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ELECTRODYNAMICS TODAY **

E. T. Jaynes

Arthur Holly Compton Laboratory of Physics

Washington University, St. Louis, Missouri 63130

CONTENTS

Abstract. The question whether Quantum Optics can provide any new evidence that might clear up the then outstanding difficulties in QED was raised at the 1966 Rochester Coherence Conference, in the author's talk entitled, "Is QED Necessary?" This was done in a deliberately provocative way because of a complacency in the air that tended to regard Quantum Optics as nothing more than an application of completely established theory. If this field has no long-range goals of its own relating to fundamental physics, it must inevitably degenerate into a kind of Engineering Service Facility, whose main function is to provide bigger and better lasers for use in other fields.

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It was clear immediately that still more provocation would be needed if any serious attention was to be turned to the great range of possibilities for new fundamental tests of quantum theory, that were opening up. Accordingly, some old ideas were revived under the name "Neoclassical Theory" (NCT) whose purpose was to demonstrate the existence of many areas where alternative theories make predictions different from those of QED, on which we had then no experimental evidence, but in which such evidence could be obtained by Quantum Optics techniques.

The second provocation succeeded better than I had expected--or indeed wished--and commentary on NCT became a minor industry. Partly as a result, but equally from independently motivated research, our understanding of Electrodynamics is today very different from what it was in 1966. The question posed then was: "At what point, if any, is it necessary to quantize the radiation field in order to calculate the correct experimental numbers?" Today, that question is only partially answered, but in a surprising way; it appears that both the defenders and critics of QED were about half right and half wrong, while the truth lay in a direction that no one foresaw. In the following we give a brief review of the rather dramatic changes in our understanding of Electrodynamics--insofar as it pertains to optics--since the 1966 Rochester Coherence Conference.

1. BACKGROUND

"Für den Rest meines Lebens will Ich darüber nachdenken,
was das Licht ist."

--- Albert Einstein[1]

In 1966, QED was in one of its recurrent states of pessimism, just as it had been twenty years earlier. In 1946, the teacher from whom I first learned about it--J. R. Oppenheimer--described it publicly as "a monumental flop." But starting just at that time, the work of Tomonaga, Schwinger, Feynman, Dyson and others quickly restored the patient to vigorous health. These developments were pretty well consolidated by 1953 with the successful treatment of the Lamb shift and the anomalous moment, and optimism again ran high, with the belief that QED had been vindicated and could now advance to many new applications and elegant formulations.

However, the next decade saw very little of that advance. Wigner,[2] Schwinger,[3] Feynman,[4] and Weisskopf[5] expressed dissatisfaction with the theory on grounds of logical consistency and lack of conceptual clarity. On the mathematical side, far from advancing to new applications, we became aware of more and more difficulties, whose discussion left room for fewer and fewer real applications in textbooks.

Perhaps the low point came just before 1966. Arthur Wightman,[6] in his 1964 Cargèse lectures, said that the problem, not of solving, but only of proving the existence of solutions for the standard models had "conspicuously and completely defeated two generations of theoretical physicists." To appreciate the kind of difficulty that made the future seem bleak, see Wightman's discussion of strange representations and

Haag's theorem. Some had suggested that the whole apparatus of fields and Hamiltonians ought to be abandoned in favor of the S-matrix. Dirac,[7] in his Belfer lectures of 1963-64, described the usual treatment of quantum field theory as "a stopgap, without any lasting future." So we were back just about to Oppenheimer's remark.

Now as I like to put it, any modern physical theory is a rather complicated blend, containing important elements of truth, but all scrambled up, inevitably, with some elements of nonsense. Each major advance in understanding comes when we accomplish one more step in disentangling them. In 1966, then, we faced a seemingly desperate problem of separating the truth from the nonsense. What parts of QED are really required by experimental facts, what parts might be modified?

Undoubtedly, the most sacred part of QED was field quantization itself; indeed, since the 1927 work of Dirac[8] that first used field quantization and derived the Einstein A-coefficients, QED was in the minds of most physicists <u>defined</u> as the theory which starts out by quantizing the EM field. And there was a weight of authority supporting the belief that field quantization and the resulting vacuum fluctuations were the essential physical cause of the Lamb shift and the anomalous moment. Schwinger[9] and Weisskopf[5] had stated this very explicitly. Dyson,[10] in concurring, pictured the quantized field as something akin to hydrodynamic flow with superposed random turbulence, and said, "The Lamb-Retherford experiment is the strongest evidence we have for believing that our picture of the quantum field is correct in detail."

Furthermore, we had Welton's[11] elementary derivation of the Lamb shift directly from field fluctuations; and as if to emphasize the point, on the day the 1966 Conference opened there arrived in the mail the paper of E. A. Power[12] which derived the Lamb shift directly from the change of zero-point energy in the fields surrounding a hydrogen atom in its 2S state. At the 1966 Conference, Roy Glauber told us that vacuum fluctuations are "very real things."

Yet my own thinking had led me to doubt whether vacuum fluctuations are the real cause of the Lamb shift; or indeed whether they could be said to be "real" at all, compared to the unquestioned reality of the thermal fluctuations that we observe as Nyquist noise in electrical circuits or Planck black-body radiation. It seems to me that, if you say radiation is "real," you ought to mean by that, that it can be detected by a real detector. But an optical pyrometer sees only the Planck term, and not the zero-point term, in black-body radiation. And, if the Einstein A-coefficients arise physically from zero-point fluctuations, then why is it that the derivation of the black-body radiation density from the A-coefficients gives only the Planck term; and not the zero-point term? Some further facts about the fantastic numerical values of the zero-point energy density and the resulting turbulent power flow in space ($\sim 10^{20}$ megawatts/cm^2) required by the cutoff at the Compton wavelength used in Welton's and Bethe's[13] calculations, were noted in my paper at the last conference.[14]

Of course, a staunch defender of present theory will say immediately that such objections reflect only naive metaphysical preconceptions of "reality," not unlike pre-relativistic notions of absolute simultaneity, of just the kind that the Copenhagen interpretation of quantum theory

has recognized, and rightly removed from science. To this I can only reply with what Heinrich Hertz[15] said on a similar occasion: "A doubt which makes an impression on our mind cannot be removed by calling it metaphysical." It is a supple ontology which supposes that vacuum fluctuations are just real enough to shift the hydrogen 2s level by 4 microvolts; but not real enough to be seen by our eyes, although in the optical band they correspond to a flux of over 100 kilowatts/cm^2. Nevertheless, the dark-adapted eye, looking for example at a faint star, can see <u>real</u> radiation of the order of 10^{-15} watts/cm^2.

Another piece of evidence strengthened these doubts. The first volume of Bjorken and Drell was just out, with its development of the Feynman rules of calculation and the usual first applications (Compton effect, Bethe-Heitler formula, Lamb shift, anomalous moment, vacuum polarization). But how many readers were surprised to note that this volume contains no mention at all of EM field quantization! Mathematically, the propagator $D_F(x-y)$ is equally well a Green's function for the classical Maxwell equations, and its role as the elementary response function is the same whether the EM field is or is not quantized.

But while the theoretical picture was bleak, the experimental picture had never been brighter, with the creation of new optical technology beyond the dreams of a few years before. Any physicist worthy of the name must have an interest in fundamental questions; but to raise and pursue them actively when we see no way to settle them is not a profitable occupation and may be left to philosophers (who, as a colleague of mine remarked, "are free to do whatever they

please, because they don't have to do anything right"). But when we do see the means by which deep fundamental issues can be removed from the realm of philosophical debate and settled on the level of demonstrable fact, then we ought to retrieve them from the philosophers and see what we can learn about them.

Now from a pragmatic standpoint, our present quantum theory of electrons has been an unqualified success, yielding thousands of quantitatively correct predictions from straightforward, relatively easy calculations. It was in the extension of that theory to include radiation phenomena that we got into a seemingly endless series of difficulties. Yet probably 95% of the clues that led to present quantum theory were provided by optical experiments performed in the period 1880-1925. It would be astonishing if all this new optical capability could not provide any new fundamental tests of the theory which grew out of the original crude optical experiments. But I could see no sign of recognition of this; the prevailing opinion was that QED had in it the complete and final answer to all questions in the optical region, and the remaining difficulties could be resolved only by more evidence from high-energy experiments. This made it surprising that so many physicists took up Quantum Optics; for I personally would not choose to work in this field if I believed there was no new fundamental knowledge to be had from it.

Since provocation is much more effective than exhortation, I gave a talk at the 1966 Rochester Coherence Conference entitled, "Is QED Necessary?" which marshalled as many arguments against QED as possible, noted that semiclassical theory has far more truth in it than was generally recognized, and suggested that optical experiments just then

feasible might provide important new evidence about the range of validity of QED, as well as that of semiclassical theory.

However, it seems that those remarks succeeded only in provoking Peter Franken, although he could hardly be classed as a defender of QED (see the Conference report[16] of a year earlier, which has him "postulating that quantum mechanics applies only to the matter and not to the light"). The result was a bet, recorded by D. L. MacAdam.[17] The key issue, on which my statements contrasted most strongly with the prevailing view (the above quotation from Dyson) was the Lamb shift, as Franken correctly saw. The utter certainty with which the defenders of QED believed that this was a direct proof of the reality of vacuum fluctuations arising from field quantization, made this the favored ground on which to challenge my own speculation that the effect would be found already in semiclassical theory, if we complete it by adding terms giving the effect of the atom on the field, which had been left out in the first semiclassical theories.

The issue was then whether the Lamb shift could be calculated using any commonly accepted formalism (i.e., nonrelativistic Schrödinger equation, Pauli equation, Dirac equation) for the electron, but without quantizing the electromagnetic field.

2. RADIATION REACTION AND SOURCE-FIELD THEORY

Soon after the 1966 meeting, my students and I had realized that the usual modal expansions of the EM field, although correct in principle, were complicated and tended to obscure the physics if one wants only the field in the immediate vicinity of an oscillating charge distribution.

For the sum of all mode contributions is there just the radiation reaction field. Usually one sees only the term $(2e^2/3c^3)\dddot{x}$, found by Lorentz, which is independent of the exact charge distribution and gives rise to radiation damping. But there is another term proportional to $\ddot{x}$ which was sometimes held to be physically meaningless on the grounds that it depends on the charge distribution and diverges in the limit of a point charge. For an extended charge, however, it is finite and calculable. Being 90° out of phase with the damping component, it gives rise to reactive, frequency shift effects. M. Crisp[18] studied the effect of this term in NCT, using the nonrelativistic spinless Schrodinger equation and the two-level approximation. For the Lyman alpha line [$\psi(t) = a(t)\psi_{1s} + b(t)\psi_{2p}$], where one would expect the two-level approximation to be best, he found a result slightly different from the QED prediction, but within the experimental error. But for the Balmer alpha ending on 2s the result was only about two-thirds of the hoped-for 1058 + 27 = 1085 MHz. The exact numerical value is unimportant, because in any event the two-level approximation is basically inconsistent; i.e., if we demand that $\psi(t) = a_1(t)\psi_1 + a_2(t)\psi_2$ with no other terms present, then the charge density $\rho(x,t) = e|\psi|^2$ cannot oscillate at any other frequency than $(E_1 - E_2)/\hbar$ without violating charge conservation, $\nabla\cdot J + \dot{\rho} = 0$.

A more complete calculation is then needed; this was done by J. Mahanty[19] by an elegant contour integral method. In first order, the result agreed with the original "Bethe logarithm" term; the last several equations of Mahanty are identical with those of E. A. Power.[12]

Nevertheless, we are not entitled to claim success; for the NCT equations also predict the "dynamic Lamb shift" chirp discussed before,[14] the emitted frequency varying from $\omega_o + \Delta\omega_{Lamb}$ to $\omega_o - \Delta\omega_{Lamb}$ as the atom moves down from the excited state to the ground state. There is now very convincing experimental evidence of Citron, Gray, Gabel, and Stroud[20] indicating that this chirp does not, after all, exist. Their experiment is in principle identical with one I performed in 1951, observing unsymmetrical resonance curves of piano strings due to nonlinearities that cause the pitch to rise with amplitude. But in the optical case no asymmetry could be detected. The mere fact of getting the right numerical magnitude of $\Delta\omega_{Lamb}$ cannot be claimed as a valid "derivation" of the Lamb shift if we do not get also the correct qualitative behavior.[17]

However, this emphasis on the radiation reaction field did point to what now appears as the correct answer. Let us use the radiation reaction field, but interpret it as an operator. However, it is an operator not on the "Maxwell Hilbert space" of a quantized field, but on the "Dirac Hilbert space" of the electrons. This is the "source field" approach, which need not be discussed at length here, since it has already moved out of the research journals and become textbook material. Allen and Eberly[21] give a unified discussion of the work of Series,[22] Senitzky,[23] Milonni, Ackerhalt and Smith,[24] and Fain and Khanin.[25] When extended from two-level systems to real systems, it now appears that source fields will give a proper account of the Lamb shift, the anomalous moment, and presumably all of the usual "electrodynamic" effects. This approach is being carried much further in a series of articles by R. K. Bullough and collaborators.[26]

Of course, source-field theory is not in conflict with QED; it is a truncated form of QED in which one notices that, with proper ordering of the operators at t = 0, the vacuum fluctuations of the quantized EM field play no role in the phenomenon; and so the quantized free field and its whole Maxwell-Hilbert space need never be introduced at all. This should be adequate for any problem of electrodynamics, since in a very fundamental sense every EM field is a source field from somewhere.

On the other hand, if we define QED as the theory based on quantizing the radiation field, representing it by operators on a new Maxwell-Hilbert space, then source-field theory could hardly be called "Quantum Electrodynamics." If the above speculations should prove correct, the electrodynamics of the future will be far simpler than QED, having no use for the quantized free field, its Maxwell-Hilbert space, and its vacuum fluctuations.

3. WHERE ARE THE VACUUM FLUCTUATIONS?

Why then, was there so much early confidence that vacuum fluctuations are "very real things," essential to account for experimental facts? Why did calculations like those of Welton[11] and Power[12] succeed? Part of the answer is well-known, and is discussed at length in references 21-25. At time t = 0 the field and current operators commute. Whatever order we use, the results of the calculation must be the same, but the physical interpretation is different. With one ordering, it appears that the effects are due only to the source field. With any other ordering, vacuum fluctuations play a role; but there is no ordering for which vacuum fluctuations are the sole mechanism at work.

This independence of the initial ordering is, then, just a very simple, general, and elegant fluctuation-dissipation theorem; but let me suggest a different physical interpretation from the usual one. This complete interchangeability of source-field effects and vacuum-fluctuation effects does not show that vacuum fluctuations are "real." It shows that source field effects are the same as if vacuum fluctuations were present. For many years, starting with Einstein's relation between diffusion coefficient and mobility, theoreticians have been discovering a steady stream of close mathematical connections between stochastic problems and dynamical problems. It has taken us a long time to recognize that QED was just another example of this.

But in another sense, I do have to concede that vacuum fluctuations are, after all, "very real things." Consider an atom emitting light. The energy density of these hypothetical zero-point fluctuations, in a small frequency band $\Delta\omega$, is

$$W_{ZP} = \rho(\omega)\Delta\omega = \frac{1}{2}\hbar\omega\cdot\frac{\omega^2}{\pi^2 c^3}\Delta\omega \text{ ergs/cm}^3 \quad . \tag{1}$$

Over what bandwidth $\Delta\omega$ should this be effective in causing the atom to radiate? Presumably, over the width of the natural emission line, as determined by the Einstein A-coefficient

$$A = \frac{4\mu^2\omega_o^3}{\hbar c^3} \tag{2}$$

where μ is the electric dipole moment matrix element for the transition, ω_o the natural line frequency. Exponential decay at this rate, energy $\sim\exp(-At)$, leads to the usual Lorentzian spectral density of the radiation:

$I(\omega) \sim [(\omega - \omega_0^2) + (A/2)^2]^{-1}$, which has no sharply defined width; but the effective width $\Delta\omega$ as far as energy is concerned, is determined by the condition that $I(\omega_0)\Delta\omega$ shall equal $\int I(\omega)d\omega$, the total energy radiated. This yields the result $\Delta\omega = \pi A/2$.

W_{ZP} is the sum of six equal contributions from the averages of $\{E_x^2, E_y^2, E_z^2, H_x^2, H_y^2, H_z^2\}$, only one of which (say E_z, the component parallel to the atom's dipole moment) interacts with the atom. The energy density in the effective field E_z is then $\left(W_{ZP}\right)_{eff} = (1/6)\rho(\omega)(\pi A/2)$, or

$$\left(W_{ZP}\right)_{eff} = \frac{1}{18\pi}\,\mu^2\left(\frac{\omega}{c}\right)^6 \text{ ergs/cm}^3 \,, \tag{3}$$

and we note with interest that Planck's constant has cancelled out.

Now in classical electromagnetic theory, radiation from an oscillating dipole $\mu(t)$ is not attributed to "zero-point fluctuations" but to the radiation reaction field against which the dipole must do work:

$$E_{RR} = \frac{2}{3c^3}\frac{d^3\mu}{dt^3} = \frac{2\omega^3}{3c^3}\,\mu \,. \tag{4}$$

This provides an energy density at the position of the atom, of

$$W_{RR} = \frac{E_{RR}^2}{8\pi} = \frac{1}{18\pi}\,\mu^2\left(\frac{\omega}{c}\right)^6 \,. \tag{5}$$

But this is identical with (3)! The radiating atom is indeed interacting with an EM field of the intensity predicted by the zero-point energy; but this is just the atom's own radiation reaction field.

The fantastic numbers noted before[14] disappear as soon as we realize that, in order to account for spontaneous emission, there is no need for this energy density to be present in all space, at all times, in all frequency bands. It is produced automatically by the radiating atom, but in a more economical way; only the field component that is needed, where it is needed, when it is needed, and in the frequency band needed.

But are there other phenomena which require zero-point energy throughout space? What are we to make of the calculation of E. A. Power[12] obtaining the Lamb shift from the change in total zero-point energy of a region due to coupling the field to a hydrogen atom in its 2s state?

4. LAMB SHIFT IN CLASSICAL MECHANICS

Space not permitting an explicit, detailed answer to this question, let us note a quite general relation between two methods of calculating line shifts, which holds even in classical mechanics. We have a set of classical harmonic oscillators (p_i,q_i), the "field oscillators," coupled to one "extra oscillator" (P,Q) via coupling constants α_i, the total Hamiltonian being

$$H = \sum_{i=1}^{n} \frac{1}{2}(p_i^2 + \omega_i^2 q_i^2) + \frac{1}{2}(P^2 + \Omega^2 Q^2) + \sum_{i=1}^{n} \alpha_i q_i Q \qquad (6)$$

and define the dispersion function

$$K(\nu) \equiv \sum_i \frac{\alpha_i^2}{\omega_i^2 - \nu^2} = \int_0^\infty K(t)e^{-st}\,dt\,, \quad s = i\nu \qquad (7)$$

If $q_1(0) = \dot{q}_1(0) = 0$, the extra oscillator decays according to a Volterra equation:

$$\ddot{Q} + \Omega^2 Q = \int_0^t K(t-t')Q(t')dt' \tag{8}$$

which has the exact solution

$$Q(t) = Q(0)\dot{G}(t) + \dot{Q}(0)G(t) \quad , \quad t > 0 \tag{9}$$

with the Green's function

$$G(t) = \frac{1}{2\pi} \int_{-\infty}^{\infty} \frac{e^{i\nu t}\, d\nu}{\Omega^2 - \nu^2 - K(\nu)} \tag{10}$$

the integration contour passing below all the poles ν_k, which lie only on the real axis [since $\nu^2 + K(\nu)$ cannot be real unless ν is real] and represent the normal mode frequencies.

Method 1. In the limit of large mode density, $\Sigma_i(\ )_i \to \int(\ )\rho_0(\omega)d\omega$, then on the path of integration $\mathrm{Im}(\nu) < 0$, $K(\nu)$ goes into

$$K(\nu) \to \int_0^{\infty} \frac{\alpha^2(\omega)\rho_0(\omega)}{\omega^2 - \nu^2}\, d\omega \quad . \tag{11}$$

Supposing $\alpha(\omega)$ a smooth function, and assuming a sharp resonance, certain small terms may be neglected; and $G(t)$ goes into

$$G(t) \to \exp(-\Gamma t)\, \frac{\sin(\Omega+\Delta)t}{(\Omega+\Delta)} \quad , \quad t > 0 \tag{12}$$

where we have defined the "spontaneous emission rate"

$$\Gamma \equiv \frac{\alpha^2(\Omega)\rho_0(\Omega)}{4\Omega^2} \tag{13}$$

and the "radiative frequency shift"

$$\Delta \equiv \frac{1}{2\Omega} P \int_0^\infty \frac{\alpha^2(\omega)\rho_0(\omega)}{\Omega^2 - \omega^2} \, d\omega \quad . \tag{14}$$

Thus, for example, the vibrations of a plucked guitar string are damped and shifted by its coupling to the acoustical radiation field, the expressions for these effects having a rather familiar appearance. Obviously, we have invoked no field "vacuum fluctuations," having started with the explicit initial conditions of a quiescent field, $q_i = \dot{q}_i = 0$. The damping and shifting are due entirely to the source field reacting back on the extra oscillator.

Method 2: The mode density $\rho_0(\omega)$ of the free field is changed by the added oscillator by a small increment: $\rho_0(\omega) \to \rho(\omega) = \rho_0(\omega) + \rho_1(\omega)$. By a somewhat delicate analysis of the limiting behavior of $K(\nu)$ on the real axis, to be given elsewhere, we can deduce

$$\rho_1(\nu)d\nu = \frac{1}{\pi} \frac{\Gamma \, d\nu}{(\nu - \Omega - \Delta)^2 + \Gamma^2} \tag{15}$$

which is just the spectrum of the damped oscillation (12) of the dynamical solution:

$$\int \rho_1(\nu) e^{i\nu t} \, d\nu = \exp(i\Omega + i\Delta - \Gamma)t, \qquad t \geq 0 \ , \tag{16}$$

a connection which holds generally, even when [due to variations in $\alpha(\omega)$] $\rho_1(\omega)$ is not Lorentzian and the damping is not simple exponential. Every detail of the transient decay of the dynamical problem (9) is, so to speak, "frozen into" the static mode density increment function $\rho_1(\omega)$.

It is normalized $[\int \rho_1(\omega)d\omega = 1]$, since the "global" effect of the coupling is to add one more mode to the system.

Recognizing this, we could as well calculate the line shift Δ by the time-honored methods of "subtraction physics." Before the coupling is turned on, the total frequency of all modes is a badly divergent expression:

$$\Omega + \int_0^\infty \omega \rho_0(\omega)d\omega = (\infty)_1 \quad . \tag{17}$$

Afterward, it is

$$\int_0^\infty \omega[\rho_0(\omega) + \rho_1(\omega)]\, d\omega = (\infty)_2 \tag{18}$$

which is no better. But then the change in total mode frequency due to the coupling is

$$(\infty)_2 - (\infty)_1 = \int_0^\infty \omega \rho_1(\omega)d\omega - \Omega = \Delta \ . \tag{19}$$

It is an awkward way of asking the question; but it leads to the same answer.

Perhaps from this one can understand why Power[12] and Mahanty[19] were able to calculate the Lamb shift from the total change in the (infinite) zero-point energy, without any need for the zero-point energy to be physically real. In fact, they calculated the total change in all mode frequencies; a quantity that is equal to the shift in the dynamical problem even if all modes are perfectly quiescent. Perhaps also, one can now look at calculations of the Casimir attraction effect through new eyes.

5. CONCLUSION

It is clear that, over the past ten years, theoretical and experimental work in Quantum Optics has yielded--just as I had hoped it would--important and, to all of us, surprising fundamental new information about electrodynamics. In both QED and classical theory, our judgment as to which parts are elements of truth, which are elements of nonsense, which parts are necessary to account for experimental facts, which parts were unnecessary complications, are very different today. In QED, both the pessimism at the highbrow level and the childlike faith at the lowbrow level have been greatly reduced; and as a result, I think we can now continue the pursuit of truth in a more rational way, with more emphasis on demonstrable fact, less on ideology.

But it is equally clear that we are still very far from having separated all the truth from all the nonsense. Having shown that the Maxwell Hilbert space and vacuum fluctuations are not necessary for the Lamb shift does not prove that they are not needed at all. But it does lead us to raise again the question of 1966: Is there now any experimental fact in electrodynamics which still requires the Maxwell Hilbert space and/or vacuum fluctuations for its explanation? If not, then the Electrodynamics of the future can be considerably simpler than QED, for source field theory can be re-cast in a mathematical form appropriate to its own nature; and not dictated by Ancient History.

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POST MORTEM ON THE BET

This note is added because the final moments of Peter Franken's little circus act were complicated, and several eyewitnesses came away with quite different impressions of what had actually happened. The decision imposed a considerable--and unforeseen--burden on Professor Lamb, for which I alone am responsible, and for which I apologize to him. I know that Peter would have been just as happy as I to hear a different verdict, provided this could have been clear and unequivocal. What is important now is to understand the technical situation as it emerges from this. Those who came away confused about the facts had plenty of company. Briefly, the present situation is this:

(1) NCT did fail to predict the facts for the Lamb shift, as we now believe them to be. Professor Lamb's verdict was therefore entirely proper and just according to the conditions of the bet.

(2) However, the failure of NCT lay not in the numerical value of the shift, but in the qualitative matter of the "dynamical Lamb shift" chirp. At the 1972 meeting[14] I stressed the importance of obtaining experimental evidence about this, because a dozen theoretical decisions, and the interpretation of several other experiments, all hung on the issue whether this chirp does or does not exist. The Citron[20] experiment now seems to show that it does not.

(3) The implications of this for the Lamb shift are the following. NCT will still agree with existing experiments, which measure only the stimulating frequency needed to initiate a transition starting from

a metastable S state.[18] But if the Lamb shift could be measured starting from a P state, it now appears that NCT would predict not the wrong magnitude, but the wrong sign, of the shift. It is for this reason that I stated in my presentation, "we are not entitled to claim success."

(4) But this leads us into another mystery; for the sign reversal that leads to this discrepancy has nothing to do with the anti-quantum heresies of NCT. It has been a hitherto unquestioned part of quantum theory, appearing already in the Kramers-Heisenberg dispersion formula. Here an atom in state n irradiated with frequency ν has an electric polarizability

$$\alpha_n(\nu) = \frac{2}{\hbar} \sum_m \frac{|\mu_{mn}|^2 \omega_{mn}}{\omega_{mn}^2 - \nu^2}$$

The factor ω_{mn} has opposite signs for upward and downward transitions, as analyzed by Ladenburg [Zeit. f. Phys. 48, 15 (1928)].

This same sign reversal appears in a more modern context. An atom, in absorbing energy $E = \hbar ck$ from an incident plane wave k, picks up momentum (on either classical or quantum theory) $E/c = \hbar k$. An atom stimulated to emit by the same plane wave responds in opposite phase, feels the opposite (J x B) Lorentz force, and ought to experience the opposite recoil $(-\hbar k)$ if the stimulated emission is fast compared to the spontaneous. In photon language, the photon is emitted in the forward direction. The same is true in NCT; an atom absorbs radiation by emitting a spherical wavelet with such phase that it partially cancels the incident wave in the forward direction (incipient shadow formation). In stimulated emission the wavelet has the opposite phase,

and makes instead a bright spot in the forward direction. Indeed, it was Professor Lamb who pointed out this directional property of stimulated emission, at the 1961 Quantum Electronics Conference.

(5) We see, then, how acute the mystery is: In NCT, this same sign reversal carries through to reactive as well as dissipative effects; i.e., reversing the sign of the atomic currents reverses the sign of the frequency shift, as well as the direction of energy flow. Does this seem right on physical grounds? It seems to me that it does; for at low frequencies this becomes: if you reverse the sign of the current in an inductor, it becomes a capacitor and has the opposite tuning effects. Evidently, we are still far from understanding how a real atom emits or absorbs light, in the sense of having any self-consistent picture.

(6) Could we, then, get more experimental evidence trying to pin down the exact role of this sign reversal? A direct experimental test of the directional properties of stimulated emission would establish whether it is still operative there. Also, if some experimentalist could figure out how to measure the Lamb shift via a $P \rightarrow S$ transition instead of $S \rightarrow P$, this would check what the Citron experiment seems to imply. The difficulty thus far has been that metastable P states are hard to come by.

It would be important to get such evidence, because as we noted, a two-level wave function cannot oscillate at other than its natural frequency without violating charge conservation. Perhaps in the Citron experiment the pumping, so carefully adjusted to give a pure two-level state, also inadvertently wiped out the very effect that one was trying to observe!

(7) Finally, it should be noted that there was considerable uncertainty in all our minds as to just what the real issue of the bet had been, the only written record being that of MacAdam.[17] Two quite different issues are: (1) whether QED and vacuum fluctuations are necessary; and (2) whether NCT is adequate, to account for the Lamb shift. In fact, it was issue (1) that I stressed in my 1966 talk, just as MacAdam's report suggests. At the time, of course, issue (2) was nothing but a barely formulated conjecture. Today it is clear that, if I was wrong on issue (2), then the defenders of QED were equally--and perhaps more importantly--wrong on issue (1), as shown by the source field theory that none of us foresaw. However, all had agreed to accept Professor Lamb's decision; and so he had the burden not only of choosing the winner, but also of choosing the issue.

This being the case, if he had merely announced his verdict without stating his reasons, there would be nothing more to say, except perhaps for some petulant remarks that to opt for issue (2) was to stack the deck so that I was to do all the work, and any error on my part was penalized, while those with the opposite view did nothing, and had all their sins forgiven in advance; but such is life. I have only myself to blame for backing into such a trap.

But that is not quite the end; for those of us familiar with NCT were thrown into utter bewilderment when Lamb chose to reveal his reasoning. He held the question of the chirp unimportant, on the grounds that "we didn't see it in our experiment." He then decided that I had failed to give him a full calculation of the magnitude of the effect, and expressed surprise that I did not do the calculation by looking at the resonance condition, the thing actually observed in his experiments.

Now at the 1972 meeting I told him that I knew how to get the right numerical value, but was claiming nothing because I was dissatisfied with the result on qualitative physical grounds. In June 1976 I sent Lamb and Franken a long but not complete calculation, which addressed itself precisely to determining that resonance condition via Mahanty's contour integral representation of Eq. (19), and nearly made contact with the already published calculations of Power[12] and Mahanty.[19] Admittedly the manuscript, being unfinished for reasons that have nothing to do with the scientific issue, failed to refer to Power and Mahanty, and I promised to send the rest of it presently. However, the manuscript has never been finished, because evidence for the non-existence of the chirp began to appear, and I decided that if the effect is going to come out qualitatively wrong, then a mere quantitative number is irrelevant.

What is bewildering now, is the realization that if I had dreamt that Lamb would look only at the number, and consider the qualitative nature of the predicted effect unimportant, I could have won the bet effortlessly--even on issue (2)--five years ago, and saved us all a great deal of trouble. Nevertheless, I am glad that did not happen, because then we probably would not have the Citron experiment today, and a great deal more has been learned (still unpublished) about these matters since my 1972 report.[14] We are closer to the real truth about electrodynamics for the way events have turned out.