Quantum Physics: A Text for Graduate Students

N. G. Deshpande

Physics Today **56** (8), 53–54 (2003);
https://doi.org/10.1063/1.2409992

Care and Consensus Smooth the Errors Away

Selectivity and Discord: Two Problems of Experiment

Allan Franklin
U. of Pittsburgh Press, Pittsburgh, Pa., 2002. $37.50 (290 pp.).
ISBN 0-8229-4191-0

Reviewed by Benjamin Bederson

Selectivity and Discord is a careful study of conflict and controversy in experimental physics. Drawing from his extensive writings on controversial experiments and on the epistemology of science measurement, author Allan Franklin sums up many of his ideas concerning how measurements are made and how they relate to theory.

In introductory and concluding chapters, Franklin distinguishes rationalists from constructionists. He aligns himself squarely with rationalists: mostly mainstream physicists, philosophers, and historians of physics who really believe in—forgive me for saying so—reality. In contrast, constructionists believe that scientific truth is shaded by social, political, and economic context. Franklin carefully and fairly explains the constructionist case, but then politely skewers it. These sections are more for philosophers than for bread-and-butter physicists, who generally need no convincing about the choice between objectivity and subjectivity.

Most of the book comprises nine case studies of controversies in experiment that were gradually resolved: K-meson branching ratios, detection of gravity waves, the Milliken oil drop experiment, purported observations of the 17-keV neutrino, low-mass electron–positron states, the fifth force, early observations of monoenergetic beta rays, neutrino oscillations, and atomic parity violations. Franklin and others have published much of the material elsewhere, but Franklin has done readers a service by summarizing it here in sufficient detail to convey the subtleties. Each study has a different object lesson. Franklin chose the studies partly to illustrate ways in which experiments can be in error—or, more subtly, ways in which one can make experiments more reliable. Such ways include properly analyzing the data, considering background effects, carefully analyzing theoretical assumptions, and avoiding preconceptions and other pitfalls.

Benjamin Bederson, *an experimental atomic physicist, is a professor of physics emeritus at New York University and an editor-in-chief emeritus at the American Physical Society. He has been the victim, over the years, of many of the experimental pitfalls described in the Franklin book.*

The easiest case to explain, the gravity-wave coincidence measurement, is one in which the experiment was just plain wrong. It involved a simple recording blunder that offset by hours the observation times at two separate locations.

Beyond such simple errors, experimenters encounter innumerable pitfalls, particularly when they are searching for small deviations from accepted results. Here the first half of the book's title, selectivity, comes into play. When should one question the theoretical tools one uses to interpret data? For example, how reliable must heavy-atom wavefunction computations be to allow interpretation of experiments in nonconservation of parity? When should an experimenter watch out for a tendency to take data preferentially in a region where a peak would reveal a sought-for resonance or particle? (I myself have been guilty of this transgression.) One way to avoid such biases is to conduct a blind experiment in which all the data is shifted by an amount that is unknown to the experimenter until after all the data are taken. That technique, occasionally used by large high-energy collaboration teams, would be difficult for smaller groups that could not afford the inordinate time needed to search where there is nothing of interest.

Franklin also exhaustively explores when to discard data. For example, in the famous Milliken oil drop experiment, surprisingly, even if Milliken's rejected data had been included in his results, the conclusion that all electrons have the same charge, and the value of that charge, would not have changed significantly.

Franklin analyzes many variations of experiments that do not agree with each other or that appear to violate an accepted theoretical canon—two examples of the discord in his title. Virtually always, the physics community eventually reaches consensus on the "correct" results, informed by an emerging awareness of experimental subtleties. Franklin describes, painstakingly and sometimes painfully, how the consensus evolves. In a way, he is describing physics democracy at work. Of course, if there are many wrongs and one right, physics cannot be truly democratic.

Implicit in Franklin's analysis is the need to understand as fully as possible all the intricacies of difficult experiments. He rightly indicates that there are no incorrect experiments, in the sense that Nature is always at work and does not deceive, provided one takes all its subtleties into account. However, one can carry the principle of "no incorrect experiments" too far. My favorite example is an appeal of a rejected paper at *Physical Review*. The author purported to have observed the scattering of one photon beam by another, but the beam intensities seemed far too low. It turned out that the beams were "interacting" with each other with the help of a small background gas. The author argued that, although that might have been true, he was certainly observing a true phenomenon, and therefore the work merited publication.

Skilled experimenters know through experience and instinct the many lessons that are carefully articulated in *Selectivity and Discord*, but can still benefit from reading the case studies. I also found it great fun to read about those famous cases with the enormous benefit of hindsight and with the careful assistance of a skilled guide.

Quantum Physics: A Text for Graduate Students

Roger G. Newton
Springer-Verlag, New York, 2002.
$69.95 (411 pp.).
ISBN 0-387-95473-2

Roger Newton has had a distinguished career in physics, with several significant contributions to scattering theory and high-energy physics. He has also written noteworthy physics books, both technical and popular. His new book, *Quantum Physics: A Text for Graduate Students*, faces stiff competition in an area already crowded with excellent texts.

I find Newton's text comprehensive but rather terse: It would be tough going for most first-year graduate students. Within only 400 pages, it covers topics from such basics as the harmonic oscillator to the Dirac equation. One finds all the major areas of a standard first-year graduate curriculum, including angular momentum, time-independent and time-dependent perturbation theory, and multiparticle systems. The treatments are logical and accurate, but explanations are minimal. Furthermore, some topics are discussed before a student is prepared for them. For example, field quantization appears in the very first chapter, but might better have been deferred until after discussion of the operator form of the harmonic-oscillator solution.

The book has its high points. A section on interaction of radiation with matter is exceptionally good because the groundwork for quantization of the electromagnetic field was laid earlier. That allows for an elegant treatment of spontaneous emission. Several good exercises end each chapter, and concise, well-written appendices end the book. One such appendix, on group theory, is particularly useful as a springboard to additional study.

I would recommend Newton's book more as a reference than as a general text. It is a difficult book for learning quantum mechanics on one's own, but might work as a companion to detailed lectures. As introductory graduate texts, I prefer Ramamurti Shankar's *Principles of Quantum Mechanics* (2nd ed., Plenum, 1994) and J. J. Sakurai's *Modern Quantum Mechanics* (2nd ed., San Fu Tuan, ed., Addison-Wesley, 1994). Albert Messiah's *Quantum Mechanics* (Dover, 1999), although voluminous, is always a good supplement.

N. G. Deshpande
University of Oregon
Eugene

From Nuclear Transmutation to Nuclear Fission, 1932–1939

Per F. Dahl
IOP, Philadelphia, 2002. $75.00 (304 pp.). ISBN 0-7503-0865-6

Nuclear physics started with radioactivity. At first, no way could be found to alter radioactive decay rates: Nuclear transmutations occurred at their own inexorable pace. Then, in 1919, Ernest Rutherford showed that alpha particles from natural radioactive decay that pass through nitrogen could generate hydrogen nuclei. That was the first observation of an externally-induced nuclear transmutation. Ten years of further study led to some extension of the initial results, but also stimulated a strong desire for beams of artificially accelerated particles that could be more intense and more under the experimenters' control.

In 1928, George Gamow, and independently Edward Condon and Ronald Gurney, explained quantum-mechanical barrier penetration. Their explanations helped physicists understand how charged particles get out of a nucleus in radioactive decay—and further, how such particles might tunnel into the nucleus. Thus began what Per F. Dahl calls, in the preface to his book *From Nuclear Transmutation to Nuclear Fission, 1932-1939*, ". . . a race, circa 1930, between four laboratory teams to be the first to achieve the transmutation of atomic nuclei with artificially accelerated nuclear projectiles." Dahl's title is a bit of a misnomer; much of the book concerns what happened before 1932.

Dahl follows closely the work of the research groups led by John Cockcroft at the Cavendish Laboratory in Cambridge, England; Merle Tuve at the Carnegie Institution of Washington, DC; Ernest Lawrence at the University of California, Berkeley; and Charles Lauritsen at Caltech. Cockcroft and Ernest Walton won the race in April 1932, when they saw the alphas generated by 600-keV protons on lithium, but the other labs were not far behind, and each made impressive contributions in the next few years. Dahl's story is based in part on archives at the University of California, Berkeley; in Cambridge at Churchill College, the Cambridge University Library, and the Cavendish Laboratory; at the Center for History of Physics in College Park, Maryland; and at the Library of Congress and the Carnegie Institution, both in Washington, DC. The story is rich in the interplay among physicists in the context of economic depression and the rise of fascism. Anyone with even a moderate interest in how physics developed in the 1920s and 1930s will enjoy the book.

Dahl amply documents an exciting time for physics. Consider just the first half of 1932. January saw the report by Harold Urey, Ferdinand Brickwedde, and George Murphy of the discovery of deuterium. February brought James Chadwick's discovery of the neutron and a crucial advance in cyclotron operation at Berkeley. In April came the Cockcroft–Walton work, and June saw Carl Anderson's discovery of the positron and Werner Heisenberg's description of the nucleus in terms of protons and neutrons as isospin partners (in all but name).

The Cavendish lab won the race for artificial disintegration, but four years later Tuve, Gregory Breit, Lawrence Hafstad, and Norman Heydenburg at the Carnegie Institution made the first direct studies of the nucleon–nucleon interaction. Much information about the interaction had been inferred from data on nuclear masses and sizes, as one can see from the review by Hans Bethe and Robert Bacher (*Rev. Mod. Phys.* **8**, 82, 1936). But if one wants to learn about an interaction, nothing is more fundamental than a scattering cross section. Scattering of all sorts—elastic, inelastic, and transmutational—would become the preeminent tool in nuclear and particle physics.

Dahl concludes his history with an original and exciting account of the early developments in nuclear fission—a special kind of transmutation in which one element becomes two. The discovery of fission in 1938–39 was a major paradigm jolt, which led to an eruption of activity among physicists and chemists that culminated in the Manhattan Project.

The raisins in this plum pudding of a book include accounts of the lifelong friendship and occasional rivalry between Tuve and Lawrence, the difficulties faced by Tuve and his group in trying to achieve high DC voltage by using a Tesla coil, the development by Robert J. Van de Graaff of his moving insulator-belt generator of high voltages, the work of Lawrence and Stanley Livingston on the cyclotron, and the work of Lauritsen in developing high-voltage x-ray generators for medical use and then adapting them to nuclear physics.

An especially interesting theme brought out by Dahl is the importance of Norwegians and other Scandinavians in the development of accelerators and early nuclear physics. Tuve and Lawrence were of Norwegian descent, Hafstad was the son of a Norwegian immigrant, and Norwegian engineer Rolf Wideröe had conceived the basic ideas of several methods of particle acceleration. The Swedish physicist Gustaf Ising thought up the traveling-wave linear accelerator in Stockholm. Lauritsen was Danish. Even more intriguing is the extraordinary career of