nova.tex, 7-8-1996.

SCATTERING OF LIGHT BY FREE ELECTRONS

as a Test of Quantum Theory*

E. T. Jaynes
Wayman Crow Professor of Physics
Washington University
St. Louis MO 63130, U.S.A.

Abstract: Schrödinger and Heisenberg gave two very different views about the physical meaning of an electron wave function. We argue that Schrödinger's view may have been dismissed prematurely through failure to appreciate the stabilizing effects of forces due to Zitterbewegung, and suggest experiments now feasible which might decide the issue.

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* Presented at the Workshop on "The Electron 1990", St. Francis Xavier University, Antigonish, Nova Scotia, August 3, 1990. To be published in the Proceedings volume, A. Weingartshofer & D. Hestenes, Editors, Kluwer Academic Publishers, Holland.

1. INTRODUCTION

We are gathered here to discuss the present fundamental knowledge about electrons and how we might improve it. On the one hand it seems strange that this is the first such meeting, since for a Century electrons have been the most discussed things in physics. And for all this time a growing mass of technology has been based on them, which today dominates every home and office. But on the other hand, this very fact makes it seem strange that a meeting like this could be needed. How could all this marvelously successful technology exist unless we already knew all about electrons?

The answer is that technology runs far ahead of real understanding. For Centuries practical men grew better varieties of grapes and bred faster horses without any conception of chromosomes and DNA. The most easily perceived facts give sufficient knowledge to start a technology, and trial–and–error experimentation takes over from there. Because of this, the practical men who give us our technology sometimes see no need for fundamental knowledge, and even deprecate it.

This happens even within a supposedly scientific field. The mathematics of epicycles was a successful 'technology' found by trial–and–error for describing and predicting the motion of planets, and because of this success the idea that all astronomical phenomena *must* be described in terms of epicycles captured men's minds for over 1000 years. The efforts of Copernicus, Kepler, and Galileo to find the 'chromosomes and DNA' underlying epicycles were not only deprecated, but violently opposed by the practical men who, being concerned only with phenomenology, found in epicycles all they needed.

We know today that the mathematical scheme of epicycles was flexible enough (a potentially unlimited number of epicycles available, whose size and period could be chosen at will) so that however the planets moved, it could always have been 'accounted for' by invoking enough epicycles. But a mathematical system that is flexible enough to represent any phenomenology, is empty of physical content. Indeed, the real content of any physical theory lies precisely in the *constraints* that it imposes on phenomena; the stronger the constraints, the more cogent and useful the theory.

In the next two Sections, we summarize the historical background of the puzzled thinking that motivates our present efforts. The reader who wants to get on with the job currently before us may turn at once to Section 4 below.

2. IS QUANTUM THEORY A SYSTEM OF EPICYCLES?

Today, Quantum Mechanics (QM) and Quantum Electrodynamics (QED) have great pragmatic success – small wonder, since they were created, like epicycles, by empirical trial–and–error guided by just that requirement. For example, when we advanced from the hydrogen atom to the helium atom, no theoretical principle told us whether we should represent the two electrons by two wave functions in ordinary 3-d space, or one wave function in a 6-d configuration space; only trial–and–error showed which choice leads to the right answers.

Then to account for the effects now called 'electron spin', no theoretical principle told Goudsmit and Uhlenbeck how this should be incorporated into the mathematics. The expedient that finally gave the right answers depended on Pauli's knowing about the two-valued representations of the rotation group, discovered by Cartan in 1913.

In advancing to QED, no theoretical principle told Dirac that electromagnetic field modes should be quantized like material harmonic oscillators; and for reasons to be explained here by Asim Barut, we think it still an open question whether the right choice was made. It leads to many right answers but also to some horrendously wrong ones that theorists simply ignore; but it is now known that virtually all the right answers could have been found without, while some of the wrong ones were *caused by,* field quantization.

Because of their empirical origins, QM and QED are not physical theories at all. In contrast, Newtonian celestial mechanics, Relativity, and Mendelian genetics are physical theories, because their mathematics was developed by reasoning out the consequences of clearly stated physical principles which constrained the possibilities. To this day we have no constraining principle from which one can deduce the mathematics of QM and QED; in every new situation we must appeal once again to empirical evidence to tell us how we must choose our mathematics in order to get the right answers.

In other words, the mathematical system of present quantum theory is, like that of epicycles, unconstrained by any physical principles. Those who have not perceived this have pointed to its empirical success to justify a claim that all phenomena must be described in terms of Hilbert spaces, energy levels, etc. This claim (and the gratuitous addition that it must be interpreted physically in a particular manner) have captured the minds of physicists for over sixty years. And for those same sixty years, all efforts to get at the nonlinear 'chromosomes and DNA' underlying that linear mathematics have been deprecated and opposed by those practical men who, being concerned only with phenomenology, find in the present formalism all they need.

But is not this system of mathematics also flexible enough to accommodate any phenomenology, whatever it might be? Others have raised this question seriously in connection with the BCS theory of superconductivity. We have all been taught that it is a marvelous success of quantum theory, accounting for persistent currents, Meissner effect, isotope effect, Josephson effect, etc. Yet on examination one realizes that the model Hamiltonian is phenomenological, chosen not from first principles but by trial–and–error so as to agree with just those experiments.

Then in what sense can one claim that the BCS theory gives a *physical explanation* of superconductivity? Surely, if the Meissner effect did not exist, a different phenomenological model would have been invented, that does not predict it; one could have claimed just as great a success for quantum theory whatever the phenomenology to be explained.

This situation is not limited to superconductivity; in magnetic resonance, whatever the observed spectrum, one has always been able to invent a phenomenological spin–Hamiltonian that "accounts" for it. In high–energy physics one observes a few facts and considers it a big advance – and great new triumph for quantum theory – when it is always found possible to invent a model conforming to QM, that "accounts" for them. The 'technology' of QM, like that of epicycles, has run far ahead of real understanding.

This is the grounds for our suggestion (Jaynes, 1989) that present QM is only an empty mathematical shell in which a future physical theory may, perhaps, be built. But however that may be, the point we want to stress is that the success – however great – of an empirically developed set of rules gives us no reason to believe in any particular physical interpretation of them. No physical principles went into them.

Contrast this with the logical status of a real physical theory; the success of Newtonian celestial mechanics does give us a valid reason for believing in the restricting inverse–square law, from which it was deduced; the success of relativity theory gives us an excellent reason for believing in the principle of relativity, from which it was deduced. Theories need not refer specifically to physics: the success of economic predictions made from the restricting law of supply and demand gives us a valid reason for believing in that law.

3. BUT WHAT IS WRONG WITH IT?

Of course, finding a successful empirical equation can be an important beginning of real understanding; perhaps even the necessary first step. In this sense, the mathematics of QM does contain some very important and fundamental truth; but the process by which it was found reveals nothing about its meaning, and it remains not only logically undefined, but pragmatically incomplete. It can, for example, predict the relative time of decay of two Co^{60} nuclei only with a probable error of about five years; but the experimentalist can measure this interval to a fraction of a microsecond.

Contemplating this, we understand why Bohr once remarked that the 'deep truths' are ones for which the opposite is also true; repeatedly, the attempt to present a unified front on questions of interpretation forces QM into schizoid positions. In spite of the fact that the experimenter can measure details about individual decays that the theory cannot predict, those who speculate about the deeper immediate cause of each individual decay are considered incompetent, and the currently taught physical interpretation claims that QM is already a complete description of reality.

Then it contradicts itself by its inability to describe reality at all. For example, we are not allowed to ask: "What is really happening when an atom emits light?" We may ask only: "What is the probability that, if we make a measurement, we shall find that a photon has been emitted?" As Bohr emphasized repeatedly, the Copenhagen interpretation of quantum theory cannot, as a matter of principle, answer any question of the form: "What is really happening when - - - ?" Yet we submit that such questions are exactly the ones that a physicist ought to be trying to answer; for the purpose of science is to understand the real world. If there were no such thing as a reality that exists independently of human knowledge, then there could be no point to physics or any other science.

It is not only in radioactivity that QM is pragmatically incomplete; for example, the data record from a Stern-Gerlach experiment can tell not only the number of particles in each beam, but the time order in which 'spin up' and 'spin down' occurred. Indeed, as we noted long ago (Jaynes, 1957), in every real experiment the experimenter can observe things that the theory cannot predict. Always, official quantum theory takes a schizoid position, admitting that the theory is observationally incomplete; yet persisting in the claim that it is logically complete.

Even the EPR paradox failed to force retraction of this claim, and so currently taught quantum theory still contains the schizoid elements of local acausality on the one hand – and instantaneous action at a distance on the other! We find it astonishing that anyone could seriously advocate such a theory.

In short, the currently taught physical interpretation has elements of nonsense and mysticism which have troubled thoughtful physicists, starting with Einstein and Schrödinger, for over sixty years. The more deeply one thinks about these things, the more troubled he becomes and the more convinced that the present interpretive scheme of quantum theory is long overdue for a drastic modification. We want to do everything we can to help find it.

Not surprisingly, there has been no really significant advance in basic understanding since the 1927 Solvay Congress, in which this schizoid mentality became solidified into physics. Theoretical physics can hardly hope to make any further progress in such understanding until we learn how to separate the permanent mathematical truths from the physical nonsense that now obscures them.

To be fair, we should add that some of these contradictions disappear when we note that "currently taught" quantum theory is quite different from the "Copenhagen theory", defined as the teachings of Niels Bohr. The latter is much more defensible than the former if we recognize that Bohr's intention was never to describe reality at all; only our information about reality. This is a legitimate goal in its own right, and it has a useful – indeed, necessary – role to play in physics as discussed further in Jaynes (1986, 1990).

The trouble is that this is far from the *only* legitimate goal of physics; yet for 60 years Bohr's teachings have been perverted into attempts to deprecate and discourage any further thinking aimed at finding the causes underlying microphenomena. Such thinking is termed 'obsolete mechanistic materialism', but those who hurl such epithets then reveal their schizoid mentality when they ascribe unquestioning ontological reality – independent of human information – to things such as 'quantum jumps' and 'vacuum fluctuations'. This does violence to Bohr's teachings; yet those who commit it claim to be disciples of Bohr.

For a time we were optimistic because it appeared that the new thinking of John Bell (1987) might show us the way. It was refreshing to see from his words that he was not brainwashed by the conventional muddled thinking and teaching, but was able to discern the real difficulty. But his recent work (Bell, 1990) shows him apparently at the end of his rope, reduced to destructive criticism of the ideas of everybody else but offering nothing to replace them. Therefore it is up to us to find the new constructive ideas that theoretical physics needs.

4. OUR JOB FOR TODAY

Theoretical work of the kind presented at this meeting is sometimes held to be "out of the mainstream" of current thinking; but that is quite mistaken. There is no mainstream today; it has long since dried up and our vessel is grounded. We are trying rather to start a new stream able to carry science a little further. Indeed, our efforts are much closer to the traditional mainstream of science than much of what is done in theoretical physics today. Talk of tachyons, superstrings, worm holes, the wave function of the universe, the first 10^{-40} second after the big bang, etc., is speculation vastly more wild and far–fetched than anything we are doing.

In the present discussion we want to look at the problems of QM from a very elementary, lowbrow physical viewpoint in the hope of seeing things that the highbrow mathematical viewpoint does not see. I want to suggest, in agreement with David Hestenes, that Zitterbewegung (ZBW) is a real phenomenon with real physical consequences that underlie all of quantum theory; indeed, such important consequences that without ZBW the world would be very different and we would not be here. But my ZBW differs from his in some basic qualitative respects, and so our first order of business is to describe this difference and see whether it could be tested experimentally. Then we can proceed to some speculations about the role of ZBW in the world.

5. THE PUZZLE OF SPACE–TIME ALGEBRA

It is now about 25 years since I started trying to read David Hestenes' work on space-time algebra. All this time, I have been convinced that there is something true, fundamental, and extremely important for physics in it. But I am still bewildered as to what it is, because he writes in a language that I find indecipherable; his message just does not come through to me. Let me explain my difficulty, not just to display my own ignorance, but to warn those who work on space–time algebra: nearly all physicists have the same hangup, and you are never going to get an appreciative hearing from physicists until you learn how to explain what you are doing, in plain language that makes physical sense to us.

Physicists go into a state of mental shock when we see a single equation which purports to represent the sum of a scalar and a vector. All of our training, from childhood on, has ground into us that one must never even dream of doing such an absurd thing; the sin is even worse than committing a dimensional inhomogeneity. How can David get away with this when the rest of us cannot?

If u and v are vectors, then in Hestenes' equation $(uv = u \cdot v + u \wedge v)$ the symbol '+' must have a different meaning than it does in conventional mathematics. But then the symbol '=' must also have some different meaning, and he does not choose to enlighten us, so the above equation remains incomprehensible to me. The closest I can come to making sense out of it is to note that we cannot speak of a sum of apples and oranges; yet we may place an apple and an orange side by side and contemplate them together. Perhaps this is something like the intended meaning.

There is another possibility. Perhaps '+' and '=' have their conventional meanings after all, but u and v do not. In my view it simply does not make sense to speak of the sum of a scalar and a vector, any more than of the proverbial square circle; but it makes perfectly good sense to speak, as Cartan does, of the sum of two *matrices*, interpreted as abstract mathematical *representations* of a scalar and a vector. If this is what David really means, he could have prevented decades of confusion by a slight change in verbiage. Physicists are very touchy about the distinction between an physical or geometrical object and a mathematical representation of that object, because the mathematical representation usually holds only in some restricted domain that does not apply to the object itself.

But I suspect that what he "really means" is something more abstract than either of these two suggestions, and neither his writings nor his talks provide enough clues for me to decide what it is. I have never been able to get past that equation, with any comprehension of what is being said.

Passing on to the next part of Hestenes' work, we encounter a physical difficulty. In discussing the Dirac equation we read that some symbol "stands for a rotation". Now it is evident that from a mathematical standpoint there is an abstract correspondence with rotations (composition law of the rotation group). But from a physical standpoint, rotation of *what*? When I look at the Dirac equation, I see just what Schrödinger did: a four–component wave function $\{\psi_\sigma(x,t)\}$ representing a continuous distribution of something (perhaps probability or charge); but that something itself has no further internal directional structure that could 'rotate'.

But just at this meeting, I finally picked up the first clue as to what David is talking about here. It seems that when he looks at the Dirac equation, he sees not a continuous distribution of anything, but *a tangle of all the different possible trajectories of a point particle*! Presumably, these are the things that are rotating. This must be the most egregious example of a hidden–variable theory ever dreamt of, and nothing of his that I have ever read prepared me for this revelation.

I would never, in 1000 years, have thought of looking at the Dirac equation in that way. For, in any theory where the underlying reality is conceived to be a single particle hiding somewhere in the wave packet, the behavior of the packet is determined, not by what that particle actually does, but by the range of *possible* things that it might have done, but did not. Thus the wave packet cannot itself describe any reality; it represents only a state of knowledge about reality. Indeed, this is the view that Heisenberg (1958) stated very explicitly.

You can, of course, account for many facts by such a picture – namely, those so unsharp in time and space that what the experimentalist observes can be regarded as some kind of average over an ensemble of many different trajectories. But when the experimental result ought to depend on *one* such motion, I think that the point particle trajectory picture will surely fail, because there is no such thing in Nature as a point particle.

Put more constructively, a point particle theory can be confirmed only by an experiment which actually sees that particle, removed from its ensemble or wave packet, doing something *as a particle*, all by itself. In an exactly similar way, Louis Pasteur's microbe theory of diseases could be confirmed only by developing the instruments by which a single microbe could be seen and its behavior observed.

Of course, we must be as demanding as Pasteur about the resolving power of our instruments. A continuous structure that is small compared to our resolution distance will appear to us as a point particle. That a microbe is not a point particle, but a continuous structure with definite shape and internal moving parts, can be learned only with a microscope that has a resolution distance small compared to the dimensions of the microbe.

We must accomplish something like this, in order to check the reality of those tangled trajectories which were supposed to be the 'chromosomes and DNA' hiding in the Dirac wave function. But is there any such experiment where our observation is so sharp in both time and space that it depends on a single trajectory? It seems to me that there are simple (in concept) experiments now on the borderline of feasibility, which are capable of testing this rather fundamental issue. In fact, they do not differ in principle – or even in the relevant dimensions and resolving power – from Pasteur's microscopes.

6. WHAT IS A FREE ELECTRON?

We have long been intrigued with the fact that in applications of quantum theory, in our equations we write only wave functions ψ, either explicitly or implicitly (as matrix elements between such wave functions). But in the interpretive words between those equations we use only the language of point particles. Even the Feynman diagrams are a part of that inter–equation language, depicting particles rather than waves.

Thus the wave–particle duality is partly an artifact of our own making, signifying only our own inability to decide what we are talking about. But the predictions of observable facts come entirely from wave functions $\psi(x,t) = r(x,t)\exp[i\phi(x,t)]$; and not merely the magnitudes $|\psi|^2 = r^2$, but even more from the phases $\phi(x,t)$. Then if anything in the mathematics of QM could be held to represent some kind of reality, it is surely the complex wave function itself, not that point particle imagined to be hiding somewhere in it, but which plays no part in our calculations. This is just Schrödinger's original viewpoint.

The idea that a free electron is something more like an amoeba than a point particle was suggested by David Bohm (1951); but there he was only trying to help us visualize the mathematics of wave packets. Here we want to endow that suggestion with physical reality and suppose, with Schrödinger, that a wave packet $\psi(x,t)$ is not merely a representation of a state of information about an electron; but a physically real thing in its own right, with a shape and internal moving parts that are capable of being changed by external interactions and observed by us. The arguments that were raised against this picture long ago (spreading of the wave packet, etc.) are easily answered today, as we shall see. The spreading wave packet solution does not, in our theory, describe the physical free electron; among all solutions of the Dirac equation it represents a set of measure zero.

This thinking – long on the back burner, so to speak – was moved to the front burner by an incident that occurred at the 1977 meeting on free electron lasers at Telluride, Colorado. I gave a talk (Jaynes, 1978) on the general principles for generating light from electrons, which contained some speculation about possible explanations of the then much discussed 'blue electron' effect reported by Schwartz & Hora (1969). Here 50 kev free electrons are irradiated by blue light from an Argon laser, then drift 20 cm and were reported to emit the same color blue light on striking an alumina screen which is normally not luminescent.

It requires very little thought to see that such an effect cannot be accounted for by mutual coherence properties of different electrons, which would require impossible collimation of the electron beam; somehow, each individual electron must be made to carry the information about the light wavelength, for a million light cycles after irradiation.

My calculation showed that interaction with laser light can perturb the wave packet of a free electron, $\psi_0(x,t) \rightarrow \psi_0 + \psi_1$, so that it is partially separated into a linear array of smaller lumps, making the transverse density profile

$$\rho(x) \;=\; |\psi_0 + \psi_1|^2 \;\propto\; |\psi_0|^2\,[1 + A\,\cos(2\pi x/\lambda)]$$

look somewhat like a comb, with teeth [those internal moving parts] separated by the light wavelength λ; here A is proportional to the light amplitude and the cosine of a polarization angle. Then when all these lumps, moving in the z-direction, strike a screen simultaneously, there is in effect a pulse of simultaneous current elements with that separation, which can act like an end–fire array and radiate light in the x-direction with the original wavelength λ (but, of course, with a much broader spectrum, whose width indicates the lateral size of the electron wave packet $|\psi_0|^2$ in the x-direction).

To my astonishment, Willis Lamb objected strongly to this, saying "You don't understand quantum theory. The electron is not broken up into many little electrons; the lumps are only lumps of probability for one electron. There is no interference effect in the radiation emitted when the electron is suddenly decelerated, because the electron is in reality in only one of those lumps."

This is a beautiful example of that schizoid attitude toward reality that believers in QM are obliged to develop; for he believed at the same time that if an electron wave packet is broken up into separated lumps by passing through the standard two slit apparatus, there is interference between those lumps, making the standard electron diffraction pattern (and showing, at least to me, that the electron *was* in both lumps simultaneously).

I was so taken aback by Willis's objection that I then did a conventional QED calculation, which showed to my satisfaction that standard QED *does* predict the interference effect that I had obtained so much more easily from a semiclassical picture. In fact, my calculation predicted just the dependence on polarization and drift distance that Schwartz and Hora reported seeing.

Other experimentalists have insisted that the effect does not exist (or at least could not have been observed because of impossible requirements for collimation of the electron beam) and it is not considered respectable to mention it today. But according to our wave packet theory, the collimation should not matter; perhaps their failure to confirm the effect was due just to their concentration on the irrelevancy of collimation, while the essential thing was that Schwartz's electron gun, with its small holes, produced electrons in wave packets of the right size.

If the effect does not exist, this would seem to be a major embarrassment for quantum theory; surely, predicting so easily an effect that does not exist is just as bad as failing to predict an effect that does exist [as someone put it at the time, if the effect is confirmed it will become known as *der Schwartz–Hora Effekt*; if not, it will be *die schwarze Aura*]. In any event, if the effect was not reproducible at the time – for whatever reason – the technology available today might overcome the old difficulties.

What has remained from this incident is the picture of the wave packet of a free electron as something with a physical meaning, its size and shape in principle measurable. For example, an electron in a wave packet ten microns long is physically different from one in a wave packet two microns long; a spherical wave packet electron is physically different from one a cigar–shaped one, and those differences should be observable in experiments now becoming feasible.

This issue now suggests our proposed experiment. Eventually we shall return to Zitterbewegung, but for the time being the only issue before us is: *Does interference exist between light waves scattered from different parts of a free electron wave packet*? If it does not, then something like the Lamb–Hestenes picture must be correct; if it does, then a whole new world of observable physical phenomena is opened up. As the old 1977 calculation showed in one case, quantum theory predicts a mass of very detailed, observable, new phenomena. They would make a free electron, when prepared in various sizes and shapes by previous irradiation, even more versatile in behavior than an amoeba.

In fact, that technology noted in the Introduction has already indicated something of the possibilities here. The electron diffraction microscope is able to reveal to us an amazing variety of fine detail. All this information is contained somehow in the electron wave functions; but it is surely not in coherence properties of different electrons, which could never be collimated well enough for that. Each individual electron must be carrying, in its phases (*i.e.* the distortion of its wave fronts) a vast amount of information about what it has passed through. It seems to us that if electrons lacked this plastic, amoeba–like character, electron microscopes would not work.

Consider, then, the exact analog of Pasteur's observation of a microbe: scattering of light of wavelength λ by a free electron which we represent by a wave packet of dimensions perhaps 2λ – 10λ. If we are to see any interference between different parts of the wave packet, then that hypothetical point particle must be in two different places at the same time (or at least, at two places with spacelike separation). But it is easy for a continuous wave structure to give such interferences.

7. RELATIVISTIC BASIS OF THE NR SCHRÖDINGER EQUATION

To define the proposed experiment it will be sufficient to use the nonrelativistic spinless Schrödinger equation, if we note first how and under what circumstances it can arise from the relativistic Klein–Gordon equation satisfied by each Dirac wave component:

$$\nabla^2\phi - \frac{1}{c^2}\frac{\partial^2\phi}{\partial\phi^2} = \left(\frac{mc}{\hbar}\right)^2 \phi \tag{1}$$

This has plane wave solutions of the form

$$\phi(x,t) = \exp(ik\cdot x - \omega t) \tag{2}$$

with

$$\omega \equiv c\sqrt{k^2 + \left(\frac{mc}{\hbar}\right)^2} \tag{3}$$

and thus $|\omega| \geq mc^2/\hbar$. The possible frequencies of propagating waves lie into two ranges separated by $2mc^2/\hbar$, which is just the ZBW frequency. Frequencies $|\omega| < mc^2/\hbar$ correspond to waves evanescent in space. Now given any solution of (1) which has only frequencies in one of those propagating ranges, say $\exp(-i\omega t)$ with $mc^2/\hbar \leq \omega$, we can view the solution as a rapid oscillation at frequency $mc^2/\hbar$, modulated by an envelope function which may be slowly varying. To separate them, make the substitution

$$\phi(x,t) = \psi(x,t)\exp\left(-\frac{imc^2}{\hbar}t\right) \tag{4}$$

whereupon $\psi(x,t)$ is found to satisfy the equation

$$i\hbar\dot{\psi} = -\frac{\hbar^2}{2m}\nabla^2\psi + \frac{\hbar^2}{2mc^2}\ddot{\psi} \tag{5}$$

which is exact. Now if $\psi(x,t)$ contains frequencies up to order ν, the two time derivative terms are in approximately the ratio

$$\frac{\hbar^2\,\ddot{\psi}/2mc^2}{i\hbar\dot{\psi}} \approx \frac{\hbar\nu}{2mc^2} \tag{6}$$

and if this is small compared to one, we have the NR Schrödinger equation

$$i\hbar\dot{\psi} = -\frac{\hbar^2}{2m}\nabla^2\psi \tag{7}$$

but we now understand when it applies and what it means. It describes the slow 'secular' variations in the envelope when the relativistic wave function has only components $\exp(-i\omega t)$ with no admixture of terms $\exp(+i\omega t)$.

The point we stress is that solving the NR Schrödinger equation does not give us an approximation to an arbitrary relativistic solution; but only to those particular solutions *which contain no ZBW effects.* But the solutions we have discarded in making this approximation comprise in a sense the 'vast majority' of all possible relativistic solutions!

With this perhaps 'new' understanding, we may reexamine the conventional solutions of (7) for our problem. It has the general initial-value solution

$$\psi_0(x,t) = \int G(x-x')\,\psi_0(x',0)\,d^3x' \tag{8}$$

with the Green's function

$$G(x-x';t) = \left(\frac{m}{2\pi i\hbar t}\right)^{3/2} \exp\left\{\frac{imr^2}{2\hbar t}\right\} \tag{9}$$

corresponding to diffusion with an imaginary diffusion coefficient. In particular, with an initial packet of RMS radius a,

$$\psi_0(x,0) = \left(\frac{1}{2\pi a^2}\right)^{3/4} \exp\left(-\frac{r^2}{4a^2}\right) \tag{10}$$

we find the canonical spreading wave packet solution of our textbooks:

$$\psi_0(x,t) = \left(\frac{a}{\sqrt{2\pi}\,(a^2+\frac{i\hbar t}{2m})}\right)^{3/2} \exp\left\{-\frac{r^2}{4(a^2+\frac{i\hbar t}{2m})}\right\} \tag{11}$$

for which the probability density or normalized charge density is given by

$$|\psi_0(x,t)|^2 = \left(\frac{1}{2\pi\sigma^2}\right)^{3/2} \exp\left\{-\frac{r^2}{2\sigma^2}\right\} \tag{12}$$

with

$$\sigma^2(t) \equiv a^2 + \frac{\hbar^2 t^2}{4m^2 a^2} \tag{13}$$

so the packet grows with an ultimate spreading velocity which 'remembers' its initial size compared to the Compton wavelength:

$$\left(\frac{v}{c}\right)_{final} = \frac{(\hbar/mc)}{2a} \tag{14}$$

Doubtless all of us were, as students, assigned the homework problem of calculating from (13) how long it would require for some object like a marble to double its size (which now seems to me a totally wrong conception of what quantum theory is and says).

8. NON–ARISTOTELIAN SCATTERING OF LIGHT BY FREE ELECTRONS

We want to look at the wave packets of free electrons in the same way that Pasteur looked at microbes. But conventional quantum mechanical scattering theory does not contemplate this, being based on an older idea, namely Aristotle's theory of Dramatic Unity. This prescribes that "The action must be complete, having

(1) a beginning which implies no necessary antecedent, but is itself a natural antecedent of something to come,

(2) a middle, which requires other matters to precede and follow, and

(3) an end, which naturally follows upon something else, but implies nothing following it." (Taylor, 1913)

Conventional scattering theory follows this plan perfectly, presupposing an initial state in which the particles involved are propagating as free particles but we ask not whence they come, an intermediate state in which the action takes place, and a final state in which they again propagate as free particles and we ask not where they go.

Note that the conventional textbook arguments (the Heisenberg γ–ray microscope, etc.) warning us that there is an uncertainty principle making it impossible to do so many things, always presuppose Aristotelian scattering and draw those conclusions from overall momentum conservation, coupled with a naïve 'buckshot' picture of a photon and ascribing a separate ontological reality to the scattering of each individual photon. But the actual mathematical formalism of QED, as developed afterward, does not have anything in it corresponding to the concept of 'a given photon'.

It seems to us that these things should be pointed out in elementary QM courses; the Heisenberg conclusion, far from being a firmly established foundation principle of physics, is not an experimental fact at all, only an unverified conjecture which presupposes just the things that we want to test here. It is high time that we found out whether it is true, and our technology is just now coming to the point where the experiments are possible.

But the scattering theory that we and Pasteur need is non–Aristotelian, in that the action does not have a beginning or an end at any times that are relevant to what we observe; rather we have something akin to a slowly changing nearly steady state, the incident light constantly bathing the electron or microbe and the scattered radiation constantly proceeding from it.

This makes an important technical difference, in that principles like overall momentum conservation, although we do not deny them, do not have the same application that they have in conventional scattering theory. This is not a handicap; on the contrary, it means that we, like electron microscopists, shall be able to see details that Aristotelian scattering theory does not describe. Requiring that the action be complete before we observe anything greatly restricts what one could see.

In the above we have used the notation ψ_0 to denote solutions of the free-particle equation (7). In a transverse EM field, the minimal coupling *ansatz* makes the replacement $p \rightarrow p - (e/c)A$, giving in first order

$$i\hbar\dot{\psi} = \frac{p^2}{2m} - \frac{e}{mc}\,(A \cdot p)\,\psi \tag{15}$$

To see what this interaction term means physically, consider a classical point electron in the same EM field:

$$m\dot{v} = eE = -\frac{e}{c}\,\dot{A} \tag{16}$$

or, in a gauge where $A = 0$ when $v = 0$,

$$v(t) = -\,\frac{e}{mc}\,A(t) \tag{17}$$

as in the London theory of superconductivity (where this equation accounts for a great deal of that phenomenology noted above). Therefore, we have

$$\frac{e}{mc}\,A \cdot p\,\psi = -v \cdot p\,\psi = i\hbar\,(v \cdot \nabla)\,\psi \tag{18}$$

and two terms of (15) combine thus

$$i\hbar\,\dot{\psi} + \frac{e}{mc}\,A \cdot p\,\psi = i\hbar\left(\frac{\partial}{\partial t} + v \cdot \nabla\right)\,\psi \tag{19}$$

which we recognize as the 'convective derivative' of hydrodynamics, so we have simply

$$i\hbar \frac{D\psi}{Dt} = -\frac{\hbar^2}{2m} \nabla^2 \psi\,, \tag{20}$$

which is exactly equivalent to (15). The change in ψ as seen by a local observer moving at the classical electron velocity is just the free particle spreading! The external perturbation, in first order, merely translates the wave function locally by the classical motion. Recognizing this, we can write the solution and the current response immediately without any need to work out perturbation solutions of (15). A fairly good solution of the initial value problem for (15) is simply

$$\psi(x,t) = \psi_0(x - \int v dt,\ t) \tag{21}$$

which we may visualize as in (Fig. 1).

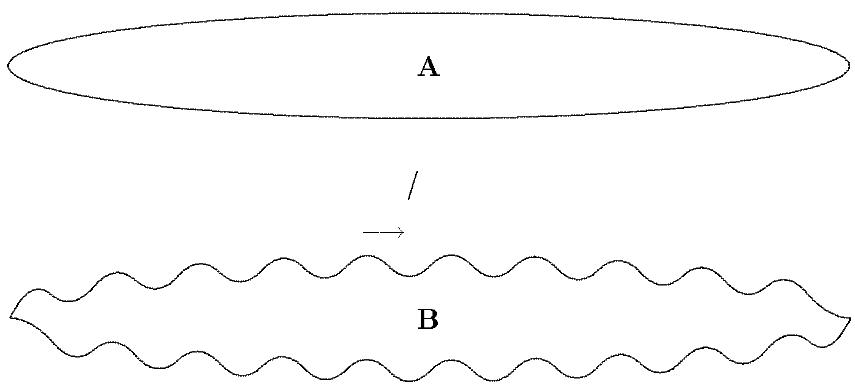

Fig. 1. (A) A cigar-shaped wave packet 5 microns long, unperturbed. (B) The same packet with light of wavelength $\lambda = 0.5$ micron incident from the left, polarized vertically. The undulations are moving to the right at the velocity of light.

To fix orders of magnitude, think of a wave packet a few microns in size, optical wavelengths of perhaps a half micron. Whenever the optical wavelength is small compared to the size of the wave packet, then the perturbation converts an initially smooth wave packet into something more like a caterpillar than an amoeba, with undulations moving forward at the speed of light (of course, the size of the undulations is greatly exaggerated in Fig. 1; they are the same size as the displacement ($x_0 = eA/mc\omega$) of a classical point electron, and amount only to perhaps 10^{-9} cm in rather intense laser light at optical frequencies).

Then the incident wave $A_{inc}(x,t)$ induces a local current response

$$J(x,t) = \frac{e}{c}\,|\psi(x,t)|^2\, v(t) = -\frac{e^2}{mc^2}\,|\psi|^2\, A_{inc}(x,t) \qquad emu \tag{22}$$

and we note that the original Klein-Nishina derivation of the Compton cross–section, and the Dyson (1951) calculation of vacuum polarization started from this same local current response. To calculate the field scattered by it they used standard classical EM theory (thus providing two early examples of the fact that QED does not actually use field quantization); and we shall do the same.

Resolving the current and field into time fourier components:

$$J(x,t) = \int \frac{d\omega}{2\pi} J(x,\omega) \exp(-i\omega t) \tag{23}$$

etc., the scattered field at position x' is

$$A_{sc}(x',\omega) = \int d^3x \, J(x,\omega) \left(\frac{e^{i\omega r/c}}{r}\right) \tag{24}$$

where $r \equiv |x - x'|$. Using (23) and making the far-field approximation this becomes

$$A_{sc}(x',\omega) = -\frac{e^2}{mc^2}\frac{e^{ikR}}{R} \int d^3x \, |\psi(x)|^2 \, e^{-ik\cdot x} A_{inc}(x,\omega) \tag{25}$$

where R is the distance from the center of gravity x_0 of the wave packet to the field point x', and k is a vector of magnitude ω/c pointing from x_0 to x'. Finally, noting that the coefficient is the classical electron radius $r_0 = e^2/mc^2$ and taking the incident field as a plane wave with propagation vector k_0:

$$A_{inc}(x,\omega) = A_0 e^{ik_0 \cdot x} \tag{26}$$

we have the scattered wave in the direction of k:

$$A_{sc}(x',\omega) = -r_0 A_0 \, \frac{e^{ikR}}{R} \, \rho(k_0 - k) \tag{27}$$

where

$$\rho(k_0 - k) \equiv \int d^3x \, |\psi(x)|^2 \, e^{i(k_0 - k)\cdot x} \tag{28}$$

is the space fourier transform of the wave packet density. Note that $\rho(0) = 1$ is the statement that the wave function is normalized. The differential scattering cross–section into the element of solid angle $d\Omega$ is then

$$\frac{d\sigma}{d\Omega} = r_0^2 |\rho(k_0 - k)|^2 \, \sin^2\theta \tag{29}$$

where $\theta = (A_0, k)$ is a colatitude polarization angle. As a check, if the wave packet size $a << \lambda$, then $\rho(k_0 - k) \to 1$, and we have for the total scattering cross–section

$$\sigma = \oint d\Omega \, r_0^2 \, \sin^2\theta = \frac{8\pi}{3} r_0^2 \, , \tag{30}$$

the usual classical Thompson cross-section. Put most succinctly, the scattering experiment measures

$$\left[\frac{Cross\ section\ of\ wave\ packet}{Cross\ section\ of\ classical\ electron}\right] = |\rho(k_o - k)|^2 \tag{31}$$

so if the experiment is feasible and the wave function is something physically real, one should get information about the size and shape of the wave packet from the directional properties of the scattered light. If the true object is only a point particle and the wave function represents only a state of knowledge about its possible positions, then at optical frequencies one should see only the classical cross–section at all scattering angles, whatever the size of the wave packet.

Scattering of laser light by free electrons has been observed experimentally – as long ago as 1963 experimenters were reporting it – but to the best of our knowledge they have not looked for this effect. The first order of business is simply to verify whether these interference effects are or are not observable; but we have not yet specified what size wave packets are to be expected. One experimental clue is provided by electron interference downstream from a fine Wollaston wire; how thick can the wire be while we still see interference? The answer appears to be about 4 microns; thus we would conclude that the wave packet must be about this size.

The result is interpreted differently by those experimentalists who talk in terms of collimation of electron beams instead of wave packets; they represent an electron by a plane wave and view the 4 microns as the coherence range of plane waves with slightly different directions. Of course, a wave packet $\psi(x)$ can be fourier analyzed and the components $\Psi(k)$ would have that meaning; but the spread of k–values in a wave packet has nothing to do with the spread of velocities of the different electrons in the beam; it appears to us that they fail to see this distinction. In our view, the 4 microns must be seen as a property of a single electron, not an indication of lack of parallelism of trajectories of different electrons.

9. BACK TO ZITTERBEWEGUNG

We propose that, while ZBW is such a high-frequency effect that it does not play any great role in the scattering of optical frequency light, nevertheless ZBW is the origin of forces that modify the electron wave packet, so that the 'spreading wave packet' solution (11) does not describe the real free electron. Indeed, we have seen from the above derivation that the conventional spreading wave packet solution describes only the NR approximation to a very special relativistic motion in which ZBW effects are absent. But the description of a real free electron must use the relativistic Dirac equation, and include the effects of the interaction of the electron with its own electromagnetic field.

Mathematically, in early QED this interaction diverged, and in no theory can it be considered a negligibly small perturbation. Conceptually, as Einstein warned us long ago (Jaynes, 1989), neither the electron nor the field can exist without the other and their interaction is never turned off, so it is not possible to describe either correctly if we ignore the other. But whenever ZBW oscillations are present this represents a current oscillating at frequency $\omega \sim 2mc^2/\hbar$, whose electromagnetic field reacts back on the electron and modifies its behavior.

A current $J(x', t')$ generates a field $A(x,t) = \int D(x-x', t-t')J(x', t')d^3x'dt'$. This in turn exerts a local force density on the current $J(x,t)$ given by $F = J(x,t) \times [\nabla \times A(x,t)]$. If the currents at both positions have the same ZBW frequency, then there is a time–average secular force that depends on their relative phases and can be either attractive or repulsive.

We emphasize again that these speculations are quite modest and respectable compared to those utterly wild ones noted above, which dominate present theoretical physics. The effect we are proposing is not strange or new; it is predicted by standard relativistic Quantum Theory (which does not forbid the use of wave packets in our calculations); only it was not heretofore overtly mentioned. We are only trying to anticipate, by physical reasoning, what observable effects this secular force might have.

One reason why the effect was not noted before is that the current-current interaction

$$\int\int J(x)D(x-y)J(y)$$

was perceived as only an *energy* term. That it also represents a *force* could, of course, have been found by carrying out a variation δa of the size of the wave packet, but here the physical outlook of the calculators prevented them from doing this. One did not think of a free electron wave packet as a physically real thing that might be distorted by local forces; scattering theories calculated only matrix elements between those Aristotelian plane–wave states. Thus the actual size and shape of a wave packet never got into the calculations.

Why do we include only transverse fields here? We think the answer is that, because of fine features of the Dirac equation not presently in view, these are the only fields actually generated by the Dirac current. In any event, to include a longitudinal interaction in the present calculations would introduce a Coulomb repulsion between different parts of the wave packet, which would probably be much stronger than the transverse forces studied here. That would cause the wave packet to explode in a time far shorter than the Gaussian spreading time of (13).

But we knew from the start that we must never include a coulomb interaction of an electron with itself. For example, if in the hydrogen atom we interpret $\rho(x,t) = e|\psi(x,t)|^2$, we must still use the Hamiltonian $H = p^2/2m + V(x)$ including only the coulomb field $V_{prot} = -e^2/r$ of the proton. We must not include a term $V_{int} = e\int(\rho(x')/r)d^3x'$, or our hydrogen atom would be completely disrupted into something qualitatively different from the atom that we know experimentally. In quantum theory, longitudinal and transverse fields have quite different properties (and, perhaps, different physical origins). This issue requires more study, both in our theory and in conventional QED (where we have also managed only to get around it in a pragmatic sense, not actually resolve it theoretically).

10. FORCES DUE TO ZITTERBEWEGUNG

Our calculation is not different in principle from those that Asim Barut and Tom Grandy do, trying to find solutions of the coupled Maxwell and Dirac equations. However, we are looking at a different phenomenon for which specific solutions may be much harder to find, so in the present work we concentrate on getting a clear picture of the physical mechanisms at work; only after this is well understood would we be ready to tackle the explicit solutions that we shall demand eventually.

Our current comes from the Dirac equation in the standard way: $j^\mu = e\,\bar{\psi}\gamma^\mu\psi$. But if the wave function is an admixture of what are usually called positive and negative energy solutions [which we think should be called only positive and negative frequency solutions], this has high frequency oscillations at the ZBW frequency $\omega = 2mc^2/\hbar$. For a monochromatic component of the current, the time component of j^μ is determined by charge conservation from the ordinary vector components $J(x,t) = \{j^1, j^2, j^3\}$, which therefore determine the entire radiated field. In the present case, we consider the oscillating current solenoidal: $\nabla \cdot J = 0$, so only transverse fields are generated.

Consider now two small regions of space d^3x_1, d^3x_2 both inside the wave packet but separated by a distance $r \equiv |x_2 - x_1|$ large compared to the ZBW wavelength $\hbar/2mc \simeq 10^{-10}$ cm. Denote the current elements in these by

$$\begin{aligned} J(x_1,t)\,d^3x_1 &= I_1\,\cos(\omega t + \phi_1) \\ J(x_2,t)\,d^3x_2 &= I_2\,\cos(\omega t + \phi_2) \end{aligned} \tag{32}$$

The current element I_1 generates at the position x_2 an EM field

$$A_1(x_2,t) = f(r,t,\phi_1)\,I_1 \tag{33}$$

where the 'propagator' is

$$f(r,t,\phi_1) \equiv \frac{\cos(\omega t - kr + \phi_1)}{r}. \tag{34}$$

and, as usual, $k \equiv \omega/c$. This exerts on d^3x_2 an element of force

$$dF_2(t) = J(x_2,t) \times [\nabla \times A_1(x_2,t)]\, d^3x_2 = \cos(\omega t + \phi_2)\, I_2 \times [\nabla f \times I_1] \tag{35}$$

But

$$\nabla f = \frac{\partial f}{\partial r} \nabla r = k\, \frac{\sin(\omega t - kr + \phi_1)}{r}\, n \tag{36}$$

where $n \equiv \nabla r$ is a unit vector pointing from $x_1 \to x_2$, and we used the aforementioned long distance condition $kr >> 1$ to discard a near field term that is appreciable only at points within a ZBW wavelength of the current.

Now the force density at x_2 takes the form

$$F(x_2,t) = \frac{k\, \sin(\omega t - kr + \phi_1)\, \cos(\omega t + \phi_2)}{r}\, I_2 \times (n \times I_1) \tag{37}$$

This has terms oscillating at frequency 2ω and constant ones: to get the time average over a ZBW cycle, note that

$$\overline{\sin(\omega t - kr + \phi_1)\, \cos(\omega t + \phi_2)} = (1/2)\, \sin(\phi_1 - \phi_2 - kr) \tag{38}$$

so the time average force density seen at x_2 due to the current element at x_1 is

$$\overline{F} = \frac{k\, \sin(\phi_1 - \phi_2 - kr)}{2r}\, [n(I_1 \cdot I_2) - I_1(n \cdot I_2)] \tag{39}$$

Its component along n depends only on the product of transverse components of the currents:

$$n \cdot \overline{F} = \frac{k\, \sin(\phi_1 - \phi_2 - kr)}{2r}\, [(I_1 \cdot I_2) - (n \cdot I_1)\,(n \cdot I_2)] \tag{40}$$

which can be either positive (repulsive) or negative (attractive) depending on the relative phases. Thus ZBW currents generate secular force terms which must modify the wave packet of a free electron, and give it a tendency to expand or contract, in addition to the conventional spreading in (11).

There is also a secular term from the ZBW electric field; but this is purely transverse to n and does not contribute to attraction or repulsion. Indeed, since it is unnatural to suppose that two isolated current elements can produce a net torque on themselves, the time average of the electric field forces must just cancel the transverse component of (39): $n \times \overline{F}_{total} = 0$, and (40) gives the entire force. Note that the phases ϕ can vary with position along r in such a way that there would be virtually no external radiation in the n direction, while the total phase term $(\phi_1 - \phi_2 - kr)$ remains mostly at values which give attractive internal forces; this hints at a more general stability property.

But this is of no interest unless the ZBW forces are large enough to compete with the spreading tendency exhibited in (11). Now it is evident already that these forces are orders of magnitude stronger than the corresponding ones due to the same currents at optical frequencies, because from (36) the magnetic field in the radiation zone generated by a given current is proportional to the frequency. Let us estimate their general magnitude, very crudely.

Supposing a wave packet with dimensions a, a normalized wave function will have a magnitude indicated by $|\psi|^2\, a^3 \simeq 1$. Therefore the current densities $J = e\, \bar{\psi} \gamma^\mu \psi$ might conceivably be as large as about e/a^3; let us suppose they are a tenth of that, about $e/10a^3$. Then consider the wave packet broken up into two volume elements $d^3x \simeq a^3/2$, separated by an average distance of about $a/2$. The current coefficients I_1, I_2 above are of the order of $(e/10a^3)\,(a^3/2) = e/20$. If phases are optimal, the attractive force (40) between them could be as large as $F \simeq kI^2/a = ke^2/400a$. The binding energy of these parts to each other would then be of the order of $Fa/2 = (1/800)\, mce^2/\hbar = mc^2/(800 \cdot 137)$, or about 4.5 ev. The parts would also have some internal binding energy of their own.

We see that the ZBW forces are easily strong enough to do the job we require of them; one can depart considerably from the 'optimum' conditions and still have 0.5 ev of binding energy, enough to stabilize the packet. We might put it thus: in a kind of bootstrap operation, the wave packet digs its own potential well and is confined in it (unable to spread), while remaining free to move about and carry the well along with it, like a turtle trapped in its own shell.

There is a close analogy – perhaps more than an analogy – to some well known things in solid–state theory. As in Fig 2, the propagating frequency ranges are the 'conduction bands' of space, while the 'forbidden band' in which solutions oscillatory in time are spatially evanescent, lies between them. But in the solid–state case, any additional potential which perturbs the periodic lattice potential, can make additional solutions possible in the forbidden band; the localized bound states lying just outside the conduction band, caused by donor or acceptor impurity atoms. This is depicted io Fig 2; and having seen it, we find it hard to avoid supposing that free positrons must be represented by short lines just above the lower conduction band. Some physical arguments in support of this view of things are given below.

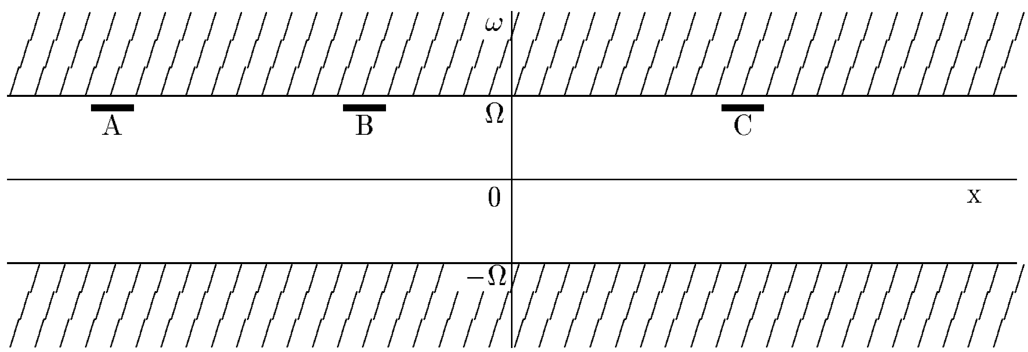

Figure 2. The solid-state analogy. The 'conduction' or propagating regions have frequencies $\omega > \Omega$, $\omega < -\Omega$, while the 'forbidden' region lies between them. Then the physical free electron is thought of as corresponding to localized but mobile states (A, B, C), with wave function oscillating at just below the propagating frequency $\Omega \equiv mc^2/\hbar$.

Given this picture, let us close by indulging in some perhaps wild and free speculation. Evidently, there is a bewildering variety of possible solutions here; presumably, many different initial conditions can be realized, for which the subsequent motions do almost every conceivable thing. But attractive forces always win over repulsive ones because they lower the energy, and then there is no way to escape from them; the energy needed to expand the packet against attractive forces has been radiated away. So after some time the solutions must, inexorably, settle down to some final stable nonradiating limit cycle.

This, we suggest, means that the original arguments against Schrödinger's interpretation of the wave function no longer hold; the real free electron wave packet does not spread indefinitely, but settles down into some steady state of definite size (perhaps about 4 microns) in which the attractive ZBW forces just balance the spreading tendency in (11). This would be the experimental 'aged' free electron, loosely analogous to the 'dressed' particle of current theory, but quite different in essential properties.

The final stable solution will be easier to find than any particular transient one because we have some guiding principles for this. Firstly, the balancing of attractive and spreading tendencies calls for an analysis rather like Einstein's original argument relating mobility and diffusion constant. Secondly, mechanical stability against arbitrary small deformations gives us a variational principle. Thirdly, the condition of no net external radiation imposes conditions on the possible stable current distributions. Finally, all this must satisfy the Dirac equation, in such a way as to just maintain this steady current oscillation.

In fact, a very general electro–mechanical theorem tells us that the second and third principles are closely related, and gives us the additional information that the actual oscillation frequencies $\pm\Omega$ in the final steady packet will be slightly less than $mc^2/\hbar$, the difference indicating the 'binding energy' of the wave packet relative to a plane wave as depicted in Fig. 2.

The condition for mechanical stability is the same as the condition for no net external radiation. To state this, let a current distribution $j(x,t)$ be confined to a finite spatial volume, and take the fourier transform

$$J(k,\omega) \equiv \int d^3x dt\, j(x,t) \exp[i(\omega t - k\cdot x)]$$

Then from standard EM theory, the necessary and sufficient condition for no external radiation is $k \times J(k,\omega) = 0$ whenever $c|k| = \omega$. When this condition is not met, the radiation can exert relatively strong forces on external objects, inducing changes in their states; thus stability is a joint property of a system and its surroundings.

Why do we not see fragments of electrons (leptoquarks?) flying about, produced by collisions, slits, etc? Perhaps we do, but are not mentally prepared to recognize them. But in those experiments there are always very many electrons involved, and in those fragments the nonradiation condition is far from satisfied; they interact resonantly with neighboring fragments to bring them back together into the stable 'aged' electrons before they arrive at our detectors (and indeed, very few electron detectors actually measure the charge of the things they are detecting). Again, even though the initial phases are random, attractive forces always win out because their effects reinforce, drawing the pieces together and thus increasing the attractive forces. Note also that the ZBW forces are long range compared to Coulomb forces, falling off only as $1/r$; thus fairly distant fragments can interact strongly.

On any slow perturbation of an oscillating system, there is a principle of stationary action, which makes changes of energy proportional to changes of frequency. This was known to Lord Rayleigh in acoustical systems, and was used by Wien in the theory of black–body radiation, and by Einstein and Born as the 'adiabatic principle' in the early days of quantum theory.

In the writer's Neoclassical theory of electrodynamics (Jaynes, 1973) we showed a classical Hamiltonian for which this result is not merely an approximation for slow perturbations, but is an exact conservation law; and our present equations of motion can be derived from a Hamiltonian of the same form. In effect, it is a 'new' constant of the motion never contemplated in classical statistical mechanics because it is not a conservation of energy or momentum. It is a law of conservation of *action* E/ω and much of the phenomenology of quantum theory (which Bohr saw as revealing 'the inadequacy of classical concepts') is explained by it, very easily.

11. CONCLUSIONS

The full story that we have started here is much too long to tell in a single article, but let us take a glimpse at what lies ahead, if these speculations prove to have some truth in them. Of course, it is too much to expect that every detail of our present thinking will be correct; but that is unnecessary.

Indeed, even if all our speculations prove to be wrong, these ideas may stimulate new constructive thought in the right direction, which would not have happened without them.

We have developed a physical picture of ZBW as performing two essential functions in the world. Its transient solutions provide the "dither" that initiates changes of state; while its limit cycles provide the stabilizing forces that hold particles together. As early as 1904 Poincaré perceived the need for these forces, but to the best of our knowledge every previous attempt to find them has sought them in static rather than oscillatory models. The great 'advantage' of high frequency oscillations is that a given current generates much stronger EM fields, so that quite large energies and forces result.

Once one has seen this much, a great variety of new effects can be seen, which begin to suggest simple causal explanations for many of those mysterious "quantum effects" previously thought to defy explanation in classical terms. But it is evident that a very large amount of hard work remains to be done before much of this picture can be realized.

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