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QUANTUM ELECTRODYNAMICS - AN INDIVIDUAL VIEW

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Résumé - Ce rapport a pour but de décrire le développement de l'électrodynamique quantique des années 30 aux années 50. Il repose sur ce que l'auteur a vécu et ce à quoi il a contribué. Il part de la préparation (1934 - 1946) puis passe à la théorie relativiste non covariante (1947) pour aboutir à la première théorie covariante relativiste (1947 - 1948) puis à la seconde (1949 - 1950). Une description technique détaillée est présentée. L'auteur indique l'influence de l'électrodynamique dans les autres domaines de la physique.

Abstract - The aim of this report is to describe the development of the quantum electrodynamics in the years from the 1930's to the 1950's. It is based on the way the author saw and participate to this development. Four phases are discussed : preparation (1934 - 1946) ; non-covariant relativistic theory (1947) ; first covariant relativistic theory (1947 - 1948) ; second covariant relativistic theory (1949 - 1950). A detailed technical description is presented. The author shows the influence of quantum electrodynamics in other areas of physics.

My assignment today is to testify. To tell the story, as I saw it and as I participated in it, of the development of quantum electrodynamics in the years from the 1930's to the 1950's. Yet I am also conscious that emphasis must be placed on documentation, rather than mere remembrance, an ideal that, like the speed of light, can be approached but never attained.

My story will be divided into four phases: Preparation (1934-1946); Non-Covariant Relativistic Theory (1947); First Covariant Relativistic Theory (1947-1948); Second Covariant Relativistic Theory (1949-1950).

The only exhibit I have with me is a paper I wrote, but did not publish at the age of 16. Called "On the Interaction of Several Electrons" it is about quantum electrodynamics. It combines the space-time varying operator fields of the Dirac, Fock, Podolsky electrodynamics[1] of 1932 with second quantized operator fields for electrons,[2] asking whether the usual formalism continues to apply when the electron interaction is the non-local retarded interaction of Møller.[3] In the process it makes the first tentative introduction of what I would later call the interaction representation, which is no more than the extension to all operator fields of what Dirac, Fock, and Podolsky had done for the electromagnetic field. Let me quote one sentence from the paper: "The second term in equation (20) represents the infinite self-energy of the charges and must be discarded." The last injunction merely parrots the wisdom of my elders, to be later rejected, that the theory was fatally flawed as witnessed by such infinite terms, which, at best, had to be discarded, or subtracted. Thus, the "subtraction physics" of the 1930's.

I skip over the events of the next eleven years, except to note the following. In the fall of 1939 I came to Berkeley for the first time, not as a student of Oppenheimer, but armed with a Columbia Ph.D. and a National Research Council Fellowship. Our first collaboration, later that year, used quantum electrodynamics to describe the electron-positron emission from an excited oxygen nucleus,[4] which emphasized for me the physical reality of such virtual photon processes. Also important was the 1941 work on strong coupling mesotron theory[5] where I gained experience in using canonical transformations for extracting the physical consequences of the theory.

We come to 1945. With the War winding down and an enormous capability in microwave technology developed, it was natural that frustrated physicists should begin to think of using their expertise in devising electron accelerators. I took a hand in that, myself, and designed parameters for an instrument I called the microtron, but that's another story. What is significant here was the radiation emitted by relativistic electrons moving in circular paths under magnetic field guidance. It's an old problem, but the quantitative implications of relativistic energies hadn't been appreciated. In attacking this classical relativistic situation I used the invariant proper-time formulation of action, including the electromagnetic self-action of a charge. That self-action contained a resistive and a reactive part, to use the engineering language I had learned. The reactive part was the electromagnetic mass effect, here automatically providing an invariant supplement to the mechanical action and thereby introducing the physical mass of the charge. Incidentally, in the paper on synchrotron radiation that was published several years later,[6] a more elementary expression of this method is used and the reactive effect is dismissed as "an inertial effect with which we are not concerned." But here was my reminder that electromagnetic self-action, physically necessary in one context, was not to be, and need not be, omitted in another context. And, in arriving at a relativistically invariant result, in a subject where relativistic invariance was notoriously difficult to maintain, I had learned a simple but useful lesson: to emerge with relativistically invariant physical conclusions, use a covariantly formulated theory, and maintain covariance throughout the calculation.

Of course, the concept of electromagnetic self-action, of electromagnetic mass, had not entirely died out in that age of subtraction physics; it had gone underground, to surface occasionally. Kramers must be mentioned in this connection. In a book published[7] in 1938 he suggested that the correspondence principle foundation of quantum electrodynamics was unsatisfactory because it was not related to a classical theory that already included the electromagnetic mass and referred to the physical electron. He proposed to produce such a classical theory by eliminating the proper field of the electron, the field associated with uniform motion. Very good--if we lived in a non-relativistic world. But it was already known from the work of Weisskopf and Furry[8] that the electromagnetic mass problem is entirely transformed in the relativistic theory of electrons and positrons, then described in the unsymmetrical hole formulation--the relativistic electromagnetic mass problem is beyond the reach of the correspondence principle. Nevertheless, I must give Kramers very high marks for his recognition that the theory should have a structure-independent character. The relativistic counterpart of that was to be my guiding principle, and, over the years has become generalized to the Commandment: Thou shalt not entangle that which is known, and reliable, with that which is unknown, and speculative. The effective range treatment of nuclear forces,[9] which evolved just after the War, also abides with this philosophy.

The next phase opened with the famous Shelter Island Conference of June 1947. Not recalling the exact dates, I looked at the Lamb-Retherford paper[10] and learned that it was June 1-3; then I glanced at Bethe's paper[11] and read that it was June 2-4. Anyway, it was in June. On the train down to New York, Weisskopf and I discussed the already leaked news that Lamb and Retherford had used the wartime developed microwave techniques to confirm Pasternack's[12] suggested upward shift of the 2S level in hydrogen. We agreed that electrodynamic effects should be responsible, and that a finite result would emerge from a relativistic calculation. I do not recall actually saying anything at Shelter Island, but Bethe acknowledges such remarks. As we all know, Bethe then instantly proceeded to exploit his great familiarity with hydrogenic dipole matrix elements and sum rules to compute the non-relativistic aspects of these ideas. Owing to the comparative insensitivity of the calculation to the unknown high energy cutoff, a better than order of magnitude number emerged. The agreement of that number with the observed level shift ended any doubt, if doubt there was, concerning the electrodynamic nature of the phenomenon. Yet the relativistic problem, of extracting from the theory a finite and unique prediction, remained.

The Lamb-Retherford measurement had been foreshadowed by pre-war spectroscopic observations. But the Shelter Island Conference also brought a totally unanticipated announcement, from Rabi: the hyperfine structures in hydrogen and deuterium were too large by a fraction of a percent. The significance of the small difference between these two fractions would later be explained by Aage Bohr.[13] But it was their similarity that counted first, suggesting that there was yet another flaw in the Dirac description of the electron, now referring to magnetic properties. The hypothesis that the electron had an additional magnetic moment was first explicitly published by Breit,[14] later that year, in a curiously ambivalent way, "It is not claimed that the electron has an intrinsic moment. Aesthetic objections could be raised against such a view." Perhaps that ambivalence caused Breit to falter, for he, and here I quote myself, did "not correctly draw the consequences of his empirical hypothesis." He arrived at a value of the additional magnetic moment about five times larger than what more direct experiments, not to mention the relativistic electrodynamic theory, would soon disclose. An additional magnetic moment that large would contribute about one third of the observed upward relative displacement of the 2S level of hydrogen. It was not necessary--the empirical hypothesis of an additional electron moment is easily handled correctly--but, in fact, it took the development of the relativistic electrodynamic theory to straighten out the confusion. However, we are getting ahead of the story.

At the close of the Shelter Island conference, Oppenheimer and I took a seaplane from Port Jefferson to Bridgeport, Connecticut, where civilization, as it was then understood--the railroad--could be found. As the seawater closed over the airplane cabin I counted my last remaining seconds. But somehow primitive technology triumphed. A few days later I abandoned my bachelor quarters and embarked upon an, accompanied, nostalgic trip around the country that would occupy the whole summer. Not until September did I set out on the trail of relativistic quantum electrodynamics. But I knew what to do.

This is how I would shortly put it, in the first published report[15] of the new electrodynamics: "Attempts to evaluate radiative corrections to electron phenomena have heretofore been beset by divergence difficulties, attributable to self-energy and vacuum polarization effects. Electrodynamics unquestionably requires revision at ultra-relativistic energies (sic), but is presumably accurate at moderate relativistic energies. It would be desirable, therefore, to isolate those aspects of the current theory that essentially involve high energies, and are subject to modification by a more satisfactory theory, from aspects that involve only moderate energies and are thus relatively trustworthy. This goal has been achieved by transforming the Hamiltonian of current hole theory electrodynamics to exhibit explicitly the logarithmically divergent self-energy of a free electron, which arises from the virtual emission and absorption of light quanta. The electromagnetic self-energy of a free electron can be ascribed to an electromagnetic mass, which must be added to the mechanical mass of the electron. Indeed the only meaningful statements of the theory involve this combination of masses, which is the experimental mass of a free electron." Then, skipping a bit, "it is important to note that the inclusion of the electromagnetic mass with the mechanical mass does not avoid all divergences; the polarization of the vacuum produces a logarithmically divergent term proportional to the interaction energy of the electron in an external field. However, it has long been recognized that such a term is equivalent to altering the value of the electron charge by a constant factor, only the final value being properly identified with the experimental charge. Thus the interaction between matter and radiation produces a renormalization of the electron charge and mass, all divergences being contained in the renormalization factors." The statement beginning "However, it has long been recognized..." harkens back to the very beginnings of the hole theory of positrons. Allow me to translate from the French of Dirac's 1934 report to the 7th Solvay Congress[16]: "In consequence of the preceding calculation it would seem that the electric charges normally observed on electrons, protons or other electrified particles are not the charges actually carried by these particles and occurring in the fundamental equations, but are slightly smaller."

One more sentence from my not yet written report[15]: "The simplest example of a radiative correction is that for the energy in an external magnetic field." In mid-November of 1947 I went to Washington to attend a small meeting at George Washington University and give a status report on that calculation, of the additional magnetic moment of the electron. It was not complete at the time, but I have the finished calculation, which was discovered in a pile of manuscripts on January 24, 1976, and then labeled "Original Calculation of $\alpha/2\pi$ (1947)." But the magnetic moment of the electron was not my sole concern at that time. My one distinct memory of the Washington meeting is of sitting at a big table and apparently taking notes during a lecture--was it Gamov explaining his ideas on the black body residual radiation of the big bang? I don't recall. What I do recall is that I was actually doing some simple computations, using my knowledge of the hydrogenic wave functions in momentum space, to understand the "amazingly high value," as Bethe put it, of his average excitation energy for hydrogen. I still have fragments of those clandestine calculations. I had easily found that the logarithm of the excitation energy in Rydberg units should be approximately 211/84, or a little more than 2.5. The actual value, which requires rather extensive numerical calculations, is about 2.8.

The first report on renormalized quantum electrodynamics, excerpts of which have just been quoted, was submitted to the Physical Review at the end of 1947. It gives the predicted additional magnetic moment of $\alpha/2\pi$ and points out that, not only are the hyperfine structure discrepancies accounted for, but also the later more accurate atomic moment measurements in states of sodium and gallium.[17] The report continues, "The radiative corrections to the energy of an electron in a Coulomb field will produce a shift in the energy levels of hydrogen-like atoms and modify the scattering of electrons in a Coulomb field.... 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. Finally, the finite radiative correction to the elastic scattering of electrons by a Coulomb field provides a satisfactory termination to a subject that had been beset with much confusion." Now, what is that last bit all about?

While the question of bound state energies had been largely ignored, theorists had given attention to radiative corrections in scattering. In 1937 Bloch and Nordsieck[18] recognized that arbitrarily soft photons are emitted with certainty in a collision, implying that the cross section for a perfectly elastic collision is zero. Yet, in a treatment that considers only soft photons, the total cross section is unchanged from its value in the absence of electromagnetic interaction. The real problem begins when hard virtual photons are reintroduced. In 1939 Dancoff[19] performed such a relativistic calculation for both spin 0 and spin 1/2 charged particles. Incidentally, on reading Dancoff's paper not long ago, I was somewhat astonished to see the word "renormalization." But the context there was not mass or charge renormalization; it referred to the additional terms that maintain the normalization of the state vector. The confusing outcome of Dancoff's calculation was that, whereas spin 1/2 produced a divergent radiative correction, spin 0, usually associated with more severe electromagnetic self energy problems, gave a finite correction. The new theory removed the difficulty for spin 1/2. At about the same time Lewis[20] reconsidered Dancoff's spin 1/2 work and recognized that it was inconsistent in its treatment of the mechanical and the physical masses of the electron. Then, on subtracting the effect of the electromagnetic mass, the divergences did cancel. But such a subtraction of two ambiguous expressions does not automatically produce an unambiguous finite residue. Lewis acknowledged that the canonical transformation method I had developed was better suited to that purpose. All this raises a question. After reporting that finite radiative corrections were attained in both bound state and scattering calculations, why was I not specific about their precise values?

Within a month the reason would be given publicly. The American Physical Society held its 1948 New York meeting from January 29-31 at Columbia University. I was invited to give a paper on Recent Developments in Quantum Electrodynamics. By the way, another invited paper at that meeting was a report from the General Electric

Laboratory on the observation and satisfactory spectral analysis of the visible synchrotron radiation emitted by 70 Mev electrons. On January 31 I gave my talk--twice. The only record I have of that event is a typed copy of my already submitted report, on the back page of which is written a formula for the energy shift of hydrogenic levels. One of the terms is a spin-orbit coupling, which should be the relativistic electric counterpart of the $\alpha/2\pi$ additional magnetic moment effect. But it is smaller by a factor of 3; relativistic invariance is violated in the non-covariant theory. Oppenheimer would later record this in his report[21] to the 8th Solvay Congress. But the back of the page also contains something else--the answer to the obvious question: what happens if the additional magnetic moment coupling to the electric field is given its right value, no other change being introduced? What emerges, and therefore was known in January 1948, is precisely what other workers using non-covariant methods would later find, which is also the result eventually produced by the covariant methods. Of course, until those covariant methods were developed and applied, there could be no real conviction that the right answer had been found.

The third stage, the development of the first covariant theory, had already begun at the time of the New York meeting in January. I have mentioned that the simple idea of the interaction representation had presented itself 14 years earlier, and the space-time treatment of both electromagnetic and electron-positron fields was inevitable. I have a distinct memory of sitting on the porch of my new residence during what must have been a very late Indian summer in the fall of 1947 and with great ease and great delight, arriving at invariant results in the electromagnetic mass calculation for a free electron. I suspect this was done with an equal time interaction. The space-like generalization, to a plane, and then to a curved surface took time, but all that was in place at the New York meeting. I must have made a brief reference to these covariant methods; the typed copy of my report contains such an equation on another back page, and I know that Oppenheimer told me about Tomonaga after my lecture.

Tomonaga's work on a covariant Schrödinger equation had, in 1943, been published in Japanese and then, in 1946, was translated into English to appear in an early issue of a new Japanese journal.[22] I have read remarks to the effect that, if scientific contact had not been broken during the Pacific war, the theory that we are now reviewing would have been significantly advanced. Of course, lacking an unlimited number of parallel universes in which to act out all possible scenarios, such statements are meaningless. Nevertheless, I shall be bold enough to disagree. The preoccupation of the majority of involved physicists was, not with analyzing and carefully applying the known relativistic theory of coupled electron and electromagnetic fields, but with changing it. The work of Tomonaga and his collaborators, immediately after the War, centered about the idea of compensation, the introduction of the fields of unknown particles in such a way as to cancel the divergences produced by the known interactions.[23] Feynman also advocated modifying the theory, and would later intimate that a particular, satisfactory modification could be found.[24] My point is merely this. A formalism such as the covariant Schrödinger equation is but a shell awaiting the substance of a guiding physical principle. And, the specific concept of the structure-independent, renormalized relativistic electrodynamics, while always abstractly conceivable, in fact required the impetus of experiments to show that electrodynamic effects were neither infinite nor zero, but finite and small, and demanded understanding.

The first covariant formulation, in action, was exhibited at the Pocono Manor Inn Conference of March 30 - April 1, 1948. I possess a copy of the notes that were taken of the 14 lectures, including those of Feynman and myself. On reading over what was written about my work, I felt no conviction that it was a reliable record of what was actually said; the intrusive hand of the reporter lies heavy on those pages. However, much the same material appears in notes of lectures delivered several months later at the University of Michigan. Beyond the formalities of field equations, commutation relations, vacuum expectation values, and the like, the topics discussed were: free electron mass, photon mass and vacuum polarization, and the electron in an external field, leading to the additional magnetic moment and the energy shifts of hydrogenic atoms. Although it is a vast improvement over the non-covariant methods, what is contained here is still quite primitive. But it introduces the essential computational device of relativistically invariant parameters, quantum counterparts of proper time. It is those parameters that appear in the various outcomes, where they greatly facilitate the separation of the renormalization terms from the actual physical effect under consideration. A logarithmically divergent, invariant electromagnetic mass for the free electron emerges in this way, as it had in the Indian summer of 1947. The photon mass would be a more vexing subject. As Oppenheimer is cited as remarking at Pocono, a covariant gauge invariant theory could not have a non-zero photon mass, and there is no need to compute it. Yet people, notably Wentzel,[25] would insist on doing so and end up with non-zero answers. The real subtlety underlying this problem did not emerge for another decade, in the eventual explicit recognition[26] of what others would call Schwinger terms.

While the Pocono Conference was in session, Tomonaga was completing a covering letter, directed to Oppenheimer, which was attached to a collection of papers describing the work that had been done in Japan, both independently and in reaction to the news from the West. In a subsequent review paper, written in response to Oppenheimer's telegraphed request, Tomonaga comments on the problem raised by the "infinity (that) is to be attributed to the vacuum polarization effect," in other words, the photon mass. Characteristically, one of the suggested remedies is compensation, the introduction of another charged particle that would produce a photon mass term of opposite sign. In transmitting this communication[27] to the Physical Review, Oppenheimer added a note about the photon mass, or, as he put it, "the familiar problem of the light quantum self-energy." He remarked that "as long experience and the recent discussions of Schwinger and others have shown, the very greatest care must be taken in evaluating such self-energies lest, instead of the zero value which they should have, they give non-gauge covariant, non-covariant, in general infinite results."

The Pocono Conference was my first opportunity to learn what Feynman was doing with quantum electrodynamics. I had seen his work with Wheeler[28] on classical electrodynamics, and the idea of abolishing the electromagnetic field, in a fundamental sense, didn't appeal to me at all. Feynman had discarded the operator field formulation and yet, as his talk proceeded I could see points of similarity and, of course, points of difference, other than formalistic questions. We agreed in the emphasis on a manifestly covariant, four-dimensional description including the use of a four-dimensional electromagnetic gauge. It is interesting that, where we differed in techniques of computation, time has seen a mutual accommodation. Feynman used, not invariant parameters, but non-covariant integration methods; he would later adopt invariant parametrization. Where I used two kinds of invariant functions arising from commutator and vacuum expectation value considerations, Feynman, as had Stückelberg[29] before him, used a complex combination of the two. At the later stage of the second covariant theory I would also find it to be the natural element. The mention of Stückelberg brings me back to the remark made in connection with Tomonaga. I regret that I didn't find the occasion to review the papers, but I gather that Stückelberg had early anticipated several of the later features of the invariant perturbation theory of coupled relativistic fields. But Stückelberg also failed to develop renormalized quantum electrodynamics prior to the experimental impetus of 1947.

The subject of vacuum polarization is a point on which, throughout this 1948 period, and beyond, Feynman and I disagreed, a point not of individual mathematical style, but of fundamental physics. In his report to the 8th Solvay Congress,[30] Bethe said, "The polarization of the vacuum is consciously omitted in Feynman's theory." The reasoning went this way. A modification of the electromagnetic interaction made the electromagnetic mass finite, but did nothing for the apparently more severely divergent--here it is again--photon mass. Therefore things would be simpler if all such effects--closed loops, in Feynman's graphical, acausal language--were omitted. But I knew that the virtual photon emitted by the excited oxygen nucleus created an electron-positron pair; the vacuum is polarizable. In a later paper,[31] I would use this very example to illustrate a manifestly gauge invariant treatment of vacuum polarization.

The effect on the electron spin of an external magnetic field poses no problem in the covariant formulation. The additional $\alpha/2\pi$ magnetic moment in a static field is regained, but now one also sees explicitly that this is a dynamical effect, disappearing as the invariant measure of space-time variation of the field becomes increasingly large on the relativistic scale. It is when we, Feynman and I, turned to an electrostatic field, to the relativistic justification and extension of the Bethe calculation, that an unfortunate and quite unnecessary bit of confusion entered. The problem was the joining of the relativistic calculation, where the Coulomb potential is regarded as a perturbation, to the non-relativistic calculation, which treats the Coulomb potential exactly. Later developments would avoid the unphysical separation, but the first attacks used it. And both Feynman and I goofed--we blew it. The physical problem of bound states is not sensitive to arbitrarily soft photons--the atom defines a natural scale of frequencies. But the relativistic treatment of the Coulomb potential as a perturbation, a scattering situation, is sensitive, as in the Bloch-Nordsieck discussion. This is the so-called infra-red divergence. And the non-relativistically calculated difference between the correct and the perturbation treatments of the Coulomb field must also be sensitive, in such a way as to cancel out the infra-red divergence in the complete expression. But clearly that will happen without error only if the treatment of soft photons in the relativistic and non-relativistic parts is consistent. With our eyes on the high energy end of the photon spectrum, both Feynman and I were careless about the low energy end.

The following remarks are intended to clarify, not to excuse that lapse. One provisional technique for handling the infra-red problem is to pretend that the photon does have--horrors!--a non-zero mass. Actually, in a theory that otherwise is gauge invariant, the unphysical processes thereby introduced will quickly disappear as that mass is finally set equal to zero. The relativistic perturbation calculation easily accepts a small photon mass. In the non-relativistic dipole approximation it is only the photon energy that makes an appearance. It's not hard to remember that the integration over photon energy is actually a momentum space integral and take into account the altered momentum-energy relation demanded by the non-zero mass. But there's more. The non-relativistic treatment refers only to transversely polarized photons, as is appropriate to their motion at the speed of light. But, with diminishing energy a massive photon slows down and the longitudinal polarization begins to contribute. It's not natural to think of slow, longitudinally polarized photons, and we didn't, but one must, if the whole treatment is to be consistent.

Sometime in 1948, Weisskopf and French completed their non-covariant calculation of the bound state energy shift, using every possible clue to maintain relativistic invariance, including the known effect of a magnetic field. Their result was similar to, but not quite identical with what the covariant calculations of Feynman and myself had produced, which were the same, apart from Feynman's omission of the vacuum polarization effect. Somewhat shaken, French and Weisskopf retreated to their blackboards and pondered. I, of course, believed the covariant calculation. But I happened to chance on the, by then, almost forgotten outcome of my own non-covariant calculation using the right spin-orbit coupling. It was identical to the French-Weisskopf result! That shook me up to the point that, as Dyson in 1949 attested,[32] I found the careless slip in the use of the photon mass. This reconciled all the calculations,[33] vacuum polarization aside. And so, as far as the relativistic energy shift is concerned, while Weisskopf was not the first to find the correct result, he was the first to insist on its correctness.

From July 19 to August 7, 1948, a period of three weeks, I lectured at the University of Michigan Summer School on--what else!--Recent Developments in Quantum Electrodynamics. It seems that I supplied the notes for the first part of the course, which must have been the manuscript for the paper[34] received by the Physical Review on July 29. The notes for the second part of the course were taken by David Park. I have read recently words to the effect that what I presented there was like a cut and polished diamond, with all the rough edges removed, brilliant and dazzling. Or, if you don't care for that simile, you can have "a marvel of polished elegance, like a difficult violin sonata played by a virtuoso--more technique than music." I gather I stand accused of presenting a finished, elaborate mathematical formalism from which had been excised all the physical insights that provide signposts to its construction. To all charges, I plead: Not Guilty. The paper to which I have referred[34] has a long historical and physical introduction that motivates the development, and sets out the goals, of relativistic renormalization theory. Beyond that, the lectures presented the explicit working out of the interaction of a non-relativistic electron with the radiation field, in the dipole approximation. The canonical transformation that isolates the electromagnetic mass is an elementary one, and the further details leading to the solution of the bound state and scattering problems are provided. This was the simple model on which the relativistic theory was erected. It was good enough for the immediate purposes but, as I have already remarked, still quite primitive. I needed no one to tell me that it was but a first step to an aesthetically satisfactory and effective relativistic theory of coupled fields. Incidentally, at about this same time the canonical transformation method was being successfully applied, by Corinaldesi and Jost,[35] to the radiative correction for the cross section of Compton scattering on a spinless charged particle.

Sometime in mid 1948 I became aware that the National Academy of Sciences was offering a prize for "an outstanding contribution to our knowledge of the nature of light." Entries could be in either of two categories, of which one was a contribution published or submitted in manuscript before October 1, 1948, "which is a comprehensive contribution to a logical, consistent theory of the interaction of charged particles with an electromagnetic field including the interaction of particles moving with high relative speeds." Well! And, when I noticed that Feynman was on the committee to award the prize, and therefore presumably ineligible to receive it, I decided that someone out there had me in mind. The reason I mention this "ain't the money; it's the principle of the thing."[36] I submitted the manuscripts of two completed papers and the incomplete, provisional version of a third paper. What survives of that third paper begins with the relativistic treatment of radiative corrections to Coulomb scattering, a topic that was experimentally remote at the time, but is now a routine aspect of interpreting high energy experiments that employ electrons and positrons. Then the manuscript takes up the topic "Radiative Corrections to Energy Levels" and begins "In situations that do not permit the treatment of the external field as a small perturbation, it is convenient to employ a representation in which the matter field spinors obey equations that correspond to a particle moving under the influence of the external potential." This is what, several years after, would be called the Furry representation.[37] The manuscript goes on to study solutions of those field equations and, in the process, exhibits integral equations that are the space-time, relativistic versions of what Lippman and I would present, more symbolically, a year or so later.[38] The manuscript ends abruptly in the middle of a sentence: deadline time had arrived.

I may have been seriously distracted by the pressure of other work, for the completed third and last paper in the Quantum Electrodynamics series[39] was not submitted until May 26, 1949, although a summary of the results for relativistic Coulomb scattering corrections and energy shifts was sent in at the beginning of that year.[40] I cite in this connection my only memory of the Old Stone on the Hudson meeting, held in April of 1949. On arriving, I was somewhat disconcerted to be immediately asked to report what I was thinking about. To which I replied, half facetiously and half factually that "the Harvard group was not thinking, it was writing." But it is more probable that the delay had a psychological basis. The impetus of the experimental discoveries of 1947 was waning. The pressure to account for those results had produced a certain theoretical structure that was perfectly adequate for the original task, but demanded simplification and generalization; a new vision was required. There already were visions at large, being proclaimed in manner somewhat akin to that of the Apostles, who used Greek logic to bring the Hebrew god to the Gentiles. I needed time to go back to the beginnings of things; not yet would I go back to the source.

My retreat began at Brookhaven National Laboratory in the summer of 1949. It is only human that my first action was one of reaction. Like the silicon chip of more recent years, the Feynman diagram was bringing computation to the masses. Yes, one can analyze experience into individual pieces of topology. But eventually one has

to put it all together again. And then the piecemeal approach loses some of its attraction. Speaking technically, the summation of some infinite set of diagrams is better and more generally accomplished by solving an integral equation, and those integral equations usually have their origin in a differential equation. And so, the copious notes and scratches labeled New Opus, that survive from the summer of 1949, are concerned with the compact, operator expression of classes of processes. And slowly, in these pages, the integral equations and the differential equations emerge. There is another collection of scraps which, at sometime in the past, I put into a folder labeled New Theory - Old Version (1949 - 1950), although I now believe that the reference to 1950 is erroneous--by then the New Theory in its later manifestation had arrived. There is a way to tell the difference. With the emphasis on the operator field description of realistic, interacting systems, the interaction representation had begun to lose its utility, and fields incorporating the full effects of interaction enter. The unpublished essay of the National Academy of Sciences competition had already taken a step in that direction. If fields of both types, with and without reference to interaction, appear in an equation, the historical period is that of the Old Version. The later version has no sign at all of the interaction representation. On one of these pages there is an Old Version, 1949, equation giving the first steps toward the relativistic equation for two interacting particles now known as the Bethe-Salpeter equation. Accordingly, it is not surprising to read in a footnote of a 1951 paper,[41] presenting an operator derivation of the two-particle equation, that I had already discussed it in my Harvard lectures. Before I take up what is really important in this new theory, which is the second covariant relativistic theory, the realization of the new vision that I sought, let me, for a moment, turn anecdotist.
I had been invited to the 1948 Solvay Congress meeting in Brussels, but did not go, and regretted it. Accordingly, I was more than pleased to accept an invitation to present a paper at the International Congress for Nuclear Physics, Quantum Electrodynamics and Cosmic Rays, jointly sponsored by the Italian and Swiss Physical Societies, and to be held in Basel and Como from September 5-16, 1949. My story does not concern the meeting itself, which was a great social occasion; it is about a side trip to Zürich. Rabi was in Paris, the first stop of my epic journey, and he insisted that I talk to Pauli, to soothe his ruffled feelings. Apparently I had transgressed, but the precise nature of my sin I do not now recall. And so we went to Pauli. He, along with Villars had just completed a paper[42] that had taken them through all the recent publications in quantum electrodynamics. He sat me down and voiced his unhappiness with various aspects of my papers. To each of his complaints I would, in effect, reply "Yes, but I don't do it that way anymore." This refusal to be a stationary target left Pauli utterly exasperated. Nevertheless, I think we parted friends.[43]

Feynman had found his vision in a paper of Dirac[44] that gave a correspondence principle setting for action, the natural, invariant starting point of a relativistic theory. I found my vision in the same place. Working with simple mechanical systems,[45] Feynman noticed that Dirac's asymptotic connection, between the quantum description of time evolution and the classical action, sharpened into an equality, for infinitesimal time changes. The indefinite repetition of infinitesimal displacements gave a quantum description of time development in an integral form, similar to the one Wiener had earlier introduced in another context. One could easily generalize particle variables to Bose-Einstein fields and emerge with the type of functional integral that is commonly regarded today as the starting point of quantum field theory. But quantum field theory must deal with Bose-Einstein fields and Fermi-Dirac fields on a fully equivalent footing. There is nothing in these correspondence principle based integrals that suggests the need for anticommuting objects, or supplies the meaning of integration for such variables without reference to independent knowledge of some properties of that kind of system. This was not my idea of a fundamental basis for the theory. And, as the history of physics, and my own experience indicated, integral statements are best regarded as consequences of more basic differential statements. Indeed, the fundamental formulation of classical mechanics, Hamilton's principle, is a differential, a variational, principle.

There was my challenge. What is the general quantum statement of Hamilton's principle in variational form? It's not hard to find--Dirac's paper already contains some steps in this direction. Here it is.[46] Time development is represented by a transformation function, relating the states of the system at two difference times, or, if you like, on two different space-like surfaces. Apart from a factor of $i = \sqrt{-1}$, the variation of this transformation function is just the corresponding matrix element, referring to those states, of the variation of the action operator--for a certain class of operator variations. It is the introduction of operator variations that cuts the umbilical cord of the correspondence principle and brings quantum mechanics to full maturity. The way is now open for Fermi-Dirac fields to appear naturally and on an equal footing with Bose-Einstein fields.

This development must have begun in late 1949 or early 1950, to judge by a set of notes entitled Quantum Theory of Fields, A New Formulation. They were taken by the now President of the California Institute of Technology, then known as Marvin Goldberger. Dated July, 1950, they refer to a field theory course that was given in the semester between January and June. First for particles, and then for fields, the notes trace how the single quantum action principle leads to operator commutation relations, equations of motion, or field equations, and conservation laws. In the relativistic field context, the postulate of invariance under time reflection (remember, this is 1950) leads to two kinds of fields--two statistics--as a consequence of the more elementary analysis into two kinds of spin, integral and half-integral. This occurs because time reflection is not a canonical, a unitary, transformation, but also requires an inversion in the order of all products. That discloses the fundamental operator nature of the field, distinguishing essential commutativity from essential anticommutativity, as demanded by the spin character of the field. In a subsequent version[47] the existence of two kinds of fields with their characteristic operator properties is recognized at an earlier stage. Here also the non-Hermitian fields of charged particles are replaced by Hermitian fields of several components, facilitating the description of the internal degrees of freedom that would later proliferate. In this version, time reflection implies a transformation to the complex conjugate algebra, and the postulate of invariance predicts the type of spin to be associated with each statistic. An inspection of the proof shows that what is really used is the hypothesis of invariance under time and space reflection. That invariance and the spin-statistics connection are equivalent. But, with the later discovery of parity non-conservation, the common emphasis as embodied in the so-called TCP (or is it PTC?) theorem, is to regard the spin-statistics relation as primary and the invariance under space-time reflection as a consequence.

The Theory of Quantized Fields is the title of a series of papers that developed and exploited the quantum action principle. The first of this series[46] was largely written during the summer of 1950, again at the Brookhaven National Laboratory. Also begun at this time was a paper[31] that I have already mentioned as a manifestly gauge invariant treatment of vacuum polarization. But more significant here is the glimpse it gives of the new spirit, in use, but without detailed introduction. An Appendix contains a modified Dirac equation involving a so-called mass operator that is constructed from the Green's functions of electron and photon. The reader is referred to a footnote that most unhelpfully says, "The concepts employed here will be discussed at length in later publications." The purpose of the Appendix is to provide a short, but not yet the shortest rederivation of the $\alpha/2\pi$ magnetic moment. I cannot refrain from remarking that this same year saw the first application of the Feynman-Dyson methods to a problem that had not already been solved by other procedures. This was the calculation by Karplus and Kroll[48] of the α^2 modification of the electron magnetic moment. They got it wrong. That error remained unnoticed until 1957, when Sommerfield, as his doctoral thesis, used the mass operator technique to produce the right answer.[49]

I have earlier stated my goal of achieving an aesthetically satisfactory and effective relativistic theory of coupled fields. What I have just discussed about the two statistics is, I believe, aesthetically satisfactory. Effectiveness came with the introduction of sources.[50] The concept of source uses numerical space-time functions; totally commutative numbers for Bose-Einstein fields, totally anticommutative numbers for Fermi-Dirac fields. The latter constitute a Grassmann algebra. Often considered bizarre thirty years ago, anticommutative number systems are now the darlings of the super-symmetryists. A source enters the action operator multiplied by its associated field. Those additional action terms symbolize the interventions that constitute measurement of the system, as the test charge in electrostatics probes the electric field. The action principle expresses this succinctly. Apart from the ubiquitous i, the functional derivative of the transformation function with respect to a source is the matrix element of the associated field. That enables all operator field equations to be represented by numerical functional derivative equations. And, the commutation properties of the fields at equal times, or on a space-like surface, are implicit in the fact that the operator field equations now contain the sources, acting as driving terms. The sources serve yet a third function. Through their dynamical action, any desired initial or final state of the system can be produced from the physical ground state, the invariant vacuum state. Accordingly, it suffices to consider the transformation function connecting the vacuum states on two different space-like surfaces, in the presence of arbitrary sources. The functional differential equations are given a less concise but more elementary form on expanding the vacuum probability amplitude as an infinite power series in the sources. The coefficient of a particular product of sources, referring to a set of space-time points, is a function of those points. I gave the name Green's function to the totality of those multi-point functions. As the equivalent of the functional differential equations, the Green's functions obey an infinite linear, inhomogeneous set of coupled differential equations. The accompanying boundary conditions, implied by the reference to the vacuum state, are the generalization of those introduced by Stückelberg and Feynman.

But the set of coupled Green's function equations is only one way of applying this flexible source method. Do you want to work directly with a perturbation expansion of the transformation function? Then use functional derivatives with respect to sources to construct the interaction term of the action operator. The transformation function for the physical, interacting system will now be produced, from the interactionless transformation function, by the effect of an exponential involving that functional derivative replacement for the field interaction term. (Confronted with a sentence like this one appreciates why mathematics is the preferred language of theoretical physics.) The power series expansion of the exponential then generates, order by order, the desired perturbation series. Topology--the Feynman diagrams--is optional here; that is a matter of pedagogy, not physics. And, for sufficiently complicated situations, it should be advantageous to have a method that supplies all relevant terms analytically, rather than by geometrical intuition. Would you rather manipulate functional integrals? Then begin with a formal solution of the functional differential equations in which an exponential function of the action--multipled by i, of course--with operators replaced by functional derivatives, acts on a grand delta functional of all sources. The Fourier construction of that delta functional, using well defined functional integration concepts, then yields the functional integral construction of the transformation function. And, there are mixed procedures, with functional derivatives for one kind of source entering numerical differential equations for the other type of field.

What I have just described is all technique. Now, here is the music. It is probably a fairly wide-spread opinion that renormalized quantum electrodynamics is just the old, quantized, version of the combined Maxwell and Dirac equations, with some rules for hiding divergences. That is simply not true. A theory has two aspects. One is a set of equations relating various symbols. The other is, at some level, the physical interpretation to be associated with the symbols. In the course of the development here being described, the equations did not change, but the interpretation did. In the late 1930's most people would not have challenged these statements: e and m, as they enter the Dirac and Maxwell equations, are the charge and the mass of the electron; an electromagnetic field operator creates or annihilates a photon; a Dirac field operator creates an electron or annihilates a positron, its adjoint field does the inverse. And all this would be true if the two fields were uncoupled. But, in the real world, the localized excitation represented by an electromagnetic field, for example, does not just create a photon; it transfers energy, momentum, angular momentum, and then Nature goes to work. And so, it may create a photon, or an electron-positron pair, or anything else with the right quantum numbers. The various Green's functions are the correlation functions among such localized excitations, and the study of their space-time behavior is the instrument for the identification of the physical parties, and of their interactions. Renormalization, properly understood, is an aspect of the transfer of attention from the initial, hypothetical world of localized excitations and interactions to the observable world of the physical particles. As such, it is logically independent of divergences. Could we construct a convergent theory of coupled fields, it would still need to be renormalized.

All that I have been saying was explicit or implicit in work performed before the end of the fifth decade, although actual publication would be delayed, sometimes indefinitely.[51] Thereafter, quantum electrodynamics was incorporated into the general quantum theory of particles and fields. But I feel that I cannot conclude without saying something about the more recent influence that electrodynamics has had in other areas of physics. And I do not see how I can avoid mentioning the ultimate fate of renormalization in my hands. Rather than march into the sixties and seventies, I turn back in time.

Here is an anecdote of 1941, unattested and, unfortunately, now unattestable. I had been thinking about Fermi's theory of β-decay, wherein appears a very small coupling constant of order 10^{-12}. It occurred to me that the electron mass, then used as the significant mass scale, was not necessarily the relevant quantity. The neutron and proton were also involved, and possibly the nucleon mass was the appropriate unit. On introducing it, the coupling constant became of order 10^{-5}. And then I thought--perhaps the really significant mass unit is several tens of nucleon masses, for then the coupling constant could be the electromagnetic coupling constant $\alpha \cong 1/137$. One day I mentioned this bit of numerology to Oppenheimer. He stared at me, and then said coldly, "Well, it's a new idea." Indeed it was, and is.[52]

And finally, I turn to the last section of a 1949 paper by Dyson,[53] which I think it reasonable to assume was strongly influenced by Oppenheimer. In any event, here is a quotation "...[w]hat is to be looked for in a future theory is not so much a modification of the present theory which will make all infinite quantities finite, but rather a turning-round of the theory so that the finite quantities shall become primary...," and then, "One may expect that in the future a consistent formulation of electrodynamics will be possible, itself free from infinities and involving only the physical constants m and e." That is just what I have accomplished in a program called Source Theory,[54] which is in no way limited to quantum electrodynamics.

And so, if I were asked to respond to criticisms of the path I followed prior to the beginning of the sixth decade, I would answer:

"I don't do it that way anymore."

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DISCUSSION

E.C.G. SUDARSHAN.- Professor Schwinger's presentation must have left you speechless ! Is Professor Weisskopf here ? Professor Weisskopf you are the only person in the audience who was mentioned twice in Professor Schwinger's talk. Would you care to comment ?

V.F. WEISSKOPF.- Schwinger's talk has special significance. His approach differs from the one that is used by most theorists. I believe that the content and the results are the same, but he uses a very different terminology and a different way of reasoning. In some instances it brings out certain physical features of the theory that are hidden in the customary approach. I don't think that problems can be solved by his approach that cannot be solved by the ordinary one. But Schwinger's formulations are of great value just because they are so different. In poetry, art and music we value highly new ways of expressing the same contents. In theoretical physics there is not enough variety of presentation. Most of the theorists stick to, the generally employed ways of arguing and of calculating. This brings about too much uniformity although it helps to understand the papers of those authors. We must be grateful to Schwinger for showing us another way and we should devote more efforts to understand it. Perhaps the physical content is not so different but some of the problems of the orthodox approach appear in a new light. So let us rejoice that there is a Julian Schwinger who says it in an other tune !

E.C.G. SUDARSHAN.- You mentioned anticommuting numbers, and compensating fields. In super-symmetry, which employs anticommuting numbers and superfields there seems to be compensation of divergences and hence even simpler methods to deal into calculations of physical quantities. Could you care to comment on (1) compensations of divergences, (2) superfields and supersymmetry ?

J. SCHWINGER.- Well ! That's a very difficult question of course ! I should say that when super-symmetry came on on the scene I scratched my head and said can't I understand it simply, and I found a way of doing it that I thought to reduce super symmetry to a kinematical rather than a dynamical way of looking at things. I beat myself on the head for not having discovered it myself but basically of course one is saying if you like any angular momentum can be made out by a spin 1/2 which I once made a profession out of ; so I myself have understood supersymmetry as simply a kinematical relation, between or among various kinds of fields we all know that there is no sign of in the real world and I wonder if perhaps that is the way it lies because of course the fact that in simple supersymmetric models there are miraclous cancellation is most intriguing. But of course cancellations involves the question of attitude towards divergences, and I think towards the end of my lecture today I expressed the attitude that divergences are perhaps not fundamental and not necessarily a guiding post for developping the theory and they may be false leads. I'm not sure if that is the sort of answer you wanted.

E. WIGNER.- Does the present quantum electrodynamics and renormalization theory satisfy Einstein requirement that every physical theory should be simple and mathematically beautiful ?

J. SCHWINGER.- Thank you for a simple question. The trouble is that your question ambiguous ; for example in my own talk I traced the history of the development of renormalization theory and indicated that I myself no longer use it, that renormalization per se is not necessarly an integral part of quantum electrodynamics, that there are formulations in which it does not appeared, and I regard some formulations as intensely beautiful. Beautiful is of course in the eye of the creator, if you like.

E.C.G. SUDARSHAN.- I had spent two years as an assistant of Professor Schwinger and I had always wondered in later life how come I did not ask him more questions and now I see that it is not just a problem that I face but others too. Julian gives such brillant presentations, has brillant tasks accomplished that you don't dare ask any questions. I'm pleased to see that at least Professor Wigner was an exception to the rule ; and it remains for me my honour to thank Professor Schwinger for this brillant presentation of a vast and complex field, I'm not sure that I have understood his answer to the question about whether he is satisfied with the theory or not but nevertheless I am sure that you will all agree with me, that this is perhaps one of those presentations in which we could not add very much by asking him questions.