# Revisiting Tomonaga—And a Lot of Physics

## The Story of Spin

Sin-itiro Tomonaga
Translated by Takeshi Oka
*U. Chicago P., Chicago, 1997. 272 pp. $50.00 hc*
*ISBN 0-226-80793-2*

*Reviewed by Fritz Rohrlich*

Of the three theoretical physicists who shared the Nobel prize for the development of quantum electrodynamics in the mid-1940s, both Richard Feynman and Julian Schwinger are well known to physicists in this country, while Sin-itiro Tomonaga is hardly known at all. I myself knew Tomonaga only from his scientific papers, which, typically, are very impersonal. It was therefore a great pleasure for me to read *The Story of Spin*. Contrary to the ordinarily dry and formal style of scientific papers, this book of informal lectures reveals the author's warm and attractive personality. He writes in the epilogue: "As I was reading many of the old papers, I often remembered the old days when I first read them, and I could not resist the temptation to relive how I felt." This captures exactly what comes through in these lectures and what makes reading them an engaging and exciting adventure.

Tomonaga talks about the history of the discovery of spin and the development of its theory from about 1920 to 1940. He experienced much of that history (he was born in 1906), and he makes the subject come alive by leading the reader through the thought processes behind the original literature. The latter makes up most of the references. We become witness to the many great successes as well as frustrating failures of those brilliant but groping leaders who led the revolution into quantum mechanics and later into nuclear physics. And he provides photographs of many of the people who contributed to the various periods, such as Arnold Sommerfeld, Alfred Lande and Frederick Hund, Werner Heisenberg, P. A. M. Dirac and Wolfgang Pauli, James Chadwick, Enrico Fermi and Hideki Yukawa, and quite a few others.

The book was published in Japanese in 1974, only five years before Tomonaga's death. It has now been translated into English by Takeshi Oka, to whom we must all be grateful. He has succeeded well in conveying the informal style and humor Tomonaga intended.

The book contains twelve lectures. The first ten recall the history of the theory of spin from the spin-statistics theorem to the development of isospin. The last lecture is a collection of anecdotal material that Tomonaga either experienced or learned from others; it provides an additional human touch to the preceding story. Lecture 11 is a little out of keeping with the rest. It explains somewhat laboriously (because he uses only elementary notions) the mathematics that leads to the Thomas precession for the anomalous magnetic moment.

The book contains many highlights that are not generally known. Examples are Pauli's deep insight, as early as 1924 (which he expressed privately but not in print), that the failure to explain the doublet structure of the alkali spectra is due to "a classically indescribable two-valuedness" of the electron; the sad story of Ralph de Laer Kronig, who suggested in 1926 that the electron has a spin of ½ and a g-factor of 2, only to be discouraged from publication by Pauli, who found it to be *"ein ganz witziger Einfall"* (a quite humorous idea) but showed no interest in it until half a year later, when George Uhlenbeck and Sam Goudsmit published their paper suggesting the electron spin; and the view expressed by both Niels Bohr and Heisenberg (but not by Fermi and Pauli) that quantum mechanics is probably not valid inside the atomic nucleus and that even energy conservation might be violated there.

I cannot help but mention a few of my quibbles: I do not concur that second quantization fulfills Erwin Schrödinger's wish to give the wavefunction a meaning as a matter wave in ordinary three-dimensional space (page 105). And I believe that the book would have profited greatly from a name index.

But such gripes do not detract from the considerable value of this book. There is nothing in the literature quite like it. The lectures can be understood by an advanced undergraduate who has had introductory courses in quantum mechanics and atomic and nuclear physics. This piece of the history of physics will provide excellent and exciting reading material complementary to such courses. It also provides the personal touch of an expert in the field that is so often lacking in the physics literature. I recommend it very highly.

FRITZ ROHRLICH, *of Syracuse University, wrote (with Josef M. Jauch) the first book on the Dyson–Feynman–Schwinger–Tomonaga quantum electrodynamics in 1955 (second edition 1976) and has been working in this and related fields ever since.*

## The Quark Machines: How Europe Fought the Particle Physics War

Gordon Fraser
*IOP, Philadelphia, 1997. 210 pp. $20.00 pb* *ISBN 0-7503-0447-2*

Gordon Fraser's *The Quark Machines* is an accurate and rather complete historical record of the evolution of elementary-particle physics from the theoretical speculations of Hideki Yukawa in 1935 and the discovery of cosmic-ray mu mesons in 1937 to the present. The book was written by the editor of the CERN *Courier* and, despite its accuracy, I have trouble with the author's judgment and with the book's tone. The subtitle says the book describes the "war," presumably between Europe and the US, over particle physics. News to me! Yes, I knew about a friendly competition, sometimes even intense competition, that was beneficial to both sides (in correcting false measurements, enhancing theoretical effort and even stimulating governments to spend on high-energy physics). But this is the first I have heard of a "war."

The text, by way of historical background, describes the efforts to develop radar and the atomic bomb. In these areas, about which a great deal has been written and some controversy generated over the historical record, the book is remarkably accurate.

Similarly, when the author turns to particle physics, which is, of course, primarily a postwar development, he offers a reliable chronicle of who did what experiments, in what order they were done, how and by whom the theoretical framework was developed and what the relationship was between experimental findings and theoretical developments. For those reasons, you should read this book.

But don't read this book to obtain a balanced view of European and American contributions. Fraser describes a "war," and does so from the point of view of one side. We remember what happens in wars, where the government and the media outdo each other in demonizing the enemy. Well, this book doesn't quite demonize US physics, but it starts down that road. Do we need to be told that "[t]he skills and knowledge of the new immigrants, coupled with the economic might of the USA, together created . . . the Manhattan Project?" Is that fair? Sure, Enrico Fermi, Hans Bethe, Victor Weisskopf and many more from Europe had "skills and knowledge" that allowed them to contribute mightily. But what about Robert Oppenheimer, Robert Serber, Ernest Lawrence, Glenn Seaborg, Robert Wilson, Richard Feynman, Donald Kerst, Luis Alvarez and many other US-born physicists? Didn't they also have some "skills and knowledge?"

The Berkeley school, prior to World War II, is characterized by Fraser as having "missed several major physics discoveries, but they did open one valuable new applications area." (And do you know what that was? Not a topic in pure physics, according to Fraser, but in radioactive tracers.) True, they missed artificial radioactivity and a number of other things, but is Fraser's sentence a fair characterization of that great place and those great times in physics?

In the caption for the photograph of Niels Bohr is the statement that "he was smuggled . . . to Los Alamos, where his arrival was a major boost for the Manhattan Project." Well, a psychological boost, no doubt, but I am not aware of any specific contributions that he made to the atomic bomb. And if you were composing a short caption for Bohr, wouldn't it emphasize some other things?

Then there is the book's general tone, which tends to ignore or downplay American physics, while treating European physics as serious and significant. The balance is tilted by even such things as making fun of flippant US-born names like "quark" and "color," or describing the serious machines, like the Proton Synchrotron, Intersecting Storage Ring and Large Hadron Collider, on which Europeans do physics, in contrast with the flamboyantly named American machines like the Cosmotron, Bevatron and Supercollider.

The contributions of US physics to European physics are described, but hardly in proper perspective. Thus the PS was built upon the American concept of strong focusing, which fact is acknowledged by Fraser, but after reading that section, one comes away thinking that the really significant point was the adoption of the idea, not the idea itself. For the colliding-beam ISR, essentially all the concepts were American; that fact is hardly acknowledged by Fraser. The slew of particle resonances found by the Berkeley bubble chamber groups is dutifully recorded in three sentences, while the CERN discovery of neutral currents is described, in glowing terms, over three pages.

I could go on, but I think I have said enough—perhaps more than enough—about my reaction to the tone and balance of the book. I recommend reading the book to live, or relive, the exciting times of the development of the Standard Model, and especially to learn of the European work on particle physics, but don't read it to get a balanced perspective or to learn which developments were significant and which were of secondary importance. We don't have those in this book; perhaps another 50 years is needed to gain the proper historical perspective.

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## Before the Beginning: Our Universe and Others

Martin Rees
*Addison Wesley Longman, Reading, Mass., 1997. 304 pp. $25.00 hc ISBN 0-201-15142-1*

I was somewhat surprised to find a book with this title written by Martin Rees, one of the world's leading astrophysicists and a brilliant researcher and expositor. One of Rees's hallmarks is that his work is generally firmly and astutely grounded in observation. The topic of multiverses—related to the possibility that our universe is merely one of perhaps an infinite number of universes that are causally disconnected and in each of which the boundary conditions could be quite different—is, however, quite speculative. Such an idea arises more naturally in the context of inflationary theories, which are themselves at present only indirectly grounded in observation and experiment and which are also somewhat removed from much of Rees's own research.

It is clear that Rees has made an attempt to reach a broader audience with *Before the Beginning*, and I suspect that his publishers have also focused on the multiverse idea as a factor in marketing. The idea is a good one: Anyone who has ever given a popular lecture on cosmology knows that two questions that will inevitably be asked are: What happened before the big bang? and Is anything out there "beyond" our universe? (whatever this may mean).

Any effort to address these issues leads to a discussion of some of the fascinating, if vague, ideas associated with the physics of the very early universe. Some of this physics is based on ideas that, like the concept of Grand Unification, involve a more or less direct extrapolation of current knowledge. Some of this proposed physics is based on far more speculative ideas associated with a theory that at present does not fully exist: quantum gravity. When discussing such topics, one can easily blur the distinction between what is understood and what is simply speculation. Such is generally not the case with Rees's book, however.

From the beginning, it is clear that this is work of substance, derived in large part from the central discoveries that underscore modern cosmology. The first half of the book presents an overview of the field, devoid of much of the traditional hype and in a readable style having great logical clarity. And, true to form, each idea is introduced in the context of its empirical motivation. Interspersed throughout the book are short, informative descriptions of some of the central figures in cosmology, from Einstein onward—a number of whom Rees has known and worked with and thus about whom he can provide an informed personal perspective. While some of these descriptions may distract a reader from the general line of argument, several are quite illuminating.

Rees was appointed to the Plumian Chair in Astronomy and Experimental Philosophy at the University of Cambridge at the age of 30, as successor to Fred Hoyle, and he held this chair until his recent appointment as Royal Society Professor and Britain's Astronomer Royal. It is thus appropriate that, early in the book, Rees presents a thorough review of his predecessor's contributions. Hoyle's name has been most closely associated, at least for the current generation of physicists (including me), with the steady-state cosmological model, which long ago fell out of favor. I had not fully appreciated Hoyle's contributions, and Rees does