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Tomonaga Sin-Itiro : A Memorial – Two Shakers of Physics

Julian Schwinger

Abstract This address was presented by Julian Schwinger as the Nishina Memorial Lecture at the Maison Franco-Japanese (Tokyo), on July 8, 1980.

Minasama:

I am deeply honored to have the privilege of addressing you today. It is natural that I should do so, as the Nobel prize partner whose work on quantum electrodynamics was most akin in spirit to that of Tomonaga Sin-Itiro. But not until I began preparing this memorial did I become completely aware of how much our scientific lives had in common. I shall mention those aspects in due time. More immediately provocative is the curious similarity hidden in our names. The Japanese character —the kanji— shin (振) has, among other meanings those of 'to wave', 'to shake'. The beginning of my Germanic name, Schwing, means 'to swing', 'to shake'. Hence my title, "Two Shakers of Physics".

Julian Schwinger
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One cannot speak of Tomonaga without reference to Yukawa Hideki and, of course, Nishina Yoshio. It is a remarkable coincidence that both Japanese Nobel prize winners in physics were born in Tokyo, both had their families move to Kyoto, and also both were sons of professors at Kyoto Imperial University, both attended the Third High School in Kyoto, and both attended and graduated from Kyoto Imperial University with degrees in physics. In their third and final year at the university, both learned the new quantum mechanics together (Tomonaga would later remark,

Julian Schwinger (1918 – 1994). Nobel Laureate in Physics (1965)
University of California, Los Angeles (USA) at the time of this address

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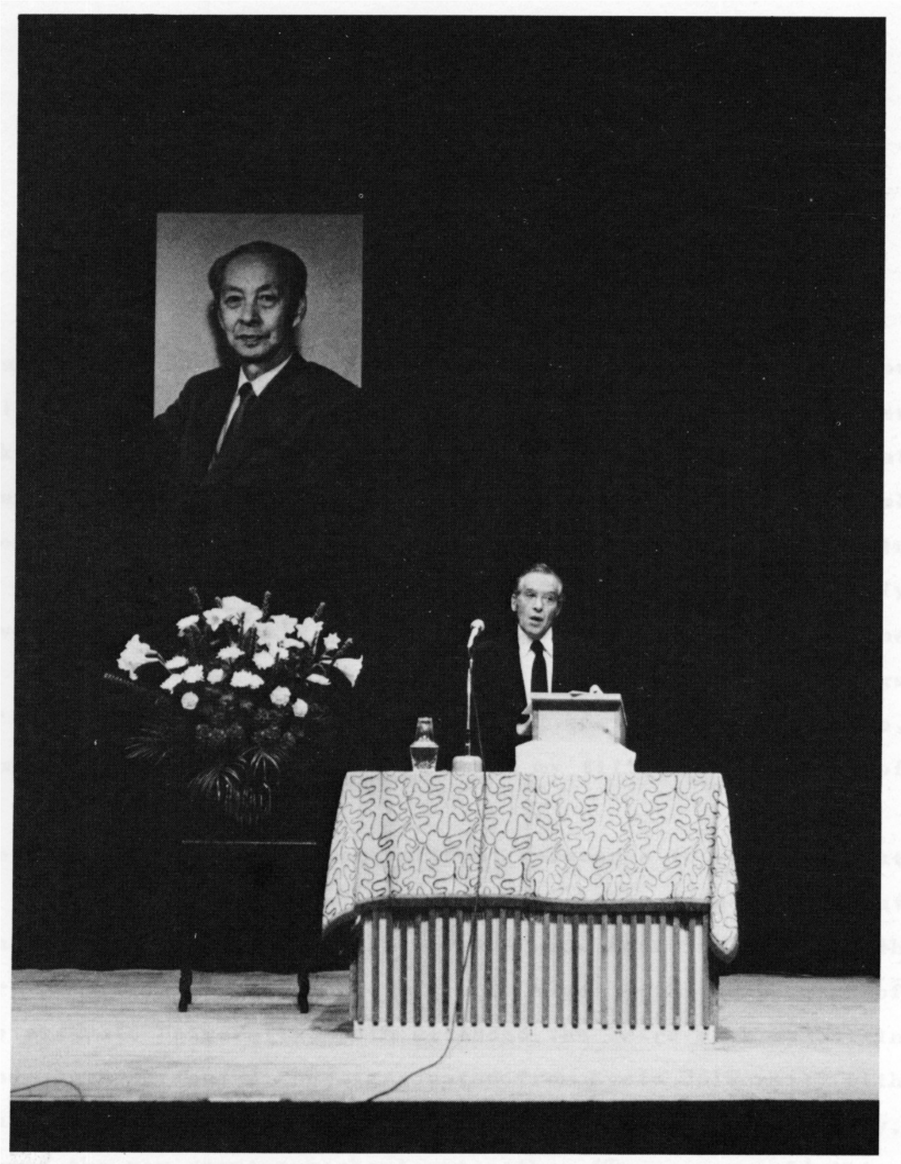

Fig. 3.1 Memorial lecture of Professor Julian Schwinger for Professor Sin-itiro Tomonaga (July 8, 1980, Tokyo)

about this independent study, that he was happy not to be bothered by the professors). Both graduated in 1929 into a world that seemed to have no place for them, (Yukawa later said "The depression made scholars"). Accordingly, both stayed on as unpaid assistants to Professor Tamaki Kajyuro; Yukawa would eventually succeed him. In 1931, to Nishina comes on stage. He gave a series of lectures at Kyoto Imperial University on quantum mechanics. Sakata Shoichi, then a student, later reported that Yukawa and Tomonaga asked the most questions afterward.

Nishina was a graduate in electrical engineering of the Tokyo Imperial University. In 1917 he joined the recently founded Institute of Physical and Chemical Research, the Rikagaku Kenkyusho —– RIKEN. A private institution, RIKEN, was supported financially in various ways, including the holding of patents on the manufacture of sake. After several years at RIKEN, Nishina was sent abroad for further study, a pilgrimage that would last for eight years. He stopped at the Cavendish Laboratory in Cambridge, England, at the University of Göttingen in Germany, and then, finally, went to Denmark and Niels Bohr in Copenhagen. He would stay there for six years. And out of that period came the famous Klein-Nishina formula. Nishina returned to Japan in December, 1928, to begin building the Nishina Group. It would, among other contributions, establish Japan in the forefront of research on nuclear and cosmic ray physics —– soryushiron.

There was a branch of RIKEN at Kyoto in 1931 when Nishina, the embodiment of the 'Kopenhagener Geist', came to lecture and to be impressed by Tomonaga. The acceptance of Nishina's offer of research position brought Tomonaga to Tokyo in 1932. (Three years earlier he had traveled to Tokyo to hear lectures at RIKEN given by Heisenberg and Dirac.) The year 1932 was a traumatic one for physics. The neutron was discovered; the positron was discovered. The first collaborative efforts of Nishina and Tomonaga dealt with the neutron, the problem of nuclear forces. Although there were no formal publications, this work was reported at the 1932 autumn and 1933 spring meetings that were regularly held by the RIKEN staff. Then, in the 1933 autumn meeting the subject becomes the positron. It was the beginning of a joint research program that would see the publication of a number of papers concerned with various aspects of electron-positron pair creation and annihilation. Tomonaga's contributions to quantum electrodynamics has begun.

While these papers were visible evidence of interest in quantum electrodynamics, we are indebted to Tomonaga for telling us, in his Nobel address, of an unseen but more important step — he read the 1932 paper of Dirac that attempted to find a new basis for electrodynamics. Dirac argued that "the role of the field is to provide a means for making observations of a system of particles", and therefore, "we cannot suppose the field to be a dynamical system on the same footing as the particles and thus something to be observed in the same way as the particles". The attempt to demote the dynamical status of the electromagnetic field, or, in the more extreme later proposal of Wheeler and Feynman, to eliminate it entirely, is a false trail, contrary to the fundamental quantum duality between particle and wave, or field. Nevertheless, Dirac's paper was to be very influential. Tomonaga says,

> "This paper of Dirac's attracted my interest because of the novelty of its philosophy and the beauty of its form. Nishina also showed a great interest in this paper and suggested that I

investigate the possibility of predicting some new phenomena by this theory. Then I started computations to see whether the Klein-Nishina formula could be derived from this theory or whether any modification of the formula might result. I found out immediately. however, without performing the calculation through to the end, that it would yield the same answer as the previous theory. The new theory of Dirac's was in fact mathematically equivalent to the older Heisenberg–Pauli theory and I realized during the calculation that one could pass from one to the other by a unitary transformation. The equivalence of these two theories was also discovered by Rosenfeld and Dirac–Fock–Podolsky and was soon published in their papers."

I graduated from a high school that was named for Townsend Harris, the first American consul in Japan. Soon after, in 1934, I wrote but did not publish my first research paper. It was on quantum electrodynamics. Several years before, the Danish physicist Møller had proposed a relativistic interaction between two electrons, produced through the retarded intervention of the electromagnetic field. It had been known since 1927 that electrons could also be described by a field, one that had no classical, counterpart. And the dynamical description of this field was understood, when the electrons interacted instantaneously. I asked how things would be when the retarded interaction of Møller was introduced. To answer the question I used the Dirac–Fock–Podolsky formulation. But now, since I was dealing entirely with fields, it was natural to introduce for the electron field, as well, the analogue of the unitary transformation that Tomonaga had already recognized as being applied to the electromagnetic field in Dirac's original version. Here was the first tentative use of what Tomonaga, in 1943, would correctly characterize as "a formal transformation which is almost self-evident" and I, years later, would call the interaction representation. No, neither of us, in the 1930's, had reached what would eventually be named the Tomonaga–Schwinger equation. But each of us held a piece which, in combination, would lead to that equation: Tomonaga appreciated the relativistic form of the theory, but was thinking in particle language; I used a field theory, but had not understood the need for a fully relativistic form. Had we met then, would history have been different?

The reports of the spring and autumn 1936 meetings of the RIKEN staff show something new — Tomonaga had resumed his interest in nuclear physics. In 1937 he went to Germany — to Heisenberg's Institute at Leipzig. He would stay for two years, working on nuclear physics and on the theory of mesons, to use the modern term. Tomonaga had come with a project in mind: treat Bohr's liquid drop model of the nucleus, and the way an impinging neutron heats it up, by using the macroscopic concepts of heat conduction and viscosity. This work was published in 1938. It was also the major part of the thesis submitted to Tokyo University in 1939 for the degree of Doctor of Science — Rigakuhakushi. Heisenberg's interest in cosmic rays then turned Tomonaga's attention to Yukawa's meson.

The not yet understood fact, that the meson of nuclear forces and the cosmic ray meson observed at sea level are not the same particle, was beginning to thoroughly confuse matters at this time. Tomonaga wondered whether the problem of the meson lifetime could be overcome by including an indirect process, in which the meson turns into a pair of nucleons — proton and neutron — that annihilate to produce the final electron and neutrino. The integral over all nucleon pairs, resulting from the perturbation calculation, was — infinite. Tomonaga kept a diary of his impressions during this German period. It poignantly records his emotional reactions to the difficulties he encountered. Here are some excerpts:

> "It has been cold and drizzling since morning and I have devoted the whole day to physics in vain. As it got dark I went to the park. The sky was gray with a bit of the yellow of twilight in it. I could see the silhouetted white birch grove glowing vaguely in the dark. My view was partly obscured by my tired eyes: my nose prickled from the cold and upon returning home I had a nosebleed. After supper I took up my physics again, but at last I gave up. III-starred work indeed!"

Then,

> "Recently I have felt very sad without any reason, so I went to a film. Returning home I read a book on physics. I don't understand it very well. Meanwhile I comprehensible?"

Again,

> "As I went on with the calculation, I found the integral diverged — was infinite. After lunch I went for a walk. The air was astringently cold and the pond in Johanna Park was half frozen, with ducks swimming where there was no ice. I could see a flock of other birds. The flower beds were covered with chestnut leaves against the frost. Walking in the park, I was no longer interested in the existence of neutron, neutrino."

And finally,

> "I complained in emotional words to Professor Nishina about the slump in my work, whereupon I got his letter in reply this morning. After reading it my eyes were filled with tears. — He says: only fortune decides your progress in achievements. All of us stand on the dividing line from which the future is invisible. We need not be too anxious about the results, even though they may turn out quite different from what you expect. By-and-by you may meet a new chance for success."

Toward the close of Tomonaga's stay in Leipzig, Heisenberg suggested a possible physical answer to the clear inapplicability of perturbation methods in meson physics. It involved the self-reaction of the strong meson field surrounding a nucleon. Heisenberg did a classical calculation, showing that the scattering of mesons by nucleons might thereby be strongly reduced, which would be more in conformity with the experimental results. About this idea Tomonaga later remarked, "Heisenberg, in this paper published in 1939, emphasized that the field reaction would be crucial in meson-nucleon scattering. Just at that time I was studying at Leipzig, and I still remember vividly how Heisenberg enthusiastically explained this idea to me and handed me galley proofs of his forthcoming paper. Influenced by Heisenberg, I came to believe that the problem of field reactions far from being meaningless was one which required a "frontal attack". Indeed, Tomonaga wanted to stay on for another year, to work on the quantum mechanical version of Heisenberg's classical calculation.

The growing clouds of war made this inadvisable, however, and Tomonaga returned to Japan by ship. As it happened, Yukawa who had come to Europe to attend a Solvay Congress, which unfortunately was cancelled, sailed on that very ship. When the ship docked at New York, Yukawa disembarked and, beginning at Columbia

University, where I first met him, made his way across the United States, visiting various universities. But Tomonaga, after day's sightseeing in New York that included the Japanese Pavilion at the World's Fair, continued with the ship through the Panama Canal and on to Japan. About this Tomonaga said, "When I was in Germany I had wanted to stay another year in Europe, but once I was aboard a Japanese ship I became eager to arrive in Japan". He also remarked about his one day excursion in New York that "I found that I was speaking German rather than English, even though I had not spoken fluent German when I was in Germany".

Tomonaga had returned to Japan with some ideas concerning the quantum treatment of Heisenberg's proposal that attention to strong field reactions was decisive for understanding the meson-nucleon system. But soon after he began work he became aware, through an abstract of a paper published in 1939, that Wentzel was also attacking this problem of strong coupling. Here is where the scientific orbits of Tomonaga and myself again cross. At about the time that Tomonaga returned to Japan I went to California, to work with Oppenheimer. Our first collaboration was a quantum electrodynamic calculation of the electron-positron pair emitted by an excited oxygen nucleus. And then we turned to meson physics. Heisenberg had suggested that meson-nucleon scattering would be strongly suppressed by field reaction effects. There also existed another proposal to the same end — that the nucleon possessed excited states, isobars, which would produce almost cancelling contributions to the meson scattering process. We showed, classically, that the two explanations of suppressed scattering were one and the same: the effect of the strong field reaction, of the strong coupling, was to produce isobars, bound states of the meson about the nucleon. The problem of giving these ideas a correct quantum framework naturally arose. And then, we became aware, through the published paper, of Wentzel's quantum considerations on a simple model of the strong coupling of meson and nucleon. I took on the quantum challenge myself. Not liking the way Wentzel had handled it, I redid his calculation in my own style, and, in the process, found that Wentzel had made a mistake. In the short note that Oppenheimer and I eventually published, this work of mine is referred to as "to be published soon". And it was published, 29 years later, in a collection of essays dedicated to Wentzel. Recently, while surveying Tomonaga's papers, I came upon his delayed publication of what he had done along the same lines. I then scribbled a note: "It is as though I were looking at my own long unpublished paper". I believe that both Tomonaga and I gained from this episode added experience in using canonical-unitary-transformations to extract the physical consequences of a theory.

I must not leave the year 1939 without mentioning a work that would loom large in Tomonaga's later activities. But, to set the stage, I turn back to 1937. In this year, Block and Nordsieck considered another kind of strong coupling, that between an electric charge and arbitrarily soft — extremely low frequency — light quanta. They recognized that, in a collision, say between an electron and a nucleus, arbitrarily soft quanta will surely be emitted; a perfectly elastic collision cannot occur. Yet, if only soft photons, those of low energy, are considered, the whole scattering process goes on as though the electrodynamic interactions were ineffective. Once this was understood, it was clear that the real problem of electrodynamic field reaction begins when arbitrarily hard — unlimited high energy — photons are reintroduced. In 1939 Dancoff performed such a relativistic scattering calculation both for electrons, which have spin 1/2, and for charged particles without spin. The spin 0 calculation gave a finite correction to the scattering, but, for spin 1/2, the correction was infinite. This was confusing. And to explain why that was so, we must talk about electromagnetic mass.

Already in classical physics the electric field surrounding an electrically charged body carries energy and contributes mass to the system. That mass varies inversely as a characteristic dimension of the body, and therefore is infinite for a point charge. The magnetic field that accompanies a moving charge implies an additional momentum, and additional electromagnetic, mass. It is very hard, at this level to make those two masses coincide, as they must, in a relativistically invariant theory. The introduction of relativistic quantum mechanics, of quantum field theory, changes the situation completely. For the spin 1/2 electron-positron system, obeying Fermi–Dirac statistics, the electromagnetic mass, while still infinite, is only weakly logarithmically, so. In contrast, the electromagnetic mass for a spin 0 particle, which obeys Bose–Einstein statistics, is more singular than the classical one. Thus, Dancoff's results were in contradiction to the expectation that spin 0 should exhibit more severe electromagnetic corrections.

Tomonaga's name had been absent from the RIKEN reports for the years from 1937 to 1939, when he was in Germany. It reappears for the 1940 spring meeting under the title "On the Absorption and Decay of Slow Mesons". Here the simple and important point is made that, when cosmic ray mesons are stopped in matter, the repulsion of the nuclear Coulomb field prevents positive mesons from being absorbed by the nucleus, while negative mesons would preferentially be absorbed before decaying. This was published as a Physical Review Letter in 1940. Subsequent experiments showed that no such asymmetry existed: the cosmic ray meson does not interact significantly with nuclear particles. The RIKEN reports from autumn of 1940 to autumn of 1942 trace stages in the development of Tomonaga's strong and intermediate coupling meson theories. In particular, under the heading "Field Reaction and Multiple Production" there is discussed a coupled set of equations corresponding to various particle numbers, which is the basis of an approximation scheme, now generally called the Tamm–Dancoff approximation. This series of reports on meson theory was presented to the Meson Symposium — Chukanshi Toronkai — that was initiated in September 1943, where also was heard the suggestion of Sakata's group that the cosmic ray meson is not the meson responsible for nuclear forces.

But meanwhile there occurred the last of the RIKEN meetings held during the war, that of spring 1943. Tomonaga provides the following abstract with the title "Relativistically Invariant Formulation of Quantum Field Theory":

> "In the present formulation of quantum fields as a generalization of ordinary quantum mechanics such nonrelativistic concepts as probability amplitude, canonical commutation relation and Schrödinger equation are used. Namely these concepts are defined referring to a particular Lorentz frame in space-time. This unsatisfactory feature has been pointed out by many people and also Yukawa emphasized it recently. I make a relativistic generalization of these concepts in quantum mechanics such that they do not refer to any particular

coordinate frame and reformulate the quantum theory of fields in a relativistically invariant manner."

In the previous year Yukawa had commented on the unsatisfactory nature of quantum field theory, pointing both to its lack of an explicit, manifestly covariant form and to the problem of divergences — infinities. He wished to solve both problems at the same time. To that end, he applied Dirac's decade earlier suggestion of a generalized transformation function by proposing that the quantum field probability amplitude should refer to a closed surface in space-time. From the graphic presentation of such a surface as a circle, the proposal became known as the theory of maru. Tomonaga's reaction was to take one problem at a time, and he first proceeded to "reformulate the quantum theory of fields in a relativistically invariant manner". And in doing so he rejected Yukawa's more radical proposal in favor of retaining the customary concept of causality — the relation between cause and effect. What was Tomonaga's reformulation?

The abstract I have cited was that of a paper published in the Bulletin of the Institute, RIKEN–Iho. But its contents did not become known outside of Japan until it was translated into English to appear in the second issue, that of August–September, 1946, of the new journal, Progress of Theoretical Physics. It would, however, be some time before this issue became generally available in the United States. Incidentally, in this 1946 paper Tomonaga gave his address as Physics Department, Tokyo Bunrika University. While retaining his connection with RIKEN, he had, in 1941, joined the faculty of this university which later, in 1949, became part of the Tokyo University of Education. Tomonaga begins his paper by pointing out that the standard commutation relations of quantum field theory, referring to two points of space at the same time, are not covariantly formulated: in a relatively moving frame of reference the two points will be assigned different times. This is equally true of the Schrödinger equation for time evolution, which uses a common time variable for different spatial points. He then remarks that there is no difficulty in exhibiting commutation relations for arbitrary space-time points when a non-interacting field is considered. The unitary transformation to which we have already referred, now applied to all the fields, provides them with the equations of motion of non-interacting fields, while, in the transformed Schrödinger equation, only the interaction terms remain. About this Tomonaga says, "... in our formulation, the theory is divided into two sections. One section gives the laws of behavior of the fields when they are left alone, and the other gives the laws determining the deviation from this behavior due to the interactions. This way of separating the theory can be carried out relativistically". Certainly commutation relations referring to arbitrary space-time points are four-dimensional in character. But what about the transformed Schrödinger equation, which still retains its single time variable? It demands generalization.

Tomonaga was confident that he had the answer for, as he put it later, "I was recalling Dirac's many-time theory which had enchanted me ten years before". In the theory of Dirac, and then of Dirac–Fock–Podolsky, each particle is assigned its own time variable. But, in a field theory, the role of the particles is played by the small volume elements of space. Therefore, assign to each spatial volume element an independent time coordinate. Thus, the "super many-time theory". Let me be more precise about that idea. At a common value of the time, distinct spatial volume elements constitute independent physical system, for no physical influence is instantaneous. But more than that, no physical influence can travel faster than the speed of light. Therefore any two space-time regions that cannot be connected, even by light signals, are physically independent; they are said to be in space-like relationship. A three-dimensional domain such that any pair of points is in space-like relationship constitutes a space-like surface in the four-dimensional world. All of space at a common time is but a particular coordinate description of a plane space-like surface. Therefore the Schrödinger equation, in which time advances by a common amount everywhere in space, should be regarded as describing the normal displacement of plane space-like surface. Its immediate generalization is to the change from one arbitrary space-like surface to an infinitesimally neighboring one, which change can be localized in the neighborhood of a given space-time point. Such is the nature of the generalized Schrödinger equation that Tomonaga constructed in 1943, and to which I came toward the end of 1947.

By this time the dislocation produced by the war became dominant. Much later Tomonaga recalled that "I myself temporarily stopped working on particle physics after 1943 and was involved in electronics research. Nevertheless the research on magnetrons and on ultra-short wave circuits was basically a continuation of quantum mechanics". Miyazima Tatsuoki remembers that "One day our boss Dr. Nishina took me to see several engineers at the Naval Technical Research Institute. They had been engaged in the research and development of powerful split anode magnetrons, and they seemed to have come to a concrete conclusion about the phenomena taking place in the electron cloud. Since they were engineers their way of thinking was characteristic of engineers and it was quite natural that they spoke in an engineer's way, but unfortunately it was completely foreign to me at the beginning. Every time I met them, I used to report to Tomonaga how I could not understand them, but he must have understood something, because after a month or so, he showed me his idea of applying the idea of secular perturbation, well-known in celestial mechanics and quantum theory, to the motion of the electrons in the cloud. I remember that the moment he toll me I said 'This is it'. Further investigation actually showed that the generation of electromagnetic oscillations in split anode magnetrons cannot be essentially understood by applying his idea".

When Tomonaga approached the problem of ultra-shortwave circuits, which is to say, the behavior of microwaves in waveguides and cavity resonators, he found the engineers still using the old language of impedance. He thought this artificial because there no longer are unique definitions of current and voltage. Instead, being a physicist, Tomonaga begins with the electromagnetic field equations of Maxwell. But he quickly recognizes that those equations contain much more information than is needed to describe a microwave circuit. One usually wants to know only a few things about a typical waveguide junction: if a wave of given amplitude moves into a particular arm, what are the amplitudes of the waves coming out of the various arms, including the initial one? The array of all such relations forms a matrix, even then familiar to physicists as the scattering matrix. I mention here the amusing episode of the German submarine that arrived bearing a dispatch stamped Streng Geheim —

Top Secret. When delivered to Tomonaga it turned out to be — Heisenberg's paper on the scattering matrix. Copies of this Top Secret document were soon circulating among the physicists. Tomonaga preferred to speak of the scattering matrix as the characteristic matrix, in this waveguide context. He derives properties of that matrix, such its unitary character, and shows how various experimental arrangements can be described in term of the characteristic matrix of the junction. In the paper published after the war he remarks, concerning the utility of this approach, that "The final decision, however, whether or not new concept is more preferable to impedance should of course be given not only by a theoretical physicist but also by general electro-engineers". But perhaps my experience is not irrelevant here.

During the war I also worked on the electromagnetic problems of microwaves and waveguides. I also began with the physicist's approach, including the use of the scattering matrix. But long before this three year episode was ended, I was speaking the language of the engineers. I should like to think that those years of distraction for Tomonaga and myself were not without their useful lessons. The waveguide investigations showed the utility of organizing a theory to isolate those inner structural aspects that are not probed under the given experimental circumstances. That lesson was soon appllied in the effective range description of nuclear forces. And it is this viewpoint that would lead to the quantum electrodynamics concept of self-consistent subtraction or renormalization.

Tomonaga already understood the importance of describing relativistic situations covariantly — without specialization to any particular coordinate system. At about this time, I began to learn that lesson pragmatically, in the context of solving a physical problem. As the war in Europe approached its end, the American physicists responsible for creating a massive microwave technology began to dream of high energy electron accelerators. One of the practical questions involved is posed by the strong radiation emitted by relativistic electrons swinging in circular orbits. In studying what is now called synchrotron radiation, I used the action of the field created by the electron's motion. One part of that reaction describes the energy and momentum lost by the electron to the radiation. The other part is an added inertial effect characterized by an electromagnetic mass. I have mentioned the relativistic difficulty that electromagnetic mass usually creates. But, in the covariant method I was using, based on action and proper time, a perfectly invariant form emerged. Moral: to end with an invariant result use a covariant method and maintain covariance to the end of the calculation. And, in the appearance of an invariant electromagnetic mass that simply added to the mechanical mass to form the physical mass of the electron, neither piece being separately distinguishable under ordinary physical circumstances, I was seeing again the advantage of isolating unobservable structural aspects of the theory. Looking back at it, the basic ingredients of the coming quantum electrodynamic revolution were now in place. Lacking was an experimental impetus to combine them, and take them seriously.

Suddenly, the Pacific War was over. Amid total desolation Tomonaga reestablished his seminar. But meanwhile, something had been brewing in Sakata's Nagoya group. It goes back to a theory of M}oller and Rosenfeld, who tried to overcome the nuclear force difficulties of meson theory by proposing a mixed field theory, with both pseudoscalar and vector mesons of equal mass. I like to think that my modification of this theory, in which the vector meson is more massive, was the prediction of the later discovered — meson. Somewhat analogously, Sakata proposed that the massless vector photon is accompanied by a massive scalar meson called the cohesive or C-meson. About this, Tomonaga said, "in 1946, Sakata proposed a promising method of eliminating the divergence of the electron mass by introducing the idea of a field of cohesive force. It was the idea that there exists an unknown field, of the type of the meson field, which interacts with the electron in addition to the electromagnetic field. Sakata named this field the cohesive force field, because the apparent electromagnetic mass due to the interaction of this field and the electron, though infinite, is negative and therefore the existence of this field could stabilize the electron in some sense. Sakata pointed out the possibility that the electromagnetic mass and the negative new mass cancel each other and that the infinity could be eliminated by suitably choosing the coupling constant between this field and the electron. Thus the difficulty which had troubled people for a long time seemed to disappear insofar as the mass was concerned". Let me break in here and remark that this solution of the mass divergence problem is, in fact, illusory. In 1950, Kinoshita showed that the necessary relation between the two coupling constants would no longer cancel the divergences, when the discussion is extended beyond the lowest order of approximation. Nevertheless, the C-meson hypothesis served usefully as one of the catalysis that led to the introduction of the self-consistent subtraction method. How that came about is described in Tomonaga's next sentence: "Then what concerned me most was whether the infinities appearing in the electron scattering process could also be removed by the idea of a plus-minus cancellation."

I have already referred to the 1939 calculation of Dancoff, on radiative corrections to electron scattering, which gave an infinite result. Tomonaga and his collaborators now proceeded to calculate the additional effect of the cohesive force field. It encouragingly gave divergent results of the opposite sign, but they did not precisely cancel Candoff's infinite terms. This conclusion was reported in a letter of November 1, 1947, submitted to the Progress of Theoretical Physics, and also presented at a symposium on elementary particles held in Kyoto that same month. But meanwhile parallel calculations of the electromagnetic effect were going on, repeating Dancoff's calculations, which were not reported in detail. At first they reproduced Dancoff's result. But then Tomonaga suggested a new and much more efficient method of calculation. It was to use the covariant formulation of quantum electrodynamics, and subject it to a unitary transformation that immediately isolated the electromagnetic mass term. Tomonaga says,

> "Owing to this new, more lucid method, we noticed that among the various terms appearing in both Dancoff's and our previous calculation, one term had been overlooked. There was only one missing term, but it was crucial to the final conclusion. Indeed, if we corrected this error, the infinities appearing in the scattering process of an electron due to the electromagnetic and cohesive force fields cancelled completely, except for the divergence of vacuum polarization type."

A letter of December 30, 1947, corrected the previous erroneous announcement.

But what is meant by "the divergence of vacuum polarization type"? From the beginning of Dirac's theory of positrons it had been recognized that, in a sense, the vacuum behaved as a polarizable medium; the presence of an electromagnetic field induced a charge distribution acting to oppose the inducing field. As a consequence the charges of particles would appear to be reduced, although the actual calculation gave a divergent result. Nevertheless, the effect could be absorbed into a redefinition, a renormalization, of the charge. At this stage, then, Tomonaga had achieved a finite correction to the scattering of electrons, by combining two distinct ideas: the renormalization of charge, and the compensation mechanism of the C-meson field. But meanwhile another line of thought had been developing. In this connection let me quote from a paper, published at about this time, by Taketani Mitsuo:

> "The present state of theoretical physics is confronted with difficulties of extremely ambiguous nature. These difficulties can be glossed over but no one believes that a definite solution has been attained. The reason for this is that, on one hand, present theoretical physics itself has logical difficulties, while, on the other hand, there is no decisive experiment whereby to determine this theory uniquely."

In June of 1947 those decisive experiments were made known, in the United States.

For three days at the beginning of June, some twenty physicists gathered at Shelter Island, located in a bay near the tip of Long Island, New York. There we heard the details of the experiment by which Lamb and Retherford had used the new microwave techniques to confirm the previously suspected upward displacement of the 2S level of hydrogen. Actually, rumors of this had already spread, and on the train to New York, Victor Weisskopf and I had agreed that electrodynamic effects were involved, and that relativistic calculation would give a finite prediction. But there was also a totally unexpected disclosure, by Isador Rabi: the hyperfine structures in hydrogen and deuterium were larger than anticipated by a fraction of a percent. Here was another flaw in the Dirac electron theory, now referring to magnetic rather than electric properties.

Weisskopf and I had described at Shelter Island our idea that the relativistic electron-positron theory, then called the hole theory, would produce a finite electrodynamic energy shift. But it was Hans Bethe who quickly appreciated that a first estimate of this effect could be found without entering into the complications of a relativistic calculation. In a Physical Review article received on June 27, he says,

> "Schwinger and Weisskopf, and Oppenheimer have suggested that a possible explanation might be the shift of energy levels by the interaction of the electron with the radiation field. This shift came out infinite in all existing theories, and has therefore always been ignored. However, it is possible to identify the most strongly (linearly) divergent term in the level shift with an electromagnetic mass effect which must exist for a bound as well as a free electron. This effect should properly be regarded as already included in the observed mass of the electron, and we must therefore subtract from the theoretical expression, the corresponding expression for a free electron of the same average kinetic energy. The result then diverges only logarithmically (instead of linearly) in non-relativistic theory. Accordingly, it may be expected that in the hole theory, in which the main term (self-energy of the electron) diverges only logarithmically, the result will be convergent after subtraction of the free electron expression. This would set an effective upper limit of the order of mc to the frequencies

> of light which effectively contribute to the shift of the level of a bound electron. I have not carried out the relativistic calculations, but I shall assume that such an effective relativistic limit exists."

The outcome of Bethe's calculation agreed so well with the then not very accurately measured level shift that there could be no doubt of its electrodynamic nature. Nevertheless, the relativistic problem, of producing a finite and unique theoretical prediction, still remained.

The news of the Lamb–Retherford measurement and of Bethe's non-relativistic calculation reached Japan in an unconventional way.

Tomonaga says,

> "The first information concerning the Lamb shift was obtained not through the Physical Review, but through the popular science column of weekly U.S. magazine. This information about the Lamb shift prompted us to begin a calculation more exact than Bethe's tentative one."

He goes on:

> "In fact, the contact transformation method could be applied to this case, clarifying Bethe's calculation and justifying his idea. Therefore the method of covariant contact transformations, by which we did Dancoff's calculation over again, would also be useful for the problem of performing the relativistic calculation for the Lamb shift."

Incidentally, in speaking of contact transformations Tomonaga is using another name for canonical or unitary transformations. Tomonaga announced his relativistic program at the already mentioned Kyoto Symposium of November 24–25, 1947. He gave it a name, which appears in the title of a letter accompanying the one of December 30 that points out Dancoff's error. This title is 'Application of the Self-Consistent Subtraction Method to the Elastic Scattering of an Electron'. And so, at the end of 1947 Tomonaga was in full possession of the concepts of charge and mass renormalization.

Meanwhile, immediately following the Shelter Island Conference I found myself with a brand new wife, and for two months we wandered around the United States. Then it was time to go to work again. I also clarified for myself Bethe's nonrelativistic calculation by applying a unitary transformation that isolated the electromagnetic mass. This was the model for a relativistic calculation, based on the conventional hole theory formulation of quantum electrodynamics. But here I held an unfair advantage over Tomonaga, for, owing to the communication problems of the time, I knew that there were two kinds of experimental effects to be explained: the electric one of Lamb, and the magnetic one of Rabi. Accordingly, I carried out a calculation of the energy shift in a homogeneous magnetic field, which is the prediction of an additional magnetic moment of the electron, and also considered the Coulomb field of a nucleus in applications to the scattering and to the energy shift of bound states. The results were described in a letter to the Physical Review, received on December 30, 1947, the very same date as Tomonaga's proposal of the self-consistent subtraction method. The predicted additional magnetic moment accounted for the hyperfine structure measurements, and also for later, more accurate, atomic moment measurements. Concerning scattering I said "$\cdots$ the finite radiative correction to the elastic scattering of electrons by a Coulomb field provides a satisfactory termination to a subject that has been beset with much confusion". Considering the absence of experimental data, this is perhaps all that needed to be said. But when it came to energy shifts, what I wrote was,

> "The values yielded by our theory differ only slightly from those conjectured by Bethe on the basis of a non-relativistic calculation, and are, thus, in good accord with experiment."

Why did I not quote a precise number?

The answer to that was given in a lecture before the American Physical Society at the end of January, 1948. Quite simply, something was wrong. The coupling of the electron spin to the electric field was numerically different from what the additional magnetic moment would imply; relativistic invariance was violated in this non-covariant calculation. One could, of course, adjust that spin coupling to have the right value and, in fact, the correct energy shift is obtained in this way. But there was no conviction in such a procedure. The need for a covariant formulation could no longer be ignored. At the time of this meeting the covariant theory had already been constructed, and applied to obtain an invariant expression for the electron electromagnetic mass. I mentioned this briefly. After the talk, Oppenheimer told me about Tomonaga's prior work.

A progress report on the covariant calculations, using the technique of invariant parameters, was presented at the Pocono Manor Inn Conference held March 30 — April 1, 1948. At that very time Tomonaga was writing a letter to Oppenheimer which would accompany a collection of manuscripts describing the work of his group. In response, Oppenheimer sent a telegram: "Grateful for your letter and papers. Found most interesting and valuable mostly paralleling much work done here. Strongly suggest you write a summary account of present state and views for prompt publication in Physical Review. Glad to arrange." On May 28, 1948, Oppenheimer acknowledges the receipt of Tomonaga's letter entitled "On Infinite Field Reactions in Quantum Field Theory". He writes, "Your very good letter came two days ago and today your manuscript arrived. I have sent it on at once to the Physical Review with the request that they publish it as promptly as possible··· I also sent a brief note which may be of some interest to you in the prosecution of higher order calculations. Particularly in the identification of light quantum self energies, it proves important to apply your relativistic methods throughout. We shall try to get an account of Schwinger's work on this and other subjects to you in the very near future". He ends the letter expressing the "hope that before long you will spend some time with us at the Institute where we should all welcome you so warmly".

The point of Oppenheimer's added note is this: In examining the radiative correction to the Klein–Nishina formula, Tomonaga and his collaborators had encountered a divergence additional to those involved in mass and charge renormalization. It could be identified as a photon mass. But unlike the electromagnetic mass of the electron, which can be amalgamated, as Tomonaga put it., into an already existing mass, there is no photon mass in the Maxwell equations. Tomonaga notes the possibility of a compensation cancellation, analogous to the idea of Sakata. In response, Oppenheimer essentially quotes my observation that a gauge invariant relativistic theory cannot have a photon mass and further, that a sufficiently careful treatment would yield the required zero value. But Tomonaga was not convinced. In a paper submitted about this time he speaks of the "somewhat quibbling way" in which it was argued that the photon mass must vanish. And he was right, for the real subtlety underlying the photon mass problem did not surface for another 10 years, in the eventual recognition of what others would call 'Schwinger terms'.

But even the concept of charge renormalization was troubling to some physicists. Abraham Pais, on April 13, 1948, wrote a letter to Tomonaga in which, after commenting on his own work parallel to that of Sakata, he remarks, "it seems one of the most puzzling problems how to 'renormalize' the charge of the electron and of the proton in such a way as to make the experimental values for these quantities equal to each other". Perhaps I was the first to fully appreciate that charge renormalization is a property of the electromagnetic field alone, which results in a renormalization, a fractional reduction of charge, that is the same for all. But while I'm congratulating myself, I must also mention a terrible mistake I made. Of course, I wasn't entirely alone — Feynman did it too. It occurred in the relativistic calculation of energy values for bound states. The effect of high energy photons was treated covariantly: that of low energy photons in the conventional way. These two parts had to be joined together, and a subtlly involved in relating the respective four- and three-dimensional treatments was overlooked for several months. But sometime around September, 1948, it was straightened out, and, apart from some uncertainty about the inclusion of vacuum polarization effects, all groups, Japanese and American, agreed on the answer. As I have mentioned, it was the result I had reached many months before by correcting the obvious relativistic error of my first non-covariant calculation.

In that same month of September, 1948, Yukawa, accepting an invitation of Oppenheimer, went to the Institute for Advanced Study at Princeton, New Jersey. The letters that he wrote back to Japan were circulated in a new informal journal called Elementary Particle Physics Research — Soryushiron Kenkyu. Volume 0 of that journal also contains the communications of Oppenheimer and Pais to which I have referred, and a letter of Heisenberg to Tomonaga, inquiring whether Heisenberg's paper, sent during the war, had arrived. In writing to Tomonaga on October 15, 1948, Yukawa says, in part, "Yesterday I met Oppenheimer, who came back from the Solvay Conference. He thinks very highly of your work. Here, many people are interested in Schwinger's and your work and I think that this is the main reason why the demand for the Progress of Theoretical Physics is high. I am very happy about this".

During the period of intense activity in quantum electrodynamics, Tomonaga was also involved in cosmic ray research. The results of a collaboration with Hayakawa Satio were published in 1949 under the title "Cosmic Ray Underground". By now, the two mesons had been recognized and named: $\underline{\pi}$ and $\underline{\mu}$. This paper discusses the generation of, and the subsequent effects produced by, the deep penetrating meson. Among other activities in that year of 1949, Tomonaga published a book on quantum mechanics that would be quite influential, and he accepted Oppenheimer's invitation to visit the Institute for Advanced Study. During the year he spent there he turned in a new direction, one that would also interest me a number of years later. It is the quantum many-body problem. The resulting publication of 1950 is entitled "Remarks on Bloch's Method of Sound Waves Applied to Many-Fermion Problems". Five years later he would generalize this in a study of quantum collective motion.

But the years of enormous scientific productivity were coming to a close, owing to the mounting pressures of other obligation. In 1951 Nishina died, and Tomonaga accepted his administrative burdens. Now Tomonaga's attention turned toward improving the circumstances and facilities available to younger scientists, including the establishment of new Institutes and Laboratories. In 1956 he became President of the Tokyo University of Education, which post he held for six years. Then, for another six years, he was President of Science Council of Japan, and also, in 1964, assumed the Presidency of the Nishina Memorial Foundation. I deeply regretted that he was unable to be with us in Stockholm on December 10, 1965 to accept his Novel Prize. The lecture that I have often quoted today was delivered May 6, 1966.

Following his retirement in 1970, he began to write another volume of his book on quantum mechanics which, unfortunately, was not completed. Two other books, one left in an unfinished state, were published, however. To some extent, these books are directed to the general public rather than the professional scientist. And here again Tomonaga and I found a common path. I have recently completed a series of television programs that attempt to explain relativity to the general public. I very much hope that this series, which was expertly produced by the British Broadcasting Corporation, will eventually be shown in Japan.

Just a year ago today, our story came to a close. But Tomonaga Sin-Itiro lives on in the minds and hearts of the many people whose lives he touched, and graced.