

Experimentation and Scientific Realism

IAN HACKING

University of Toronto

Experimental physics provides the strongest evidence for scientific realism. Entities that in principle cannot be observed are regularly manipulated to produce new phenomena and to investigate other aspects of nature. They are tools, instruments not for thinking but for doing.

The philosopher's standard "theoretical entity" is the electron. I shall illustrate how electrons have become experimental entities, or experimenter's entities. In the early stages of our discovery of an entity, we may test hypotheses about it. Then it is merely an hypothetical entity. Much later, if we come to understand some of its causal powers and to use it to build devices that achieve well understood effects in other parts of nature, then it assumes quite a different status.

Discussions about scientific realism or anti-realism usually talk about theories, explanation and prediction. Debates at that level are necessarily inconclusive. Only at the level of experimental practice is scientific realism unavoidable. But this realism is not about theories and truth. The experimentalist need only be a realist about the entities used as tools.

A Plea for Experiments

No field in the philosophy of science is more systematically neglected than experiment. Our grade school teachers may have told us that scientific method is experimental method, but histories of science have become histories of theory. Experiments, the philosophers say, are of value only when they test theory. Experimental work, they imply, has no life of its own. So we lack even a terminology to describe the many varied roles of experiment. Nor has this one-sidedness done theory any good, for radically different types of theory are used to think about the same physical phenomenon (e.g., the magneto-optical effect). The philosophers of theory have not noticed this and so misreport even theoretical inquiry.¹

Different sciences at different times exhibit different relationships between "theory" and "experiment." One chief role of experiment

is the creation of phenomena. Experimenters bring into being phenomena that do not naturally exist in a pure state. These phenomena are the touchstones of physics, the keys to nature and the source of much modern technology. Many are what physicists after the 1870s began to call "effects": the photo-electric effect, the Compton effect, and so forth. A recent high-energy extension of the creation of phenomena is the creation of "events," to use the jargon of the trade. Most of the phenomena, effects and events created by the experimenter are like plutonium: they do not exist in nature except possibly on vanishingly rare occasions.²

In this paper I leave aside questions of methodology, history, taxonomy and the purpose of experiment in natural science. I turn to the purely philosophical issue of scientific realism. Call it simply "realism" for short. There are two basic kinds: realism about entities and realism about theories. There is no agreement on the precise definition of either. Realism about theories says we try to form true theories about the world, about the inner constitution of matter and about the outer reaches of space. This realism gets its bite from optimism; we think we can do well in this project, and have already had partial success.

Realism about entities—and I include processes, states, waves, currents, interactions, fields, black holes and the like among entities—asserts the existence of at least some of the entities that are the stock in trade of physics.³

The two realisms may seem identical. If you believe a theory, do you not believe in the existence of the entities it speaks about? If you believe in some entities, must you not describe them in some theoretical way that you accept? This seeming identity is illusory. The vast majority of experimental physicists are realists about entities without a commitment to realism about theories. The experimenter is convinced of the existence of plenty of "inferred" and "unobservable" entities. But no one in the lab believes in the literal truth of present theories about those entities. Although various properties are confidently ascribed to electrons, most of these properties can be embedded in plenty of different inconsistent theories about which the experimenter is agnostic. Even people working on adjacent parts of the same large experiment will use different and mutually incompatible accounts of what an electron is. That is because different parts of the experiment will make different uses of electrons, and the models that are useful for making calculations about one use may be completely haywire for another use.

Do I describe a merely sociological fact about experimentalists? It

is not surprising, it will be said, that these good practical people are realists. They need that for their own self-esteem. But the self-vindicating realism of experimenters shows nothing about what actually exists in the world. In reply I repeat the distinction between realism about entities and realism about theories and models. Anti-realism about models is perfectly coherent. Many research workers may in fact hope that their theories and even their mathematical models "aim at the truth," but they seldom suppose that any particular model is more than adequate for a purpose. By and large most experimenters seem to be instrumentalists about the models they use. The models are products of the intellect, tools for thinking and calculating. They are essential for writing up grant proposals to obtain further funding. They are rules of thumb used to get things done. Some experimenters are instrumentalists about theories and models, while some are not. That is a sociological fact. But experimenters are realists about the entities that they use in order to investigate other hypotheses or hypothetical entities. That is not a sociological fact. Their enterprise would be incoherent without it. But their enterprise is not incoherent. It persistently creates new phenomena that become regular technology. My task is to show that realism about entities is a necessary condition for the coherence of most experimentation in natural science.

Our Debt to Hilary Putnam

It was once the accepted wisdom that a word like "electron" gets its meaning from its place in a network of sentences that state theoretical laws. Hence arose the infamous problems of incommensurability and theory change. For if a theory is modified, how could a word like "electron" retain its previous meaning? How could different theories about electrons be compared, since the very word "electron" would differ in meaning from theory to theory?

Putnam saves us from such questions by inventing a referential model of meaning. He says that meaning is a vector, refreshingly like a dictionary entry. First comes the syntactic marker (part of speech). Next the semantic marker (general category of thing signified by the word). Then the stereotype (clichés about the natural kind, standard examples of its use and present day associations. The stereotype is subject to change as opinions about the kind are modified). Finally there is the actual reference of the word, the very stuff, or thing, it denotes if it denotes anything. (Evidently dictionaries cannot include this in their entry, but pictorial dictionaries do their best by inserting illustrations whenever possible.)⁴

Putnam thought we can often guess at entities that we do not literally point to. Our initial guesses may be jejune or inept, and not every naming of an invisible thing or stuff pans out. But when it does, and we frame better and better ideas, then Putnam says that although the stereotype changes, we refer to the same kind of thing or stuff all along. We and Dalton alike spoke about the same stuff when we spoke of (inorganic) acids. J. J. Thomson, Lorentz, Bohr and Millikan were, with their different theories and observations, speculating about the same kind of thing, the electron.

There is plenty of unimportant vagueness about when an entity has been successfully “dubbed,” as Putnam puts it. “Electron” is the name suggested by G. Johnstone Stoney in 1891 as the name for a natural unit of electricity. He had drawn attention to this unit in 1874. The name was then applied in 1897 by J. J. Thomson to the subatomic particles of negative charge of which cathode rays consist. Was Johnstone Stoney referring to the electron? Putnam’s account does not require an unequivocal answer. Standard physics books say that Thomson discovered the electron. For once I might back theory and say Lorentz beat him to it. What Thomson did was to measure the electron. He showed its mass is 1/1800 that of hydrogen. Hence it is natural to say that Lorentz merely postulated the particle of negative charge, while Thomson, determining its mass, showed that there is some such real stuff beaming off a hot cathode.

The stereotype of the electron has regularly changed, and we have at least two largely incompatible stereotypes, the electron as cloud and the electron as particle. One fundamental enrichment of the idea came in the 1920s. Electrons, it was found, have angular momentum, or “spin.” Experimental work by Stern and Gerlach first indicated this, and then Goudsmit and Uhlenbeck provided the theoretical understanding of it in 1925. Whatever we think about Johnstone Stoney, others—Lorentz, Bohr, Thomson and Goudsmit—were all finding out more about the same kind of thing, the electron.

We need not accept the fine points of Putnam’s account of reference in order to thank him for providing a new way to talk about meaning. Serious discussions of inferred entities need no longer lock us into pseudo-problems of incommensurability and theory change. Twenty-five years ago the experimenter who believed that electrons exist, without giving much credence to any set of laws about electrons, would have been dismissed as philosophically incoherent. We now realize it was the philosophy

that was wrong, not the experimenter. My own relationship to Putnam's account of meaning is like the experimenter's relationship to a theory. I don't literally believe Putnam, but I am happy to employ his account as an alternative to the unpalatable account in fashion some time ago.

Putnam's philosophy is always in flux. At the time of this writing, July 1981, he rejects any "metaphysical realism" but allows "internal realism."⁵ The internal realist acts, in practical affairs, as if the entities occurring in his working theories did in fact exist. However, the direction of Putnam's metaphysical anti-realism is no longer scientific. It is not peculiarly about natural science. It is about chairs and livers too. He thinks that the world does not naturally break up into our classifications. He calls himself a transcendental idealist. I call him a transcendental nominalist. I use the word "nominalist" in the old fashioned way, not meaning opposition to "abstract entities" like sets, but meaning the doctrine that there is no nonmental classification in nature that exists over and above our own human system of naming.

There might be two kinds of Putnamian internal realist—the instrumentalist and the scientific realist. The former is, in practical affairs where he uses his present scheme of concepts, a realist about livers and chairs, but he thinks that electrons are mental constructs only. The latter thinks that livers, chairs, and electrons are probably all in the same boat, that is, real at least within the present system of classification. I take Putnam to be an internal scientific realist rather than an internal instrumentalist. The fact that either doctrine is compatible with transcendental nominalism and internal realism shows that our question of scientific realism is almost entirely independent of Putnam's present philosophy.

Interfering

Francis Bacon, the first and almost last philosopher of experiments, knew it well: the experimenter sets out "to twist the lion's tail." Experimentation is interference in the course of nature; "nature under constraint and vexed; that is to say, when by art and the hand of man she is forced out of her natural state, and squeezed and moulded."⁶ The experimenter is convinced of the reality of entities some of whose causal properties are sufficiently well understood that they can be used to interfere *elsewhere* in nature. One is impressed by entities that one can use to test conjectures about other more hypothetical entities. In my example, one is sure of the electrons that are used to investigate weak neutral currents and

neutral bosons. This should not be news, for why else are we (non-sceptics) sure of the reality of even macroscopic objects, but because of what we do with them, what we do to them, and what they do to us?

Interference and intervention are the the stuff of reality. This is true, for example, at the borderline of observability. Too often philosophers imagine that microscopes carry conviction because they help us see better. But that is only part of the story. On the contrary, what counts is what we can do to a specimen under a microscope, and what we can see ourselves doing. We stain the specimen, slice it, inject it, irradiate it, fix it. We examine it using different kinds of microscopes that employ optical systems that rely on almost totally unrelated facts about light. Microscopes carry conviction because of the great array of interactions and interferences that are possible. When we see something that turns out not to be stable under such play, we call it an artefact and say it is not real.⁷

Likewise, as we move down in scale to the truly un-seeable, it is our power to use unobservable entities that make us believe they are there. Yet I blush over these words "see" and "observe." John Dewey would have said that a fascination with seeing-with-the-naked-eye is part of the Spectator Theory of Knowledge that has bedeviled philosophy from earliest times. But I don't think Plato or Locke or anyone before the nineteenth century was as obsessed with the sheer opacity of objects as we have been since. My own obsession with a technology that manipulates objects is, of course a twentieth-century counterpart to positivism and phenomenology. Their proper rebuttal is not a restriction to a narrower domain of reality, namely to what can be positivistically "seen" (with the eye), but an extension to other modes by which people can extend their consciousness.

Making

Even if experimenters are realists about entities, it does not follow that they are right. Perhaps it is a matter of psychology: the very skills that make for a great experimenter go with a certain cast of mind that objectifies whatever it thinks about. Yet this will not do. The experimenter cheerfully regards neutral bosons as merely hypothetical entities, while electrons are real. What is the difference?

There are an enormous number of ways to make instruments that rely on the causal properties of electrons in order to produce desired

effects of unsurpassed precision. I shall illustrate this. The argument—it could be called the experimental argument for realism—is not that we infer the reality of electrons from our success. We do not make the instruments and then infer the reality of the electrons, as when we test a hypothesis, and then believe it because it passed the test. That gets the time–order wrong. By now we design apparatus relying on a modest number of home truths about electrons to produce some other phenomenon that we wish to investigate.

That may sound as if we believe in the electrons because we predict how our apparatus will behave. That too is misleading. We have a number of general ideas about how to prepare polarized electrons, say. We spend a lot of time building prototypes that don't work. We get rid of innumerable bugs. Often we have to give up and try another approach. Debugging is not a matter of theoretically explaining or predicting what is going wrong. It is partly a matter of getting rid of "noise" in the apparatus. "Noise" often means all the events that are not understood by any theory. The instrument must be able to isolate, physically, the properties of the entities that we wish to use, and damp down all the other effects that might get in our way. *We are completely convinced of the reality of electrons when we regularly set out to build—and often enough succeed in building—new kinds of devices that use various well understood causal properties of electrons to interfere in other more hypothetical parts of nature.*

It is not possible to grasp this without an example. Familiar historical examples have usually become encrusted by false theory-oriented philosophy or history. So I shall take something new. This is a polarizing electron gun whose acronym is PEGGY II. In 1978 it was used in a fundamental experiment that attracted attention even in *The New York Times*. In the next section I describe the point of making PEGGY II. So I have to tell some new physics. You can omit this and read only the engineering section that follows. Yet it must be of interest to know the rather easy-to-understand significance of the main experimental results, namely, (1) parity is not conserved in scattering of polarized electrons from deuterium, and (2) more generally, parity is violated in weak neutral current interactions.⁸

Methodological Remark

In the following section I retail a little current physics; in the section after that I describe how a machine has been made. It is the latter that matters to my case, not the former. Importantly, even if present

quantum electrodynamics turns out to need radical revision, the machine, called PEGGY II, will still work. I am concerned with how it was made to work, and why. I shall sketch far more sheer engineering than is seen in philosophy papers. My reason is that the engineering is incoherent unless electrons are taken for granted. One cannot say this by merely reporting, "Oh, they made an electron gun for shooting polarized electrons." An immense practical knowledge of how to manipulate electrons, of what sorts of things they will do reliably and how they tend to misbehave—that is the kind of knowledge which grounds the experimenter's realism about electrons. You cannot grasp this kind of knowledge in the abstract, for it is practical knowledge. So I must painfully introduce the reader to some laboratory physics. Luckily it is a lot of fun.

Parity and Weak Neutral Currents

There are four fundamental forces in nature, not necessarily distinct. Gravity and electromagnetism are familiar. Then there are the strong and weak forces, the fulfillment of Newton's program, in the *Optics*, which taught that all nature would be understood by the interaction of particles with various forces that were effective in attraction or repulsion over various different distance (i.e., with different rates of extinction).

Strong forces are 100 times stronger than electromagnetism but act only for a miniscule distance, at most the diameter of a proton. Strong forces act on "hadrons," which include protons, neutrons, and more recent particles, but not electrons or any other members of the class of particles called "leptons."

The weak forces are only 1/10,000 times as strong as electromagnetism, and act over a distance 1/100 times smaller than strong forces. But they act on both hadrons and leptons, including electrons. The most familiar example of a weak force may be radioactivity.

The theory that motivates such speculation is quantum electrodynamics. It is incredibly successful, yielding many predictions better than one part in a million, a miracle in experimental physics. It applies over distances ranging from diameters of the earth to 1/100 the diameter of the proton. This theory supposes that all the forces are "carried" by some sort of particle. Photons do the job in electromagnetism. We hypothesize "gravitons" for gravity.

In the case of interactions involving weak forces, there are charged currents. We postulate that particles called bosons carry

these weak forces.⁹ For charged currents, the bosons may be positive or negative. In the 1970s there arose the possibility that there could be weak “neutral” currents in which no charge is carried or exchanged. By sheer analogy with the vindicated parts of quantum electrodynamics, neutral bosons were postulated as the carriers in weak interactions.

The most famous discovery of recent high energy physics is the failure of the conservation of parity. Contrary to the expectations of many physicists and philosophers, including Kant,¹⁰ nature makes an absolute distinction between right-handedness and left-handedness. Apparently this happens only in weak interactions.

What we mean by right- or left-handed in nature has an element of convention. I remarked that electrons have spin. Imagine your right hand wrapped around a spinning particle with the fingers pointing in the direction of spin. Then your thumb is said to point in the direction of the spin vector. If such particles are traveling in a beam, consider the relation between the spin vector and the beam. If all the particles have their spin vector in the same direction as the beam, they have right-handed (linear) polarization, while if the spin vector is opposite to the beam direction, they have left-handed (linear) polarization.

The original discovery of parity violation showed that one kind of product of a particle decay, a so-called *muon neutrino*, exists only in left-handed polarization and never in right-handed polarization.

Parity violations have been found for weak *charged* interactions. What about weak *neutral* currents? The remarkable Weinberg-Salam model for the four kinds of force was proposed independently by Stephen Weinberg in 1967 and A. Salam in 1968. It implies a minute violation of parity in weak neutral interactions. Given that the model is sheer speculation, its success has been amazing, even awe inspiring. So it seemed worthwhile to try out the predicted failure of parity for weak neutral interactions. That would teach us more about those weak forces that act over so minute a distance.

The prediction is: Slightly more left-handed polarized electrons hitting certain targets will scatter, than right-handed electrons. Slightly more! The difference in relative frequency of the two kinds of scattering is one part in 10,000, comparable to a difference in probability between 0.50005 and 0.49995. Suppose one used the standard equipment available at the Stanford Linear Accelerator in the early 1970s, generating 120 pulses per second, each pulse

providing one electron event. Then you would have to run the entire SLAC beam for 27 years in order to detect so small a difference in relative frequency. Considering that one uses the same beam for lots of experiments simultaneously, by letting different experiments use different pulses, and considering that no equipment remains stable for even a month, let alone 27 years, such an experiment is impossible. You need enormously more electrons coming off in each pulse. We need between 1000 and 10,000 more electrons per pulse than was once possible. The first attempt used an instrument now called PEGGY I. It had, in essence, a high-class version of J. J. Thomson's hot cathode. Some lithium was heated and electrons were boiled off. PEGGY II uses quite different principles.

PEGGY II

The basic idea began when C. Y. Prescott noticed, (by "chance"!) an article in an optics magazine about a crystalline substance called Gallium Arsenide. GaAs has a number of curious properties that make it important in laser technology. One of its quirks is that when it is struck by circularly polarized light of the right frequencies, it emits a lot of linearly polarized electrons. There is a good rough and ready quantum understanding of why this happens, and why half the emitted electrons will be polarized, $\frac{3}{4}$ polarized in one direction and $\frac{1}{4}$ polarized in the other.

PEGGY II uses this fact, plus the fact that GaAs emits lots of electrons due to features of its crystal structure. Then comes some engineering. It takes work to liberate an electron from a surface. We know that painting a surface with the right substance helps. In this case, a thin layer of Cesium and Oxygen is applied to the crystal. Moreover the less air pressure around the crystal, the more electrons will escape for a given amount of work. So the bombardment takes place in a good vacuum at the temperature of liquid nitrogen.

We need the right source of light. A laser with bursts of red light (7100 Ångstroms) is trained on the crystal. The light first goes through an ordinary polarizer, a very old-fashioned prism of calcite, or Iceland spar.¹¹ This gives longitudinally polarized light. We want circularly polarized light to hit the crystal. The polarized laser beam now goes through a cunning modern device, called a Pockel's cell. It electrically turns linearly polarized photons into circularly polarized ones. Being electric, it acts as a very fast switch. The direction of circular polarization depends on the direction of current in the cell. Hence the direction of polarization can be varied randomly. This is

important, for we are trying to detect a minute asymmetry between right and left handed polarization. Randomizing helps us guard against any systematic “drift” in the equipment.¹² The randomization is generated by a radioactive decay device, and a computer records the direction of polarization for each pulse.

A circularly polarized pulse hits the GaAs crystal, resulting in a pulse of linearly polarized electrons. A beam of such pulses is maneuvered by magnets into the accelerator for the next bit of the experiment. It passes through a device that checks on a proportion of polarization along the way. The remainder of the experiment requires other devices and detectors of comparable ingenuity, but let us stop at PEGGY II.

Bugs

Short descriptions make it all sound too easy, so let us pause to reflect on debugging. Many of the bugs are never understood. They are eliminated by trial and error. Let us illustrate three different kinds: (1) The essential technical limitations that in the end have to be factored into the analysis of error. (2) Simpler mechanical defects you never think of until they are forced on you. (3) Hunches about what might go wrong.

1. Laser beams are not as constant as science fiction teaches, and there is always an irremediable amount of “jitter” in the beam over any stretch of time.

2. At a more humdrum level the electrons from the GaAs crystal are back-scattered and go back along the same channel as the laser beam used to hit the crystal. Most of them are then deflected magnetically. But some get reflected from the laser apparatus and get back into the system. So you have to eliminate these new ambient electrons. This is done by crude mechanical means, making them focus just off the crystal and so wander away.

3. Good experimenters guard against the absurd. Suppose that dust particles on an experimental surface lie down flat when a polarized pulse hits it, and then stand on their heads when hit by a pulse polarized in the opposite direction? Might that have a systematic effect, given that we are detecting a minute asymmetry? One of the team thought of this in the middle of the night and came down next morning frantically using antidust spray. They kept that up for a month, just in case.¹³

Results

Some 10^{11} events were needed to obtain a result that could be

recognized above systematic and statistical error. Although the idea of systematic error presents interesting conceptual problems, it seems to be unknown to philosophers. There were systematic uncertainties in the detection of right- and left-handed polarization, there was some jitter, and there were other problems about the parameters of the two kinds of beam. These errors were analyzed and linearly added to the statistical error. To a student of statistical inference this is real seat-of-the-pants analysis with no rationale whatsoever. Be that as it may, thanks to PEGGY II the number of events was big enough to give a result that convinced the entire physics community.¹⁴ Left-handed polarized electrons were scattered from deuterium slightly more frequently than right-handed electrons. This was the first convincing example of parity-violation in a weak neutral current interaction.

Comment

The making of PEGGY II was fairly non-theoretical. Nobody worked out in advance the polarizing properties of GaAs—that was found by a chance encounter with an unrelated experimental investigation. Although elementary quantum theory of crystals explains the polarization effect, it does not explain the properties of the actual crystal used. No one has been able to get a real crystal to polarize more than 37 percent of the electrons, although in principle 50 percent should be polarized.

Likewise although we have a general picture of why layers of cesium and oxygen will “produce negative electron affinity,” i.e., make it easier for electrons to escape, we have no quantitative understanding of why this increases efficiency to a score of 37 percent.

Nor was there any guarantee that the bits and pieces would fit together. To give an even more current illustration, future experimental work, briefly described later in this paper, makes us want even more electrons per pulse than PEGGY II could give. When the parity experiment was reported in *The New York Times*, a group at Bell Laboratories read the newspaper and saw what was going on. They had been constructing a crystal lattice for totally unrelated purposes. It uses layers of GaAs and a related aluminum compound. The structure of this lattice leads one to expect that virtually all the electrons emitted would be polarized. So we might be able to doubt the efficiency of PEGGY II. But at present (July 1981) that nice idea has problems. The new lattice should also be coated in work-reducing paint. But the cesium oxygen stuff is applied at high

temperature. Then the aluminum tends to ooze into the neighboring layer of GaAs, and the pretty artificial lattice becomes a bit uneven, limiting its fine polarized–electron–emitting properties. So perhaps this will never work.¹⁵ The group are simultaneously reviving a souped up new thermionic cathode to try to get more electrons. Maybe PEGGY II would have shared the same fate, never working, and thermionic devices would have stolen the show.

Note, incidentally, that the Bell people did not need to know a lot of weak neutral current theory to send along their sample lattice. They just read *The New York Times*.

Moral

Once upon a time it made good sense to doubt that there are electrons. Even after Millikan had measured the charge on the electron, doubt made sense. Perhaps Millikan was engaging in “inference to the best explanation.” The charges on his carefully selected oil drops were all small integral multiples of a least charge. He inferred that this is the real least charge in nature, and hence it is the charge on the electron, and hence there are electrons, particles of least charge. In Millikan’s day most (but not all) physicists did become increasingly convinced by one or more theories about the electron. However it is always admissible, at least for philosophers, to treat inferences to the best explanation in a purely instrumental way, without any commitment to the existence of entities used in the explanation.¹⁶ But it is now seventy years after Millikan, and we no longer have to infer from explanatory success. Prescott et al., don’t explain phenomena with electrons. They know a great deal about how to use them.

The group of experimenters do not know what electrons are, exactly. Inevitably they think in terms of particles. There is also a cloud picture of an electron which helps us think of complex wavefunctions of electrons in a bound state. The angular momentum and spin vector of a cloud make little sense outside a mathematical formalism. A beam of polarized clouds is fantasy so no experimenter uses that model—not because of doubting its truth, but because other models help more with the calculations. Nobody thinks that electrons “really” are just little spinning orbs about which you could, with a small enough hand, wrap the fingers and find the direction of spin along the thumb. There is instead a family of causal properties in terms of which gifted experimenters describe and deploy electrons in order to investigate something else, e.g., weak neutral currents and neutral bosons. We know an enormous

amount about the behavior of electrons. We also know what does not matter to electrons. Thus we know that bending a polarized electron beam in magnetic coils does not affect polarization in any significant way. We have hunches, too strong to ignore although too trivial to test independently: e.g., dust might dance under changes of directions of polarization. Those hunches are based on a hard-won sense of the kinds of things electrons are. It does not matter at all to this hunch whether electrons are clouds or particles.

The experimentalist does not believe in electrons because, in the words retrieved from mediaeval science by Duhem, they “save the phenomena.” On the contrary, we believe in them because we use them to *create* new phenomena, such as the phenomenon of parity violation in weak neutral current interactions.

When Hypothetical Entities Become Real

Note the complete contrast between electrons and neutral bosons. Nobody can yet manipulate a bunch of neutral bosons, if there are any. Even weak neutral currents are only just emerging from the mists of hypothesis. By 1980 a sufficient range of convincing experiments had made them the object of investigation. When might they lose their hypothetical status and become commonplace reality like electrons? When we use them to investigate something else.

I mentioned the desire to make a better gun than PEGGY II. Why? Because we now “know” that parity is violated in weak neutral interactions. Perhaps by an even more grotesque statistical analysis than that involved in the parity experiment, we can isolate just the weak interactions. That is, we have a lot of interactions, including say electromagnetic ones. We can censor these in various ways, but we can also statistically pick out a class of weak interactions as precisely those where parity is not conserved. This would possibly give us a road to quite deep investigations of matter and anti-matter. To do the statistics one needs even more electrons per pulse than PEGGY II could hope to generate. If such a project were to succeed, we should be beginning to use weak neutral currents as a manipulable tool for looking at something else. The next step towards a realism about such currents would have been made.

The message is general and could be extracted from almost any branch of physics. Dudley Shapere has recently used “observation” of the sun’s hot core to illustrate how physicists employ the concept of observation. They collect neutrinos from the sun in an enormous disused underground mine that has been filled with the old cleaning

fluid (i.e., Carbon Tetrachloride). We would know a lot about the inside of the sun if we knew how many solar neutrinos arrive on the earth. So these are captured in the cleaning fluid; a few will form a new radioactive nucleus. The number that do this can be counted. Although the extent of neutrino manipulation is much less than electron manipulation in the PEGGY II experiment, here we are plainly using neutrinos to investigate something else. Yet not many years ago, neutrinos were about as hypothetical as an entity could get. After 1946 it was realized that when mesons disintegrate, giving off, among other things, highly energized electrons, one needed an extra nonionizing particle to conserve momentum and energy. At that time this postulated "neutrino" was thoroughly hypothetical, but now it is routinely used to examine other things.

Changing Times

Although realisms and anti-realisms are part of the philosophy of science well back into Greek prehistory, our present versions mostly descend from debates about atomism at the end of the nineteenth century. Anti-realism about atoms was partly a matter of physics: the energeticists thought energy was at the bottom of everything, not tiny bits of matter. It also was connected with the positivism of Comte, Mach, Pearson and even J. S. Mill. Mill's young associate Alexander Bain states the point in a characteristic way, apt for 1870:

Some hypotheses consist of assumptions as to the minute structure and operations of bodies. From the nature of the case these assumptions can never be proved by direct means. Their merit is their suitability to express phenomena. They are Representative Fictions.¹⁷

"All assertions as to the ultimate structure of the particles of matter," continues Bain, "are and ever must be hypothetical" The kinetic theory of heat, he says, "serves an important intellectual function." But we cannot hold it to be a true description of the world. It is a Representative Fiction.

Bain was surely right a century ago. Assumptions about the minute structure of matter could not be proved then. The only proof could be indirect, namely that hypotheses seemed to provide some explanation and helped make good predictions. Such inferences need never produce conviction in the philosopher inclined to instrumentalism or some other brand of idealism.

Indeed the situation is quite similar to seventeenth-century epistemology. At that time knowledge was thought of as correct

representation. But then one could never get outside the representations to be sure that they corresponded to the world. Every test of a representation is just another representation. "Nothing is so much like an idea as an idea," as Bishop Berkeley had it. To attempt to argue for scientific realism at the level of theory, testing, explanation, predictive success, convergence of theories and so forth is to be locked into a world of representations. No wonder that scientific anti-realism is so permanently in the race. It is a variant on "The Spectator Theory of Knowledge."

Scientists, as opposed to philosophers, did in general become realists about atoms by 1910. Michael Gardner, in one of the finest studies of real-life scientific realism, details many of the factors that went into that change in climate of opinion.¹⁸ Despite the changing climate, some variety of instrumentalism or fictionalism remained a strong philosophical alternative in 1910 and in 1930. That is what the history of philosophy teaches us. Its most recent lesson is Bas van Fraassen's *The Scientific Image*, whose "constructive empiricism" is another theory-oriented anti-realism. The lesson is: think about practice, not theory.

Anti-realism about atoms was very sensible when Bain wrote a century ago. Anti-realism about *any* sub-microscopic entities was a sound doctrine in those days. Things are different now. The "direct" proof of electrons and the like is our ability to manipulate them using well understood low-level causal properties. I do not of course claim that "reality" is constituted by human manipulability. We can, however, call something real, in the sense in which it matters to scientific realism, only when we understand quite well what its causal properties are. The best evidence for this kind of understanding is that we can set out, from scratch, to build machines that will work fairly reliably, taking advantage of this or that causal nexus. Hence, engineering, not theorizing, is the proof of scientific realism about entities.¹⁹

NOTES

1. C. W. F. Everitt and Ian Hacking, "Which Comes First, Theory or Experiment?"
2. Ian Hacking, "Spekulation, Berechnung und die Erschaffung der Phänomene," in *Versuchungen: Aufsätze zur Philosophie Paul Feyerabend's*, (P. Duerr, ed.), Frankfurt, 1981, Bd 2, 126-58.
3. Nancy Cartwright makes a similar distinction in a sequence of papers, including "When Explanation Leads to Inference," in the present issue. She approaches realism from the top, distinguishing theoretical laws (which do not state the facts) from phenomenological laws (which do). She believes in some "theoretical" entities and rejects much theory on the basis of a subtle analysis of modeling in physics. I proceed in the opposite direction, from experimental practice. Both approaches share

an interest in real-life physics as opposed to philosophical fantasy science. My own approach owes an enormous amount to Cartwright's parallel developments, which have often preceded my own. My use of the two kinds of realism is a case in point.

4. Hilary Putnam, "How Not To Talk About Meaning," "The meaning of 'Meaning'," and other papers in the *Mind, Language and Reality, Philosophical Papers*, Vol. 2. Cambridge, 1975.

5. These terms occur in e.g., Hilary Putnam, *Meaning and the Moral Sciences*, London, 1978, 123–30.

6. Francis Bacon, *The Great Instauration*, in *The Philosophical Works of Francis Bacon* (J. M. Robertson, ed; Ellis and Spedding, Trans.), London, 1905, p. 252.

7. Ian Hacking, "Do We See Through a Microscope?" *Pacific Philosophical Quarterly*, winter 1981.

8. I thank Melissa Franklin, of the Stanford Linear Accelerator, for introducing me to PEGGY II and telling me how it works. She also arranged discussions with members of the PEGGY II group, some of which are mentioned below. The report of experiment E-122 described here is "Parity Non-conservation in Inelastic Electron Scattering," C. Y. Prescott et al., *Physics Letters*. I have relied heavily on the in-house journal, the *SLAC Beam Line*, Report No. 8, October, 1978, "Parity Violation in Polarized Electron Scattering." This was prepared by the in-house science writer Bill Kirk, who is the clearest, most readable popularizer of difficult new experimental physics that I have come across.

9. The odd-sounding bosons are named after the Indian physicist S. N. Bose (1894–1974), also remembered in the name "Bose-Einstein statistics" (which bosons satisfy).

10. But excluding Leibniz, who "knew" there had to be some real, natural difference between right- and left-handedness.

11. Iceland spar is an elegant example of how experimental phenomena persist even while theories about them undergo revolutions. Mariners brought calcite from Iceland to Scandinavia. Erasmus Batholinus experimented with it and wrote about it in 1609. When you look through these beautiful crystals you see double, thanks to the so-called ordinary and extraordinary rays. Calcite is a natural polarizer. It was our entry to polarized light which for 300 years was the chief route to improved theoretical and experimental understanding of light and then electromagnetism. The use of calcite in PEGGY II is a happy reminder of a great tradition.

12. It also turns GaAs, a $\frac{3}{4}$ - $\frac{1}{4}$ left/right hand polarizer, into a 50–50 polarizer.

13. I owe these examples to conversation with Roger Miller of SLAC.

14. The concept of a "convincing experiment" is fundamental. Peter Gallison has done important work on this idea, studying European and American experiments on weak neutral currents conducted during the 1970s.

15. I owe this information to Charles Sinclair of SLAC.

16. My attitude to "inference to the best explanation" is one of many learned from Cartwright. See, for example, her paper on this topic in this issue.

17. Alexander Bain, *Logic, Deductive and Inductive*, London and New York, 1870, p. 362.

18. Michael Gardner, "Realism and Instrumentalism in 19th-Century Atomism," *Philosophy of Science* 46, (1979), 1–34.

19. (Added in proof, February, 1983). As indicated in the text, this is a paper of July, 1981, and hence is out of date. For example, neutral bosons are described as purely hypothetical. Their status has changed since CERN announced on Jan. 23, 1983, that a group there had found *W*, the weak intermediary boson, in proton-antiproton decay at 540 GeV. These experimental issues are further discussed in my forthcoming book, *Representing and Intervening* (Cambridge, 1983).