

We did not do too much about it until shortly before the conference. We then proposed to split an infinite mass term from other terms and get a finite term shift, just as I demonstrated it at the conference. Isn't it exactly what you are doing? Your great and everlasting deed is your bright idea to treat this at first nonrelativistically.^{9†}

Bethe's work stimulated Bruce French and Weisskopf at MIT, Lamb and Kroll at Columbia, and Julian Schwinger—as soon as his honeymoon was over (he got married right after the Shelter Island Conference, and spent two months traveling throughout the United States)—to start hole-theoretic calculations of the Lamb shift in Dirac's radiation theory. But they used noncovariant procedures for calculation, which had been described in the second edition of Heitler's book on the quantum theory of radiation.¹¹ The relativistically invariant approach to calculate the quantum electrodynamical effects was hardly needed.

12.2 'I can do that for you!'

The unfolding of the story of the Shelter Island Conference, and what happened soon thereafter, is a fascinating chapter in the historical develop-

† In an interview with the author in May 1988, Weisskopf made the following remarks concerning Bethe's nonrelativistic calculation of the Lamb shift: 'When he [Hans Bethe] sent me this note [Bethe's draft of his calculation], I was actually really unhappy. First of all, he could have told me [that he was going to do this calculation]. I was interested in the Lamb shift problem even before the war; at that time it was called the Pasternack effect. At the Ann Arbor [University of Michigan] summer school in 1940 I had a lot of conversations with Kramers, with whom I was very close since the old Copenhagen days. He believed, as did I, that the Pasternack effect was real and he asked me to calculate it. He first brought to me the idea that true enough the self-energy is infinite, but maybe the self-energy difference between a bound and a free electron can be calculated and will be finite, and that [later on, in 1947] should be the Lamb shift. From then on I was sort of living with this problem. During the war I became occupied with other problems [at the Manhattan Project], and the Pasternack problem was put on the back burner. But, after the war, I again wanted to take it up and I definitely knew about the problem when I came to MIT [from Los Alamos after the war]. Then came the Lamb shift, Lamb's observation that Pasternack was right and one even had quantitative results.

'Schwinger and I went together on the train to New York [to attend the Shelter Island Conference], and we discussed this problem; we arrived at the conclusion that the nonrelativistic part could be calculated with matrix elements. Then I talked a lot with Hans [Bethe] about where the difficulty lies and that the nonrelativistic part is not so difficult; the difficulty lies in the relativistic region, but I did not know how to do that.

'So when he sent me that note [Bethe's preliminary calculation], because first of all he could have told me about it, and in some ways my name should have been on that paper. Personally I think that he should have asked me to publish this note together with him.

'I could actually have made the calculation myself of what then was the Pasternack effect, already in the early forties. And when Lamb measured the shift accurately, I should have won the Nobel Prize.'¹⁰

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Already at the Shelter Island Conference, Feynman had tried to make use of
Kramers's suggestion about the electromagnetic origin of the Lamb shift. He
tried to estimate how much his damped oscillator shifted in its frequency, but
he didn't understand the real problem.¹²

Right after his calculations during the train-ride from New York to
Schenectady, and their completion there, Bethe telephoned excitedly from
Schenectady to Feynman, who, at that moment was visiting Bethe's house. As
Feynman recalled: 'He said to me that he understood the Lamb shift, that he
had calculated it, and he explained the idea about mass renormalization to me.
I don't remember the details, because I didn't follow it very well. And he said
that he got about 1000 megacycles for the shift; he was very excited and wanted
to talk about it. Although I didn't understand it too well, but I realized it from
his excitement that it was something very important.'¹²

Upon returning to Ithaca early in July, Bethe gave a lecture, in which he
explained in detail his calculation of the Lamb shift. Bethe stressed the point
that the self-energy of the free electron diverges linearly. However, if you take
the difference of the self-energies of the bound electron and the free electron,
calculated nonrelativistically, a logarithmically divergent expression is
obtained. The upper limit of the integration is to be taken to be mc^2 instead of
infinity, and Bethe obtained a level shift of about 1000 megacycles per second.
So he knew that he was on the right track. The only problem that remained
was to deal with the relativistic case precisely; exactly what do you do with the
upper limit, not just cut it off arbitrarily if you get the right estimate.†

'So it was a relativistic problem. In this lecture Bethe said that you have to
make so many subtractions of such large terms, really infinite terms, that it's
very confusing at times, exactly what to subtract from what. And he thought
that if there were any way whatever to make the theory finite, even though it
didn't agree with experiment, some artificial way of cutting off electrodyna-
mics which was relativistically invariant, then we could cut off all these things
which were infinite, you could subtract them exactly, and it would be very
much simpler, and then you could do the relativistic end without ambiguity.
Otherwise it was very confusing.'^{12,14}

After Bethe's lecture, Feynman went up to him and told him, "I can do that
for you! I'll bring it in for you tomorrow." I knew every way to modify
quantum electrodynamics known to man, at that time.¹¹ 'I had done
electrodynamics now with path integrals. I turned it upside-down, turned it in
and out. It was easy for me to handle. I could change a delta-function [see
equation (5.15)] to a sharp function instead of a delta-function and take a limit

† Kroll and Lamb pointed out that if the effect of retardation were taken into
account then the upper limit in Bethe's integration would have become $2mc^2$
instead of the arbitrary value of mc^2 which he had taken.¹³

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later. There was no problem. I had complete freedom to structure it. If you were to try to change a delta-function to a Hamiltonian form, you could be in a hell of a lot of trouble because you would have to define how to come out from the differential equations for the different functions and keep the relativistic invariance and to me, by this time, nothing was difficult. I could do electrodynamics in any way I wanted. So I told Bethe that I could that.

'So I went home and, believe it or not, this shows you how stupid a man can be: because, for the first time I applied the path integrals to electrodynamics in the *conventional representations* instead of half-advanced and half-retarded scheme—just plain, ordinary, common usage of electrodynamics. I probably had written it a few times, but I had never tried to do anything with it. So, I took the normal electrodynamics, modified it, and found a way to translate what I saw into conventional description and that was effectively that you subtract the relation with the frequency k from a higher mass and integrate it over that mass. That was the idea of the convergence scheme. So I saw the convergence scheme, but now what was surprising was that I had never done any real problem—like calculating the self-energy, vacuum polarization, or the energy level shift, or anything.

'The next day I went to Bethe and told him: "Tell me how to compute the self-energy of the electron and I'll show you how to correct it, so you'll get a finite answer." I didn't know how people computed the self-energy of an electron, which was quite stupid of me; it's simply a second-order perturbation. I had gone too far on my own, but I had not looked at simple problems. So, Bethe showed me how to calculate the self-energy of the electron, and we tried to work it out. I told him the rule, and he found that the divergence went to the sixth power, instead of converging at all, which was much worse.

'So, having failed miserably, I went home and thought about it, and I couldn't figure out what was wrong, why it didn't converge. We didn't know what we did wrong, but when we went over it again, following directly the rules which I was proposing, it converged! What we had done before, I didn't know, but in the meantime I had to learn how to do it myself. So I learned how to calculate the self-energies, energy level differences, and the whole business, during that period.

'I learned how to do conventional quantum electrodynamics, still working from my path integrals. Thus I was trying to connect my path integrals with the conventional language, and saw what the perturbation theory was from the point of view of path integrals. I noticed lots of things, including the fact that several things in perturbation theory, like Coulomb correction and the transverse wave correction, were just one correction—the exchange of a photon. They could be represented by summing over the four directions of polarization. It was obvious from the path integrals that I would do that, and (other people) wouldn't understand me, but they would check and it would always be right. I thought that they must know that if they take the regular

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Dirac theory, instead of using the conventional directions, it takes care of the directions you don't know. And I discovered a simpler calculation.

'As far as I was concerned, the conventional perturbation theory, seeing what they were doing. The reason why they were still using the conventional invariant. Everything I was doing was formulated everything, they were using the transverse waves [Section 11]. They say that the divergence of the action for a system. So they had done even what I was trying to answer for a physical problem. The calculation of scattering, is simple, but it was a mess of terms which all added together. My calculation was now simplified whereas theirs was not. I kept the relativistic invariance. I kept the gauge invariance together, and how to generalize to higher dimensions. It was obvious; *always work*. I thought I was going to do what they had done and I was going to see somebody, they would be so surprised they would check and say, 'Yes, you were already had a powerful instrument. It was like an airplane instead of having

12.3 The genesis of quantum electrodynamics:

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Feynman's idea concerning the action-at-a-distance theory was proposed by Bethe in the action-at-a-distance theory. The particle action S_p and the action for the charged particles:

$$S = S_p + S_{int} = \sum_a m_a \int ds$$

where a_μ represents, for $\mu = 1, 2, 3, 4$ the time coordinate of the particle. $(a_\mu - b_\mu)$ is the four-vector between a and b , and $\delta(s^2)$ is Dirac's delta-function.

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Dirac theory, instead of using transverse waves and summing over four directions, it takes care of the Coulomb correction, but apparently they did not know. And I discovered great simplifications in the methods of calculation.

'As far as I was concerned, I was just taking path-integrals and avoiding the perturbation theory, seeing what the terms were. They were all much simpler. The reason why they were simpler is quite clear: they were all relativistically invariant. Everything I was computing was covariant. The way others had formulated everything, they had separated the Coulomb potential and the transverse waves [Section 11.2]. That depends on the coordinate system. If you say that the divergence of A is equal to zero, it depends on the coordinate system. So they had done everything noncovariantly and, of course, the final answer for a physical problem like the scattering of two electrons, Bhabha scattering, is simple, but it was the result of a rather complicated bunch of terms which all added together, and a whole lot of junk that was complicated was now simplified whereas when I started with my path integrals, I could see the relativistic invariance. I knew which terms went together, how they went together, and how to generalize to four dimensions from the two transverse dimensions. It was obvious; it would work; that was the fun of it. *It would always work.* I thought I was trying to learn how others did it, but I would try to do what they had done and I'd get their answers; but when I would talk to somebody, they would be so shocked that it was the right answer, and they would check and say, 'Yes, it is the right answer.' I began to realize that I already had a powerful instrument; that I was sort of flying over the ground in an airplane instead of having so many terms.'¹⁵

12.3 The genesis of Feynman's approach to quantum electrodynamics: between the Shelter Island and Pocono conferences

Feynman's idea concerning how to modify classical electrodynamics in the direction proposed by Bethe was very simple. He started from Fokker's action in the action-at-a-distance theory (cf. equation (5.15), which is a sum of the free particle action S_p and the action S_{int}), which describes the interaction between the charged particles:

$$S = S_p + S_{int} = \sum_a m_a \int (da_\mu da_\mu)^{1/2} + \frac{1}{2} \sum_{a,b} e_a e_b \iint \delta(s_{ab}^2) da_\mu db_\mu, \quad (12.5)$$

where a_μ represents, for $\mu = 1, 2, 3$, the three space coordinates, and, for $\mu = 4$, the time coordinate of the particle a of mass m_a and charge e_a ; $s_{ab}^2 = (a_\mu - b_\mu)(a_\mu - b_\mu)$ is the four-dimensional distance between the particles a and b , and $\delta(s^2)$ is Dirac's delta-function, which grows to infinity if $s = 0$. The prime