

LETTERS

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for the generally excellent opinion piece that followed. However, physicists must hold to another key belief, without which physics would be impossible: the idea that physical truth is ultimately absolute. Adherence to this belief is important since the prevailing mood today is quite the opposite. Many people seem to want truth to be relative, a pure construct of the feelings and social preferences of the observer. Indeed, in other areas of academic inquiry, people do argue passionately for a relative view of truth that is decided by social convention or status. They often have little tolerance for those who view truth as absolute. Within such a worldview, physics becomes an intolerant intellectual activity.

Granted, our knowledge of physical truth is only an approximation of a more complete understanding. However, once we subscribe to the idea that a complete understanding is and always will be a human invention and that no knowledge stands independent of human viewpoint, then physical inquiry ceases to be a rational activity.

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Theory, Experiment, and Shelter Island

Norman Ramsey has added to the discussion of the relative importance of experimental and theoretical events in major advances in physics (PHYSICS TODAY, July 2000, page 15; January 2001, page 13; September 2001, page 78). Ramsey discusses the two deviations from the predictions of Dirac theory for atomic hydrogen that were measured with high precision in early 1947. The presentation of these results at the Shelter Island meeting in June 1947 to an audience that included theorists such as Hendrick Kramers, Hans Bethe, Julian Schwinger, and Richard Feynman provided the impetus for solving the problem of divergences and completing the structure of quantum electrodynamics (QED). Ramsey mentions especially the measurement of the hyperfine anomaly in hydrogen, an experiment he and I. I. Rabi had designed. He also stresses the importance of the measurement by Willis Lamb and Robert Retherford¹ that definitively proved the reality and

magnitude of the splitting between the $2^2S_{1/2}$ and $2^2P_{1/2}$ hyperfine levels of atomic hydrogen, the famous Lamb shift. I can add a footnote to that history.

In 1953 or early 1954, Lamb was a visiting professor at Harvard University. He devoted his course (which I took) to a detailed review of theoretical and experimental aspects of the Lamb and Retherford work, based on the series of *Physical Review* papers describing it.

I still have a clear memory of his telling that class how William Houston's measurements, in 1937, of the fine-structure splitting in the Balmer lines showed a distinct displacement from the theoretically expected value,² and what a mistake theorists had made in relying on a claim that it should be explained away as an experimental error. Houston was a supremely skilled spectroscopist. It was very unlikely that he could have been misled in such an important measurement. Lamb used this history to point out the perils of the temptation to treat evidence from a particularly reliable source with diminished weight because it deviates inconveniently from a well-established prevailing model. Since this is the thing that most clearly comes back to my memory 48 years later, he must have been very impressive about it. It was a good lesson to heed.

Lamb must also have mentioned the results, in 1938, of Robley C. Williams,³ which confirmed Houston's observation of the shift, and the work of J. W. Drinkwater and colleagues in 1940,⁴ whose results contradicted Houston and Williams and supported the Dirac theory; Drinkwater and colleagues put forward the argument that both Houston and Williams were misled by impurities in the source. All three papers and the experimental controversy are mentioned in the first short paper by Lamb and Retherford.¹

My memory of this incident may be of historical value in that it reveals the special credence Lamb gave to Houston, and therefore to the value of his results. There is no clue to this in the Lamb and Retherford paper, where the three papers are treated totally even-handedly. Lamb, however, had specific reason to evaluate the Houston and Williams papers in 1939. He was then engaged in a controversy about some calculations that aimed to explain the 2S shift by a deviation from Coulomb's law arising from

a meson contribution to the electron-proton interaction.⁵ (Incidentally, Lamb never thought this shift was an electrodynamic effect, even after the discussion at Shelter Island in 1947, until he received Bethe's calculation of it a couple of weeks later.⁶)

I conclude that Houston is a half-forgotten hero of this story, the grandfather of the Lamb shift measurement (and a great uncle, maybe, of the QED revolution after Shelter Island). Lamb must have had confidence from the beginning in just about what the result would be, and that confidence would have helped enormously in supporting the continuing determination needed for such a tour de force experiment.

Lamb was not the only theorist who believed Houston. Victor Weisskopf, the only theorist to start (with Anthony French) the electrodynamic calculation before Lamb's results were known, began it because he believed the Houston and Williams measurements (ref. 6, p. 245).

All the tracks lead back to Houston, then. And those experiments were done to test the Dirac theory—and the Coulomb law. So, which came first, theory or experiment?

Houston was right, of course. From his results, the value of the shift was about 0.03 cm^{-1} . Lamb and Retherford's first result in 1947 was 10% higher. The final value moved up about 6% more. Not bad for 1937.

References

1. W. E. Lamb, R. C. Retherford, *Phys. Rev.* **71**, 914 (1947).
2. W. V. Houston, *Phys. Rev.* **51**, 446, (1937).
3. R. C. Williams, *Phys. Rev.* **54**, 558 (1938).
4. J. W. Drinkwater, O. Richardson, W. E. Williams, *Proc. R. Soc.* **174**, 164 (1940).
5. W. E. Lamb, *Phys. Rev.* **56**, 384 (1939).
6. S. S. Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger and Tomonaga*, Princeton U. Press, Princeton, New Jersey (1994), p. 191.

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Correction

November 2001, page 26—In the figure caption, Hans Bethe and Boyce McDaniel are bicycling around the 10 GeV Cornell electron synchrotron tunnel, not the Cornell Electron Storage Ring. CESR was added to the tunnel in 1979, more than 10 years after the photograph was taken. ■