

The Theory Ladenness of Experiment

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In 1958 Norwood Russell Hanson remarked that “There is more to seeing than meets the eyeball.” Hanson was correctly pointing out that what we know influences what we observe. This view was later transformed by Thomas Kuhn and Paul Feyerabend into the “theory ladenness of observation” and its cousin “incommensurability.” Each of these problems has both a philosophical and pragmatic component.

I will deal first with theory ladenness. This is the view that observation cannot function as an unbiased way of testing theories because observational judgments are affected by the theoretical beliefs of the observer. Some philosophers of science, including myself, attempted to avoid this problem by looking at the theory of the experimental apparatus and the theory of the phenomenon under investigation. They argued that if the two theories are distinct then the problem can be avoided. Sometimes, however, that distinction cannot be made. An exemplar is the use of a mercury thermometer to test whether objects expand as their temperature increases. The operation of the thermometer depends on the hypothesis under test. One may argue, however, that one may still use a mercury thermometer in such an experiment if that thermometer can be calibrated against another independent thermometer such as a constant-volume-gas thermometer.

It is clear that Kuhn did not intend the above view of theory ladenness because in his theory of scientific revolutions the motor is provided by “anomalies,” experiments that disagree with the predictions of the paradigm or theory under test. Examples of this are the experiments of Lummer and Pringsheim and of Rubens and Kurlbaum on the spectrum of blackbody radiation

that provided evidence against Wien's Law and provided the impetus for the introduction of quantization by Max Planck.

Kuhn, Feyerabend, and others have argued that there can be no comparison between competing paradigms, or worldviews, based solely on experimental evidence. As Barry Barnes stated, "There is no appropriate scale available with which to weigh the merits of alternative paradigms: they are incommensurable." Briefly stated the argument is as follows. There can be no neutral observation language. All observation terms are theory laden and thus we cannot compare experimental results because in different paradigms terms describing experimental results have different meanings, even when the words used are the same. An example would be the term "mass," which in Newtonian mechanics is a constant, whereas in Einstein's relativistic mechanics it depends on the velocity of the object.

I disagree. I will demonstrate that a procedurally defined experiment, loosely called the elastic scattering of equal mass objects (protons, if you will), can distinguish between Newtonian and Einsteinian mechanics. In such an experiment the Newtonian prediction for the angle between the two outgoing particles is 90° , whereas in relativistic mechanics the angle is less than 90° . Although adherents of the two competing views will describe the experiments differently, they will agree on the respective predictions and on the measurement of the angles in the laboratory system. Thus, the two paradigms can be compared.

A real-life example of this is the experiment that demonstrated that parity, or left-right- or mirror-symmetry, is violated in the weak interactions. In this episode one examined the beta decay of aligned nuclei (the spins point in the same direction). If, for example, more electrons are emitted opposite to the nuclear spin direction than in the same direction, then this would demonstrate that parity, or mirror symmetry, is violated. In a mirror the spin of the nucleus is reversed, whereas the momentum direction remains the same. Thus, the real and mirror experiments would differ. In the mirror experiment more electrons are emitted in the same direction as the nuclear spin, whereas in real space more electrons are emitted opposite to the nuclear spin direction. This asymmetry was, in fact, observed in an experiment done by Wu and her collaborators. That experiment, along with two others, decided the issue. Although this may not be as general as a paradigm shift, the violation of a general discrete symmetry principle should be, and was, regarded as a major change in theory. In this case, because there are only

two classes of theory, those that conserve parity and those that do not, one can even avoid the Duhem-Quine problem.

The practical problems are perhaps more difficult to solve. Virtually all experiments, except for those we can regard as exploratory, are design and conducted under the auspices of some theory. One might worry that adherence to a particular theory may result in an experimental design that precludes observation of phenomena not predicted by the theory. An example of this, discussed by Peter Galison, was experiment E1A at Fermilab, one of the experiments that first discovered the existence of weak neutral currents. When the experiment was initially conceived it was a rule of thumb in particle physics that weak neutral currents did not exist. The initial design included a muon trigger, which would only be present in charged current interactions. After discussion with theorists, who pointed out that the recently proposed Weinberg-Salam unified theory of electroweak interactions predicted neutral currents, the trigger was changed so that neutral currents could be observed. In its original form, the experiment could not have detected those currents.

An episode in which both of these problems are illustrated occurred in the experiments that investigated the double scattering of electrons from heavy nuclei in the 1920s and 1930s. In analogy with x-ray scattering, it was believed that the first scatter would polarize the electrons and the second scatter would detect that polarization by observing a forward-backward ($0^\circ - 180^\circ$) asymmetry in the second scattering. None of the early experiments, those performed in the 1920s found such an asymmetry or evidence for electron polarization. One experiment, performed by Richard Cox, Charles McIlwraith, and Bernard Kurrelmeyer, did, however, observe an unexpected left-right ($90^\circ - 270^\circ$) asymmetry in the second scattering. In 1932, Neville Mott, on the basis of Paul Dirac's electron theory, proposed a quantitative theory of double-scattering and predicted a forward-backward asymmetry of approximately 10 %. The failure to observe that asymmetry cast serious doubt on Dirac's theory, which had, at the time, very strong support because of its prediction of the positron and its subsequent confirmation by Carl Anderson. Mott admitted that his theory did not say anything about a ($90^\circ - 270^\circ$) asymmetry. Subsequent experiments during the 1930s, all unsuccessful, searched for the ($0^\circ - 180^\circ$) asymmetry to try to resolve the anomaly for Dirac theory. No experiments attempted to replicate the ($90^\circ - 270^\circ$) asymmetry found by Cox and his collaborators. It wasn't thought to be

theoretically important, or, at the very least, as theoretically important as the failure to observe the ($0^\circ - 180^\circ$) asymmetry.

There were, in addition, several theoretical attempts to resolve the discrepancy. All were unsuccessful. It wasn't until the early 1940 that an experimental problem was found that had precluded the observation of the effect predicted by Mott. When that problem was corrected, the predicted asymmetry was observed. Ironically, it was the work of Cox and others who solved the problem. By that time even they did not recall their earlier results on the ($90^\circ - 270^\circ$) asymmetry. It seems clear that the lack of a theoretical context was responsible for the failure to even attempt replication of the Cox results.

Interestingly, the effect observed by Cox et al., who did not recognize its importance, demonstrates, at least in retrospect, the nonconservation of parity. One can see that Cox and his collaborators came tantalizingly close to recognizing the significance of their work. "It should be remarked of several of the suggested explanations [of their result] that their acceptance would offer greater difficulties in accounting for the discrepancies among the different results than would the acceptance of the hypothesis that *we have here a true polarization due to the double scattering of asymmetrical electrons* (emphasis added)." Electrons from beta decay are, in fact, longitudinally polarized so that the first scatter transforms that longitudinal polarization into a transverse polarization, which results in the ($90^\circ - 270^\circ$) asymmetry found in the second scatter. The longitudinal polarization implied by the ($90^\circ - 270^\circ$) asymmetry is itself evidence for parity nonconservation. Although parity conservation in quantum mechanics had been suggested in 1927 by Eugene Wigner, its importance was not widely appreciated. The lack of a theoretical context, unlike that which was available in the 1950s, when the Wu experiment was done, accounts, in all probability, for the failure to recognize the significance of the ($90^\circ - 270^\circ$) asymmetry found by Cox et al.

It is unlikely that had other experimenters attempted to replicate the Cox experiment that they would have observed the same effect. Cox and collaborators used electrons from beta decay, which are longitudinally polarized. The later experiments all used electrons from thermionic sources, which are unpolarized, and which would have precluded observing the same effect.

Although I believe the philosophical problems associated with theory ladenness of observation and incommensurability have been solved, the practical problems remain. There are very few universal detectors and so all experimental apparatuses, and their associated analysis procedures, restrict the kind of observations and measurements that can be made. Scientists are certainly aware of these practical problems, which gives some hope that they can be minimized, although not completely avoided.