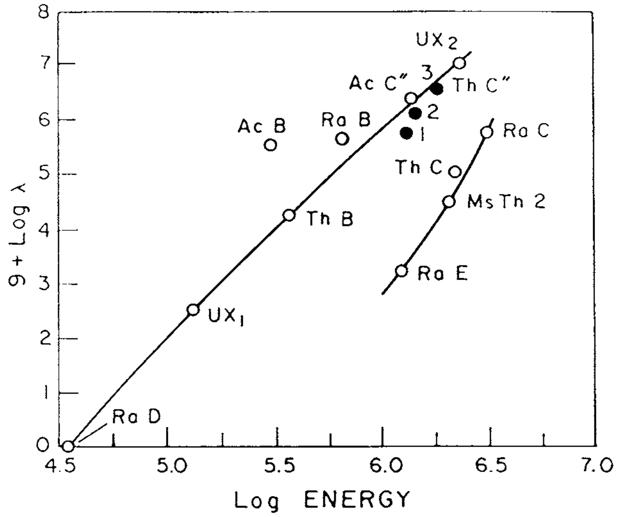

Figure 1.1. Logarithm of the decay constant (inversely proportional to the lifetime) plotted against the logarithm of the maximum decay energy. From Sargent (1933).

was quickly pointed out by Konopinski and Uhlenbeck (1935) that more detailed examination of the spectra showed that his theory predicted too few low-energy electrons and an average electron energy that was too high. They proposed their own model which modified Fermi's theory, but only to the extent that it included the derivative of the neutrino wavefunction rather than the wavefunction itself. They obtained the energy spectrum

$$P(W)dW = G^2|M|^2 f(Z,W)\,(W_o - W)^4(W^2 - 1)^{1/2}\,WdW \tag{1.3}$$

which differs from the Fermi prediction by an extra factor of $(W_o - W)^2$ (see Equation 1.2). This predicted more electrons at lower energy and a lower average energy than did Fermi's theory. They cited as support for their modification the spectra obtained from P^{30}, a positron emitter, by Ellis and Henderson (1934) and from RaE (Bi^{210} in modern notation) by Sargent (1932). The RaE spectrum is shown in Figure 1.2 and indicates the superior agreement of their modification with the experimental data. Their model also predicted that $F\tau_o$ would be approximately constant.

They remarked, however, that their improvement did not solve one of the outstanding problems of Fermi's theory. This was that using his theory to explain neutron–proton interactions resulted