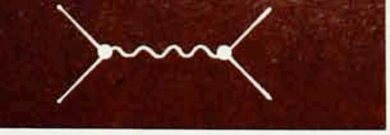

A PATH TO QUANTUM ELECTRODYNAMICS

A youthful fascination with electrodynamics drove Feynman through a succession of ideas until, with a prod by experiment, he reached an intuitive view of quantum electrodynamics.

Julian Schwinger

On 10 December 1965 three people shared a Nobel Prize "for their fundamental work in quantum electrodynamics." I am the sole survivor of that trio. Almost a decade ago I delivered a memorial lecture for Sin-itiro Tomonaga. Now I join with others in a tribute to Richard P. Feynman.

I have been asked to write on Feynman's contribution to the development of quantum electrodynamics. In the course of the past 40 years I have had several occasions to present the history of quantum electrodynamics, and these presentations naturally included accounts of Feynman's work. But all these articles were dominated by my point of view—they were in my voice. It is more fitting here that Feynman's voice be heard. And I believe we should have him speak not about technical details, but about motives, insights and lessons for the future. The many quotations from Feynman that follow come from the three sources listed at the end of the article. I have heavily favored the Nobel lecture,[1] not only for its extensive coverage, but in the belief that, in contrast with the other two books, it has not undergone editing, and therefore more truly projects the voice of Richard Feynman.

The challenge

I first met Feynman at Los Alamos, about a week after the Trinity test that ushered in the age of nuclear terror. No, I was not a member of the Manhattan Project, although I did spend a little time at the Metallurgical Laboratory in Chicago to see if I wanted to join up. I didn't. I was at Los Alamos on a purely cultural mission, from the MIT Radiation Laboratory, to give a few lectures about electromagnetic waveguides and electron accelerators. The talk on the latter topic included a discussion of synchrotron radiation.

One evening I ran into Feynman, looking rather glum (perhaps Robert R. Wilson had just said to him, "It's a terrible thing that we made"?[2]). He began to lament the loss of irreplaceable time to do physics, of which I was also keenly aware; we were both 27 years old then. He said something like, "I haven't done anything, but you've already got your name on something." I still wonder what he was referring to.

It wasn't true that he hadn't done anything. Already, as an undergraduate at MIT in the late 1930s, he had realized "that the fundamental problem of the day was that the quantum theory of electricity and magnetism was not completely satisfactory."[1] Concerning the books of Walter Heitler and Paul Dirac, for example, Feynman said[1]:

> I was inspired by the remarks in those books; not by the parts in which everything was proved and demonstrated [but by] the remarks about the fact that this doesn't make any sense, and the last sentence of the book of Dirac I can still remember, "It seems that some essentially new physical ideas are here needed." So I had this as a challenge and an inspiration. I also had a personal feeling, that since they didn't get a satisfactory answer to the problem I wanted to solve, I didn't have to pay a lot of attention to what they did do.

However, Feynman did gather from his reading that[1]

> two things were the source of the difficulties with quantum electrodynamical theories. The first was an infinite energy of interaction of the electron with itself. And this difficulty existed even in the classical theory. Well, it seemed to me quite evident that the idea that a particle acts on itself, that the electrical

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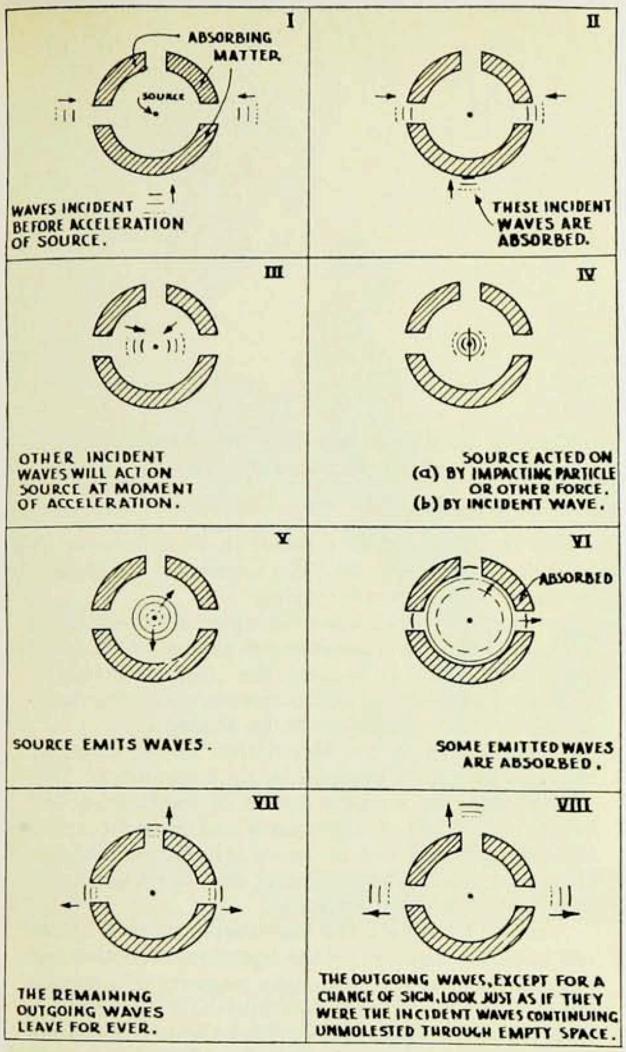

FIG. 1. Advanced effects in two examples of an incompletely absorbing system.

Diagrams that John Wheeler and Richard Feynman used to illustrate the constructive and destructive interactions between advanced and retarded fields in their time-symmetric electrodynamics. (From J. A. Wheeler, R. P. Feynman, *Rev. Mod. Phys.* **17**, 157, 1945.)

force acts on the same particle that generates it, is not a necessary one—it is a sort of silly one, as a matter of fact. And, so I suggested to myself, that electrons cannot act on themselves, they can only act on other electrons. That means there is no field at all.

Feynman was very happy with this. He saw it as solving what he then thought to be the second problem of quantum electrodynamics as well: the infinite vacuum energy associated with the infinite number of degrees of freedom of the electromagnetic field. No field, no infinite number of degrees of freedom. As Feynman said[1]:

> That was the beginning, and the idea seemed so obvious to me that I fell deeply in love with it. . . . I was held to this theory . . . by my youthful enthusiasm.
>
> Then I went to graduate school and somewhere along the line I learned what was wrong with the idea that an electron does not act on itself. When you accelerate an electron it radiates energy and you have to do extra work to account for that energy. The extra force against which this work is done is called the force of radiation resistance. The origin of this extra force was identified in those days . . . as the action of the electron on itself. The first term of this action . . . gave a kind of inertia. But that inertia-like term was infinite for a point charge. Yet the next term in the sequence gave an energy loss rate, which for a point-charge agrees exactly with the rate that you get by calculating how much energy is radiated. So, the force of radiation resistance, which is absolutely necessary for the conservation of energy would disappear if I said that a charge could not act on itself.
>
> So, I learned . . . the glaringly obvious fault of my own theory. But, I was still in love with the original theory, and was still thinking that with it lay the solution to the difficulties of quantum electrodynamics.

Feynman eventually took his problem to John Wheeler, for whom he was working as a research assistant during 1940–41. They came up with an answer that had two elements. The ordinary classical theory says that the motion of a charged particle at a certain time is influenced by the behavior of other charges at earlier times such that light can cover the relevant distance in the time available. Wheeler and Feynman changed this so-called retarded action-at-a-distance electrodynamics into one that is half retarded, half advanced. This, of course, seemed to wreak havoc with conventional ideas about causality. Nevertheless, it was equivalent to the retarded description and contained the radiative resistance force, provided one assumed that any emitted radiation was totally absorbed within the complete system of charges.

Wheeler and Feynman also found that, unlike the situation with retarded interactions, a theory that is symmetrical between retarded and advanced interactions permits an action principle description. That in itself was hardly new—Adriaan D. Fokker showed it in 1929, for example—although the suggestion that suitable boundary conditions could encompass the causal, dissipative situation of radiating, interacting charges certainly was.

About this success Feynman said[1]:

I was now convinced that since we had solved the problem of classical electrodynamics (and completely in accordance with my program from MIT, only direct interactions between particles, in a way that made fields unnecessary) that everything was definitely going to be all right. I was convinced that all I had to do was make a quantum theory analogous to the classical one and everything would be solved.

Wheeler urged Feynman to give a seminar on their classical theory, promising at the same time that he would work out the quantum theory version himself and give a later seminar on that. Feynman has described how his inaugural technical lecture attracted such luminaries as John von Neumann, Wolfgang Pauli and Albert Einstein, and he records Pauli's accurate prediction that Wheeler would never give the promised seminar on the quantum formulation.

Feynman would find his own path to quantum mechanics. But before we enter on it, we should note some other aspects of this classical odyssey, beginning with "suggestions for interesting modifications of electrodynamics."[1]

The part of the action that describes the interaction between charged particles contains a discontinuous function δ, which is zero except when the space–time locations of the charges are such that they can exchange light signals—that is, when each is on the other's light cone. So, Feynman and Wheeler figured, one might

replace this delta function . . . by another function, say, f, which is not infinitely sharp [but is] a narrow peaked thing, [and] all of the tests of electrodynamics that were available in Maxwell's time would be adequately satisfied. . . . You have no clue of precisely what function to put in for f, but it was an interesting possibility to keep in mind when developing quantum electrodynamics.

It also occurred to us that if we did that (replace δ by f) we could reinstate [the term referring to a single charge] . . . a finite action of a charge on itself. In fact, it was possible to prove that . . . the main effect of self-action . . . would be to produce a modification of the mass. [Indeed] all the mechanical mass could be electromagnetic self-action.

Looking back at this part of his voyage, Feynman said[1]:

I would also like to emphasize that by this time I was becoming used to a physical point of view different from the more customary [view, in which] things are discussed as a function of time in very great detail. For example, you have the field at this moment, a differential equation gives you the field at the next moment and so on; a method which I shall call the Hamiltonian method. . . . We have, instead (in [the action]) a thing that describes the character of the path throughout all of space and time.

Dirac's groundwork

In 1933 Dirac published a paper in *Physikalische Zeitschrift der Sowjetunion* on "The Lagrangian in Quantum Mechanics." He begins by saying:

Quantum mechanics was built up on a foundation of analogy with the Hamiltonian theory of classical mechanics. This is because the classical notion of canonical coordinates and momenta was found to be one with a very simple quantum analogue. . . .

Now there is an alternative formulation for classical dynamics, provided by the Lagrangian. This requires one to work in terms of coordinates and velocities instead of coordinates and momenta. The two formulations are, of course, closely related, but there are reasons for believing that the Lagrangian one is the more fundamental.

In the first place the Lagrangian method allows one to collect together all the equations of motion and express them as the stationary property of a certain action function. (This action function is just the time integral of the Lagrangian.) There is no corresponding action principle in terms of the coordinates and momenta of the Hamiltonian theory. [This is not true, but it doesn't matter.] Secondly the Lagrangian method can easily be expressed relativistically, on account of the action function being a relativistic invariant; while the Hamiltonian method is essentially nonrelativistic in form, since it marks out a particular time variable. . . .

For these reasons it would seem desirable to take up the question of what corresponds in the quantum theory to the Lagrangian method of the classical theory.

From the earliest days of nonrelativistic wave mechanics it had been recognized that the expression of a wavefunction as $\exp[(i/\hbar)W]$, with W expanded in powers of $\hbar$, gave the semiclassical approximation, in which the leading term of W, the one independent of $\hbar$, is the classical action. The next term, imaginary and proportional to $\hbar$, can then be found by integration over known classical quantities. Furthermore, for a free particle these two terms suffice to give the exact answer. That is, they completely determine, respectively, the real phase and real amplitude of the free-particle wavefunction. (It might be added that a particular form of the free-particle wavefunction was well known through the evident analogy between the Schrödinger equation and the heat conduction or diffusion equations.)

Dirac considered the wavefunction that relates a coordinate eigenvalue state at one time, say t_1, to any analogous state at another time, say t_2. The totality of

Paul A. M. Dirac and Feynman in conversation during the International Conference on Relativistic Theories of Gravitation held in Warsaw, Poland, on 25–31 July 1962. A 1933 paper by Dirac gave Feynman the key to developing a quantum version of the classical theory of electrodynamics he had worked out with Wheeler.

such wavefunctions for fixed times t_1 and t_2 constitutes the time transformation function that connects the descriptions of the physical system at the two times. As the inventor of quantum transformation theory, Dirac knew, and stated explicitly, that the transformation function in question could be constructed from a sequence of transformation functions relating states at times intermediate between t_1 and t_2. In the limit where these successive intermediate times differ infinitesimally, the transformation function appears as an infinity of independent integrals extended over all coordinate values, each integral being labeled by a value of the time between t_1 and t_2. The integrand is the product of all the transformation functions associated with the successive infinitesimal increments of time.

And what is the transformation function associated with the infinitesimal displacement from time t to time $t + \mathrm{d}t$? Dirac says it *corresponds* to $\exp[(\mathrm{i}/\hbar)\,\mathrm{d}t\,L]$, where "we ought to consider the classical Lagrangian, not as a function of the coordinates and velocities, but rather as a function of the coordinates at time t and the coordinates at time $t + \mathrm{d}t$." Then the integrand is $\exp[(\mathrm{i}/\hbar)W]$, where

$$W = \int_{t_2}^{t_1} \mathrm{d}t\, L$$

This integral, which Dirac denotes by F, is the sum over all the individual coordinate-dependent terms that refer to the successive values of t.

Now, we know, and Dirac surely knew, that to within a constant factor the "correspondence," for infinitesimal $\mathrm{d}t$, is an equality when we deal with a system of nonrelativistic particles possessing a coordinate-dependent potential energy V. (Relative to noninteracting particles, the presence of V only supplies an additional phase factor, that which is conveyed by the contribution $-V$ in L.) Why, then, did Dirac not make a more precise, if less general, statement? Because he was interested only in a general question: What, in quantum mechanics, corresponds to the classical principle of stationary action?

Dirac answered his fundamental question with the aid of the formal device that represents the classical limit as the limit $\hbar \to 0$. Evidently in that limit $\exp[(\mathrm{i}/\hbar)W]$ will in general have an infinitely oscillatory dependence on any of its myriad of variables. Thus the multiple-integral construction of the transformation function "contains the quantum analogue of the action principle, [because] the importance of our considering any set of values for the intermediate [coordinates] is determined by the importance of this set of values in the integration. If we now make $\hbar$ tend to zero, this statement goes over into the classical statement that . . . the importance of our considering any set of values for the intermediate [coordinates] is zero unless these values make the action function stationary."

Path integral formulation

Why, in the decade that followed, didn't someone pick up the computational possibilities offered by this integral approach to the time transformation function? To answer this question bluntly, perhaps no one needed it—until Feynman came along. He has described how, at a Princeton beer party, he was accosted by Herbert Jehle, newly arrived from Europe, who wanted to know what Feynman was working on. After telling Jehle about his struggles with electrodynamics, Feynman turned to Jehle and asked, "Listen, do you know any way of doing quantum mechanics starting with action?"[1] As it happened, Jehle was aware of Dirac's early paper, and so Feynman found what he wanted, a formulation of quantum mechanics that could be applied to his classical action-at-a-distance electrodynamics—if one took for granted that Dirac's construction still worked when a Lagrangian did not exist. Feynman called this approach to quantum mechanics the path integral formulation because a value of the action W is assigned to any sequence of intermediate coordinate values—to any path between the initial and the final coordinates—and all such values of $\exp[(i/\hbar)W]$ are added together.

It didn't take Feynman long to discover that[1]

> I could not get the thing to work with the relativistic case of spin one-half. However, although I could deal with the matter [electrons] only nonrelativistically, I could deal with the light or the photon interactions perfectly well. . . .
>
> It was also possible to discover what the old concepts of energy and momentum would mean with this generalized action. And, so I believed that I had a quantum theory of classical electrodynamics—or rather of this new classical electrodynamics described by [the half-retarded, half-advanced action]. . . .
>
> It was also easy to guess how to modify the electrodynamics, if anybody ever wanted to modify it. I just changed the delta to an f, just as I would for the classical case. So, it was very easy, a simple thing. . . . Yet, as I worked out many of these things and studied different forms and different boundary conditions, I got a kind of funny feeling that things weren't exactly right. I could not clearly identify the difficulty and in one of the short periods during which I imagined I had laid it to rest, I published a thesis and received my PhD.

Feynman became involved with the Manhattan Project at an early stage. He had been recruited by Wilson to work on a method for separating isotopes of uranium, which, as it turned out, was never used. He was one of the first to arrive when the Los Alamos Laboratory began in 1943. About the war years and his preoccupation with quantum electrodynamics, Feynman said[1]:

> During the war, I didn't have time to work on these things very extensively, but wandered about on buses and so forth, with little pieces of paper, and struggled to work on it and discovered indeed that there was something wrong, something terribly wrong. I found that if one generalized the action from the nice Lagrangian forms [that is, from the time integral of the Lagrangian to the action of action-at-a-distance electrodynamics] then the quantities which I defined as energy, and so on, would be complex. The energy values of stationary states wouldn't be real and probabilities of events . . . would not add up to one.

Feynman summarized the position he was in prior to the Shelter Island conference of June 1947 as follows[1]:

> I would say, I had much experience with quantum electrodynamics, at least in the knowledge of many different ways of formulating it, in terms of path integrals of actions and in other forms. One of the important by-products, for example, of much experience in these simple forms, was that it was easy to see how to combine together what was in those days called the longitudinal and transverse fields, and in general to see clearly the relativistic invariance of the theory. [But] I never used all that machinery which I had cooked up to solve a single relativistic problem. I hadn't even calculated the self-energy of an electron up to that moment, and was studying the difficulties with the conservation of probability, and so on, without actually doing anything, except discussing the general properties of the theory.

Experimental input

The Lamb-shift measurement, and Hans Bethe's nonrelativistic calculation that accounted for a major portion of it, spotlighted the need for an effective relativistic quantum electrodynamics. Bethe had suggested that a theory giving finite results, even if it violated some physical principle, would be useful in identifying the physical quantities of interest. Feynman was sure he knew how to do that, until he tried it and, as he said,[1]

> finally realized that I had to learn how to make a calculation. So, ultimately, I taught myself how to calculate the self-energy of an electron. [Then] I simply followed the program outlined by Professor Bethe and showed how to calculate all the various things, the scattering of electrons from atoms without radiation, the shift of levels and so forth, calculating everything in terms of the experimental mass. . . .
>
> The rest of my work was simply to improve the techniques then available for calculations, making diagrams to help analyze perturbation theory quicker. Most of this was first worked out by guessing—you see, I didn't have the relativistic theory of matter. For example, it seemed to me obvious that the velocities in nonrelativistic formulas have to be replaced by Dirac [matrices]. I just took my guess from the forms that I worked out using path integrals for nonrelativistic matter, but relativistic light. It was easy to develop rules. In addition, I included diagrams . . . improved notations . . . worked out easy ways to evaluate integrals . . . and made a kind of handbook on how to do quantum electrodynamics.
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> But one step of importance . . . involved . . . the negative energy sea of Dirac, which caused me so much logical difficulty. [Here Feynman recalls a suggestion by Wheeler that a positron is an electron going backward in time.] Therefore, in the time-

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Shelter Island conference participants, June 1947. From left to right: I. I. Rabi, Linus Pauling, John Van Vleck, Willis Lamb, Gregory Breit, Duncan MacInnes, Karl Darrow, George Uhlenbeck, Julian Schwinger, Edward Teller, Bruno Rossi, Arnold Nordsieck, John von Neumann, John Wheeler, Hans Bethe, Robert Serber, Robert Marshak, Abraham Pais, J. Robert Oppenheimer, David Bohm, Feynman, Victor Weisskopf, Herman Feshbach.

dependent perturbation theory that was usual for getting self-energy, I simply supposed that for a while we could go backward in time. [The extra terms thus produced] were the same as the terms that other people got when they did the problem . . . using holes in the sea, except, possibly, for some signs. These, I, at first, determined empirically by inventing and trying some rules.

I have tried to explain that all the improvements of relativistic theory were at first more or less straightforward, semi-empirical shenanigans. Each time I would discover something, however, I would go back and I would check it so many ways . . . until I was absolutely convinced of the truth of the various rules and regulations which I concocted to simplify all the work. . . .

At this stage, I was urged to publish this because everybody said it looks like an easy way to make calculations, and wanted to know how to do it. I had to publish it, missing two things; one was a proof of every statement in a mathematically conventional sense. Often, even in a physicist's sense, I did not have a demonstration of how to get all of these rules and equations, from conventional electrodynamics. But, I did know from experience, from fooling around, that everything was, in fact, equivalent to the regular electrodynamics and had partial proofs of many pieces, although, I never really sat down, like Euclid did for the geometers of Greece, and made sure that you could get it all from a single simple set of axioms. As a result, the work was criticized, I don't know whether favorably or unfavorably, and the "method" was called the "intuitive method." For those who do not realize it, however, I should like to emphasize that there is a lot of work involved in using this "intuitive method" successfully. Because no simple clear proof of the formula or idea presents itself, it is necessary to do an unusually great amount of checking and rechecking for consistency and correctness in terms of what is known. . . . In the face of the lack of direct mathematical demonstration . . . one should make a perpetual attempt to demonstrate as much of the formula as possible. Nevertheless, a very great deal more truth can become known than can be proven. . . .

This brings me to the second thing that was missing when I published the paper, an unresolved difficulty. With δ replaced by f the calculations would give results . . . for which the sum of the probabilities of all alternatives was not unity. . . . I believe there is really no satisfactory quantum electrodynamics, but I'm not sure.

Therefore I think that the renormalization theory is simply a way to sweep the difficulties of the divergences of electrodynamics under the rug. I am, of course, not sure of that.

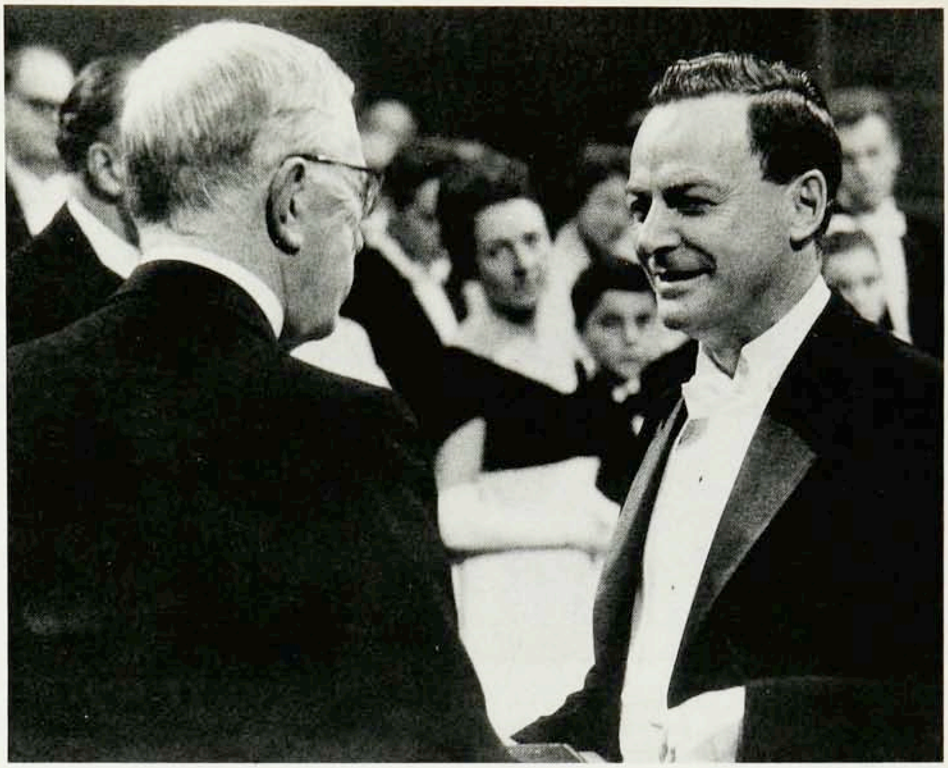
Some 20 years later Feynman had not really changed his mind, writing that[3]

> The shell game that we play [is] called "renormalization." But no matter how clever the word, it is what I would call a dippy process! Having to resort to such hocus-pocus has prevented us from proving that the theory of quantum electrodynamics is mathematically self-consistent. It's surprising that the theory still hasn't been proved self-consistent one way or the other by now; I suspect that renormalization is not mathematically legitimate. What *is* certain is that we do not have a good mathematical way to describe the theory of quantum electrodynamics.

Value of physical reasoning

Feynman's account begins to wind down as he says[1]:

> This completes the story of the development of the space–time view of quantum electrodynamics. I wonder if anything can be learned from it. I doubt it.



Receiving the Nobel Prize from King Gustav VI Adolf of Sweden, 10 December 1965. (Wide World photo; courtesy of California Institute of Technology.)

It is most striking that most of the ideas developed in the course of this research were not ultimately used in the final result. For example, the half-advanced and half-retarded potential was not finally used, the action expression [for action at a distance] was not used, the idea that charges do not act on themselves was abandoned. The path integral formulation of quantum mechanics was useful for guessing at final expressions and at formulating the general theory of electrodynamics in new ways—although, strictly, it was not absolutely necessary. The same goes for the idea of the positron being a backward moving electron; it was very convenient, but not strictly necessary. . . .

We are struck by the very large number of different physical viewpoints and widely different mathematical formulations that are all equivalent to one another. The method used here, of reasoning in physical terms, therefore, appears to be extremely inefficient. On looking back over the work, I can only feel a kind of regret for the enormous amount of physical reasoning and mathematical re-expression which ends by merely re-expressing what was previously known, although in a form which is much more efficient for the calculation of specific problems. Would it not have been much easier to simply work entirely in the mathematical framework to elaborate a more efficient expression? This would certainly seem to be the case, but it must be remarked that although the problem actually solved was only such a reformulation, the problem originally tackled was the (possibly still unsolved) problem of avoidance of the infinities of the usual theory. Therefore, a new theory was sought, not just a modification of the old. Although the quest was unsuccessful, we should look at the question of the value of physical ideas in developing a *new* theory. . . .

I think the problem is not to find the *best* or most efficient method to proceed to a discovery, but to find any method at all. Physical reasoning does help some people to generate suggestions as to how the unknown may be related to the known. Theories of the known, which are described by different physical ideas may be equivalent in all their predictions and are hence scientifically indistinguishable. However, they are not psychologically identical when trying to move from that base into the unknown. For different views suggest different kinds of modifications which might be made. . . . I, therefore, think that a good theoretical physicist today might find it useful to have a wide range of physical viewpoints and mathematical expressions of the same theory . . . available to him. This may be asking too much of one man. Then new students should as a class have this. If every individual student follows the same current fashion in expressing and thinking about [the generally understood areas], then the variety of hypotheses being generated to understand [the still open problems] is limited. Perhaps rightly so, for possibly the chance is high that the truth lies in the fashionable direction. But [if] it is in another direction . . . who will find it?

So spoke an honest man, the outstanding intuitionist of our age and a prime example of what may lie in store for anyone who dares to follow the beat of a different drum.

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